INCREDIBLE LIFE:

A HANDBOOK OF BIOLOGICAL MYSTERIES

WILLIAM R. CORLISS

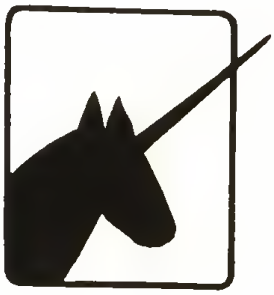
Monsters, sports, and unrecognized species of life have fascinated humans from time immemorial. But this is not another "monster" or "unknown animal" book, although a few pages have been assigned to such subjects. Rather, other biological anomalies and curiosities receive the most attention here: the amazing cooperative actions of many species, mimicry and remarkable adaptations, the convergence of external appearance, suspended animation, the effects of solar and lunar cycles on life, the manifold problems of evolutionary theory, and that "ultimate parasite" DNA. This Handbook collects and categorizes hundreds of such anomalies and curiosities so that scientists and laymen alike can focus on what is not well-explained by current hypotheses. To find these phenomena, the entire files of many scientific journals were searched, including the *Quarterly Review of Biology, Natural History, Nature Magazine, Sea Frontiers, American Naturalist, Nature, Science, New Scientist,* and others.

Although designed primarily as a reference book, this Handbook reads as intirguing nonfiction. Open it anywhere and you will find fascinating accounts of biological enigmas. It is an ideal stimulus for high school and college science classes and a unique source of ideas and data for term papers and theses. Almost 800 articles and 100 illustrations provide an unparalleled collection of reliable information for scientist and layman alike.

SCIENCE
RED DOT
C. 1
2300
R574
In2
SCI
Incredible life
Reference Book
For use in library only

INCREDIBLE LIFE:

A HANDBOOK OF BIOLOGICAL MYSTERIES



Compiled by

WILLIAM R. CORLISS

Illustrated by John C. Holden

Published and Distributed by

The Sourcebook Project

Glen Arm, MD 21057

ISBN 0-915554-07-0

LC 80-53971

Notice

This handbook is one of a series published by the Sourcebook Project, P.O. Box 107, Glen Arm, MD 21057. The handbooks and closely related loose-leaf sourcebooks may be purchased directly from the Sourcebook Project. Write for prices and details.

First printing: April 1981

Printed in the United States of America

TABLE OF CONTENTS

Each chapter begins with a detailed table of contents.

Other titles in the Handbook Series are:

HANDBOOK OF UNUSUAL NATURAL PHENOMENA
ANCIENT MAN: A HANDBOOK OF PUZZLING ARTIFACTS
MYSTERIOUS UNIVERSE: A HANDBOOK OF ASTRONOMICAL ANOMALIES
UNKNOWN EARTH: A HANDBOOK OF GEOLOGICAL ENIGMAS

Prices and details may be obtained from the Sourcebook Project,
P.O. Box 107, Glen Arm, MD 21057.

PREFACE

Despite the implications of the title, INCREDIBLE LIFE is not another book on Bigfoot, the Loch Ness Monster, and sundry biological aberrations. The subject of unrecognized species is not ignored, but this book proceeds on a much broader front. The primary objective is the recording of all types of biological anomalies, from synchronous bamboo flowering to the problem of excess DNA. Nature, as any biologist knows, is much more mysterious than a collection of monster reports.

Several threads run through almost all of the chapters. Challenges to the Theory of Evolution constitute one of the main subjects; some are overt, others subtle. Many items in this collection seem to deny a slow development of life via tiny steps. In addition, the Recapitulation Theory is often questioned; while some evidence supports the long disdained Inheritance of Acquired Characters. Since INCREDIBLE LIFE deals with anomalies, these attacks on the status quo should not be surprising.

Other persistent themes include the repeated development of flight, the luminosity of life forms, the apparent intelligence of other life forms, synchronous and collective actions, and the possible influence of geophysical and astronomical parameters upon terrestrial life. Some examples of adaptation, mimicry, parasitism, and symbiosis are so remarkable that they at least "seem" to be anomalous. Evolutionary mechanisms have been proposed for many of these phenomena but they are not as convincing as I should like to see.

With this introduction of the first person singular, I should emphasize that the following collection of phenomena and curiosities is based upon personal feelings and choices, and that some readers will doubtless find some items rather routine. My criteria for selecting "anomalous" material were: (1) the information contradicted current biological theories, or (2) the article in question raised personal questions not answered adequately to my knowledge. Usually both criteria were satisfied simultaneously. Much of the information printed herein will prove controversial, particularly some of the older, supposedly interred articles and some items from the more recent literature. It will soon become obvious to the reader that secondary objectives of the book are the posing of challenges to establishment science and the stimulation of useful controversy.

I make no claim of completeness because new and relevant material is being discovered constantly as my search of the literature continues. Indeed, the near-infinite mine of government reports, university theses, and foreign journals has been scarcely touched. Even so, I have collected much more intriguing biological information than I can publish here conveniently. Additional, more specialized books are planned.

My hope is that the present Handbook, despite its limited size and scope will become a useful reference work on the frontiers of biology. To this end, I have utilized reports taken primarily from scientific journals. The screening provided by editors and referees of these publications helps to minimize hoaxes and errors. In the expectation that establishment biology is too conservative and too constrained by dogmas, I have introduced a handful of articles from fringe periodicals and books that are doubtless considered offbeat and "wild" by most professional biologists.

Most of the nearly 100 illustrations consist of line drawings by John C. Holden and are based on sketches and photos appearing in the original articles. Since many of the articles are decades old, it was impossible to ferret out the original photos.

The bulk of this Handbook consists of direct quotations from the original sources. I hasten to acknowledge the many writers of papers, letters-to-the-editor, and other items who have contributed anomalies to the world literature. When lengthy quotations are taken from publications still protected by copyright, permissions have been obtained.

William R. Corliss

Box 107
Glen Arm, MD 21057
January 15, 1981.

Chapter 1

HUMANS AND HUMANOIDS

Chapter Contents

MORPHOLOGICAL PHENOMENA

UNUSUAL PHYSICAL ABILITIES

CURIOSITIES OF BEHAVIOR

BIOCHEMICAL ANOMALIES

UNRECOGNIZED SPECIES

MORPHOLOGICAL PHENOMENA

• Excessively Hairy Humans

HAIRY MEN

Anonymous; *Scientific American*, 29:407–408, 1873.

Two remarkable instances of hairy men arrived recently in Berlin. They are Russians, father and son, and have excited so much interest that Professor Virchow has delivered a lecture upon the phenomenon, an abstract of which appears in the Edinburgh Medical Journal.

They are peculiarly remarkable in being edentulous. They are not hairy men in the ordinary acceptance of the term, but more resemble some of the monkey tribe (the Diana monkey, cuxio, etc.); while their edentulous condition carries them yet lower in the animal scale. The eldest is a man aged over 55, Andrian by name, said to be the son of a Russian soldier from the district of Kostroma. He was born during the period of service of his reputed father, and has no resemblance to him, to his mother, or to a brother and sister whom he possesses. To escape the unkindness of his fellow villagers, Andrian fled to the woods, where he lived in a cave, and was much given to drunkenness; even yet he is said to live chiefly on sauerkraut and schnapps; but his mental condition, which is truly none of the sharpest, does not seem to have suffered, and he is, on the whole, of a kindly disposition, and affectionate to his son, and to those about him. Andrian was married, and had two children, who died young; one of these was a girl resembling her father; but of the other, a boy, nothing can be ascertained. Fedor, the boy, exhibited with him, is three years old, and comes from the same village; he is said to be Andrian's son, born in concubinage; and it is most probable that this is the case, as it would be singular were two such creatures to originate independently in one small village. The peculiarity of these individuals is that they have an excessive growth of hair upon one particular part of the body, namely, the face and neck; on the body and lower extremities there is also a stronger growth of hair; and particularly on the back and arms of the child, there are sundry patches of 0.15 inch to 0.24 inch in diameter, covered with soft yellowish white hair 0.12 to 0.24 inch long. Andrian himself has on his body isolated patches strewn, but not thickly, with hair 1.5 inches to 2 inches long. But all this is trifling and subordinate compared with the

hair growth on the face, to which attention is mainly directed. Andrian has only the left eye tooth in the upper jaw; Virchow has not stated how many teeth are in his lower jaw, but from the context it is improbable that he has more than his son, namely, four incisors. The son has no teeth, hardly any alveolar process, and the upper lip is very narrow, so that the upper jaw appears depressed; the father presents the same appearance. It is somewhat singular that a similar family has long been known to exist at Ava, and was first described by Crawford in 1829, and since then by Beigel. Three generations of this family are now known to exist. The grandfather, Shwe-maon, had a daughter Maphoon, and she again a son, all of whom present precisely the same peculiarities as in the family of Andrian, not only as to the growth of hair, but also as to the teeth. The grandfather has in the upper jaw only four incisors, in the lower jaw four incisors and one eye tooth; and these teeth did not appear till he was twenty years of age. Maphoon has only four incisors in each jaw; the eye teeth and molars are wholly wanting, the first two incisors appeared during her second year. The peculiarity of the hairiness in these individuals is of the same type as in Adrian and his son, in whom every part of the face and neck, usually only covered with lanugo, is covered with long hair, the very eyelids being so covered, the eyelashes being normal, while flowing locks come out of both nostrils, and also out of the meatus auditorius externus. At first sight, the occurrence of two such families in two such distinct parts of the world, seems to point them out as "missing links"--as the unreformed descendants of an earlier race of man. And our thoughts are carried back to the Ainos or hairy Kuriles, who are believed to be the remains of the aborigines of Japan, and who now inhabit the northern parts of the Island of Yesso and the southern part of the island of Saghalien. At first these aborigines were stated to be as hairy as our wild men; but from more accurate information, obtained by the Berlin Anthropological Society through the German resident Herr Von Brandt, accompanied by numerous photographs and Japanese pictures of these Ainos, and from an examination of a skull recently obtained through Privy Councillor Von Pelican, Virchow is able to state with positiveness that, neither in respect of the formation of hair nor in regard to the teeth, have the Ainos any analogy with the Russian or Burmese hairy men. The Ainos are certainly hairier on the chest and extremities than the nations around them, but there is nothing peculiar in the distribution of the hair, and the males have hair only on the typical parts peculiar to man. There is not a shadow of a race connection between the Ainos and the Russian hairy men; and only the most prurient imagination could connect the latter with the Burmese family. No doubt, careful breeding could raise a new race of men from this accidental variety, just as various new races of domestic animals, dogs, for instance, have been propagated from accidental varieties. Virchow, however, believes that the peculiarities, belonging to the Russian as well as to the Burmese families, depend upon idiosyncrasies of innervation, and these upon accidental congenital abnormalities in the trigeminus

within whose domain all these features present themselves, only to be ascertained by careful dissection.

A HAIRY HUMAN FAMILY

Williston, S. W.; *Science,* 9:33-34, 1887.

The abnormal growth of hair, that has been not rarely observed since antiquity in individuals of different races of mankind, presents various points of interest other than anthropological ones. As Professor Mason has stated (Science, ix. No. 205), its recently recognized cause is the persistence of the prenatal downy hair, 'lanugo' as it is called, and its rich growth through life; or rather, to speak more accurately, the non-development of the hair-follicles to adapt them to the growth of normal hair. This persistence of the embryonal covering is most strikingly shown, as a normal condition, in the ostrich (Ratitae), Apteryx, and penguin, where the hair-follicles, or, what is anatomically the same, the feather-follicles, produce through life the soft downy plumage of the chick only. This loss of the foetal hair, which takes place with the general exfoliation of the cuticle during the first year of life, is not characteristic of man, but occurs in many other, though not all, mammals. Wiedersheim (Vergl.anat., 31) sees in this lanugo, and its abnormal development in the 'hair-men,' a probable evidence of an abundant covering of hair at some early period of man's ancestry.

Julia Pastrana

The extent to which this abnormal growth of the downy hair may reach will be better appreciated from the picture, here given, of Teftichew (or Testichew), the elder Russian 'dog-man,' than can be from any description. The 'animal' or dog-like appearance in this case is more striking than in any other of which I have seen illustrations, though the Amras family of the sixteenth century presented a very similar aspect. In this family , the father, son, and daughter were all covered, according to the paintings and descriptions now extant, over the entire body with long hair, with the exception of a space below the eyes.

In the notable case of Julia Pastrana of Mexico, a most repulsive-looking person in her picture, the hair of the head, forehead, and face, was coarse like ordinary hair, and her cheeks and nose were nearly bare. She died in 1860, in giving birth to a son, who early showed similar hairiness on head and face. The prenatal hair is not necessarily soft and downy. Pathological conditions will cause it in places to be coarse, like that of the adult; and cases are known where the larger part of the body has remained through life covered with a thick coat of strong hair, due, in reality, to an enormously large mother's mark. A similiar condition is found in the coarser and more bushy growth of the beard from long-continued neuralgia or nerve-irritation.

Yet another point of interest is the undoubted tendency to heredity which these abnormal cases show. Thrice has the anomaly been known to be developed in the second generation; and once, the Birman family, in the third generation. On the other hand, the precisely opposite condition, that of absolute hairlessness from prenatal causes, not a few cases of which have been observed among different races, shows the same tendency to heredity. Likewise, supernumerary fingers, toes, teeth, and breasts in both male and female, and the presence of a short tail, are all undoubtedly capable of hereditary transmission.

The thinly haired African, or the hirsute Tasmanian, as also the great variations in the pilosity of the civilized races, present questions more within the province of the anthropologist; bearded females and beardless males, that of the physiologist, or, possibly, of the suffragist.

A HAIRY CHILD

Anonymous; *Scientific American*, 48:247, 1883.

The picture is that of a girl, six years of age, covered from head to foot with soft, silky hair. Upon first sight little Kra-o, as the child is named, would appear to be the "missing link"

A hairy child from Borneo

between the ape and man, but a closer examination of this peculiar being will prove that this diagnosis is faulty in all respects. We have simply an excellent type of hypertrichopherosis (superabundance of hair), cases of which have been known in this and previous centuries. Kra-o, who is being exhibited in London at present, is quite an intelligent child, and had acquired enough knowledge of the English language within a few months to be able to make herself understood; and this is an ample proof that, although her outward appearance is that of an animal, she has a bright mind and considerable intelligence. A correspondent of the Institution Ethnologique, Mr. H. Kaulitz-Jarlow, writes as follows to the editor of the Illustrite Zeitung:

"Kra-o is about six years old; she is of the same size as other children of her age, but of a finer build; thick, jet black hair covers her head and reaches down to the backbone, and forms a perfect mane on the shoulders; the eyebrows are wide, glossy, and silky, and the eyes are of a deep black with open pupils, and

the iris is missing entirely, as in the gorilla; the resemblance to the face of the latter is very great and astonishing; the nose is flat, and has wide nostrils inclined diagonally toward the cheek bones; the cheeks hang down and are baggy, and in them Kra-o stores her food and carries it about with her in the same manner as her cousins of the ape tribe.

Her head, like the human type more than any other part of her body, and the intelligent eyes, the agreeably rounded mouth with full lips, which can smile very pleasantly when Kra-o plays and talks, do not at all correspond with the ape-like body of the child. Kra-o is of a brownish-yellow color, and the hair extends from the crown of her head to the soles of her feet. She is generally very jolly, loves to play, and is more thankful than most children if persons take the trouble to amuse her. If she is molested and teased, her wild nature shows itself; she throws herself on the ground, screams, strikes the person, and finds great pleasure in tearing out some of her superabundance of hair."

We must call the attention of our readers to the fact that the above is only an extract from a letter from Mr. Kaulitz-Jarlow, who seems to be very enthusiastic in the matter of classifying Kra-o as one of the apes. Kra-o was found in the presence of her parents in the Loas district, in Borneo. Her father died while traveling to Bangkok, and her mother is at present at the court of the King of Loas. Mr. Karl Bock brought the child to England, and it is now exhibited by Mr. Jarini.

KRAO FARINI, "THE MISSING LINK"

Anonymous; *English Mechanic*, 60:429, 1894.

At the museum opened at St. Petersburg by Mr. Schultze, a number of remarkable things are to be seen. Krao Farini is among them, the girl who has been nicknamed the "Missing Link," and she belongs to the hairy tribes inhabiting the wild country of Laos, in the northern part of Siam. She is perfectly formed, and although she is not a beauty, her appearance is by no means repulsive.

Her whole body is covered with a thick growth of dark hair. The growth on her cheeks, lips, and chin reminds one of a beard and whiskers; and the nose is also covered with a light down. Still more wonderful to say, she has cheek-pouches, where, like the monkeys, she can stow away tit-bits for future munching, and, like our prototypes, she possesses limbs of singular pliancy. But the most remarkable thing about the freak of Nature is the great wealth of hair on her head. It grows in thick, glistening masses, which fall below the knees. Krao was seen in England some years since, where she very quickly adapted herself to the

ways of civilization. She can now speak three languages with fluency--English, French, and German. While Krao was on a visit to Berlin, one of the enterprising Germans proposed to her, but this offer of marriage was not accepted; Krao learned too much independence during her wild life in the woods. Even the celebrated Virchow declared, when he saw Krao, that she was a very extraordinary phenomenon which deserved the fullest attention.

• Sudden Hair Blanching and Color Changes

GREY HAIR

Savill, Agnes F., and Warren, Clara; *The Hair and Scalp,* Williams and Wilkins, Baltimore, 1962, pp. 39–40.

Sudden blanching of the hair has been reported by so many competent observers that the existence of the phenomenon is probable. Ephraim, reviewing the literature since 1800 for cases of sudden or rapid whitening of the hair, collected twenty-six cases thought to be caused by excitement, fright, or mental strain; seven cases observed by neurologists, and five reported by opthalmologists.

A man aged fifty-two, whose truthfulness I cannot doubt, told me that at the age of thirty-six his hair turned grey in a single night. His history was that he was motor-cycling in a thick fog on a dark night. Towards midnight he fell into a canal and spent a long time trying to reach the bank. Exhausted, he collapsed. When he regained consciousness he was told by the police that he had been picked up insensible at 6 a.m. His hair had turned grey that night. R. McNeill Love reported a case of sudden blanching. In reply to my letter asking for particulars he answered: "The patient to whom I referred was a male aged sixty-five who was nearly blown up by V bombs twice in one night. I saw him the following day and did not recognize him at first. Nor did his friends."

Sometimes the blanching of the hair takes place with abnormal rapidity. One of my patients, an intelligent observer, aged sixty-five, said that her golden-brown hair had developed its present shade of marked greyness within a week of the tragic death of her only son twenty years previously. Her doctor had remarked on the change at the funeral. Sir Arthur Hurst quoted two instances of rapid greying after terrifying experiences; one was a boy of seventeen. Jackson knew a case whose hair and beard changed from black to white and back again three times in thirty years: the change from black to white was rapid; the return to colour took five years. The hair has become white during attacks of mental insanity, remained so for a few days or hours, then re-

stored its natural colour. Dr. Claye Shaw wrote that it was not uncommon to see symmetrical white bands of hair develop during insanity and the usual colour restored during convalescence. Brown-Sequard described how the hairs of the beard can change colour very rapidly. He pulled out the few white hairs he had, so that he could observe how soon a black hair became white. He had not long to wait. Where only black hairs had been seen, and a few hairs which showed white only close to the root, he saw, two days later, five hairs, white along their entire length.

Carefully noted details of a remarkable case of rapid alteration in colour of the hair are described by Raymond. The patient was a Frenchwoman, thirty-eight years of age, with black hair. After she had suffered a period of great grief, she had been told of severe financial loss. The bad news arrived when she was menstruating, and the flow was at once checked. She became very ill and complained of acute neuralgia. After the patient had endured two days of excruciating pain in the head and shoulders, on January 30, 1882, at 2 a.m. the hair was still its normal deep-black colour. On January 31, 7 a.m., much of the hair had turned white. Some of the original dark colour remained at the sides and back of the scalp; the hair on the upper part of the head was red, and the shorter hairs were white. No alteration of colour had taken place on the hairs on the body. The severe neuralgic pains continued, and on February 1 practically all the hair of the head had changed colour. By February 2 most of the hair which had turned red on January 31 had become white. Then the hair began to fall out. The pain diminished. In fifteen days hardly any hairs were left on the head except a few on the sides and the occiput. On March 3 her remaining hairs on the sides and the occipital region were of mixed colours--white, red, and black. A fortnight later she was almost bald. The eyebrows and the eyelashes had remained black throughout. (pp.28-30)

. . . .

Many explanations of sudden and rapid blanching of the hair have been advanced. Some maintain that the altered circulation allows loosening of the cells and thus air enters the spaces between the cells of the cortex, that these reflect the light and cause the hair to appear white, although there may still be pigment present. This may be true in a few cases, just as was found in the apparent white portions of ringed hair and in Case 3 described on page 29. Jackson stated that the change of colour is due to air bubbles in the cortex rendering the cortex opaque, and so hiding the pigment. He added: "This is proved by placing one of the affected hairs in hot water, ether or turpentine, when the air bubbles will be driven out and the hair will resume its natural colour." However, in his description of sudden as distinguished from unduly rapid blanching of the hair, he, like other dermatologists, does not quote cases which have come under his personal observations.

It seems a reasonable theory that with sudden and profound emotion there may occur constriction of the vessels supplying the

hair papillae, and that when these cells are deprived of their normal nutriment their function of pigment formation must be in abeyance.

With prolonged depressing emotion the changes in the circulation resemble those produced by disease and by age; the slow decrease of pigment formation and alteration of the cortex cells in such conditions is readily understood. A more scientific explanation has been provided by the observation that Simmonds' disease (often associated with greyness) may follow sudden shock. It is probable that this effect is due to interference with the hypothalamic pituitary relationship. The hair has for too long been regarded as a dead object from the time it reaches the level of the skin. May not the medulla be a channel along which the hair shaft can receive nourishment and pigment? Its structure, with its loose cells and definite cavities, lends itself to this function. In the event of sudden deprivation of circulation in the papilla, the medulla may absorb the fluid pigment from the cortex and may convey no further supply from below; this would account for rapid greying of the appearance of the hair. Future research will determine whether this occurs more rapidly in hairs which have a medulla than in those which have none. Other arguments against the hypothesis of the hair being regarded as a dead object are: (1) Cut and singed hair ends behave as if they still had life; (2) repigmentation of white hair has been watched in alopecia areata and similar cases; and (3) the appearance and diameter of the hair varies with health and with constitutional disease (pp. 39-40).

SUDDEN BLANCHING OF HUMAN HAIR

Gowers, W. R.; *Knowledge*, 24:231, 1901.

In his paper on the whitening of the hair of animals, Mr. Lyddeker remarks that, from the evidence, the fact of rapid blanching of human hair from emotion must be accepted. It may interest him and some of your readers to know of a definite instance of this effect, not from emotion, but from disease. I have recorded it where it is not likely to be generally accessible ("Man, Dis. Nerv. Syst.," Vol. II). A man, in consequence of an injury, had hemorrhage over the greater part of the left hemisphere of the brain. During the next two days the hairs of his head and moustache on the opposite side, the right, were observed to become paler and paler, until they were almost white at the time of his death on the third day. The change extended up to the middle line and there ceased. A very curious fact is that the pale region was separated from the normal brown, by a very narrow darker zone, almost black, in the middle line. Of course emotion must act by its profound derangement of the function of the cortex of the brain. Here we had a like effect produced by an organic influence, occurring under observation, within two days, and limited to the opposite side that which the disease

would influence. The escape of the hair of the scalp, and affection of that of the face, may be ascribed to the special seat of the chief cerebral irritation. The hairs were made pale throughout their length. This, as an absolute fact, a result produced in two days, is significant. The only possible explanation is that the process, at the root of the hair, by which the normal pigment is produced, is so changed that a material is formed capable of discharging the colour of the pigment, and that this ascends the tubular hair and causes its effect, at least as far from the root as the length of the hairs on the face. The degree to which this chemical process is under the influence of the nervous system is strikingly obvious, but a similar action is treated on most of the secretory processes of the body. Tears are an illustration.

The dark line which limited the change is not easy to explain. We know that pigment is apt to accumulate at the limit of an area in which it is lessened, but we can hardly apply the idea of any movement of pigment. It would rather seem as though, at the median line, at which the innervation of the two sides mingle, the defective influence of the one side in some way, permitted that of the other side to become excessive. There are curious conditions yet to be worked out regarding the median blending of the nerve influence. In the lower part of the face the zone of coalescence is more considerable in extent and degree than in many other regions.

CHANGES IN COLOR OF HAIR AND FEATHERS

Prentiss, D. W.; *Science*, 16:183–187, 1890.

The question of change in color of the hair is an interesting one both from a physiological point of view and from the practical one of pathology. The physiological aspect embraces the question of how a change of color takes place,--whether in existing hairs, or produced by shedding of the hair and a new growth taking its place of a different color.

It has been doubted by good authority (Hebra and Kaposi) if the hair, after being once developed, can change upon the theory that the hair has no vascular or nerve connection with the general system, and must therefore be independent of nervous or systemic influence. This position is, however, not tenable. The clinical evidence is positive that the hair does change color under systemic influences, sometimes gradually, and sometimes suddenly. We hear frequently of the hair turning white in a night from violent emotions, as fright, great grief, or great joy; and it has come to be a method of expressing extreme emotion to say, "It was enough to turn one's hair white." I say it is not an uncommon thing to see mention of such cases in popular literature, but well-authenticated cases are not so often found. It is recorded in history that the hair of Marie Antoinette and Mary Queen of Scots became white suddenly from the horrors to

which they were subjected. Poets have not failed to avail themselves of the idea. Byron, in the "Prisoner of Chillon," says,--

"My hair is gray, but not with years;
Nor grew it white
In a single night,
As men's have grown from sudden fears."

A short time since, in conversation with an eminent microscopist and pathologist, I asked how he would explain from the basis of minute anatomy the sudden change in color of the hair. He replied that he did not explain it; that he did not believe it happened; that the reported cases were not authenticated. He further said, that, from the structure of the hair and its relation to the skin, he considered it impossible.

Dubring (third edition) is authority for the statement that Hebra and Kaposi discredit sudden canities. There is nevertheless no doubt of the fact that such change does sometimes occur; and, to set the matter defintely at rest, I looked up the subject in the Library of the Surgeon-General's Office. The following are some of the references found:--

Dr. William P. Dewees reports a case of puerperal convulsions under his care. From 10 a.m. to 4 p.m. fifty ounces of blood were taken. Between the time of Dr. Dewees's visits, not more than an hour, the hair anterior to the coronal suture turned white. The next day it was less light, and in four or five days was nearly its natural color. He also mentions two cases of sudden blanching from fright.

Dr. Robert Fowler reports the case of a girl sixteen years of age, apparently in good health, hair black, who found one morning in combing her hair that a strip the whole length of the back hair was white, starting from a surface about two inches square around the occipital protuberance. Two weeks later she had patches of Ephelis over the whole body.

In the Canada Journal of Medical Science, 1882, p. 113, is reported a case of sudden canities due to business worry. The microscope showed a great many air-vesicles both in the medullary substance and between the medullary and cortical substance.

Dr. Graves says most authors are of the opinion that the hair, once formed, is independent of the organism, with which opinion he disagrees, instancing Plica polonica as opposed to such a theory. He gives the following cases:--

1. A British officer in India, forty-eight years old, fell into bad health, and became prematurely gray. He returned to England, regained his health, and in four years his hair returned to its original brown.

2. In a man sixty-seven years of age, hair white, chest covered with long white hair, the chest was blistered; and when hair grew out over the blistered surface, it was black.

3. In a man, aged thirty-five, bald, a small blister the size of a crown piece was applied to vertex for congestion of the brain. Growth of hair followed over the blistered surface.

4. A lady, hair of vertex gray and very scanty, applied tar-water. Hair grew, and was of natural color.

5. The same occurred in another case after application of

citrine ointment.

In the Boston Medical and Surgical Journal, 1851, is reported a case of a man thirty years old whose hair was scared white in a day by a grisly bear. He was sick in a mining camp, was left alone, and fell asleep. On walking, he found a grisly bear standing over him.

A second case is that of a man of twenty-three years who was gambling in California. He placed his entire savings of eleven hundred dollars on the turn of a card. He was under tremendous nervous excitement while the cards were being dealt. He won. The next day his hair was perfectly white.

In the same article is the statement that the jet-black hair of the Pacific Islanders does not turn gray gradually; but when it does turn, it is sudden, usually the result of fright or sudden emotions.

The following cases are of change of color from white to black:--

Dr. Bruley, physician to the Fontainebleau, reported to the Society Medicale, Paris, in 1798, the case of a woman sixty years old, whose hair, naturally white and transparent as glass, became jet-black four days before her death (phthisis). On examination after death, the bulbs of the black hairs were of immense size and engorged with dark pigment. The roots of white hairs that remained were dried up, and two-thirds smaller in size.

Dr. Alanson Abbe mentions the case of Dr. Capen, who had become gray, but, on recovery from disease, his hair became quite dark.

In the St. Louis Medical and Surgical Journal, 1845, p. 310, there is reported the case of an old man eighty-one years of age, robust and hale. His hair, from being perfectly white, became black, and the same of the beard. This man also presented the phenomena of second sight. He could read readily without glasses. The text-books on skin-diseases also mention cases. Several cases of sudden canities are referred to in Ziemssen.

Brown-Sequard, in his own person, noticed one day a white hair in his beard where there was none the day previous. He pulled it out, and the next day others appeared. This was observed repeatedly, and there was no doubt the hair in its entire length turned white in one night. Under the microscope these white hairs showed small air-bubbles in place of the normal pigment. In a case of hemiplegia the hair became white on the paralyzed side. The same has been reported in cases of neuralgia. Other anomalous cases have been noted where the hair became white in patches, and where individual hairs have been seen alternately white and black at different stages of its growth, to which condition Karsh and Landois have given the name of "ringed hair," and ascribed it to an intermittent trophic disease affecting the hair-follicle. Wilson mentions a case where the hair was gray in winter, and regained its normal color in summer.

Alibert and Beizel relate cases of women with blond hair which all came out after severe fever, and when new hair grew it was black. Alibert also relates the case of a young man who lost brown hair during illness, and that which replaced it was red.

In the case of an epileptic girl of idiotic type, with alternating phases of stupidity and excitement, during the stage of stupidity the hair was blond, during excitement it was red. This change of color took place in two or three days, the change always beginning at the ends of the hairs. Pale hairs showed an increased number of air-spaces. It has been frequently observed, that, when the hair changes color gradually, the change begins in the end, and extends toward the bulb. In conversation with an eminent ornithologist on the change of color in the plumage of birds, he said, "I have lately been watching hairs in my mustache turn gray, and they always begin at the ends, and it extends to the roots."

Speaking on the subject with a lady, she mentioned the case of the physician who attended her at the seashore last summer. The doctor's hair was long and quite gray. One day he came in to see her after having his hair cut, and she was surprised to notice that the gray hair had given place to black. Examination showed that his hair towards the ends had been white, and that nearer the skin black. The white portion had been removed by the cutting.

The cases here collected are only a few in comparison to what might be found; but they are sufficient to prove beyond all reasonable doubt that the hair does suddenly change color under certain circumstances, and that the change takes place in existing hairs.

. . . .

It is apparent, from what has been said, that there is very much concerning the changes of color of the hair and other appendages of the skin in man and the lower animals that is not understood. In its normal condition, the color of the hair is dependent upon the hair-bulb. It is here that the melanine is secreted from the coloring-matter of the blood; and from this point, as the hair grows, it permeates its cells, the intensity and shades, from black to blond, depending pincipally upon the amount of the coloring-matter. In black hair the hair-bulb is larger, contains a greater amount of melanine, and the hair itself is coarser and of more vigorous growth. In those cases where the hair has turned from white to black, and minute examination has been made, this has been found true.

In the case reported by Bruley, already referred to, of a woman aged sixty, whose hair, previously white, became jet-black four days before her death, the bulbs of the black hairs are described as being of immense size and engorged with dark pigment, while the roots of the white hairs that remained were dried up and two-thirds smaller in size. So, on the other hand, in change from dark to white, the hair is finer in texture, less vigorous in growth, and the hair-bulbs smaller.

The sudden change in canities, when due to violent emotions, can be explained in no other way than through the bulb. It is true that there is no direct vascular or nerve connection between the bulb and its hair after it emerges from the skin, but it is also undoubtedly true that there is communication by osmosis between the cells of the papilla and those of the shaft and dif-

ferent layers of the hair.

Wilson ascribes the cause of sudden whitening of hair to insufficient nutritive power of the skin, and also suggests that there may generate a gaseous fluid in the hair in place of its normal constituents. He says, further, that the fluids from the blood-vessels of the skin permeate the hair, and thus change in fluids may alter the color.

In all of the cases of sudden change to white, where the hair has been examined, the coloring matter has disappeared, and in its place is found an accumulation of minute air-globules. The same is true of gray hair of advancing age. How the air gets into the capillary structure has never been explained. Two possible explanations are offered: one is, that in the destruction of the coloring-matter a gaseous substance may be developed; the other is, that air may find entrance from without, through the sides or end of the hair. It is possible to suppose a condition of the bulb producing a vacuum in the hair-shaft that shall cause, by suction, a drawing-in of air. The view that the air finds entrance through the end of the hair is supported in the fact that the change of color begins at the extremity.

. . . . .

CHANGES IN THE COLOR OF THE HAIR

Anonymous; *Popular Science Monthly*, 27:138, 1885.

Cases of changes in the color of hair other than to gray are not uncommon. Workers in cobalt-mines and indigo-works sometimes have their hair turned blue, and workers in copper green, by deposition of coloring-matter upon it. This, however, is only a superficial coloring, and can be washed off. Prentiss records a case of a patient to whom muriate of pilocarpine was administered hypodermically whose hair was changed from light blonde to nearly jet-black, and his eyes from light blue to dark blue. These changes were due to increase of normal pigment. Hauptmann relates a case of a body exhumed twenty years after burial, the hair on which had changed from dark brown to red. Leonard cites a case in which, after death, red hair was changed to gray within thirty hours. Other cases have been mentioned in which the color of the hair has been variously changed in consequence of disease.

• Unusual Hair Growth after Death

GROWTH OF THE HUMAN HAIR AFTER DEATH

Anonymous; *English Mechanic,* 26:35, 1877.

Dr. Caldwell, of Iowa, states that in 1862 he was present at the exhumation of a body which had been buried two years before. The coffin had sprung open at the joints, and the hair protruded through the openings. On opening the coffin, the hair of the head was found to measure eighteen inches, the whiskers eight inches, and the hair on the breast five to six inches. The man had been shaved before being buried. In 1847, a similar circumstance occurred in Mercer county, Pa. In digging a grave, the workmen came upon the skeleton of a man that had been buried ten years. The hair was as firm as during life, and had grown to a length of eleven or twelve inches.

• Hair Patterns and the Aquatic Man Theory

WAS MAN MORE AQUATIC IN THE PAST?

Hardy, Alister; *New Scientist,* 7:642-645, 1960.

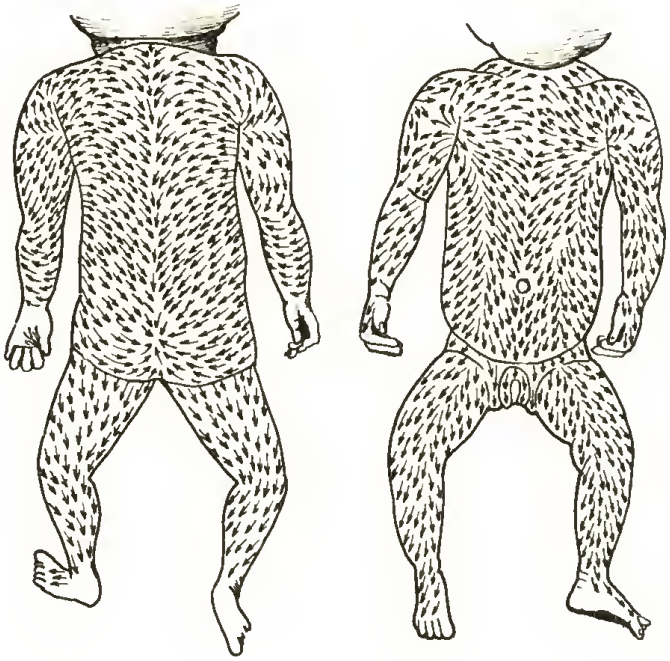
The suggestion I am about to make may at first seem far-fetched, yet I think it may best explain the striking physical differences that separate Man's immediate ancestors (the Hominidae) from the more apelike forms (Pongidae) which have each diverged from a common stock of more primitive ape-like creatures which had clearly developed for a time as tree-living forms.

My thesis is that a branch of this primitive ape-stock was forced by competition from life in the trees to feed on the sea-shores and to hunt for food, shell fish, sea-urchins, etc. in the shallow waters off the coast. I suppose that they were forced into the water just as we have seen happen in so many other groups of terrestrial animals. I am imagining this happening in the warmer parts of the world, in the tropical seas where Man could stand being in the water for relatively long periods, that is, several hours at a stretch. I imagine him wading, at first perhaps still crouching, almost on all fours groping about in the water, digging for shell fish, but becoming gradually more adept

at swimming. Then, in time, I see him becoming more and more of an aquatic animal going farther out from the shore; I see him diving for shell fish, prising out worms, burrowing crabs and bivalves from the sands at the bottom of shallow seas, and breaking open sea-urchins, and then, with increasing skill, capturing fish with his hands.

. . . . .

Whilst not invariably so, the loss of hair is characteristic of a number of aquatic mammals; for example, the whales, the Sirenia (that is, the dugongs and manatees) and the hippopotamus. Aquatic mammals which come out of the water in cold and temperate climates have retained their fur for warmth on land, as have the seals, otters, beavers, etc. Man has lost his hair all except on the head, that part of him sticking out of the water as he swims; such hair is possibly retained as a guard against the rays of the tropical sun, and its loss from the face of the female is, of course, the result of sexual selection. Actually the apparent hairlessness of Man is not always due to an absence of hair; in the white races it is more apparent than real in that the hairs are there but are small and exceedingly reduced in thickness; in some of the black races, however, the hairs have actually gone, but in either case the effect is the same: that of reducing the resistance of the body in swimming. Hair, under water, naturally



Hair tracts on a human foetus

loses its original function of keeping the body warm by acting as a poor heat conductor; that quality, of course, depends upon the air held stationary in the spaces between the hairs--the principle adopted in Aertex underwear. Actually the loss or reduction of hair in Man is an adaptation by the retention into adult life of an early embryonic condition; the unborn chimpanzee has hair on its head like Man, but little on its body.

Whilst discussing hair it is interesting to point out that what are called the "hair tracts"--the directions in which the hairs lie on different parts of the body--are different in Man from those in the apes; particularly to be noted are the hairs on the back, which are all pointing in lines to meet diagonally towards the midline, exactly as the streams of water would pass round the body and meet, when it is swimming forward like a frog. Such an arrangement of hair, offering less resistance, may have been a first step in aquatic adaptation before its loss.

The graceful shape of Man--or woman!--is most striking when compared with the clumsy form of the ape. All the curves of the human body have the beauty of a well-designed boat. Man is indeed streamlined.

These sweeping curves of the body are helped by the development of fat below the skin and, indeed, the presence of the subcutaneous fat is again a characteristic that distinguishes Man from the other primates. It was a note of this fact in the late Professor Wood Jones's book Man's Place among the Mammals (p. 309) that set me thinking of the possiblity of Man having a more aquatic past when I read it more than thirty years ago. I quote the paragraph as follows:

> "The peculiar relation of the skin to the underlying superficial fascia is a very real distinction, familiar enough to everyone who has repeatedly skinned both human subjects and any other members of the Primates. The bed of subcutaneous fat adherent to the skin, so conspicuous in Man, is possibly related to his apparent hair reduction; though it is difficult to see why, if no other factor is invoked, there should be such a basal difference between Man and the Chimpanzee."

. . . . .

• Some Respectable Human Tails

CAUDAL APPENDAGE IN MAN

Anonymous; *Scientific American*, 60:295-296, 1889.

Naturalists have up to the present time given little attention to the study of tailed men. Such an organ has simply aroused the curiousity of any one who has seen a specimen, quite as a bearded woman arouses interest in a dime museum. The works on this subject are very incomplete, a few lines here and there scarcely throwing any light on the subject. There is one monograph on this subject. There are few documents on the subject, the monstrosity is seldom found, and few of the well known cases could bear a close scrutiny.

It is not our intention to translate or give an abstract of this German production, but we have the good fortune to be able to reproduce an engraving from two very good photographs taken from life, and we shall accompany them with a few descriptive words.

A number of travelers in South Africa have testified to the existence of men of the Nyams-Nyams tribe who had tails. This may or may not be true. If such is the case, Mr. Quatrefages does not consider that the evidence to that end is very trustworthy, and rather comes to the conclusion that it has not been absolutely proved, although he believes the fact not improbable. Mr. Ecker, on the contrary, after examining this subject thinks that the testimony of explorers is credible.

Although there may not be a race of human monkeys, it must be admitted that there are some cases of individuals having a caudal appendage. When I say "some" cases, it should be understood that Pliny and Ptolemy, and after them Marco Polo, Strup Maillet, and others, speak of this anomaly, and their citations would comprise in all not 15 or 20 cases, but from 150 to 200. The well authenticated cases, however, are very few in number.

Bartels enumerates and describes twenty-one cases, which he classifies as: 1. Tail connected with body. 2. Tail with free movement. 3. Cutaneous prolongation. The first includes the triangular base bone unusually developed, and which extends to the anus and partially covers it. The second includes those tails that form a sort of projection at the back, and which separate from the body at the sacrum. The third will not be reviewed, as it does not seem necessary, as it could take either of the other forms mentioned and still be simply a cutaneous prolongation.

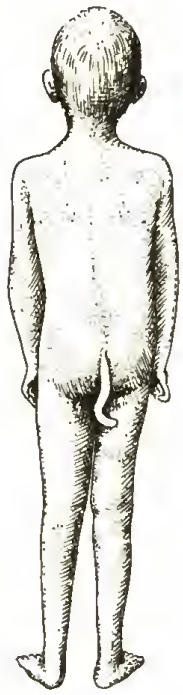
The specimens already observed enable us to give a general description, which will be done, however, with due caution. The tail is conical of spherical in shape, rarely cylindrical. The

end nearly always curls slightly and sometimes is twisted like that of a pig. It is seldom more than 3 or 4 inches in length. It is sometimes covered, and sometimes it is quite smooth. In the former case the hair grows in places and resembles somewhat the tail of a cow. Bartels cites and has shown a case of this kind, while Quatrefages, who lived before his time, had considered this condition as a mere legend.

Whether these tails are soft or rigid, they possess in general no movement of their own. This physical peculiarity is usually accompanied with some other defect, with atresia ani in particular, and almost always with general constitutional weakness. Such are the general characteristics of this class of monstrosities. Can the cause of this be discovered? Adhuc sub judice lis est. Bartels considers that it is due to imperfect development, induced perhaps by inheritance from an ancestral type, while Quatrefages had not believed in this, but considered that it was caused by some abnormal development in the embryo. It is not for us to decide which theory is to be accepted. It is a curious circumstance, however, that this peculiarity is found principally among the male species. I do not think that the females of the lower orders are less favored in this respect than the males. It is also curious that the white races appear not to be privileged in this respect. If we admit that they are a superior race to the others, then perhaps atavism would explain it. In none of the cases that have been examined, however, could vertebrae be found in the tail; while the specimen that was dissected by Virchow simply consisted of fat and muscles. Atavism would explain alone the existence of an hereditary monstrosity.

The question of heredity was, at an early day, discussed by Mr. De Quatrefages. He came to the conclusion that such an organ, whatever its origin might be, could be transmitted from father to son. Such a conclusion is affirmed in the cases of other monstrosities in which this is found to be the case. The subject of the engraving, however, which we have not as yet described, is a very remarkable case. He is a young Moi, twelve years of age, who was taken to Saigon some time ago, where he was examined and photographed. Although so young, his tail was already nearly a foot in length. With the exception of its great length, this specimen resembled in almost every other respect those that had been examined by Bartels. From the information we have been able to procure, it appears that the organ is soft and smooth and has no bony frame. Bartels cites only four cases of this kind, while its cylindrical shape appears to be very rare. We stated above that, as a rule, this peculiarity was accompanied with other malformations, and such is found to be the case in the present instance. By examining the photograph it will be observed that a small mound or bunch is found upon each buttock. The shoulder blades seem to project abnormally, but it should be borne in mind that the subject is very thin.

The tail seems ordinary and possesses none of the peculiari-



A Moi boy with a nine-inch tail

ties described by Mr. De Quatrefages. He describes tails two or three inches in length, while Bartels describes them as long as five inches, but this young Moi's tail is about ten inches in length. Therefore, up to the present time this is the best specimen on record. This matter ought not to be passed over carelessly, but it merits a thorough and complete investigation and study which should be no longer neglected.

A TAILED CHILD

Eaton, H. W.; *Science,* 3:673, 1884.

The Commercial of this city, Louisville, for the 17th and 18th inst. gave accounts of a tailed child recently born here. As such cases are of scientific interest, and are very rare, a party of four, including a prominent doctor and the writer, concluded to investigate the case.

We found a female negro-child, eight weeks old, normally

formed in all respects, except that slightly to the left of the median line, and about an inch above the lower end of the spinal column, is a fleshy pedunculated protuberance about two and one-half inches long. At the base it measures one and one-quarter inches in circumference. A quarter of an inch from the base it is somewhat larger, and from that it tapers gradually to a small blunt point. It closely resembles a pig's tail in shape, but shows no signs of bone or cartilage. There seems to be a slight mole-like protuberance at the point of attachment. The appendage has grown in length about a quarter of an inch since the birth of the child.

The mother, Lucy Clark, is a quadroon, seventeen years old, and the father, a negro of twenty,--both normally formed.

In Darwin's 'Descent of Man', Vol. i. p. 28, he speaks of a similar case, and refers to an article in Revue des cours scientifiques, 1867-68, p. 625. A more complete article is that by Dr. Max Bartels, in Archiv fur anthropologie for 1880. He describes twenty-one cases of persons born with tails, most of them being fleshy protuberances like the one just described.

SEVEN-INCH TAIL ON BABY

Anonymous; *Science News Letter,* 14:307-308, 1928.

A human tail of almost record-breaking length has just been discovered appended to a baby girl born at Knoxville, Tennessee, one of the states that outlaw evolution. This tail was reported to be seven inches long. The record is a nine-inch tail on a twelve-year-old boy from French Indo-China.

Only about twenty-five authentic cases of babies born with tails are known to science. However, every human being, including the late William Jennings Bryan, had a tail at an early stage of his life, stated Dr. Adolph H. Schultz, associate professor of physical anthropology at the Johns Hopkins University and research associate of the Carnegie Institution of Washington. Dr. Schultz has asked that the unique appendage be sent him for study.

Before birth, when man is in the embryo stage of his life, he has a tail one-sixth the length of his body. Generally this tail disappears before birth, though the rudiments of the vertebrae in it may be found in man's spinal column, where they are known as the small bones of the coccyz at the end of the spine.

Man's evolutionary relatives, the higher apes, have even less of a tail than man himself, Dr. Schultz said. In the orang-outan the embryonic tail disappears more completely, leaving only two or three rudimentary tail vertebrae. In man there are four or five of these and sometimes six at the base of the spine.

When the tail persists externally, it never has any bones but is made up of nerves, blood vessels and muscles. In man and apes it is evidence of evolution from a tailed ancestor.

• Horned Humans

A WOMAN WITH HORNS

Anonymous; *Scientific American,* 10:323, 1864.

The New York Observer of the 12th inst., contains a letter from its correspondent at Larnaca, in the Island of Cyprus (Turkish dominions), describes a most remarkable lusus naturae recently discovered there. It is nothing less than a woman with horns growing out of her head! She has one large horn on the side of her head of the size and consistancy of an ordinary ram's horn, besides three or four cornicles on other parts of the head. The writer states that he has seen her and that she has been visited by nearly all the Consuls and Europeans in that place, some of whom are making an effort to secure her for exhibition.

HORNED MEN IN AFRICA

Anonymous; *English Mechanic,* 24:9, 1876.

Captain Cameron read a portion of a paper, written by Captain J.S. Hay, relating to a strange malformation in the males of a tribe of people he had visited in the district of Akem, in West Africa. The extract was as follows:--"Two diagrams which are laid before you depict to the best of my ability, and will at least convey some idea of, a malformation which is, I believe, peculiar to this tribe. The malformation in question is confined to the male sex, and consists in a protuberance or enlargement of the cheek bones under the eyes, which takes the form of horns on each side of the nose. This malformation begins in childhood, but is not so far as I am aware, hereditary. It presents no appearance of being a diseased structure, nor is it a raised cicatrice after the fashion adopted by many African tribes. On the contrary, I have seen children with this peculiarity of structure whose parents were doing their utmost (though ineffectually) to stop it by medicines and applications."

HORNS IN GIRL'S NECK FROM EMBRYO GILLS

Anonymous; *Science News Letter,* 68:231, 1955.

The case of a nine-year-old girl who had "horns" on both

sides of her neck was reported by Dr. Verner V. Lindgren of the University of Oregon Medical School, Portland, at the American Society of Plastic and Reconstructive Surgery meeting in Atlantic City, N.J.

The horns are probably remnants of gills. During the early stages of pre-birth development, human babies have gills like a fish. Sometimes when these gills disappear, as the embryo baby develops, cysts develop where the gills were. When the cysts remain after the baby is born, they look like small horns.

The little girl reported on had such horns, each about a half inch long. Dr. Lindgren removed them.

Five generations of this child's family, Dr. Lindgren said, were born with similar horns. The youngster's mother had them, as did eight other family members, all of them female except two. The nine-year-old and one other member of her family had horns on both sides of the neck. In other cases, the condition appeared on one side only.

• Human Gills and Recapitulation in Man

TRACES OF GILLS IN THE THROAT OF MAN

R., A. C.; *Knowledge,* 13:149–150, 1890.

It is well known that many parts are present in the embryo which are not represented in the mature animal, and that when parts are precociously developed in the embryo and but feebly developed in the adult it indicates that they were of high importance to the early ancestors of the animal. Embryology furnishes abundant evidence supporting the view that the ancestors of existing vertebrata were aquatic in their habits, that respiration was carried on in them by means of gills, and that many structural peculiarities in mammals result from the transformation of an aquatic into a terrestrial animal. Mr. Bland Sutton says that the type of respiratory organs in these ancestral forms is to be seen in such fish as the dog-fish and the lamprey. Water, charged with air, enters the mouth and is forced through openings in the walls of the pharynx. The pharyngeal orifices, or branchial slits, are furnished with vascular processes known as gills. In the gills, or branchiae, the blood and water are merely separated from each other by an extremely delicate layer of tissue. Hence venour blood circulating in the gills readily gives up the excess of carbon dioxide, and as readily obtains oxygen from the surrounding water. The gills of fish and batrachians are supported upon a cartilaginous or bony framework known as the branchial bars; and in such fish as sharks a small cutaneous

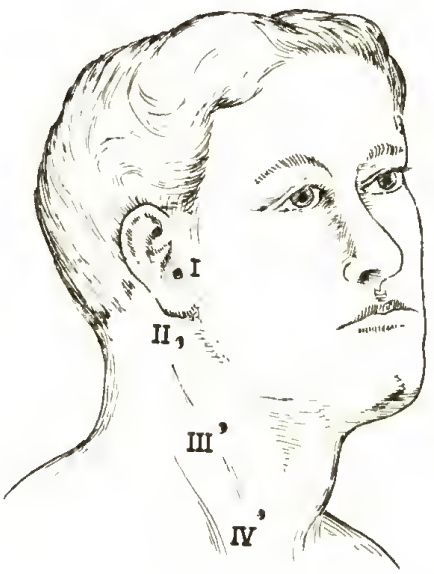
fold projects from each bar and covers the gill-slit as with a lid. These cutaneous lids are named, in consequence, opercula.

. . . . .

The first slit bears no gills in the adult fish, and is known as the spiracle or blow-hole. In the embryo it is furnished with beautiful external delicate vascular tufts.

The neck of a mammalian embryo is furnished with four similar slit-like orifices, communicating with the pharynx as in the dog-fish, and though they never support gills they are furnished with a small swelling or tubercle, representing the operculum. In the human embryo four branchial slits present themselves. The first of these represents the spiracle of the shark, and becomes the tympano-eustachian passage subservient to the sense of hearing. The small tubercles surmounting it coalesce, and gradually give rise to the pinna or external ear. In cases of normal development the posterior gill-slits disappear; but it is by no means uncommon to find in the sides of the neck of a child, along the anterior border of the sterno-mastoid muscle, small openings in the skin capable of admitting a thin probe. These congenital fistulae, especially when they exist in the upper part of the neck, communicate with the pharynx. This in some cases may be demonstrated by allowing the child to swallow milk, when drops of the milk will find their way through the fistula and appear in the neck.

. . . . .



The four common locations of branchial fistulae

OBSESSIONS OF BIOLOGISTS

Dewar, Douglas, and Davies, L. M.; *Nineteenth Century*, 136:19-27, 1944, and 137:78-86, 1945.

II. That Man Is Descended From Fish. This obsession is quite as general as the last. Haeckel is mainly responsible for its popularity. He had the effrontery to proclaim it to be a "Biogenetic Law" that: "The history of the foetus is a recapitulation of the history of the race." (The Evolution of Man, Eng. tr., 1906, p.2). As the evolution theory asserts that men are descended from fishes, the human embryo must, according to this law, pass through a fish stage; and Haeckel claims that it does so. This claim is mainly supported by the fact that the embryo, although never generally resembling a fish (or any other adult), does at an early stage exhibit what some regard as gill clefts, which are never functional in man and land animals.

> The fact [he says] that they are found in the same form as in the fishes is one of the most interesting proofs of the descent of these three higher classes (reptiles, birds and mammals) from the fishes." (ibid., p. 110).

This has since been repeated parrot-like by hundreds of biologists. Thus Professor Munro Fox, F.R.S., writes:

> The embryos of both birds and mammals, including man himself, have gill slits....This means that land vertebrates once had fish-like ancestors." (Biology; An Introduction to the study of Life, 1934, p. 303).

Similarly H.G. Wells, G.P. Wells and Julian S. Huxley write:

> The early embryos of man, cat, hen and snake...are alike ...in having their heart, main arteries and neck region built on the same plan as in fish...This means nothing, indeed makes nonsense, if we are to believe that land animals were created as land animals. But it at once becomes pregnant with meaning if we accept the fact of evolution, for then we can understand that snakes and hens and human beings and all other air-breathing vertebrates are fundamentally fish-like, that they start on the fishward road and turn away from it towards their higher structural achievement. When they reproduce the old disposition asserts itself; they start towards the old water way and then turn aside towards the uplands." (The Science of Life, p. 368).

Unfortunately for our propagandists, at no stage is the embryo of a land animal fish-like: at no stage does it have gill clefts or slits in the wall of the alimentary canal. These clefts exist only in the imagination of those obsessed by the notion of a fish ancestry. In truth, the developing embryo of a land animal takes the shortest possible course to its final form. It never seems to occur to those who are dominated by the fish-ancestor idea, that

during the whole period of development every cell of the embryo needs oxygen, also nutrition, and also a means of ridding itself of waste products while nourishment is being converted into tissue. Thus these worthies, under the blinding effects of their fixed idea, ignore the first principles of embryonic development! Before the human embryo is an inch long, it requires a heart and blood-vessels. Do evolutionists imagine that it would be possible, in the first sixteen days of its existence, for it to develop a complicated heart like that of man? Since this is physically impossible, a simple tubular heart---such as suffices for a simple animal like a fish---is quickly developed ventrally to the alimentary canal. Then the artery issuing from it, with the surrounding tissues, forms the foremost visceral arch; and so, without piercing the alimentary canal, reaches the upper part of the body, and then bends back and conveys blood to the middle and hind regions. Then, as more blood is required, more of these visceral arches are formed in quick succession.

They are called arches, because each arches the very thin wall of the alimentary canal---both inside and out. They are close together, and the very small space between each forms a furrow, the bottom of which is furnished by the thin wall of the alimentary canal. It is these visceral furrows or grooves that Haeckel and his dupes call 'gill-slits' or 'branchial-clefts.' They are, of course, nothing of the sort, and have no respiratory function. They are absolutely necessary for the development of every vertebrate embryo, whether of fish or land animal; and that is why they appear. The subsequent development of these visceral arches depends on the type of animal concerned; if it be a fish, they develop into gills and gill-slits; in land animals they never do this, but take a different course, varying with the class to which the embryo belongs. In the case of man (as can be ascertained by consulting a book on human anatomy) these arches and furrows give rise to the lower jaw and part of the face, ear, tongue, neck, etc., such as the eustachian tube and the thymus gland.

In the embryos of reptiles, birds and mammals there are no clefts between the arches; and the blood vessels in the arches do not split into two. It is therefore incorrect, and most misleading, to call them 'gill' arches in these embryos, or to call the grooves between them 'clefts' or 'slits.' (pp. 25-27)

• Possible Inheritance of Physical Injuries

INHERITANCE OF PHYSICAL INJURIES

Bishop, Irving P.; *Science,* 3:144, 1884.

Well-authenticated instances of the inheritance of a physical injury are so rare, that I wish to put upon record one which has recently fallen under my observation. A gentleman, when a boy about seven years of age, had the second toe of the right foot deformed by wearing a tight boot. The first and third toes were crowded together, forcing the second one under and backwards, and causing a curvature of the second joint, which, in time, became permanent. The joint, being somewhat elevated above those of the other toes, received the pressure of the shoe, and always after was more or less troublesome in consequence. The gentleman was twice married. By his first wife he had six children, the second of which was a daughter; the rest, sons. The daughter inherited the crooked toe; but the feet of all the sons were normal. The deformity appeared, however, in the son of one of these,--the brother next younger than the sister,--affecting the same foot and toe as on the grandfather. By his second wife the gentleman had only one child, a son, who also inherited the peculiarity; but in this instance it was the second toe of the left foot, instead of the right, that was affected.

Knowing that much doubt still exists whether the results of a slight physical injury, like the one I have described, are ever transmitted, I have taken pains to examine carefully all the evidence under my observation; and I feel assured of its correctness. All four having the deformed toes are now living, and all agree upon the facts. The gentleman is positive that his feet were normal until he was about seven years old, and says he remembers very distinctly wearing the boots which caused the deformity. An examination of the foot does not show any congenital peculiarity which might have been transmitted. The toe, when restored to its correct position, appeared normal in every way. No peculiarity of this kind has ever appeared in any other of the gentleman's relatives. I can see no way, then, of avoiding the conclusion that the injury, or rather its results, have been transmitted to two generations.

The case presents some features which render it especially interesting. The peculiarity's appearance in the children of both wives seems to eliminate altogether the element of the mother's influence in producing it. The recurrence of the variation in the grandchild, the father being normal, indicates how powerful was the tendency to perpetuate this slight deviation from nature's standard. In the other cases which I have studied personally, if a variation did not appear in a child, that child's children were free from it also. I should be glad to know if any one of your readers has observed this tendency toward reverting to the an-

cestral type under similar circumstances.

. . . .

NATURAL EAR-RING HOLES FOUND IN TWO FAMILIES

Anonymous; *Science News Letter,* 39:99, 1941.

Holes in earlobes, looking as if pierced for ear-rings but actually "birthmarks," are described in a report sent to the American Genetics Association by Dr. Henry W. Edmonds of Children's Hospital, Boston, and Dr. Clyde E. Keeler of the Wistar Institute of Anatomy and Biology, Philadelphia. (Journal of Heredity, December.)

The holes do not go all the way through the ear, but consist of deep, narrow pits. In some individuals they start from both sides of the earlobe and nearly meet in the middle; in others, they are on only one side of the lobe.

A naive explanation would be that these natural "piercings" are inherited from a long line of ancestors who have had their ears pierced for ear-rings. A more exact explanation is that they are hereditary, all right, but are not due to any artificial treatment of ancestors.

They have been found in two wholly unrelated family lines, one Italian, the other Russian, identified in the report only as the "Bo" and the "Kr" families. The "Bo" family started with grandparents both of whom had the peculiar markings. All their children (three sons, one daughter) were similarly marked, as were three granddaughters. Three grandsons do not have the natural "ear-ring holes."

The "Kr" family shows one great-uncle who had a deformed ear, although the pittings were lacking. All other members of first and second generations had normal ears so far as known. Then, in the third generation, four out of nineteen members of the cousinship have the natural piercings, and there are in addition two others who have ears otherwise abnormal.

. . . .

• Skin Shedding and Other Skin Anomalies

MAN SHEDS SKIN YEARLY

Anonymous; *Science*, 18:74, 1891.

At a recent meeting of the Chicago Medical Society, says the Medical Record, Dr. J. Frank reported a case where a man every

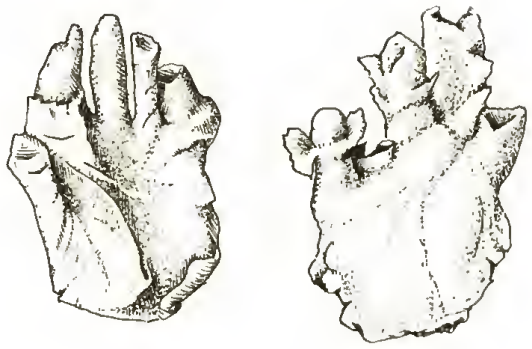
July shed his skin. He was taken with feverish tremors, increasing almost to paroxysms. He undressed, lay down, and within a few minutes the skin of the chest began to turn red. The redness rapidly extended over the entire skin, and the feverish tremors continued uninterrupted for about twelve hours. Then he arose, dressed, and walked about in perfect health. The skin now commenced to peel, and ten hours later it began to come off in great patches. From the arms and legs it could be pulled off exactly like gloves or stockings. As the old skin came away a new epidermis, as soft and pink as a baby's, was revealed. This new skin was very sensitive; the patient has to wear softened gloves and moccasins for about a week. After the old cuticle had been entirely removed the finger and toe nails began to drop off--new nails literally crowding them out. Finally the change was complete, the man had a new skin and a new outfit of nails, and was ready to return to the mines. The shedding began in his first year and recurred every July thereafter.

A CURIOUS CASE OF SKIN-SHEDDING

Anonymous; *Scientific American,* 83:100, 1900.

A resident of Clark County, Missouri, Mr. S.O. Buskirk, has shed his skin annually since his birth, which occurred in 1850. He is well-built, robust and agile, and was never ill. He takes very little medicine for the annual attacks when his epidermis is shed. Physicians have tried to prevent this exfoliation, but they have been unsuccessful, and Mr. Buskirk, not withstanding the fact that the operation is disagreeable and debilitating, has decided that he will not make any more attempts to prevent it by medical means. The operation requires several days, and for the last five years has begun exactly on June 27. Prior to that time it came either in July or August. About a month is consumed in discarding the old cuticle and the appearance of the new. During this time the finger and toe nails become loose and are discarded, new nails come in, and more time is required in growing the nails to maturity than is ordinarily needed. After the old skin has been shed he says that he feels like a boy of eighteen. The discarded cuticle looks like thin, white rubber gloves.

Our photographs show the remarkable peeling of the hands. The peeling begins at the root of the fingers and gradually spreads in all directions. The hardened skin begins to break away from the new skin which is forming underneath, and if it is tapped with a lead pencil it gives off a sharp sound like caused by striking a piece of celluloid or stiff leather. By opening and closing the hands often the skin parts along the edge, and then by helping along the process with a penknife, the piece comes off whole. He has several interesting souvenirs in the form of patches of skin which he has shed from his hands and feet at various times, and in every instance they present



perfect outlines of the members from which they come.

The thick, callous-like skin from the hands retains its lines, and this remarkable case tends to disprove the entire theory of palmistry, being evidence that the lines of the hands change with time and are not unalterably preserved, as has been supposed. A piece of the skin taken from his right hand when he was ten years old shows that the general conformation of the lines correspond with those of his hand to-day. Still, the new lines are longer now than then, making allowance for the growth of the member as a whole. Fully a third has been attached to the length of the famous "life" line. Evidently nature had decided to increase Mr. Burkirk's span of life. This is not altogether surprising, as his father is now one hundred and three and his grandfather died at the age of one hundred and ten years.

PORCUPINE MEN

Anonymous; *Nature,* 133:185–186, 1934.

During January 1834, a middle-aged man, of very athletic and robust form of body, completely covered with a green horny substance in the form of quills, not dissimilar to those which are produced on the porcupine, presented himself at the Westminster Hospital for exhibition. The parts which had escaped the deformity were his face, the palms of his hands and the soles of his feet; every other part of his person was abundantly supplied with this green horny substance. He stated that he shed his horns, or quills, annually, and a fresh crop succeeded.

A description of the case appears in the London Medical and Surgical Journal of February 6, 1834. The man was a member of the celebrated Lambert family, in which this remarkable condition, an extraordinarily scarce form of the skin disease named ichthyosis hystrix, was present in at least six generations. In every case the condition appeared about two months after birth

and affected the males only. The case of the first member of the Lambert family to be affected was reported to the Royal Society on March 16, 1731, by John Machin, the secretary, and Prof. Gresh (Phil. Trans., 38, 299; 1731) at fourteen years of age, and in 1755 at the age of thirty-eight with his son Edward by Baker (ibid., 49, 21; 1755). Edward and his two sons, who all presented a similar skin condition, visited Germany and France, where they were described under the name of "Porcupine Men" by Blumenbach, Autenrieth and Tilesius. Other members of the family similarly affected were afterwards described by Elliotson in 1831, Pettigrew in 1834 (in the subject of this note) and by Pickells in 1851. Further details concerning the Lambert Family, including a reproduction of the figure published in 1802 by Tilesius, will be found in E. A. Cockayne's "Inherited Anomalies of the Skin and its Appendages" (1933), pp. 182-85, from which most of the above information is taken.

• Dwarfs and Giants: Some Extreme Cases

A REMARKABLE DWARF CHILD

Anonymous; *English Mechanic*, 33:84, 1881.

There is now being exhibited in Paris, we learn from La Nature, a child of remarkably small size. She is only 38 centimetres, say 15 in. in height. In a representation given of her, she stands on a table, and appears only a little taller than an ordinary quart bottle on the table. True, she is only four years old, and it is probable that she will yet grow a little. But a new-born child, of the average length of 50 centimetres (20 in.) would be almost a giant in comparison.

She is being exhibited at the Cirque Fernando. There she walks along the galleries, where all may see her, and may speak with her (the latter at least on the rather difficult condition of being able to speak Dutch, her native tongue). She was born on the frontier of Belgium and Holland, a few kilometres from Antwerp.

M. Bertillon gives a few further particulars in our contemporary. Her head, though small, is large for her poor little body. She has a very small pale face, with the lines already formed. Her blue eyes are not without intelligence. She has a good crop of fine blonde hair. Her teeth are in very good condition. Her cephalic index is about 80.8. She is very restless, though her walk is difficult and embarrassed. She speaks often, in a thin voice, and she is fond of the amusements of her age. Consider-

ing her bad walk, M. Bertillon thought at first she was deformed. It appears that her vertebral column is normal, but her small thin legs lack supporting force, so that they are always bent in walking. It is only 14 months ago that she became able to walk alone.

Exact measurements of the child are shortly to be published in the Bulletin of the Societe d'Anthropologie. The calf of the leg at its thickest part is 9.8 ctm. in circumference. If we remember that the ring of an average man is about 7 ctm. in circumference, we shall be able to appreciate the thinness of this little leg. The limbs are pretty well shaped. The feet are somewhat turned outwards.

The Princess Paulina, as she has been named, is the seventh child of her parents, who, it seems, are expecting an eighth. Her brothers and sisters are of normal size. She was born at the ordinary term, and was then extraordinary for her small size. She had not grown from birth until quite lately, when she gained two finger-widths in height. Her weight is six pounds and a half.

She enjoys good health, and sleeps ten or eleven hours at night; but her little heart beats with great rapidity. As she is constantly restless, it is very difficult to ascertain her pulse. Applying his ear to her chest, M. Bertillon counted a hundred pulsations per minute. In new-born infants the number ordinarily reaches 120. It is remarkable that she has passed through an attack of whooping cough.

The general aspect of this child suggests that of athreptic children, who do not benefit by what they eat. Her sorry exterior is happily counterbalanced by a pretty mobile and lively physiognomy. Someone having given her a doll, she embraced it with transport, and seemed delighted to possess it.

ACCOUNT OF PATAGONIAN GIANTS

Gould, George M., and Pyle, Walter L.; *Anomalies and Curiosities of Medicine,* Bell Publishing Co., New York, 1956, p. 325.

In the olden times there were extraordinary stories of the giants who lived in Patagonia. Some say that Magellan gave the name to this country because its inhabitants measured 5 cubits. The naturalist Turner says that on the river Plata near the Brazilian coast he saw naked savages 12 feet high; and in his description of America, Thevenot confirms this by saying that on the coast of Africa he saw on a boat the skeleton of an American giant who had died in 1559, and who was 11 feet 5 inches in height. He claims to have measured the bones himself. He says that the bones of the leg measured 3 feet 4 inches, and the skull was 3 feet and 1 inch, just about the size of the skull of Borghini, who, however, was only of ordinary height. In his account of a voyage to the Straits of Magellan, Jacob Lemaire says that on December 17, 1615, he found at Port Desire several graves cover-

ed with stones, and beneath the stones were skeletons of men which measured between 10 and 11 feet. The ancient idea of the Spaniards was that the men of Patagonia were so tall that the Spanish soldiers could pass under their arms held out straight; yet we know that the Patagonians exhibit no exaggeration of height---in fact, some of the inhabitants about Terra del Fuego are rather diminutive. This superstition of the voyagers was not limited to America; there were accounts of men in the neighborhood of the Peak of Teneriffe who had 80 teeth in their head and bodies 15 feet in height. (p. 325)

THE COUNTRY OF THE GIANTS

Anonymous; *New Scientist,* 3:11, April 24, 1958.

The first giant form of man was identified by Weidenreich from teeth found in Chinese apothecary shops. They were ground up for use in various medicines. These remains of Asian Gigantopithecus were impossible to date and caused considerable scientific controversy.

Leakey has now found a human molar in association with other giant forms on Chellean living floors at Olduvai. These early men apparently lived on the bank above a small stream and threw their refuse in it. The living site shows no sign of the use of fire, but numerous bones had been broken for their marrow.

The fauna represented is nearly all extinct and shows a high proportion of giganticism among the herbivores. A sheep (Pelorovis) had a horn spread of no less than twelve feet. There were also remains of a giant ox and of two giant pigs of the size of a hippopotamus, with teeth like normal elephant tusks. Carnivores and rodents were about the same size as today.

The outsize human tooth found among these remains is clearly a milk molar. The question is, Did it belong to a Chellean man or was it the remains of some other species of man hunted by the Chelleans for food? Dr. Leakey leans towards the former view and thinks the Chelleans may have been giants. But since most of the animal remains found at the site were immature, probably because they were easier to kill, the fact that the tooth comes from a giant child might mean that it, too, was caught and eaten for food.

All contemporary men belong to one species, but at this early period there were probably several species of man, one of which may have preyed on another. Dr. Leakey's confirmation of the existence of giant species raises once again the puzzling question of the general relation of giganticism to the evolution of the mammalian and primate forms.

• A Case of Extremely Rapid Growth

A FAST-GROWING CHILD

Anonymous; *Scientific American,* 2:405, 1847.

The French Courier says that Mr. Glatz, one of its subscribers in this City, has a child which offers a singular example of the caprices of nature. It is hardly twenty-two months old, and is thirty-nine inches in height. Recently it grew five inches in three days, and then after twelve days' intermission it grew three inches more in a week. As the Courier is told by M. Legrand, the French physician who attends it, it is the most curious phenomenon ever observed: the most rapid growth known before being three inches in eight days. We add with regret that this abnormal development has exhausted the strength of the child, which is reduced to such a state that the circulation of the blood can be seen in its veins. It lives only by the nursing it receives, and they despair of raising it.

• Prehensile Properties of Human Feet

THE PREHENSILE FOOT OF EAST INDIANS

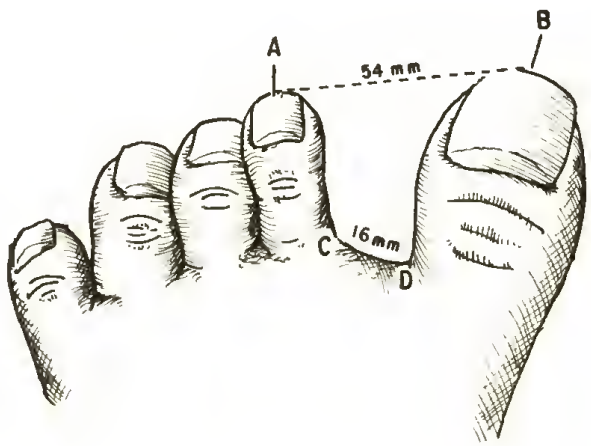
Regnault, M. F.; *Popular Science Monthly,* 41:489–495, 1892.

The traveler who walks in the native quarters of the cities of India can easily study there all industries in their beginnings, as they were probably practiced in Europe in the middle ages. The shops are usually open, and the workmen can be seen inside; textile industries, pottery, shoemaking, joinering, armoring, jewelry, confectioners--all can be observed in a single street like Chitpore Street, Calcutta. If we take pains to examine attentively the methods of working, we shall be struck by the enormous function played by the lower limb. Whatever the industries, the Indian, squatting or sitting on the ground, works with his feet as well as with his hands; and it might be said that all four of his limbs are in constant exercise. The joiner, for example, has no assistant to hold his plank, but makes his great toe serve that purpose. The shoemaker does not employ a fixed clamp for the

shoe on which he is sewing, but holds it in his feet, which change position to suit his convenience, while his nimble hands do the sewing. The metal-worker holds the joint of his shears on his feet in cutting copper.

In the making of wooden combs I have seen the comb held straight up by the feet, while the workmen marked the teeth with one hand and with the other directed the instrument that cut them. The wood-turner directs the hand-rest with his great toes; so, generally, do Egyptian and Arabian turners. In smoothing twine or sewing a bridle the Indians hold the article between the first and second toes. When the butcher cuts his meat into small pieces, he holds his knife between the first and second toes, takes the meat in both hands, and pulls it up across the knife. I have seen a child climb a tree and hold a branch between his toes. These are enough details concerning the constant, universal use of the foot.

In considering this property of the lower limb, it is well to distinguish between the parts that relate, first, to the articulation of the hip, which, being very loose, permits the Indian to squat in such a position that his foot shall not be very far from his hands, so as to make all four participate in the work and permit the whole lower limb to engage in wide movements. The position is very different from that of our tailors or of the Arabs. It brings the knees to a level with the chest. The man is supported on his ischia and his feet; and he keeps in this position for whole hours, while we can maintain it only for a few minutes. It is their way of resting, and we can see them by groups squatting in this manner, and smoking. In the second place, the articulation of the instep and the medio-tarsal permit wide lateral movements of the foot, as in the examples of the shoemaker, joiner,



Sketch showing the enormous distance between the first and second toes of a Tamil

comb-maker, and turner; and on the toes, which are peculiarly flexible, as with the butcher cutting meat and the child climbing trees.

The great toe is capable of considerable lateral movements from the second toe, so that the Indian can easily pick up articles from the ground with his foot, and even exert some force sidewise.

But great as is their skill, there is no movement of opposition between the great toe and the other toes, as there is in the monkey. The great toe has very extended movements of adduction and abduction, and of elevation and depression, but all is limited. The property is frequent among savages and half-civilized peoples. Broca pointed out in 1869 the part which the foot could be made to serve. Morice has remarked that the great toe of the Annamites could be used by them in picking up small objects; and he saw a boatman take his hand from the helm and steer very correctly with his foot, while he rolled his cigarette.

. . . .

The separation of the great toe at the base is not special to the East Indians. M. Manouvrier has reproduced it on two drawings of the feet of Caribs on exhibition in the Jardin d'acclimatation; but this author has not observed that the foot has any special part with this people as an organ of prehension. Among the numerous casts of feet in the museum of the Societe d'Anthropologie are some very interesting impressions of the feet of Annamites, presented by M. Mondiere. The separation on the foot of one of these Annamites, named Van, is very marked. It measures twelve millimetres at the base of the first and second toes, and forty-one millimetres, taking the middles of the nails as points of measurement, at the tips. On the impression of the foot of another Annamite, named Thi-Finhi, the separation is less notable, but is still four millimetres at the base and forty-one millimetres at the ends; while a third impression, still by M. Mondiere, shows a still different degree of separation. This separation has been noticed frequently among the Annamites, as well as the prehensile faculty of the foot. They therefore enjoy that property in common with the Indians.

It does not follow, however, that this faculty is common to all peoples that go barefooted, or even to all savages. There are at the museum castings of three feet of negroes in which nothing like it appears; an American Indian foot from the lower Amazon, the gift of Dr. Crevaux, also normal; two feet of young Bushmen, normal likewise; and thirteen feet of Fuegian men and women, normal. Only in the cast of the right foot of one young man did I find a separation of four millimetres at the base of the first two toes. The Ecole d' Anthropologie has several traces of feet taken by M. Manouvrier among Fuegians, Araucanians, Omahas, and Arabs of Algeria and Morocco, in none of which is there any example of this anatomical peculiarity. I have not observed it in any European or in any white child. The habit of walking barefooted may produce a slight divergence of the great toe, but not at the base. The function of prehensibility must therefore be considerably developed for such a divergence to exist. Still,

heredity appears to have a part in it; for we do not observe it except among peoples who have exercised the function from a remote antiquity. It would be interesting to dissect a foot presenting this formation and compare it with the foot of a white. We should most likely find the oblique and transverse abductor muscles very highly developed. It is a current fact that exercise strengthens the muscles. It would also be desirable to learn the origin of the separation at the base of the first and second toes. It can not be caused, as in the monkey, by the head of the first metatarsal playing on that of the second, for there is no movement of opposition here. It all takes place in the metatarso-phalangeal articulation. M. Testut, in a work on the Quaternary skeleton of la Chancelade, remarks that the anterior articular surfaces of the metatarsi which are destined for the phalanges are more extended, in length as well as in breadth, than those which have been observed on the metatarsi of European races. Unfortunately, we have data only for the articular surfaces of the last four metatarsi--the first, the one that interests us, having probably been suppressed. M. Testut concludes that this disposition is related to the mobility of the toes on the metatarsus--a mobility which has probably been considerably diminished in man since he has made his foot exclusively an organ of support. Whether the skeleton of the Indian is like this, and whether the separation of the base of the toes can be explained in this way, suggest hypotheses which dissection alone can verify.

The examination of the prehensile foot suggests forcibly the thought of comparing it with the foot of the monkey. The difference between the opposable foot of the monkey and the foot of man has been variously explained. The non-transformists base upon it an argument against the application of the transformist theory to man. Some Darwinians believe that if man used his foot constantly and generally as a prehensile organ, an opposition of the great toe would be gradually evolved in the adaptation of the organ to that function. The preceding study, however, proves that this is not the fact. Among a people who have for centuries commonly used their feet as a prehensile organ no movement of opposition has been produced; while in some persons an adaptation to the new function has been observed, namely, a separation of the great toe and wide and strong lateral movements, but only lateral--a pincers-foot, not a hand-foot. It will be seen, on reflection, that the condition could not be otherwise.

In walking, the weight of the body is borne on the heads of the five metatarsi, but mostly on the head of the first one. If that was not united solidly to the second metatarsus, and could turn around it as is done in the hand, it would give way every time the foot touched the ground, and the foot would want a sufficient internal point of support; walking would still be possible, but it would be hard and laborious--occasional, and not a habitual normal act. It is thus with the monkey, which is supported solely on the outer edge of the foot. Even the anthropoid walks rarely and awkwardly; its foot, adapted to living in the woods, has the opposition movement convenient for climbing easily; it has a foot-hand. The man who, continuing to walk, likewise

wants a prehensile foot, can not enjoy this movement, which is incompatible with walking. He satisfies himself with lateral movements between the great toe and the second toe, or a pincers-foot. All this is simply a consequence of the general biological law of the adaptation of the organ to the function.

• Some Skeletal Curiosities

A BONE SHEDDER

Anonymous; *Scientific American*, 65:132, 1891.

Dr. Bell, of Parrottsville, Tenn., reported the case of a woman who sheds her bones, and showed some of the specimens before the East Tennessee Medical Society (Medical Standard). He said: "The patient is seventy-one years of age, seemingly in perfect health, a well preserved woman of medium height, average weight, and normal in every other respect. Twenty-one years ago the exfoliation of bone began in her fingers, and has during the succeeding years continued until she has twice shed ulna and radius, humerus, scapula, and part of inferior maxillary. This shedding takes place spontaneously without pain, hemorrhage, suppuration, inflammation, or inconvenience. On one occasion when churning she shed the radius. There is no deformity, sapination, pronation, extension, flexion, and circumflexion being perfect. The bones shed (about six hundred pieces) were, on careful inspection by the society, found to be entirely natural. She has given about one hundred pieces of bone away as souvenirs. The woman is conscious of the pending expulsion of a bone about ten minutes before it takes place, and a perfect bone is always left in its stead. The bone makes its way out, always on the posterior side, and the wound heals by first intention, though at the 'exit of the bones' were numerous small scars. She has always been in comfortable circumstances and is cheerful, a very interesting fact, as showing the power of the mind to adapt itself to extraordinary circumstances."

BONY GROWTHS IN THE HUMAN JAW

Anonymous; *Nature*, 146:524, 1940.

Certain secondary formations on the human jaw were first reported in 1884 by Danielli. Though noted repeatedly since by

various scientific workers, they are still far from being generally known and understood. Ales Hrdlicka, in a recent discussion of the condition (Amer. J. Phys. Anthrop., 27,1; 1940), has reviewed and analysed recorded observations and has added to them new data based upon an examination of 5,632 lower jaws--cases which offered no possible doubt as to the nature of the formation. These hyperostoses consist of various grades and forms of supplementary hard bony tissue above the mylo-hyoid line on the lingual surface of the mandible. They have no connexion with anything pathological, though they may, when over-developed, cause trouble mechanically. They do not as a rule develop in the higher apes. In man they occur sporadically from palaeolithic times--they are reported in Sinanthropus--are more frequent in the neolithic period, and later grow more or less common in various human groups. They do not show any clear racial selectiveness, but are more common in the Yellow-Browns than in White or Black. This preponderance, however, appears more regional than racial. It is most marked in the northernmost or cold, least marked in the southern or warm, regions. A remarkable and instructive difference is seen to exist between the North American and the Old Peruvian Indians; in the latter the hyperostoses are almost wanting, while in the North Americans they are four times in number and even more in weight. The evidence furnishes a strong indication that the mandibular hyperostoses are neither of phylogenetic transmission nor show any plain racial heredity. Apparently they are brought about by environmental conditions, which can only mean food, and hence mastication. They tend to be both more frequent, and on the whole more strongly developed in the males. It is impossible to reach any conclusion other than that they are caused by stresses of mastication in excess of the capacity of the individual bones, and that they are the efforts of the organism to provide additional strengthening to the parts affected.

. . . .

THE INHERITANCE OF ACQUIRED CHARACTERS

Anonymous; *Scientific American,* 75:268, 1896.

Prof. Retzius has lately published an account of certain observations on the foetus of Swedes, which, in connection with similar observations recorded by Surgeon Havelock Charles on the Punjabite, he believes to support the Lamarckian view that acquired characters are inherited. He endeavors to show that the evidence in support of the theory is to be found in our own skeletons. Some years ago, Prof. Arthur Thomson pointed out that in certain races of men who habitually adopt a "squatting position," the tibia and astragalus present additional articular facets, allowing greater flexure of these bones upon one another than is possible (or at any rate normal) in Europeans and other civilized races who have given up squatting, and in which these

facets are absent. Accompanying these facets there is a retroversion of the head of the tibia. Both these characters are present in apes and in certain prehistoric races, and Surgeon Havelock Charles described, a year or two back, a series of instances of their presence not only in the adult Punjabite, but in the foetus. At the meeting of the British Association at Oxford, Prof. A. Macalister exhibited these specimens, as well as similar specimens taken from British infants, and a discussion followed on the meaning of these peculiarities. Now Retzius ("Ueber die Vererbung erworbener Eigenschaften," Biol. Untersuch, N.F. vii) records these same characters in foetal Swedes, from an early age, even up to eight months; and reviewing the facts, he comes to the conclusion--in which I think most of us would agree--that the presence of these characters, viz., the retroversion of the head of the tibia, and "Thomson's facets," is a more primitive condition than their absence in normal Europeans of the present day: that they have been inherited from early times; and in those peoples which habitually adopt the "squatting" position they have become gradually further developed. This last conclusion is perhaps open to question; it is quite possible that even in these races they are less developed than in ancestral forms. But Retzius proceeds to contend that Europeans have undergone gradual change in their skeletons from generation to generation; they no longer sit on their haunches, and have gradually lost the power to do so, and as a consequence "Thomson's facets: have disappeared; and he concludes that "it is, therefore, we Europeans who, on account of changed habits, have undergone changes, and it is in us that these changes have gradually been inherited."

But here it seems to me that Darwinians would join issue with Retzius. His own and other observations show that the changes are not inherited; for the characters of the bones are inherited from the ancestral apelike forms, and it is, surely, only on account of individual habit that the peculiarities are not present in the adult. It is by no means clear what is the "acquired" character on which Retzius hangs his views. It is the osteological peculiarity, or the habit of using chairs to sit upon, instead of employing the squatting posture? His own researches show that the osteological characters are not acquired, while the habit of walking upright and sitting on chairs distinctly acquired, and it is in relation to this acquirement that the osteological peculiarities cease to be evident. Young children, as we know, can and do sit upon their haunches, and can move their legs and ankles in a way that an adult, unless he is fairly athletic, finds it impossible to do; and it appears probably that the disappearance of the facets in the adult is closely connected with the ossification of the bone, which will obliterate the facets now no longer brought into use. It would be interesting to examine in this connection the leg bones of "contortionists" and others who make a free use of their legs and ankles, for a very little practice enables even civilized men to employ exaggerated movements of their limbs. Another point to which attention might be directed (which indeed may have been looked into) is the char-

acter of the articulation of the bones of the great toe in those races which make use of this digit. A casual observation on the skeleton of an Andaman shows that the articular surface of the first metatarsal with the entocuneiform is distinctly more rounded than in a European--a feature in which there is an approach to the condition in the apes. It might have been presumed that some difference, similar to that in Europeans and Punjabites, would be found in digitigrade and plantigrade mammals; but the result of a brief examination of skeletons of such forms is sufficiently surprising to be referred to; for instance, in the lion there is a facet of the same kind as, but not really homologous with, Thomson's facet, at the lower end of the tibia. This is absent in the bear and the dog: it is also absent in the sea otter. It is present, however, in the beaver and other rodents; it exists in some ruminants, as well as in the horse, but is only slightly developed in the tapir, and is absent in the Suidae.

• Extra Ribs in Humans

THE OCCASIONAL EIGHTH TRUE RIB

Anonymous; *English Mechanic,* 50:94, 1889.

Prof. D.J. Cunningham read a paper on "The Occasional Eighth True Rib in Man, and its Relation to Right-handedness." In the course of his remarks he said that there were seven ribs attached to the sternum as a rule on each side, but occasionally an eighth rib was attached. Those attached they called true, and those unattached were known as false ribs. Nearly 70 subjects had been examined--42 females and 21 males, and an eighth true rib was found in 20 per cent. In five, this eighth true rib was present on both sides, and in nine it was present on one side only, and that the right side, and only in one on the left side. He drew attention to the marked preference it seemed to have for the right side, and he believed it was due to right-handedness.

ESKIMO MEN MAY BE GETTING EXTRA RIB

Anonymous; *Science News Letter,* 22:325, 1932.

Eskimo men may be getting an extra rib. If so, it is apparently a case of evolution, according to Dr. T.D. Stewart, phy-

sical anthropologist of the Smithsonian Institution, who has just reported the appearance of the extra rib. Laymen who remember their Bible, however, may consider it a step backward towards Adam, who was said to have lost a rib when Eve was created.

About 12 per cent. of some 200 Eskimo skeletons which Dr. Stewart examined had 25 instead of the customary 24 presacral vertebrae. The condition was present in nearly 16 per cent. of the males but in less than 1 per cent. of the females. It was considerably more frequent in skeletons secured north of the Yukon River.

The extra vertebra is apparently being added in the thoracic and lumbar region.

In his report to the American Journal of Physical Anthropology, Dr. Stewart does not try to explain this anatomical variation. The geographic distribution and the predominance of the anomaly among males suggest a possible hereditary factor.

Whether such an extra vertebra would have any particular advantage or disadvantage for the survival of the individual is not known.

Other races besides the Eskimo have shown this tendency toward getting an extra rib, previous studies have shown. The frequency among the northern Eskimo, however, is nearly twice as great as the maximum recorded before. Among Europeans it runs from 3 to 6 per cent., while one study of Japanese showed a frequency of slightly over 7.

EXTRA RIBS MORE COMMON IN FEMALES

Anonymous; *Science News Letter,* 60:183, 1951.

An extra rib near the neck, which occurs twice as often in females as in males, may be the cause of shoulder and arm pains, Dr. Alfred W. Adson of the Mayo Clinic reported at the U.S. Chapter of the International College of Surgeons meeting in Chicago.

The extra rib near the neck, called a cervical rib, is found in six of every 1,000 patients, he said. In more than half the cases they are discovered accidentally in routine X-ray examinations for other complaints.

When the extra rib causes pain by pressure on an artery or nerve, a muscle cutting operation, sometimes combined with partial removal of the extra rib, gives relief. Complete relief of all symtoms followed this operation in about 60% of the patients, Dr. Adson reported, with great improvement in another third who could return to their vocations with annoying twinges of pain only during certain movements.

• Unusually Large Variations of Sex Ratio

HIDDEN VARIABILITY IN MAN

Dyer, Kenneth F.; *New Scientist,* 44:72-74, 1969.

The first of these variations is the human sex-ratio at birth, which normally varies between 104 and 107 (male births per hundred female births). This can be confirmed from data in the United Nations Demographic Yearbook for 1959 and 1965, particularly from those countries in which the registration of births is said to be relatively complete. There are statistics of birth from 80 different populations for the decade 1949-58 and 50 of them fall within the range 104-107. Many of those with values outside this range over the decade 1949-58 such as Iceland, West Berlin, and Panama, have relatively low total number of births and random error can explain the deviation. When calculated over a longer period of time (1949-64), a large number of values fall within the range 104-107. There are still a number of exceptions, however, and Table 1 shows the sex-ration at birth in those cases that fall significantly below 104 and significantly above 107 respectively.

. . . . .

Table 1 Extreme sex ratios in different countries 1949-64

Country	Number of births	Sex ratio
1. Sex ratio significantly below 104.00		
Monserrat	5869	94.34
St. Lucia	44884	98.77
St. Kitts	28592	100.06
Antigua	28228	100.13
Grenada	50358	100.20
S. Africa (coloureds)	922963	102.04
Bermuda	17893	102.07
2. Sex ratio significantly above 107.00		
Hungary	2754122	107.15
Poland	10753456	107.17
Hong Kong	1405479	108.04
Gambia	13050	110.18
Aden	60852	120.31

. . . . .

The second inexplicable variation concerns frequencies of multiple births. The statistics here are of two sorts. First, there are the total numbers of live children resulting from single, double or multiple births. Secondly, there are the total frequencies of twins among all births, these twin births being divided

into either monozygotic or dizygotic twins (ie the products of one egg or two). Both of these statistics show considerable variation. The proportion of live born children resulting from twin births varies from 2.57 per cent in Jamacia (1652 in a total of 64 338), 2.46 per cent in Northern Ireland (802 in a total of 32 505), and 2.35 per cent in Scotland (2456 in 104355), down to 1.33 per cent (1739 in 130 958) in Bulgaria, 1.19 per cent (2595 in 216 531) in Ecuador, and 1.06 per cent (17 587 in 1 659 521) in Japan. The frequencies of children from multiple births (three or more) are equally variable. For instance, Ecuador has only four per 100 000 births whereas Jamaica has four per 1000. The British Isles exhibit more medium values but even they show curious variation; five per 10 000 in Scotland, three per 10 000 in England and Wales, and one per 10 000 in Northern Ireland. What the reason for this variation is, we cannot tell.

. . . .

A third source of mystery is differential fertility. Between 1955 and 1964 in England and Wales the number of children born every year increased from less than 700 000 to more than 900 000. The number of these children born to mothers aged more than 50 varied between 12 in 1955 and one in 1964. Over the whole decade about eight million children were born in England and Wales, only 50 of them to women aged 50 or more.

In France the figures are slightly higher. There are about 800 000 children born every year and of these about 50 annually are to women more than 50 years old. No other country in western Europe has higher figures than those of France and most are rather lower. The overall figure for children born to women aged 50 and more in western Europe lies between about 10 and 60 for every million births. Clearly these are rather infrequent events.

In at least two countries of eastern Europe, however, the situation is very different. Tiny Albania is the most striking. There are only about 60,000 children born every year in this country but between 300 and 500 of these are to women more than 50 years old. Therefore, women over 50 are involved in about five in every 1000 births or 50000 in every million; this is an astonishing contrast with the 10 in England and Wales. In Yugoslavia, one or two births in every 1000 are to women over 50, which is also very different from western Europe. The reasons for this most curious prolonged fertility in these two countries is just not known.

. . . .

VARIABILITY IN MAN

Biddle, W. F.; *New Scientist,* 44:368, 1969.

In connection with Dr. Dyer's article ("Hidden Variability in Man" 9 October, p. 72) and subsequent correspondence, I believe

it is possible to find variations in the sex ratio (that is, girls to boys in a family) quite near at home.

Some years ago, a number of us noticed at various large research establishments where we had worked, that the scientists appeared to have daughters. At one establishment we compiled the following table:

	Qualified scientists and engineers	Non-scientific staff
Families	41	47
Boys	28	55
Girls	42	42
Ratio	1.5	0.76

Statistical analysis shows that the difference between the samples is significant at the 0.05 level, that is, the difference is fairly certainly real--although I would like to see the experiment repeated with more vigorous controls--including a test to see if there is any difference between scientists and engineers. On the other hand I believe that even higher ratios existed at some of the other establishments, and there were almost certainly more single-sexed families, particularly of girls, than would be found by pure chance.

. . . . .

• Anomalous Pigmentation

A PIEBALD FAMILY OF WHITE AMERICANS

Jenks, Albert Ernest; *American Anthropologist,* 16:221-237, 1914.

In an American university is a member of an old-line American family now living in the Northwest and scattered over at least four states, which for three succeeding generations exhibits an unusual marking of the skin. I have examined and photographed piebald members of each of these three generations. My informant tells me of a piebald cousin of his father whom he well remembers, thus affording knowledge of piebaldism in four succeeding generations.

I shall not take time to describe the exact markings of the skin of the three persons shown in the accompanying illustrations; however, there are some important facts that should be noted. The light areas, or spots, are strikingly bilateral, with

considerable symmetry; they occur with marked consistency at the more important joints of the body--as ankles, knees, hips, wrists, elbows, and shoulders. There is also a tendency to a median dorsal light line. This light dorsal area is the opposite of what Castle reports in his family of spotted negroes. He found a dark dorsal area. [Photos omitted.] (p. 221)

A PIEBALD NEGRO

Anonymous; *Scientific American,* 5:74, 1849.

There is colored man belonging to Wilmington county, Del., now trading, we believe on board of a shallop between that city and Philadelphia, who is in color both white and black. He is covered over with white spots (as white as the fairest white man,) from the size of a dollar to several inches, and even feet in length and breadth. There is a white ring round each of his eyes, and also white round his mouth, one half of one of his arms is white. His predominant color is black, only about a third being white.

ON A PECULIAR CHANGE OF COLOUR IN A MULATTO

Foster, Balthazar W.; *Report of the British Association, 1866,* p. 91.

After alluding to the present state of our knowledge of the conditions connected with the development of pigment, the author related an instance in which he had observed a gradual disappearance of the cutaneous pigment in a Mulatto aged 43. Minute white spots first appeared on the man's back, and by coalescing gradually formed large white patches. These constantly extending, in the course of six years the whole of the trunk became perfectly white, spots of the original colour remaining only on the extremeties. The face retained its dark hue, and an irregular margin encircling the neck formed the limit of the upward advance of the white colour. Isolated spots of white had appeared, however, on the forehead and at the angles of the jaw. The white skin was perfectly healthy in appearance and not to be distinguished from that of a European. The man's hair was black and crispy, and of a flattened elliptical form on section. Blisters applied to the bleached surface restored the dark colour in irregular spots, which remained unchanged for several months. No discoverable condition in the man's constitution or habits could

be regarded as the antecedent of the remarkable change, except possibly a very weak and sluggish state of the circulation. A series of large photographs illustrated the communication.

• Reports of Human Luminosity

THE LUMINOUS WOMAN OF PIRANO

Anonymous; *Scientific American,* 152:323, 1935.

Certain animals and plants are well known to give off phosphorescent light, and from time to time luminous human beings have been reported. Generally, but not always, this emission of light has been noticed just before death. Little is known of the cause of this luminosity, and curiosity has been quickened by Anna Monaro, the Luminous Woman of Pirano, of whom Dr. G. Protti, of Venice, has recently published an account.

Dr. Protti first collected the evidence of eye-witnesses. The usual time of the light's appearance was during the early part of the night, never in the daytime, or when Monaro was only lightly asleep; it lasted never longer than three to four seconds, it always appeared in the region of the heart, it varied in color from green to red. Monaro herself was unaware of the light and it left no trace of odor, heat, or color.

. . . . .

A LUMINOUS TOE

Anonymous; *Scientific American,* 21:171, 1869.

A lady correspondent wrote to the Boston Transcript, that "upon retiring to rest, the gas being out and the room quite dark, the writer's attention was directed to her foot which was illuminated by light, which, upon examination, was found to be phosphorescent, and proceeded from the upper side of the fourth toe of the right foot. Upon rubbing it with the hand the light increased and followed up the foot, the fumes filling the room with a disagreeable odor. This lasted some time, when the foot was immersed in a basin of water, hoping to quench the light, but to no purpose for it continued beneath the surface of the water, the fumes rising above. The foot was taken out and wiped dry, but the light still remained. A second immersion of the foot followed,

and soap applied, with the same result. No more experiments were tried, and after a time it gradually faded and disappeared. The time occupied by the phenomenon was about three quarters of an hour. The lady's husband substantiates the above facts, as he also witnessed them. Will someone please explain the above, as the emitting of phosphorus from a live body is new to the writer?"

SCIENCE FOCUSES ON THE 'LIGHT OF LIFE'

Shawver, Lisa J.; *Science News,* 104:202-204, 1973.

Kirlian photography, invented in 1939 by Russian researchers Semyon and Valentina Kirlian and introduced to the United States three years ago, produces startling photographs of pulsating, multicolored lights streaming from the human body and from plants. Psychologists, psychiatrists, biologists and physicists, as well as investigators of psychic phenomena, are looking into Kirlian photography (also known as radiation-field photography) as a new way of observing energy fields associated with living organisms.

. . . . .

The Kirlians report that their electrophotographs show certain points on the human body radiating light flares more forcibly than the areas around them and that these points correspond exactly to the 741 acupuncture points mapped out by the ancient Chinese. It is also reported that they have produced photographs showing that a plant is diseased before the physical symptoms of the disease appear.

. . . . .

Repeated experiments suggest that light emanations around fingertips vary with the emotional, physiological and psychic state of the person being photographed. Moss has photographed the fingertips of a medical student before, during and after the consumption of 17 ounces of bourbon. The emanations around his fingertips became increasingly brighter and rosier after each consecutive drink until he became "all lit up" and, shortly thereafter, ill. This experiment has been repeated by Tiller and psychiatrist Gerald G. Jampolsky of the Child Center in Kentfield, Calif.; their results corroborate those of Moss.

More puzzling are photographs taken of fingers of "faith healers" --persons who claim they can cure illness by touching others. Often the person being treated by a healer reports a "heat" sensation in the area of the body touched. Pictures taken by Moss show that the glow around the healer's fingers is smaller than before healing, while the patient's emanations increase in size and intensity after healing. "It should be emphasized," she says, "that this phenemonon does not demonstrate

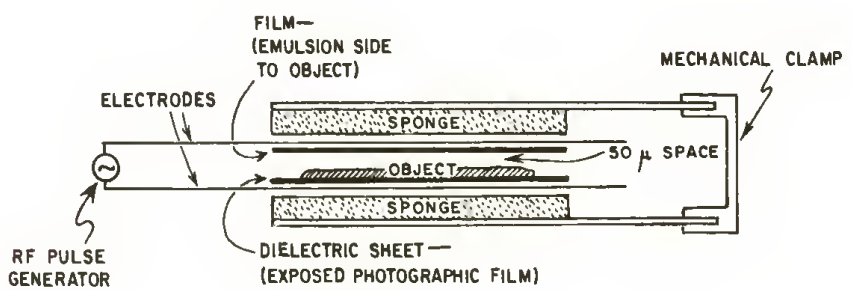
healing; it simply seems to reveal a transfer of energy from healer to patient."

. . . . .

"In living things," the Kirlians wrote, "we see the signals of the inner state of the organism reflected in the brightness, dimness and color of the flares. The inner life activities of the human being are written in these 'light' hieroglyphs. We've created an apparatus to write these hieroglyphs. But to read them we're going to need help."

. . . . .

Jane H. Hu, electrophysiologist and director of research and education at the Acupuncture Institute and Research Center in Washington, says she is "very interested" in Kirlian photography. "It is direct evidence that shows the energy circulation in the body that coincides with the theories behind acupuncture and



In Kirlian photography, the object is placed between two electrodes.

meridians. Whether or not this is an electrical or nonelectrical energy we do not know."

Other investigators are attempting to explain the phenomenon from the viewpoint of traditional Western science.

Tiller says that is a "specific physical explanation called the streamer phenomenon of corona discharge that can account for all the observations made to date."

Electrons, emitted from the object, move to the positively charged plate. By collision, they ionize the air molecules. When the density of positive ions is great enough, they attract the electrons back to the positive ion cloud, producing recombination events that lead to the emitting of radiation. Nitrogen recombination gives rise to the emission of light in the blue and ultra-

violet range. The light patterns thus produced, vary with the distribution of the electrical field which is in turn influenced by the physiological state of the person being photographed. The physiological state is governed by the emotional state. But he adds that although there is a physical explanation this does not mean that some unknown type of energy may not be intimately involved.

. . . . .

IMAGE MODULATION IN CORONA DISCHARGE PHOTOGRAPHY

Pehek, John O., et al; *Science*, 194:263-270, 1976.

Summary. Photographic images obtained by the Kirlian technique are principally a record of corona activity during an exposure interval. Most of the variations in the images of the corona of a living subject who is in contact with the photographic film can be accounted for by the presence of moisture on or within the subject's surface. During exposure, moisture is transferred from the subject to the emulsion surface of the photographic film and causes an alteration of the electric charge pattern on the film. Hence the electric field at the surface of the subject. As a result, large variations in the density of corona images, corona streamer trajectories, and image coloration can be brought about.

The radial extent of corona images--that is, the range of corona streamers--is an inverse function of the resistance in the circuit formed by the high-voltage supply, the subject, and the film-electrode configuration. This is because the voltage at which corona is initiated is dependent on the rate of rise of the voltage impressed between the subject and the electrode, and the rate of rise is governed by the applied voltage waveform and the voltage drop across the resistance. The range of streamers is proportional to the corona onset voltage. However, we have not seen any influence of large changes in skin resistance on streamer range. Presumably, this is due to the shunting effect of skin capacitance.

In general, the photographic response to moisture suggests that corona discharge photography may be useful in the detection and quantification of moisture in animate and inanimate specimens through the orderly modulation of the image due to various levels of moisture.

DEADLY RADIATIONS PRODUCED FROM LIVING HUMAN BODY

Anonymous; *Science News Letter*, 22:3, 1932.

Evil rays emitted from human blood, finger tips, the ends of

noses or flashed from eyes, which have been discovered at Cornell University to kill yeast and presumably other microorganisms, may have an influence on studies of germ diseases.

As yet these studies of baneful effects of human radiation are in their prelimary stages but Prof. Otto Rahn, eminent bacteriologist, has carried them sufficiently far to become convinced that his findings are worthy of more extended investigation.

Prof. Rahn, who formerly worked in Germany and is now professor of bacteriology at Cornell University, Ithaca, aroused great interest among scientists attending the meeting of the American Association for the Advancement of Science and the Society of American Bacteriologists in Syracuse by announcing experiments that seem to parallel scientifically in some respects old superstitions that the human body can exert an evil influence on its surroundings.

Yeast, such as is used in making bread, was killed in five minutes merely by the radiation from the finger tips of one person. When a quartz plate a twelfth of an inch thick, was placed between the finger tips and the yeast, it took fifteen minutes for the yeast to die.

In experiments completed this week Prof. Rahn also found that the end of the nose and the eye produce the yeast-killing radiation. The effect of the rays from the eyes is strangely reminiscent of the "evil eye" of superstition, so far as yeast is concerned. The human chest does not produce the radiation, however. In the tests of fingers it was found that the right hand was stronger than the left even in the case of lefthanded persons.

Prof. Rahn's experiments show that the blood and saliva produce the radiation, but that with different people the rays emitted vary greatly. Some people have the power of producing effective radiations and others do not, while it varies with the same person under different conditions.

It was also demonstrated that the human body as a whole sends out rays.

The exact nature of the radiation is not yet determined but it may be some variety of ultraviolet rays, the invisible radiations of wavelengths shorter than visible light. This seems probable because the human rays are effective, as are ultraviolet rays, after being passed through quartz.

Four years ago German and Russian investigators discovered that active muscles of the human body emit a very weak ultraviolet radiation which stimulated the growth of microorganisms, especially yeast. They found that resting muscle and most of the other body tissues did not produce the rays, but that blood from healthy, normal people did radiate.

Tissue from human carcinoma growths or cancers showed strong radiation properties in these early experiments, while, unlike normal blood, the blood from cancer patients did not have the power of radiation.

Prof. Rahn explained that another investigator several years ago found that the blood of women at certain periods sent out a radiation that killed or damaged microorganisms.

EMISSION OF N-RAYS BY THE HUMAN BODY

Anonymous; *English Mechanic*, 79:97, 1904.

M. Charpentier's discovery that the nerve centres and muscles of the human body give off a special kind of radiation has already been discussed, as reported by us, at a recent meeting of the Academie des Sciences.

While repeating in his laboratory some of M. Blondlot's experiments on the production and effects of the N-rays, M. Charpentier observed a series of new phenomena which seemed to have considerable importance from a physiological standpoint. One of the most convenient methods of observing the N-rays is to receive them in the dark upon a phosphorescent substance of small luminosity, and the rays show themselves by increasing the light given off by the body. Fluorescent substances answer very well for the test screens, and one of the best methods is to use a platino-cyanide of barium screen whose luminous intensity is regulated by a radium salt covered with black paper and placed at a variable distance. The rays from the radium thus excite the screen and make it more or less brilliant. Such a screen then serves to reveal the presence of the N-rays by increasing in brightness when the latter are allowed to fall upon it.

The phosphorescent or fluorescent screen is found to increase in brightness when it is brought near the human body. The effect is strongest in the neighborhood of the muscles and nerves. Contracting the muscle heightens the effect. In the case of nerves or nerve centres, the phenomenon is shown more clearly as the degree of working of the nerve or centre increases. In this way the presence of a surface nerve can be recognised, and the path of the nerve can even be followed by exploring it with the test body. These effects are not only observed on contact with the skin, but can be perceived at a distance. The action takes place through substances which are transparent for the N rays (Aluminum, paper, or glass), and it is stopped by screens which are opaque for the rays, such as lead or wet paper. The effect is not due to an increase of temperature in the neighborhood of the skin, as it keeps up when several sheets of aluminum of paper are interposed (separated by layers of air), and thus screen off the heat.

The rays given off by the body are reflected and refracted like the N-rays, and M. Charpentier was able to produce foci, which were indicated by the maximum brightness, by means of glass lenses. The index of refraction of the rays seems to be near that of the N-rays.

It may be thought that the body only receives and stores up the rays during the day, like the bodies which M. Blondlot exposed to the sun. But after remaining for nine hours in complete darkness, the phenomena showed themselves as usual.

The fact seems to be demonstrated that the human body gives off the N-rays. It is the tissues of the organism whose activity is the strongest which emit the rays in the greatest degree. These phenomena seem to be of capital importance in studying nervous action especially, as the nerves or brain are now found

to exert an action on the exterior which remained unknown up to the present.

One striking experiment as to the effect of the muscles is that the area of the heart can be defined by exploring the region with a small test-screen. As this organ is in great muscular activity, its effect is considerable. When the small luminous screen is moved about the surface of the body in the region of the heart, the outline of this organ and its surface are manifested by the variations in brightness. Similar experiments are now being carried on with the brain and the rest of the nervous system.

• Is the Appendix Truly Vestigial?

VESTIGIAL STRUCTURES

Dewar, Douglas; *The Transformist Illusion,* DeHoff Publications, Murfreesboro, 1957, pp. 168-173.

Before considering the structures cited as vestigial let me insist that no structure should be deemed to be vestigial unless it can be proved to be of no use to its possessor in adult, embryonic or larval life and that it is not a structure inevitably resulting from the manner in which embryos develop. If any animal exhibits a structure of which we have not discovered the use, it is premature in the present state of knowledge to assert that it is a useless vestige. Scores of structures once declared to be useless are now known to be of very great use. All that can be done safely with structures of which the use is not known is to place them in a suspense account. Probably in time the use of most of these will be discovered, so it may be true to say that the number of vestigial structures in animals is the measure of the ignorance of zoologists.

Let me here mention certain structures which until quite recently were cited by transformists as useless vestiges, but which are now universally acknowledged as having most important functions. Chief among these are the endocrine glands: thyroid, parathyroid, thymus, pituitary, and pineal or epiphysis. Rolleston is largely responsible for our knowledge of the functions of these ductless glands. His book "The Endocrine Organs in Health and Disease" (1936) is a classic. Despite recent advances in psychology, it may safely be said that what we know of these and other ductless glands is a very minute fraction of what we have to learn, and that there are other glands in the body of the use of which we are largely ignorant. But today few will dare to say that these glands have no function.

It is hardly necessary to mention that no one now suggests

that the flattened outer ear of man is a useless vestige of a movable ear.

Thanks to the ease with which man can move his head from side to side, he does not need a mobile ear, and the flattened form of the outer ear is a safeguard of against its getting torn or damaged.

It is however necessary to speak of "Darwin's point" i.e. the little conical projection on the margin of the ear, because, in that strange book "The Science of Life" (p. 411) we are told that it is "the remains of the tip of the pointed ear of lower forms, now folded downwards and inwards." Unfortunately for Dr. Julian Huxley and Messers H. G. and G. P. Wells it does not correspond to the tip of the ear of a lower animal. In any case, since many breeds of domestic dog exhibit no trace of this point, it is not absurd to imagine that it persists in man today just because man must recapitulate ancestral stages in embryonic development?

Mention must here be made of the claws or spurs which occur in several kinds of snakes on either side of the vent. e.g. in Boa, Python, Eryx and Tortrix. These claws are often said to be useless relics of the hind legs of snake ancestors. In fact it is almost certain that these appendages assist in locomotion, particularly in the case of large constrictors when climbing trees or hanging from branches. Further A. K. Martin in "The Ways of Man and Beast in India," says that these horny protuberances assist the python in driving itself forwards when on the ground.

In this connection it is necessary to mention that these claws are attached to a small pelvis consisting of ilium, ischium and pubes by means of a leg bone which seems to be homologous with the femur.

Moreover in no known snake is there any trace of a pectoral girdle on a fore-leg.

It may be asked: even if, as seems probable, these hind claws and the pelvis are useful, does not their existence in their present form denote that the snakes of today are modified descendants of ancestor which walked on four legs? For reasons to be given later (see page 176) I submit that the reply to this question is in the negative.

In my "Difficulties of the Evolution Theory," I cited as useless vestiges the following which I have since discovered, serve useful purposes: the splint bones of the horse, the lateral toes which do not reach to the ground of deer and other artiodactyls, the teeth that appear in the foetus of whalebone whales, the eyes of some animals that live in dark caves, probably the stumps of wings exhibited by some flightless insects, and possibly the wings of struthious birds and the vermiform appendix in man.

We have now to notice the uses to which these various structures are put. [Sections 1-3 omitted]

4. The Vermiform Appendix in Man. Birmingham seems to have been the first to suggest that this is not a useless vestige, and at present this seems to be the prevailing view. Thus Le Gros Clark writes ("Early Forerunners of Man" (1934) p. 205): "The significance of the vermiform appendix is still quite obscure, but

in view of its rich blood supply it is almost certainly correct to regard it as a specialized and not a degenerate structure."

In addition to its rich blood supply the appendix has a complex wall composed of an external muscular coat, followed by a mucous layer, under which is a mass of lymphoid tissue which does not appear until after birth, and finally an inner coat of mucous membrane. The use of this organ would probably have been discovered ere this, but for the fact that useless organs, being required as evidence for evolution, are eagerly sought after by transformists!

• Problems in the Evolution of the Human Brain

WAS DARWIN WRONG ABOUT THE HUMAN BRAIN?

Eiseley, Loren C.; *Harper's Magazine,* 211:66-70, 1955.

How did man get his brain? Many years ago Charles Darwin's great contemporary and co-discoverer with him of the Principle of Natural Selection, Alfred Russel Wallace, propounded that simple question. It is a question which has bothered evolutionists ever since, and when Darwin received a copy of an article Wallace had written on this subject he was obviously shaken. It is recorded that he wrote in anguish across the paper, "No!" and underlined the "No" three times heavily in a rising fervor of objection.

. . . . .

As everyone who has studied evolution knows, Darwin propounded the theory that since the reproductive powers of plants and animals potentially far outpace the available food supply, there is in nature a constant struggle for existence on the part of every living thing. Since animals vary individually, the most cleverly adapted will survive and leave offspring which will inherit, and in their turn enhance, the genetic endowment they have received from their ancestors. Because the struggle for life is incessant, this unceasing process promotes endless slow changes in bodily form, as living creatures are subjected to different natural environments, different enemies, and all the vicissitudes against which life has struggled down the ages.

Darwin, however, laid just one stricture on his theory: it could, he maintained, "render each organized being only as perfect or a little more perfect than other inhabitants of the same country." It could allow any animal only a relative superiority, never an absolute perfection---otherwise selection and the struggle for existence would cease to operate. To explain the rise of

man through the slow, incremental gains of natural selection, Darwin had to assume a long struggle of man with man and tribe with tribe.

He had to make this assumption because man had far outpaced his animal associates. Since Darwin's theory of the evolutionary process is based upon the practical value of all physical and mental characters in the life struggle, to ignore the human struggle of man with man would have left no explanation as to how humanity by natural selection alone managed to attain an intellectual status so far beyond that of any of the animals with which it had begun its competition for survival.

To most of the thinkers of Darwin's day this seemed a reasonable explanation. It was a time of colonial expansion and ruthless business competition. Peoples of primitive cultures, small societies lost on the world's margins, seemed destined to be destroyed. It was thought that Victorian civilization was the apex of human achievement and that other races with different customs and ways of life must be biologically inferior to Western man. Some of them were even described as only slightly superior to apes. The Darwinians, in a time when there were no satisfactory fossils by which to demonstrate human evolution, were unconsciously minimizing the abyss which yawned between man and ape. In their anxiety to demonstrate our lowly origins they were throwing modern natives into the gap as representing living "missing links" in the chain of human ascent.

It was just at this time that Wallace lifted a voice of lonely protest. The episode is a strange one in the history of science, for Wallace had, independently of Darwin, originally arrived at the same general conclusion as to the nature of the evolutionary process. Nevertheless, only a few years after the publication of Darwin's work, The Origin of Species, Wallace had come to entertain a point of view which astounded and troubled Darwin. Wallace, who had had years of experience with natives of the tropical archipelagoes, abandoned the idea that they were of mentally inferior cast. He did more. He committed the Darwinian heresy of maintaining that their mental powers were far in excess of what they really needed to carry on the simple food-gathering by which they survived.

"How, then," Wallace insisted, "was an organ developed so far beyond the needs of its possessor? Natural selection could only have endowed the savage with a brain a little superior to that of an ape, whereas he actually possesses one but little inferior to that of the average member of our learned societies."

At a time when many primitive peoples were erroneously assumed to speak only in grunts or to chatter like monkeys. Wallace maintained his view of the high intellectual powers of natives by insisting that "the capacity of uttering a variety of distinct articulate sounds and of applying to them an almost infinite amount of modulation . . . is not in any way inferior to that of the higher races. An instrument has been developed in advance of the needs of its possessor."

Finally, Wallace challenged the whole Darwinian position upon man by insisting that artistic, mathematical, and musical abilities

could not be explained on the basis of natural selection and the struggle for existence. Something else, he contended, some unknown spiritual element must have been at work in the elaboration of the human brain. Why else should men of simple cultures possess the same basic intellectual powers which the Darwinists maintained could only be elaborated by competitive struggle?

"If you had not told me you had made these remarks," Darwin said, "I should have thought they had been added by someone else. I differ grieviously from you and am very sorry for it." He did not, however, supply a valid answer to Wallace's queries. Outside of murmuring about the inherited effects of habit---a contention without scientific validity today, Darwin clung to his original position. Slowly Wallace's challenge was forgotten and a great complacency settled down upon the scientific world. (pp. 66-67)

SCIENCE AND PSEUDOSCIENCE

Davies, L. M.; *Nineteenth Century,* 141:108–112, 1947.

Meanwhile he [H. S. Shelton] either forgets, or never knew, many most uncomfortable (to him) facts about fossil man. For most fossil human remains are of distinctly high type---'above the modern standard' as Sir A. Keith, Sir A. Smith Woodward, W. P. Pycraft and others have sadly admitted. The huge-brained Wadjak man (found in Java, like Pithecanthropus, but ignored as less satisfactory by most evolutionists) is supposed to be the ancestor of the small-brained modern aboriginal Australians. Even the later Talgai, Keilor, etc., fossil Australians were quite as brainy as their living counterparts. Similarly, while modern Bushmen are small in size and brain, their remote fossil predecessors---Fish Hoek men, Grimaldi boy, the Strandloopers, etc.---were very big brained. And the oldest of them all---the magnificent Boskop man---had the biggest brain of all. Where did he spring from? Even the modern Somali, fine specimen of humanity as he is, had a still finer fossil counterpart in Springbok man. Our own (Caucasian) Pleistocene counterparts---Cromagnon, Engis, Brunn, and the pre-Neanderthal Dartford man (with Cromagnon affinities)---were all better endowed with brains than ourselves.

Indeed, Neanderthal man himself usually had a bigger brain (both as boy and man) than any living race. And his brutal characters were surpassed in the later Rhodesian fossil man; while both were surpassed in brutal characters by Gardar man, who was not a fossil at all, but a twelfth-century Viking buried in a Christian cemetery. With what joy would our straw-clutching enthusiasts have acclaimed Gardar man as a link, had he only appeared, duly mineralised, in Pleistocene deposits! As it is, they forget him, while making as much as they can of less brutalised---and so doubtless quite as effectively human---fossil types. (pp. 111-112)

• Human Rumination

RUMINATION IN THE HUMAN SUBJECT

Anonymous; *Science,* 14:418, 1889.

In the London Medical Recorder for Nov. 20, 1889, Dr. Ireland summarizes the contents of a paper on this curious phenomenon by Dr. Sievers in the Finska Lakaresallskapets Handlingar, Nov. 5, 1889.

This author first gives a resume of the different opinions upon rumination since 1618 (when Fabricius ab Aquapendente published the first case of this affection) until the present time. He recalls that since the appearance of the classical work by Adrien Dumur on the "Paralysis of the Cardiac Orifice or Merycism," the most recent authors see in this affection a nervous moving of the stomach accompanied by more or less diminution of the tone of the cardiac orifice. He thinks, however, that the true nature of rumination has not yet been thoroughly studied. Like Johannessen, to whom we owe the most detailed examination of this subject, Dr. Sievers says, that, before drawing any conclusion, the details should be more minutely studied. But while the researches already made do not explain satisfactorily the nature of rumination, they furnish us with very important facts for the therapeutic treatment.

Dr. Sievers publishes three cases of rumination which he observed in private practice at Helsingfors. Beside these, so far as he knows, there are only three other cases of rumination mentioned in Scandinavia, and reported by Johannessen in Zeitsch, fur klin, Medicin, Bander X. and XII. In the first case described, the patient, aged twenty-seven, who had been a governess and sick-nurse, belonged to a very nervous family, though none of them suffered from insanity or any other grave disorder. She had previously enjoyed good health. She always ate very quickly, and did not properly masticate her food. It is now ten years since she commenced to ruminate her food, after a sea-voyage lasting from three to four days, during which time she had not defecated, owing to want of convenience. Five, ten or thirty minutes after eating, the food is collected in little balls in the mouth in order to be subjected to a second mastication. The patient seems quite at ease during rumination. After an ordinary dinner the rumination lasts from an hour and a half to two hours. If she moves about, or even if she is disturbed, rumination begins sooner, and is more active. Trying to restrain the process brings on such distress that the patient is compelled immediately to give in. During rumination she prefers to be seated. She leans forward, and at every mouthful which returns she lowers her head.

On scrutinizing the abdomen during the act of ruminating, one notices a dimple-like depression under the ribs. This is accompanied by an uneasy sensation passing from right to left. This

does not extend farther than about the cardiac orifice. The patient feels a slight shock, and the food returns to the mouth. The stomach was found to be moderately distended with air. There was no retardation of digestion, and no excessive secretion of gastric juice; but there was found to be unusual acidity of the contents of the stomach, owing to the increased production of hydrochloric acid. No lactic acid could be detected. For this patient Dr. Sievers prescribed a teaspoonful of Carlsbad salts before dinner and supper, and a teaspoonful of bicarbonate of soda after each meal. The diet was to consist of milk, eggs, meat, and a very little bread. Under this treatment there was a gradual improvement; and at the end of five weeks the rumination had entirely ceased, nor did it return after she had discontinued using the alkalies.

The second case was a priest sixty years old. He had always ruminated. His father, now eighty-eight, did the same. The process commenced after a meal, and lasted from two to three hours. He never tried to stop it, and does not think he could, as it goes on independently of his will. He did not desire medical treatment with a view to remove it.

The third case was a Jewess, thirty-five years old, of a highly neurotic family. Her father also ruminated; and one brother out of the family of nine occasionally did the same. She herself has ruminated from childhood. The food returns of itself. The act causes her no uneasiness, which would not be the case if she tried to resist it. She did not desire medical treatment. The contents of the stomach were found to be very acid.

In La Psichiatria (Fasc. III.-IV.) there is a paper on "Rumination," by Dr. Cantarano. He had opportunities of studying this affection in four idiots, two imbeciles, and three patients deeply demented. No uneasiness seems in these cases to have followed the process. Dr. Sievers, among other contributions to this curious subject, refers to the papers of Alt (Berl, klin. Wochensch., 1888, Nos. 26 and 27) and of Boas (No. 31 same journal); and in the Archives de Neurologie (VII. 1884) the reader will find an interesting paper on "Merycism," by Drs. Bourneville and Seglas.

. . . .

• Strange Cases of Transpiration

CASE OF TRANSPIRATION OF HALF THE BODY

O'Beiln, Lucius; *American Journal of Science*, 1:39:400, 1840.

In 1834 I called the attention of the scientific community to an anomalous case of cutaneous transpiration which presented itself

in the person of a friend of mine, a merchant of Baltimore. The particulars of the case were published in the "Medical Library," a journal conducted by Prof. Pattison, and copied from thence into the "Transylvania Journal of Medical Sciences;" but the explanation of the phenomenon attempted by the editor was to me unsatisfactory. Will you oblige me by giving the 'case' a place in your Journal, with such comments as its perusal may suggest.

The merchant referred to, from his earliest youth enjoyed a marked immunity from disease; and assured me that from infancy to the moment at which he spoke, his "sweating" was confined to one side of his body. The right side of his scalp, the right side of his face, right side of his thoracic, hypochondriac, iliac, lumbar, and pelvic regions, right thigh, leg, and foot, were frequently saturated with the matter of perspiration, whilst their corresponding opposites maintained an unvarying aridity. My intimacy with him afforded me repeated opportunities of observing the phenomenon. It was often a subject of merriment with him, and never one of the least concern. Could paralysis of the exhalents of the opposite side have existed without other morbid manifestations? I think not.

• Anomalous Human Odors

SINGULAR CASE OF ODIFEROUS EMANATIONS

Anonymous; *American Journal of Science,* 1:22:368-369, 1832.

In the 34th Volume of the Memoirs of the Royal Academy of Sciences of Turin, (1830) Dr. Speranza of Parma relates the case of an individual whose left fore arm emitted an odor of Amber, or of Benzoin, or Balsam of Peru. The odiferous emanations were sometimes so strong that they filled the whole of the large room in which the Doctor conducted his experiments upon this personage, whom he suspected at first of some charlatanry, but of whose sincerity he was soon convinced. He was a man of thirty four years of age, of a robust constitution, (having, until that time enjoyed constant health) agreeable eyes, expressive features, dark thick hair, a ruddy countenance, muscles prominent,--a man of ardent feelings and quick penetration; to whom nature had been liberal in her endowments. It did not appear that electricity had any part in the production of this singular phenomenon. An attack of bilious fever, in the course of two months, destroyed the cause, and the effect did not return after his recovery.

THE SMELL OF THE INSANE

B.; *Journal of Science*, 18:623, 1881.

Although you ridiculed Dr. Richardson's opinion that the insane have a peculiar smell, perhaps the subject may be worthy of inquiry. I have been assured, by one who had means of observing, that nearly all the insane have a peculiar odour, which varies much in different patients, but which my informant thinks can be generally distinguished from other personal odours. May not disorders of the brain be associated with disorders of the excretions?

• Human Eyes That Shine at Night

SHINING IN HUMAN EYES

Anonymous; *Nature*, 145:737, 1940.

From time to time, reports have appeared of the occurrence in human eyes of the phenomenon of "night-shining," whch as was indicated in a recent paragraph (Nature, March 30, p. 506) has been observed in many animals by Ernest P. Walker, assistant director of the National Park of the Smithsonian Institution. It was there stated that Mr. Walker had no definite proof of such cases, although he had encountered reports of them. That statement has led Denis G.A. Dyson, King Edward VI School, Stratford-on-Avon, to write saying that for many years he has known of an "undoubted case" of shining eyes in a shop assistant in Birmingham. Although, not being aware of the rarity of such an occurrence he made no particular observations, his impression is that the glow was of a "dark red colour". In view of the apparent lack of information about such a condition in human beings, it would be of interest if some scientific worker could follow up Mr. Dyson's clue.

NIGHT-SHINING EYES

Glennie, E. A.; *Nature*, 146:366, 1940.

Spelaeologists have perhaps rather exceptional opportunities for observing 'night-shining' in human eyes. I have seen it on

three occasions with different individuals, and once was accused of exhibiting the phenomenon myself.

On all occasions the observer was below the object and the illuminant was the concentrated beam of a focused electric torch. The glow from the eyes is a most uncanny tawny orange, causing exclamations of horror.

Once only have I seen night-shining by a human outside a cave. An Indian woman, who was bending low down, looked back from that position at the headlights of my car--a momentary gleam from her eyes, which ceased as she stood up and faced the light. Hence the incidence of the light on the eyes on this occasion was similar to that on the previous occasions in caves. Is it possible that the normal individual only exhibits the phenomenon when, with dilated pupils, he is caught by a concentrated beam of light coming from this rather unusual direction?

• Spontaneous Human Combustion

SPONTANEOUS COMBUSTION

Anonymous; *Nature,* 138:694, 1936.

The Medico-Chirurgical Review of October 1836 gives the following account of a case of spontaneous combustion reported in the French Press to have taken place at Aunay in the Department of Avalon: A very fat woman, aged 74 years, addicted to drinking brandy at 27 degrees, lived alone, and one evening returned home as usual, but, as she did not appear among her neighbours the next morning, they knocked at her door. No answer being returned to repeated demands, they summoned the mayor, who forced the door and exposed a horrible spectacle, accompanied by an extraordinary smell. Near the chimney lay a heap of something burnt to cinders, at the end of which was a head, a neck, the upper part of a body, and one arm. At the other end were some of the lower parts, and one leg still retaining a very clean shoe and stocking. No other traces of fire were to been seen, except a blue flame which played along the surface of a long train of grease, or serous liquor, which had been produced by combustion of the body. The mayor found it impossible to extinguish the flame, and summoned all the authorities and, from the state of the apartment and comparison of circumstances, it was concluded among them that previous to going to bed, for which she had evidently been making preparations, the woman had been trying to ignite some embers with her breath. The fire communicating with the body by means of the breath, com-

bustion took place, and would appear to confirm an opinion entertained by several learned men, that that which is called spontaneous combustion of the human frame never takes place without the presence of some ignited body near the person predisposed to combustion."

DEATH BY SPONTANEOUS COMBUSTION

Anonymous; *Scientific American,* 5:243, 1850.

The following extraordinary occurrence is related by the Gazette des Tribunaux:

"A few days ago in the tavern near the Barriere de l'Etoile, a journeyman painter, named Xavier C----, well known for his intemperate habits while drinking with some comrades, laid a wager that he would eat a lighted candle. His bet was taken, and scarcely had he introduced the flaming candle when he uttered a slight cry, and fell powerless to the ground. A bluish flame was seen to flicker about his lips, and on an attempt being made to offer him assistance the bystanders were horrorstruck to find that he was burning internally. At the end of half an hour his head and the upper part of his chest were reduced to charcoal. Two medical men were called in, and recognized that Xavier had fallen a victim to spontaneous combustion. This conflagration of the human frame is frightfully rapid in its progress; bones, skin and muscle, are all devoured, consumed, and reduced to ashes.

A handful of dust on the spot where the victim lay is all that remains."

A DRUNKARD ON FIRE

Anonymous; *Scientific American,* 2:181, 1847.

Dr. Nott, in his lectures, gives the following account of a young man, about 25 years of age:

"He had been a habitual drinker for many years. I saw him about 9 o'clock in the evening on which it happened; he was then an usual, not drunk, but full of liquor; about 11 o'clock the same evening I was called to see him. I found him literally roasted, from the crown of his head to the sole of his feet. He was found in a blacksmith's shop, just across from where he had been. The owner, all of a sudden discovered an extensive light in his shop, as though the whole building was in one general flame. He ran with the greatest precipitancy, and on throwing open the door, discovered a man standing erect in the midst of a widely extended

silver-colored flame, bearing, as he described it, exactly the appearance of the wick of a burning candle in its own flame. He seized him (the drunkard) by the shoulders and jerked him to the door, upon which the blaze was instantly extinguished. There was no fire in the shop, neither was there any possibility of any fire having been communicated to him from any external source. It was purely a case of spontaneous ignition. A general sloughing soon came on, and his flesh was consumed or removed in the dressing, leaving the bones and a few of the larger blood-vessels; the blood, nevertheless, rallied round the heart, and maintained the vital spark until the thirteenth day, when he died, not only the most loathsome, ill-featured, and dreadful pictured that ever was presented to human view, but his shrieks, his groans, and his lamentations, also, were enough to rend a heart of adamant. He complained of no pain of body; his flesh was gone. He said he was suffering the torments of hell; that he was just upon the threshold, and should soon enter its dismal caverns; and in this frame of mind he gave up the ghost."

SPONTANEOUS COMBUSTION OF THE HUMAN BODY

Anonymous; *English Mechanic*, 10:398-399, 1870.

A question having appeared in "Notes and Queries" on the Spontaneous Combustion of the Human Body," we give the following on the matter from the Boston Journal of Chemistry:--

Let us examine this matter in the light of what is actually known. We must in the first place, carefully distinguish between the notion of "a preternatural combustibility" of the body under certain abnormal conditions, and that of its spontaneous combustion. The former is not impossible; indeed, there are tolerably "well authenticated instances" of the kind. The latter, if not absolutely inconceivable, is in the highest degree improbable, and eminent physiologists who have carefully investigated all the cases in which it is alleged to have occurred, do not find a single one established beyond a doubt.

The earliest case of the kind which has a semi-balance of authority to sustain it, is said to have happened in 1725, and from that time down to the year 1847, when the last alleged case occurred, some fifty instances are recorded. Liebig made an analysis of all these cases in 1851, and found that they nearly all agree in the following points:--

1. They took place in winter. 2. The victims were hard drinkers, and were drunk at the time. 3. They happened where the rooms were heated with fires in open fire-places or pans of glowing charcoal. Cases where rooms are heated by means of closed stoves are exceedingly rare. 4. It is admitted that no one has ever been present during the combustion. 5. No one of the physicians who collected the cases, or attempted to explain them, has ever observed the process, or ascertained what preceded the

combustion. 6. No one has known how much time had elapsed from the beginning of the combustion to the moment when the consumed body was found.

Out of forty-five cases collected by Frank, of Berlin, in 1843, there were only three in which it is assumed that the combustion occurred when there was no fire in the neighbourhood; and Liebig clearly shows that these three cases are totally unworthy of belief. The conclusions to which he comes is that "spontaneous combustion in a living body is absolutely impossible." Flesh which has been saturated with alcohol for a great length of time, as anatomical preparations, is not combustible; if ignited, the alcohol burns off, scarcely charring the flesh. The corpses of drunkards have never been found to be combustible.

M. Duvergie has opposed Liebig's views, and has expressed the opinion that molecular changes may take place in the living body by which it becomes more combustible from the absorption of alcohol, or from its conversion into more inflamable compounds; but he admits that the combustion is probably never spontaneous. Dr. Marc has suggested that inflammable gases, and possibly even phosphuretted hydrogen, which, under certain circumstances, inflames on contact with the air, may be generated in the living body, and may thus given rise to its spontaneous combustion; but this is merely a theory to account for such cases of combustion, if they have occurred.

On the whole, this idea of spontaneous combustion, appears to be one of those old medical delusions which, having once gained a sort of credence, are not readily given up. It is easy to see, as Liebig observes, that it arose at a time when men entertained entirely false views on the subject of combustion, its essence, and its cause. It is only since the time of Davy, or for about half a century, that combustion has come to be thoroughly understood. After people had once got into their heads that the body might take fire of itself, it is not singular that when a man happened to be burned up, the case was explained in that way if it could not readily be accounted for in any way; just as hundreds of fires caused by carelessness, not easily detected, are charged to the mysterious "incendiary." Then, again, other things being equal, the more marvellous explanation of strange phenomena is usually the more popular one. The Latin proverb Omni ignotum pro magnifico est, might be read Omni ignotum pro mirifico est; with everybody, whatever is unknown passes for a marvel. We need not be surprised, therefore, that this idea of human combustibility, which was not inconsistent with the scientific knowledge of the age in which it had its origin and which consequently came to be accepted by the scientific men of the time, should still live as a popular superstition, and even find an occasional defender among the savans of this more enlightened day.

. . . .

SPONTANEOUS COMBUSTION

Godfrey, H. E.; *English Mechanic,* 10:430-431, 1870.

The following is from a sixpenny work on the third law of chemical affinity, or that which relates chemistry, published by Hardwicke:--"In illustrating to the influence of temperature, examples have been given of cases of spontaneous combustion, which are traced to the evolution of heat, in consequence of absorption of atmospheric air, or the oxygenuous portion of it, by large masses of combustible matter. That electricity is concerned in the production of these phenomena is at least highly probable, though it may be difficult to determine its mode of action. The evolution of electric light from living animals has been noticed by several writers; and though such luminous appearances often take place without inflammation, it may be conceived that the electricity thus evolved, either owing to its extraordinary intensity, or to the presence of highly inflammable matter, might excite combustion, and even cause the destruction of the human body. The number of cases on record renders it impossible to deny that some process of igneous decomposition may commence during life under circumstances very different from those of ordinary combustion from the application of fire.

"Among the instances of spontaneous combustion commencing during life in the human subject, that of the Countess Cornelia Taugari and Bandi, of Cesena, has, perhaps, been most frequently quoted, and is therefore best known. The circumstances of this appalling accident were published in Italy by Signor Blanchini, of Verona, and subsequently (in 1745) by Dr. Cromwell Mortimer, in the 'Philosophical Transactions.' This lady, who was 62 years of age, having been left one night by her attendant in bed asleep, was found the next morning on the floor of the room, reduced to a heap of ashes, except her arms and legs and part of the head. The air of the apartment was loaded with fine soot, which had a noisome smell. The bed was not damaged, and the bed-clothes were lifted on one side, as is usual by a person getting out of bed. Neither the floor nor furniture were injured by fire, the combustion manifestly having been excited by causes acting solely on the body, and being entirely confined to it. The countess, it seems, was accustomed to bathe her body with camphorated spirit.

"Mr. Wilmer, an eminent surgeon of Coventry, published an account of a similar occurrence, on which he had an opportunity for making observations. Mary Chies, 50 years of age, was much addicted to intoxication, and for about a year before her death scarcely a day had passed in the course of which she had not drunk at least half a pint of rum or aniseed water. Her health gradually declined, and about the beginning of February, 1773, she was attacked by jaundice, and confined to bed. She still continued her habit of drinking spirits every day, and smoking a pipe of tobacco. On Saturday morning, March 1, having risen, she fell on the floor, and being too weak to get up, she remained lying there until some person came in and helped her into bed.

The following night she requested to be left alone, and a woman who was with her quitted her at eleven o'clock, having shut the door and locked it. according to custom. At half-past five in the morning, smoke was observed issuing from the window, and the room door being speedily broken open, some flames which were in the room were soon extinguished. Between the bed and the fire-place were found the remains of the unfortunate woman. One leg and thigh were still entire, but nothing was left of the skin, muscles, or intestines. The bones of the skull, body, and arms were wholly calcined, and covered with whitish ashes. The side of the bedstead next to the chimney was slightly burned, but the feather bed and clothes were uninjured. The walls and everything in the room were blackened, and the air filled with a very disagreeable vapour, though nothing except the body exhibited any strong traces of fire.

"Another case, of later date, in which the sufferer survived the accident a few days, is recorded in a German journal. Don G. Maria Bertholi, a friar who lived at Mount Volere, went to the Fair of Ziletto, and having walked about all day, retired in the evening to the house of a relative at Fenille to pass the night. Upon his arrival, he went directly to his bedroom and went to bed, having a handkerchief placed between his shoulders, under his shirt. In a few minutes after he had been left alone, a stifled noise, mingled with cries, was heard from his room; and when the people in the house rushed in, they found him on the floor, enveloped in a lambent flame. He was visited next day by a surgeon, who found that his body was much burned, and after suffering a good deal he died on the fourth day after the accident.

There are certain points wherein all authenticated cases of spontaneous combustion agree:--1. The victims have been addicted to alcoholic liquors. 2. They have generally been advanced in years. 3. They have most frequently been females. 4. Some burning substance has come into contact with the body, or been in the apartment, or there have been reasons to infer electric agency. 5. The extremities have generally escaped combustion. 6. The clothes have been little or not at all injured. 7. The effusion of water has increased the activity of the flames. 8. The product of the combustion has been greasy and very fetid ashes, in a very small quantity, as compared with the animal matter decomposed.

"These phenomena are evidently quite distinct from ordinary combustion; and the term spontaneous seems scarcely as applicable (since there is no evidence that the source of ignition has, in any instance, originated in the body itself) as preternatural combustion.

"There is, however, one source to which the excitement of these alleged spontaneous combustions may with probability be attributed, because the circumstances are favorable to its production; and that is, the electric spark on the extrication of electricity possessing such a degree of intensity as is requisite to inflame combustible matter. Several instances of the evolution of electric light from animal bodies, when excited by friction, and

others in which these appearances were accompanied with actual combustion, are noticed in Sir D. Brewster's 'Letters on Natural Magic.' To what circumstance the production of electric sparks, that could set fire to inflammable vapours, should be ascribed, in any given case, can only be conjectured; but as most of the accidents referred to occurred to persons while in bed, it is not improbable that the electricity might be excited by the heat and pressure of the body on the feathers or wool, but especially the former, constituting the stuffing of the bed."

. . . .

SPONTANEOUS COMBUSTION

Anonymous; *Science*, 13:277–278, 1889.

The following case of spontaneous combustion is reported in the British Medical Journal by Dr. Booth: "On the morning of Sunday, Feb. 19, I was sent for to examine the remains of a man, aged 65, a pensioner of notoriously intemperate habits. I found the charred remains of the man reclining against the stone wall of the hay-loft. The main effects of combustion were limited to the corpse, and only a small piece of the adjacent flooring and the woodwork immediately above the man's head had suffered. The body was almost a cinder, yet retained the form of the face and figure so well that those who had known him in life could readily recognize him. Both hands and the right foot had been burnt off, and had fallen through the floor into the stable below, among the ashes; and the charred and calcined ends of the right radius and ulna, the left humerus, and the right tibia and fibula, were exposed to view. The hair and scalp were burnt off the forehead, exposing the bare and calcined skull. The tissues of the face were represented by a greasy cinder, retaining the cast of the features, and the incinerated mustache still gave the wonted military expression to the old soldier. The soft tissues were almost entirely consumed. On my return from other work, later on, I found that the whole had been removed. The bearers told me that the whole body had collapsed when they had tried to move it en masse. From the comfortable recumbent attitude of the body, it was evident that there had been no death-struggle, and that, stupefied with all the whiskey within and the smoke without, the man had expired without suffering, the body burning away quietly all the time."

. . . .

THE ALLEGED SPONTANEOUS COMBUSTION OF THE HUMAN BODY

Anonymous; *Science*, 14:94–95, 1889.

When "Bleak House" appeared, in 1853, novel-readers were treated to a new sensation in the way of a death-scene, when Krook was taken off the stage by spontaneous combustion, "of all the deaths that can be died." The public shuddered, and medical readers smiled. The subject was then to most physicians, as it is now, well inside the border of medical mythology.

Within the past year or two, several cases have been put on record, which, with the list previously accumulated, serve to establish pretty clearly, in the opinion of The Boston Medical and Surgical Journal, "the fact of an occasional abnormally increased combustibility of the human body, which, it should be observed, does not necessarily imply ignitability, or true spontaneous combustion."

For instance: Dr. Booth's case, which is reported, with a photograph of the nearly consumed remains, in the British Medical Journal (vol. i. 1888, p. 841), is of a pensioner, aged sixty-five, of very intemperate habits, who climbed into a hay loft while drunk, at nine p.m. Neighbors saw by a skylight a light struck, which after a while was put out. At eight the next morning, the body, with all its soft parts consumed, was seen lying over a hole in the floor which had nearly burned through but had one or two joists that kept the body from falling through. The change of the application of fire to the man's clothes is here distinctly stated; and the combustion, remarkable as it was, is not shown to have been spontaneous.

Again, Middlekamp, in the St. Louis Medical and Surgical Journal, October, 1885, reported a similar case of nearly complete combustion, where the victim, a man of sixty-six and a drunkard of twenty years' standing, fired a gun at his own breast with a ramrod. Here the heat was so intense as to melt the ramrod and a metal buckle. The body was consumed entirely, except the lower part of the legs, the head, and the arms.

In the Therapeutic Gazette of the current year, two more such instances are reported. One of these, Dr. Clendenin's case, was an old Irish woman, addicted to the excessive use of whiskey, of which she had drunk a quart the day she died. She had always been the last of the household to go to bed, and so always extinguished the tallow candle (their sole means of illumination). There was also a fire in the kitchen stove. The inner walls of the house were covered with greasy soot, and the two old men who were the only other occupants were both asphyxiated. A hole was found burned through the kitchen floor about two and one-half by three feet square. Upon examining the opening in the floor, a mass of cinders was discovered on the ground beneath. Upon removing them, the skull, the cervical, and half the dorsal veretebrae were found reduced very nearly to a cinder, also about six inches of the right femur, together with part of the ilium in about the same state as the vertebrae. The feet were found in the shoes: the left foot was reduced to a cinder, the shoe being partially cal-

cined; the other foot and shoe were reduced to a complete cinder. On removing the entire remains of a woman, who a few hours previous had weighed one hundred and sixty pounds, were placed in a box that would hold less than one bushel. The entire remains weighed twelve pounds. The pine joint against which the remaining cinders lay were slightly charred, but not burning when found.

To burn the human body, under ordinary circumstances, as the editor of the journal states, is not an easy thing. The great heat secured in crematories, and the length of time even then requisite to incinerate the body, illustrate this fact. It has been shown that the body is three-quarters water, and a great deal of combustible material is a necessary adjunct to the successful reduction of so non-flammable a substance. What, then, is it that occasionally imparts to it so abnormal a susceptibility to flame? Here theories are at fault. We may safely say that it is not, as has been claimed by some, alcohol deposited in the tissues: for Liebig found that flesh saturated in that liquid would burn only until the alcohol was consumed. The hydrogen theory is also fanciful; and the best explanation, namely, an abundant deposit of fat in the cells of the body in such cases, fails to account for the fact that not all fat people are subject to this fate, but that it is only the fat, elderly alcoholic subjects that have been shown to manifest abnormal combustibility. Possibly the alcohol in such cases has the double effect of laying up fat and stupefying the subject, so that he is unable to save himself when he does take fire.

One of the best recent monographs on this subject is that of Dr. F. Ogsten (British Foreign Medico-Chirurgical Review, vol. xlv. p. 179), which details a case of his own,--again, be it observed, one where the spontaneous element was wanting, or was not proved. A woman, fat, intemperate, was in front of smouldering ashes in a grate, and was almost wholly consumed, with little burning of the surrounding objects, and with nothing specially inflammable about her. Ogsten seems to have had some doubt on this subject, seven years later, in referring to the same case (Medical Times and Gazette, vol. i. 1877, p. 27), when he says the question is still sub judice; but he admits that one cannot explain the facts in this case without assuming that the body was in a condition unusually favorable for and predisposed to the feeding at its own fire.

In the monograph referrred to, Ogsten collected the opinions of thirty-five authors who treated of this subject, and he thus classified them: five were quite sceptical on the whole subject, three believed in increased combustibility only, and twenty seven believed in spontaneous ignitability as well.

The opinion of the editor of the Medical and Surgical Journal is, therefore, that the necessarily isolated condition of all persons who perish in this way, and the commonness of fires and lights or of the means of producing them in all places, would make it extremely difficult to establish the fact of spontaneous ignitability, even did it exist. Certainly such proof has not yet been given us. The other point, that of increased combustibility, seems to have received considerable confirmation.

HUMAN SPONTANEOUS COMBUSTION

Anonymous; *Science*, 19:100-101, 1892.

On 12th of May, 1890, while making a professional call in the outskirts of the town, B.H. Hartwell, M.D., of Ayer, Mass., was summoned into the adjacent woods by a messenger, who stated that her mother was "burned alive." In a paper read before the Massachusetts Medico-Legal Society, and published in the Boston Medical and Surgical Journal, Dr. Hartwell says: "Hastily driving to the place indicated (about forty rods distant) a human body was found in the actual state of conflagration. The body was face downward; the face, arms, upper part of the chest, and left knee only touching the ground; the rest of the body was raised and held from the ground by the rigidity of the muscles of the parts. It was burning at the shoulder, both sides of the abdomen, and both legs. The flames reached from twelve to fifteen inches above the level of the body. The clothing was nearly all consumed. As I reached the spot the bones of the right leg broke with an audible snap, allowing the foot to hang by the tendons and muscles of one side, those of the other side having burned completely off. Sending my driver for water and assistance, I could only watch the curious and abhorrent spectacle, till a common spading fork was found with which the fire was put out by throwing earth upon it. The flesh was burned from the right shoulder, exposing the joint from the abdomen, allowing the intestines to protrude, and more or less from both legs. The leg bones were partially calcined. The clothing unburned consisted of parts of a calico dress, cotton vest, woollen skirt, and thick, red, woollen undergarment. The subject of the accident was a woman, forty-nine years of age, about five feet five inches in height, and weighing not far from one hundred and forty pounds of active habits and nervous temperament. A wife and mother she was strictly a temperate person, accustomed through life to hard work, one who, in addtion to her household duties, went washing and cleaning, besides doing a good share of the work in a large garden. On the fatal afternoon she had--as the place showed--been clearing a lot of stumps and roots, and had set fire to a pile of roots, from which it had communicated to her clothing or it had spread into the woodland and had set fire to the clothing during her endeavors to stop it. The body lay about two rods from the burning pile. As proof that the flesh burned of itself and nothing but the clothing set it afire, it may be stated that the accident occurred after a rain; that the fire merely skimmed over the surface of the ground, not burning through the leaves; that there was nothing but charred leaves under the body; that her straw hat which lay several feet distant was simply scorched; that the wooden handle of the spade was only blackened. The above case is interesting in several particulars. It is the first recorded case in which a human body has been found burning (that is, supporting combustion) by the medical attendant. It differs from nearly all of the recorded cases, in that it occurred in a person in middle life, not very fat, and

not addicted to the use of alcohol. It is interesting in a medico-legal sense. It proves that under certain conditions--conditions that exist in the body itself--the human body will burn. We have abundant proof in the many recorded cases of so called spontaneous combustion (seventy-three are chronicled in medical literature) that the body has been more or less completely destroyed by fire, under circumstances that show that it will support combustion, and this has given rise to the belief in the spontaneous origin of the fire."

• Electrical and Magnetic Humans

AN ELECTRIC INFANT

Anonymous; *English Mechanic,* 10:381, 1869.

The French papers report the death of an electrical child, aged ten months, at S. Urbain, near Lyons. This interesting but inconvenient infant was, it is stated, so endowed with electricity that nobody could enter the room where it was without receiving constant electric shocks. It is stated to have passed away painlessly so far as it was concerned itself, but there is reason to fear that the survivors who attended it must have suffered much, for it is affirmed by the doctors that at the instant of death luminous effluvia proceeded from its body and continued for several minutes after its decease. The Medical Times and Gazette, alluding to the case, says that it is supposed to be quite unprecedented in the world of science.

EXTRAORDINARY CASE OF ELECTRICAL EXCITEMENT . . . . .

Hosford, Willard; *American Journal of Science,* 1:33:394-398, 1838.

The facts stated below, were, by my request, kindly communicated for this Journal by Dr. Willard Hosford, a respectable physician of Orford, New Hampshire, the place where the occurrence happened. Being in that place in September, and finding the belief in the facts to be universal, particularly on the part of persons of judgment and science, (as at the neighboring University, Dartmouth, at Hanover, eighteen miles south,) I became desirous of preserving a record of them.

Dr. Hosford remarks in the letter accompanying his communi-

cation, that abundant evidence from the most intelligent persons is at hand for the support of every point in the case. He observes also, that the appearance of the aurora during which the electrical excitement of the lady took place, "was precisely the same as that described by some gentlemen at New Haven."

Speaking of it Dr. Hosford adds, that "the heavens were lighted with a crimson aurora of such uncommon splendor, as to excite no ordinary emotions in every observer, and we had, he observes, in addition, an electrical exhibition much less dazzling, but more singular and to the parties concerned more interesting."

A lady of great respectability, during the evening of the 25th of January, 1837, the time whe the aurora occurred, became suddenly and unconsciously charged with electricity, and she gave the first exhibition of this power in passing her hand over the face of her brother, when, to the astonishment of both, vivid electrical sparks passed to it from the end of each finger.

The fact was immediately mentioned, but the company were so sceptical that each in succession required for conviction, both to see and feel the spark. On entering the room soon afterward, the combined testimony of the company was insufficient to convince me of the fact until a spark, three fourths of an inch long, passed from the lady's knuckle to my nose causing an involuntary recoil. This power continued with augmented force from the 25th of January to the last of February, when it began to decline, and became extinct by the middle of May.

The quantity of electricity manifested during some days was much more than on others, and different hours were often marked by a like variableness; but it is believed, that under favorable circumstances, from the 25th of January to the first of the following April, there was no time when the lady was incapable of yielding electrical sparks.

The most prominent circumstances which appeared to add to her electrical power, were an atmosphere of about 80° Fah., moderate exercise, tranquility of mind, and social enjoyment; there, severally or combined, added to her productive power, while the reverse diminished it precisely in the same ratio. Of these, a high temperature evidently had the greatest effect, while the excitement diminished as the mercury sunk, and disappeared before it reached zero. The lady thinks fear alone would produce the same effect by its check on the vital action.

We had no evidence that the barometrical condition of the atmosphere exerted any influence, and the result was precisely the same whether it were humid or arid.

It is not strange that the lady suffered a severe mental perturbation from the visitation of a power so unexpected and undesired, in addition to the vexation arising from her involuntarily giving sparks to every conducting body that came within the sphere of her electrical influence; for whatever of the iron stove or its appurtenances, or the metallic utensils of her work box, such as needle, scissors, knife, pencil, etc., etc. she had occasion to lay her hands upon, first received a spark, producing a consequent twinge at the point of contact.

The imperfection of her insulator is to be regretted, as it was

only the common Turkey carpet of her parlor, and it could sustain an electrical intensity only equal to giving sparks one and a half inch long; these were, however, amply sufficient to satisfy the most sceptical observer, of the existence in or about her system, of an active power that furnished an uninterrupted flow of the electrical fluid, of the amount of which, perhaps the reader may obtain a very definite idea by reflecting upon the following experiments. When her finger was brought within one sixteenth of an inch of a metallic body, a spark that was heard, seen, and felt, passed every second. When she was seated with her feet on the stove-hearth (of iron) engaged with her books, with no motion but that of breathing and the turning of leaves, then three or more sparks per minute would pass to the stove, notwithstanding the insulation of her shoes and silk hosiery. Indeed, her easy chair was no protection from these inconveniences, for this subtle agent would often find its way through the stuffing and covering of its arms to its steel frame work. In a few moments she could charge other persons insulated like herself, thus enabling the first individual to pass it on to a second, and the second to a third.

When most favorably circumstanced, four sparks per minute, of one inch and a half, would pass from the end of her finger to a brass ball on the stove; these were quite brilliant, distinctly seen and heard in any part of a large room, and sharply felt when they passed to another person. In order further to test the strength of this measure, it was passed to the balls by four persons forming a line; this, however, evidently diminished its intensity, yet the spark was bright.

The foregoing experiments, and others of a similar kind, were indefinitely repeated, we safely say hundreds of times, and to those who witnessed the exhibitions they were perfectly satisfactory, as much so as if they had been produced by an electrical machine and the electricity accumulated in a battery.

The lady had no internal evidence of this faculty, a faculty sui generis; it was manifest to her only in the phenomena of its leaving her by sparks, and its dissipation was imperceptible, while walking her room or seated in a common chair, even after the intensity had previously arrived at the point, of affording one and a half inch sparks.

Neither the lady's hair or silk, so far as was noticed, was ever in a state of divergence; but without doubt this was owing to her dress being thick and heavy, and to her hair having been laid smooth at her toilet and firmly fixed before she appeared upon her insulator.

As this case advanced, and supposing the electricity to have resulted from the friction of her silk, I directed (after a few days) an entire change of my patient's apparel, believing that substitution of one of cotton, flannel, & c. would relieve her from her electrical inconvenience, and at the same time a sister, then staying with her, by my request, assumed her dress or a precisely similar one; but in both instances the experiment was an entire failure, for it neither abated the intensity of the electrical excitement in the former instance, or produced it in the latter.

My next conjecture was, that the electricity resulted from the friction of her flannels on the surface, but this suggestion was soon destroyed when at my next visit I found my patient, although in a free perspiration, still highly charged with the electrical excitement. And now if it is difficult to believe that this is a product of the animal system, it is hoped that the sceptics will tell us from whence it came.

In addition to the ordinary appurtenances of a parlor, it may be proper to add, that the lady's apartment contained a beautiful cabinet of shells, minerals, and foreign curiosities.

This lady is the wife of a very respectable gentleman of this place; she is aged about thirty, of a delicate constitution, nervous temperament, sedentary habits, usually engaged with her books or needlework, and generally enjoying a fine flow of spirits.

She has, however, never been in sound health, but has seldom been confined to her bed by sickness even for a day.

During the past two years she has suffered several attacks of acute rheumatism, of only a few days' continuance, but during the autumn, and the part of winter preceding her electrical development, she suffered much from unseated neuralgia in the various parts of her system, and was particularly affected in the cutis vera, in isolated patches; the sensation produced being precisely like that caused by the application of water heated to the point a little short of producing vesication; in no instance, however, did it produce an apparent hyperaemia, but about the last of December a retrocession took place of this peculiar irritation, to the mucous membranes of the fauces, oesophagus, and stomach, there producing a very apparent hyperaemia, and attended, during the exacerbations, with burning sensations that were torturing indeed; and it was the for the relief of these symptoms that medical means were used, but it was found no easy matter to overcome this train of morbid action.

It was nearly immaterial what medicines were used; no permanent relief was obtained, and no advantage resulted from the use of the alkalies, or their varied combinations. In a few instances, a dose of the acetate of morphine was given to secure a night's rest, but she seldom made use of an anodyne.

The effervescing soda draught being very acceptable was freely given--from which, in addition to a rigid system of dietetics, the influence of the opening spring, and the vis medicatrix nature, relief came of her electrical vexations, of most of her neuralgia, and other corporeal infirmities, and to this time, a much better state of health has been enjoyed than for many years.

THE ELECTRIC GIRL

Figuier, Louis; *Popular Science Monthly,* 6:588–592, 1875.

In the beginning of 1846, a year memorable in the history of table-turning and spirit-rapping, Angelique Cottin was a girl of fourteen, living in the village of Bouvigny, near La Perriere,

department of Orne, France. She was of low stature, but of robust frame, and apathetic to an extraordinary degree both in body and mind. On January 15th of the year named, while the girl was with three others engaged in weaving silk-thread gloves, the oaken table at which they worked began to move and change position. The work-women were alarmed; work was for a moment suspended, but was soon resumed. But, when Angelique again took her place, the table began anew to move with great violence; she felt herself attracted to it, but, so soon as she touched it, it retreated before her, or was even upset. The following morning similar phenomena were observed; and before long public opinion was very decided in affirming that Agelique Cottin was possessed of a devil, and that she should be brought before the parish priest. But the cure was a man of too much common sense to heed their request for an exorcism, and resolved to see the facts for himself. The girl was brought to the cure's house, and there the phenomena were repeated, though not with the same intensity as before: the table retreated, but was not overturned, while the chair on which Angelique was seated moved in a contrary direction, rocking the while, and giving Angelique great difficulty in keeping her seat. These effects were so remarkable as to attract a great deal of attention; and so many came to see the demonstrations that the girl's relations, who were in straitened circumstances, thought to make a lucrative business of her singular faculty by exhibitions from time to time. Various professional men testified to her performances, of which the following letter from Dr. Beaumont-Chardon, of Mortagne, gives the usual account:

"This is what I saw," says this physician.

"1. Repulsion and attraction, bounding and displacement of a massive table; also of another table, mounted on casters, about three metres by two; another square table, in oak, about a metre and a half in size; an arm-chair, of mahogany, very massive. All these movements took place from the voluntary or involuntary contact of Angelique's clothes.

"2. When she was seated, overturning and repulsion of the young girl and the person who was occupying the same chair; a momentary adherence of the chair to the girl's dress was seen several times. Cessation of these effects when the chair and the young girl were placed upon glass or oil-cloth, or when the girl was placed upon the chair without having the feet of the latter touch the floor, effects generally less upon waxed floors or carpets.

"3. Great disturbance noticed in the girl, recalling that which is produced by an electric discharge, when a piece of wood, a stick, a shovel, or tongs, was brought in contact with the vertebral column. My finger held toward her forehead, or the top, and above all, the back of her head, either by actual contact, or at a distance of two centimetres, produced the same effect as it had done when brought in contact with the elbow of the left arm--disappearance of this effect when a piece of oil-cloth was interposed between the arm and the object.

"4. Painful and insupportable sensation of itching when one or two iron rods, strongly magnetized, were held several centimetres from the extended fingers of her left hand, or from her head; non-magnetized iron did not produce this effect. A magnetized needle, suspended horizontally from the ceiling by a long thread, deviated from the direction of the terrestrial magnetic axis, and oscillated at the approach of the girl's left arm.

"The young girl was generally heavily charged when I was near her, because I did not arouse in her any feeling of mistrust, but always endeavored to spare her suffering; I thought that, in order to appear to the best advantage, her mind must be free, and she herself gay and lively, although her will seemed to be entirely void of influence."

Angelique's relatives resolved, at length, to take the electrical girl to Paris in order to submit her to the examination of the academicians. During the public seance on the 17th of February at the Adademe des Sciences, the secretary, Arago, published the experiments to which Dr. Sanchon had subjected the girl, and read a notice given him by this physician, which appeared in the official Compte Rendu of this session. It is as follows:

> "I have twice," says Dr. Sanchon, "seen the electrical girl (Angelique Cottin). A chair which I was holding as firmly as possible with my foot and both hands, was suddenly torn from my grasp when she sat upon it. A little strip of paper which I balanced on my finger was blown away several times as though by a sudden rush of wind. A dining-table of considerable size, and quite heavy, was several times displaced and pushed by the mere contact of the girl's clothes. A small paper wheel, placed vertically or horizontally upon its axis, received a rapid motion from the emanations which proceeded from the child's wrist and elbow-joint. A very large and heavy sofa, on which I was seated, was violently pushed against the wall when the young girl placed herself beside me. A chair held to the ground by strong persons, on which I sat in a manner so as to occupy but half of it, was suddenly pushed from under me when the girl sat upon the remaining half. A remarkable fact is, that each time the chair was raised, it seemed to adhere to the girl's clothes; she followed it for an instant, and then disengaged herself. Two little balls of elder-wood, or small pieces of quill suspended by a silk thread, were disturbed, attracted, and finally separated one from the other.
>
> "The young girl's emanations were not permanent during the day; they appeared in the evening, from seven to nine o'clock: this gives me the idea that her last meal, which she takes at six o'clock, has some influence in regard to them. They recurred on the anterior surface of the body, particularly at the wrist and elbow. They only recurred in the left side; the arm on this side was warmer than the other; a gentle warmth was radiated from it as from a member on which a vivid reaction takes place. This part of the body was tremb-

ling and continually affected by unusual contractions and quivering, which seemed to be communicated to any hand that touched her. During the time that I noticed this young person, her pulse varied from 105 to 120; it often appeared irregular. When the girl was removed from the common reservoir, either by placing her in a chair without letting her feet touch the ground, or by resting her feet against those of another person standing before, her, the phenomenon did not take place; it also ceased when she put her hands beneath her on sitting down. A waxed floor, a piece of oil-cloth, or a plate of glass, put under her feet or upon her chair, equally destroys her electric faculty.

"During the paroxysm, the girl can hardly touch any thing with her left hand without causing it to be thrown some distance; when her clothes come in contact with pieces of furniture, she attracts, displaces, and finally overturns them. This will be better conceived when the fact is known that, at each electric discharge, she endeavors to escape in order to avoid the result; she says that she always feels a prickling sensation at her wrist and elbow. In searching for the pulse in the temporal artery, not being able to appreciate it in the left arm, my fingers inadvertently touched the nape of the neck; instantly the girl uttered a cry, and disengaged herself from me. In the region of the cerebellum (I have tested this many times), where the muscles of the upper portions of the neck are inserted into the cranium, there was such a sensitive spot that the girl could not bear to have it touched--a spot which retained all the sensations she felt in her left arm.

"The child's electric emanations seemed to occur at intervals, and successively, in different portions of the anterior part of her body. I remarked on this occasion that in the displacement of the table, which requires great force, the electricity was in the base of the brain.

"Whatever these emanations are, they are produced by a gaseous current producing a sensation of cold. I distinctly felt a momentary breath upon my hands, similar to that made by the lips.

"This irregularity in the emission of the fluid seems to result from numerous causes. First, the continual preoccupation of the girl, who always glances behind her when any one or any thing touches her; and, finally, the apprehension which she has of the phenomenon; for, as soon as it becomes manifest, she rapidly endeavors to escape, as though repulsed by some contrary force. It is only when she thinks of nothing, or when her thoughts are otherwise engaged, that the phenomenon is most sudden and intense.

"Each phenomenon is marked by extreme fright in the girl, flight, and a general air of alarm. When she touches the north pole of a magnet with the end of her finger, she receives a violent shock. The south pole produces no effect. The magnet was charged in such a manner that the north pole could not be distinguished from the south; but the young girl knew the difference.

"She is very strong and in excellent health. Her intellectual capacities are but little developed; she is a village girl in every sense of the word.

"Paris, February 15, 1846."

After having read the preceding notice, Arago related all that he had himself observed concerning Angelique Cottin, whom her relations had taken to the Observatory. In the presence of M.M. Mathieu, Laugier, and Gougin, he confirmed the following phenomena: When Angelique Cottin touched a sheet of paper lying on a table it was instantly attracted toward her hand. Angelique approached a table, which was repelled as soon as it came in contact with her apron. She seated herself upon a chair with her feet resting on the floor, and chair was thrown violently against the wall, while the young girl was sent in another direction. This experiment succeeded whenever it was tried. Neither Arago, Gougin, nor Laugier, could hold the chair immovable, and M. Gougin, seating himself in one-half of it, while the girl occupied the other, was thrown upon the floor as soon as she took possession of it.

Arago then asked that a committee should be appointed to examine these phenomena. The Academy selected Arago, Becquerel, Isidore Geoffroy Saint-Hilaire, Babinet, Rayer, and Pariset.

This committee assembled, on the following day, in the Jardin des Plantes, but the experiments were unfavorable in regard to the reality of the electric properties of Angelique Cottin.

Here is the committee's report:

"In the session of the 16th of February the Academy received from M. Cholet and Dr. Sanchon two notices relative to the extraordinary faculty which, it is said, was developed about a month ago in a young girl from Orne, Angelique Cottin, aged fourteen years. The Academy, in conformity with its usual custom appointed a committee to examine these alleged facts, and to give an account of the result. We will discharge this duty in very few words.

"It was affirmed that Mdlle. Cottin exercised a most intense action of repulsion upon bodies of all kinds whenever a portion of her garments touched them. Accounts were even given of heavy tables being overturned by the simple contact of a silk thread. No effect of this kind was manifested before the committee.

"In the narratives communicated to the Academy it was affirmed that a magnetized needle, under the influence of the girl's arm, performed rapid oscilations, and finally fixed itself quite far from the magnetic meridian. When tried before the committee, a needle, delicately suspended in the same way and under the same circumstances experienced neither permanent nor momentary displacement.

"M. Sanchon thought that Mdlle. Cottin possessed the faculty of distinguishing the north pole of a magnet from the south pole, by merely touching them with her fingers. The committee was convinced, by varied and numerous experiments, that the young

girl does not possess the capacity attributed to her of determining the poles.

"The committee need not enumerate these useless attempts. It will simply content itself with declaring that the only one of the alleged facts which was realized before them was that concerning the sudden and violent movements of chairs in which the young girl seated herself. Upon serious suspicious arising as to the manner in which these movements occurred, the committee has decided that they shall be submitted to an attentive examination. It frankly announces that the investigations tended to discover the fact that certain habitual maneuvres hidden in the feet and hands could have produced the observed fact. M. Cholet now declared that the young girl had lost her powers of attraction and repulsion, and that we should be notified as soon as they were restored. Many days have passed since, yet the committee has received no intelligence. We have learned, however, that Mddle. Cottin is daily received in drawing-rooms where she repeats her experiments.

"After having fully weighed the circumstances, the committee is of the opinion that the communications transmitted to the Academy on the subject of Mdlle. Angelique Cottin should be considered as never having been sent in.

DAILY VARIATIONS OF STANDING POTENTIAL DIFFERENCES IN HUMAN SUBJECTS

Ravitz, Leonard J.; *Yale Journal of Biology and Medicine,* 24:22-25, 1951.

Biological and psychological sciences have tended to lag far behind physics in their approach to the nature of design in living systems. To test the hypothesis that protoplasmic systems might also exhibit charges and field forces responding in an orderly fashion to the same physical laws controlling the inorganic universe, Burr perfected a microvoltmeter, with the assistance of Lane and Nims, by means of which he could study basic electrical properties of living creatures. Experiments of the past twenty years have shown unequivocally that in all living membranes, tissues, cells, and organisms, there is a well-defined, relatively steady, stable voltage gradient of considerable magnitude between any two points--altered only by biological changes in the organism--which is characteristic of the species and, to some extent, characteristic of the individual. Lund's experiments furnish additional evidence for the existence of electrical fields and indicate that inherent electrical forces are important determinants of polarity and of organization in all living systems.

To ascertain whether humans evidenced any day-to-day EMF alterations, DC spot determinations were taken daily from two to eight months on ten subjects, seven men and three women. All except one were university students. The 3A5 DC-powered microvoltmeter was used which, employing essentially a Wheat-

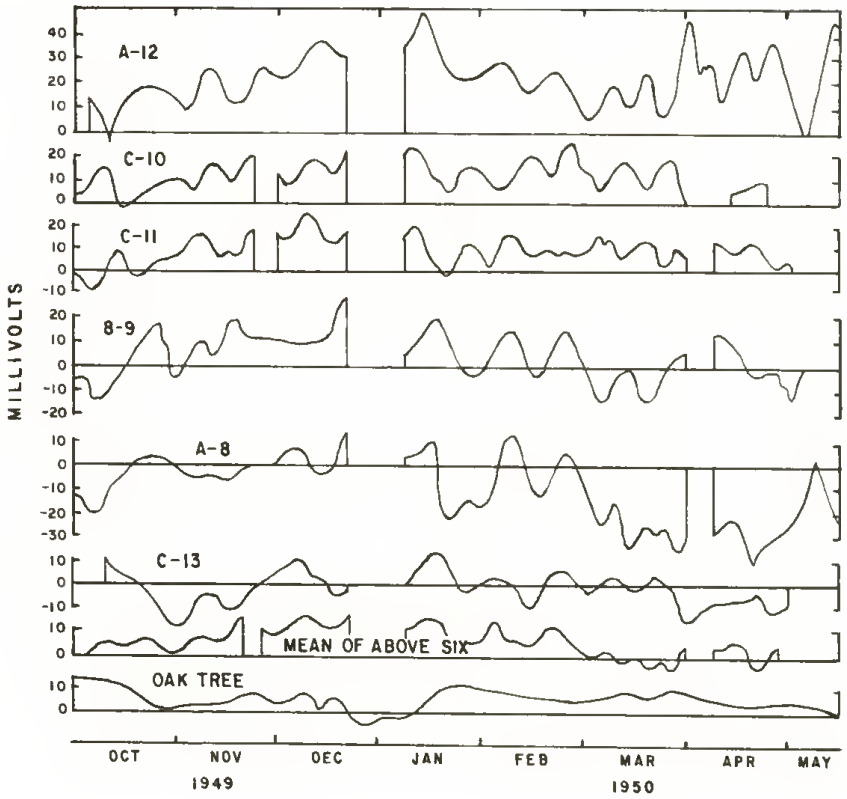
stone bridge circuit, with a 10 megohm input impedance, high sensitivity, and high stability, measures DC potentials as pure voltage gradients independent of current and resistance changes. Reversible, non-polarizing Ag-AgCl electrodes immersed in physiological saline were brought into contact with the subjects. To wash out any errors in single determinations, a bilateral series of four potential differences between head and anterior chest wall were summated:

Grid electrode		Ground electrode
Right temporal region	--1--	right chest, midclavicular line
Right temporal region	--2--	left chest, midclavicular line
Left temporal region	--3--	right chest, midclavicular line
Left temporal region	--4--	left chest, midclavicular line

Two such series were taken at each reading, and the results were averaged both as absolute values and with signs. Care was taken that no metal contacted the individuals; electrode drift, balance, sensitivity, and the possibility of air bubbles in the electrodes were constantly checked. Electrodes were reversed at least every third or fourth reading. Notation was made of local variations in the external environment, e.g., temperature, barometric pressure, humidity, weather, and sunlight, and of body temperature, blood pressure, and pulse of the subjects. Results in millivolts were plotted with and without signs as five-day running averages, deviations of the daily mean from the monthly means, and as monthly ranges (the difference between the highest positive and highest negative daily averages for the month).

In all subjects, 14- to 17-day cycles were present which roughly correlated with lunar phases. In addition, there was evidence to indicate four-month cycles, and in one individual, six-week cycles. As these phenomena varied somewhat among the subjects, combined five-day running averages of the six subjects measured for the longest period of time were plotted. These averages maintained approximately the same cyclic pattern seen in individuals, which, as in individuals, was more marked when polarity was taken into account. These are similar to tree cycles, although they show greater voltage fluctuations and decreased polarity stability. As with older trees, voltage fluctuations were relatively minimal in a middle-aged subject, C-13 (who also showed six-week cycles). No apparent correlation existed between DC measurements and temperature, blood pressure, and pulse, or with local environmental variations. The possibility of recurring instrument artifact was ruled out by attaching electrodes immersed in a column of saline to a microvoltmeter and taking continuous readings for several months without any potential change.

There was a definite long-range tendency for most subjects to increase in positivity (i.e., head positive to chest) from October through the middle of December, 1949, and to decrease in positivity from the middle of January to May, 1950. The head was usually positive or slightly negative to the chest;



Five-day running averages of mean daily potential differences for each of six human subjects and an oak tree.

high negative readings were less frequent than high positive readings. The normal monthly mean was roughly -7 to +18 millivolts, although on occasion it deviated as widely as -29.8 and +34.9 millivolts. The highest daily averages were -70.5 and +69.5 millivolts.

Other than approximate periodicity with the full or new moons (a marked PD increase or decrease occurred at these times, although marked daily deviation from the monthly mean more frequently preceded or followed the lunar day by 24 to 72 hours), the only other correlation with daily DC measurements seemed to be with disturbed emotional states, which for the most part could not be associated with high isolated PD readings, but only with sustained high PD's exceeding -15 or +25 millivolts. Although the cycles appear more significant when polarity is taken into account, monthly means correlate better with emotional disturbances when determined as absolute values, rather than with signs. Deviations of the daily mean from the monthly mean often appeared similar to tree deviations. In many instances the human and the tree potential differences were mirror images. Further

studies now in progress at Duke and at the Veterans Administration Hospital, Roanoke Virginia, reveal the same rhythmic variations.

THE GEORGIA WONDER-GIRL AND HER LESSONS

Newcomb, S.; *Science*, 5:106-108, 1885.

The people of the interior states are now being amused by an exhibition the success of which offers a striking example of the unreliability of human testimony respecting the phenomena of force and motion. Some months since, the writer received a polite invitation to witness the wonderful performances of Miss Lulu Hurst, the Georgia 'magnetic girl,' in causing objects to move as if acted on by powerful forces, without any muscular action on her part. Another engagement prevented his acceptance; but, on the morning following, he received such a description of the phenomenon as to make him regret that he had not sacrificed every thing to the opportunity of seeing it. It was substantially this:--

A light rod was firmly held in the hands of the heaviest and most muscular of the select circle of spectators. Miss Lulu had only to touch the rod with her fingers, when it immediately began to go through the most extraordinary manoeuvres. It jerked the holder around the room with a power which he was unable to resist, and finally threw him down into one corner completely discomfited. Another spectator was then asked to take hold of the rod; and Miss Lulu, extending her arms, touched each end with the tip of a finger. Immediately the rod began to whirl around on its own central line as an axis, with such rapidity and force that the skin was nearly taken off the holder's hands in his efforts to stop it. A heavy man being seated in a chair, man and chair were both lifted up by the fair performer pressing the palms of her hands against the sides of the back. To substantiate the claim that she herself exerted no force, the chair and man were lifted without her touching the chair at all. The sitter was asked to put his hands under the chair; the performer then put her two hands around and upon his in such a way that it was impossible for her to exert any force on the chair except through his hands; yet the chair lifted him up without her exerting any pressure heavier than a mere touch upon his hands. Several men were then invited to hold the chair still. The performer began to deftly touch it here and there with her fingers, when the chair again began to jump about in the most extraordinary manner, in spite of all the efforts of three or four strong men to keep it still or to hold it down. A hat being inverted upon a table, she held her extended hands over it. It was lifted up by what seemed an attractive force similar to that of a magnet upon an armature, and was in danger of being torn to pieces in the effort to keep it down, though she could not possibly have had any hold upon the object.

This was the account of the performance given, not by a gaping crowd nor by uncritical spectators, but by a select circle of educated men. To the reminder that no force could be exerted upon a body except by a reaction in the opposite direction upon some other body, and to the question upon what other body the reaction was exerted, the narrators expressed themselves unable to return an answer. All they could do was to describe things as they had seen them. Of only one thing could they be confident: the reaction was not exerted through or against the body of the performer. Among the spectators were physicians and physiologists who grasped Miss Lulu's arms while the extraordinary motions went on without finding any symptoms of strong muscular action, and who, feeling her pulse after the most violent motions, found that it remained in its normal state. Apparently the objects which she touched were endowed with a power of exerting force which was wholly new to science. Altogether, the weight of evidence seemed as strong as in the best authenticated and most inexplicable cases of 'spirit' manifestation, while none of the obstacles to investigation connected with the latter were encountered.

Such was the case as it appeared on a first trial; but the spectators were not men to be satisfied without further investigation. Accordingly, they had made arrangements with the managers to have another private exhibition at the Volta laboratory two days later. They proposed also to have decisive tests to determine whether or not she exerted any force upon the objects which she moved.

The party duly appeared at the appointed time. At this point I think it only just to mention the perfect frankness with which the most thorough investigation of the case was permitted by those having the exhibition in charge. There was no darkening of rooms, no concealing hands under tables, no fear that spirits would refuse to come at the bidding of a sceptic, no trickery of any sort. The opportunities for observation were entirely unrestricted.

Miss Lulu was a rosy country girl, somewhat above the average height, but did not give the impression of muscular training; still, when she was presented to those present, the first thing which struck the writer was the weight of her arm. Shaking hands with her felt like moving the arm of a giant, and led to the impression that she had a much better muscular development than would have been supposed.

Before proceeding to the tests which had been pre-arranged, it was thought best to try what she could do under ordinary circumstances. Among the first performances to be tried was that of the hat. A spectator held a light straw hat in his hands, the opening upwards. Miss Hurst extended her hands over it so that the balls of her thumbs just touched the inner face of the rim. At first there was no result, but after a few trials the hat was gently attracted upwards as if by electricity. Had those in charge been professionals, I can not doubt that they would have stopped right there, and declined to repeat the performance. Not being such, they yielded to the invitation to go on, so that the

holder could see how it was done. This was soon effected without difficulty. Whenever the apparent attraction was exerted, it was through the inner edge of the brim being caught in the fold of the ball of the extended hand. After a few moments the observer was enabled to say, "She cannot lift it now, because her hand is not rightly arranged", and he learned to adjust her hand so that the lifting could be executed. Of course, the force was not very strong. The idea that the hat would have been in any danger had a weight been in it was simply a mistake.

Next the jumping-staff was tried. The writer took the latter in his hands, and Miss Lulu placed the palm of her hand and her extended thumb against the staff near its two ends, while the holder firmly grasped it near the middle. He was then warned to resist with all his force, with added assurance that the resistance would be vain. Sure enough, the staff began to be affected with a jerking motion, producing the disastrous effects which had been described upon the holder's equilibrium. An unwise repetition of the performance, however, did away with all its mystery; for, although the performer began with a delicate touch of the staff, the holder soon perceived that she changed the position of her hands every moment; sometimes seizing the staff with a firm grip, and that it never moved in any direction unless her hands were in such a position that she could move it in that direction by ordinary pressure. An estimate of the force which she exerted on the staff could be roughly made. It might have

The Georgia Wonder Girl

been as high as forty pounds. A very little calculation will show that this would be sufficient to upset the equilibrium of a very heavy man. It is impossible for the latter so to place his feet that he will be supported on a rectangle of more than one foot in breadth. He may indeed change at pleasure the direction of the longer side of this rectangle by extending his feet in different directions; but, arrange them as he will, his base will under any circumstances be a rectangle who length is equal to the distance between his feet, and whose breadth is at the very maximum equal to the length of his feet. A pressure of one-fifth his weight would, under the most favorable circumstances, throw him off his balance, and make a new adjustment necessary. The motion given by the performer to the rod was not a regular one, which could be anticipated and guarded against, but a series of jerks, first in one direction, and then in another; so that it was impossible for the holder to brace himself against them: consequently, by a force which might not have exceeded forty pounds, he was put through a series of most undignified contortions, and finally compelled to retire in total defeat.

The holder of the rod then asked that it might be made to whirl in his hands in the manner which had been described to him. No attempt was made to do it, and no satisfaction on the subject could be obtained. It was evidently a simple mistake in memory or narration, for not even Miss Lulu seemed to have any idea of producing such an effect. The lifting of the chair with the sitter's own hands under it, and Miss Lulu's hands under his, was then tried. It was quite true that the fingers of the performer were under those of the sitter. But the chair refused to budge until the ball of her hand came firmly in contact with it; and then it proceeded, not indeed to lift the sitter, but to incline itself in such a way that he felt compelled to get out of it. The chair was made to repeat its performance a great number of times. The writer watched most carefully, and, in every instance in which he was able to see the performer's hands at the time of the motion, the ball was pressed firmly against the chair, and the direction of motion was that of the pressure.

Three men, or indeed as many as could get hold of the chair, were then invited to hold it still if they could. This was the most amusing and exciting part of the spectacle. The men tried in vain to hold the chair still, while Miss Lulu simply moved around in the quietest imaginable way, touching it with her finger first here and then there, until finally the force became so great that the chair began to crack, and seemingly almost pull itself to pieces. The explanation was, however, perfectly obvious. There was no concert of action among the four muscular holders, more than that each one tried to keep the chair still by resisting any force which he felt it to exert. A few jerks in various directions by the performer led them to begin resisting her motion by pulling the chair first this way and then that. It was of course impossible for any one holder to tell whether the motion came from the performer or from his companions. The result was, that they all began to wrench desperately against each other until the chair came to pieces.

The scientific tests were productive of the usual result,--that ghosts, spirits, and occult forces absolutely refuse to perform their functions in the presence of scientific paraphernalia. A platform had been placed on rollers in the middle of the room and Miss Hurst was invited to set the rod in motion while she stood on that platform. Her parents were perfectly confident that she could do it, and she did go so far as to commence one feeble attempt; but the forces refused to operate, or rather the platform persisted in rolling about, and the attempt had to be given up. She then stood upon the platform of a pair of scales, the counterpoise of which was so adjusted, that, when she exerted a lifting-force exceeding forty pounds the arm would be raised. A spectator sat in the chair in front of the scales. It was soon found, that, owing to the platform being some six inches above the floor, the chair was lower than she had been accustomed to have it: it was therefore set upon a little platform of the same height as that of the scales, so that the position was the same as if both stood on the floor. The performer pressed her hands against the sides of the back of the chair, according to custom. The motion was long in commencing, and, when it began to appear, click! went the lever of the scales, showing that a force of more than forty pounds was exerted. This seemed to demoralize the performer, and, notwithstanding a great deal of chiding from her parents, nothing more could be done while she stood in this position.

From various allusions in the public press, it would seem that the wonderful 'magnetic girl' has not yet ceased to draw full houses. The editor of the Chicago Inter-ocean made a careful investigation of the case, and showed that it could not possibly be electricity which caused the motion; but he does not essay an explanation of what the force was.

Although it would be unjust and pretentious to say that no one sees the absurdly simple character of the performance, it would appear that there are many who are mystified by it, and that, should we accept the existing testimony on the subject as complete, we should be compelled to admit that some new form of force had been discovered. It is indeed possible that the absurd simplicity of the affair may help to give it vitality; for, as already indicated, not only is there no mystery or concealment, but there is not even a resort to the tricks of legerdemain, which consist very largely in distracting the observers' attention at the critical moment. The assumption, that, because Miss Lulu begins by touching the articles deftly with her fingers, she never takes them with a firm grip, is one which the spectator takes upon himself without any effort on the performer's part to cause that illusion.

This account is presented to the readers of Science, because, taken in connection with descriptions of the performance given by thousands of spectators, many of them critical observers, it affords the basis of a reply to those who have seen chairs, tables, and pianos dance without human agency.

THE GEORGIA WONDER-GIRL AND HER LESSONS

Shufeldt, R. W.; *Science*, 5:189-190, 1885.

I read with no little interest the article with this title which appeared in this journal on Feb. 6.

I was privileged to make a private examination of Miss Lulu Hurst, the person referred to in the article, on several occasions, in the presence of her parents, and usually of her business-manager. On one occasion I was permitted to make a careful examination of the subject's physical development, and take notes upon her normal temperature, heart-beat, and respiration. I found her to be a healthy, intelligent country-girl, plump rather than muscular, presenting nothing very unusual in her constitution; and I certainly did not note the fact that I might be shaking hands with a 'giant.' The muscles of her arm and fore-arm were not unusually developed; nor did they stand out prominently, as they do in muscular subjects of either sex. She is above the average stature for women, but does not strike one as being either exceedingly active in movement or overpowerful in frame; as to the former, rather the reverse, I think.

Of the experiment with the staff, I shall simply state that in my case, on two occasions, the staff gyrated rapidly about its long axis, obliging me to quit my hold. This was observed by other persons present during the experiment. In the test with the hat, Miss Lulu stands before you with her hands extended horizontally, palms up, with the little fingers and sides touching each other. On the surface thus presented we place our hat, with the outer aspect of the crown resting on the two palms. The experimenter is then invited to lift the hat off. When I tried this experiment, the hat was only removed after considerable force was exerted, and then came away with a crackling noise, as if charged with electricity. That Professor Newcomb's explanation would not account for the result here, I would say that I knelt in such a position that my eyes were but a short distance away; and my line of vision was in the same plane with the opposed palmar surface, and the crown of the hat. This latter was of very light Manila straw, with the outer periphery of the crown rounded. Now, as the form of this surface was a broad ellipse, with a major axis of perhaps seven inches, and a minor axis of six, quite smooth, it would be simply an impossible feat for Miss Lulu to seize it when the distance between the inner margins of the opposite thenar eminences in a right line is less than six inches.

Permit me now to present a test which Professor Newcomb did not witness. It consisted in standing upright, with one foot in advance of the other to act as a brace, and holding in the hands with a firm grasp an ordinary chair. This is to be done by seizing it at the rear uprights, about where the back joins the bottom; the former being towards you, and parallel with your anterior chest-wall, against which you place your elbows at a convenient distance apart. This position evidently leaves a space between your chest and the back of the chair equal in length to

your fore-arms, which are extended horizontally. Miss Lulu now takes a position beside you, and, holding her body back, simply places the palmar surface of her hand on the back of the chair on the side towards your body. After a few moments she seems to make the effort to detach her hand from the chair, which latter you are privileged to push forwards. The force at work, however, is too strong for you, and both yourself and the chair are carried backwards, without her hand having changed its position. The chair being a cane-backed one, it is evident that she could in no way gain a hold upon it, and the back of her hand never could come in contact with your chest, as the spanning of such a distance would at once be detected.

Professor Newcomb's conclusions, after having witnessed the test of lifting a chair with someone sitting in it, are to me far from satisfactory. I saw the girl lean over an ordinary chair, with a man weighing over two hundred pounds sitting in it, and placing the palmar surfaces of her hands on the outer sides of the rear uprights near their middles, and without any contraction of the muscles of the arm or fore-arm, or increase of pulse (remained at 80) or respiratory effort, or change of countenance due to exertion, so far lift that chair and its heavy contents from the floor as to compel the latter to get out of it; and this without fracturing any of the bones of her upper extremities, or the sides of the chair. The simplest computation will prove that the lateral pressure required must be enormous in order to get a hold, and prevent such a weight absolutely slipping between her hands when the upward force comes to be exerted.

ANOTHER MAGNETIC MAN

Hall, E. W.; *Science,* 16:221, 1890.

My attention has been called to the following account from a Lewiston paper, dated Sept. 25, of a scientific phenomenon in which your readers may be interested.

"The writer was entertained Friday evening by a wonderful man, a resident of College Street. The gentleman is a well known citizen of most trustworthy character. After an evening's performance he feels exhausted in the morning. He can do nothing with the palms of his hands on the object, but must use the tips of his fingers only. He first began with a common table with swinging sides. Placing his fingers nearly in the centre, he could cause either end to rise and remain suspended. It would rock, beat time to music, or turn a complete somersault. No part of his person touches the object but his finger-tips; and there are no secret wires, for we examined the table carefully. Next he let down the swinging sides, which are on hinges, and, by placing his finger-tips, could raise the leaf, and hold it in that position several minutes while we tried to pull it away from his fingers. Then we took a heavy braided rug, and folded it in

four thicknesses, and placed it on the table. With this obstruction he easily lifted the table, and turned it completely over. A plate was put upon the table, and this proved no hinderance. Then a tin dish inverted was given to him, and still the table not only came up, but the tin dish stuck fast to the table. He lifted chairs and other objects while resting. Then the table was inverted; and, placing his finger-tips upon the table-legs, the heavy table came up, and remained clearly suspended from the floor, with one foot of clear space beneath.

"The writer then sat upon the end of the table, which came up so suddenly as to throw him off upon the floor. At last came a strong test. We seated ourselves in chairs at opposite sides of the table, the writer grasped the table-legs with all his strength, and the gentleman stood clear of the table, and, placing only his fingers upon the smooth surface, drew the table so violently as to bring us down upon the floor in confusion.

"Finally we grasped hands and tested our natural strength in pulling, and the writer was the stronger man; but, as the current came down in his arms, it went up into ours, feeling just the same as when we hold the handles of a battery. Then the strength of the gentleman was wonderful, throwing us around the room as one would handle a toy. The whole evening was filled with these performances. There is no possible chance for deception, and those who have seen this say that the only motive power which these objects receive comes directly from the finger-tips of this man. He can perform the same feats in any room, or with any soft-pine table, which may be placed in any position."

• Effects of Electricity on Human Health

CASE OF A PARALYTIC AFFECTION, CURED BY A STROKE OF LIGHTNING

Olmsted, D.; *American Journal of Science,* 1:3:100–102, 1821.

The following case of recovery from a paralytic affection by a violent stroke of lightning, was first mentioned to me by a very respectable gentleman in whose hearing I had recited the well known galvanic experiments, performed by Dr. Ure, of Glasgow, on the body of a culprit. My informant not having had opportunity to investigate the facts, was so obliging as to direct me to such sources of information as could be relied on: and I have since been favoured with letters from the individual himself, and from respectable gentlemen in his neighborhood. Common report, as usual, had represented the case in the most marvellous colours,

from which it would appear, that tottering and wrinkled age being restored, in an instant, to vigorous and blooming youth, was no longer a matter of fable. According to this authority, "the patient (Mr. Samuel Leffers of Carteret County, N.C.) having reached a very advanced age, and suffering so severely under a paralytic affection that his feet were unable to support him, and his face was greatly distorted, acquired at once the full activity of his early years, and a remarkable smoothness and beauty of complexion. This complete exemption from decay and infirmity, and the entire possession of his intellectual faculties, he had retained ever since, during a period of fourteen years, which had brought him upon the verge of four-score years and ten."

Desirous to ascertain how much of this story was matter of fact, I commenced a correspondence which finally introduced me to Mr. Leffers himself, who is still living on the eastern coast of this state, and has attained the age of eighty-four years. As was anticipated, the marvellous circumstances reported of the case have dwindled away to a small compass; but enough, perhaps, remains to render the case somewhat interesting to those who cultivate the study of medical electricity.

I beg leave to give the facts in the language of Mr. Leffers himself, only premising, that I have received the most ample and satisfactory testimonials of his perfect integrity, particularly from the Rev. Mr. Arendell of Beaufort, who characterizes his life "as affording a model of every virtue." The presumption, also, that the facts, as related by himself, may be relied on, is strengthened by the simplicity of his narrative, divested as it is of all those marvelous appendages, with which common report had amplified and embellished the story. The account is as follows.

> In the summer of 1806, waking after a night of quiet rest, I felt an unusual numbness in the left side of my face. I was not alarmed, thinking it might be occasioned by lying too long on that side; but on rising, I felt the effects more painfully---I could not throw the spittle from my mouth; and found great difficulty in speaking; my eyelid was permanently fixed, while the eye remained open, and I was unable to close it. These symptoms made me apprehensive of having received a stroke of the palsy. After some time, the disorder abated in other parts, and centered in the eye, which, remaining uncovered both by day and night, was exposed to constant injury.
>
> Such was my situation until the 10th of August following, when, as I was walking my floor during a thunderstorm, I was struck down by lightning. After lying senseless fifteen or twenty minutes, (as it appeared,) I revived so far as to be sensible of my situation, and to perceive the objects around me. I recovered the use of my senses and of my limbs, by degrees, during the remainder of the day and night, and felt so well the next day, that I was inclined to give to a distant friend, an account of what had happened. I expected my letter would be short and imperfect through want of eye-sight;

but was most agreeably surprised to find myself able to write a long letter without the use of glasses. Since that time, I have not felt a symptom of the paralytic disorder, but have reason to conclude that it was effectually cured by the shock. But I have reason to think, that the same cause which restored my sight, impaired my hearing, since a deafness commenced at the same time, which has continued to the present hour.

CURE OF ASTHMA BY A STROKE OF LIGHTNING

Emerson, Ralph; *American Journal of Science*, 1:6:329-330, 1823.

I know not whether electricity has ever been tried for the relief of persons afflicted with Asthma. If not, perhaps the following circumstances may suggest the propriety of making the experiment.

One year ago last August, Mr. Martin Rockwell of Colebrook, Conn. was severely affected by a shock of lightning, which struck his buildings within about ten feet of him. He was standing at the time in a leaning position, looking out at a window, bearing most of his weight on his left foot, and supporting himself by his right arm, with his hand on a moist platform connected with a sink, and these together forming a connection with the part of the building where the charge fell. Without his either seeing the flash or hearing the noise, his right arm and left leg were instantly paralyzed, and sense and reason were for a few moments suspended. On recovery, his first thought was that his arm was gone, and he put his left hand to feel if it were yet on him. He did not recover the use of his arm or leg for an hour; and they continued sensibly affected for some days. No other part of his body was particularly affected, except the chest. He felt a strong sensation at the lungs, and they continued sore for a number of days. I state these circumstances, as they evince that a heavy charge of electricity passed through the vitals.

Mr. R. is now fifty years of age, and from his youth had been so subject to the asthma as to be often unable to rest in bed for a number of months together, especially in autumn. Since this event, however, he has been entirely free from it, except in one or two instances he has felt a very slight degree of it, after great fatigue, and under the pressure of a severe cold. He has now passed the second autumn in health since this kind preservation of life and removal of disease.

. . . . .

THUNDERSTORMS VITIATE ASTHMA TREATMENT

Anonymous; *Science News Letter*, 27:68-69, 1935.

Thunderstorms and asthma have baffled a group of scientists at the University of Illinois Medical College, Chicago.

These investigators, Drs. Tell Nelson, B.Z. Rappaport, William H. Welker and A.G. Canar, know they can relieve asthma sufferers by putting them in an air-conditioned ward or room, but they are up against a blank wall as to why the thunderstorm sets the patients back. Some factor other than pollen, temperature, humidity and ozone must play a part in bringing on asthma attacks, they believe.

For several seasons they have carried on investigations of the effect of both filtered air and air-conditioning on asthma.

Filtering out the offending pollens helps the asthma sufferers materially, they found. Keeping the humidity low and the temperature relatively constant helps even more, they have reported to the American Gas Association. But even patients who were free of symptoms developed attacks of asthma while in the air-conditioned ward shortly after a severe thunderstorm. Patients in the air-conditioned ward, however, suffered less severe attacks after the thunderstorm and recovered more quickly than patients who had been in a room with filtered but not conditioned air.

The effects of barometric changes and ionization in relation to thunderstorm-induced attacks have not yet been studied, the investigators state.

ATMOSPHERIC ELECTRICITY AND DISEASE

Perry, John; *Nature*, 61:471-473, 1900.

Last summer I had the honour of making the acquaintance of Dr. Schliep, of Baden-Baden. He is well known to English medical specialists. He urged me to design a recording electrometer, such as would enable medical men to study atmospheric electricity. I found that he himself had made daily observations for twenty years, using a gold-leaf electroscope, which enabled him to say whether the air had strong or weak, positive or negative, electric potential, at the end of a water-dropping collector. He showed me that he had made an earnest study of the connection between atmospheric electricity and diseases, and I am convinced that his conclusions are of great importance. I feel, therefore, that I am doing a service in bringing before the notice of readers of Nature the following account of a paper, by Dr. Schliep, in Sonderabdruck aus Deutsche Medizinal-Zeitung.

He first refers to the meteorological observations usually made, and goes on to say that our knowledge of atmospheric electricity is now as vague as was the knowledge of warmth before the ther-

mometric observation became systematic. Dove, in 1837, and Humboldt, in his Cosmos, mention the importance of the study of atmospheric electricity. Dr. Graves, of Dublin, made observations and said: "Practically these experiments are of importance, because some causes of the periodicity of certain acute diseases, their decrease and increase at certain hours of the day, may be deduced from them." Hufeland also refers to this matter. Dr. Buzorine, of Wurtemburg, in 1841, drew attention to the fact that during the cholera epidemics of the third decade of this century, there was a prevalence of negative electrification of the atmosphere. Dr. Pallas, a French physician, wrote on this subject in 1847, and Dr. Craig, an Englishman, wrote about it in 1859.

Dr. Schliep now describes his method of observation with the gold-leaf electroscope, and gives the following results. The first part of these may be said to be well known to us. What seems to me of most importance is the effect on organisms.

Atmospheric electricity is generally positive. If the sky is covered, the potential decreases or shows variations, and is from time to time negative. During rain, negative potential is often observed. The approach of a thunderstorm is generally marked by great alteration towards the negative, followed by considerable oscillations in both directions, with a predominance of negative. Usually the positive potential is higher and more regular during the night than during the daytime. From 9 p.m. to 3 a.m. the potential changes little. It diminishes by daybreak, reaches its lowest value at 3 p.m., then increases and reaches the maximum at 9 p.m. There is, therefore, a minimum during the day, and an almost constant maximum during the greater part of the night; that is to say, there is only one daily period. These facts are deduced from the use of the registering apparatus of Mascart. Other observers have found two maxima and two minima, but they are probably only accidental variations. In every month there are a number of days on which negative electrification can be observed, others, and they are rare, when there is scarcely any electrification noticeable. On most days there is positive potential.

According to Marie Davy's observations in Paris, and Dr. Schliep's at Baden, there are two days of positive electrification for 28 negative. The winter shows higher potential than the summer.

Many terrestrial phenomena, such as earthquakes, are said by trustworthy observers (Schubler, Humboldt) to greatly influence atmospheric electricity. After an auroral display there is strong positive electrification. At greater elevations, especially on steep and high mountains, the electrification is greater.

Dr. Schliep makes the following statements about the influence of atmospheric electricity on human beings:--Negative electrification is tiring, positive is exciting. Positive is favourable to the process of oxidation, increases metabolism, circulation and secretion. It may be that the increased formation of ozone has an influence in this way also, but we can imagine a direct stimulating influence of positive electricity on the nervous system. We may affirm the existence of this influence as, during strong electrifi-

cation, disturbances of the normal condition are noticeable, as in sleeplessness, the existence of states of anxiety, hysteria, neuralgia, and even sometimes inflammation of the respiratory organs. One interesting confirmation of this opinion is found in the observations which Eyselein has made regarding the behaviour of nervous people, as influenced by the amount of ozone in the air. It seems that if there is too little ozone, and especially if it completely and suddenly disappears, there is considerable bodily disturbance; whilst its sudden reappearance causes a quick return of healthy feeling. It has also been proved that a continuance of much ozone is not unfavourable to health. Ozone intensity less than No. 10 of Zender's scale, but not much less, has a tonic effect on nervous people, but intensities from 9 to 4 cause disturbances. These facts agree with the observations I have made in regard to the health of my patients, as affected by atmospheric electricity. From these observations I conclude that a certain amount of nervous disorder, as well as a power of resistance, are associated with positive electrification. As in many other cases, there is therefore in this instance the possibility of having too much of a good thing.

Unhealthy symptoms, unfavourable to tissue-change accompany negative electrification. Feelings of fatigue and lassitude, exhaustion of the nervous system, arrest of perspiration, loss of tone in the blood-vessels, accompany negative electrification. Congestion bilious and apoplectic attacks and haemorrhages are the results. The development of bad gas, processes of decomposition, and increase of bacilli are the accompanying phenomena. Certain forms of disease, as angina, pneumonia, herpes, may, to extents depending upon local conditions, increase with negative electricity, and seem to be related to the souring of milk, the decomposition of meat, and the development of bad smells in the street gutters and drains. If we say that the bacilli are the cause of these things, it may be true; but it does not explain why bacilli find more favourable conditions for their existence on some days than on others with equal warmth, moisture, air-pressure, &c. Dr. Schliep goes on to say that we get clearer notions if we consider the difference between animal and plant metabolism.

We know the astonishing effect of a close thunderstorm-day on vegetation, the sudden breaking forth of buds, leaves and flowers, the quick development of the young seed, and the sometimes rapid growth of such plants as asparagus. Light, warmth and moisture are of course the first conditions. The observation of this remarkable phenomenon gave rise to an interesting experiment of Becquerel. He selected four hyacinth roots of equal size and sort, which he put in a weak salt solution, two in a frame of glass, the third in a frame of zinc, and the fourth in a frame of copper. The copper and zinc were attached to each other by a wire. The vegetation developed most at the negative pole, less in the neutral frame, and was least at the positive pole. It seems that the roots of plants need a negative electric medium, and the crust of the earth is constantly negative. What increases the tissue-change in plants, decreases that of the ani-

mal organism. Thus, very often, days good for vegetation become tiresome for us. Walking in the streets causes great fatigue. All animals are tired on these days. They are the days of negative atmospheric electricity, days on which the bacilli are triumphant; wounds become septic, and germs of epidemic diseases find favourable conditions for development. It will concern bacteriology to pay attention to the facts. In balneometeorology, the most important object is the influence of atmospheric electricity on the anomalies of the constitution. From its better study we shall be able to derive hypotheses for our hygenic and therapeutic study, and besides the importance of geographical position, warmth, moisture, &c., atmospheric electricity will also play an important part in the classification of climates. We shall not only have to distinguish between land and sea climates, wet and dry climates, cold and warm climates, but we shall also have to characterise a climate by its electrification and define with greater exactness the terms "relaxing" and "bracing."

Perhaps we shall also be able to speak of a "spending" and a "saving" climate. We must not separate one characteristic of the climate from another and prefer it; in nature all phenomena work more or less together, they depend on one another and exercise mutual influences on one another. The electric conditions of the air are indicated by other meteorologic records, and hence we have important sources of information which ought not to be neglected, as our methods of making direct electric observations are not yet satisfactory. One can, from the daily increase or diminution of pressure, warmth and moisture of the air, say something of its electrification.

In this connection it is of no importance whether the barometer is high or low, but whether it rises or falls. It is not important to know whether the moisture of the air is great or not; it is important to know whether the moisture decreases or increases, whether the process of condensation or of evaporation prevails.

Dr. Schliep here described at length the meaning of dew-point in hygrometric observations. He exhibited also a reduction disc made by Lambrecht, of Gottinger, a sort of circular slide rule, to faciltate the reduction of observations. He showed that the atmospheric electrification becomes negative if the average temperature and dew-point fall whilst the barometer rises, one may assume a positive electrification. He pointed out on the curves which represent his registrations at Baden during the previous ten years, that the air-pressure on one hand, and the temperature and moisture on the other, altered mostly in opposite directions. It was noticeable also that an exceptional steadiness for a few days was accompanied by the reverse of these movements as soon as the lines went far asunder. The graphic representations of meteorologial phenomena show more than one would think at first sight. More plainly than lists of numbers, they allow a comparison of climatic conditions of different years or of certain periods with the statistics of the prevalent diseases during those periods.

Without a good graphic representation such statistics are never complete, however valuable the material which has been collected

may be. Thus, for example, consider the work of Hippius, published in the Archives for Clin. Medic. vol. xl., about dysentery and meteorological influences upon it, in which there was an inquiry about the relation between meteorological changes and bleedings of the lungs with no apparent result. May not the failure of this inquiry be due to the fact that the meteorologic information was incomplete?

. . . . .

HEALING BY ELECTROMAGNETISM—FACT OR FICTION?

Bentall, Richard; *New Scientist*, 70:166-167, 1976.

In recent years a pulsed, high frequency electomagnetic field source has become available for treating soft tissue injuries, such as sprained ankles, strained shoulders and swollen joints. Although it is not clear who first introduced the technique, it seems to have emerged in the 1940s when physicists were investigating ways of reducing the hazards of deep-tissue heating associated with continuous short-wave therapy for sprains etc. Although a wealth of clinical success is claimed with pulsed-EM therapy, few investigators have offered any explanation of how it is effected. That we have little statistical evidence doesn't help either. Certainly the interaction of electromagnetic energy and the living organism has too long been shrouded in mystery.

The Diapulse Corporation of America produces a pulsed electromagnetic field generator with an output in the very high frequency (VHF) band, suitable for medical use. It is gaining increasing use in both conventional and heterodox medical centres, especially for reducing tissue swelling following injury. Dr. Abraham Ginnesburg, in New York, has shown for example that the calcification which develops with swollen joints (bursitis) disappears with Diapulse therapy. Over the past few years experimental work on wound healing suggests that there are grounds for using pulsed-EM in a wide variety of clinical situations. Bruce Cameron, at Travis, Houston, found that when he treated experimentally produced skin wounds with pulsed-EM, the excess tissue fluid (oedema) associated with cellular damage was reduced (American Journal of Orthopedics, November 1961, p. 336). Subsequently Dr. E.B. Chung from Howard University, Washington, has shown that this therapy produces fewer leucocytes within the damaged tissue and a quicker formation of new skin in burns.

. . . . .

One possible way in which electromagnetic energy may affect a biological system is in repolarising a partially depolarised cell system, as in an injured cell. This may be linked at ionic level to the maintenance of the cell membrane potential by the so-called "sodium pump". In the late 1930s, H.S. Burr at Yale was demonstrating, recording and analysing electrical skin potentials during wound healing. Although the work has been updated and

become more complex, the findings correlate with the general picture of ionic changes associated with cell injury. Innervated tissue heals better than does a denervated tissue. Thus pulsed-EM may assist healing by affecting the local nervous system rather than through any effect on the cells alone. In any repair and injury situation, bloodflow and oxygenation at the micro-capillary level are extremely important to would healing. It is possible that changes in the nerve supply to the very small vessels that distribute blood to the tissues, and the direct action of the field on the minute tissue vessels themselves, produce the beneficial effects.

. . . . .

• Human Health and Astronomy

SUN-SPOTS AND THE PLAGUE

Jevons, W. Stanley; *Nature*, 19:338, 1879.

Apropos of the plague--I do not know whether the following curious coincidence has been noticed. In that admirable work, John Graunt's "Natural and Political Observations upon the Bills of Mortality" (second edition, London, 1662), which is probably the earliest treatise on vital statistics, I find the following statement (p.31):--"There have been in London, within this age, four times of great mortality, that is to say, the years 1592 and 1593, 1603, 1625, and 1636." He shows that large numbers died of the plague in each of these years. Now, if we take the solar period to be 10½ years, nearly in accordance with Dr. Lamont's and Mr. J.A. Broun's estimates, we get the subjoined table, which sufficiently explains itself.

Corresponding solar years.		
1592·5 ...	Plague in London	1592-3.
1603 ...	" "	1603.
1613·5		
1624 ...	" "	1625.
1634·5 ...	" "	1636.
1645		
1655·5 ...	Great Plague in Naples	1656.
1666 ...	" of London	1665.
:		
.		
1718·5 ...	" at Marseilles ...	1720.

If this particular conincidence has not already been pointed out, it deserves notice as supporting the theory that the rate of mortality is remotely connected with the solar period. There may be several chains of causation leading to the increase of mortality, but one chain is doubtless through the Asiatic famine, which would naturally develop the worst forms of germ disease.

SOLAR ACTIVITY AND HUMAN HEALTH

Anonymous; *New Scientist,* 34:8, 1967.

Soviet physicists at a meeting in Moscow last week heard Dr. Clara Novikova from Sverdlovsk put forward the suggestion that there may be a statistical connection between the incidence of ill health and death and the level of solar activity. Her analysis was derived from statistics amassed over the past 22 years.

Mstislav Gnevyshev, director of an observatory in the North Caucasus, said the solar effects would be reflected in the fluctuations of the Earth's magnetic field and he suggested that a closer study be made of people with a heart condition to discover any relationship with the periods of fluctuation. Biologists at Irkutsk claim to have found changes in the properties of the blood of healthy people at such times. The effects of climate on health are receiving increasing attention from European medical scientists and the Moscow conference called for further studies.

SEASONAL PEAKS OF COMMON COLD INCIDENCE AND SUNSPOT ACTIVITY

Cogan, L. Peter; *Cycles,* 24:183–184, 1973.

Studies of the common cold indicate that two peaks of incidence often occur during the year--a major one, on average, between September and October, and the other between February and March. The peaks of incidence, noted in a study by Torney and Lake of employees of Macy's department store in New York City, occurred in October and February. Downes found a big peak in September and one in February-March among persons of ages 19 and over as well as ages 5-18, in a study conducted in Westchester County, New York. Dingle found a September peak among a Cleveland, Ohio group. On the basis of years of research, Dr. Perrin Long of the Department of Medicine of the Kings County Hospital, considers the phenomenon of the September-October and February-March peaks, rather than one continuous peak from September to March, as one of the greatest mysteries of the common cold.

Studies of sunspots indicate that there are often two similar peaks--one, on average, between September-October, and the

other, on average between February-March. Clayton found two maximum areas of sunspots each year for the period 1887-1938--one peak averaged between February and March and another averaged between September and October. Arctowski, studying sunspot numbers for the period 1874-1913, found a peak averaged in September and the next averaged in February. Hess and Huystic confirm the existence of regular seasonal variations in sunspot numbers. Reports on the International Geophysical Year 1957-58 indicate that in September 1947 the intense solar activity produced auroras that could be mapped on twenty-five nights. The great auroral storm of February 10-11, 1958 was one of the most spectacular in many years. In March 1958, auroras were seen on every night except one. The "Observed Sunspot Numbers" for this period show one peak in September-October 1957 and another in February-March 1958. Perhaps the sun's crossing of the equator in September and March is a factor.

The similarity of the seasonal peaks of common cold incidence and sunspot activity indicate a relationship between these two phenomena; the former appears to be caused by the latter. The mechanism of the relationship appears to be the change in ionization of the atmosphere during increased sunspot activity.

. . . . .

SEARCH FOR CORRELATION BETWEEN GEOMAGNETIC DISTURBANCES AND MORTALITY

Lipa, B. J., et al; *Nature,* 259:302-304, 1976.

It is generally accepted that the Sun affects the Earth's magnetosphere and ionosphere by variations in the ultraviolet and X-ray radiation and in the solar wind due both to short lived disturbances such as flares and to long lived effects such as that associated with the 'sector structure' of the interplanetary magnetic field. There have, however, been repeated claims for well over a century that the Sun influences many other processes on Earth, including terrestrial weather and human disorders. Friedman, Becker and Bachman have presented evidence for an association between geomagnetic storms, cosmic-ray flux variations and psychological behaviour. The term geomagnetic storm is used for worldwide fluctuations in the Earth's field with a scale of about 100γ over a period of several hours, caused by the impact of a solar plasma front on the magnetosphere. A number of Russian scientists have claimed that there is a real association between geomagnetic storms and the incidence of various human diseases. Within this general area, one of the most active areas of current research seems to be the correlation of solar activity and myocardial infarction and stroke. We have searched for a similar correlation in the USA but have failed to find one.

Gnevyshev and Novikova refer to the work of several other groups, published in Russian and unavailable to us. Seventeen papers are referenced, all of which are said to suggest the possibility of direct effects of solar activity on living organisms.

They present evidence of two types: first, the results of direct experiments in which biological systems were placed in artificially induced electromagnetic oscillations and second, studies of correlation between geomagnetic indices and mortality due to heart disease and stroke, using the method of superposed epochs and comparison of averaged medical data on days of different geomagnetic activity. The results presented seem to indicate positive correlations, but no estimates of statistical significance are given. We therefore performed similar statistical analyses using US data. Daily numbers of deaths due to specific causes were obtained from the National Center of Health, referring to the actual date of death rather than the registration date.

[Analysis omitted.]

In summary, our study does not support the findings of Gnevyshev and Novikova, nor their proposal for a new branch of science--heliobiology. It is possible that the correlations they find are either not statistically significant or not due to a casual relationship between geomagnetic disturbances and coronary heart disease and stroke. If their correlations are indeed indicative of a casual relationship, it will be necessary to determine whether it is sensitive to geographical location, to phase of the solar cycle or to some other parameter which might distinguish the samples we have analysed from those of the Soviet scientists.

. . . .

BLIND MAN LIVING IN NORMAL SOCIETY HAS CIRCADIAN RHYTHMS OF 24.9 HOURS

Miles, L. E. M., et al; *Science,* 198:421-423, 1977.

Abstract. A psychologically normal blind man, living and working in normal society, suffered from a severe cyclic sleep-wake disorder. Investigations showed that he had circadian rhythms of body temperature, alertness, performance, cortisol secretion, and urinary electrolyte excretion which were desynchronized from the 24-hour societal schedule. These rhythms all had periods which were longer than 24 hours and indistinguishable from the period of the lunar day.

UNUSUAL PHYSICAL ABILITIES

• Human Sense of Direction

SENSE OF DIRECTION

Comstock, Milton L.; *Science,* 3:31, 1884.

I have been much interested in the different methods of preserving the relative situation of places as given in late numbers of Science, and will venture to add my own experience.

I refer all objects to two rectangular co-ordinate axes which agree with the cardinal points. In all places where I feel at home, these lines are consciously present, and all roads running north and south, or east and west, coincide with, or seem to be parallel to, these axes. All places which I have visited, from Massachusetts to Nebraska, are, with few exceptions, connected together in one system.

The principal origin of this system is the northwest corner of a schoolhouse in Hamilton county, O. There, when a boy, I sat under the direction of a teacher to study geography. With face toward the north, I looked through a window along the meridian. I could at pleasure see east or west, or, if need be, south, through opposite windows. A thorough course in geography fixed in my mind the axes of my system, which have been present with me ever since, a secondary origin going with me everywhere. All places with which I am familiar form parts of this system, and any new place visited is immediately referred to its proper location.

Now for the exceptions. There was another schoolhouse, where I attended sometimes, at which I was turned a quarter round. East was north, south was east, etc. I account for the anomaly in this way: in going to the schoolhouse where my system was fixed, I went east, along a road from which I turned to the left into the south or front door of the schoolhouse; but, in going to the second school mentioned, I went through fields into a road along which I passed toward the south some distance, and then turned toward the left into the west or front door of the schoolhouse. I lost the direction of my axes of reference in crossing the fields; so that the west side of the new schoolhouse seemed to coincide with the south of the old, and thus unconsciously my axes were turned a quarter round. No plan I could adopt had the least effect in changing the apparent position of the cardinal points. Many a laugh was raised at my expense

because of my promptness in pointing wrong directions; and to this day, after nearly half a century, if I wish to think of directions from that schoolhouse, I am obliged to change my first decisions through an angle of ninety degrees.

Washington City is another place which is entirely out of my system. I entered the city after nightfall. Somewhere between Baltimore and Washington, I lost my co-ordinate axes, so that, when I came to consider directions, Pennsylvania Avenue was turned half round, east was west, west, east; and I had not and have not the least sense of north or south. No study of maps, and no thinking over the subject, has the least effect in arranging things properly.

. . . . .

• A Magnetic Sense in Humans

ON THE EXISTENCE OF A "MAGNETIC SENSE"

Anonymous; *Society for Psychical Research, Journal*, 1:41-44, 1884.

In connection with this subject, it may be of interest to note the idiosyncrasies of some individuals, who affirm that they can only sleep well when their bed is in a definite direction with regard to the magnetic meridian. Dr. W. H. Stone has mentioned* his own inability to sleep soundly in a north and south position.

Baron Reichenbach, however, states that in the sensitive tem-

---

*Reports of St. Thomas' Hospital, Vol. X, 1880. The following is the passage:--"One or two rather trivial facts, such as the inability of many persons of nervous temperament, of whom the writer is one, to sleep soundly in the north and south position, a position obviously forced for a diamagnet such as the human body, and the singular vitality of magnetic treatment from very early times down to the present, are hardly strong enough to countervail the negative evidence here adduced. Still the subject, not being in direct contravention of any known scientific proofs, deserves further investigation in a fair and dispassionate spirit. It seems prima facie improbable that so important a factor in cosmogony should be otiose and inefficacious in its highest development--human life."

peraments he examined he invariably found that the most refreshing sleep was obtained when the head was to the north and the feet to the south, positive discomfort being experienced by several of his subjects when the east and west position was assumed. Among other cases the Baron mentions that of a Miss Sturmann, a patient in one of the hospitals at Vienna, who enjoyed "a night of unusually peaceful sleep such as she had not experienced for a long time," together with other favourable symptoms, when her bed was moved with its head to the north. The same patient when in this position appeared to be wonderfully sensitive to the magnet. At the whole length of the ward, a distance of some thirty feet from her bed, the Baron states that the removal of the keeper from a powerful magnet he had brought caused her to become unconscious:--"She stopped speaking in the middle of a word that was on her tongue; she had half said it, the rest died away on her lips. I found her lying rigid with spasms and with clenched hands, her eyes open and cast upwards, so unconscious that I could place my fingers on her eyeballs without the lids moving." The experiment was tried three times with exactly the same results.

Such extreme susceptibility is doubtless rare, but it ought surely to be possible to ascertain if Baron Reichenbach was or was not mistaken in the conclusion he drew from his experiments. The effect of the imagination is so powerful that it will be difficult, if not impossible to devise any experiments that will exclude its influence, and unless this is done no conclusions can safely be drawn. We shall be glad, nevertheless, if our readers will communicate to us any experience they may have had which seem to bear on this question.

Many years before Reichenbach's time the sensory effect of a magnet and the so-called magnetic light appear to have been noticed. Mr. E. T. Bennett has disinterred the following extract, which he has been good enough to translate from the "Tagebuch einer Magnetischen Behandlung," by P. G. van Ghert, Secretary of the Ducal Mineralogical Society, at Jena, in Holland, and member of several learned societies.

* * * * *

"1810. October 15th.--To-day, when I arranged some experiments with the magnet, while the patient was in a room upstairs, and I was below, so that she could not see what I did, she immediately came down, and said that she did not know what was the matter with her, but that it was impossible for her to resist the influence.

I put her to sleep in the usual manner . . . . . I held the magnet a few inches from her knee; but she immediately besought me to take it away. I requested her to touch it with her hands. At first she would not do this, but finally took hold of it, and held it for a considerable time in her hand without experiencing any inconvenience. I made passes with it from the head to the knee, which produced a strong influence. She said she saw a blue glow proceed from it, which penetrated into herself. I made passes over her hand with the north pole, which she could bear very well. I then brought

the south pole over the same spot, which did not produce any different influence from the north pole. But at the distance of about a foot-and-a-half, she saw a glow proceed from the part of the hand where the passes had been made to the south pole only. When I placed the magnet in her hand, and held a piece of steel before it, she assured me that it produced a much stronger influence, and said:--'A stream goes from the magnet to the steel, which looks exactly like a rainbow, except that the colours are not so bright.'"

ON A "MAGNETIC SENSE"

Barrett, W. F.; *Nature*, 29:476-477, 1889.

Sir William Thomson, in his presidential address at the Midland Institute, which is reported in Nature for March 6 (p. 438), draws attention to the marvellous fact that hitherto we have no evidence to show that even the most powerful electromagnets can produce the slightest effect upon a living vegetable or animal body. But Sir William "thinks it possible that an exceedingly powerful magnetic effect may produce a sensation that we cannot compare with heat, or force, or any other sensation," and hence he cannot admit that the investigation of this question is completed,--for although the two eminent experimenters, named by Sir W. Thomson felt nothing when they put their heads between the poles of a powerful electromagnet, it does not follow that, therefore, every member of the human race would feel nothing.

May I be permitted to point out that some slight evidence already exists in the direction sought by Sir W. Thomson? Scattered in different publications there are numerous statements made by different observers in different countries during the present century, which, if trustworthy, indicate that upon certain human organisms a powerful magnet does produce a very distinct and often profound effect. Unfortunately, with the exception of the careful and excellent observations made by Dr. W. H. Stone, who tried Charcot's experiments on a patient of his at St. Thomas's Hospital, the observations referred to are singularly wanting in precision of statement and in a due recognition of the precautions needful in order to avoid fallacious or ambiguous results from illusions of the senses.

This being the case, an attempt is being made by the Society for Psychical Research to ascertain--by direct and careful experiment, extending over a wide range of individuals--whether any trustworthy evidence really exists on behalf of a distinct magnetic sense. The sectional Committee of that Society intrusted with this and cognate work has published a preliminary report, which contains a fragment of evidence pointing in the direction of the existence of a magnetic sense in certain individuals. Three persons have been found by the Committee, who, when their heads were placed near the poles of a powerful electromagnet,

could tell by their sensations when the magnet was excited or not. One of these "sensitives" told the investigating Committee accurately twenty-one times running whether the current was "on" or "off" from a peculiar and unpleasant sensation he alleges that he experienced across his forehead. Every precaution that suggested itself was taken to prevent the subjects gaining any information through the ordinary channels of sensation of what was being done at the contact-breaker placed in another room. But I am sure the Committee will gratefully welcome any criticism of their procedure or suggestions for future experiment which Sir William Thomson may feel inclined to give. The honorary secretary of the Committee is Mr. W. H. Coffin, Cornwall Gardens, S. W.

Two or three months ago one of the gentlemen who appeared to have this magnetic sense was in Dublin, and I took the opportunity of repeating with care in my own laboratory the experiments previously made at the Society's rooms in London. The result satisfied me that this individual did in general experience a peculiar sensation, which he describes as unpleasant, when his head was within the field of a powerful magnet. Nevertheless the keenness of his magnetic sense, if such it be, varied considerably on different days, and sometimes he stated that he could detect little or no sensory effect. Usually the effect was felt most strongly when the forehead was in the line joining the two poles; but one day, when he was suffering from facial neuralgia, he found that his face was the most sensitive part, and complained of a sudden increase of pain whenever the magnet was excited, his face being near the poles. Sufferers from neuralgia among the students of science may therefore have a new and useful career before them, in the pursuit of which their increased torture will, it is to be hoped, be vanquished by a far stronger intellectual joy.

The peculiar and unpleasant sensation which the magnet appeared to produce on the subject just referred to was described as slowly rising to a maximum in fifteen or twenty seconds after the current had been sent round the coils of the electromagnet. In like manner the effect seemed to die down slowly after the contact was broken. Unknown to the subject, the circuit was closed and opened several times, and the magnetism correspondingly evoked or dissipated, the result being that there was a fairly accurate correspondence between the physical and the psychical effect. The faint molecular crepitation which accompanies the magnetisation of iron, and can be heard when the ear is very near the magnet is, however, very apt to mislead the imagination. To avoid this, the subject was placed at a distance where this faint sound could not be heard, and he was then requested to walk up to the electromagnet, and, judging only from his sensations, to state if the current were "on" or "off." The experiment was made twelve times successively, and he was correct in ten out of the twelve trials. He had no means of seeing or hearing the contact-breaker; of course, it is possible for a trickster, using a concealed compassneedle, to be able to impose on a careless experimenter, but care was taken, and I have not

the least reason to doubt the entire bona fides of the subject of this experiment. Obviously the foregoing observation is but of little value unless corroborated by a far more extensive series of experiments, conducted with the most stringent precautions to avoid the creation of illusory effects.

I have tried experiments with large helices encircling the limbs and head, and animated by powerful currents, but have not observed any peculiar sensory effect in my own case, though I am inclined to think the headache which I have often experienced when working with a large magnet may not be altogether an accidental coincidence. Meanwhile experiments are in progress in my laboratory to ascertain, if possible, whether any sensory effect is produced upon lower organisms. I hardly anticipate any affirmative results, but it seemed worth making a systematic investigation from minute structures up to man. Sir W. Thomson's address will, I hope, stimulate other workers in this field.

• The Sensing of Electromagnetic Signals

A CURIOUS PHENOMENON

Hutt, Stanley B.; *Nature,* 64:233, 1901.

A curious phenomenon occurred to some volunteers while on outpost duty on the Delagoa Bay Railway in the Transvaal.

A search-light was fixed up in the station, which was used nightly in scanning the wide stretches of veldt. We were on solitary outpost duty about three miles from the station, and on the still silent nights which are frequently experienced in the clear atmosphere of the high veldt we distinctly heard a low "purring" sound as the ray of light of the station passed over us. As the light approached us one could hear the sound gradually increasing, being loudest as it switched over us and passing away into the nothingness of the silent night. We were too far off the station to hear any vibrations from the mechanism of the search-light, and we all came to the conclusion (being a collection of unscientific men) that the high velocity of the light waves created a sound audible to our ears. On other nights when there was only a slight breeze no noise could be detected.

Can any one of Nature's readers tell me if this is a known physiological effect?

CAN THEY REALLY "HEAR" RADAR?

Anonymous; *New Scientist,* 36:582, 1967.

That some people can "hear" radar is now an established fact, according to Dr. Clyde E. Ingalls, an electrical engineer at Cornell University. Writing in the New York State Journal of Medicine (1967, Vol. 67, p. 2992), he describes some recent experiments on a phenomenon that has been reported occasionally in the past but has never been taken very seriously.

Dr. Ingalls studied human sensitivity to radar at a radar installation in the United States. Using a field strength meter to avoid hazardous over-exposure, he found that some individuals heard "a high-pitched buzzing sound" when they stood near the edge of a radar beam. The sound, which could be heard despite considerable ambient noise, was experienced as coming from some three to six feet above the head. Placing the fingers in the ears to cut out other noises made the source of the sound appear to come down to the top of the head.

It seems that the radar beam may directly stimulate the human nervous system--probably the brain--rather than act through the normal hearing apparatus in the ear. When a three foot square of screening material was used to shield the subjects from the radar beam, the sound disappeared. And nothing could be heard by poking an ear through a hole in the screen. The only part of the body receptive to the sound was an area on the head above the forehead.

. . . . . .

HUMAN PERCEPTION OF ILLUMINATION WITH PULSED ULTRA-HIGH-FREQUENCY ELECTROMAGNETIC ENERGY

Frey, Allan H., and Messenger, Rodman, Jr.; *Science*, 181:356-358, 1973.

Abstract. A psychophysical study of the perception of "sound" induced by illumination with pulse-modulated, ultrahigh-frequency electromagnetic energy indicated that perception was primarily dependent upon peak power and secondarily dependent upon pulse width. The average power did not significantly affect perception. Perceived characteristics of pitch and timbre appeared to be functions of modulation.

• Human Detection of Cosmic Rays

VISUAL PHENOMENA NOTED BY HUMAN SUBJECTS ON EXPOSURE TO NEUTRONS .....

Budinger, Thomas F., et al; *Science*, 172:868-870, 1971.

Abstract. Six subjects reported multiple starlike flashes and short streaks on exposure to neutrons of energies up to 25 million electron volts. The probable mechanism is interaction with the retinal rods by proton recoils and by alpha particles released from neutron reactions with carbon and oxygen. These observations are similar to light flashes and streaks seen by astronauts who are exposed to high-energy cosmic rays on translunar flight.

• Facial Vision and Heightened Awareness

THE SPIDER SENSE

Anonymous; *Nature*, 93:118, 1914.

Following an article on "The Spider Sense," several letters upon this subject have appeared in the Times (March 18-26). Certain people, we are told, are able to detect the presence of a spider (or cat) by means of a "sixth sense." The use of the term "sixth sense" indicates the complete innocence of psychology that characterises the whole correspondence. As to the fact, Prof. Meldola (March 26) is fully justified in pointing out that the "negative instance" makes the proffered evidence totally unconvincing. Probably many people believe they can tell when they are being stared at from behind, but a recent experimental test revealed no such ability. On the other hand, such sensitivity does not seem a priori impossible. Sensory acuity varies greatly in different individuals and in special conditions. Thus some blind persons can perceive objects at a distance. This seems to be an abnormal development of a normal form of cutaneous sensitivity, the sense-organ being the skin of the face and the drum of the ear. Again, remarkable degrees of hyperaesthesia occur in certain stages of hypnosis, and in the present instances there may possibly be something of the nature of hypnotic auto-suggestion. That smell may play some part, as suggested by Mr. Ponder (March 25) is possible. This sense is imperfectly under-

stood; how, for instance, is a hound able to avoid "back-tracking"? Even in the human its potentialities seem very elastic. Helen Keller, having lost her sense of smell for a few days, says: "A loneliness crept over me as vast as the air whose myriad odours I missed." But the existence of the "sense" and its nature should be quite simply determinable by experiment. Mr. C. Sully, assistant lecturer in psychology at King's College, London, will be glad to hear of a suitably endowed person willing to act as subject.

"FACIAL VISION": THE PERCEPTION OF OBSTACLES BY THE BLIND

Supa, Michael, et al; *American Journal of Psychology*, 54:133-183, 1941.

The avoidance of obstacles by the blind has long been a topic of special interest and speculation. The history of the problem dates from 1749 when Diderot recorded the "amazing ability" of a blind acquaintance not only to perceive the presence of objects but also to judge accurately of their distance from him. Diderot thought his subject judged the proximity of the obstacles by the action of the air on his face--that is by the increased sensitivity of the facial nerves and end-organs.

Since the appearance of Diderot's account, numerous cases possessing this special ability have been reported and numerous theories have been advanced in explanation of the phenomenon. Hayes, in his excellent summary of the work in this field, lists fourteen theories which are divided into three groups according as they rest upon a sensory, a perceptual, or an occult basis.

The sensory theories postulate a heightened response of some of the organs of sense; of pressure or temperature in the face; of pressure in the tympanic membranes in the ears; or of audition in the increased discrimination and analysis of sound. The perceptual theories involve the interpretation of sensory cues which are derived from the action of the air- or sound-waves on the skin or aural mechanisms. The occult theories explain the phenomenon in terms of magnetism, of electricity, of vibration of the ether or some other hypothetical substance, of the action of vestigial organs in the skin, and of the subconscious.

Historical Background. As stated above, Diderot (1749) was the first to publish an account of the ability of the blind to perceive obstacles at a distance. His explanation of the phenomenon, i.e. the action of the air on his subject's face, was accepted without question by the early writers in this field. For example, Zeune (1808) claimed that the blind used their cheeks and foreheads as "feelers;" Knie (1821) regarded air pressure as the stimulus; Sergel (1868) concluded from the results of his own experience, that "the distance sense" was clearest around the eyes and ears, weaker at the temples and on

the forehead, still weaker on the cheeks, and almost lacking on the lips. Levy (1872) names this ability "perceptio facialis," and described some remarkable feats that he himself could perform by means of his heightened facial sensitivity to minimal pressure stimulation. Scherer (1874), agreeing with Sergel that the phenomenon was sensory, developed a theory relating it to the physical laws of electricity.

More important than these anecdotal accounts of the blind was the work of the early experimenters. The studies of Heller, around the turn of the last century, mark the beginning of the scientific research in the field. Heller wrote that "sensations of approach do not depend upon a special touch quality nor upon stimulation of a certain part of the skin." He came to this conclusion from the results of a series of experiments in which he had blind subjects approach an obstacle (a school chart, 1.65 cm. wide and 1 m. high, mounted on a 1-m. stand) with touch or hearing eliminated. He found that the blind, under favorable conditions, perceived the presence of objects by sound at about 3-4 m., and by pressure at about 60-70 cm. He concluded, therefore, that "the perception of changes in the sound of his (the blind's) footsteps leads to careful attention for sensations of pressure in the forehead. If these characteristic sensations then arise, he is sure that an obstacle is in his path and he turns aside in good time. The sound components of the experience then serve as a signal which inhibits other processes which might prevent full attention.

At about the same time that Heller was working in Germany, some American psychologists became interested in the problem. William James (1890) suggested that the sense of obstacles might be due to pressure sensations from the tympanic membrane. Dresslar (1893) tested this hypothesis in a series of carefully controlled experiments and found that the amount of pressure needed to stimulate the tympanic membrane far exceeded the amount derived from the air waves aroused by a subject's approach to an obstacle. Dresslar then turned to the investigation of other possible clues for "facial vision." He had his subjects distinguish between different types of surfaces of an obstacle (1) when vision alone was eliminated; (2) when thermal sensations and "facial vision" were eliminated by covering the ears, face, and neck with cloth and cardboard, leaving a hole opposite the auditory meatus; and (3) when the face was exposed but both ears stopped with cotton. Dresslar concluded, as a result of these experiments that "the basis for judgment was due to differences in sound."

Following the work of Heller and Dresslar no serious experimental studies were made upon facial vision until the turn of the century. Javal (1903) introduced the term "sixth sense" of the blind, and supposed that it was akin to touch and aroused by ether waves. MacDougall (1904) repeated Dresslar's experiments with somewhat different results. He questioned the importance of audition as the essential factor for all individuals since he found that plugging the ears made no difference in judgment whereas preventing facial stimulation resulted in a

lowering in correctness of response. MacDougall concluded that sound, pressure, and perhaps temperature all help in the perception of obstacles. Hauptvogel (1906) suggested that the "sixth sense" was due to stimulation of the ear drum by some mysterious substance in the ether.

The theories that the perception of obstacles by the blind was due to multiple sensory stimuli were superseded during the first two decades of this century by mono-sensory views. Three German investigators--Truschel, Kunz, and Krogius--maintained that the perception of obstacles by the blind was achieved by the increased sensitization of a single sense department. Beyond this generalization, however, there was little agreement among them. Each postulated that the principal role was played by a different sense department and each conducted experiments and marshalled the results to support his own hypothesis. Theory not only directed procedure but prejudiced interpretation as well. A bitter three-cornered controversy resulted which did little to clarify the problem.

Of those involved in the controversy, Truschel (1907) was the first to publish the results of his studies. He claimed that auditory stimulation was necessary for the perception of an obstacle by the blind. He was unable to decide, however, whether the organs involved were those in the cochlea or in the vestibule.

Truschel observed that, as one approaches an obstacle, the sound of the footsteps rise steadily in pitch. The localization of stationary objects outside the path of approach was explained by him as being due to the reflection of diffuse sounds. Since these diffuse sounds did not arouse auditory perceptions, Truschel thought that the perception of the objects was due to the vestibular organs. According to him, therefore, this "X-sense"--as he called it--was due to aural stimulation entirely and it was in no way dependent upon pressure or thermal stimuli.

Kunz (1907) was equally certain that the blind who perceive obstacles rely solely on pressure sensations. He performed a number of experiments measuring pressure sensitivity of the face, acuity of hearing, localization of sound, musical ability, and bone conduction of sound. He concluded from his results not only that the pressure sensations were the important factors in the perception but also that audition played no role.

Krogius (1907), using motionless subjects with movable stimuli (as Kunz did) espoused a thermal theory of the sense of obstacles although his experimental results were not decisive.

Wolfflin (1908) followed Heller's technique and concluded that the ability of the blind to perceive obstacles was due neither to hearing, touch, or temperature, but probably had its basis in the nerves of the face, particularly the trigeminus.

Villey (1918) criticized Kunz and Krogius and concluded that the ears are the mechanism by which the blind avoid obstacles. There are sounds around us all the time and when there is some change in these sounds we interpret an obstacle between us the source of the sound. Villey explained the feelings of pressure upon the forehead as an auditory illusion similar to the phantom sound localized in the middle of our heads. In 1923 Villey ques-

tioned soldiers who had been blinded by the war and found that 25% of them thought that they detected obstacles by the ear, 25% by the sense of touch, and 50% by a combination of the two senses. He concluded that both audition and touch play a role in the sense of obstacles and that the blind could accordingly be classified as audiles or tactiles. A combination of the two would give the maximal ability to avoid obstacles.

A highly fanciful explanation of the perception of obstacles by the blind was given by Romains (1924). He believed that the vestigial Ranvier corpuscles were really little eyes and that these "ocelles" were brought into function by the blind. In his book, Romains did not present the method of his experiments but gave only his results and explanation.

In 1929 Villey published an account of the work of Lamarque done in 1910. Lamarque was a true experimentalist. Instead of establishing a theory first and then trying to find facts to corroborate the theory, he let interpretation follow the facts. He developed techniques to supplement the commoner practices of eliminating one sense after another. Lamarque was interested in detecting the physical changes in the stimulus--something which nobody else had attempted to do. He recorded the sound-waves from a tuning fork alone and, when different obstacles were placed at varying distances between the sound source and the recorder, found that the height of the curve never changed but that the form of the curve was modified; in short, that the pitch differed whereas the loudness remained the same.

Dolanski (1931) in a series of experiments moved disks of various sizes toward his observers who sat (a) with faced covered, (b) wearing a flap in front of the ears, (c) wearing a cardboard mask the shape of the face, and (d) with ears plugged with cotton. He found that his subjects failed to perceive the disks under conditions (b) and (d) in which hearing was restricted. Dolanski postulated a physiological theory of the sense of obstacles. He claimed that cues from any sense department suggested danger to an individual and thus caused contraction of small muscles in the skin. The sensations from these muscles were, he believed, the basis of the perception of obstacles by the blind.

Mouchet (1938), independent of Villey and Dolanski, found that auditory processes were involved in the perception of obstacles. He believed, however, that subliminal auditory stimuli may play a role in this ability of the blind.

Not only are blind who possess the "sense of obstacles" unable to explain the basis of their performance, but, as this review shows, the investigators of the phenomenon are themselves unable to come to any agreement regarding it. Fact is entangled with theory and theory has, all too often, prejudiced interpretation of the experimental results.

Problem. The present study was undertaken to resolve the contradictions between theory and experimental result. We hoped, despite--or because of--our theoretical biases, which differed greatly among us, to follow without prejudice the lead of the ex-

perimental facts and to determine the necessary and sufficient conditions for the perception of obstacles by the blind.

[Experimental details omitted.]

Summary of Results and Conclusions. In this study we dealt with two different sources of sensation: (1) the exposed areas of the skin and (2) the ears. With each of those sources of sensation two kinds of stimuli concerned us: (a) air-currents and air-waves which were outside the auditory range, hence they could arouse only cutaneous sensation if they aroused anything; and (b) sound-waves which could be heard and which might also be capable of arousing cutaneous sensation in the ear and the exposed areas of the skin. It was upon those variables that we ran our experimental changes.

We eliminated the action of 'air-waves' and air-currents on the exposed areas of the skin but left the skin and the ears open to stimulation by sound-waves; we plugged the ears and shielded them from stimulation but left the exposed areas of the skin open to stimulation by 'air-' and sound-waves; we drowned by means of a sound-screen all stimuli which might have reached the ears but again left the exposed areas of the skin open to stimulation by 'air-' and sound-waves; and lastly we reduced the stimuli to sound-waves and limited their action to the ears.

The results obtained from those experimental changes led us to the following conclusions:

(1) Stimulation of the face and other exposed areas of the skin by 'air-' and sound-waves is neither a necessary nor a sufficient condition of the perception of obstacles by our Ss.

(2) Stimulation of the skin by reflected breath is neither a necessary condition nor, as far as 'facial pressure' is concerned, a sufficient condition for the 'final appraisals' by our Ss.

(3) The pressure theory of the "obstacle sense," insofar as it applies to the face and other exposed areas of the skin, is untenable.

(4) Aural stimulation is both a necessary and a sufficient condition for the perception of obstacles by our Ss.

'BLINDSIGHT' SEEING WITHOUT REALIZING THAT YOU CAN SEE

Hansen, James; *Science Digest,* 87:14-17, January 1980.

Can the blind see without knowing it? The answer, in some cases, may be yes. That's the startling conclusion of Dr. Lawrence Weiskrantz of Oxford University and three colleagues at London's National Hospital. They've discovered a form of unconscious vision which permits certain blind people to "see" without being aware of the ability to do so. With an eye toward a catchy name, the researchers call the phenomenon "blindsight."

This is not a "throw away your white cane and leader dog" discovery. There are many causes of blindness and the category of blind people able to "see without knowing it" is a very small minority. What the research team has discovered is this: Working with patients who had acquired visual defects as a result of damage to the brain cortex, they found that their subjects, if asked what they saw, said they saw nothing. In testing, though, asked to guess, they "guessed" right nearly 90 percent of the time. This is far beyond the success rate accountable to pure chance.

The researchers theorize that the kind of "sight" detected by these tests depends on a different neural pathway from the eye to the brain, one that passes through the mid-brain rather than the cortex. In evolutionary terms, the midbrain is much older than the cortex. In a sense, it may be the animal part of our brain---an evolutionary holdover from our pre-human past.

The properties of "blindsight" are quite different from those of ordinary sight and suggest a type of visual ability necessary to a hunting animal, rather than the vision required to read a book or appreciate a painting. As Dr. Weiskrantz explains: "Blindsight detects events but isn't able to identify them. It sees what is attention evoking, but can't say just what it has seen." A common example of this type of visual ability in persons with ordinary sight is extreme peripheral vision. The midbrain vision (the blindsight) tells us something is going on at the corners of our sight and we look around to see just what it is.

This kind of vision is extremely sensitive to motion and to sharp contrasts of light and dark. In the strictest sense, the word "vision" may not even be appropriate here since it appears that no visual image is created in the mind. Indeed, blind subjects express great surprise and insist they are only guessing, when confronted with the results of their tests. They had "seen nothing"; they just had a "feeling" about the things that appeared in their visual field. Difficult as it is to imagine this concept, it does appear that this form of sight is not visualized---it is not projected in living color on the subject's mental movie screen. Blindsight, in fact, appears to be sight-without-the-picture.

. . . . . .

• Did Color Vision Change in Historical Times?

THE PUZZLE OF COLOR VOCABULARIES

Woodworth, R. S.; *Psychological Bulletin,* 7:325–334, 1910.

It was Gladstone who first, in 1858, called attention to the rather extraordinary vagueness of early color nomenclature. Collating from the Iliad and Odyssey the passages which referred to color, he found such uncertainty and inconsistency in the application of color names as to lead him to deny to the Greeks of Homeric times any clear notions of color whatever. "I conclude," he says, "that the organ of color and its impressions were but partially developed among the Greeks of the heroic age."

This hypothesis of Gladstone was made more precise and given a definite evolutionary character by Geiger, who, extending the study to many ancient literatures, found them all defective in the same respect; they all showed a lack of any clear term for blue, and the oldest of them had also none for green. Speaking of the Vedas, Geiger says:

"These hymns, consisting of more than 10,000 lines, are nearly all filled with descriptions of the sky. Scarcely any subject is more frequently mentioned; the variety of hues which the sun and dawn display in it, day and night, clouds and lightnings, the atmosphere and the ether, all these are with inexhaustible abundance exhibited to us again and again in all their magnificence; only the fact that the sky is blue could never have been gathered from these poems by any one who did not already know it himself . . . . The Bible, in which, as is equally well known, the sky or heaven plays no less a part, seeing that it occurs in the very first verse, and in upwards of 430 other passages besides . . . yet finds no opportunity either of mentioning the blue color.

"The color green is met with in antiquity one stage farther back than the blue. . . . The, ten books of Rigveda hymns, though they frequently mention the earth, no more bestow on it the epithet green than on the heavens that of blue."

In the very earliest literary remains, according to Geiger, there is not even a name for pure yellow, though there is one for golden or reddish yellow; red appears more firmly entrenched than yellow. And, by aid of etymology, the author believes it possible to go back of even the earliest literature, and "arrive at a still earlier stage, when the notions of black and red coalesce in the vague conception of something colored."

Following his general doctrine that the development of the human mind can be traced by aid of the history of language, Geiger concludes that in an early stage of human development, only a vague sense of indefinite color existed; that red first took on the character of a definite sensation, and that the other colors followed in the order of the spectrum.

The views of Geiger were warmly espoused by Magnus, who,

besides attempting to trace a gradual evolution in the use of color names in Greek literature, took the important step of examining, on a wide scale, both the color vocabularies and the color sense of existing primitive people. As a result of a questionnaire, with a set of colors to be named and distinguished, sent out to traders and missionaries, Magnus discovered that most primitive tribes possessed a color nomenclature which was incomplete in about the same way as that of Homer or of the Vedas. But he also found that the limits of color vision were the same among these tribes as among Europeans. They could see and distinguish all the colors from red to violet, though usually they did not possess names for them all. This discrepancy between color vision and the color vocabulary was in itself an important discovery, since it betrayed the weak foundation of the philological method. (pp. 325-326)

EVOLUTIONARY CHANGES IN PRIMATE SENSORY CAPACITIES

King, James E., and Fobes, James L.; *Journal of Human Evolution*, 3:435-443, 1974.

This paper is a review of laboratory research on sensory capacities in Primates with emphasis on evolutionary changes in the capacities. Man, great apes and Old World monkeys have virtually identical color vision, a result consistent with the generalization that these relatively diverse species have reached a plateau in the evolution of color vision, Capuchin and squirrel monkeys are protonomalous trichromats thus suggesting a widespread relative insensitivity to red among New World monkeys. Of the prosimians, only tree shrews have been extensively tested for color vision capability and they are deuteranopic dichromats.

There is clear evidence of a decline in the upper audible frequencies and the most audible frequencies during Primate evolution. Although the olfactory portion of the brain has declined markedly in relative size during Primate evolution, there is no corresponding research showing difference in olfactory sensitivity across the living Primate genera. Existing research on gustatory sensitivity in Primates is too limited to permit any evolutionary conclusions. (Abstract)

• Accounts of Moon Blindness

ON MOON BLINDNESS

Robinson, G.; *Report of the British Association, 1858,* part 2, p. 19.

The author gave several instances of his men who had slept on deck exposed to the moonbeams being so blind on landing that they had to be led by the hand. Also the sailors were in the habit of waking up the soldiers who attempted to sleep on deck, and warning them that they would be blinded.

EFFECT OF MOONLIGHT ON FISH AND MEN

Murdoch, Samuel; *English Mechanic,* 85:496, 1907.

From June, 1885, til June, 1859, I was an apprentice of the ship "Langdale," of Liverpool, an East India trader. In 1856 we called at St. Helena for provisions and water. Just before leaving the island the captain bought a few dozen mackerel, just newly caught, which were immediately cleaned, split open, and then hung on lines stretched between the davits of the port-quarter boat, where, not being covered they were exposed to the weather for some few days and nights.

During every night the moon was high in the heavens, and shone brilliantly. About a week after leaving St. Helena, some of the fish were cooked for the cabin breakfast, and the captain, the the second mate, and the steward made a hearty meal of them, but the chief mate would not touch them. After breakfast, the second mate began using most unparliamentary language, and commenced to vomit violently, his face becoming very red and swollen, and he continued vomiting and swearing alternately for quite a half-hour. During this half-hour, the skipper and the steward could be seen in the lee scuppers, with red and rueful faces, going through the same performance as the second mate was doing, while the chief mate was parading the poop with a broad smile on his countenance; he, not having eaten any of the fish, had, of course, escaped the poisoning.

About half-way between St. Helena and the Line, some of the watch below (that part of the crew off duty), finding it too hot in the fo'castle for sleeping, came on deck, and picking out the softest planks, coiled up on them and went to sleep. Again the moon was high and shining brilliantly, and when the sleepers were roused up, three of them were quite blind---moonblind. Those who are striken with moonblindness can see nothing at night, but their sight is quite normal during the day. The

victims, in this case, had to be led about at night, and the ropes put into their hands, and, of course, they could not take their trick at the helm, nor keep their look-out. Before reaching Falmouth, where we called for orders, two of the three had almost recovered from their affliction; but the third one, a strapping young Aberdonian, named Moffat, was, when he left us in Falmouth, just as sightless at night as when he was first stricken. These cases will, I hope, throw light on the subject for they are stern realities.

• Anomalous Adhesion or "Sticky Fingers"

A PECULIAR CASE OF ADHESION

Simon, W.; *Science,* 14:427-428, 1889.

A very singular case of adhesive power has come under my notice lately, and the results of an investigation made with the view of establishing its nature are recorded in the following lines.

Mr. Lewis Hamburger of Baltimore, sixteen years old, and of rather delicate build, noticed for the first time, about the middle of November last, that a cane would, as he expressed it, "stick" to his fingers, and that wiping off the cane and washing his hands would not prevent this occurrence. Laying his fingers on other light articles, such as lead-pencils, penholders, etc., he found that he could lift them up by simply placing his fingers upon them, the objects adhering firmly to the skin. Not being able to explain these phenomena, Mr. H. came to see me on Nov. 19, and surprised me by performing a few of the experiments which he had learned to execute and which consisted in the raising of various objects by their adherence to his fingers. The heaviest of these articles did not weigh twenty grams.

At a loss to understand the nature of these phenomena, I began a series of experiments, which, in the course of a few weeks, brought to light a number of facts more interesting, and even more startling, than those which had been observed by Mr. H. himself up to the time he first called upon me. The experiments performed were made with the view of determining (1) the quality and nature of the adhering substances, i.e., their chemical composition and texture; (2) the quality or weight of adhering masses, and their relation to the hand's surface brought into play in a given experiment; (3) the exact points or surfaces of the fingers or other parts of the body which exhibit this adhesive power; (4) the length of time during which substances will adhere.

Before stating the results of the various experiments made, I

will mention that it was soon found that the hands had to be carefully cleaned by washing with soap and water, and then with alcohol and ether, in order to attain the highest degree of adhesive power; and that the surface of the articles experimented upon had likewise to be well cleaned, and rubbed absolutely dry. Particles of dust or moisture greatly interfere in all experiments where the highest power is demanded.

In regard to the first point of inquiry, the nature of the material which would adhere, it was easily proven that chemical composition had nothing whatever to do with the adherence. Metals, stone, glass, rubber, wood, etc.,---all probably adhere equally well, provided their surfaces possess the same degree of smoothness. As a general rule, it may be stated that the adhesive power increases with the degree of smoothness of surface. It is for this reason only that well-polished metals or glass show the highest degree of adhesion. The latter substance answers especially well, because it can be cleaned easily. In proportion as the surface becomes less even, the adhesive power diminishes; and porous substances, such as paper, cloth, etc., or articles covered with them, cannot be made to adhere at all.

The second question, regarding the determination of the extreme limit of the weight of matter adhering, was found more difficult to answer. A number of factors influence the results of experiments made in this direction. It was found that not only the shape of the adhering mass had to be considered, but also the position of the hand itself. Cylindrical forms seem to be preferable, while flat surfaces adhere but poorly; and a much larger weight may be attached to the fingers while the hand is held perpendicularly then when in a horizontal position. In order to reach some definite results, glass rods of different diameters were used. They were so arranged as to allow an increase of their weights by attachments, and so that the hand might be applied in a perpendicular position. When first examined, on Sept. 22, it was found that the extreme limit of weight which could be made to adhere, by means of a glass rod of 10 millimeters diameter, to the surface of the front part of the four fingers of the right hand, when held perpendicularly, was 1,450 grams. A glass tube of 20 millimeters diameter was next substituted, and would yet adhere when its weight had been increased to 1,900 grams.

When the experiments were repeated on subsequent days, the same glass tube could each time be loaded heavier, and Mr. H. can now lift the comparatively enormous weight of 2,610 grams, after having pressed his fingers tightly to the glass rod, which stands in a perpendicular position upon a metallic disk to which it is fastened, and which also carries the weights.

I ought to state that the thumb is never used to cause the adhesion, and that, in commencing a series of experiments, Mr. H. can never at the beginning lift the greatest weight. It appears that the power of adhesion increases during a series of experiments made within a period of fifteen or twenty minutes. So far, the power has continued to increase almost from day to day, but appears to have now reached its maximum. Following are the re-

sults of a few of the experiments made as described above; the first figure representing the diameter of the glass tube (in millimetres), and the second the maximum weight suspended (in grams): 5, 1,530; 10, 2,120; 15, 2,400; 20, 2,610; 25, 2,260; 30, 1,860.

The weights recorded above are nearly one hundred times greater than those which can be lifted by adhesion when the corresponding tubes are used horizontally. Exact measurements of that portion of the hand's surface which comes in contact with the adhering mass are difficult to make. However, the determinations were made sufficiently accurately to show that very nearly 3 square centimetres surface enter into action during the adherence of a 20-millimetre rod, when supporting 2,500 grams.

When the investigation was first begun, Mr. H. not only firmly believed in his utter inability to use his left hand as he did his right, but also looked for the seat of the adhesive power only in the front part of his fingers. It has now been demonstrated that the left hand does all the work equally as well as the right one, and that the surface of adhesion extends, though different in intensity, over almost the whole of the inner part of the hands. The power is strongest in the front part of the fingers, and weakest in the centre of the palm and on that portion of the fingers which is nearest to it. All the protruding portions, including the ball below the thumb, possess adhesive power, however. Neither the back of the hands, nor other parts of the body, including the surface of the soles and toes, show any signs of adhesion. The power of the hand to sustain objects may be shown by suspending upon it, for instance, four 6-inch test-tubes alongside of one another, or by applying an iron rod, a wooden stick, and a glass tube simultaneously to different parts of the hand. A test-tube adhering to the hand may be made to roll to and fro by jerking the hand backward and forward while the tube is in a perpendicular position.

The intensity of adhesive power in the various fingers differs widely. It is strongest in the index and middle finger, and weakest in the little finger; the latter doing so little work, that the three others may lift almost as much as the four. What is most singular is that one finger possess very little power. The greatest weight shown to adhere to one finger has been about 35 grams, while two fingers may lift 1,400 grams. In order to decide whether or not the aid given by a second or third finger, in balancing or steadying the weight of the suspended mass, was the cause of this inability of one finger to do much work, three fingers were covered with a thin film of collodion, which rendered them unfit to act by adhesion, but not by their muscular support.

The experiments thus performed showed conclusively that the three fingers covered with collodion were absolutely unable to assist the fourth one. It can therefore not be the steadying power which causes two fingers to do forty times the work of one finger. That this should be so, might have been inferred from the fact that Mr. H. can suspend a combustion tube about four feet long on two fingers, and cause it to swing like a pendulum

through a distance of at least three feet.

The length of time during which substances adhere depends chiefly upon their weight. Light objects, such as test-tubes, will remain suspended even horizontally for ten minutes or longer, and can then be removed only by the application of some force, when a slight click, caused by the concussion of air, can be heard. Very heavy articles will fall off sooner; but whether in consequence of a diminution in the adhesive power of the surface, or in consequence of the strain exerted upon the muscles, it is difficult to say. Another cause of the falling-off is to be found in the perspiration which at times oozes freely from the pores, and interferes greatly with the experiments.

It may be added, that neither the shape of Mr. H.'s hands nor the structure of the skin, even when examined under a magnifying-glass, shows any thing abnormal, though the skin is very soft and smooth. These are the principal results of the investigation made, and the next question is, how to account for the phenomenon. I need not mention the reasons which exclude the possibility of an electric or a magnetic action, because the facts presented show this conclusively. We therefore seem to be limited to a consideration of surface action, or atmospheric pressure, or both. The reasons for this assumption are, (1) that it has been found impossible to notice any attraction whatever exerted at a distance; (2) that the power increases with the cleanliness and smoothness of the surface, i.e., with the number of actual points of contact; (3) that the peculiar sound heard on breaking contact is characteristic of the concussion of air; (4) that the power increases with the increase of surfaces in contact, as shown in the experiments with glass tubes of different diameters.

Whether, or to what extent, the pressure of atmospheric air induces these phenomena, I am unable to say. I have not had an opportunity to examine Mr. H. under a diminished or increased pressure, but hope to do so ere long. Certain it is, that the ratio of one square inch of adhering surface to fifteen pounds in suspended weight has not been exceeded, though approached to within twenty per cent. But even if air-pressure participates, as it most likely does, we have to assume that the skin of Mr. H. is peculiarly fitted to show these phenomena of skin-adhesion, and in a degree, to my knowledge, unnoticed heretofore. That he is not the only person possessing this power, I have good reason to believe. Among a large number of people examined, there were many whose hands showed at least signs of this power, and certainly a few who promised to develop it sufficiently to exclude doubt in regard to the occasional existence of the force. It may be well to warn persons who may try experiments, not to mistake for actual adhesion of the suspension of tubes by means of counter-pressure exerted by portions of the terminal phalanges or the fleshy portions surrounding them. The unmistakable signs of adhesion is the performance of the experiments with the fingers kept absolutely close to one another, in which case it becomes next to impossible to exert counter-pressure. That muscular action may come into play in some of Mr. H.'s experiments is not absolutely impossible, yet very doubtful. I leave it to physiolo-

gists to furnish a more satisfactory explanation of these phenomena than I myself have so far been able to give.

• The Voluntary Erection of Hair

STUDIES MAN WHO CAN MAKE HIS OWN HAIR STAND ON END

Anonymous; *Science News Letter,* 38:211, 1940.

Dr. Donald B. Lindsley, of Bradley Home and Brown University, showed before the meeting of the American Psychological Association a motion picture film of a man who can raise the hair on his arms whenever he wants to. More than that, he can step from a hot shower into a cold draft and yet keep the gooseflesh from coming up on his skin.

He doesn't scare himself to bring on the hair-raising. He doesn't even picture in his mind a painful or terrifying experience. He just raises the hair, so far as he knows, in much the same way that he works his muscles. He found he could do it when he was a ten-year-old boy.

Dr. Lindsley, studying the man while making the motion pictures, found that a lot goes on during this hair-raising experience of which the man was not aware. The pupils of his eyes dilate as though he were scared. His heart speeds up. Breathing is faster. Even his brain waves are changed. These are the physiological changes that usually accompany the erection of hairs during fright.

. . . . .

Two things are evident from the case, however. No matter how he starts his sympathetic nervous system to working, it nevertheless works, as is customary, as a unit. A man can't raise his hair without changing heart rate, breathing, and so on.

Of more importance to psychologists are the observed changes in the electric potentials over a single area of the brain, the pre-motor area. This seems to be evidence of some connection between the sympathetic nervous system and the brain cortex.

• Correlation of Strength and Hair Color

ARE BRUNETTES STRONGER THAN BLONDES?

Anonymous; *English Mechanic,* 67:192, 1898.

It is not improbable, says Appleton's Popular Science Monthly, that there is in brunetteness, in the dark hair and eye, some indication of vital superiority. If this were so, it would serve as a partial explanation for the social phenomenon which we have been at so much pains to describe. If in the same social community there were a slight vital advantage in brunetteness, we should expect to find that type slowly aggregating in the cities; for it requires energy and courage, physical as well as mental, not only to break the ties of home and migrate, but also to maintain oneself afterwards under the stress of urban life. Selection thus would be doubly operative. It would determine the character both of the urban immigrants and, to coin a phrase, of the urban persistents as well. The idea is worth developing a bit.

Eminent authority stands sponsor for the theorem that pigmentation in the lower animals is an important factor in the great struggle for survival. One proof of this is that albinos in all species are apt to be defective in keenness of sense, thereby being placed at a great disadvantage in the competition for existence with their fellows. Pigmentation, especially in the organs of sense, seems to be essential to their full development. As a result, with the coincident disadvantage due to their conspicuous colour, such albinos are ruthlessly weeded out by the processes of natural selection; their non-existence in a state of nature is noticeable. Darwin and others cite numerous examples of the defective senses of such non-pigmented animals. Thus, in Virginia, the white pigs of the colonists perished miserably by partaking of certain poisonous roots which the dark-coloured hogs avoided by reason of keener sense discrimination. In Italy, the same exemption of black sheep from accidental poisoning, to which their white companions were subject, has been noted. Animals so far removed from one another as the horse and the rhinoceros are said to suffer from a defective sense of smell when they are of the albino type. It is a fact of common observation that white cats with blue eyes are quite often deaf. Other examples might be cited of similar import. They all tend to justify Alfred Russell Wallace's conclusion that pigmentation, if not absolutely necessary, at least conduces to acuteness of sense; and that where abundantly present it is often an index of vitality.

. . . . .

BLONDES AND BRUNETTES

Anonymous; *English Mechanic,* 73:537, 1901.

Fair people and dark people are arranged in the United Kingdom according to the geographical rules, blondes on the eastern sides of the two islands and brunettes on the western sides. So says Dr. John Beddoe, who also thinks that the shade of a man's complexion has a direct bearing on his calling. Men connected with horses and grooms and butchers are notably fair, shoemakers and tailors are notably dark.

Mr. Havelock Ellis has made a census of the National Portrait Gallery, which contains portraits of so many persons of eminence, with a view to finding out whether the dark men or the light men have done the most for the Empire. Arranging the individuals in the order of their "decreasing fairness," he finds that the political reformers and agitators are the lightest of the lot in complexion. These gentlemen in the "index of pigmentation" figure as 223. The sailors are 150, the men of science 121, the soldiers 113, the artists 111, the poets 107, members of the Royal family 107, lawyers 107, and created peers and their sons 102. The brunettes are statesmen 89, men and women of letters 85, hereditary aristocrats 82, divines 58, men of low birth 50, explorers 33, and actors and actresses 33. In this table an index of more than 100 means that the fair element predominates over the dark in that group. An index of less than 100 means that the dark element predominates. The list includes persons of both sexes. The new aristocracy is fairer than the old. "We see that the aristocracy tend to be fairer than the ordinary population," says Mr. Ellis in the Monthly Review, "because it is from the fair elements in the ordinary population that the aristocracy is chiefly recruited. In other words, the fair tend to attain greater success than the dark in those careers that most frequently lead to the peerage. Thus it is that both created peers and peers and their sons are decidedly fairer than the old aristocracy." Persons of "extreme pigmentation" are too radical and too opposed to the powers that be to reach the House of Lords. They possess in too extreme a degree the sanguine, irrepressible energy, the great temporal ambitions, the personal persuasive force, the oratorical aptitudes.

The fairness of the group of artists is associated with the fact that artists tend largely to come from among the fair populations along the east coast of England and Scotland.

"An excess of fairness," says Mr. Ellis, "prevails among the men of restless and ambitious temperament, the sanguine and energetic men, the men who easily dominate their fellows and who get on in life, the men who recruit the aristocracy, and who, doubtless, largely form the plutocracy."

The fair man, Mr. Ellis thinks, tends to be bold, energetic, restless, and domineering, not because he is fair, but because he belongs to an aboriginal fair stock of people who possess those qualities. The dark man tends to be resigned and religious and imitative, yet highly intelligent, not because he is dark, but because he belongs to a dark stock possessing these characteristics.

• Fiery Breath

A BREATH OF FIRE

Anonymous; *Scientific American,* 47:329, 1882.

Dr. L. C. Woodman, of Paw Paw, Michigan, contributes the following interesting though incredible observation: I have a singular phenomenon in the shape of a young man living here, that I have studied with much interest, and I am satisfied that his peculiar power demonstrates that electricity is the nerve force beyond dispute. His name is Wm. Underwood, aged 27 years, and his gift is that of generating fire through the medium of his breath, assisted by manipulations with his hands. He will take anybody's handkerchief, and hold it to his mouth, rub it vigorously with his hands while breathing on it, and immediately it bursts into flames and burns until consumed. He will strip and rinse out his mouth thoroughly, wash his hands, and submit to the most rigid examination to preclude the possibility of any humbug, and then by his breath blown upon any paper or cloth, envelop it in flame. He will, when out gunning and without matches, desirous of a fire, lie down after collecting dry leaves, and by breathing on them start the fire and then coolly take off his wet stockings and dry them. It is impossible to persuade him to do it more than twice in a day, and the effort is attendant with the most extreme exhaustion. He will sink into a chair after doing it, and on one occasion, after he had a newspaper on fire as narrated, I placed my hand on his head and discovered his scalp to be violently twitching as if under intense excitement. He will do it any time, no matter where he is, under any circumstances, and I have repeatedly known of his sitting back from the dinner table, taking a swallow of water, and by blowing on his napkin, at once set it on fire. He is ignorant, and says that he first discovered his strange power by inhaling and exhaling on a perfumed handkerchief that suddenly burned while in his hands. It is certainly no humbug, but what is it? Does physiology give a like instance, and if so, where?

FIERY BREATH

Anonymous; *Science,* 8:630-631, 1886.

We had occasion in a recent number of Science to refer to a remarkable case in which the breath of an individual, or rather the eructations from his stomach, took fire when brought in contact with a lighted match. This case, which was reported in

the Medical record, has called forth communications from physicians by which it would appear that the phenomenon is not such a rare one as was at first supposed. In one case of disordered digestion the patient emitted inflammable gas from the mouth, which, upon analysis, was found to be largely composed of marsh gas. In another case the gas was sulphuretted hydrogen. A case is reported in the British medical journal, in which, while blowing out a match, the patient's breath caught fire with a noise like the report of a pistol, which was loud enough to awaken his wife. One evening, while a confirmed dyspeptic was lighting his pipe, an eructation of gas from his stomach occurred, and the ignited gas burned his mustache and lips. In Ewald's book on indigestion, the analysis of the gas in one of these cases was, carbonic acid, 20.57; hydrogen, 20.57; carburetted hydrogen, 10.75; oxygen, 6.72; nitrogen, 41.38; sulphuretted hydrogen, a trace. The origin of these gases is undoubtedly the undigested food, which in these cases undergoes decomposition.

• Sleepers and Suspended Animation

THE SLEEPING WOMAN OF THENOLLES

Anonymous; *English Mechanic,* 77:367, 1903.

Marguerite Boyenval, "the sleeping woman of Thenolles," died on May 28, after remaining in a trance for 20 years. On May 21, 1883, she was thrown into a cataleptic sleep through fear of a visit from the police, and it was found impossible to arouse her. Dr. Charlier, who attended the case during the whole period, succeeded in causing sensibility in his patient by giving subcutaneous injections of sulphate of atropine. The feet were first affected and gradually the whole body, as far as the neck, after which he could make no further progress, and ceased his treatment. The corpse-like rigidity immediately returned. The arms remained stretched out in any position in which they were placed. The doctor is of opinion that the woman was never conscious of what was going on around her, but Prof. Voitin, of the Salpetriere, thought that at times she heard vaguely what was being said to her. Through the whole 20 years' sleep her respiration remained perfectly normal, though her temperature was a little above the ordinary. About five months ago the doctors saw signs of returning consciousness, and renewed their efforts to revive her. For the first time on May 28 she opened her eyes, and remarked, "You are pinching me." When she fell asleep she was 22 years of age. During the whole of the time since then she has

been artificially fed. She began, however, to show signs of consumption, and wasted away to a skeleton. The progress of the disease accounted for her death. Doctors from all parts of the world visited Thenolles to see the sleeper, and the case is declared to be absolutely without precedent in medical science.

PREMATURE BURIALS

Mackay, G. Eric; *Popular Science Monthly,* 16:389-397, 1880.

The difference between death and a state of trance---or, as the Germans put it, Todt and Scheintodt---has never been quite clearly understood by the generality of mankind. Society, which sometimes does its best for the living, does not always do its best for the dead (or those who appear to be dead), and he would be a bold man who, without statistics, should assert that men, women, and children are never, by any chance, buried alive. Are the bodies of the poor always examined with care before burial? Are deaths properly verified in days of epidemic, that is to say in days of social panic?

I propose in this article to call attention to a few instances of premature burials on the Continent of Europe: instances which involve stories of trance, or Scheintodt---a trance, the semblance of death, holding its sway over the human body for hours and days, and not merely for minutes, as in the case of ordinary fainting-fits. In days when land is dear, and burial rights are less sacred than the rights of builders and contractors, coffins have been opened with the pickaxe, in the act of converting cemeteries into streets and gardens. Here a grave has been discovered whose inmate has turned in its shroud; here a corpse clutching its hair in a strained and unnatural position: dead men and dead women lying in their graves as dead men never lie in a Christian land at the moment of burial. The presumption is, that these people have been legally murdered.

A few months ago a young and beautiful woman, on the eve of her marriage with the man she loved, was buried in the neighborhood of Lodi, in Piedmont, in accordance with the doctor's certificate. The doctor was of opinion that the girl had died from excitement---overjoy, it is said, at the prospect of being married, but the legal name for the catastrophe was disease of the heart, and with this verdict her place in society was declared vacant. When the first shovelful of earth was thrown down on the coffin, strange noises were heard proceeding therefrom, "as of evil spirits disputing over the body of the dead." The grave-diggers took to flight, and the mourners began praying; but the bridegroom, less superstitious than the others, insisted on the coffin being unnailed. This was done; but too late: the girl was found in an attitude of horror and pain impossible to describe; her eyes wide open, her teeth clinched, her hands clutching her hair. Life was extinct; but, when laid in her shroud the day before,

her eyes were closed, her hands were folded on her breast as if in prayer. (pp. 379-390)

LATENT LIFE, OR, APPARENT DEATH

Harris, D. Fraser; *Scientific Monthly,* 14:429-440, 1922.

What is known as trance or narcolepsy is the form which latent life takes in the human being. Every now and again we hear of cases of persons, usually young women, going into profound and prolonged sleep from which they do not awake for weeks or months. During that time they take no food, they scarcely breathe, their heart's action is at a minimum. This is of course quite different from the hypnotic or mesmeric trance. Some people fear this state of trance very much; they are in dread of falling into it and being buried before they are really dead. Hence they insert explicit injunctions in their will that their physician is to open a vein or in some other way assure himself that they are dead before burial is permitted.

It is doubtless true that certain persons have been buried alive in the sense that while the heart's action was still at a minimum, they have been placed in a coffin. Stories of persons "laid out" for the undertaker, and reviving on his arrival are not unknown. Some persons have revived on the bier; but the number of persons buried while the body as a whole lived is in reality very small. Moribund persons have been buried at times of great confusion during plagues and epidemics.

Possibly the most famous case of narcolepsy is that of Colonel Townsend of Dublin which has been described by the well-known Dr. Cheyne:

> He could die or expire when he pleased, and yet . . . by an effort he could come to life again. He composed himself on his back and lay in a still posture for some time . . . . I found his pulse sink gradually, till at last I could not feel any by the most exact and nice touch.
>
> Dr. Baynard could not feel the least motion in the breast nor Dr. Skrine perceive the least soil on the bright mirror he held to his mouth . . . . could not discover the least symptom of life in him. We began to conclude he had carried the experiment too far, and at last we were satisfied he was actually dead . . . . By nine in the morning . . . . as we were going away we observed some motion about the body, and upon examination found his pulse and the motion of his heart gradually returning; he began to breathe heavily and speak softly.

Still more extraordinary are the narratives of the Fakirs of India who are said to allow themselves to be built up in sealed tombs for weeks without food and to be alive at the end of that time. Reports of these cases of human suspended animation are

now too numerous and too well authenticated by European eye-witnesses of unimpeachable integrity to be set aside as either in themselves untrue or as due to collective hallucination.

Many people if asked to give an example of suspended animation would refer to the case of some one apparently dead through drowning. Strictly speaking a person rescued from downing may be moribund, but not quite dead; there is, in physiological language, enough local tissue life present to ensure the living of the entire organism provided oxygen be got into the blood and so to the tissues before they utterly perish. Therefore, still speaking strictly, a drowned person is not inlatent life, not in a condition which can be kept up indefinitely and which will pass into full life in due time. On the contrary, a drowned person is dying; but most fortunately, the several tissues do not die the moment the individual as a whole dies but can survive long enough to be revivable if only enough oxygen can reach them sufficiently soon. Of course, it all depends on the heart and nervous system; if the heart is dead the individual cannot live again; if the heart, though moribund, is capable of absorbing oxygen and of beating again, the individual will live provided also his central nervous system and particularly the center for breathing is still alive. In the actual practice of "first aid," it is well to assume that the person is alive and to persevere with artificial respiration while keeping the body warm for as long a time as two or three hours before pronouncing life extinct. (pp. 437-438)

PHENOMENALLY SLOW BREATHING

Anonymous; *Science*, 70:sup xii-xiv, December 27, 1929.

A physiological celebrity, a young woman physiologist who breathes only three to five times a minute, has been the object of great scientific interest and public curiosity during the Carnegie Institution of Washington's annual exhibition of scientific work.

The average adult breathes fifteen to eighteen times a minute, and the extremely slow normal breathing of the young woman, now an assistant in physiology in Goucher College, Baltimore, is considered by Dr. Francis G. Benedict, of the Carnegie Institution's Nutrition Laboratory at Boston, to be absolutely unique.

Not even ill patients in hospitals who exhale and inhale ten times a minute approach the usual rate of respiration of this physiologist who, while waiting to aid science by studying the cause of her unique ability and demonstrating it to the public, desires to remain anonymous in printed reports.

Her unique rate of respiration was discovered about five years ago when as a student of physiology at Mt. Holyoke she noted that she did not breathe as fast as her fellow students. The phenomenon came to the attention of Dr. Benedict, a leading research worker in the field of human physiology. In lecturing to some 70,000 physicians in some 20 cities of Europe he tried to

find a similar case. A thorough search of the scientific literature was also made. No other instance of such slow breathing has been found.

The depth of the breathing is greater than normal. She inhales three pints of air at a time whereas ordinary people take in and let out only one pint. Her lung capacity, however, is normal and not larger than that of the average person. The air passing through her lungs is normal in amount and the amount of oxygen she extracts from it is also normal.

If she were a singer, she would be able to hold her notes a phenomenally long time. Dr. Benedict has urged her to attempt singing as an experiment, but her interests lie in the field of scientific research rather than in music. If she were a sprinter, which she is not, she would probably be able to run 200 yards on a single breath whereas the ordinary runner completes a hundred-yard-dash in one intake of air.

. . . . .

SUSPENDED ANIMATION

Anonymous; *Nature*, 138:215, 1936.

The Indian-Journal-of-Medical-and-Physical-Science of August 1, 1836, gives the following account of a man who submitted to be buried alive for a month and was dug out alive at the end of that period: "He is a youngish man, about 30 years of age . . . he generally travels about the country, and allows himself to be buried for weeks or months by any person who will pay him handsomely for the same. The man is said by long practice to have acquired the art of holding his breath by shutting the mouth and stopping the interior opening of the nostrils with his tongue; he also abstains from solid food for some days previous to his internment, so that he may not be inconvenienced by the contents of his stomach, while put up in his narrow grave; and, moreover, he is sewn up in a bag of cloth, and the cell is lined with masonry, and floored with cloth so that the white ants and other insects may not easily be able to molest him. The place in which he was buried at Jaisulmer is a small building about 12 ft. by 6 ft., built of stone, and in the floor was a hole about 3 ft. long, 2½ ft. wide, and the same depth, in which he was placed in a sitting posture, sewed up in his shroud, with his feet turned inwards towards the stomach, and his hands also pointed inwards towards the chest. Two heavy slabs of stone, 5 or 6 ft. long, several in. thick and broad enough to cover the mouth of the grave, so that he could not escape, were then placed over him. The door of the house was also built up, and people placed outside that no trick might be played. At the expiration of a full month the walling up of the door was broken and the buried man dug out of his grave. . . . He was taken out in a perfectly senseless state, his eyes closed, his hands cramped and power-

less, his stomach shrunk very much, and his teeth jammed so fast together that they were forced to open his mouth with an iron instrument to force a little water down his throat. He gradually recovered his senses and the use of his limbs."

CURIOSITIES OF BEHAVIOR

• Effects of Magnetic Fields on Human Behavior

ARE MAGNETIC FIELDS LIKE STROBE LIGHTS?

Anonymous; *New Scientist,* 69:674-675, 1976.

Seven years ago V. M. Mikhailovsky and others (Proceedings of the Ukranian Academy of Sciences, 1969, no 10, series B, p 929) noted that a 1000 gamma magnetic field (about 1 per cent of the normal magnetic field on the surface of the earth) varied at frequencies of 0·1-12 Hz caused a "measurable reaction" to the EEGs of three of 10 people studied. In one case, the field caused a "radical change in the character of the EEG", producing a new rhythm of 0·4-0·7 Hz. The magnetic wave also caused increased pulse rate, cold sweat, and headaches for this person.

Now Dr. Stanton Maxey, a Florida surgeon, has found similar results in a study at Martin Memorial Hospital, Stuart, Florida. In two of 13 people tested, 7-13 Hz magnetic waves produced brain waves of similar frequency. The patterns revert to normal as soon as the magnetic field is cut off, Maxey said in a paper presented late last year at an International Union of Radio Science meeting in Boulder, Colorado.

Maxey also noted that magnetic waves of this intensity occur naturally in storm systems, and that low frequency magnetic waves easily penetrate airplanes. "One may suspect that sensitive subjects will experience an altered state of consciousness and that their reaction times will be prolonged."

Finally, Maxey---himself a pilot and certified flight instructor ---points to the "pilot error" weather related air crashes that occur approximately once in 500,000 flying hours. Many of these are quite inexplicable, with tapes showing that pilots totally failed to respond to obvious warnings. Could it be, he asks, that these

are the pilots who are most sensitive to low frequency magnetic waves?

EFFECT OF MAGNETIC FIELDS ON REACTION TIME PERFORMANCE

Friedman, Howard, et al; *Nature,* 213:949-950, 1967.

In previous investigations we indicated some significant empirical relationships between selected geophysical parameters and gross measures of human behaviour. The present investigation attempts to demonstrate the effects of artificially produced magnetic fields on a standard, relatively uncomplicated, psychomotor task, simple reaction time.

In our initial attempt, twin Helmholtz coils, 14.5 in. in diameter, were mounted in a concealing wooden frame to provide an 11.5 in. coil interspace. The frame, vertically movable, was attached to a wooden chair so that seated subjects, using a chin rest, could have the cerebrum approximately at the centre of the transverse magnetic fields. A gaussmeter probe was mounted in the wooden frame so as to monitor constantly the magnetic field at 1-2 in. above the centre of the subject's head. In a darkened room, each subject was instructed to press and promptly release a telegraph key, mounted on a lapboard, as quickly as possible after the appearance of an eye-level red light 7 ft. away. Three experiments were conducted: the first with equipment as described and visual read-out of a timer, the second with equipment as described but with an automatic print-out timer and an added oscillator, and the third with more efficient, commercially fabricated coils and heavier power supply. It should be cautioned that all gauss levels mentioned in these experiments reflect not absolute levels of magnetic intensity to which a subject is exposed, but those levels over and above the naturally occurring magnetic field intensity already extant in the experimental area. Further, because of the lack of field homogeneity in such a large interspace with 14 in. coils and the need to keep the probe 1 or 2 in. above the subject's head, the centre of the force field tended to give field strengths approximately one third greater than was read from the gaussmeter.

[Experimental details omitted.]

In general, then, the findings indicate that experimentally produced modulated magnetic fields can significantly affect reaction time performance.

GEOMAGNETIC ENVIRONMENT TO HUMAN BIOLOGY

Becker, Robert O.; *New York State Journal of Medicine*, 63:2215-2219, 1963.

Physiologic effects of changes in magnetic field strength. To determine whether any of these variations would be of physiologic significance we must know the effects of exposure of organisms to increased and decreased field strengths as well as what effects the naturally occurring earth's fields have on the normal organism. To interpret these data in a rational fashion it is further necessary to know the physiologic mechanisms that are involved in causing the effects.

Some work has already been done on the physiologic effects of exposure of organisms to high-strength fields. Most of these studies have involved simple living systems or exposure of larger organisms in a random orientation to the field direction. The majority of the reports have noted growth disturbances of some type. The exposure of fruit flies to very intense fields resulted in death. Speculation on the possible mechanism of these actions of high-strength fields is generally based on the differential action of a magnetic field on various molecules, depending on whether they are para- or diamagnetic. One objection to this thesis is that the thermal agitation of the molecules in the cellular material would have a greater range than any separation or displacement produced by the differential action of the field. This objection no longer appears valid in view of the findings of the electron microscopists. Protoplasm is no longer regarded as a disorganized soup with random molecular structure but as a well-ordered complex with important functions being performed by structures with very tight molecular tolerances. It is not too difficult, therefore, to conceive of a differential effect producing a spatial separation in a multimolecular system, the functioning of which is a factor of spatial arrangements. The differential effects on dia- and paramagnetic molecules should be greater with very nonuniform fields or fields having steep gradients. That this is precisely the observation that has been made appears to lend support to the initial hypothesis.

We find that little experimentation has been done on the effects of fields lower than the earth's normal. Tchijevsky, who was primarily interested in air ionization and cosmic radiation, probably produced in his experimental procedure a decrease in the natural magnetic field. He reported a rather rapid onset of inanition and death in rats under these conditions. In our laboratory we became interested in this problem, and using magnetic shielding materials we have exposed cultures of Staphylococcus aureus to a reduced magnetic environment. This was actually a "resistive" reduction, and the remaining field inside the enclosure was both detectable and varied with the same frequency as did the natural field. However, both average field strength and magnitude of fluctuation were reduced by a factor of 10. The cultures were introduced four hours after plating from a previous broth culture. The organisms grew well at room temperature, and the exposure

time of seventy-two hours covered the major exponential growth period.

The experimental cultures in all dilutions showed a 15-fold reduction in number of colonies as well as some reduction in size of colonies as compared to the control cultures. These were actually preliminary experiments, and while they were performed on Staph aureus several times with uniform results on any differential, para- or diamagnetic action. However, a possible clue to the mechanism of action may be in a consideration of the physiologic significance of the earth's natural magnetic field.

It has long been noted that certain cyclic phenomena in living organisms have periods closely approximating the major geophysical cycles (circadian and lunar month). These biologic cycles have been shown to continue in regular rhythmic fashion even in the total absence of environmental cues of light, temperature, barometric pressure, and so forth. How can one explain the persistence of a circadian rhythm of activity in an organism that is exposed to absolutely constant environmental factors? Two major hypotheses have been advanced. One suggests that the organism possesses an internal autonomous clock or oscillator that regulates the cyclic behavior. However despite extensive work, the exact nature of this master clock remains unknown. The alternative hypothesis advanced is that the biologic rhythms are dependent for their timing on subtle geophysical rhythms. Some evidence has been obtained indicating that the magnetic field with its rhythmic fluctations is the geophysical parameter of significance. Marked disturbances in cyclic behavior of organisms have been produced by exposure to magnetic fields as low as a few gauss. The human organism also exhibits biologic cyclic behavior, demonstrating a time relationship to the variations in the magnetic field. If cyclic behavior can be viewed as a rhythmic variation in the level of irritability of the organism, would the naturally occurring magnetic storms produce any demonstrable variation in irritability level?

Previous work indicated a statistical relationship between magnetic storms and suicides in the human population. In a preliminary statistical study of the relationship between the average daily magnetic field variations and the incidence of hospital admissions for neuropsychiatric disturbances, we obtained a coefficient of correlation of + 0.26. The probability of obtaining this result by chance alone is less than 1 in 10,000. At present, this work is continuing with much larger volumes of data and automatic processing equipment. It therefore appears that the variations in the earth's magnetic field may be in part responsible for driving the cyclic activity of living organisms. Again, the strength of this field is far too low to permit any serious consideration of dia- or paramagnetic effects in this relationship.

There is, however another class of phenomena worth consideration. The galvanomagnetic effects are the result of interaction between the flow of direct current electricity and steady or pulsating magnetic fields. These effects are normally quite small in ordinary electrical equipment and can be disregarded. However, for certain reasons they are increased over a million times in magni-

tude in semiconductors and other similar solid-state electronic systems. Work in the author's laboratory over the past few years has indicated that such an electronic phenomenon is an important characteristic of the nervous system. This solid-state electronic current appears to be a prime factor in determining the level of irritability of the neurones, as judged by their threshold and action potential activity. Further, in all the vertebrates examined, including man, it appeared that some midline structure or structures in the brain, possibly brain stem reticular formation, demonstrated this activity to a high degree and that the magnitude and perhaps polarity of this electronic current flow directly determined the level of consciousness. Considering that in the vertebrates activity of the brain determines the over-all behavior of the organism, it is not difficult to conceive of a galvanomagnetic effect between the fluctuations of the magnetic environment and this solid-state electronic system in the brain stem producing the subtle behavioral alterations of biologic cycles. In this regard, it is interesting to note that the latest observations on the earth's magnetic field indicate that it is subject to continuous pulsations of low magnitude, with a frequency ranging from 0.1 to 100 cycles per second. The major components of the phenomenon appear to be centered around 8 to 16 cycles per second. The suggestion has been made that this phenomenon may be related to the average frequencies of the electroencephalogram.

Since this current system in the brain has a definite vector orientation (midline, frontoccipital) and can be measured on the exterior surface of the head, it can be subjected to experimentation under varying circumstances. In theory, the imposition of a steady magnetic field at right angles to the direction of this current flow should produce a deviation in the path of some of the charges flowing. If these can find new circuit paths at this new vector, then the amount of current to be delivered along the original vector will be decreased. From our experiments on the effects of increasing or decreasing the total electrical current flow along the path by the addition of externally generated current, we know that a relative diminution in this amount of current will cause a decrease in the level of consciousness. Therefore the imposition of a magnetic field oriented at right angles to the brain stem should alter the electroencephalographic pattern in a fashion to indicate a decrease in the level of consciousness. In addition, a rough linearity should exist between the field strength applied and the magnitude of the charge induced.

Such experiments have been done, and in every case when field strengths of from 2,500 gauss or more were applied, the electroencephalographic pattern has changed from that of a moderate amplitude alpha to a high amplitude delta type, which is typical of moderate to deep anesthesia. The behavior of the experimental animal is also appropriate; he remains immobilized within the field of the magnet. It was further noted that no change could be produced in the electroencephalographic patterns of heavily anesthetized animals. Since our other data indicate that this state is associated with a maximal reduction in current flow, no effect was anticipated. These results clearly indicate that a

magnetic field of proper magnitude and orientation can reduce consciousness to levels approximating general anesthesia, as judged by the electroencephalogram. It seems quite likely that fields of much lower magnitudes would also be capable of changing the level of irritability sufficiently to produce a deterioration in the performance of complex tasks. Other work with alteration of consciousness induced by application of externally generated direct currents has indicated that modulation of these currents by low frequencies (0.1 to 10 cycles per second) enhanced the physiologic effect. While it has not yet been possible to evaluate the effect of magnetic fields varying at these same frequencies, the inference would be that similar potentiation would occur.

. . . . .

• Effects of Natural Infrasound

CORRELATION OF NATURALLY OCCURRING INFRASONICS AND SELECTED HUMAN BEHAVIOR

Green, J. E., and Dunn, F.; *Acoustical Society of America, Journal,* 44:1456-1457, 1968.

It has been known that intense weather phenomena have demonstrable, though subtle, influences upon human behavior. The literature, since antiquity, is replete with examples dealing with numerous types of effects extending from increased incidence of such catastrophic events as suicide attempts to more gentle effects such as forgetfulness and mild malaise. Several basic physical phenomena known to attend storm activity have been considered responsible for the observed changes in behavior---among which are changes in barometric pressure, changes in relative humidity, changes in ozone concentrations of the air, and high winds. More recently, it has been discovered that very-low-frequency sound waves having periods of oscillation in the range 10^3 to 10^{-1} sec are produced during severe storm activity such as high winds and tornadoes, as well as during other naturally occurring phenomena such as earthquakes and volcanic activity. Such naturally produced infrasonic waves travel over great distances through the lower atmosphere, from the point or origin, experiencing little attenuation. Further, recent experiments with man-made infrasonics have shown that disturbances are produced in normal human behavior similar to those attending severe weather phenomena. Thus, the question arises whether the origin of such dis-

turbances is associated with local phenomena such as winds, barometric pressure, ozone concentration, etc., or whether distant phenomena, such as tornadoes, producing low-frequency acoustic waves that propagate over appreciable distances are responsible. This paper reports on an initial attempt at such a determination.

The plan for study involved: (1) locating a high-population-density region that had, for a period of approximately one month, mild or innocuous local weather conditions during a period when severe weather phenomena were abundant elsewhere; (2) obtaining information on such severe weather phenomena on the North American continent that could produce infrasonic waves arriving in the locality of study during the chosen period; (3) obtaining information on selected aspects of human behavior in the locality of study during the chosen period as a function of time; and (4) determining whether a correlation existed between the presence, in the locality of study, of distantly produced infrasonic waves and time-dependent disturbances to behavior in this locality.

The area defined by "Chicago, Illinois," was chosen for the locality of study because of the investigators' ability to obtain the pertinent data for this area. The first 28 days of the month of May 1967 were selected as the period of the study since weather conditions in the immediate area of Chicago, Illinois, were suitably mild---while, at the same time, severe storms occurred in other regions of North America. Weather data were obtained from the U. S. Department of Commerce; and barograph perturbations, which have been shown to be indicators of the presence of infrasonic activity, were examined for the Chicago area. By these methods, which yielded nearly identical results, it was determined that infrasonic waves produced by distant storms (up to 1500 miles away) should have been present in the Chicago area for 13 of the first 28 days of May 1967.

Two types of human activities were selected for correlation with the determined periods of infrasonic disturbances--viz., automobile accidents and absenteeism among school children. It was felt that these activities, both of which are well-defined events, would most likely be affected by the presence of infrasound waves. As shown by others, infrasonic waves may produce symptoms that could cause an increase in accident rate. Similarly, nausea, fatigue, and other infrasonic effects could induce individuals to remain at home rather than going to school. To emphasize any infrasonic effects, absenteeism among primary school children was chosen over absenteeism among secondary school students since it was felt that the latter group would be less inclined to absent themselves because of minor symptoms.

. . . . .

The entries of Table I suggest that a relationship exists between the presence of strong infrasonic waves, generated by natural phenomena, and the frequency of automobile accidents. It further appears that a similar relationship exists between the presence of infrasonic waves and the absence rate of students in elementary schools, although the significance of this relationship cannot be determined without further investigation of the normal

daily absence pattern of these students.

. . . . .

• Human Activities and Astronomy

SUNSPOTS AND THE BUSINESS CYCLE

Sparkes, J. R.; *Nature*, 252:520, 1974.

The report of the work of Dr. J. W. King and his colleagues at the Appleton Laboratory (252, 2; 1974) recalls articles contributed to Nature almost a century ago by William Stanley Jevons, economist and statistician, in which he sought to evidence the relation between sunspots and commercial crises (19, 33: 1878 and 19, 588; 1879).

. . . . .

Jevon's ideas were spelt out in a second paper to the British Association, "The Periodicity of Commerical Crises and its Physical Explanation", and the first of two articles contributed to Nature. Although still concerned about the evidence on which his theory of crises was based, he had great confidence in its substantial truth.

Keynes, in his essay, listed the new discoveries which were his excuse for returning to his theory:

(1) He had succeeded in carrying back the history of commercial crises at ten or eleven-year intervals almost to the beginning of the eighteenth century.

(2) He was now advised by his astronomical friends that the solar period was not 11.1 years, as he has previously supposed, but 10.45 years, which fitted his series of commercial crises much better.

(3) He now abandoned European harvests, the price statistics for which yielded negative results, as the intermediary through which sunspots affected business, in favour of Indian harvests, which, he argued, transmitted prosperity to Europe through the greater margin of purchasing power available to the Indian peasant for buying imported goods.

But apart from the coincidence of the periodicity of the commercial crises and the solar period, the causal link was never much stronger than a personal conviction that the decennial crises depended on meteorological variations of similar periodicity. His 1875 evidence, which depended on European harvests, was overturned in 1878 in favour of fluctuations in foreign trade resulting

from cyclical crop changes in India and elsewhere. But even then the timing of the relationship was such as almost to suggest that the effect preceded its cause.

Later empirical work has largely discredited the theory that there is a link between business cycles and a 10 to 14 year cycle in sunspot activity. Business cycles have recurred at shorter intervals than 10 or 11 years, and in spite of ingenious interpretations of harvest statistics over the years, it is now generally agreed that such an explanation could never completely account for the business cycle. Keynes's assessment is summed up as follows: "Jevon's notion, that meteorological phenomena play a part in harvest fluctuations and that harvest fluctuations play a part (though more important formerly than today) in the trade cycle, is not to be lightly dismissed."

COMMERCIAL CRISES AND SUN-SPOTS

Jevons, W. Stanley; *Nature,* 19:33-37, 1878.

It is curious to notice the variety of the explanations offered by commercial writers concerning the cause of the present state of trade. Foreign competition, beer-drinking, over-production, trades-unionism, war, peace, want of gold, superabundance of silver, Lord Beaconsfield, Sir Stafford Northcote, their extravagant expenditure, the Government policy, the wretched Glasgow Bank directors, Mr. Edison and the electric light, are a few of the happy and consistent suggestions continually made to explain the present disastrous collapse of industry and credit.

It occurs to but few people to remember that what is happening now is but a mild repetition of what has previously happened time after time. October 1878, is comparable with May, 1866, with November, 1857, with October, 1847, and, going yet further back, with a somewhat similar condition of things, in 1837, in 1825-26, and even in 1815-16. The incidental circumstances of these commercial collapses have indeed been infinitely diversified. At one time the cause seemed to be the misconduct of the great firm of Overends; in 1857 there was the mutiny in India, the peace with Russia, and a commercial collapse in the United States; in 1847 occurred the Irish famine and a failure of European harvests generally; following upon the great railway mania; the crisis of 1837 succeeded an immense expansion of home trade, the establishment of joint stock banks, and the building of multitudes of factories and other permanent works; 1825 was preceded by extravagant foreign speculations and foreign loans; 1815 was the year of the general peace. All kinds of distinct reasons can thus be given why trade should be now inflated and again depressed and collapsed. But, so long as these causes are various and disconnected, nothing emerges to explain the remarkable appearance of regularity and periodicity which characterises these events.

The periodicity of the earlier portion of the series is so re-

markable that, even without the corroboration since received, it convinced scientific inquirers that there was some deep cause in action. Dr. Hyde Clarke, for instance, wrote, more than thirty years ago, a paper entitled "Physical Economy--a preliminary inquiry into the physical laws governing the periods of famines and panics." This paper was published in the Railway Register for 1847, and is well worth reading. In the commencement he remarks: "We have just gone through a time of busy industry, and are come upon sorrow and ill-fortune; but the same things have befallen us often within the knowledge of those now living. Of 1837, of 1827, of 1817, of 1806, of 1796, there are men among us who can remember the same things as we now see in 1847. A period of bustle, or of gambling, cut short in a trice and turned into a period of suffering and loss, is a phenomenon so often recorded, that what is most to be noticed is that it should excite any wonder." Dr. Hyde Clarke then proceeds to argue in a highly scientific spirit that events so regularly recurring cannot be attributed to accidental causes; there must, he thinks, be some physical groundwork, and he proposed to search this out by means of a science to be called Physical Economy. In the third page of his paper he tells us that he had previously written a paper on the laws of periodical or cyclical action printed in Herapath's Railway Magazine for 1838. "At this time," he says, "it way my impression that the period of speculation was a period of ten years but I was led also to look for a period of thirteen or fourteen years . . . In the course of these inquiries I looked at the astronomical periods and the meteorological theories without finding anything at all available for my purposes." A little below Dr. Hyde Clarke continues:--"Still thinking that the interval was an interval of about ten years, I was, during the present famine, led to look for a larger period, which would contain the smaller periods, and as the present famine and distress seemed particularly severe, my attention was directed to the famine so strongly felt during the French Revolution. This gave a period of about fifty-four years, with five intervals of about ten or eleven years each, which I took thus:--

"1793 1804 1815 1826 1837 1847."

Dr. Hyde Clarke was by no means the only statist who adopted a theory of periodicity thirty or forty years ago. In February, 1848, Mr. J. T. Danson read a paper to the Statistical Society of London, attempting to trace a connection between periodic changes in the condition of the people and the variations occurring in the same period in the prices of the most necessary articles of food. Mr. James Wilson had published, in 1840, a separate work or large pamphlet upon "Fluctuations of Currency, Commerce, and Manufactures," in which he speaks of the frequent recurrence of periods of excitement and depression. In later years Mr. Wm. Langton, the esteemed banker of Manchester, independently remarked the existence of the decennial cycle, saying: "These disturbances are the accompaniment of another wave, which appears to have a decennial period, and in the generation of which moral causes have no doubt an important share." The paper in which this remark occurs is contained in the

Transactions of the Manchester Statistical Society for 1857, and is one of the most luminous inquiries concerning commercial fluctuations anywhere to be found. In still later years Mr. John Mills of the Manchester Statistical Society has almost made this subject his own, insisting, however, mainly upon the mental origin of what he has aptly called the Credit Cycle.

The peculiar interest of Dr. Hyde Clarke's speculations consists in the fact that he not only remarked the cycle of ten or eleven years, but sought to explain it as due to physical causes, although he had not succeeded in discovering any similar astronomical or meteorological variation with which to connect it. Writing as he did in 1838 and 1847, this failure is not to be wondered at. His supposed period of fifty-four years is perhaps deserving of further investigation, but it is with his period of ten or eleven years that we are now concerned.

My own inquiries into this interesting subject naturally fall much posterior to those of Dr. Clarke; but, about the year 1862, I prepared two elaborate statistical diagrams, one of which exhibited in a single sheet all the accounts of the Bank of England since 1844, while the other embraced all the monthly statements I could procure of the price of corn, state of the funds, rate of discount, and number of bankruptcies in England from the year 1731 onwards. Subsequent study of these diagrams produced upon my mind a deep conviction that the events of 1815, 1825, 1836-39, 1847, and 1857, exhibited a true but mysterious periodicity. There was no appearance, indeed, of like periodicity in the earlier parts of my second diagram. In the first fifteen years of this century statistical numbers were thrown into confusion by the great wars, the suspension of specie payments, and the frequent extremely high prices of corn. It must be allowed, moreover, that the statistical diagram, so far as concerns the eighteenth century, presents no appreciable trace of decennial periodicity. The recent continual discussions concerning the solar or sun-spot period much increased the interest of this matter, and in 1875 I made a laborious reduction of the data contained in Prof. Thorold Rogers' admirable "History of Agriculture and Prices in England from the Year 1259." I then believed that I had discovered the solar period in the prices of corn and various agricultural commodities, and I accordingly read a paper to that effect at the British Association at Bristol. Subsequent inquiry, however, seemed to show that periods of three, five, seven, nine, or even thirteen years would agree with Prof. Rogers' data just as well as a period of eleven years; in disgust at this result I withdrew the paper from further publication. I should like, however, to be now allowed to quote the following passage from the M.S. of the paper in question:--

"Before concluding I will throw out a surmise, which, though it is a mere surmise, seems worth making. It is now pretty generally allowed that the fluctuations of the money market, though often apparently due to exceptional and accidental events, such as wars, great commercial failures, unfounded panics, and so forth, yet do exhibit a remarkable tendency to recur at intervals approximating to ten or eleven years. Thus the principal commer-

cial crises have happened in the years 1825, 1836, 1847, 1857, 1866, and I was almost adding 1879, so convinced do I feel that there will, within the next few years, be another great crisis. Now if there should be in or about the year 1879, a great collapse comparable with those of the years mentioned, there will have been five such occurrences in fifty-four years, giving almost exactly eleven years (10.8) as the average interval, which sufficiently approximates to 11.1, the supposed exact length of the sun-spot period, to warrant speculations as to their possible connection."

I was led to assign the then coming (that is, the now present) crisis to the year 1879, because 11.1 years added twice over to 1857, the date of the last perfectly normal crisis, or to 1847, the date of the previous one, brings the calculator to 1879. If I could have employed instead Mr. J. A. Brown's since published estimate of the sun-spot period, to be presently mentioned, namely 10.45 years, I should have come exactly to the present year 1878. My mistake of one year was due to the meteorologists' mistake of eight months, which, as crises usually happen in October and November, was sufficient to throw the estimate of the event into the next twelve months.

While writing my 1875 paper for the British Association, I was much embarrassed by the fact that the commercial fluctuations could with difficulty be reconciled with a period of 11.1 years time after time, we get 1836.1, 1847.2, 1858.3, 1869.4, 1880.5, which show a gradually increasing discrepance from 1837, 1847, 1857, 1866 (and now 1878), the true dates of the crises. To explain this discrepancy, I went so far as to form the rather fanciful hypothesis that the commercial world might be a body so mentally constituted, as Mr. John Mills must hold, as to be capable of vibrating in a period of ten years, so that it would every now and then be thrown into oscillation by physical causes having a period of eleven years. The subsequent publication, however, of Mr. J.A. Brown's inquiries, tending to show that the solar period is 10.45 years, not 11.1, placed the matter in a very different light, and removed the difficulties. Thus, if we take Mr. John Mills' "Synopsis of Commercial Panics in the Present Century," and, rejecting 1866 as an instance of a premature panic, count from 1815 to 1857, we find that four credit cycles occupy forty-two years, giving an average duration of 10.5 years, which is a remarkably close approximation to Mr. Brown's solar period. Thus encouraged, it at last occurred to me to look back into the previous century, where facts of a strongly confirmatory character at once presented themselves. Not only was there a great panic in 1793, as Dr. Hyde Clarke remarked, but there were very distinct events of a similar nature in the years 1783, 1772-3, and 1763. About these dates there can be no question, for they may all be found clearly stated on pp. 627 and 628 of the first volume of Mr. Macleod's unfinished "Dictionary of Political Economy." Mr. Macleod gives a concise, but, I believe, correct account of these events, and as he seems to entertain no theory of periodicity, his evidence is perfectly unbiased. Yet in the space of a few lines, he unconsciously states

this periodicity, saying:--"Ten years after the preceding crisis of 1763 another of a very severe nature took place in 1772 and the beginning of 1773. It extended over all the trading nations of Europe." A few lines below he goes on to state that in May, 1783, a rapid drain of bullion to the Continent set in, which greatly alarmed the Bank directors and embarrassed the merchants. The paragraph in which this occurs is headed "The Crisis of 1763." and on turning the page we at once come on another paragraph headed "The Crisis of 1793." Here then we have, in a few lines of a good authority concerning the history of finance, a statement of four crises occurring at almost exactly decennial intervals. It is wonderful that no writer has, so far as I know, previously pointed out the strictly periodic nature of these events; and I may add that I have several times lectured to my college classes about these crises without remarking their periodicity. It is true that we cannot, by any management of the figures, bring them into co-ordination with later crises so long as we adhere to the former estimate of the solar period. If, starting from 1857, we count back nine intervals of 11.1 years each, we get to 1757 instead of 1763; we are landed in the middle of a cycle instead of in the beginning or end; and there can be no possible doubt about the crises of 1763 and 1857. But, if we are once allowed to substitute the new estimate of Brown, which is the same as the old one of Lamont, the difficulty disappears; for the average interval is 10.44 . . . years!

This beautiful coincidence led me to look still further backwards, and to form the apparently wild notion that the great crisis generally known as that of the South Sea Bubble might not be an isolated and casual event but only an early and remarkable manifestation of the commercial cycle. The South Sea Bubble is generally set down to the year 1720, and the speculations in the shares of that company did attain their climax and commence their collapse in that year. But it is perfectly well known to the historians of commerce that the general collapse of trade which profoundly affected all the more advanced European nations, especially the Dutch, French, and English, occurred in 1721. Now, if we assume that there have been since 1721, up to 1857, thirteen commercial cycles, the average interval comes out 10.46 years; or if we consider that we are in this very month (November, 1878) passing through a normal crisis, then the interval of 157 years from 1721 to 1878 gives an average cycle of 10.466 years.

. . . . .

If it were permitted to draw any immediate conclusion from these speculations, I should point to the necessity of at once undertaking direct observations upon the varying power and character of the sun's rays. There are hundreds of meteorological observatories registering, at every hour of the day and night, the most minute facts about the atmosphere; but that very influence, upon which all atmospheric changes ultimately depend, the solar radiation, is not, I believe, measured in any one of them, at least in the proper manner. Pouillet showed long ago

(1838) how the absolute heating power of the sun's rays might be accurately determined by his Pyrheliometer. This instrument, and the results, which he drew from its use, are fully described in his "Elements de Physique Experimentale et de Meteorologie" (livre 8me, chap. i., section 285). But I have never heard that his experiments have been repeated, except so far as this may have been done by Sir John Herschel, with his so-called Actinometer, as described by him in the Admiralty Manual of Scientific Inquiry. I fancy that physicists still depend upon Pouillet's observations in 1837 and 1838 for one of the most important constants of the solar system, if constant it can be called. While astronomers agitate themselves and spend infinite labour about the two-hundredth planetoid, or some imperceptible satellite, the very fountain of heat and light and life is left unmeasured. Pouillet indeed assumed that the heating power of the sun's rays is a constant quantity, which accounts for his not continuing the solar observations. But, if there is any truth in all these sun-spot speculations, there must be a periodic variation in the sun's rays, of which the sun-spots are a mere sign, and perhaps an unsatisfactory one. It is possible that the real variations are more regular than the sun-spot indications, and thus perhaps may be explained the curious fact that the decennial crises recur more regularly on the whole than the maxima and minima of sun-spots.

But why do we beat about the bush when all that is needed is half-a-dozen of Pouillet's pyrheliometers with skilled observers, who will seize every clear day to determine directly the heating power of the sun? Why do we not go direct to the Great Luminary himself, and ask him plainly whether he varies or not? If he answers No! then some of us must reconsider our theories, and perhaps endure a little ridicule. But if, as is much more probable, he should answer Yes! then the time will come when the most important news in the Times will be the usual cablegram of the solar power. Solar observatories ought to be established on the table-lands of Quito or Cuzco, in Cashmere in Piazzi Smyth's observatory on the Peak of Teneriffe, in Central Australia, or wherever else the sun can be observed most free from atmospheric opacity. An empire on which the sun never sets, and whose commerce pervades every port and creek of the sunny south, cannot wisely neglect to keep a watch on the great fountain of energy. From that sun, which is truly "of this great world both eye and soul," we derive our strength and our weakness, our success and our failure, our elation in commercial mania, and our despondency and ruin in commercial collapse.

THE MOON'S EFFECT ON STAMMERING

Anonymous; *English Mechanic,* 62:222, 1895.

The following paragraph apears in Nature last week, but the "facts" require some verification:--"Facts are always worth re-

cording, and we publish the following note because it contains an interesting fact, which is, moreover, in accordance with other observations. The note came to us from Mr. Mata Prasad, Benares: 'It was quite accidentally observed, by a stammering friend of mine during the months of May and June last, that on moonlight nights he stammered more than on dark nights, and when he slept exposed to the rays of the moon during the month of June, he found that he stammered the most on days succeeding full moons, while a day just after the new moon, and a day before, he had not a single attack of the fit.'" It would be interesting to have other observations of a similar character, provided they could be verified.

IS THERE AN ASSOCIATION BETWEEN ASTROLOGICAL DATA AND PERSONALITY?

Hume, Nicholas, and Goldstein, Gerald; *Journal of Clinical Psychology,* 33:711-713, 1977.

Abstract. A test was made of the hypothesis that personality characteristics can be predicted on the basis of various features of the individual's astrological chart. Astrological charts were prepared for 196 college-age Ss who also were administered the MMPI and the Leary Interpersonal Check List. Ss were divided into those who had extreme scores on any of the 13 personality variable studied and those who did not. For each personality variable, comparisons were made on a large number of astrological dimensions between distributions of Ss and without extreme test scores. Six hundred thirty-two such comparisons were made and evaluated with chi-square tests. In that the obtained number of statistically significant chi-squares was less than what would be expected on a chance basis, the hypothesis was rejected.

GEOMAGNETIC PARAMETERS AND PSYCHIATRIC HOSPITAL ADMISSIONS

Friedman, Howard, et al; *Nature*, 200:626-628, 1963.

Although the relationship of natural environmental physical forces to human behavior has always been a matter of general interest and speculation, there has been a paucity of systematic investigations in this connexion among modern scientific workers. Buettner, in a critique of the literature bearing on correlations between bioclimatological parameters and human health and behavior, pointed to the general confusion in the field as well as to

the very limited unequivocal findings consisting of the establishment of two 'potentially harmful' weather types. The literature in the field of geomagnetic parameters and human behavior is more restricted in quantity and in significance of findings.

Possibly the most ambitious pertinent investigation is that of Dull and Dull. In an analysis of approximately 40,000 cases over a period of 60 months they demonstrated graphically a clearly observable relationship between the 67 magnetic storms occurring during this time, and the incidence of nervous and mental disease and suicides. Their results, however, were not subjected to statistical treatment. Berg, reviewing the literature pertaining to the relationships between environmental geophysical parameters and the human population, concluded that firm evidence for or against such relationships is still lacking.

The relative indifference of the behavioral scientists to this problem may possibly be due to the lack of that type of theoretical framework which could permit the inter-relationship of the two parameters to be drived as a logical consequence. There has been, however, a growing appreciation of, and body of empirical findings related to, one circumscribed aspect of the overall area, the biological effects of magnetic fields, particularly with reference to infra-human organisms. A parallel related concern has been the investigations into the electrical organizations of the living organism. Thus, in a review of recent work in the relationship between direct current electricity and living organisms, it was suggested that the direct current electrical system of the organism could be conceived as a control system which could be influenced by external force fields. These considerations led to a pilot investigation which examined the relationship between gross manifestations of extreme human psychological disturbance and natural magnetic field intensity. The psychiatric admissions to two Syracuse, New York, hospitals were correlated with K-index sums as measured at the Fredericksburg Magnetic Observatory, Virginia, for approximately a four-year period. The statistically significant correlation ratios obtained for non-linear relationships, η, of 0·26 and 0·27 suggested that at least some relationship exists between the incidence of psychiatric disturbance and some geophysical parameter associated with the magnetic field.

The investigation reported here is an extension of the pilot work utilizing a larger sample of psychiatric hospital admissions and, as advised by Bartels, a linear measure of the magnetic field intensity, ak, the equivalent 3-h range, rather than the quasi-logarithmic scale of K-indices. In addition, ap, the planetary counterpart to ak, was used.

Daily admissions to seven central New York State psychiatric hospitals and to the Psychiatric Service of a Veterans Administration General Medical and Surgical Hospital during July 1, 1957-October 31, 1961, were obtained. Transfers from other psychiatric institutions were excluded. Total admissions included in the data are 28,642.

For the same period of time K-indices, as determined at the Fredericksburg Magnetic Observatory, were obtained from the Coast and Geodetic Survey, U.S. Department of Commerce. The

K-index, as described by Bartels, reflects the amplitude, a, of the most disturbed component of the magnetic field measured every 3 h. The values of a are customarily condensed, for convenience of reporting, into a quasi-logarithmic scale for K, ranging from 0 to 9. In this work each 3-h K-index was reconverted into the equivalent 3-h range, ak. by means of a table of Bartels, thus providing a linear measure. The eight daily values of ak were then summed to arrive at a daily ak sum.

The planetary 3-h K-indices, Kp, as furnished by the U.S. Department of Commerce, Bureau of Standards, Central Radio Propagation Laboratory, Boulder, Colorado, were also reconverted into 3-h equivalent planetary amplitudes, ap, and then summed to obtain a daily ap sum.

It was decided to examine 7-, 14-, 21-, 28- and 35-day time-periods to correlate with the corresponding periods of the geomagnetic parameters for the following reasons. First, the time between onset of psychiatric disturbance necessitating hospitalization and actual admission varies with a host of uncontrollable factors, not the least of which are differing hospital admission procedures. It is also impossible to specify a priori the most meaningful duration and extent of geomagnetic event. Secondly, inspection of the hospital admission data readily revealed one artefact among the many possible contributors to the number of uncontrolled variables in the crude measure of this parameter. Every Sunday was marked by a striking drop in admissions, undoubtedly reflecting differing admission policies and procedures. The selection of the aforementioned time-periods thus provided a constant number of Sundays when all the possible 7-, 14-, 21-, 28- and 35-day intervals were examined.

To obtain the 7-, 14-, 21-, 28- and 35-day values, the number of admissions for every possible consecutive 7-, 14-, 21-, 28- and 35-day period was summed and correlated with the geomagnetic parameter sums for the corresponding periods. Thus 7-day periods yielded 7 correlations obtained from using days 1 through 7, 8 through 14, 15 through 21, etc., for the first correlation; then days 2-8, 9-15, 16-22, etc., for the second correlation; and continuing on in similar fashion to the seventh correlation which used days 7-13, 14-20, 21-27, etc. This provided 3 correlations with 226 pairs of comparisons (N), and 4 with 225 pairs. The other time-periods were treated similarly, thus giving 3 correlations with N's of 113 and 11 correlations with N's of 112 for the 14-day periods; 10 correlations with N's of 75 and 11 with N's of 74 for the 21-day periods. 17 correlations with N's of 56 and 11 with N's of 55 for 28-day periods, and 10 correlations with N's of 45 and 25 with N's of 44 for the 35-day periods.

In order to compare admissions to the greater intensities of geomagnetic disturbance, a listing of the principal magnetic storms from July 1, 1957, until October 31, 1961, identified by the Fredericksburg Magnetic Observatory, Virginia was obtained from reports contained in the Journal of Geophysical Research and International Geophysical Year Bulletins. The number of days of storm in the 7-, 14-, 21-, 28- and 35-day periods were then

correlated with the hospital admissions for the corresponding time-periods.

The product moment correlation coefficients, r, derived from the comparison of total geomagnetic activity parameters, as reflected in ak and ap with hospital admissions are negligible in magnitude regardless of time-period used. Table 1 presents the correlation coefficients obtained from the comparison of the 7-, 14-, 21-, 28- and 35-day periods of hospital admissions with the number of days of magnetic storm for the corresponding periods of time. The correlations between the number of days of magnetic storm and admissions for 7-day periods can be considered as negligible. The 14 correlation coefficients obtained by using the 14-day periods range from +0·077 to +0·221, the latter figure being statistically significant (P<0·05). The correlations of the 21-day periods range from +0·116 to +0·305 with 5 statistically significant (P<0·05) coefficients. The 28-day correlations range from +0·167 to +0·332, and 13 are statistically significant (P<0·05). The 35-day correlation coefficients range from a low of +0·222 to an unexpected high of +0·345, and 11 are statistically significant (P<0·05).

Table 1. Correlations between Days of Magnetic Storm and Psychiatric Hospital Admissions

Admission periods	No. of r's	Range	Median r
7-day	3(N = 226) + 4(N = 225)	-0·010-+0·122	+0·043
14-day	3(N = 113) + 11(N = 112)	+0·077-+0·221	+0·117
21-day	10(N = 75) + 11(N = 74)	+0·116-+0·305	+0·182
28-day	17(N = 56) + 11(N = 55)	+0·167-+0·332	+0·261
35-day	10(N = 45) + 25(N = 44)	+0·222-+0·345	+0·279

The use of unscreened heterogeneous hospital admissions as a measure of alteration in human behaviour is admittedly crude. The host of uncontrolled variables makes this at best a gross approximation to psychological disturbance in the human population at large. If one becomes intrigued with aetiological considerations there are then the further problems of the unknown time-period between occurrence of geomagnetic event and its significant effect on the organism, and the specification of the duration and extent of meaningful geomagnetic events. Since the purpose of this work was simply to investigate whether any relationship could be found to exist between geomagnetic parameters and human behaviour, it was hoped that some of the effects of the uncontrolled variables in the behavioural parameter could be crudely minimized by randomization within a large sample, and that the problem of unknown time for effect of geomagnetic event could be attenuated in part by the use of 7-, 14-, 21-, 28-, and 35-day time-periods. The results indicate that statistically significant low to marked linear relationships exist between geomagnetic parameters and a gross measure of human disturbance. These relationships are evident when the measure of the geomagnetic parameter is restricted to those periods of higher disturbance which can be categorized as

magnetic storms. The total range of geomagnetic activity, as reflected in planetary (ap) and more local (ak) natural magnetic field intensity parameters, does not reveal any significant relationship with psychiatric hospital admissions.

The use of the five time periods provides no definitive information about the optimal time between geomagnetic event and onset of psychological disturbance or time to achieve hospital admission. However, inasmuch as the magnitude of the maximum correlation tends to increase with the size of the time-period used, it suggests, in part, that the optimal time may be closer to thirty-five days than seven days. In part, it probably also reflects the attenuation of transient artefacts inherent in the hospital admission data.

Although correlational investigations shed but little light on casual relationships, they nevertheless do provide a fertile field which can yield more significant testable hypotheses. In view of the diversity of the two variables under investigation and the grossness of the measures, the present investigation offers correlations of a surprising magnitude. In general, the tentative conclusion of the pilot study can be reaffirmed: a significant relationship has been shown between psychiatric disturbance as reflected in hospital admissions and natural magnetic field intensity. In any interpretation of this relationship it should be cautioned that it is beyond the scope of this investigation to determine whether the more meaningful geophysical parameter is magnetic field intensity per se, or some other geophysical parameter intimately associated with it. Speculatively, the results are in keeping with the conception of the behavior of an organism being significantly influenced, through the direct-current control system, by external force fields. Attention is thus invited to a hitherto neglected dimension in the complexity of psychopathology specifically, and generally in all human behaviour.

LUNAR PERIODICITY IN HOMO SAPIENS L.

Gunn, Donald L., et al; *Nature,* 139:841, 1937.

The belief in a connexion between sexual periodicity and the moon is very ancient. In particular, it is commonly thought that there is some connexion between the moon and menstruation during the new moon, thus associating the periodicity with the synodical lunar month of 29.55 days. Arrhenius concluded from data of nearly 12,000 menstruations that there is a slight tropical lunar rhythm of 27.32 days, and that the average duration of the menstrual cycle is just under 27 days.

We have reinvestigated these questions with data of more than 10,000 menstruations. We find a great variation in the duration of the cycle, both in one individual and between different individuals. The average duration in our cases during 1928-30 was 29 days, but there was a clear inverse correlation between age

and the duration of the cycle. Thus the cycle became shorter by a day with every five or six years increase in age, the oldest group (born before 1891) giving an average of 26.9 days, and the youngest (born after 1905) of 30.2 days. If, therefore, the average cycle of one age group is equal in length to any lunar cycle, then this equality will not be present in any other age group. Similarly, if the average for a whole population corresponds with a lunar cycle, the correspondence is unlikely to be present in another population having a different age distribution. Since Arrhenius's average for the menstrual cycle was derived from data which mostly gave the time interval in whole weeks, while in ours the individual averages err by not more than ± 0.1 day, his conclusion can no longer be accepted.

An exhaustive examination of the number of our cases starting to menstruate on each day of each kind of lunar month has been made, involving the statistical and graphical tests. No correlation of any kind was found. Arrhenius's data were known by him to be inaccurate, and they showed a systematic error, inherent in their mode of collection, which itself could give the appearance of a lunar periodicity. Our data are far more accurate and do not contain this source of error. We therefore conclude that there is no casual relation between the moon and menstruation, and that the well-known approximate coincidence in the duration of the cycles is fortuitous.

A full account of this investigation, embodying a number of other conclusions, will appear in October in the Journal of Obstetrics and Gynaecology of the British Empire.

DOES THE MOON INFLUENCE BIRTHS?

Anonymous; *New Scientist,* 35:472, 1967.

The supposed influence of the Moon on human affairs and behaviour is the subject of a rich folklore, little of which is still taken seriously in this scientific age. Those of us who are inclined to scoff, whenever such old wives' tales are told must be a little shaken to find that a scientific study has now indicated that the phases of the Moon affect human conception and birth.

The report of the research appears in a recent issue of the American Journal of Obstetrics and Gynaecology (1967, Vol. 98, p. 1002) and is the result of a painstaking analysis of the pattern of births in New York City over a period of three years. The author, Dr. Walter Menaker, took as his raw data the dates of birth of the half a million babies who were born in the city between January 3rd, 1961 and the end of 1963. He then compared the numbers of births occurring at various times during the lunar month. During the period concerned there were exactly 37 "synodic cycles" of the Moon (each lasts approximately 29 days and 13 hours).

Dr. Menaker calculated the number of babies born, over the

three years, in each of a series of "half cycles", made by bisecting the lunar cycle at 15 different points. Some of the pairs of half cycles showed no significant difference in birth rates, but a clear disparity emerged from others. The most marked difference was when the month was divided on the day after the Moon was at its first quarter. The birth rate for the following half cycle was 1.01 per cent higher--statistically significant disparity--than in the corresponding second half cycle (that beginning the day after the Moon's last quarter). Thus more babies were born during the half month centred around the full Moon than during the half cycle centred on the new Moon.

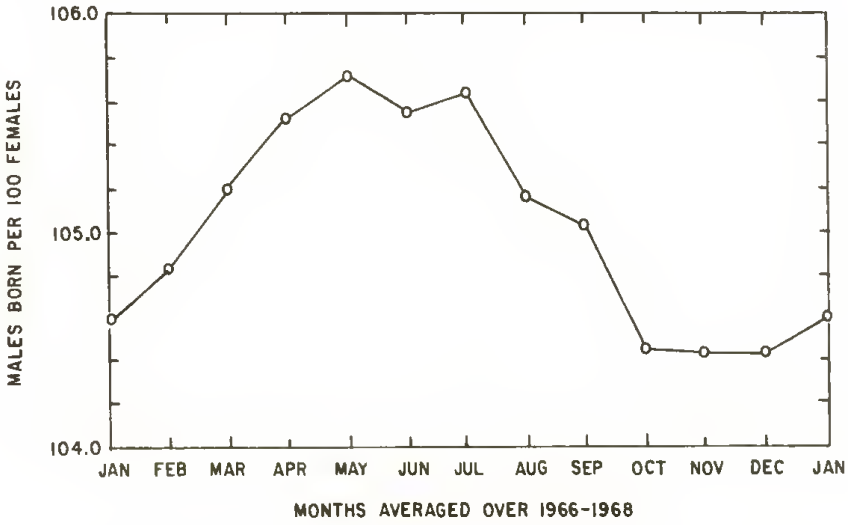
. . . . .

EARLY SUMMER FAVOURS THE BIRTH OF A BOY

Anonymous; *New Scientist*, 45:448, 1970.

The latest available statistics on birth rates in the United States reveal that a curious annual variation in the sex ratio appears to be becoming more marked. The variation was first noted in 1953, when Dr. H.M. Slatis--using data going back to 1948--showed that the ratio of males to females among the newborn of May and June was greater than the male to female ratio of November-December. For the country as a whole, Slatis noted a shift of about half of one per cent between the two periods; the phenomenon was most marked in large cities, less so in medium sized towns, and slmost absent in rural areas. The new data, from Vital Statistics of the United States shows that Slatis' shift has increased to about one and a half per cent.

The graph shows the curve obtained for averaging the monthly totals of males and females over the three years 1966 to 1968. The total number of births over this period was 10 630 000.



SEASONAL INCIDENCE OF THE BIRTHS OF EMINENT PEOPLE

Allen, F. J.; *Nature,* 110:40, 1922.

In order to find, if possible, the causes which underlie the production of increased numbers of eminent intellects at certain periods (as, for example, the year 1809 and a year or two before and after it), I collected statistics of the dates of birth of more than two hundred eminent persons. The list consists chiefly of creative intellects,--poets, literati, musicians, painters, architects, men of science, explorers, and inventors, with a few statesmen and military men. Analysis of the dates shows that the greater number of these persons were born in the colder months of the year; but the distribution of the numbers is somewhat erratic. February is distinctly the richest month, having produced a galaxy of eminent persons; December comes next; August and June are the richest among the warm months.

Sixty pre-eminent names, chosen for no reason but their pre-eminence, were found to be distributed as follows:--In warmer months: April, 4; May, 6; June, 7; July, 2; August, 5; September, 3; total, 27. In colder months: October, 4; November, 1; December, 9; January, 5; February, 9; March, 5; total, 33.

The difference is more evident when the months are taken in groups of three, as follows: December to February, 23; March to May, 15; June to August, 14; September to November, 8.

In order to find whether this distribution corresponds with the ordinary distribution of births through the twelve months, I compared the numbers with the average of twelve years taken at a venture from the Registrar General's Quarterly Returns, namely, the period 1844-55. The figures are too numerous for quotation, but it may suffice to say that I could find no correspondence between the ordinary distribution of births and the distribution of births of eminent persons. In the Registrar General's Returns the order of average frequency for the quarter-years was as follows: April to June, July to September, January to March, October to December.

Climate can scarcely explain the distribution, (See letter from Dr. Robert W. Lawson, Nature. June 3, p. 716.) Cold weather is not unhealthy for children, and in fact the diseases of the hot months are among the most fatal for them. I suggest that the reproductive organs, especially the germ cells, are more vigorous at certain seasons, producing offspring of higher quality. The many eminent persons born in the winter months, December to February, were conceived in the spring, the time of increased vigour of most living things; whereas the few born in the autumn months, September to November, were conceived in the winter.

INTELLIGENCE AND SEASON OF CONCEPTION

Anonymous; *Nature,* 153:401, 1944.

It is beyond reasonable doubt, according to Dr. J. Fraser Roberts (Brit. Med. J., March 4, 1944. (p.320), that children conceived in winter are, on the average, somewhat more intelligent than those conceived in the summer; but this fact has been almost universally misinterpreted. Practically every hypothesis brought forward to explain it has depended on the assumption that it is the season of conception that influences the intelligence of the child. Some have even urged the benefits of planned winter conception (for example, Mills, C.A., Human Biology, 13, 378; 1941). Dr. Fraser Roberts has tackled the problem by comparing winter and summer children born to the same parents, arguing that, if the season of conception influences the intelligence of the child, the usual difference will be found in such groups; if, however, the real explanation of the observed difference is the fact that the intelligence of the parents influences the season of conception, the two groups will show no difference. A second line of inquiry was the determination of the number of sibs of comparable groups of winter and summer children. Admitting that the evidence that he has gained is not based on very large numbers, Dr. Fraser Roberts concludes that the rather greater intelligence of the child conceived in winter is not due to seasonal influence on the mother or on the developing child; but to the tendency for the more intelligent parents to conceive children rather more often in the winter, while the less intelligent people tend to conceive children rather more often in the summer.

BIRTHDAY AFFECTS IQ PSYCHOLOGIST CLAIMS

Anonymous; *Science News Letter,* 85:25, 1964.

A person's birth date has a lot to do with his brain power, a British psychologist believes.

Chances are that those born in the summer or fall will be brighter than those born in the winter or spring, his studies indicate.

This seasonal effect on intelligence was found for 188 adults with far-below average intelligence. The brightest of these subnormals were summer and fall babies. There was as much as a five-point difference between summer and winter-born persons.

Temperature changes in the weather are related to intelligence, Dr. J.E. Orme of Middlewood Hospital, Sheffield, England, believes. The smarter adults, he found, had more months of embryonic development in which the temperatures were above average for those months, while the duller adults developed during relatively colder times.

"It is not known how climatic temperature precisely affects the

embryo," Dr. Orme reported in Nature, 200:1230, 1963.

He believes, however, that "75% of individual variations in ability are determined by inheritance" and that the rest is largely due to factors such as temperature operating during pregnancy.

Not all psychologists would go along with these views. A sizable group believes that the cultural climate, or environment, is as important as inheritance and more important than the physical climate, or weather, in spurring or stunting intellectual growth.

MOON PEOPLE

Shirk, Gertrude; *Cycles*, 29:71, 1978.

There is a 28-year old blind student who is more in tune with the daily cycle of the moon than with the 24-hour schedule that society decrees. This young man has bodily circadian rhythms of 24.9 hours, "indistinguishable from the period of the lunar day."

He had noticed for several years that for two to three weeks at a time he would have bouts of insomnia coupled with excessive sleepiness during the daytime. The problem created difficulties with his work and leisure schedule, and he finally underwent 26 days of hospital study. It was found that his circadian rhythms were desynchronized from the 24-hour schedule. In addition, according to the researchers, " . . . there was a remarkable coincidence between his sleep onset and a local low tide."

The research was conducted L.E.M. Miles, D.M. Raynal and M.A. Wilson of Stanford University's Department of Medicine and Sleep Research and the Palo Alto Veterans Hospital. They attempted to force Mr. X's body functions into a 24-hour rhythm, but the result was that, "his nocturnal sleep became progressively invaded by sleepiness and deteriorating performance" as reported in Science of October 28, 1977.

The researchers point to other experiments that suggest that this disorder (if it can be called that) might be more common than suspected.

WAIT TILL THE MOON ISN'T FULL

Anonymous; *Cycles*, 24:51, 1973.

Back in times of English folkore, it was supposed that the flow of man's blood was governed by the phases of the moon. While modern medicine has found little worth in old wives' tales, a doctor in Florida has acquired some scientific data which reopens the possibility of a link between blood flow and the moon.

Apparently Dr. Edson J. Andrews, an eye, ear, nose, and throat specialist, became aware that sometimes all his patients made good recoveries, whereas patients at other times hemorrhaged and required emergency treatment.

Since both cases were grouped at particular days on the calendar, his nurse suggested that the moon might have something to do with the situation. When the doctor objected, his nurse insisted, pointing out the clumps of emergency treatments around the time of full moon. In order to determine the results more scientifically, Dr. Andrews kept records of all tonsillectiomy cases for three years.

In over 1000 cases, 82% of the bleeding crises happened between the moon's first and third quarters. Actually, the disproportion was greater because fewer patients were treated during the full moon.

A colleague of Dr. Andrews kept the same type of records and got the same results.

These records were kept several years ago. Perhaps not enough attention has been paid to the correlation between the moon and blood flow. It might be a good idea to operate on dark nights only and save the full moonlit nights for romancing and tracking down nightcrawlers!

• Feral Children

WOLVES SUCKLING CHILDREN IN THE ROMULUS AND REMUS STYLE

Murchison, R. I.; *Zoologist*, 9:3299-3300, 1851.

Colonel Sleeman told me one of the strangest stories I ever heard, relative to some children, natives of this country (Oude), carried away and brought up by wolves. He is acquainted with five instances of this, in two of which he has both seen the children and knows the circumstances connected with their recapture from the animals. It seems that wolves are very numerous about Cawnpore and Lucknow, and that children are constantly being carried off by them. Most of these have of course served as dinners for their captors, but some have been brought up and educated after their own fashion by them. Some time ago, two of the King of Oude's sowars (mounted gens d'armes), riding along the banks of the Goomptje, saw three animals come down to drink. The sowars rushed in upon them and captured the three, and to their great surprise found that one was a small naked boy. He was on all fours like his companions, had callosities on his knees

and elbows, evidently caused by the attitude used in moving about, and bit and scratched violently in resisting the capture. The boy was brought up in Lucknow, where he lived some time, and may, for aught I know, be living still. He was quite unable to articulate words, but had a dog-like intellect, quick at understanding signs, and so on. Another enfant trouve under the same circumstances lived with two English people for some time. He learned at last to pronounce the name of a lady who was kind to him, and for whom he showed some affection, but his intellect was always clouded, and more like the instinct of a dog than the mind of a human being. There was another more wonderful but hardly so well authenticated story of a boy who never could get rid of a strong wolfish smell, and who was seen not long after his capture to be visited by three wolves, which came evidently with hostile intentions, but which, after closely examining him, he seeming not the least alarmed, played with him, and some nights afterwards brought their relations, making the number of visitors amount to five, the number of cubs the litter he had been taken from was composed of. I think Col. Sleeman believed this story to be perfectly true, although he could not vouch for it. There is no account of any grown-up person having been found among the wolves. Probably after a certain time they may have got into a set of less scrupulous wolves, not acquainted with the family; the result is obvious.

WOLF-CHILDREN AND WEREWOLVES

Burton, R. G.; *Chamber's Journal*, 101:306–310, 1924.

When I rode into the middle of the village, something which I took to be a dog of large size ran swiftly across the central square, where a great banyan tree, surrounded by a raised platform, formed a sheltered meeting-place for the village conclave, a rendezvous now deserted in the heat of the noonday sun. So swiftly did the creature run that I did not at first distinguish anything human in its shape. It stopped at a corner, and peered around at me with an unmistakably human face, and then I perceived that it was a human being, apparently deformed and of stunted growth.

At this moment the headman of the village with several of his friends appeared. I dismounted from my horse, which was led away to water at the trough, hollowed out of the trunk of a tree, which lay near the village well, and gradually the villagers assembled as they do when a traveller thus halts in their domain. After some conversation I asked what was the matter with the monster which had attracted my attention, and which still maintained his position at the corner. 'That,' said the headman, 'is our wolf-boy. He lived long with wolves, but has now come to dwell among men.' He called to the creature, which came up on all-

fours and squatted at a distance of a few yards. I questioned it, and it uttered a moaning whine, but showed no signs of intelligence, and could not speak.

I asked the headman to tell me its history, and he said that it had been captured in company with wolves the year before, when it had been hurt and was unable to run. It was not known how long it had been with the wolves, but was now thought to be about six years old, and had perhaps been lost during the famine of nearly six seasons past. No child had been missed from the village, but there were many people wandering and perishing in different parts of the country during the time of scarcity. The creature could scarcely move when they found it, but they tended its wounds and were kind to it, and it had taken up its abode with them.

Unfortunately at that time, many years ago, I had not learnt to make a scientific inquiry into such matters. I had to hasten on to my camp, and my route on my return was not through the village in question, which I had no occasion to visit again. So whether the child had really been brought up by wolves, or whether it was merely an idiot abandoned by travellers, is not possible to determine. But the formation of the limbs showed that it had been accustomed to walk on all-fours, and it could not stand upright.

In view of the predilection of wolves for carrying off children, it is not surprising that stories of wolf-children should have grown up and multiplied; and it is intelligible that where the she-wolf has lost her young she may in rare cases have adopted and suckled the man-child which she had taken away to devour. The wolf has from classical times been the suject of superstition and dread, probably owing to the familiarity which pastoral man acquired with the habits of the animal.

. . . . .

In India these stories are lifted out of the domain of fiction and legend in many cases of wolf-children, some of them apparently well-authenticated. There have been from time to time several of these waifs of the jungle at the Sikandra Orphanage at Agra. It is remarkable that these have always been male children. None of them has ever shown the intelligence of Mowgli of the Jungle Book, and none has even possessed the power of speech.

One was captured on the bank of the Gumti River in Oudh by two troopers, who found it in company with two wolf-cubs, with which it ran on all-fours. It had callosities on the knees and elbows, due to this mode of progression. This boy had a dog-like intellect, and was quick at understanding signs. He was said to have been visited not long after his capture by three wolves, which came evidently with hostile intentions, but which, after closely examining him, he not being in the least alarmed, played with him, and some nights afterwards brought five of their relatives.

Another Sikandra wolf-boy, smoked out of a den, was a perfect wild animal. He drank like a dog and preferred raw bones and meat to other food. He would never wear clothes, but tore them

up into fine shreds when they were given him. These children of the jungle generally died young, but one was said to have lived to advanced age. In another case a lad of fifteen, who was in the orphanage in 1874, was remarkable for the shortness of the arms, which were under twenty inches long, their growth presumably having been arrested through his going on all-fours. This boy was brought in with the body of a she-wolf and two cubs, in whose company he had been found when they were killed.

The capture of one of these children is described in an article contributed to the Academy many years ago by Professor Max Muller, who related the following story: 'A trooper, sent by the Governor of Chandour to demand payment of some revenue, was passing along the bank of the river about noon, when he saw a large female wolf leave her den, followed by three whelps and a little boy. The boy went on all-fours, and when the trooper tried to catch him he ran as fast as the whelps and kept up with the old one. They all entered the den, but were dug out by men with pickaxes, and the boy was secured. He struggled hard to rush into every hole or den they came near. He became alarmed when he saw a grown-up person, but tried to fly at children and bite them. He rejected cooked meat with disgust, but delighted in raw flesh and bones, putting them under his paws like a dog. They tried to make him speak, but could get nothing from him but an angry snarl or growl.' This boy afterwards lived in the care of Captain Nicholetts at Sultanpur, and died three years later, when he was supposed to be about twelve years old.

In another instance a man and his wife went out to cut wheat at Chupra, and, while they were at work, their child, who had a scarred knee, was carried off by a wolf. Six years later the boy was seen with a wolf and three cubs, and was caught after a fierce resistance, and recognised by his scarred knee as the boy who had been carried off. He would eat nothing but raw flesh, and a year later escaped to the jungle.

A more recent case, where a panther was the foster-parent, was reported not long since from the North Cachar Hills. In this instance a panther, whose cubs had just been killed, carried off a baby boy and kept him alive for three years, at the end of which the panther was killed and the child was rescued and recognised by the parents. He was accustomed to run on all-fours, and had callosities on the knees and elbows, but in course of time he resumed the upright position. When caught he bit and fought with every one who came within reach, and he would tear to pieces and devour any fowls he could lay hands on.

The hill people of the Himalayas believe that bears steal and bring up children, but there is no record of such an instance. A few years ago, however, a child was brought into Naini Tal who was said to have been nurtured by a monkey. This was a female about eight years of age. It had vaccination marks, but looked more simian than human, having a thick growth of hair down each side of the face and over the spine. The absence of callosities on the extremities showed that it had always walked upright. It was thought that the child may have been abandoned during a season of famine as is frequently the case, and picked up by a monkey.

Its further history is unknown. (pp. 306-308)

'WOLF CHILDREN' OF INDIA

Squires, Paul C.; *American Journal of Psychology,* 38:313-315, 1927.

In the autumn of 1926 the writer of this note came across a brief report in one of our daily newspapers to the effect that two children had been discovered in Bengal, India, living in a den with a family of wolves. This report stated that the children had been removed from the wolf-den by the Rev. J. A. L. Singh, head of the orphanage at Midnapur, Bengal, India, and that the children had been taken into this orphanage. Interested in the account, the writer communicated with the Rev. Singh and received from him a letter dated December 6, 1926, containing a short sketch from the diary that he has kept on the subject of these strange children, together with a short history of his orphanage and various personal references that might be used in case further examination should be desired as to his personal and official status.

The Rev. Singh commenced his work among such natives as Santals, Lodhas, and Kurmis from the time he was head master of the local mission school at Midnapur, this school being affiliated with Calcutta University. Feeling a call for the ministry, Mr. Singh left the education department and in 1912 was ordained to the ministry. From that time onward he came more and more into contact with the natives. In every village that Singh visited he found orphans roaming about in want of food and shelter. Gradually he gathered together a number of orphans and in this manner there evolved Singh's orphanage at Midnapur. This orphanage was started on Singh's own initiative and responsibility and depended upon his personal income. Among those who have taken active interest in the Midnapur orphanage is Mr. W. N. Delivingne, District Judge of Midnapur.

The Rev. Singh informs the writer that he hopes to publish a detailed account of the 'wolf-children.' This projected publication is to include photographs of the children, taken at various intervals after the removal of the children from the den of the wolves to the Midnapur orphanage.

The following paragraphs are quotations from the Rev. Singh's letter referred to above:

"The wolf-children were first seen by natives on various occassions. I heard of these children for the first time on August 26, 1920. The children were seen through a field glass by several people (Europeans and Anglo-Indians), from a distance of about one hundred yards, on the 9th and 10th of October, 1920.

"Three wolves were observed to come out of a tunnel-like passage from their den, closely followed by two cubs; then there appeared a human head covered with bushy hair, with a ghastly look about the face. This head tarried for a little while looking to this

side and that side, then a human form came out of the den followed by another human being at its heels. The two children crawled on all fours.

"Excavation of the wolf-den took place on October 17, 1920. I took charge of the wolf-children on November 1, 1920. I brought them home on November 4, 1920. I guessed their ages to be, the elder about eight years and the younger about two years. The younger one died September 21, 1921. She was baptized some time before she expired. We named her Amala. Kamala, the surviving one, was baptized January 1, 1925.

"At the present time Kamala can utter about forty words. She is able to form a few sentences, each sentence containing two, or at the most, three words. She never talks unless spoken to; and when spoken to she may or may not reply. She is obedient to Mrs. Singh and myself only. Kamala is possessed of very acute hearing and evidences an exceedingly acute animal-like sense of smell. She can smell meat at a great distance. She was never known to kill any domestic animal but is fond of pouncing upon any killed animal if found anywhere by her. Never weeps or smiles, but has a 'smiling appearance.' Shed a single tear when Amala died and would not leave the place where she lay dead. She is learning very slowly to imitate. Does not now play at all and does not mingle with other children. Once, both Amala and Kamala somewhat liked the company of an infant by the name of Benjamin while he was crawling and learning to talk. But one day they gave him such a biting and scratching that the infant was frightened and would never approach the wolf-children again. Amala and Kamala liked the company of Mrs. Singh; and Kamala, the surviving one of the pair, is much attached to her. The eyes of the children possessed a peculiar glare, such as that observed in the eyes of dogs or cats in the dark. Up to the present time Kamala sees better at night than during the daytime and seldom sleeps after midnight. The children used to cry or howl in a peculiar voice, neither animal nor human. Kamala still makes these noises at times. She is averse to all cleanliness, and serves the calls of nature anywhere, wherever she may happen to be at the time. Used to tear her clothes off. Hence, a loincloth was stitched to her in such a fashion that she could not open or tear it. Kamala used to eat and drink like a dog, lowering her mouth down to the plate, and never used her hands for the purpose of eating or drinking. She would gnaw a big bone on the ground and would rub it at times in order to separate the meat from the bone. At the present time she uses her hands for eating and walks straight on both legs, but cannot run at all."

"GAZELLE BOY" STORY BELIEVED ONLY MYTH

Anonymous; *Science News Letter,* 50:120, 1946.

The story told in a London newspaper reported a "human

gazelle," raised by the animals, eating grass and bounding around with the speed of his foster-parents, something like 50 miles per hour. Hunters have allegedly captured the boy and put him in an African asylum.

But Dr. Dale Stewart, anthropologist at the National Museum, Washington, D. C., warns that these stories of human-animals have "always been on hearsay evidence."

Man, for thousands of years, has been telling stories of humans raised by animals, and they are still being told in today's comic strips. One of the earliest and most famous was of Romulus and Remus, mythical first settlers of Rome, who were said to have been raised by a wolf.

Only five years ago, Kamala, "the wolf girl," was reported from India by a missionary who rescued her from the wolves with whom she had allegedly lived for her first eight years. She died in an Indian orphanage after nine years with humans.

Russia has reported "human bears," and modern literature includes Kipling's Mowgli, but scientists are still skeptical.

"No one has ever brought in proof," declares Dr. Stewart.

Incidentally, if the "gazelle boy" does turn up in a track meet and actually can run at a speed of 50 miles per hour, he'll do the 100-yard dash in about four seconds and the mile run in one minute and twelve seconds!

• Possible Genetic Transmission of Memory

INHERITED MEMORIES

Anonymous; *Popular Science Monthly,* 29:858, 1886.

Dr. H.D. Valin has communicated to "Mind in Nature" (Chicago) some instances in which specific memories appear to have been transmitted by inheritance. He believes that they are the first cases that have been published, for even Ribot confesses to have had but little success in establishing instances of the kind. The most striking case is that of a little girl fifteen months old, the child of a French Canadian father, whose principal traits of character and appearance she seems to have received, and of an American mother of German descent. She has had only the English and German languages spoken to her; yet the first word she ever spoke--when five months old was mouman, the French Canadian form of maman, mother. Her first words of assent and dissent were oui and non--French--when eight months of age, "and she does not yet know yes or ya, though she seems to have forgotten oui. When a year old she was presented with a

poodle-dog named Venus, which she called Nanan--candy, "one of the very first words that a French child talks." At about the same age she used freely the words bon, good, and pus, French Canadian for plus, no more. These six French words are the very ones that her father is likely to have exclusively used when a babe. The u of the last word was sounded as in French, as also were the nasal sounds of non and Nanan, things which her mother could not have done. Inheritance of memory has been observed in the case of birds, which soon learn to avoid the telegraph-wires, while their young seem equally ready in keeping away from them. Chauncey Wright is quoted as saying of those dreams of strange places and events that often recur to one in his sleep, with the intimation of being familiar though never seen in a wakeful state, that they are inherited memories. Dr. Valin also relates of his own personal experience: "My mother was brought up and educated in a most romantic country village, which she revisited a few months before I was born. The first time I visited it I remembered vividly having been there before. In fact, I could tell at that time what next would follow in the scenery, and I argued with my relatives who were denying my former knowledge of that place; my mother having died when I was about nine months old, and I had not had any description of it from any one, or conversed with any one in regard to the village scenery." A little girl in Burlington, Vermont, with whose family Dr. Valin at one time resided, "had inherited so good a memory of an uncle, whose funeral had been attended by her mother not long before this little girl's birth, that she could give a full description of him, and knew his picture at once the first time that she ever saw it." Some of these cases may have been maternal impressions, but the first one was undoubtedly one of inherited memory.

• Half-Brain Intelligence

INTELLIGENCE RAISED WHEN HALF OF BRAIN REMOVED

Anonymous; *Science News Letter,* 60:142, 1951.

Children who had half of each of their brains removed at the National Hospial in London in an attempt to cure them of epilepsy and other mental disorders astounded the doctors with their post-operation intelligence level.

It had been thought that removal of half a brain caused a lowering of intelligence, instead of which the children at the National Hospital were, if anything, more intelligent after the operation than they were before.

• Curious Attitudes after Sudden Death

ATTITUDES AFTER DEATH

Brown-Sequard, C. E.; *Knowledge,* 6:115–117, 1884.

Among the phenomena sometimes noticed at the hour of death there is one that offers a peculiar interest, and which, up to recent times, has remained a mystery. This phenomenon appears especially, but not exclusively, after a sudden death due either to wounds received upon the field of battle or elsewhere, or to other causes, but almost always when there has been an intense excitement, and often also when great bodily fatigue has preceded the last moment of life. The principal feature of this curious fact is the persistence after death of the expression of the face or of certain attitudes of the limbs or body, or of both. Such persistence exhibits itself clearly in certain cases; for example, when, despite the sudden cessation of life, a limb that is raised does not drop, or when the body of a man standing, or seated on horseback, does not fall over.

In order to clearly understand the terms of the problem to be solved in reference to this phenomenon, it is absolutely necessary to know (1) that our attitudes and facial expression depend upon a contraction of our muscles due to an influence of the nervous centres, and (2) that such influence necessarily ceasing at the instant of death, a relaxation must also necessarily occur in all the muscles that were contracted, unless some other agency at once replaces that which has disappeared and causes the same physical state to persist that formerly existed therein.

The question, then, is this: What is the agency that, as soon as the faculty of volition vanishes, takes the place of the latter, or at least produces in the muscles an organic state that prevents all relaxation?

The object of this article is to answer this question, and to show that the cause or agency to be discovered is not the sudden appearance of that state of muscular stiffness known by the name of rigor mortis or cadaveric rigidity, but that such agency is found in a peculiar action of the nervous centres that manifests itself a little before or at the instant of death. One of the most striking examples of the strange fact that I am about to study was observed by Dr. Rossbach, of Wurzburg, upon the battlefield of Beaumont, near Sedan, in 1870. He found the corpse of a soldier half-sitting, half reclining, upon the ground, and delicately holding a tin cup between his thumb and forefinger, and directing it toward a mouth that was wanting. The poor man had, while in this position, been killed by a cannonball that took off his head and all of his face except the lower jaw. The body and arms at the instant of death had suddenly taken on a rigidity that caused them to afterward remain in the position that they were in when the head was removed. Twenty-four hours had elapsed since the

battle, when Dr. Rossbach found the body in this state.

In the first work of any importance in which this subject has been treated of, Dr. Chenu relates that a French military surgeon, Dr. Perrier, was greatly surprised upon going over the battle-field of Alma, the day succeeding the terrible conflict, to see that many corpses of Russian soldiers had attitudes and expressions of countenance like those of living persons. Some of these corpses had the different expressions that characterize anguish, suffering, or despair. Others, on the contrary, had the appearance of greater calmness and resignation.

One case particularly attracted the doctor's attention, where the body lay stretched out upon the ground, the knees bent, the hands clasped and lifted in the air, and the head thrown back, as if death had come upon the individual while he was reciting a prayer. In addition, many other persons who have visited battle-fields immediately after a conflict tell us that they observe numbers of corpses that were still holding their guns or sabres. Some seemed to be biting their cartridges, while others, still upon horseback, continued to preserve the attitude they had at the moment of death. These phenomena have been studied with special attention by Dr. Armand at Magenta, by Baron Larrey at Solferino, and by Dr. Baudin at Inkermann.

I owe to the kindness of Dr. S. Weir Mitchell a knowledge of an excellent memoir by Dr. John Brinton, of Philadelphia, upon the "Rigidity which Accompanies Sudden or Violent Death"---a work in which the question under consideration is studied with the greatest care. Speaking of the field of battle of Antietam, Dr. Binton says that he counted forty corpses over a space of from forty to fifty yards square, and he gives us the following picture of what he observed in this place.

"Several of these corpses were lying in extraordinary attitudes, some with their arms lifted and rigid, and others with their legs drawn up toward the trunk, and stiff. With others, in quite large number, the trunk was curved forward and also rigid. In a word, these attitudes were not those of the state of relaxation

produced by death, but rather those of an apparently active character, doubtless due to a final muscular act at the very moment of the extinction of life---a spasmodic act that had left the muscles stiff and inflexible. Death, in the majority of these cases, had been caused by wounds made in the breast; and, less frequently, by balls that had traversed the head or abdomen. In the latter cases there had been considerable hemorrhage, as was proved by the pools of blood of dark colour near the sides of the bodies. This inspection was made thirty-six hours after death, or still later."

The following three cases related by Dr. Brinton (which were furnished to him by friends, are very remarkable:---

A detachment of United States soldiers, foraging around Goldsborough, N.C., came suddenly upon a small band of Southern troopers who had dismounted. These latter immediately jumped into their saddles, and all scampered away except one, after being exposed to one round of fire. The soldier who did not escape was sitting upright, one foot in his stirrup. In his left hand he held the bridle and the horse's mane, while his right hand grasped the barrel of his rifle, near the muzzle, the stock of the gun resting on the ground. The horseman's head was turned toward his right shoulder, apparently watching the approach of the assailing party. Some of the soldiers of the latter were preparing to fire again, when their officer ordered them to desist, and to go and make the defiant man a prisoner. The latter, upon being ordered to surrender, made no answer. When he was approached and examined, it would found that he was dead and rigid in the singular attitude that we have just described. It took a considerable effort to force his left hand to release the horse's mane and to remove the rifle from his right hand. When the body was laid upon the ground, the limbs preserved the same position and the same inflexibility. This man had been struck by two balls fired from Springfield rifles. One of these had entered to the right of the vertebral column, and had made its exit from the body near the region of the heart. It had left its track upon the side of the saddle, and had then dropped to the ground. The other ball had entered through the right temple, and its point of exit could not be found. The horse had remained quiet, as he was fastened by a halter.

The following is another incident: At the battle of Williamsburg, Dr. T. B. Reed examined the body of a United States Zouave who had received a ball in the forehead just as he was climbing over a low fence. He, likewise, had preserved the last attitude of his life. One of his legs was half over the fence, while his body still remained behind. One hand, which was partially closed, was raised level with his forehead, with the palm forward, as if to preserve himself against some imminent danger.

Dr. Henry Stille relates that, while seated upon a freight car on the Nashville and Chattanooga Ralroad, he saw a brakeman instantly killed by a ball which struck him between the eyes, a mortal wound that was given by a guerilla who lay in ambush in a forest through which the train was passing. The man thus killed was tightening the brake when he received the ball. After his

death his body remained fixed, the arms extended and stiff on the handwheel of the brake. The pipe which he was smoking remained fastened between his teeth. The rigidity was so perfect, and his hands were so tightly closed, that it was scarcely possible to free the corpse and make it let go its hold.

A maintenance of the last attitude may occur under circumstances other than a sudden death produced by lesions of the brain, heart, or lungs, although an injury to an organ of great importance to life is the most frequent cause of the phenomena. Dr. Brinton has observed it after wounds made in the abdomen, and Dr. Armand, in a single case, through a wound of the thigh.

Yet this phenomenon does not manifest itself exclusively in cases where death results from wounds. It was observed in a horrible accident that happened at London in 1867, when forty-one persons, skating upon Regent's Park Reservoir, perished through the sudden giving way of the ice. The following extract from The Times concerning this event is full of interest:---

"The attitude of the majority of the persons who were taken from the water has given rise to numerous discussions in the medical journals. In almost all cases the arms were raised, and sometimes the elbows were pressed against the sides. In other cases the elbows formed a right angle, and projected as in the act of skating. It may be concluded that these unfortunates were resting upon the ice with their arms, not daring to use their hands, and that when, on becoming exhausted, they died, it was not through asphyxia, but rather through the action of cold and fright; and this would explain why they preserved the position in which they were found."

Dr. Taylor had already mentioned the case of an individual who had for a long time held his arms extended to avoid being drowned, and in whom, after death, these limbs were found stiffened out in the same position.

It seems that carbonic acid is capable of producing that special rigidity of the muscles that maintains the trunk and limbs in the attitude that the last act of the will has caused them to assume.

In 1832 Dr. Von Graefe saw, in the grotto of Pyrmont, the corpse of a young man who had voluntarily put an end to his days by exposing himself to the carbonic acid gas that fills this cavern. The body was found half-seated upon the groud. One of his hands supported the head, as if the young man had desired to avoid touching the wall, against which the upper part of his body rested. The trunk was bent toward the right. The attitude of the body had the appearance of a person asleep and reposing peacefully.

How shall we explain this curious series of facts? We know that sooner or later there supervenes a stiffness (called cadaveric or post mortem rigidity) in all the limbs and all other parts of the body where there are muscles. Is not the stiffness that occurs on the battlefield, and some times elsewhere, immediately after death, mere a cadaveric rigidity that has come on suddenly? Those who know the law that I have established concerning the rapidity or retardation of cadaveric rigidity after death (see my

Croonian Lecture before the Royal Society of London, 1861) will find it evident that in the majority of the cases of preservation of attitude after death that I have just mentioned, the circumstances were very favourable for the prompt appearance of post mortem rigidity. Yet, even the cases placed under the most favourable circumstances, death could not have come on quickly enough to permit of the preservation of an ante mortem attitude. This is a sufficient reason to assure us that the fact that we have to explain is not due to the sudden intervention of cadaveric rigidity. But how, then, shall we explain this fact?

Some experiments that I cannot here give the details of have shown me that it is a fixed contraction--a tonic, persistent, muscular action which then occurs, similar to that which it replaces, and which existed during life. At the very moment that death comes on, this fixed or tonic contraction occurs. It is an act of life, but the last one. I have sometimes seen this contraction exhibit itself and then disappear, and it was not till later that the true cadaveric rigidity supervened.

Death, in man as in animals, takes place in two ways that differ radically from each other. On the one hand, it may supervene suddenly, either through the influence of excitement or that of a wound or blow, or, again, through the following causes. The impression produced by submersion in cold water, or in almost icy water, and the impression produced sometimes, in persons who are eminently nervous, by the least lesion affecting certain parts of the body. In this kind of death there may not be even the least vital manifestation after the last sigh, except a feeble action of the heart that soon disappears. All the cerebral faculties give way suddenly--consciousness, intelligence, the will, the perceptive faculties, sensorial and sensitive impressions, and respiratory motions all disappear at once. There is no agony, and none of that struggle that usually precedes death. The body suddenly loses its temperature, and cadaveric rigidity comes late, and lasts considerably.

In the other kind of death, which is the one that we usually observe, there is, on the contrary, a genuine struggle in the still living organism, especially when life is ending through the effect of certain wounds or of a great hemorrhage, or as a consequence of a complete and sudden deprivation of respiration. The heart in such a case beats violently, the efforts made to breathe are extremely energetic, consciousness and the cerebral faculties may keep up for a short space of time, and after this great agitation or general convulsions occur. The temperature of the body rises, and this increase may still continue for some little time after the last effort made to breathe. Cadaveric rigidity appears early, but never immediately.

My experiments and the details of the cases that I have related show that the persistence of the last attitude does not occur in all cases of death belonging to the first of the two types just described; but facts indicate that this singular phenomenon occurs only in cases of death that belong to this type.

In one of the conclusions of Dr. Brinton's excellent memoir, he says that in the cases of persistence of attitude that have

been observed upon the battle-field, and that he describes, death had probably been instantaneous, without being accompanied with convulsions or agony.

It results from the facts that I have studied in this paper, and from the experiments that I have done nothing more than allude to: (1) that the preservation after death of the attitudes of life, and of the facial expression, does not depend upon the sudden appearance of what is called cadaveric or post mortem rigidity, but upon the production of a vital act of rigidity or tonic contraction, like the fixed spasm that we often see in hysterical or paralytic persons; and (2) that a number of causes of death, acting without the ordinary agony, may produce that strange phenomenon which is characterised by a persistence after death of the attitude and facial expression that existed at the moment of the last sigh.

BIOCHEMICAL ANOMALIES

• Possible Correlations between Biochemistry and Astronomy

HOW THE HEAVENS INFLUENCE OUR LIVES

Cohen, Martin; *Today's Health,* 49:16-19, October 1971.

A pioneer in biomagnetics is Giorgi Piccardi, a brilliant Italian chemist who in 1935 became interested in the strange behavior of the agent used to clean boilers. This agent--physically treated, or "activated" water--sometimes failed to work, and no one could explain why. No one knew, for that matter, how or why activated water worked at all.

In a complicated series of experiments, Piccardi discovered that many different chemical reactions, including that of activated water, vary according to the magnetic influences of the eleven year sunspot cycle, and also in March and September of each year, regardless of weather conditions.

He suggested that the annual variations were caused by the "spiraloid" motion of the earth through space. (The earth revolving around the sun, while the sun itself speeds toward the con-

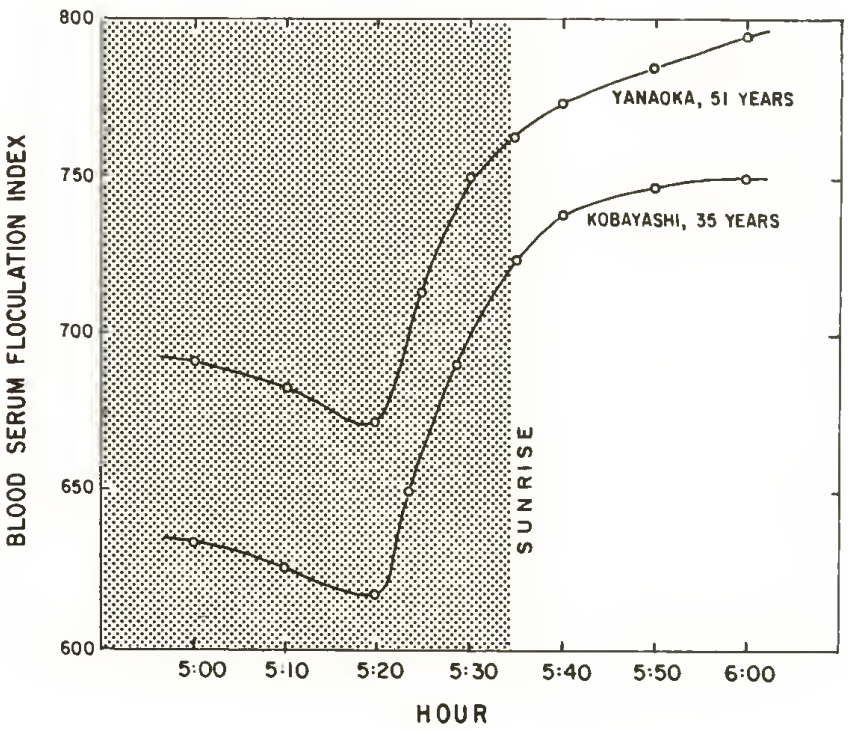
stellation Hercules.) Piccardi showed that seasonal fluctuations in radiation occurred according to the earth's position relative to the Milky Way's magnetic field, first described by Fermi in 1947.

Piccardi's experiments involved inorganic, non-living materials. But these materials form the basis of life, including human life. If cosmic forces influenced the activated water in Piccardi's boilers, the chances were good that similar influences were at work on all living things.

A related discovery in organic matter--in this case human blood--came to light in Japan late in the 1930's. Dr. Miki Takata had devised a test, known as the "Takata reaction," for determining the times of ovarian cycle by means of a "flocculation index" of blood serum. Conveniently, the index was constant in men, while it varied in predictable ways during the monthly ovarian cycle in women.

Then, early in 1938, the Takata reaction suddenly lost its usefulness, for the very good reason that the indexes of both men and women had risen abnormally. And the phenomenon was world-wide.

Dr. Takata was sure that such a universal effect had to have a cosmic cause. His experiments revealed that the sudden dramatic rise in the index was caused by the eleven year sunspot cycle, and by other occasional eruptions on the sun's surface.



The Takata Reaction, as affected by sunrise

He found also that the index rose sharply at sunrise. It fell sharply during eclipses of the sun. He even ran experiments in which he took measurements in an airplane, in order to see whether the index rose sharply, as expected, as the atmosphere thinned and the sun's powers intensified. It did. Takata's conclusion: "Man is a living sundial." (pp. 16-17)

EFFECT OF SOLAR ACTIVITY ON THE FREQUENCY OF FUNCTIONAL LEUKOPENIAS .....

Shul'ts, N. A.; *NASA Technical Translation TT F-592*, 1970.

Summary.

1. Analysis of the incidence of functional leukopenias during the last 45 years has revealed a strict pattern of fluctuations in the leukocyte count under nonpathological conditions. These fluctuations take place as independent but interconnected cyclic processes of an anharmonic character.

2. The strictly regular pattern of these fluctuations is due to coincidence of the phases of cyclic changes in the incidence of functional leukopenias with the fluctuations of solar radiation. This coincidence is clearly visible if changes in the leukocyte count in normal subjects are compared with fluctuations of solar activity. The active radiation of the sun influences the blood cells through the central nervous system, which is highly sensitive to changes in solar radiation.

3. The maximum of the number of functional leukopenias coincides in time with the maximum of solar activity, and its minimum coincides with the solar minimum. The number of functional leukopenias rises sharply after emissions of proton particles, ejected with a velocity exceeding 1000 km/sec from regions with magnetic characteristics of β, γ and γ spots, when the area of the group of spots exceeds 200 millionths of the sun's hemisphere and gives five or more flares with a classification higher than two points.

4. Everything which has been said about functional leukopenias applies equally to relative lymphocytoses. Since functional leukopenias are formed as the result of a decrease in the number of neutrophils, the disturbance of equilibrium between the various types of leukocytes in favor of lymphocytes leads to the appearance of relative lymphocytoses.

5. Coincidence between phases of fluctuation in the leukocyte count under nonpathological conditions with fluctuations in solar activity suggests that functional leukopenias and relative lymphocytoses can be used as specific leukocyte tests of solar activity. These tests are highly sensitive indices of fluctuations in solar activity, supplementing geophysical data on active radiation of the sun.

6. The percentage of functional leukopenias and relative lymphocytoses in northern latitudes is higher than in southern, and this is explained by closeness to the polar auroral zones, where the bulk of the solar corpuscles impinge.

7. These cyclic changes in the absolute and differential leukocyte counts under nonpathological conditions make the concept of stable absolute normal leukocyte counts meaningless. From this standpoint, the normal values of the leukocyte count are relative concepts and, besides age and sex differences, they must also allow for local geographical conditions and significant variations in the active radiation of the sun.

8. The most effective method of investigation of the effects of the sun's active radiation on the blood picture at the present stage of development of the study of solar-terrestrial connections in medicine are by mass observations over many years, including repeated observations, carried out at many different geographical points on the earth by the same method.

9. By introduction of the magnetograph into hospital practice, on the appearance of powerful solar flares, timely prophylactic measures can be taken on behalf of persons reacting acutely to sharp increases in solar activity.

WE MAY ALL BE LUNATICS!

Anonymous; *New Scientist,* 66:599, 1975.

Harry Rounds (from the Wichita State University, Kansas), while attempting to isolate specific factors from the blood of cockroaches, detected one factor with cardio-acceleratory activity, and monitored it over a number of weeks. Quite soon it was evident that the levels of this factor reflected a pronounced relationship to the phases of the moon! Rounds, not believing that cockroaches could be peculiar in this respect, went on to analyse blood from mice and human subjects, and has recently reported a similar relationship (Comparative Biochemistry Physiology, vol 506, p 193).

All three types of experimental animal were subjected to a slight stress, samples of blood were taken, and these assayed against a semi-isolated cockroach heart for cardioacceleratory activity. Such activity could be found in both stressed and unstressed animals but with intriguing differences. Cockroach blood taken from stressed animals did not exhibit much activity, except on certain occasions when it showed a great deal. Blood from stressed mice and human subjects displayed no cardioacceleratory activity except on certain occasions when there was none at all. The point in time when the activity rose in stressed animals coincides almost exactly with the time that the activity from unstressed animals fell to zero---some two days after both new and full moon. Later experiments with blocking agents revealed that these blood factors with cardioacceleratory activity are acetylcholine and sero-

tonin, both chemical transmitters, and that these vary in a circadian fashion. The material present in the blood of stressed animals on the critical days after the phases of the moon is yet another well known chemical transmitter, noradrenaline.

. . . . .

• The Influence of Weather on Human Biochemistry

CAN WEATHER ALTER INSULIN'S EFFECT?

Anonymous; *New Scientist,* 11:109, 1961.

Insulin lowers the blood-suger level. Biochemists carrying out routine tests of insulin preparations on animals sometimes observe fairly large deviations from the normal effects. S. Hansen and H. Brezowsky, of the meteorological-medical station at Bad Tolz, W. Germany, have made studies of the effect of the drug on 32 rabbits over a period of a year. They conclude that the variations are related to weather conditions.

Hungry animals had a stronger reaction to insulin when the weather became warm and humid, and a significantly weaker one when the weather was cold. For rabbits that were not hungry the results were opposite.

The research workers have given no explanation for the facts, but point out certain observations of changes in blood-sugar concentrations in children, apparently connected with meteorological alternations.

• Apparent Biochemical Changes during Evolution

IMMUNOLOGICAL TIME SCALE FOR HOMINID EVOLUTION

Sarich, Vincent M., and Wilson, Allan C.; *Science,* 158:1200–1202, 1967.

Abstract. Several workers have observed that there is an

extremely close immunological resemblance between the serum albumins of apes and man. Our studies with the quantitative microcomplement fixation method confirm this observation. To explain the closeness of the resemblance, previous workers suggested that there has been a slowing down of albumin evolution since the time of divergence of apes and man. Recent evidence, however, indicates that the albumin molecule has evolved at a steady rate. Hence, we suggest that apes and man have a more recent common ancestry then is usually supposed. Our calculations lead to the suggestion that, if man and Old World monkeys last shared a common ancestor 30 million years ago, then man and African apes shared a common ancestor 5 million years ago, that is, in the Pliocene era.

A CLOCK FOR EVOLUTION

Anonymous; *Science News*, 116:377, 1979.

The basic tenet of molecular anthropology--only grudgingly being accepted by traditional paleontologists--is that mutation at the molecular level takes place at a fairly constant rate. It can serve as an evolutionary "clock". Thus, no matter how much the fossils of extinct species may resemble each other, just how closely they are actually related may perhaps be best determined by examining the genetic differences among their present descendents.

To take a case in point, paleontologists emphasizing the structural differences between humans and apes have sometimes conconcluded that they diverged from a common ancestor 20 or 25 million years ago. Yet the protein structure of humans and apes is as similar as that between grizzly bears and polar bears -- indicating a common ancestor as recently as four to six million years ago.

In particular, this evidence puts pressure on paleontologists who still insist that Ramapithecus (10 to 15 million years ago) belonged to a distinctly hominid line. Molecular anthropology would insist that it be a common ancestor of both apes and humans. Says Sarich [Vincent Sarich, an anthropologist at Berkeley]: "Ramapithecus cannot be a hominid, no matter what it looks like."

UNRECOGNIZED SPECIES

• Yeti, Sasquatch, and Other Humanoids

ABOMINABLE SNOWMAN

Straus, William L., Jr.; *Science,* 123:1024-1025, 1956.

During recent years, stories have been coming out of India and Tibet about a giant mammal that lives above the snow line. According to some accounts, this creature is more than 7 feet in height, walks erect, has an apelike head and face, and is covered with heavy blond or reddish hair. The name "Abominable Snowman" has been given to the animal; the implication, of course, is that it is some sort of giant primate.

Huge footprints in the snow, at heights of from 10,000 and 21,000 feet above sea level, and attributed to the "snowman" have been reported by a variety of people, including members of various Himalayan expeditions. From the latter source have come actual photographs of the footprints, which could pass for those of a large primate. Indeed, it has even been suggested---perhaps with more levity than seriousness---that the "snowman" may be no other than the giant ape, Gigantopithecus blacki, persisting as a relic of the Pleistocene epoch in the seclusion of the Himalayas. This, of course, is at best no more than sheer speculation. As a matter of fact, Gigantopithecus itself is of decidedly uncertain status, being founded on three molar teeth, probably of Middle or Upper Pleistocene age, recovered from a Chinese drugstore in Hong Kong.

In connection with the matter of identification, it must be emphasized that there is no record of any "snowman" ever having been captured---either alive or dead---or even photographed. Identification rests solely upon the footprints and verbal evidence. It must be admitted that the footprints do bear some resemblance to those of a primate; on the other hand, they could as well be those of a bear. This alternative is not as strange as it may seem offhand, for the general superficial resemblance of ursine and primate feet has long been recognized by naturalists and comparative anatomists.

Wood Jones [Hallmarks of Mankind (1948)] points out that the animal footprint most commonly mistaken for that of man is that of the bear; in this connection, he notes that the footprint of the mysterious "orang pendek," once believed by both natives and Europeans to be that of some small jungle race of men, finally was

proved to be the footprint of the Malayan bear. Furthermore, many bears readily stand erect and even indulge in bipedal locomotion on occasion. Consequently, the identification of the "abominable snowman" as a bipedal primate has been vigorously rejected by many zoologists and anthropologists. To most of them, a large bear seems a more acceptable and more plausible explanation.

The question of the nature of the "abominable snowman" has been investigated by the Rev. Swami Pranavananda [Indian Geographical J. 30, 99 (July-Sept. 1955)], who concludes that the animal is no other than the red bear of the Himalayas. According to the author, the "snowman" is known to Tibetans as mi-te, meaning "man-bear." There are three varieties of bear in this region: black, brown, and red. The last of these is the mi-te, which is known to walk on its hindlegs like a man. The author reports several accounts of the mi-te gathered from Tibetan eyewitnesses.

A shepherd from eastern Tibet, whose sheep had been attacked by the animal at a height of 16,000 feet along the Kyang Chu, a tributary of the Brahamaputra, stated that the mi-te, after first running on all fours, rose on its hindlegs and departed following ineffectual gunfire from the shepherds; it was described as of about the height of a man and light red or reddish brown in color. A number of pilgrim nomads from northern Tibet identified the mi-te as the red bear and reported having encountered it at a height of 17,000 feet in the source region of the Kubi, a headstream of the Brahmaputra.

. . . . .

"ABOMINABLE SNOWMAN"

Swan, Lawrence W.; *Science*, 127:882-883, 1958.

Recent accounts [Science 123, 1024 (1956); 126, 858 (1957)] have given various interpretations of the Abominable Snowman or Yeti of the Himalayas. This note on the same subject is directed toward pointing out some aspects of the legend which have been overlooked. The interpretation that tracks in the snow ascribed to the Yeti may be made by man is valid in some instances, but it is clear that footprints cannot logically be attributed to even the most solitary hermit when they are made in remote glaciated terrain at great altitudes where local inhabitants simply would not travel.

The explanation that the Yeti tracks are made by red bears raises a number of difficulties. The footprints, such as those photographed by Eric Shipton in 1951, do not resemble bear tracks. Frequently, genuine red bear tracks have been attributed to the Yeti, but when photographs of these tracks have been examined, the bear origin has been clearly established. Perhaps the greatest difficulty with the bear theory, and the

point most often disregarded in statements concerning Yeti tracks, is the fact that the high-altitude red bear of the Himalayas (Ursus arctos isabellinus) is found only in the western Himalayas, whereas the origin of the Yeti legend and the source of all "genuine" Yeti tracks is in the eastern Himalayas. There is a fairly striking faunal difference between these two regions, and it is not legitimate, nor is it good zoogeography, to attempt to discredit the legend on evidence obtained from the western Himalayas or the plateau of Tibet. The Abominable Snowman, presumably, has no business in these parts.

. . . . .

There has been a curious silence in the scientific literature concerning the two unusual scalps found in separate monasteries in the Khumbu region of eastern Nepal. A mammologist of the Zoological Survey of India, Biswamoy Biswas, has examined the scalps, and it has been demonstrated that they are not artifacts. Photographs of the scalps are quite remarkable in that they indicate a somewhat conical occipital extension, as if the skull possessed prominent temporal and nuchal crests. There is in the scalps the distinct suggestion of large anthropoid ape. Coincidentally, the footprints photographed by Shipton closely resemble a cast of a foot of the mountain gorilla made by Carl Akeley in East Africa.

This cast, which was made on a dead specimen, exhibits a prominent hallux somewhat proximal to the remaining toes and perhaps more adducted than it would be in life. The line of toes arches in a slight semicircle, with the fifth digit close to and somewhat beneath the fourth digit, so that it could be inconspicuous in a footprint. The sole and heel of the foot are broad, with the lateral and medial borders approximately parallel. The general outline, the relationship of the hallux, the position and angle of the toes, and the inconspicuous fifth digit strongly suggest the Yeti footprint. Indeed, there are no other footprints which can approach the likeness of the Yeti track. Although it is true that foot impressions in the snow are open to wide subjective interpretation, it would seem to be a conservative assumption that the Yeti track, as it appears in good photographs, resembles Akeley's gorilla cast much more than it does any normal footprint of a bear.

In addition, there are reports of reversed "knuckle" prints such as might be made by an ape and, in fact, there are all sorts of tenuous indications from various Yeti tracks which can be interpreted to point toward the ape origin of the footprints. Among the apes, the mountain gorilla sometimes inhabits relatively high altitudes (its presence on snow fields has been recorded) in an alpine ecological zone not unlike that found in the Himalayas. The zoogeographical status of the eastern Himalayas as an area where relictual genera are frequent suggests that the existence of a relictual high-altitude ape with relatives in the tropics of Africa and Southern Asia is not an illogical supposition. Similar distributions are found among other mammal groups which at one time were widespread in Asia.

Whereas it is perhaps presumptuous to assume, at this time, that the Yeti is in reality some large anthropoid ape, it seems that this possibility has not been eliminated or sufficiently considered in the current arguments of the Yeti critics.

WAS THIS THE SNOWMAN?

Anonymous; *New Scientist*, 6:736, 1959.

A series of zoological prints from a Tibetan book of the eighteenth century has been reproduced in the current issue of Man. They show animals of various kinds including monkeys, small carnivores, birds, reptiles, fish and a number of invertebrates, all drawn with a minimum of stylization and none of them mythical in the manner of the European beastiaries. One of the animals, a large, hairy primate, is called a bitchun or Kumchin gorugosu, which is Mongolian for the man-animal. The bitchun, according to Dr. Emanuel Vlcek, of the Czechoslovak Academy of Sciences, seems to be none other than the famous Yeti of the Himalayas, the creature popularly known as the Abominable Snowman.

. . . . .

Another recently discovered Mongolian book, printed about a hundred years after the Anatomical Dictionary, depicts the bitchun again, although this time it is called the peeyi or zerleg khoon. The two creatures are almost identical. The two creatures are almost identical. Dr. Vlcek says that both illustrations "document in a remarkable way the existence of a creature known to the natives of Tibet for at least two centuries." He adds that a number of testimonies of encounters with the man-animal have been collected and published by the Mongolian scientist Dr. B. Rinchen in Sovremennaya Mongolia No. 5, but the document does not appear to be immediately available.

A RECONSTRUCTION OF THE FOOT OF THE 'ABOMINABLE SNOWMAN'

Tschernezky, W.; *Nature*, 186:496-497, 1960.

The clearness of the tracks of the 'Snowman' shown in the photograph taken by Eric Shipton has enabled me to make a reconstruction of its foot. This has been used to produce imprints in snow which show a great similarity to the natural tracks suggesting that the model is accurate.

The plaster foot has the following anatomical pecularities: (1) It is of great size: length 12½ in., fore breadth 7½ in., the width

thus being 60 per cent of the length. The heel is 6½ in. broad---nearly as broad as the fore part of the sole. (2) The hallux is very thick and separated from other toes. (3) The second is the longest toe, and it is separated from the hallux and third toe; it is thinner than the hallux though more powerful than the other toes. (4) The third, fourth and fifth toes are small and united towards their bases, although the distal end of each toe gives a clear impression in the snow. The little toe is less bent than the others.

These features can be seen to a lesser degree in human and gorilla feet, and by comparing them certain inferences can be made.

(1) the 'Snowman' foot is as long as that of a large gorilla but much broader. Sir Arthur Keith emphasized the increase of the tarsal, and decrease of the digital, elements of the skeleton of the foot during the evolution of higher primates. In the foot of the gorilla, the tarsal elements account for 39 per cent of the length measured along the third digit; in the human foot, it accounts for 52 per cent. At the same time, describing the soft tissues of sole, Keith states: "we see the enormous increase of the heel in the gorilla; it is relatively and absolutely larger than the heel area of man". Fig. 4 (not reproduced) shows this huge foot and it can be seen that it resembles the much larger heel of the Snowman. This may equally be regarded as a combination of the hominoid tendency towards increasing length and breadth of tarsal bones together with gorilloid tendency for a spread of the soft tissues. The great fore breadth (42.8 per cent of the length) is found also in the skeleton of the foot of the neanderthaloid from the Kiik Koba cave in the Crimea.

(2) The hallux of the 'Snowman' resembles that of the gorilla, except that it is straighter, longer and much more powerful. In conjunction with the depth of the impression made in the snow, this suggests that it carried much of the weight of the body. But at the same time the hallux has every appearance of being opposable, and could be used to grasp objects or in climbing. The first row of foot bones is shortest because of a very shortened metatarsal element, although the phalanges themselves are big. R. Neuhaus and H. Virchow call such cases in human foot "zuruck-tretende" hallux and they appear, as in the 'Snowman', to be due to an unusually short metatarsal bone.

(3) The unusually large size of the second toe of the 'Snowman' can be also compared with the human foot with "zurucktretende" hallux. This type of big toe can be accompanied with an elongated and thickened second toe. H. Klaatsch has described a similar case in an Australian aborigine, who possessed the same type of foot which was able to grasp much better then normal. This is the same combination of shortened first and elongated second axis both in the foot of the 'Snowman' and in an abnormal human foot.

These peculiarities can be explained functionally. The hallux is shortened and capable of grasping, but less effective for use in walking upright. The second toe is therefore also involved in balancing and is elongated and thickened. This type of foot

with its dual functions of grasping and balancing was probably characteristic of early prehominids in the first stages of the bipedal mode of life.

(4) The relative shortness of digits 3, 4 and 5 and their corresponding metatarsals are an obvious feature of the human foot. Also all three lateral toes of the 'Snowman' seem to show signs of partial syndactyly, but metatarsals 3-5 do not show any signs of the shortening which is observed in the human foot. The two distal phalanges of the little toe in the human foot are often fused together, and this condition may also explain the faintness of the impression made by the little toe of the foot of the 'Snowman', which we must suppose was held straighter than the others.

The impressions made by plaster casts of the feet of the mountain gorilla, langur and the tracks from the preserved feet of black bear show that the footprint of the 'Snowman' is much like that of the gorilla and very different from the other two. Yet it has been suggested that Shipton's photograph was a footprint of either a bear or langur.

Other criticisms that the footprint of the 'Snowman' was deformed by the melting of the snow do not seem well founded, in view of the sharpness of the imprint and the similarity to the fresh artificial prints from the plaster model.

Hans Gross mentions that good photographs of the tracks in the snow show even more details than do a plaster cast made from such tracks. But since heel and toes press more deeply into the snow than the sole, the arch of the foot is exaggerated and the impression is slightly shorter than the actual foot.

G. W. Gayer stressed that the deepest impressions of human tracks are: the outer side of the back of the heel and on the inner side behind the hallux. These details are well known from criminological studies and are very clearly seen in the tracks and plaster cast of the 'Snowman'.

In the original photograph it is also possible to see small heaps of snow thrown up by the back of the heel when it first touched the snow surface and was later pressed down by the flat of the heel, which is, according to Gayer, characteristic of the track of a bare human foot. This suggests that the track of the 'Snowman' was formed in the same way as a human one and that the 'Snowman' must walk in a human manner.

All the evidence therefore suggests that the so-called 'Snowman' is a very huge, heavily built bipedal primate, most probably of a similar type to the fossil Gigantopithecus.

RECENT EVIDENCE OF THE YETI, AN UNKNOWN PRIMATE, FROM THE HIMALAYAS

Cronin, Edward W., Jr.; *Pursuit,* 9:63-65, 1976.

Abstract: On December 17, 1972, the author, accompanied by Dr. Howard Emery and two Sherpa assistants, made camp on a

completely clear snowfield atop a ridge at an elevation of 12,200 feet in the Himalayas of eastern Nepal. On the morning of the 18th, a set of tracks was discovered in the snow indicating that a large creature had walked through the camp during the night; these spoor are not referrable to any known fauna of the Himalayas; these spoor closely match those recorded by earlier observers in the Himalayas and attributed to the yeti; these spoor suggest a creature using bipedal locomotion and with a foot morphologically similar to Gorilla gorilla. Based on the imprints left in the snow, a hypothetical reconstruction of the creature's foot is made and compared to the feet of known Himalayan large mammals.

Introduction. For the past two hundred years, reports of the yeti, or Abominable Snowman, have come out of the Himalayas suggesting the existence of an unknown primate hidden by the steep topography and inaccessibility of the mountains (Howard-Bury, 1922; Dyhrenfurth, 1959; Tschernesky, 1960; Tcherine, 1970). A composite description of the yeti can be constructed based on the various eye-witness sightings and photographs of footprints: Its body is stocky, ape-like in shape, and about 5¼ to 6½ feet tall, covered with short, coarse hair, reddish brown or greyish brown in color; the hair is longest on the shoulders. The head has a pointed crown indicating a marked sagittal crest. Small ears lie close to the head; the face is hairless and rather flat; the jaw is robust, the teeth are quite large although fangs are not present, and the mouth is wide. The arms are long, reaching almost to the knees. There is no tail. The creature walks primarily on two, rather than four feet, i.e., using bipedal locomotion. It foot is large, some 12½" long by 7½" wide, with the heel nearly as broad as the forepart of the sole. The great toe, or hallux, is exceptionally large, with the second toe the longest and relatively thin, while the remaining three toes are short, stubby, and united towards their base. The hallux is separated from the second toe in such a way as to suggest an opposable condition similar to that of known apes (Pongidae). Sanderson (1960) provides a thorough review and analysis of such reports, and they need not be further discussed.

From October, 1972, through March, 1975, I have been working in the Himalayas as chief scientist of the Arun Valley Wildlife Expedition. This interdisciplinary expedition conducted the first ecological survey of the remote Arun Valley located in far eastern Nepal; new species were discovered in several taxa, numerous distributional records were established, while significant ecological and behavioral data were gathered on selected species. Since reports of the yeti came from this area in the past (Ward, 1970), the expedition devoted special efforts to gathering new evidence on the yeti, and collecting background data on known mammals that would be serviceable in an evaluation of such evidence.

Methods. Photographic documentation and measurements were regularly made of the spoor, especially footprints, of large mammals frequenting the upper elevations, above 9,000 feet, where

there is an annual covering of snow, and the high-altitude zone, above 14,500 feet, where there is a perennial covering of snow. Plaster casts were made of the footprints in the snow of suspected yeti prints and of the footprints of several large mammals, including the Himalayan Black Bear (Selenarctos thibetanus), and the Snow Leopard (Panthera unicia). Possible variations produced in such prints by different snow conditions, terrain, and activities of the animal (running, walking, etc.) were noted.

New Evidence. In December, 1972, Dr. Howard Emery, expedition physician and parasitologist, Mr. Jeffery McNeely, expedition co-leader and mammalogist, and I decided to make a trip to the high-altitude areas around Kongmaa Mountain to investigate the winter conditions of the ecosystem and search for yeti prints. We left base camp in the Kasuwa Khola on the 14th. Although the weather was excellent, with a clear sky, warm sun, and little wind, an earlier snowstorm had deposited several feet of loose snow that made trekking difficult. By the 16th, our porters refused to continue, and McNeely accompanied them back towards base camp. Emery and I, assisted by two Sherpas, continued on to Kongmaa, on the 17th reaching an elevation of 12,200 feet along a ridge connecting to the peak.

At about 1600 hours, we found a depression on the ridge that was suitable for a camp (27°43' N; 87°16' E) in that there was a flat place for the tents. The area was small, less than half an acre, and was a completely clear snowfield untouched by animal prints. The slopes on the north side of the ridge descended to c. 7000 feet on the Kasuwa River; the slopes were quite steep, varying between 15 and 40 degrees. We made camp, pitching two tents about 60 feet apart at one end of the depression. We had dinner around an open fire at the other end, and retired just after dark (c. 1800 hours) to our tents. The evening was calm with a clear sky.

In the morning, Emery climbed out of our tent at c. 0540 hours, and walking away from the tent, noticed unusual footprints beside the trail that we had made from the tents to the cooking fire. I immediately climbed out and joined him; there was a remarkable series of footprints which the Sherpas identified as yeti prints. I examined the series carefully and, judging by the direction of the toes on the footprints, it appeared that during the night a creature had approached our camp by coming up the north slope, had proceeded directly between the tents, and then had crossed out onto the south slope.

I investigated the tracks on the north slope first, and then could see large punch holes in the snow where the creature had ascended; because the north slope was covered with very deep snow (a function, in part, of the angle of the sun), the prints were confused and the deep holes revealed no details. I followed the prints down the slope for several hundred yards, and could see the track continuing down towards the river. The heavy snow made walking impossible, and at times, I was forced to crawl or hang onto the vegetation to stabilize myself. I was impressed with the strength the creature displayed in coming di-

rectly up the slope. There was a dense growth of rhododendron bushes (c. 3-10 feet tall) on the slope which was buried by the snow. I noticed where the creature had stepped carefully on top of individual branches, using the displacement ability of the branches to support his weight above the snow (we have recorded bears and snow leopards using the same method). The creature was obviously well-adapted to travelling in Himalayan terrain.

I returned to the top of the ridge where, because of the snow conditions, most of the prints were distinct and well-defined. The top of the ridge had been previously exposed to the sun in such a way that the snow had melted to form a thin layer of crystalline snow on top of a hard crust; in addition, the prevailing winds from earlier days had cleared all loose snow. Thus, the prints revealed the details of the toes and the general morphology of the foot. The prints measured approximately 8½" by 4-3/4" (mean 8.6 + .3 inches long by 4.7 + .4 inches wide). The stride (distance between individual prints) was quite short (mean 21 + 8 inches) and it appeared that the creature had used a slow shuffling walk along this section. The prints displayed a short but large opposable hallux, an asymetrical arrangement of the four remaining digits, and a broad, rounded heel.

We made a full photographic record of the prints (in both black-and-white and color transparencies) prior to the sun hitting them. We sent word with one of the Sherpas down to McNeely, who later came up with the supplies to make plaster casts of the prints.

We then examined the prints which continued onto the south slope where increased exposure to the sun had melted most of the snow. We attempted to follow the track, but lost it on the bare ground and rock. Farther up the ridge towards Kongmaa, what appeared to be the prints made by the same creature crossed back onto the top of the ridge from the south slope. The ridge at this point was covered with low bushes which enabled snow to accumulate, and the prints were again punch holes in the snow revealing little detail. This series of tracks crossed back and forth across the top of the ridge several times before disappearing on the south slope. We attempted to follow the prints onto the south slope a second time, but again, the trail was confused by intermittent patches of bare ground.

It was apparent to us that the creature which had made the prints was far stronger than any of us, and it was futile to attempt to follow it under the conditions of the slope and snow covering. Indeed, just to cover the distance the creature had travelled in coming up the north slope during the night would have taken us an entire day, if not two days. We investigated the possibility of a hoax perpetrated by our Sherpas, but discounted it after realizing that the Sherpas were not strong enough nor had they had the time to make the full length of prints that we could see from the top of the ridge. During the following three days, we kept careful watch for the possible reappearance of the creature and searched neighboring areas for additional prints, but there were no further developments. Du-

ring the remaining 13 months of the Arun Valley Wildlife Expedition, we made several research trips to different areas to gather more evidence about the yeti, but they were unsuccessful and this was the only encounter we had with valid yeti prints.

Analysis of the Footprints. The footprints that we discovered in the early morning of the 18th were remarkable in their similarity to a footprint photographed by Eric Shipton and Michael Ward during the 1951 Mt. Everest Reconnaisance (Shipton, 1955). That photographed footprint has since become the "type-specimen" for yeti prints as it is exceptionally clear and distinct. The principle difference between our prints and Shipton's is size, our's being somewhat smaller. This difference could easily be accounted for by an immature creature or possibly a female (sexual dimorphism is not uncommon among Pongidae).

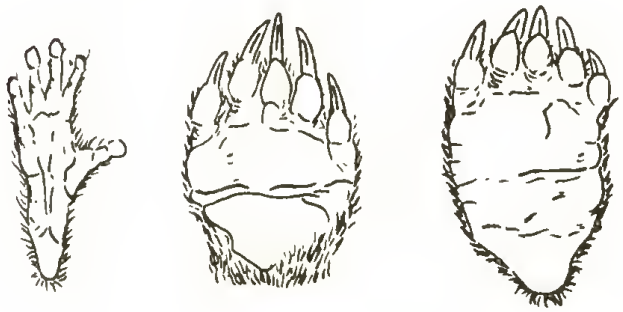
Like Shipton's print, our prints are not referrable to any known animal of the Himalayas. Suggestions for identifying yeti prints range from bears, langur monkeys, snow leopards, wolves, and eagles to the wandering yogi. Such confusion is possible because some prints (reported by earlier authors) were in fact so old, exposed to melting by the sun, wind erosion, or distortion in soft snow, that it is difficult to reach any decision. We know that our prints were made on the night of the 17th. We photographed them before sunrise; there was little wind (estimated less than c. 5 knots/hour) when we went to sleep. In examining our own footprints made the afternoon before with our own fresh footprints made on the morning of the 18th, we saw little distortion or erosion. Consequently, the creature's prints, like Shipton's, were fresh and made on a thin layer of crystalline snow so that specific comparisons of the yeti prints and known mammal prints are possible.

Figure 2 illustrates a reconstruction of the creature's foot based on the photographs of the footprints we discovered at our camp. The hallux is set down to the side in a manner similar to the foot of Gorilla gorilla (Figure 3) and thus is too short and too opposable to permit confusion with the human foot (Figure 1); although many of the local hill tribe people seldom wear shoes and thus often have splayed toes, their hallux is never as short relative to the other toes as displayed in the reconstruction. In comparison with monkey prints, such as the Langur Monkey (Presbytis entellus; Figure 4) which is known to frequent areas up to 12,000 feet in the Himalayas, the foot is too small and the entire musculature is different; the width/length ratio of a langur foot is approximately 25% while that of the creatures foot is more than 50%, making even a giant langur foot too narrow to be considered. Hypotheses that entertain the possiblities of snow leopards or wolves (which have nearly rounded prints), and eagles (which have four narrow toes in a palmate structure) are ludicrous.

Bears, because of their size, habits, and habitat, are the most likely candidates. There are three species of bear known to inhabit the Himalayas, and one, the Himalayan Black Bear (Figure 5, forefoot; Figure 6, hindfoot) is a frequent visitor to the areas

1. 2. 3.

Figure 1: Human foot
Figure 2: Reconstruction of yeti foot discovered in the morning of December 18, 1972.
Figure 3: Foot of *Gorillo gorillo*



4. 5. 6.

Figure 4: Foot of Langur Monkey *(Presbytis entellus)*
Figure 5: Forefoot of Himalayan Black Bear *(Selenorctos thibetonus)*
Figure 6: Hindfoot of Himalayan Black Bear *(Selenarctos thibetonus)*

(All Figures drawn by the author, Edward W. Cronin, Jr.)

around Kongmaa Mountain. During the expedition's stay in the Arun Valley, we encountered numerous bear prints which the local villagers would occasionally identify as yeti prints. Bears often walk by placing their hindfoot on the print of the forefoot thereby distorting individual prints and permitting villagers to mistake the actual shape of the bear's feet. However, in even a short series, bear prints invariably show the equally sized, symmetrical arrangement of the toes typical of the group. Our creature's prints, by comparison, show distinctly larger and smaller toes, arranged in a characteristic asymmetrical pattern. Further, bear prints have a narrow, drawn-out heel, and usually include impressions of the claws, neither of which were evident in our series of yeti prints.

The arrangement of the creature's prints demonstrated a left

-right - left - right pattern, without overlapping, that indicates bipedal progression. There was no indication that more than two appendages were used at any one time to make the entire series of tracks. Although numerous authors (ses also, Reynolds, 1967; Hagen, 1961) have concluded that the yeti walks on two feet, bipedalism seems to have aroused the greatest controversy and source of disbelief among yeti sceptics.

Bipedalism is often thought by the uninformed to be unique in man among primates, but it is actually by no means rare among apes. Gibbons, for example, are habitually bipedal in walking, and all other apes are capable of bipedalism at times. Adriaan Kortlandt (1962) found that chimpanzees he was studying walked bipedally for 10-15% of the distance they covered. Dr. Sydney Britton found that the captive chimpanzees he was studying walked bipedally when there was snow on the ground. George Schaller (1964) reported that gorillas frequently walked bipedally when the vegetation was wet, presumably to keep themselves dry. Nor is bipedalism a recent adaptation of the apes; according to Pilbean and Simons (1965), "the Miocene apes and their Oligocene ancestors probably showed a high degree of trunk erectness and doubtless spent much time walking or running bipedally either in trees or on the ground." In careful analysis, it not only seems possible, but altogether likely that an ape who frequents the high snows of the Himalayas would be using bipedal locomotion.

The creature that made the prints weighed less than or equal to the weight of an average man. Comparison of the depth of my own footprints (I weighed approximately 185 lbs., including winter clothing and boots) to that of the creature's footprints indicated that my prints were slightly deeper and suggested that this individual weighed c. 165 lbs. This estimate is based on the assumption that there was little change in the ability of the snow to bear weight between the time the creature made its prints and when we compared my prints.

The circumstances of the creature's visit support the often considered hypothesis that the yeti is nocturnal. This seems probable as many large mammals, such as certain elephant populations in South Asia, have adapted to nocturnal activity because of disturbance by man. The circumstances also indicate that the creature displayed some curiosity behavior (not unlikely, if it was indeed an immature). It appeared that the creature approached our camp by following a natural spur up the ridge to cross from the Barun to the Kasuwa River valleys at a point which would enable it to avoid the heavier snows further up the ridge. Although it is possible that the creature saw our camp during the first hour of darkness on the 17th, it seems more plausible that it did not know our camp was there until it was almost upon it; the point at which it first reached the top of the ridge was some 20-30 yards east of our camp. However, rather than turn back, or cross the ridge at that point, the creature made a detour west along the top of the ridge towards our camp and eventually passed directly between the tents. The tracks of the creature support the hypothesis that the yeti frequents the forested regions, using the snow-laden passes only to cross from one area to

the next. The tracks came from the heavily forested valley of the Barun; the track did not appear to go in the direction of the higher snowfields, but rather, appeared to cross the ridge and continue towards the forests of the Kasuwa.

Summary. The creature that visited our camp on the night of December 17, 1972, provided us with the opportunity to carefully document and analyze a series of tracks that are not referable to any known animals of the Himalayas, and are definitively yeti prints in that they closely match prints recorded by Shipton and attributed to the yeti. Their resemblance to Shipton's print (discovered 21 years before and a long distance from our camp) indicates a uniformity of data suggesting the existence of an unknown anthropoid alive in the Himalayas, while undermining the possibility that yeti reports are the product of a predisposition to interpret varied data or a general hoax.

References:

Dyhrenfurth, N.G. 1959. "Slick-Johnson Nepal Snowman Expedition." Amer. Alpine Jour. New York

Hagen, Toni, 1961. Nepal. Oxford Book Co., Calcutta

Heuvelmans, Bernard, 1959. On the Track of Unknown Animals. Rupert Hart Davis, London.

Howard-Bury, C.K. 1922. Mount Everest, the Reconnaissance. London

Izzard, Ralph. 1955. The Abominable Snowman Adventure. Hodder and Stroughton. London.

Kortlandt, Adriaan. 1962. "Chimpanzees in the Wild". Scientific American. May.

Reynolds, Vernon. 1967. The Apes. Cassell. London.

Sanderson, Ivan. 1960. Abominable Snowmen: Legend Come to Life. Philadelphia.

Schaller, George. 1964. The Year of the Gorilla. University of Chicago Press. Chicago.

Shipton, Eric. 1952. The Mount Everest Reconnaissance Expedition. Hodder and Stoughton. London.

Stoner, C. 1955. The Sherpa and the Snowman. London.

Tchernine, O. 1970. The Yeti. Neville Spearman. London.

Tombazi, A. 1925. Account of a Photographic Expedition to the Southern Glaciers of Kanchenjunga. Bombay.

Tschernesky, W. 1960. "A Reconstruction of the Foot of the Abominable Snowman." Nature. 186. May 7.

Ward, J. P. 1970. "The Iswa Khola-Himalayan Expedition, 1969." The Himalayan Jour. Oxford University Press. London.

SIGHTING THE YETI'S RELATIVES

Anonymous; *Nature,* 271:603, 1978.

Following reports of a possible "Loch Ness" monster in Lake Kos Kol, Soviet scholars have postulated a possible "relation" of the Yeti. The Institute of Language, Literature, and History of the Yakut Branch of the Academy of Sciences of the USSR has collected numerous accounts of sightings of a creature known locally as the "Chuchunaa", a name which apparently is connected with the Yakut word for "fugitive" or "outcast".

According to informants, the Chuchunaa is over 2m tall, wearing deerskin, and is unable to talk, uttering only a piercing whistle. He is described as a meat-eater, and is said to have the habit of creeping up to settlements and stealing food. When the Chuchunaa sights a hunter or reindeer-herder, he usually takes flight, but on occasion, it is said, will pick a fight. (No data have been released as to the outcome of such encounters.)

. . . . .

According to Semen Nikolaev, a senior staff member of the Yakut Branch of the Academy, "almost all witnesses speak of the Chuchunaa as a reality without the fantastic detail so characteristic of legends". (To a Westerner, the above description seems very much the stuff of which legends are made---and one cannot help wondering whatever Nikolaev would classify as "fantastic"---Baba Yaga riding on her pestle, maybe?). Since the details of many sitings coincide, Nikolaev and his colleagues seem willing to admit the postulate that the Chuchunaa represents the last surviving remnant of the "Palaeoasiatic aborigines" of Siberia, who have retreated to a last refuge in the upper reaches of the Yana and Indigirka rivers. Indeed, since the last reliable sighting dates from the 1950s, some of the more pessimistic experts think that the Chuchunaa may have died out during the last two decades.

THE MONSTEROUS WILD-MEN

Anonymous; *Scientific American,* 1:3, May 18, 1846.

A hoaxical correspondent of the 'South Missourian,' states that one of the monstrous animals, frequently noticed in the western

papers as wild-men, has been killed and another taken alive. The one that was killed, (says the account) proved not to be a woman, as was expected. They are of immense size; the foot of the dead one is twenty-one inches long, and he measures, extended on a plank floor, fourteen feet and eight inches; the other stands fifteen feet six inches in his bare feet. Both of them are entirely naked, although their bodies are quite hairy, and skin nearly the color of an Indian.

SOME BIGFOOT TRADITIONS OF THE NORTH AMERICAN TRIBES

Coleman, Loren E., and Hall, Mark A.; *INFO Journal*, 2:2-10, 1970.

A vast folklore and a belief in a race of very primitive people with revolting habits is found from northern California up into the Arctic lands themselves. This tradition covers not only the whole stretch of the Pacific coast, but much of the rugged territory to the east, even into Greenland. Generally, these subhominids are described as very tall, fully haired, and retiring. Sometimes they are described as carnivorous.

To study this tradition, however, a note at the beginning must be made of the folklore of northeastern Asia. Among the Chuckchee, Bogoras (1902) finds kele$_1$ -- evil spirits -- and kele$_2$ -- tribes of ancient times or cannibals. No sharp dividing line is to be found between these two words. However, Bogoras discovers that in the North American Eskimo terms with the same meanings, respectively -- tornait (singular, tornaq) and tornit (singular, tuneq) -- there exists a sharp division in import.

In Alaska (specifically Point Barrow), Spencer (1959:259) observes:

> The western Eskimo appear not to have developed the rather elaborate views, found in the central and eastern regions, of a race of elder beings, those often referred to as the tornait. The term, however, is cognate with tunarat, which in North Alaska refers specifically to the powers of the shaman.

Spencer seems to have confused tornit, the ancient tribe, with tornait, the spirits that rule objects (sometimes called upon by the shamans). He (1959:261) does record that "giants were also part of the local terrain. They had no special powers, were simply 'big men'..... They were regarded as timid and avoided contact with other men." These do have traits in common with the tornit of the east, and one wonders as to the source of Spencer's confusion. Boas (1964: 226-227) relates the tales from the central Eskimo on the tornit;

> In olden times the Inuit (i.e. the Eskimo) were not the

> only inhabitants of the country in which they live at the present time. Another tribe similar to them shared their hunting ground . . . The Tornit were much taller than the Inuit and had very long legs and arms. Almost all of them were blear eyes. They were extremely strong and could lift large boulders, which were by far too heavy for the Inuit.
> . . . . . .
>
> They made neither kayaks nor bows.
> . . . . . .
>
> The Tornit could not clean the sealskins so well as the Inuit, but worked them up with part of the blubber attached. Their way of preparing meat was disgusting, since they let it become putrid and place it between the thigh and the belly to warm it.
>
> The old stone houses of the Tornit can be seen everywhere. Generally they did not build snow houses, but lived the whole winter in stone buildings, the roofs of which were frequently supported by whale ribs.

The Tornit are to be designated from traditions relating to the Indians who are called Adla or Equigdleq, i.e. Half-dogs, (Boas, 1879). Kroeber (1899) tells of the Tornit tales from the Smith Sound Eskimos and notes that they are a frequent element in Greenland texts. He also learned of the Tutuatin a fabulous being with tangled hair; although the true importance of this creature is unknown, it may be part of the Tornit tradition under a different name.

Observing that the Akudnirmuit (the inhabitants of Akudnirn) call the Tornit the "Tuniqdjuait", Boas (1964) goes on to note the Eskimo there as well as in Labrador considered the Tornit somewhat similar to themselves. "In Greenland they are entirely a fabulous tribe, each individual being of enormous size, living inland, and seldom hunting in the upper parts of the fjords" (Boas, 1964:232). From northern Baffin Land the Tornit are referred to as the Toonijuk and appear to be similar in many traits (very large, possess disgusting habits). Some clue to the fate of these beings is reflected by their temperament, for the

> Toonijuk were not dangerous; on the contrary they were timid and terribly afraid of dogs; they were also stupid and slow-going. The Pond Inlet Eskimos say that these big people never attacked Eskimos but fought among themselves until they killed each other off (but other Eskimo tribes claim to have stalked the stupid giants and killed them one by one, like game.) They disappeared from the Canadian Arctic long before the memory of the oldest Eskimo, and only dim, distorted shreds of tales remain. When Idlouk was asked when the Toonijuk were here he could only answer, 'Long ago, before my grandfather was born.' That means, to an Eskimo, beyond memory.
>
> From grandmother to grand-child have come out of the

dark past a few derelict tales of despised, repugnant sub-humans (Scherman, 1956: 158-159).

The Tornit of the Eskimo is a widespread tratition of giant pre-humans that existed in former times which is complemented by similar tales.

> The Mahoni, who flit through the Peel River Country in the northern Yukon, are enormous hairy giants with red eyes, who eat human flesh and devour entire birch trees at a gulp. The predatory Sasquatches of British Columbia's mountain caves are eight feet tall and covered with black woolly hair from head to foot. There are others, all kin to these: the terrible Brush Man of the Loucheaux in the upper Mackenzie, with his black face and yellow eyes, preying on women and children; the Weetigo of the Barrens, that horrible naked cannibal, his face black with frostbite, his lips eaten away to expose his fanglike teeth; the eight-foot head-hunting 'Mountain Men' of the Nahanni; and those imaginary beings of Great Slave Lake whom the Dogrib Indians simply call 'the Enemy' and fear so greatly that they must always build their homes on islands safe from the shoreline where the Enemy roam (Berton, 1956: 10-11)

From the east to the west coast of Canada and the northern United States, a pattern of related descriptions of these unknown beings exists.

Among the Micmac, a group located in New Brunswick and Nova Scotia, are references to the Gugwes. "These cannibals have big hands and faces hairy like bears. If one saw a man coming he would lie down and beat his chest, producing a sound like a partridge" (Parsons, 1925: 56). Although the connection between a primate lying down and making a sound is unclear, this theme, the one-tone whistle (which is the call of the Gray Partridge, Perdix perdix of SE Canada (Robbins, et all, 1966) is repeated in other areas as an item of these beings' behavior. The beasts are known by other Micmac names: Kookwes, Chenoo, Djenu (Wallis and Wallis, 1955).

In Maine among the Penobscot, tales are told of Kiwakwe, a cannibal giant (Speck, 1935b: 81). Giants are familiar beings known, also, to the Huron and Wyandot as Strendu. They are half-a-tree tall and large in proportion to men. Furthermore, they are cannibals and covered with flinty scales. Their fate is unknown, but the belief is they are still living, perhaps east of the Lake Huron area where these tribes live (Barbeau, 1914).

These beings seem very similar to those of Upper New York State. Thus, the Stone Giants are huge men "covered" wth flint and other stones.

> The Iroquoian Stone Giants, as well as their congeners among the Algonquians (e.g. the Chenoo of the Abnaki and Micmac), belong to a widespread group of mythic be-

> ings of which the Eskimo Tornit are examples. They are ...huge in stature, unacquainted with the bow, and employing stones for weapons. In awesome combats they fight one another, uprooting the tallest trees for weapons and rending the earth in fury... Commonly they are depicted as cannibals; and it may well be that this far-remembered mythic people is a reminiscence, coloured by time, of backward tribes of historic times. Of course, if there be such an historical element in these myths, it is coloured and overlaid by wholly mythic conceptions of stone-armoured Titans and demiurges. (Alexander, 1964: 29).

These ogres are the same as the Windigo (or Witiko, Wendigo, Wittiko and other forms) of Algonkian origin. This extensive tradition covers eastern and central Canada and is well documented. Among the Tete-de-Boule of Quebec, this giant cannibalistic man is given different names: Witiko, Kokotshe, Atshen (Guinard, 1930). The Micmac Chenoo appears to be similar with the Cree Witiko, for Cooper (1933: 23) states: "Both have the same characteristics... The very name Chenoo seems to be identical with the Montagnais and Tete-de-Boule (Cree) name, Atcen, for the Witiko." For also among the Naskapi "the nearest analogy in name and character with Atcen among neighboring peoples is the Chenoo (Tcenu) of Micmac legend" (Speck 1935a: 72).

Likewise specific traits clearly resemble those of parallel creatures in other areas.

> The witiko wore no clothes. Summer and winter he went naked and never suffered cold. His skin was black like that of a negro. He used to rub himself, like the animals, against fir, spruce, and other resinous trees. When he was thus covered with gum or resin, he would go and roll in the sand, so that one would have thought that, after many operations of this kind, he was made of stone (Guinard, 1930: 69).

Cooper (1933: 23) notes that

> ... a similar habit is ascribed to the Passamaquoddy Chenoo who used to rub themselves all over with fir balsam and then roll themselves on the ground so that everything adhered to the body.
>
> This habit is highly suggestive of the Iroquoian Stone Coats, the blood-thirsty cannibal giants, who used to cover their bodies carefully with pitch and then roll and wallow in sand and down sand banks.

Windigos have a frightful and menacing mouth with no lips. Often a sinister hissing is made by them or a noise described as strident, very reverberating, and drawn out is accompanied by fearful howls. Windigo is a huge individual "who goes naked in the bush and eats Indians. Many people claim to have heard

him prowling in the woods" (Davidson, 1928b: 267). In Quebec, the Grand Lake Victoria Band relate tales of the Misabe, a giant with long hair (Davidson, 1928a). Among the Ojibwa of northern Minnesota, Coleman (1937: 41) describes "the Memegwicio, or men of the wilderness. Some called them a 'kind of monkey'.... And were described as being about the size of children of ten or eleven years of age... faces covered with hair." They appear to be a miniature Windigo and may, in fact, be the natives attempt to categorize the young of the Windigo. Among the Tingami Ojibwa the Memegwesi are "a species of creature which lives in high remote ledges. They are small and have hair growing all over their bodies. The Indians think they are like monkeys judging from specimens of the latter they have seen in picture-books" (Speck, 1915a: 82). Around James Bay the Cree know of the memgwecio, "the diminuitive being who looks like a human except that he is covered with hair and has a very flat nose" (Flannery, 1946: 269). The general theme of hair (or the embellishment of sand and stone attached to it) gives these beasts as well as all Windigo-type creatures, an unhumanlike quality. Indeed, from Micmac information Wallis and Wallis tell that the "Gugwes is a grotesque creature; in 1911-1912 he was commonly compared to a baboon; in 1950 he was described as a giant" (1955: 417).

Among the Northern Athabascan Indians in the Canadian plains and Alaska a concept persists of the Nakani, human figures living in the bush country. They are also referred to as "Bad Indians", Brush-men and by similar names. Detailed descriptions of them are lacking both from disbelief by chroniclers and because of the Nakani reputedly depart when still at a distance from any observer. These beings are said to be active only in the summer foliage, spending winters underground in camouflaged pit lodges. Their presence is often known by whistling heard in the woods or by the observation of shoe-like tracks. In the past they were known to travel in groups, but presently they are observed only singly.

In the early 1960's VanStone (1965: 105) notes this belief in the Nakani among the Chipewyan at the settlement of Snowdrift on Great Slave Lake:

> There is also much discussion among the Snowdrift Indians concerning so-called 'bush men' who are believed to roam the bush during the summer months, frequently coming near the village. They are sometimes used as a device to frighten children..... However, adults believe in them too, and several people told the author of encounters with 'bushmen.'

During the previous century at nearby Fort Resolution, Ross (1872: 309) observes among the Indians their fear of "enemies." He relates that "I have on several occasions caused all the natives encamped around to flock for protection into the fort during the night by simply whistling, hidden in the bushes."

MacNeish (1954: 185-188) and Osgood (1932: 85-86) describe

the characteristics and stories of the Nakani among the tribes in the area of Great Bear Lake. Further to the west the presence of this belief is well documented among the Kutchin in the northern Yukon and Alaska (Osgood, 1936: 154, 157 and Slobodin, 1960: 126-129) and on the Kenai Peninsula (Osgood, 1937: 171-173). Typically these beings are recounted to kidnap women and children. Attitudes toward these creatures seem to reflect recent events attributed to encounters with them in that disappearances and alleged attacks create apprehension in some areas, especially for women and children.

The origin of this widespread and consistent belief remains an unsettled question. One clearly expressed and relevant observation of this belief is made by MacNeish 1954: 186), Osgood (1937: 171) and VanStone (1965: 105). They state that the Nakani have no supernatural powers and are human figures with characteristics and habits that set them apart from ordinary humans.

The "stone" theme reappears among the Shoshonean tales. Of these beings "the northern Shoshoni say that formerly there were numerous Stone Giants (Dzoavits) dwelling in the hills" (Alexander, 1964: 134). Although no information is given as to the nature of the "stone" of these giants, the same evolution, from hairy covering to some type of resin or gum base with sand attached, most probably occurred here as with those recorded in the east.

From the northern interior of British Columbia, among the Kaska Indians are told the stories of men with coarse, thick hair (Teit, 1917). Another Athabascan group, the Sinkoyne, have tales of men who are somewhat bear-like (Gifford, 1937). The Anderson and Seton Lake Indians of British Columbia tell of numerous giants (Elliot, 1931). As can be seen this tradition comes to its highest embodiment in British Columbia. For example, among the Carrier, one of these monsters "left enormous footprints in the snow... It had the face of a human being, it was exceedingly tall...and covered with long hair" (Jenness, 1934: 221). A similar tradition is common in the Lillooet. The beings, called Hailo Laux or Haitli laux, are very tall people, men that are ten feet tall, and broad and strong. They have hair on their breasts, and look like bears. The hair on the head is long. Most of them are black-haired, some brown, but quite a number do have red hair. The Indians believe they are bad people, wander about at night, and are never known to sleep (Teit, 1912). The last idiosyncrasy may refer to the beast's natural nocturnal foraging. Among the Karok and Yorok, as Kroeber (1925: 304) observes, there is the sharp impression "of the idea of an ancient prehuman but parallel race." This same belief seems to be prevalent throughout the Northwest today via Bigfoot and Sasquatch accounts (Sanderson, 1961; Green, 1969; Patterson, 1966).

Although the present day encounters in the Pacific Northwest are almost something of an everyday occurrence, another living tradition does exist in the East. The accounts from Monroe, Michigan, are well-known, and need not be recounted in detail here. What is noteworthy is the chronology of events

before and after the major Monroe sightings. On August 9th, 1965, east of Smithville, Ontario, a Lakeview truck driver reported seeing a seven foot tall, hairy beast with broad shoulders, a small head, and long arms near the side of a back road. Near Monroe, Michigan, on the 13th of that August, a seven foot tall something with hairs "like quills" reached through an automobile window, and gave Christine Van Acker a now famous blackened left eye. Other "sightings" were made thereafter around Monroe. One of the most extraordinary in the eyes of the local authorities was the claim of two fishermen on Voorheis Lake, Oakland County, who stated thay had seen an amphibious creature emerge from the lake. Then in early September of 1965, came reports of a monster prowling around Tillsonburg, Ontario. Its footprints were plainly visible (in sand) and measured 18 inches in length. Thus a traveling "Windigo", if you will, appears to have come by Smithville on its way to Michigan, and returned via Tillsonburg. Sketchy reports from Newmarket, Ont., for 1965, may be but another leg in this beast's route. Could have the cool nature of the summer of 1965, the coolest since 1950, have forced the movement of a lone ABSM into the habitat of man? Perhaps but the record shows the August-September, 1965 reports are only outstanding because of their correlation in time and space. The Fremont, Wisconsin (Nov. 1968) case is known to most, but ABSM-people encounters also took place that year in Easterville, Manitoba, and LaCrescent, Minnesota. In June of 1964, Sister Lakes, Michigan, "happened", but this would have been of little surprise to Phillip Williams and Otto Collins who were picked up and carried for a short distance by an ape-like thing with a rotten smell near Marshall, Michigan, in May of 1956.

The basis of the aboriginal traditions may seem hazy to the folklorist and anthropologist, but to the people who have come face to face with a member of the population of Bigfoots/Sasquatches of the Pacific Northwest or with an individual of the similar remnant race of the East, one doubts if you could convince these observers of the worthless validity of subhominid tales.

References

Alexander, Hartley Burr (1964) - North American, Vol. X of Mythology of All Races, ed. L.H. Gray (13 vols.). New York, Cooper Square Pub., Inc.

Barbeau, C.M. (1914) - "Supernatural Beings of the Huron and Wyandot", American Anthropologist, 16: 288-313.

Berton, Pierre (1956) - The Mysterious North. New York, Alfred A. Knopf.

Boas, Franz (1897) - "Traditions of the Ts' ets' a ut", Journal of American Folklore, 10: 35-48.

Boas, Franz (1964) - The Central Eskimo. Lincoln, Neb. Univ. of Nebraska Press.

Bogoras, Wladimar (1902) - "The Folklore of Northeastern Asia as Compared with that of Northwestern America", American

Anthropologist, 4: 577-684.
Coleman, Sister Bernard (1937) - "The Religion of the Ojibwa of Northern Minnesota", Primitive Man, 10: 41.
Cooper, John M. (1933) - "The Cree Witiko Psychosis", Primitive Man, 6: 20-24.
Davidson, D.S. (1928a) - "Folktales from Grand Lake Victoria, Quebec", Journal of American Folklore, 41: 275.
Davidson, D.S. (1928b) - "Some Tete De Boule", Journal of American Folklore, 41: 267.
Elliot, W.C. (1931) - "Lake Lillooet Tales", Journal of American Folklore, 43:174.
Flannery, Regina (1946) - "The Culture of Northeastern Indians", in Johnson (ed.) Man in Northeastern North America, (Papers of the Robert S. Peabody Foundation for Archaeology, Vol. 3) Andover, Mass. Phillips Acad.
Gifford, E.W. (1937) - "Coast Yuki Tales", Journal of American Folklore, 50: 142.
Green, John W. (1969) - On the track of the Sasquatch. Agassiz, British Columbia, Cheam Publishing Ltd.
Guinard, Rev. Joseph E. (1930) - "Witiko Among the Tete-de-Boule", Primitive Man, 3: 69-71.
Jenness, Diamond (1934) - "Myths of the Carrier Indians of British Columbia", Journal of American Folklore, 47:
Kroeber, Alfred L. (1899) - "Tales of Smith Sound Eskimos", Journal of American Folklore, 12: 166-167.
Kroeber, Alfred L. (1925) - Handbook of the Indians of California. Bureau of American Ethnology, Bulletin 78.
MacNeish, June Helm (1954) - "Contemporary Folk Beliefs of a Slave Indian Band", Journal of American Folklore, 67: 185-198.
Osgood, Cornelius B. (1932) - "The Ethnography of the Great Bear Lake Indians", National Museum of Canada, Bulletin 70 (Annual Report for 1931), Ottawa.
Osgood, Cornelius B. (1936) - "Contributions to the Ethnography of the Kutchin", Yale Nniv. Pub. in Anthropology, No. 14
Osgood, Cornelius B. (1937) - "The Ethnography of the Tanaina", Yale Univ. Pub. in Anthroplogy, No. 16.
Parsons, Elsie Clews (1925) - "Tales of the Micmac", Journal of American Folklore, 38: 56.
Patterson, Roger (1966) - Do Abominable Snowmen of American Really Exist? Yakima, Washington, Franklin Press, Inc.
Robbins, Chandler S., B. Brunn, H.S. Zim 1966) - Birds of North America. New York, Golden Press.
Ross, Bernard R. (1872) - "The Eastern Tinneh", Annual Report to the Board of Regents of the Smithsonian Inst. (1866) Washington, D.C.
Sanderson, Ivan T. (1961) - Abominable Snowmen: Legend Come to Life. Philadelphia, Chiiton Co.
Scherman, Katherine (1956) - Spring on an Arctic Island. Boston, Little, Brown, and Co.
Slobodin, Richard (1960) - "Some Social Functions of Kutchin Anxiety", American Anthropologist, 62: 122-133.
Speck, Frank G. (1915) - Myths and Folklore of the Timiskaming

Algonquin and Timagami Ojibwa, Anthropologican Series, Canada Dept. of Mines, Geological Survey, No. 9.
Speck, Frank G. (1935a) - Naskapi. Norman, University of Oklahoma.
Speck, Frank G. (1935b) - "Tales of the Penobscot", Journal of American Folklore, 48:81.
Spencer, Robert F. (1959) - The North Alaskan Eskimo. Bureau of American Ethnology, Bulletin 71.
Teit, James (1912) - "Traditions of the Lillooet Indians of British Columbia", Journal of American Folklore, 25: 346-347.
Teit, James (1917) - "Kaska Tales", Journal of American Folklore, 30: 438.
Van Stone, James W. (1965) - "The Changing Culture of the Snowdrift Chipewyan", National Museum of Canada, Bulletin 209, Ottawa.
Wallis, Wilson D. and Ruth Sawtell Wallis (1955) - The Micmac Indians of Eastern Canada. Minneapolis, Univ. of Minnesota Press.

BRITISH COLUMBIA SASQUATCH

Green, John; *Sasquatch, The Apes Among Us,* Hancock House, Seattle, 1978, pp. 83-85.

Every field of investigation probably has its classic cases. Certainly the sasquatch investigation does. Dealing with more recent events the list would probably vary for each investigator, depending on what he or she personally has been involved in, but up to the time of the Patterson movie in 1967 I think almost everyone would agree that the main items would be "Jacko", Ape Canyon, Albert Ostman and "Bigfoot".

All of those reports have been the subject of speculation and scrutiny for nearly 20 years except for the Patterson movie, and it has undoubtedly been more thoroughly shaken down than all the others put together. I will deal with it in a later chapter, and I have already dealt with "Bigfoot". As to the other three classics, how do they look now?

"Jacko" has been around by for the longest, and has suffered the most from modern enquiry---although he has also picked up a certain amount of supporting testimony. So long after the event, (or non-event) it will certainly never be possible to settle the status of the story beyond dispute. The full story, published in the Victoria, B. C. Daily Colonist on July 4, 1884, ran as follows:

WHAT IS IT?

A Strange Creature Captured Above Yale
A British Columbia Gorilla

In the immediate vicinity of No. 4 tunnel, situated some twenty miles above this village, are bluffs of rock which have hitherto been unsurmountable, but on Monday morning last were successfully scaled by Mr. Onderdonk's employees on the regular train from Lytton. Assisted by Mr. Costerton, the British Columbia Express Company's messenger, and a number of gentlemen from Lytton and points east of that place who, after considerable trouble and perilous climbing, succeeded in capturing a creature which may truly be called half man and half beast. "Jacko" as the creature has been called by his capturers, is something of the gorilla type standing four feet seven inches in height and weighing 127 pounds. He has long, black, strong hair and resembles a human being with one exception, his entire body, excepting his hands, (or paws) and feet are covered with glossy hair about an inch long. His fore arm is much longer than a man's fore arm, and he possesses extraordinary strength, as he will take hold of a stick and break it by wrenching or twisting it, which no man living could break in the same way.

Since his capture he is very reticent, only occasionally uttering a noise which is half bark and half growl. He is, however, becoming daily more attached to his keeper, Mr. George Tilbury, of this place, who proposes shortly starting for London, England, to exhibit him. His favorite food so far is berries, and he drinks fresh milk with evident relish. By advice of Dr. Hannington raw meats have been withheld from Jacko, as the doctor thinks it would have a tendency to make him savage. The mode of his capture was as follows:

> Ned Austin, the engineer, on coming in sight of the bluff at the eastern end of the No. 4 tunnel saw what he supposed to be a man lying asleep in close proximity to the track, and as quick as thought blew the signal to apply the brakes. The brakes were instantly applied, and in a few seconds the train was brought to a standstill. At this moment the supposed man sprang up, and uttering a sharp quick bark began to climb the steep bluff. Conductor R. J. Craig and Express Messenger Costerton, followed by the baggageman and brakemen, jumped from the train and knowing they were some twenty minutes ahead of time immediately gave chase. After five minutes of perilous climbing the then supposed demented Indian was corralled on a projecting shelf of rock where he could neither ascend nor descend. The query now was how to capture him alive, which was quickly decided by Mr. Craig, who crawled on his hands and knees until he was about forty feet above the creature. Taking a small piece of loose rock he let it fall and it had the desired effect of rendering poor Jacko incapable of resistance for a time at least.
>
> The bell rope was then brought up and Jacko was now lowered to terra firma. After firmly binding him and placing him in the baggage car "off brakes" was sounded and the train started for Yale. At the station a large crowd who had heard of the capture by telephone from Spuzzum

Flat were assembled, each one anxious to have the first look at the monstrosity, but they were disappointed, as Jacko had been taken off at the machine shops and placed in charge of his present keeper.

The question naturally arises, how came the creature where it was first seen by Mr. Austin? From bruises about its head and body, and apparent soreness since its capture, it is supposed that Jacko ventured too near the edge of the bluff, slipped, fell and lay where found until the sound of the rushing train aroused him. Mr. Thos. White and Mr. Gouin, C. E., as well as Mr. Major, who kept a small store about half a mile west of the tunnel during the past two years, have mentioned having seen a curious creature at different points between Camps 13 and 17, but no attention was paid to their remarks as people came to the conclusion that they had either seen a bear or stray Indian dog. Who can unravel the mystery that now surrounds Jacko! Does he belong to a species hitherto unknown in this part of the continent, or is he really what the train men first thought he was, a crazy Indian! (pp. 83-85)

Chapter 2
MAMMALS

Chapter Contents

ANOMALOUS DISTRIBUTION IN SPACE AND TIME

UNRECOGNIZED SPECIES

MORPHOLOGICAL PHENOMENA

• Hair Blanching and Similar Anomalies

BLANCHING OF A BAY HORSE

Anonymous; *Nature*, 129:683, 1932.

In the Field for April 16, p. 582, Miss J. McAlpine gives an account of a bay horse the mane of which, black as usual in this colour, turned nearly white owing to a severe fright it received at six years of age when out at grass in a very long field. An aviator, in trying to land here, drove the horse the whole length of the field, and nearly alighted on it. No one saw the horse for three days afterwards, but it was then found to have lost the colour of its mane, as described, while the tail had also become quite grey. A photograph of the animal, now nine years old, accompanies the note, and shows the pale mane very plainly, so that the effect of the shock seems to have been permanent. The writer of this note once casually saw in a London street a bay pony with a grey mane and also an angular grey patch let in, as it were, on the brown short hair of the neck, but put it down to a freak of variation. The horse is more liable to variation in pelage then any other domestic mammal, and another bay, an aged van-horse, also seen casually in London, was spotted with white over the brown parts as clearly as any deer, but with smaller, more angular spots.

✂THE GREENING OF POLAR BEARS IN ZOOS

Lewin, Ralph A., and Robinson, Phillip T.; *Nature*, 278:445-447, 1979.

Polar bears (Thalarctos maritimus) normally have creamy-white fur, presumably an adaptation for camouflage in a snowy environment. However, during the summer of 1978, the fur on the back and sides of three adults in the San Diego Zoo turned green though the animals remained otherwise healthy. (Of these bears, one female was born in the zoo in Calgary, Alberta, Canada, in 1966 and was transferred to the San Diego Zoo in 1969; a second female was born in the wild, having been caught at Spitzbergen in 1951; the third, a male, was born in the San Diego Zoo in

1970.) This phenomenon, though less marked, has been noted in several previous summers, both here and in zoos elsewhere, for example, in Cologne, Germany (C. Hill, personal communication). The coloration was particularly evident on the flanks, on the outer fur of the legs and in a band across the rump: fur on the head and belly and inner sides of the legs was white. We first supposed that the colour was due to green algae such as Chlorella or Scenedesmus on the surfaces of hairs, growth of such algae being promoted by the presence of nitrogenous wastes in the waters of the bears' pool. (The pool in the exhibit area, which contains 12,500 gallons of tap water, is drained and cleaned twice weekly.) However, microscopic examination of samples of hair taken from the three San Diego bears and from a similarly green polar bear in the zoo at Fresno, California, revealed that this was not so. The outer surfaces of the hairs appeared clean and smooth, except for the normal squamation. The coloration was clearly attributable to the presence of algae inside the hairs, specifically in the hollow medullae of many of the wider (50-200 μm), stiffer guard hairs of the outer coat. (The thinner (<20 μm) and more undulant hairs of the under coat, which were not hollow, were colourless.) Some of the lumina were apparently filled with air, but many of these hollow spaces were partly occupied by masses of small greenish cells, which we describe here. (p. 445)

• Luminosity, Phosphorescence, Etc.

WILL O' THE WISP

Brennan, Patrick; *Scientific American,* 116:261, 1917.

The large black, the black and white, and the hydrophobia skunks are phosphorescent in darkness, giving off a continuous blue flame, the head being of a more fiery red, then tapering off into blue to tail tip. This fact may answer in a degree for the blue marsh gas flame, the jack o' lantern and ma lantern (of the southern states), and the night ghost lights of all states. I make this suggestion from my experience in trapping in deserted mine tunnels in this state.

• Sports

A REVERSED CAT

Wragg, Helen A.; *Science*, 88:475, 1938.

An adult female cat purchased for student use in comparative anatomy was found, upon dissection, to have its internal organs completely reversed in every detail studied. Lungs, kidneys, veins and arteries and all parts of the digestive tract were normal in size and shape but so situated that descriptions for the left side fitted the right perfectly and vice versa. The aortic loop arose from the larger right ventricle and arched to the right. Other parts of the heart and its vessels were changed accordingly. The animal, although heavily infested with tapeworms and undernourished, appeared sound and normal in every other respect. No reference to an entirely reversed cat has been found in the literature. It may have been one of a pair of identical twins, since it is supposed that the occurrence of the phenomenon of reversal in man and other mammals is due to splitting of the embryo at some early stage.

MIRROR IMAGE ORGANS DISCOVERED IN A CAT

Anonymous; *Science News Letter,* 64:215, 1953.

Discovery of a cat with mirror image internal organs was announced by Dr. Thomas D. Bair of Utica College of Syracuse University in Science (Sept. 18).

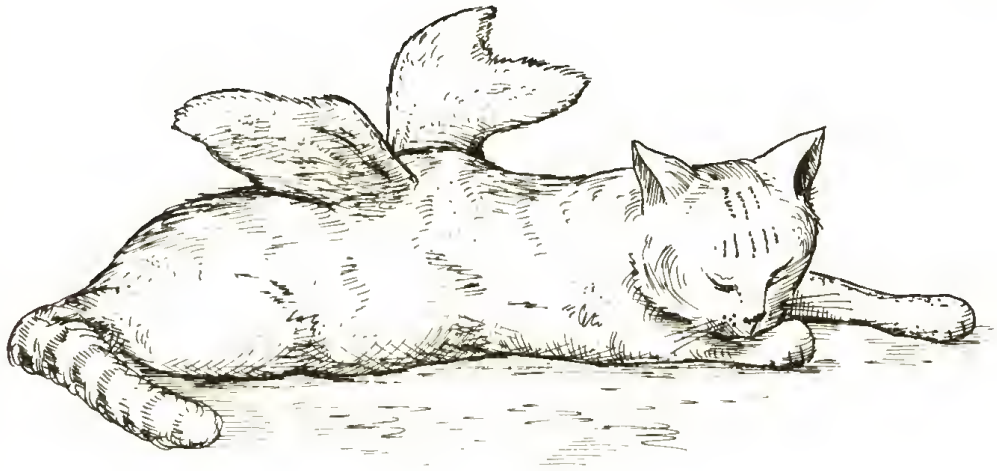
Complete reversal of all organs is so rare that Dr. Bair has only seen it this once in about six years' experience in dissecting cats in biology classes, and has seen only one other such case reported.

The kidneys seemed to be the only organs in a normal position in the Utica mirror image cat.

CAN A CAT FLY?

Anonymous; *Strand Magazine,* 18:599, 1899.

This sounds very much like a conundrum, and a very absurd one, too. If we look at the picture of pussy which is reproduced



here, our question is at least partially answered. This extraordinary cat, it will be perceived, is the proud possessor of a pair of wings. It belongs to a lady of Wiveliscombe, Somerset, to whom it was given when a kitten. There was nothing extraordinary in its appearance at first, but after a time it developed a pair of wings. They are not, of course, covered with feathers, but fur--the same as the rest of the body. Several persons have seen this wonderful cat.

SOME CASES OF SOLID-HOOFED HOGS AND TWO-TOED HORSES

Auld, R. C.; *American Naturalist*, 23:447-449, 1889.

In 1878 "soliped" pigs were reported from Texas. Dr. Coues observed that in the new breed the terminal phalanges of the toes were united, to form a single broad phalange; above this, however, the other two phalanges remained perfectly distinct. The hoof is perfectly solid, and on its sole there was a broad, angular elevation of horny substance, curiously like the frog of the horse's hoof. The breed was so firmly established that no tendency to revert to the original and normal form was then observable. It was further stated that, in the cross of a solid-hoofed boar with a sow of the ordinary type, a majority of the litter has the peculiarity of the sire apparent.

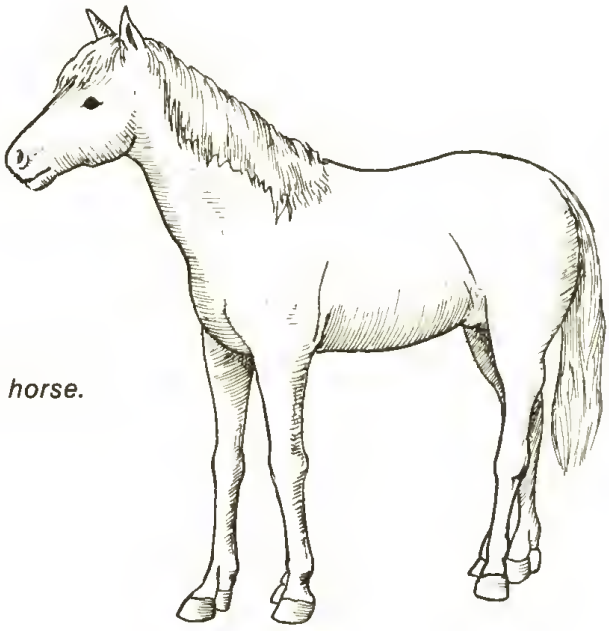
There has just been reported to me from Sioux City, Iowa, (famous for its annual "corn palace") a similar case. Indeed, it would seem as if the owner was quite alive to their rarity and had been breeding them for some time, and had now as many as induced him to advertise them for sale, "not alone for being a curiosity, but in a commercial sense a valuable production for mankind!"

The owner continues: "The experience of the writer convinces him that there is no better hog for the healthy growth of pork. These hogs are of long body, and have well proportioned hams and shoulders. It is true they have not the fine head of the 'improved' breeds. . .In size they are fair, a couple of barrows (accidentally castrated) now near thirteen months old, without special care weigh over 350 pounds each. As yet there has been no sign of any loss from disease whatever (though diseases have been common in that district for years). A few boars, six to eight weeks old, will be sold . . . ." etc.

We are making further inquiries into the above, and will report results.

But it seems quite evident that these "mule-footed" hogs are of frequent occurrence in America. Some "get into print," and some don't. For instance we are obliged to the Rural New Yorker for two more cases. A known correspondent to that excellent periodical writes thus, from Cottonville, Louisiana, in the issue for September 22d: "As a curiosity which I never saw before, or even heard of, I send the foot of a 'mule-footed' hog. There is a herd of them ranging the woods, about eight miles north of Baton Rouge. None of the old settlers can give me any further information concerning them than "that they are a herd of wild hogs.'" An exact drawing is published with the above, which is enclosed for your reproduction. The editor adds a note to the above: "We have seen several of these 'mule-footed' hogs. In a small Southern town, a large Poland-China boar had one hind foot exactly like the one shown in our picture, and a large proportion of the young pigs from him were marked in the same way."

We have also had undoubted cases of extra-toed horses reported here. During the summer of 1885, The Advertiser,



An "eight-footed" Cuban horse.

Constantine, this State, contained the following: "On Wednesday night of last week a mare belonging to Mr. Fred Hagenbuch, of Fabius, gave birth to a male colt, well formed and perfectly symmetrical in all respects, except that one of the feet is cloven and hoofed like the foot of a cow. Who has a mate for this colt?" This was quoted in Breeder's Gazette, Chicago, the leading breeder's paper of America, and brought out a response from Mr. N. C. Woolf, in issue for July 16th, thus: "My neighbor, Mr. D. M. Hall, has a two-year-old colt that exactly fills the above description. For a few months Mr. Hall has taken great pains in shoeing, and thinks he will succeed in making a pretty good hoof."

These cases are, I think, of sufficient interest to entitle them to be rescued from the oblivion that they must experience. And they are, I think, of sufficient value to have a place accorded them in The Naturalist.

• Unexpected Hybrids

COON CATS

Baskett, J. N.; *Science,* 22:220, 1893.

Speaking of cats, I saw, in a private house in Chicago recently, two cats which the owners called "coon cats." They had been obtained in the edge of the forest around Moosehead Lake, and it was claimed that they were hybrids, or descendants of hybrids of the domestic cat and the raccoon. They were larger than the ordinary house cat, had very coon-like countenances and bushy coon-like tails that were always expanded. One had the habit of ascending something high and resting stretched out, and their motions when in a little hurry were a coon-like gallop.

The claws were retractile, the foot digitigrade. I did not examine the dentition, but could find nothing but appearance that indicated a coon kinship. They interbred with the common cat. Can some one tell me more about them?

COON CATS

Howard, L. O., and Gibbs, Morris; *Science,* 22:279, 1893.

Seeing Mr. J. N. Baskett's note on page 220 of the current

volume of Science, concerning coon cats, I venture to inform you that I was struck with the extraordinary appearance of one of these cats owned by Mr. Will Carleton, who had it with him in the Catskill Mountains the present summer. I asked him about the cat and he told me the same fable which Mr. Baskett relates, but he went on to say that of course the story was incorrect and that in his opinion this peculiar race of cats from Maine is descended from some Perisian or Angora breed brought down to Maine by early French settlers from Canada. I believe that this was surmise on Mr. Carleton's part, but it seemed reasonable to me and if you receive no more satisfactory explanation in reply to Mr. Baskett's question, you are at liberty to use this. (L. O. Howard.)

In answer to Mr. J. N. Baskett's question regarding "Coon-Cats" in your issue of Oct. 20, 1893, I would say that this cross-breed of animals has been known for many years, more particularly in the State of Maine. The error attributing these mongrels to a cross between our domestic feline, and the racoon, Procyon lotor, is as general as it is ridiculous; for it stands to reason that animals of different families could not interbreed. The notion is about as ridiculous as a prevalent story among the ignorant that (cat) owls bear their young alive.

The subject of "coon-cats," or sometimes called mule-cats, has been repeatedly discussed in many papers, and it is now generally conceded that this hybrid is the result of an alliance of our domestic tabby with some Oriental feline---probably the Angora. This cross would show the long, bushy tail of the Oriental species. But Mr. Baskett is in error in supposing these animals plantigrade, and if he secures a skull, which he can easily do, he will find that the dentition is pronouncedly feline.

These cats are quite common in parts of New England, and may be purchased at a very reasonable figure, and according to the demands and the supply in the cat market. Few persons are able to distinguish between genuine Angoras and these hybrids, and many are the unsuspecting buyers who have paid a high price for a common "coon-cat" worth not more than two dollars. (Morris Gibbs)

HYBRID APE OFFSPRING OF A MATING OF GIBBON AND SIAMANG

Myers, Richard H., and Shafer, David A.; *Science*, 205:308-310, 1979.

Abstract. The serendipitous mating of a male gibbon, Hylobates moloch, and a female siamang, Symphalangus syndactylus, has produced two female offspring born 1 year apart. The hybrid karyotype of 47 chromosomes comprise the haploid complements of the parental species, 22 for the gibbon and 25 for the siamang. Chromosomal G and C banding comparisons revealed no clear homologies between the parental karyotypes except for the

single chromosome in each species containing the nucleolus organizer region. The lack of homology suggests that the structural rearrangement of chromosomes has played a major role in the process of speciation for these lesser apes.

• Convergence of External Appearance

IS THE AARDWOLF A MIMIC OF THE HYAENA?

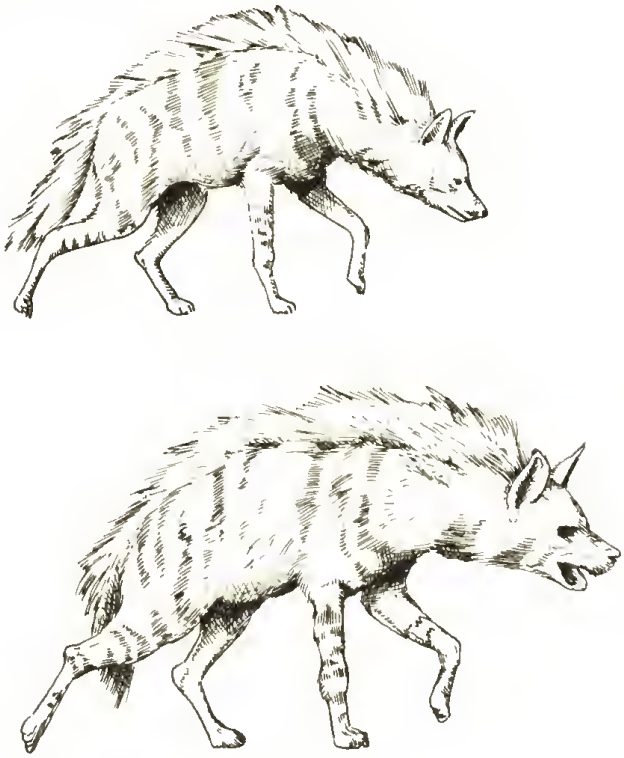
Gingerich, Philip D.; *Nature,* 253:191-192, 1975.

The genus Hyaena is represented in Africa today by two species: the striped hyaena, H. hyaena, inhabiting northern Africa; and the brown hyaena, H. brunnea, limited to southern Africa. Hyaenas (the spotted hyaena, Crocuta, excluded) weigh 50-60 kg, and inhabit open dry plains and thorn scrub, live singly or in pairs, and are chiefly nocturnal. Hyaena has a sloping back, pointed ears, and an erectile mane. It has strong teeth and jaws and it is known to kill live game occasionally, although it is primarily a scavenger.

The aardwolf Proteles cristatus is a much smaller, jackel-sized (12-15 kg) mammal with a discontinuous distribution: a northern population is found throughout most of East Africa, and a separate population inhabits southern Africa. Proteles, like Hyaena, has a sloping back, pointed ears, and a well developed erectile mane along the dorsal spine from neck to tail. This mane is composed of stiff hairs some 20 cm in length and, when erected, makes the aardwolf appear considerably larger than it actually is. Proteles inhabits open plains and thorn scrub, lives singly or in pairs, and is chiefly nocturnal. It has only vestigial cheek teeth and feeds largely on termites and other insects, and occasionally on carrion and rodents.

. . . . .

The external similarity of Proteles to Hyaena extends also to most of the characters of internal anatomy that have been studied, indicating that Proteles is closely related to the hyaenas. The chromosomes and haemoglobin mobility of Proteles are virtually identical to those of Hyaena and Crocuta, and the gyri and sulci of the cerebral hemispheres of the brain are arranged on exactly the same plan. The complete dental formula of Proteles, though not always fully developed, is the same as that of Hyaena and Corcuta. The male reproductive tract of Proteles appears to be more similar to that of Hyaena than to that of Corcuta. Proteles differs from Hyaena principally in having a dentition much reduced



The aardwolf (top) and the hyena (bottom) closely resemble each other.

in size, and in retaining the pollex (a digit lost in both Hyaena and Corcuta).

. . . . .

The nearly complete geographic sympatry, close external resemblance, and similar behaviour of Proteles and Hyaena are unusual in closely related forms, since speciation normally involves significant divergence in geographic distribution, in external appearance, or in behaviour. All of the conditions of Batesian mimicry are met by Proteles, and this may help to explain its distribution, appearance, and behaviour. Only the predator remains to be identified.

Leopards occur throughout the range of Proteles, they are most active at night, and they are predominantly visually oriented predators. Leopards routinely prey on jackals and, in view of their wide prey tolerance, might be expected to prey on Proteles as well. Because of its large size and strong skull, a Hyaena would be dangerous to an attacking leopard. The much smaller, weaker Proteles would be a poor match for a leopard, and it seems that an important component of the aardwolf's defence against predators may be its close external resemblance to the larger, more danger-

ous Hyaena. The ability of Proteles to erect its mane when excited would further confuse a potential predator as to its actual size and identity.

This possible case of mimicry in a large mammal is unique, and deserves further investigation. Unfortunately, low population densities and nocturnal activity patterns make both Proteles and Hyaena difficult to study in the field. Additional study of their behaviour and ecology will be of great interest, and observation of any interactions with leopards of particular importance.

A MARSUPIAL RHINOCEROS

Anonymous; *Scientific American,* 123:301, 1920.

As the result of an examination of a nearly complete skeleton of the extinct marsupial Nototherium Mitchelli, hitherto known only from fragmentary remains, found near Smithton (Tasmania) this year, Messrs. H.H. Scott and Clive Lord have come to the conclusion that the Nototherium was a marsupial rhinoceros. The structure and shape of the horn are matters for conjecture as no remains of it have been found but the specially constructed cervical vertebrae, the structure of the nasal regions and the curious nasal cartilage--all point to the conclusion that the animal possessed a rhinoceros-like horn. If so it is a curious case of parallel evolution for the true rhinoceros belongs to a different and "higher" group than the marsupials, none of which at the present day possesses anything remotely resembling horns, nasal or otherwise.

THE LION THAT USED TO LIVE DOWN UNDER

Anonymous; *New Scientist,* 36:209, 1967.

The marsupials, that is to say the non-placental, breach-bearing animals of Australasia are such a mixed-up assemblage of what superficially resemble cats, dogs, rats, mice, moles, shrews, squirrels, and bears that the discovery of anything new doesn't always get the attention it deserves. From Canberra House in London last week came a brief announcement about the unearthing of a marsupial lion. This is a fossil creature which has been given the name of Thylacoleo, "a big powerful animal with enormous teeth and ferocious appearance." Nobody could answer inquiries about where it was found or when the king of the antipodean marsupials died out. Until the discovery is formally published it can be regarded as a large Dasyurid. How does it fit into the accepted orders of animals?

The answer would seem to lie in David Ride's recent classifica-

tion of the marsupalia. In this the Australian native cats, tiger-cats, marsupial mice and rats, the so-called Tasmanian devil (Sarcophilus), and wolf (Thylacinus) are listed as Marsupicarnivora, the flesh-eating marsupials, together with a few New World species such as the woolly opossum, the Chilean kongoy or Monito del monte (the mountain "monkey"), the Patagonian opossum (Lestodelphys), the yapok or water opossum and a few others.

In many marsupials the hind limbs are larger than the forelimbs and supply the main motive force. The dental system, particularly the relationship of milk and permanent teeth, is not fully understood. The total number of teeth is usually 40 to 50, but it is as low as 22 in the long-snouted phalanger, Tarsipes, and as high as 52 in the banded ant-eater, Myrmecobius.

. . . . .

JAWBONE OF UNKNOWN BEAST DISPUTES DARWIN'S THEORY

Anonymous; *Science News Letter,* 30:350, 1936.

Darwin's natural selection theory got severely bitten by a 45 million year old jawbone at the meeting of the National Academy of Sciences.

The jawbone once belonged to an unknown beast of prey that roamed the American West in Late Eocene times, quite early in the age of mammals. Dr. William Berryman Scott, noted paleontologist of Princeton University, told of the fossil and explained its significance.

The fossil, a lower jawbone, was sent to Dr. Scott for examination by the Carnegie Museum in Pittsburgh. At first it seemed to be a new species of sabertooth cat, though that in itself was surprising, for Eocene is much too early for such animals. But closer examination, especially of the teeth, showed that it was an entirely different kind of a beast, which Dr. Scott termed a "most amazing imitation of a sabertooth." Once before, an imitation sabertooth of still another kind of animal had been found in South America.

This repetition of the sabertooth anatomy and way of life in three widely different kinds of animals, Dr. Scott explained, constitutes a striking case of what scientists call convergent evolution. The probabilities are almost nil that such near identity could take place on a basis of purely chance variations, as is postulated by the natural selection theory of Darwin.

• Remarkable Adaptations

EVOLUTION OF THE GIRAFFE

Brownlee, A.; *Nature*, 200:1022, 1963.

The mode of evolution in the giraffe has been a favourite subject for discussion. Darwin wrote "...it has been supposed that all the parts must have been simultaneously modified; and it has been argued that, on the principle of natural selection, this is scarcely possible...but it cannot be denied that an animal might have its neck, or head, or tongue, or forelimbs elongated a very little without any corresponding modification in other parts of the body". Graham Cannon, very properly I consider, rejects this, concluding: "It is this idea of co-ordinated variation that is, to my mind, the central core of the whole problem of evolution".

Doubts on the extent of the survival value conferred by ability to reach high twigs arise when it is considered, as Graham Cannon (p. 139) pointed out, that the male giraffe is some 2 ft. taller than the female and thus be natural selection the females would tend, disastrously, to be eliminated in times of drought; Himmelfarb similarly objects that the young would tend to be elimination likewise.

Schreider discusses the value, to certain human races living in hot climates, of dolichomorphism as an aid in achieving heat loss. So also to the giraffes, living as they do in hot climates, their dolichomorphic structure will serve a similar purpose to young or old, male or female continuously and not merely in times of drought, while at the same time enabling them to achieve that size and tallness which confers greater ability to evade, or defend against, predators and to reach a source of food otherwise unavailable to them. Which factor has been of greatest evolutionary value is uncertain. Darwin wrote in this connexion: "The preservation of each species can rarely be determined by any one advantage, but by the union of all, great and small".

To the best of my knowledge and belief, no one has ever suggested that dolichomorphic races of man evolved by elongation of first one part then another, and evolution by pattern mutation is very much the more probable.

UNANSWERED QUESTIONS

Hayward, A. T. J.; *Nature*, 243:367, 1973.

One of your correspondents asks those who, like myself, have

referred to "flaws" and "unanswered questions" in evolutionary thought if they can supply examples. We certainly can---many of them. For space reasons I will quote only three.

Only four years ago a Darwinist grudgingly admitted that "Though it is nearly a century (sic) since Darwin wrote his treatise On the Origin of Species, there are still a few weak points in the theory of evolution. Often evolution seems to have made huge jumps, leaving no traces of any intervening steps and no hint that anything but the complete system could have functioned at all.

It is surely incompatible with the scientific method of pretend that evolution is a proven fact, with these "huge jumps" unexplained. How did the whale evolve a complex nipple that enables it to suckle its young under water? This complex organ would have been worse than useless until it was fully developed, or Whale Junior would have received a mouthful of seawater instead of milk.

How did the giraffe evolve a long neck without first evolving the complex hydraulic control system that maintains an acceptable blood pressure in his brain whilst he lowers and raises his head? Or did he evolve his unique mechanism for regulating cerebral blood pressure before he evolved his long neck---and if so, how could such motiveless evolution have occurred?

Anadromous fish, grey whales and some species of migrating birds share an ability to find their way to an exact spot thousands of miles away. It is doubtful whether a submarine could equal their performance without the aid of shore-based radio beacons, even using today's highly sophisticated computer-based navigational systems. How did creatures so widely separated in the evolutionary tree as birds, fishes and mammals, all evolve these extraordinary navigational mechanisms? The problem is compounded by the experimental evidence that the spatial distribution of various elements in the oceans, the Earth's magnetic field, and the position of the stars all play a part in the overall phenomenon. Yet over geologic time these three parameters have been varying continuously; how, then, did these creatures evolve the ability to navigate against a moving datum?

. . . . .

• Supposed Transmission of Acquired Characteristics

THE INHERITANCE OF AN ACQUIRED CHARACTER

Hills, Norman E.; *American Naturalist,* 30:755, 1896.

It has been my fortune recently to have brought to my notice an instance illustrating Darwin's theory of the origin of species,

that seems to me noteworthy.

A certain Mr. J.B. Perry, a resident of Cleveland, is the owner of a very fine female fox-terrier, which has recently given birth to a litter of seven puppies, five of whom are remarkable from the fact that they were born with short tails.

These five were male puppies, while the two with tails of ordinary length are female.

Of the five short-tailed dogs one has almost no tail at all, it being but a little stump not over half an inch long.

When I examined them they were just two weeks old and barely had their eyes open. The tails of the females had been recently cut, and the scar on the stump was plainly perceptible, while the ends of the five short tails were entirely grown over with hair, and plainly were born in the condition I found them. Their length was about half the ordinary length, or about what is considered the "proper thing" by dog-fanciers, except in the case of the one already mentioned as having almost no tail at all.

When it is remembered that the custom of cutting off over half of the caudal appendage of the fox-terrior has prevailed for many generations back in the ancestry of a thoroughbred, the birth of short-tailed dogs is not to be wondered at. Yet this instance is so striking that it seems worthy of being brought to the notice of the readers of the American Naturalist.

INHERITANCE OF ARTIFICIAL MUTILATIONS

Wade, W.; *American Naturalist,* 30:837-839, 1896.

The instance cited by Mr. Norman E. Hills (in the September Naturalist) of the birth of short-tailed fox terriers, is striking in showing a larger proportion of deformed puppies than is common in such cases, but instances like the one cited are frequently noted in the press devoted to dogs, and concerning several breeds that have been mutilated for generations.

But to thoroughly consider the matter of the inheritance of artificial deformities, the cases of breeds in which the deformity is usually natural, should be considered. I believe the tailless Manx cats generally come in that shape, and this I know is often the case with the bobtail sheep-dog of England, and this is stated of several breeds of dogs, generally of the type of the bobtail sheepdog found in other countries, Norway, Southern Russia and elsewhere.

But the peculiar feature of this inheritance is its freakishness. Two naturally long-tailed parents have produced a tailless dog, in whom the potency was so strong that no bitch, no matter what the breed was, ever produced a full, natural tailed puppy to him. I remember of one puppy by this dog, ex his double grandma (he was the son of litter brother and sister) whose tail was of usual length, but had a deformity in it as though it were tied in a knot. Again, it is not at all uncommon for a bitch to begin production

with all naturally long-tailed puppies, and after some years, change to always producing some tailless ones, even when mated with mongrel dogs, while some bitches reverse the order, beginning with tailless ones and changing to full-tailed ones. I recently noted a reported instance of just such a change of production in a Manx cat, and it seems to me that this freakishness introduces a very disturbing element into consideration of the question of inheritance.

As an allied matter, please permit me to say that the notion that if a bitch has a mongrel litter she will thereafter always produce puppies showing traces of the unallied sire, is rank rubbish, and on the point that this occurrence is not invariable, any experienced breeder will concur, as very, very few such breeders have ever seen such a case. For myself, I have bred dogs for over forty years, have bred many mongrel litters, and never saw a case of telegony--or, as we commonly call it, "influence of previous sire." That this influence does sometimes show itself, is beyond doubt; but some very extended inquiries of mine some years since, showed that it was shown only in about one percent of cases of mesaliances; and when it was considered that an instance of this "influence" will be remembered from its extraordinary character, while instances where it does not occur are forgotten, being the expected result, I believe that the one-tenth of one percent of cases will be found to be the extent of its occurrence. It is very strange that those scientific men who uphold the idea of the invariable occurrence of this "influence", all overlook the potent fact that the "influence" shows itself invariably only in the skin and hair, never affecting conformation. In view of this, the theory propounded by Dr. Jonathan Hutchinson, President Royal College of Surgeons; Dr. J. Sidney Turner, President South British Medical Society, and Everett Millais, Esq. seems sound, and bears against the idea of the bitch being herself bastardized, that theory being that unripe ova are partially impregnated by the spermatozoa of the foreign male, not sufficiently to fully vivify them, the influence of this impregnation affecting only the epiblast, from which the skin is evolved, and a subsequent fertilization brings full life to the ova, determining other features of the foetus. Thus, in the case of a pug bitch, which had a mongrel litter by a Skye terrior, and at her subsequent whelping by a pug dog, had some puppies with rough hair, these "influenced" puppies had the conformation of the pug all over, even to the twisted tail.

However, be the scientific part of the question what it may, the too common idea that a bitch having a mongrel litter will show influence of that litter in all future offspring, is utterly fallacious.

CRITICISM OF SOME CASES OF APPARENT TRANSMISSION OF MUTILATIONS

vom Rath, O.; *American Naturalist,* 28:1-14, 1894.

Whenever a discussion arises as to whether characters acquired in the life of the individual are transmitted, first of all is ordinarily put the special question[1] whether the transmission of mutilations may be admitted. In various papers Weismann has shown that the previously known cases of alleged transmission of mutilations do not hold their ground before careful criticism, and are far from serving as indisputable proof. In the judgment of such cases so much greater care is necessary, since it is often very difficult to decide whether the abnormality present in the paternal or maternal individual was actually caused by some external interference, or arose as an inborn (blastogenic) variation of the germ. Of those authors who deny the transmission of acquired characters, the pathologist, E. Ziegler, approaches most closely to the Weismannian point of view, and his valuable papers in which he discusses the most recent articles on inheritance and the doctrine of descent and their importance for pathology, form important supplements to the writings of Weismann. Among other things, E. Ziegler emphasizes the fact "that pathological characters acquired in the life of the individual are not transmitted, and that the first origin of transmissable diseases and mutilations is not to be sought in the acquirement of corresponding changes during the life of one of the parents, but in variations of the germ."

It is by no means my intention to go into the extensive literature on this point. The purpose of my article is rather to communicate some interesting cases of apparent transmission of mutilations, which I learned of by personal experience, and was able to test carefully. If even now some of these cases do not permit a definite decision, still it may not be useless to recount them here, since, by just such examples which, at first glance, let no doubt of the fact of such transmission appear, it can best be shown with what extreme care an unpartisan observer must prove the true character of the case, and pass judgment upon it.

The facts of the first case are as follows: In a family closely related to myself was kept a pair of dogs (terriers), faultless in every particular; both dog and slut were known to have had fully normal parents, and on their part to have produced, in several litters, normal young. By an unfortunate fall, the dog suffered a break in the upper part of the right humerus, as a result of which, even today, there remains a peculiar posture of the damaged extremity, connected with continued limping. In the next litter, which followed some time after the complete recovery of the father, were three young, two dogs and a slut. The fully normal young slut died soon after birth. Shortly thereafter, the mother came to her end also. Of the two young dogs, the one was in every respect normally formed, and in color and form the true likeness of the mother, whereas the other dog not only

resembled exactly the father, but also like him possessed an abnormally placed right fore leg, and continually limped with this leg from birth up to today, when the animal has long since reached full growth. At the sight of the dog, all eye-witnesses were completely convinced of the fact of the transmission of a former injury.

As will be easily understood, my attention was at once directed to establishing whether the peculiarities in question really corresponded exactly in father and son. To begin with, I found that after the fall, the right fore-leg of the father was essentially different from the left, and had remained so constantly, and that the animal continually limped on this leg, and always in the same way. A certain weakness and great tenderness is noticeable even today in the entire shoulder region, and especially at the spot at which the injury took place, the entire musculature on the humerus is also strikingly degenerate. The position of the injured leg (especially from elbow joint down) differs in a curious way from that of the uninjured left leg. The entire extremity has a fully crippled appearance. The apparently shortened forearm and foot of the right foreleg assume unmistakably a bow-legged position, and the entire extremity is strikingly bent inward.

The investigation of the limping young dog showed the following: In spite of careful feeling I could find on the right foreleg neither a sensitive place nor an abnormity of humerus or of the musculature; on the contrary, the right foreleg is externally completely similar to the left, but unquestionably different in posture and shape from the latter. While now in the father the right leg is "O"-shaped (bow-legged), and the foot is turned inward, the corresponding leg of the young dog shows exactly the reverse tendency in posture. It is rather "X"-shaped ("knock-kneed") and the foot is turned outward, but by no means so much as the corresponding foot of the father turns inward. The difference in the posture of the leg does not strike one so much when the two young dogs are observed together as when the father and the limping one are compared along side of each other. In the usually very active movement of the animals, moreover, the difference in the posture of the legs does not become so evident as when the animals move slowly or stand still.

In judging this case it is to be noted, first of all, that the abnormality of the young dog which might be regarded as inherited (transmitted), does not agree in many particulars, especially in regard to the posture of the leg, with the acquired deformity of the father. There is, as it appears to me, a double interpretation of the case possible. Either one may assume that the abnormality of the young dog has appeared without any inheritance as a germ variation, which is not further traceable to its causes, and that a case of transmission has only apparently arisen, since by chance the paternal animal showed an acquired abnormality of the same leg as that on which, in the young animal, an abnormality arose by variation; or, on the other hand, one may regard the acquired abnormality of the paternal animal as the cause of the congenital abnormality of the young dog; in the latter event, it must be carefully noticed that the inherited

peculiarity is very little similar to the original. Hence, there would be present only a certain influence, but not such a transmission as we perceived in the case of individual variations (blastogenic changes) in which the transmitted peculiarity differs, perhaps, in degree from the transmitting, but is always like it.

As far as the limping of the two dogs is concerned, I do not think that further significance can be attributed to this circumstance. To be sure both dogs limp on the same leg--the father always alike, the son sometimes more and sometimes less and often scarcely noticeably; thereby is by no means proved that the same cause lies at the bottom of the limping of both animals. As is well-known, quadrupeds, and especially dogs and horses, limp in consequence of the most varied causes, and it is often very difficult to find the real ground of the limping. In the case described, the limp of the father is evidently the result of the fall. I was as little successful as other investigators in finding the true reason of the trouble in the son, since nowhere on the entire body was a tender spot to be discovered.

The following case is so simple and characteristic, that no doubt can exist as to its interpretation. A Mr. S., a perfectly normal and well-proportioned man, had from youth up, the habit of turning the tip of his right foot outward more than that of the left, a circumstance which was especially apparent in dancing, and also showed prominently in foot-prints left in the snow or on moist ground. This peculiarity all his children (three sons) have inherited, only with the difference that in one of them, besides the right, the left foot also is turned out in a like striking manner. Since now the father of Mr. S., as a young man, acquired, in consequence of an apoplectic fit, apparent lameness of the right leg, as a result of which this leg was dragged behind, with the foot strikingly turned outward, one concluded that the peculiarity of the out-turned foot inherited from the older Mr. S. (i.e., a somatogenic character) had been transmitted to his son and in still stronger measure to his grandchildren. As I stand in close connection with the family concerned, it was easy for me to acquire the necessary information, and I was able to establish the fact that the younger Mr. S. was already several years old, when his father suffered the stroke, and further, that the elder Mr. S., from youth up, had complained of a certain weakness in the right leg, and that an important deterioration in the condition of the entire leg appeared directly after the stroke. If now one wishes to bring the out-turned foot of the younger Mr. S. in connection with the infirmity of his father, a thing which, according to my view, is not at all necessary, then it can be regarded as an inherited peculiarity in both the father and the son, i.e., a blastogenic, but by no means a somatogenic character. Such habits of peculiar postures of the foot not infrequently appear suddenly and without visible cause in some person or other, without a similar case having been known in the family of the same person. I also know a man who, from youth up, had the habit of continually turning the right foot in a striking manner inward, so that it was jocosely said of him he had two left feet; but neither in parents, brothers, children, nor other relatives

of the man, has ever been noticed any special inclination to a striking position of the foot.

The third case which I shall now report came to my knowledge as in my own family, in consequence of the instance just described, the question of the transmission of mutilations was actively discussed. In that connection, this case was held up to me as an absolutely trustworthy proof of the possibility, yes, of the fact of such a transmission. The case is the more interesting since it is concerned with the apparent transmission of a scar ["Schmiss"--the term applied to the duelling scars of the German students.-- Trans.].

A Mr. H. had received, as a student, a serious saber cut, extending vertically along his right cheek, and had retained, during his entire life, the conspicuous scar. Since one of the children of the gentleman, a daughter, brought into the world, exactly on the same spot of the right cheek, a birth-mark in form on a fine red slash, the length of the father's scar, no one hesitated to bring this birth-mark into genetic connection with the cut of the father; and since, in addition, among the give children of this lady, one son also possessed from birth an equally long birth-mark, exactly on the same spot as that of his mother, no one doubted an instant that the scar of the grandfather, an acquired (somatogenic) character had been transmitted to the daughter and the grandchild. Now so convincing as indeed this case appears at first glance, it is yet very far removed from furnishing actually undisputable proof of the transmission of mutilations.

In the first place, I should not neglect to mention that I have known the family in question for many years, without the peculiar inherited birth-mark of the lady or her son ever having attracted my attention, until I was called to notice, and could confirm its actual existence. With the lady, as with her son, the characteristic family mark had been very prominent in the early years of life, it then faded gradually, without disappearing completely, however. The elder Mrs. H., grandmother of the young man, is still living, and, according to her own account, has never possessed such a birth-mark on her right cheek; at present, every trace of one is certainly lacking so that one is inclined to think of a transmission from the side of the grandfather, the elder Mr. H. Unfortunately, this gentleman died many years ago, and it was on that account impossible for me to determine whether he also did not possess from his birth, such a mark on his right cheek, the existence of which was gradually forgotten[2], especially, as on this cheek, the large scar and a number of smaller cuts were added.

Besides this possibility, there ought not to be left out of consideration the fact that not infrequently peculiar birth-marks are brought into the world by children without the same or similar marks ever having been observed in the family in question or among relatives. That sometime such a mark could appear in some child exactly on the spot on which the father had had a cut, is, of itself, nothing surprising, or indeed strange. In a like sense Weismann has already expressed himself, before, indeed, such a case of apparent transmission of a scar had come to light. "I,

indeed, do not wish to doubt," says Weismann, "that among the many thousand students whose faces are adorned by so-called cuts, one could be found sometime whose son has a birth-mark on the precise place on which the scar of the father is found. There exist many kinds of birth-marks; why not sometime, then, one exactly on the position and exactly in the form of a scar. Then we would have here a case such as the adherents of the doctrine of the transmission of acquired characters have long wished, a case of which they think that it alone would suffice to overthrow the entire structure of their opponents. But how far, then, would such a case if it were really authenticated, be more in position to establish the kind of transmission asserted than the case related by v. Baer, the claim of Versehen[3]? I think there lies in the very extraordinary rarity of such cases, a strong indication that there is concerned an accidental, not a casual, occurrence. If cuts could really be transmitted, we should have to expect to meet very often such birth-marks corresponding to the paternal scar, in nearly all such cases, namely, in which the son inherited the features of the father."

From the preceding description we have seen that the apparently so convincing cases of the transmission of former injuries, which I have described, surely do not speak in favor of this theory, which requires nothing less than unimpeachable proof; they accord in part with the cases discussed by Weissman, in which direct proof could be furnished that the peculiarities in question do not at all correspond in the child and in the father or mother (parent, Weismann), and stand in no genetic connection.

The arguments which oppose the acceptance of the theory of the inheritance of mutilations, have been discussed so much in detail, especially by Weismann and Ziegler, that I shall not refer further to them here. The objections raised have by no means grown weaker of late; they have rather been considerably strengthened by new investigations which give us a deeper insight into the character and processes of fertilization (Weismann's Amphimixis). If now there is no doubt that in the acceptance of the transmission of mutilations, and of the other peculiarities acquired in the life of the individual, the phenomena of the theory of descent find a convenient and simple explanation, this condition by no means authorizes us to an unconditional acceptance of this supposition, since, as Weismann has shown, all pheonomena of the theory of descent may also be explained just as simply and unconstrainedly without the aid of the Lamarckian principle.

Of special importance for the decision of the question at issue are Weismann's much discussed experiments on mice. As is known, the artificial mutilations of these animals were carried on in both parents through many generations, without any apparent success. Similar recently published experiments of Ritzema Bos, as well as those of J. Rosenthal, showed also the same negative result. If now indeed these experiments on mice, as Weismann states with especial emphasis, alone and without further evidence, by no means furnish direct proof of the claim "that injuries cannot at all be transmitted, since such experiments must be

prosecuted even to infinity: yet, indeed, after these unanimously negative results, the possibility of the transmission of single mutilations can be put entirely aside, and the [inheritance of ?-- Trans.] mutilations repeated on both parents through many generations, appear at least very improbable. I, as little as Weismann, Ziegler, and others, desire to doubt that modifying influence of external interference and stimulus on the germ plasm. One can be easily convinced of this, that change of climate, altered conditions of temperature, light and moisture, different manner of nourishment, transform entirely unmistakably the organism of plant and animal, and there is nothing in the way of the opinion that by the continued working of such external influences and stimuli the molecular structure of the germ plasm also experiences a change which can lead to a transmission of the transformations. Above all, it ought not to be forgotten in this case that the somatic cells are in no way the first to be modified by the stimulus, and that then by some sort of unexplained process (Pangenesis, or intracellular pangenesis) this stimulus is transmitted gradually by these cells to the plasma of the germ cells. The influence on the germ plasm is rather a direct one, and if, by continued influence, a transformation of the structure of this plasm takes place and transmission occurs, then we have simply a transmission of blastogenic, by no means of somatogenic characters, and therein is not the slightest admission of the transmission of acquired characters.

---

[1]The special question of the transmission of mutilations is of the greatest importance, since a single case of such transmission, if entirely beyond question, would be sufficient to decide finally the entire matter of the inheritance of acquired characters, since, in that case, the possibility of the transmission of all characters acquired in the lifetime of the individual, in physical as well as intellectual relations, would also have to be granted. By the by, I should like to mention just here, that at present some authors, and with them a part of the educated laity, are inclined to deny the transmission of single injuries or mutilations, but, on the other hand, to look upon the transmission of acquired characters upon the whole as possible.

[2]How easily such birthmarks, especially if prominent only in early childhood, come to be forgotten, may be seen from the following incident: The young man in question, who, like his grandfather, fought left-handed, and has also carried off a considerable number of cuts on his right cheek and forehead, is now father of two children, who show no trace of the family mark. The young wife of the gentleman, whom, as a relative, I have known from early childhood, brought into the world on her brow and scalp a red mark, about 6 cm. long, whereas, among grandparents, parents, sisters and other relatives, such a birth-mark has never appeared. Gradually it has grown rather indistinct, and has come to be forgotten, especially since the "bang" of the child was combed down over the naturally peculiar high brow. I have convinced myself personally that neither the brother nor

sisters of the young wife, nor her husband and other connections, had a suspicion of the existence of the mark. Yes, that she herself knew nothing of it, and could be convinced of the actual presence of this mark only by the assurance of her mother who confirmed my indiscreet statements. At present, moreover, only a very weak, scarcely perceptible trace of this mark is to be recognized. If a child of this couple had brought into the world any birth-mark whatsoever upon the brow, this mark surely would have been brought in genetic connection with one of the cuts on the forehead of the father, since the congenital mark on the mother's brow had long since lapsed into forgetfulness. One would then, with great probability, have spoken falsely of the transmission of a scar.

[3]In passing, I should like to mention how prevalent the belief in the Versehen of pregnant women still even in so-called educated circles, and should like to relate, only as a curiosity, the following case which occurred also in a family of acquaintances. A Mr. X. went driving with his daughter, who was in the fourth month of pregnancy. By an unlucky chance the pet dog of the young wife fell under the wheels, and was terribly mangled. At the sight of the bleeding animal the horrified woman made involuntarily a movement of the right hand towards the Kreuzgegend and behold the timely born, completely normal child, had on the same region a large blood-red spot. Mr. X. assured me that neither in his family nor in that of his wife had a similar birth-mark ever appeared, and so all were agreed that this red mark of the child must stand in direct relation to the movement of the mother at the sight of the bleeding dog. I could only express to the family my warmest congratulations that the young wife possessed such great presence of mind, joined to tender motherly love, that she made the motion of the hand towards just that region, for it, as is wont to be the rule at horrible spectacles, she had covered her eyes with her hand, then the child would have had to bring into the world a peculiar facial ornament, something in the line of a blood-red nose. To go further into detail with regard to the impossibility of the Verschen of pregnant women, I regard as superfluous. It is sufficiently well-known that from the moment of the fertilization of the egg by the spermatozoon, the fate of the embryo is decided, both so far as its form as well as its individual Anlagen are concerned. Naturally, on account of the intimate connection of the fruit with the mother, sickness which affects the entire organism of the latter, will also work detrimentally to the embryo, but neither beautiful nor horrible appearances on the part of the pregnant can produce the slightest change in the form of the embryo.

THE INHERITANCE OF ACQUIRED CHARACTERS

Hill, Leonard; *Nature,* 50:617, 1894.

It may be of interest to your readers to know that two guinea-pigs were born at Oxford a day or two before the death of Dr. Romanes, both of which exhibited a well-marked droop of the left upper eyelid. These guinea-pigs were the offspring of a male and a female guinea-pig, in both of which I had produced for Dr. Romanes, some months earlier, a droop of the left upper eyelid by division of the left cervical sympathetic nerve.

This result is a corroboration of one series of Brown-Sequard's experiments on the inheritance of acquired characteristics. A very large series of such experiments are of course needed to eliminate all sources of error, but this I unfortunately cannot carry out at present, owing to the need of a special farm in the country for the proper care and breeding of the animals.

THE PALEONTOLOGICAL EVIDENCE FOR THE TRANSMISSION OF ACQUIRED CHARACTERS

Osborn, Henry Fairfield; *Science,* 15:110-111, 1890.

Much of the evidence brought forward in France and Germany in support of the transmission of acquired characters, which has been so ably criticised in Weismann's recent essays, is of a very different order from that forming the main position of the so-called Neo-Lamarckians in America. It is true that most American zoologists, somewhat upon Semper's lines, have supported the theory of the direct action of environment, always assuming, however, the question of transmission. But Cope, the able if somewhat extreme advocate of these views, with Hyatt, Ryder, Brooks, Dall, and others, holding that "the survival of the fittest" is now amply demonstrated, submit that, in our present need of an explanation of the origin of the fittest, the principle of selection is adequate, and have brought forward and discussed the evidence for the inherited modifications produced by re-actions in the organism itself: in other words, the indirect action of environment. The supposed arguments from pathology and mutilations have not been considered at all; these would involve the immediate inheritance of characters impressed upon the organism and not springing from internal re-actions, and thus differ, both in the element of time and in their essential principle, from the above. As the selection principle is allowed all that Darwin claimed for it in his later writings, this school stands for Lamarckism plus---not versus ---Darwinism, as Lankester has recently put it. There is naturally a diversity of opinion as to how far each of these principles is operative, not that they conflict.

The following views are adopted from those held by Cope and others, so far as they conform to my own observations and apply

to the class of variations which come within the range of paleontological evidence. In the life of the individual, adaptation is increased by local and general metatrophic changes of necessity correlated, which take place most rapidly in the regions of least perfect adaptation, since here the re-actions are greatest. The main trend of variation is determined by the slow transmission, not of the full increase of adaptation, but of the disposition to adaptive atrophy or hypertrophy at certain points. The variations thus transmitted are accumulated by the selection of the individuals in which they are most marked, and by the extinction of inadaptive varieties or species. Selection is thus of the ensemble of new and modified characters. Finally, there is sufficient paleontological and morphological evidence that acquired characters, in the above limited sense, are transmitted.

In the present state of discussion, every thing turns upon the last proposition. While we freely admit that transmission has been generally assumed, a mass of direct evidence for this assumption has nevertheless been accumulating, chiefly in the field of paleontology. This has evidently not reached Professor Weismann, for no one could show a fairer controversial spirit, when he states repeatedly, "Not a single fact hitherto brought forward can be accepted as proof of the assumption." It is, of course, possible for a number of writers to fall together into a false line of reasoning from certain facts. It must, however, be pointed out that we are now deciding between two alternatives only; viz., pure selection, and selection plus transmission.

The distinctive feature of our rich paleontological evidence is that it covers the entire pedigree of variations: we are present not only at, but before birth, so to speak. Among many examples, I shall select here only a single illustration from the mammalian series,---the evolution of the molar teeth associated with the peculiar evolution of the feet in the horses. The feet, starting with plantigrade bear-like forms, present a continuous series of re-adjustments of the twenty-six original elements to digitigradism which furnish proof sufficient to the Lamarckian. But, as selectionists would explain this complex development and reduction by panmixia and the selection of favorable fortuitous correlations of elements already present, the teeth render us more direct service in this discussion, since they furnish not only the most intricate correlations and re-adjustments, but the complete history of the addition of a number of entirely new elements,---the rise of useful structures from their minute embryonic, apparently useless, condition, the most vulnerable point in the pure selection theory. Here are opportunities we have never enjoyed before in the study of the variation problem.

The first undoubted ancestor of the horse is Hyracotherium. Let us look back into the early history of its multicuspid upper molars, every step of which is now known. Upon the probability that mammalian teeth were developed from the reptilian type, Cope predicted in 1871 that the first accessory cusps would be found on the anterior and posterior slopes of a single cone; i.e., at the points of interference of an isognathous series in closing the jaws. Much later I showed that precisely this condition is filled in the

unique molars of the Upper Triassic Dromotherium. These, with the main cusp, form the three elements of the tri-tubercular crown. Passing by several well-known stages, we reach one in which the heel of the lower molars intersects, and, by wearing, produces depressions in the transverse ridges of the upper molars. At these points are developed the intermediate tubercles which play so important a role in the history of the ungulate molars. So, without a doubt, every one of the five main component cusps superadded to the original cones is first prophesied by a point of extreme wear, replaced by a minute tubercle, and grows into a cusp. The most worn teeth, i.e., the first true molars, are those in which these processes take place most rapidly. We compare hundreds of specimens of related species. Everywhere we find the same variations at the same stages, differing only in size, never in position. We extend the comparison to a widely separate phylum, and find the same pattern in a similar process of evolution. Excepting in two or three side-lines, the teeth of all the Mammalia have passed through closely parallel early stages of evolution, enabling us to formulate a law: The new main elements of the crown make their appearance at the first points of contact and chief points of wear of the teeth in preceding periods. Whatever may be true of spontaneous variations in other parts of the organism, these new cusps arise in the perfectly definite lines of growth. Now, upon the hypothesis that the modifications induced in the organism by use and disuse have no directive influence upon variations, all these instances of sequence must be considered coincidences. If there is no causal relationship, what other meaning can this sequence have? Even if useful new adjustments of elements already existing may arise independently of use, why should the origin of new elements conform to this law? Granting the possibility that the struggle for existence is so intense that a minute new cusp will be selected if it happens to arise at the right point, where are the non-selected new elements, the experimental failures of nature? We do not find them. Paleontology has, indeed, nothing to say upon individual selection, but chapters upon successful species and genera. Here is a practical confirmation of many of the most forcible theoretical objections which have been urged against the selection theory.

Now, after observing these principles operating in the teeth, look at the question enlarged by the evolution of parallel species of the horse series in America and Europe, and add to the development of the teeth what is observed in progress in the feet. Here is the problem of correlation in a stronger form even than that presented by Spencer and Romanes. To vary the mode of statement, what must be assumed in the strict application of the selection theory? (a) That variations in the lower molars correlated with coincident variations of reversed patterns in the upper molars, these with metamorphoses in the premolars and pocketing of the incisor enamel; (b) all new elements and forms, at first so minute as to be barely visible, immediately selected and accumulated; (c) in the same individuals, favorable variations in the proportions of the digits, involving re-adjustments in the entire limbs and skeleton, all coincident with those in the teeth; (d)

finally, all the above new variations, correlations, and re-adjustments not found in the hereditary germ-plasm of one period, but arising fortuitously by the union of different strains, observed to occur simultaneously and to be selected at the same rate in the species of the Rocky Mountains, the Thames valley, and Switzerland. These assumptions, if any thing, are understated. Any one of them seems to introduce the element of the inconstant; whereas in the marvellous parallelism, even to minute teeth-markings and osteological characters, in all the widely distributed forms between Hyracotherium and Equus, the most striking feature is the constant. Viewed as a whole, this evolution is one of uniform and uninterrupted progression, taking place simultaneously in all the details of structure over great areas. So nearly does race adaptation seem to conform to the laws of progressive adaptation in the individual, that, endowing the teeth with the power of immediate re-active growth like that of the skeleton, we can conceive the transformation of a single individual from the eocene five-toed bunodont into the modern horse.

The special application of the Lamarckian theory to the evolution of the teeth is not without its difficulties, some of which have been pointed out to me by Mr. E. B. Poulton. To the objection that the teeth are formed before piercing the gum, and the wear produces a loss of tissue, it may be replied that it is not the growth, but the re-action which produces it, which is supposed to be transmitted. Again, this is said to prove too much. Why is the growth of these cusps not continuous? This may be met in several ways: first, in the organism itself thse re-actions are least in the best adapted structures, a proposition which is more readily demonstrated in the feet than in the teeth (moreover, since the resulting growth never exceeds the uses of the individual, there is a natural limit to its transmission); second, the growth of the molars is limited by the nutritive supply (we observe one tooth or part growing at the expense of another); third, in some phyla we do observe growth which appears to lead to inadaptation, and is followed by extinction. In one instance we observe the recession of one cusp taking place pari passu with the development of the one opposed to it. These and many more general objections may be removed later; but they are of such force, that, even granting our own premises, we cannot now claim to offer a perfectly satisfactory explanation of all the facts.

The evidence in this field for, is still much stronger than that against, this theory. To sum up: the new variations in the skeleton and teeth of the fossil series are observed to have a definite direction; in seeking an explanation of this direction, we observe that it universally conforms to the re-actions produced in the individual by the laws of growth; we infer that these re-actions are transmitted. If the individual is the mere pendant of a chain (Galton), or upshoot from the continuous root of ancestral plasm (Weissmann), we are left at present with no explanation of this well-observed definite direction. But how can this transmission take place? If, from the evident necessity of a working theory of heredity, the onus probandi falls upon the Lamarckian,---if it be

demonstrated that this transmission does not take place,---then we are driven to the necessity of postulating some as yet unknown factor in evolution to explain these purposive or directive laws in variation, for, in this field at least, the old view of the random introduction and selection of new characters must be abandoned, not only upon theoretical grounds, but upon actual observation.

Reading between the lines of Weismann's deeply interesting essays, it is evident that he himself is coming to this conclusion.

. . . .

• The Miraculous Eye

ORIGIN OF EYES AND BRAINS

Gregory, R. L.; *Nature,* 213:369-372, 1967.

All knowledge comes through the senses, and this suggests an intimate link between the evolutionary development of brains and eyes. Somehow brains and eyes are developed by the random steps which form the evolutionary ladder according to the dictates of survival value at each rung. The restraints imposed on the development of preception by natural selection give some clue as to the way in which eyes and brains came into being. The fossil record is of no use in tracing the origins of these organs because the crucial steps have not been preserved. An attempt may, however, be made to put existing "primitive" forms into sequence according to various criteria. I shall not attempt a detailed account but rather endeavour to outline how the principal forms of eye could have come about and consider the " hen and egg" problem: which came first---the eye or the brain?

"Simple" eyes have a single optical system serving many photoreceptors. "Compound" eyes, found in arthropods, have up to many thousand individual optical systems, each with its own photoreceptor. Both kinds of eye must have appropriate neural systems to handle the information they provide. There is then a "hen and egg" problem, for it is very difficult to imagine how an eye could develop unless there were some suitable neural system already present to handle its information. Why, however, should a visual "computer" arise before there was an eye to feed it with information? If there is an answer to this, there is then the question of why there should be the two great classes of eyes.

Animals live in a world of objects, some edible, others dangerous, some protecting, such as crevices in rock, and others irrelevant to their survival. Objects have many characteristics beyond shape and colour, which alone are represented by optical images.

It is these other characteristics which are biologically important. One cannot be attacked and eaten by an image---hence the vicarious pleasures of the cinema---and neither can one feed on images. It follows that information given by eyes is only of indirect use to living creatures, and to make any use of visual information considerable computing is required. Other senses, especially touch and taste, do, however, give information of immediate value and thus it can be assumed with some confidence that these senses were the first to develop. Touch and the chemical senses directly monitor biologically vital features of the environment, and their information requires but a minimal "computer". It is therefore reasonable to suppose that vision is a lately acquired sense. How did it arise? How did a neural computer develop which was capable of reading significance into the non-biologically important optical images? The problem is especially acute because the significance of the images is not in the present state of affairs, but rather in what they presage of the immediate future. Touch and the chemical senses signal the state of the environment in immediate contact with their owner, but vision signals distant events, and may give warning of the future. The visual computer cannot be a simple reflex affair, which acts as soon as possible to a "stimulus". It must transform optical information into hypotheses of the nature of distant objects, which may be friend or foe, food or disaster. The eyes give time to compute; they allow brains to devise strategies more subtle than reflexes in response to stimuli. (pp. 369-370)

• Morphological Problems in Mammal Evolution

HORSE EVOLUTION DIAGRAMS IN ERROR

Macbeth, Norman; *Darwin Retried,* Gambit, Boston, 1971, pp. 14-15.

At this point the attentive reader will object that he has seen charts of descent with his own eyes, particularly those showing the progress of horses from the tiny Eohippus to the modern Equus. This is correct; he has seen precisely this picture on a number of occasions. But this leads to a story that is very embarrassing to the profession. I give it in Hardin's summary:

> ...there was a time when the existing fossils of the horses seemed to indicate a straight-line evolution from small to large, from dog-like to horse-like, from animals with simple grinding teeth to animals with the complicated cusps of the modern horse. It looked straight-line--like the links of a chain. But

not for long. As more fossils were uncovered, the chain splayed out into the usual phylogenetic net, and it was all too apparent that evolution had not been in a straight line at all, but that (to consider size only) horses had now grown taller, now shorter, with the passage of time. Unfortunately, before the picture was completely clear, an exhibit of horses as an example of orthogenesis had been set up at the American Museum of Natural History, photographed, and much reproduced in elementary textbooks (where it is still being reprinted today). (pp. 14-15)

64 ARMADILLOS THREATEN A THEORY

Culliton, Barbara J.; *Science News*, 94:555-557, 1968.

Dr. Roger J. Williams, working with Dr. Eleanor Storrs Burchfield, collected 62 adult female armadillos who produced 249 infants---61 quadruplet sets and one litter with a fifth wheel---of which 16 sets of four were used.

The 64 were sacrificed at birth and tested for 20 parameters including body weight, organ weight (including adrenals, brain, heart, kidney and liver) and levels of various chemicals present in these organs. Each animal was compared to his litter mates.

The ostensibly identical quadruplets showed differences ranging from 2- to 140-fold.

Although the intrauterine environment may be partially responsible for the differences, these variations are too marked to be attributed to such factors as position in the uterus or the amount of blood supplied to the fetus, Dr. Williams says. The explanation, he believes, can be more accurately attributed to something in the cytoplasm, the fluid surrounding the nucleus of the egg cell from which the animals developed.

. . . . .

HORNS AND ANTLERS

Modell, Walter; *Scientific American*, 220:114-120, April 1969.

In a curious way horns and antlers are invariably associated with certain other apparently unrelated anatomical features. Headpieces of one kind or another (horns, antlers or knobs) are possessed by virtually all even-toed ungulates with a four-chambered (or true ruminant) stomach; this includes the Bovidae, the Cervidae, the pronghorn, the giraffe and the okapi. With the single exception of the rhinoceros no odd-toed animal (which includes the horse, the ass and the zebra) has a headpiece, and

headpieces are also absent in all even-toed ungulates with false ruminant, or three-chambered, stomachs (camels, llamas and others), as well as in those with a single-chambered stomach (pigs, peccaries and hippopotamuses). What connection can there be between horns or antlers and a perfect ruminant stomach and even-toed hooves? By what odd quirk of evolution did these seemingly unrelated characteristics come to be associated with one another, if indeed the association is not mere coincidence? (p. 122)

PALEONTOLOGY AND PHYLOGENY: PATTERNS OF EVOLUTION .....

Gingerich, Philip D.; *American Journal of Science,* 276:1-28, 1976.

Abstract. The underlying tempo of the evolutionary process is determined by rates of evolution in single phyletic lineages and by rates of divergence in sister lineages following geographic speciation. These rates and hence the tempo of morphological evolution can be determined for natural populations only by detailed study of stratigraphic successions in areas where the fossil record is relatively complete. The stratigraphy of Middle Paleocene through Lower Eocene continental deposits in the Big Horn Basin of Wyoming is herein described, and fossils of the genera Hyopsodus, Haplomylus, Pelycodus, and Plesiadapis are placed in stratigraphic context. The resulting patterns of evolution at the species level illustrate the importance of gradual phyletic evolution in the origin of new species and the consistent presence of gradual morphological divergence following geographic speciation (geographic speciation seems best characterized as parapatric rather than allopatric in these examples).

Change in each phyletic lineage is nonrandom and apparently controlled by strong directional selection. Where two or more closely related species are present in the same time period, the species diverge gradually in size---presumably to minimize interspecific competition. The gradual nature of this divergence may be explained by the high heritability of most morphological characters related to body size. Although gradual, the observed rates of divergence are sufficiently rapid that an appearance of abrupt change would be registered if (1) there were significant gaps in the fossil record, (2) stratigraphic sampling was not sufficiently refined, or (3) a typological species concept was applied. Examples of "quantum evolution" or "punctuated equilibrium" have not yet been substantiated adequately in terms of these three criteria, wheras an interpretation of gradual phyletic evolution and gradual divergence following cladogenic speciation appears fully justified in the evolutionary sequences of early terrestrial mammals described here.

• Physiological Effects of Magnetic Fields

ABNORMALITIES IN ORGANS OF MICE INDUCED BY A MAGNETIC FIELD

Barthony, M. F., and Sumegh, I.; *Nature,* 221:270-271, 1969.

During the past few years it has been found that homogeneous magnetic fields produce morphological, functional and behavioural changes in living organisms. There have been various hypotheses to explain these observations. The second day minimum in the weight of mice exposed to strong (3,000 to 10,000 oersted) magnetic fields seems to have the characteristic of stress effects---alarm reaction followed by adaptation to the stress. The questions remain whether there is a similarity between stress effects and those observed in animals exposed to magnetic fields, and whether the magnetic field is a rather weak non-specific stressor. We report experiments in which the magnetic field caused significant changes in the organs of mice. [The experimental details are omitted here.]

The last column of Table I [not reproduced] lists the probability level (PL), computed by using Student's t test, that the observed effect is caused by chance alone. The high significance of the probability levels found for each of the four investigated parameters establishes the existence of the changes in organs of mice exposed for 13 days to a 9,000 oersted static magnetic field described here. Although several of the observed abnormalities in the organs seem to suggest that the effect of magnetic fields resembles the effects of non-specific stress, at least two of the symptoms---the narrowing of the zona fasciculata and the decrease in the number of megakaryocytes in the bone marrow---are the opposite of the usual effects of non-specific stress conditions. The observed symptoms show that, during exposure to magnetic fields, some hormone imbalance occurs in the hypothalamus-hypophyseal axis. It will be interesting to repeat these experiments on adrenalectomized mice and on mice which are supplied with extracts containing the hormones involved.

• Degeneracy of Species

ARE MONKEYS DEGENERATE MEN?

Anonymous; *Literary Digest,* 91:67, November 27, 1926.

The monkey is descended from man, said Dr. Max Westenhoefer, professor of pathological anatomy at Berlin University, in an address before the Anthropological Congress at Salzburg, reported in a dispatch to the New York World. He declared also that man is developed from a remote animal that itself developed from a reptilian form of life. We read:

"It is a matter of fact that chimpanzee babies are much nearer human beings than their parents," he said. Referring to Haeckel's fundamental biogenetical law that the ancestors of monkeys came from humanlike animals, he added:

"For this reason, the sentence, 'Human beings originated from monkeys' must be changed to 'Monkeys originated from human beings.'"

Dr. Westenhoefer concluded by declaring definitely that human beings have come in a direct line from the oldest mammals---the ankle-joint animals from which all other groups of mammals with their different jaw and foot formation have branched. He began by declaring that in addition to numerous known primitive characteristics of mankind, primitive conditions in the formation of some of their organs are preserved in mankind to-day. Among such primitive survivals he mentioned the appendix, the lobulations of the kidneys, and the indentation of the spleen and formation of additional spleens.

In explaining the last two, Dr. Westenhoefer said they are only found in water mammals, and that it could be taken for granted that the ancestors of mankind must have had a period of water life.

He pointed out as of special importance that not a single monkey type, not even the manlike ape, shows a lobulation of the kidneys, and so concluded that monkeys must be regarded as a further development beyond human kind. He explained that these observations led him to search for the origin of the human chin and foot, as they are of special formation, and if he could prove their origin it would be possible to show from what place in the line of vertebrate animals humanity developed.

The human chin, he said, developed from the peculiar position and function of human teeth. For this reason, he said, this point of human development could be located in connection with certain reptiles. The chin has kept its primitive characteristics in man, while it has deviated in other animals through strong development and specialization of their teeth.

He pointed out that the strong development of the teeth, jaws, chewing muscles, and mouths of mammals stands in inverse proportion to the development of their brains, while with the human being, exactly the opposite is true.

Dr. Westenhoefer declared the prehensile foot is not the oldest mammal foot, but the real walking or standing foot of man.

The preservation of the primitive form and function of the teeth and jaws and of a standing foot connected with the ability to walk erect are the main reasons for the brain development of the ankle-joint animals, so that from this creature the brain animal, the human being, could develop.

UNUSUAL PHYSICAL ABILITIES

• The Magnetic Sense

DOLPHINS MAY SENSE EARTH'S MAGNETISM

Anonymous; *EOS,* 61:538, 1980.

Birds do it. Bees do it. Now, maybe, dolphins do it, too, in a paper presented at the AGU Spring Meeting in Toronto, Mike Fuller of the University of California, Santa Barbara, announced that he and co-workers J. Dunn, also of UCSB, and John Zoeger, of Los Angeles Harbor College, had found magnetic material in the heads of dolphins. The finding opens the possibility that dolphins will join the growing list of animals that react to the earth's magnetic field, a list that includes pigeons, honeybees, and bacteria.

The researchers dissected the heads of five dolphins, which were supplied by the Los Angeles County Museum of Natural History through a program in which animals that have died of natural causes are turned over to researchers. Tiny amounts of magnetic material were found in all but one of the dolphins; that one was too badly decomposed to dissect successfully, Fuller said.

. . . .

• Unrecognized Senses

DO ANIMALS USE RADIO TELEPATHY?

Anonymous; *New Scientist,* 65:271, 1975.

J. Bigu del Blanco and Cesar Romero-Sierra of the anatomy department of Queens University, Ontario, working on a National Research Council of Canada grant, looked at the microwave emissions at 8.95 GHz from a man and a rabbit. They found that not only did both produce the natural microwave background expected from any radiating warm body, but also signals well above the background. Measurements were taken of signals from the abdomen of a man (with the antenna 4 m away) and from the man's hand and a rabbit's head (with the antenna 0.5 m away). Signals were found in all cases, and in the case of the rabbit, different size signals corresponding to different states of stress.

The experimenters note that theoretical calculations of energy distributions expected from radiating bodies show that it is reasonable to expect microwave emissions large enough to be detected by conventional microwave equipment. They suggest this sort of measurement as another measure of body activity, such as blood pressure and temperature, to show the state of the individual.

But they go on to suggest that since the signal can be emitted by an animal and detected by conventional means, it is possible that animals can receive such signals as well---in effect communicate by radio.

As stress seems to influence the signal, this could provide an explanation of why cats, for example, seem sometimes to be able to recognise when other cats are stressed, even when they are outside the normal hearing range.

Romero-Sierra and del Blanco do not actually suggest a mechanism, but they do "call the attention of the reader to an interesting hypothesis in which DNA molecules are regarded as RF signal generators, RNA as amplifiers, the cell wall as a noise filter, and enzymes and amino acids as effectors of signals."

. . . . .

HOW DO CATS SMELL OUT X-RAYS?

Anonymous; *New Scientist,* 25:378, 1965.

It has been established that cats are able to detect X-rays, and that the organ involved is definitely not the eye. Attempting to find out where the sensitive region is, a team of biologists at the Veterans Administration Hospital, Long Beach, California, set

up an experiment of the conditioning type, in which cats had to react to five-second exposures to X-radiation in order to avoid mild electric shocks.

Using a beam only five millimetres across, they were able to deliver the radiation to specific regions. They discovered that the beam had the greatest effect when it was directed at the olfactory region of the head---specifically, the olfactory bulb, which is behind the nasal and oral passages. The obvious suggestion that what the cats are detecting is the smell of ozone created by the X-rays seems to be disposed of by experiments in which the beam was directed at the nose itself, with little effect.

The question of how cats detect X-rays is certainly not completely settled. Animals deprived of their olfactory bulbs retained some sensitivity, particularly when the dose of X-rays was raised. It may be that there is more than one receptor; this view is supported by the findings of other workers, some of whom have noticed that sensitivity is apparently not even confined to the head but is spread throughout the entire body.

• Animal Dowsing?

AFRICAN ELEPHANTS AS GEOLOGICAL INDICATORS

Hyde, Herbert P. T.; *Nature,* 165:326, 1950.

During two and a half decades of African bush geology, an observation has forced itself upon me which may perhaps have been noticed by others, namely, that elephants appear to show preference for certain geological horizons. This is at times so well pronounced that the boundary line between two formations can be deduced from native information as to how far elephants circulate in the district. I have noticed it particularly in regions where crystalline rocks, mainly granite masses, come into contact with sandstone, and it is the latter which the elephants prefer.

The first locality, where indeed the boundaries of the granite could be marked according to these observations, was the country between Mouyondzi, Sibiti and Loudima, west of Brazzaville, in the French Middle-Congo, the formations involved being the "Gres de Sibiti", the Upper Kundelungu and the granite mass of Zanaga.

The second district where it was well marked was eastern Oubangui-Chari, towards the border of the Anglo-Egyptian Sudan. Here the escarpment formed by the Ouadda sandstones, generally classified as Loubilashe, formed the definite margin towards the crystalline country of Dar Challa (Ouandadjale).

The third time I came across this phenomenon was in French Adamaua, Northern Cameroons. Here it is the boundary line between the Benoue sandstones and the crystalline and young volcanic rocks of Ngaoundere and Rey-Bouba, roughly 40 km. south of Garua.

In some cases, as to a certain extent within the Forest of Sibiti, where the decomposition of sandstone gives rise to dense vegetation, the question explains itself. Otherwise, however, the country was either tree-savanna or light bush, and in such cases the reason is difficult to explain. It might be a question of food, chemical composition of water or nature of the weathering soil.

It would be interesting to have comments from Central and East Africa, also from the Gold Coast, with reference to the boundary between sandstones of the Voltaian and crystalline rocks of the Northern Territories.

• Unusual Adaptation to Cold

MICE AT -3°C

Barnett, S. A.; *New Scientist,* 27:678-679, 1965.

It might be thought that only fairly large mammals could adapt themselves to temperatures permenently below 0°C. It was therefore rather disconcerting to find, during the second World War, that meat cold stores, kept at about -10°, sometimes had large colonies of house mice. These creatures rarely weigh as much as 30 g, or one ounce. Their young, at birth, weigh less than 2 g. Nevertheless, they bred in these Arctic conditions. They fed on the meat, especially the kidneys (which are relatively soft); and they nested either in the cork linings of the walls, or in the carcasses themselves. The cavity made by the ribs often provided a suitable home. Nest material was taken from the muslin covering the carcasses.

These cold-store mice suggested some interesting problems of adaptation. Were they generically different from other mice? Did breeding for several generations in the cold affect them in any special way?

Such questions have been studied for the past twelve years in Glasgow. Laboratory mice (which, unlike wild mice, are tractable animals) have been bred for tens of generations in rooms kept at -3°C.

When one asks how these small creatures manage even to survive, a first thought is that perhaps they grow more hair.

In fact, they do. But the effect on their insulation is small. If a mouse were to be as effectively insulated as an Arctic fox, it would need exactly the same thickness of insulation. This would immobilize, indeed smother, it. Hence a small mammal cannot be effectively insulated by skin or hair.

. . . . .

A small mammal, then, to survive in such a cold environment, must produce more heat; and, at least to breed, it needs nest material. The heat production demands extra food. Young virgin females eat about twice as much at -3°C as at 21°C.

This mere doubling of food consumption is less than was expected. However, mice are much less active in a cold environment than a warm one; in this way they economize in energy. Another possible reason why food comsumption is not greater in the cold is that cold-adapted animals probably make better use of their food than those living in luxury and ease at 21°C. This has emerged especially from observations of pregnant and lactating females.

. . . . .

When mice are transferred to the cold as adults they often lose many nestlings; and when their offspring are bred, they lose even more. The worst breeders are often those of the first and second generations reared in the cold.

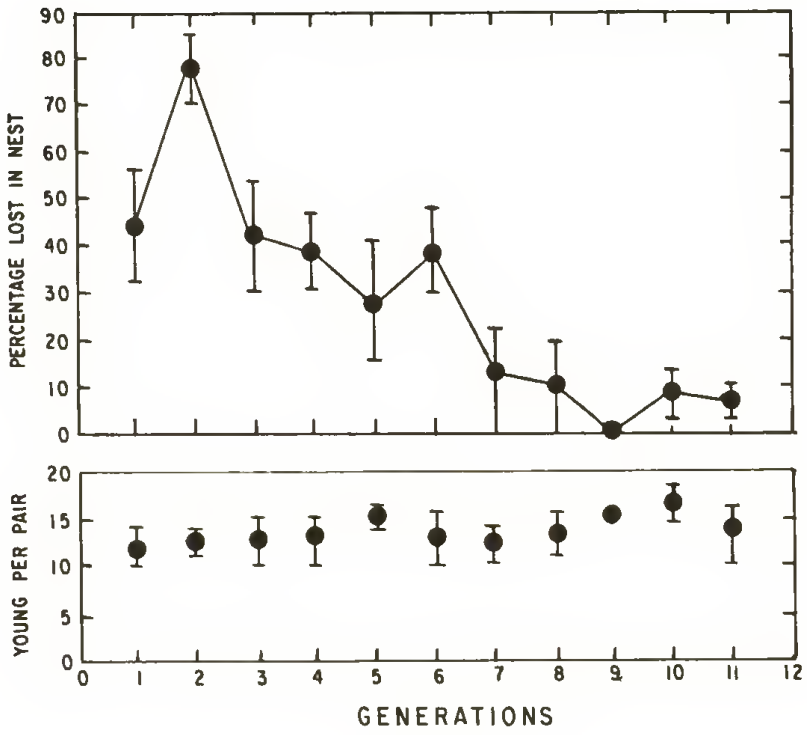
What about subsequent generations? These have displayed certain extraordinary features as yet unexplained.

Consider first a genetically mixed stock of mice--with individuals of many colours and differing in other respects also. Such mice have been bred at -3°C, and selected for success in the cold. Over the generations an improvement would be expected: this would be attributed to selection of genetically more cold-resistant individuals. In one experiment of this sort, during the first three generations, the nestling mortality averaged about forty per cent; after the twelfth generation it had fallen to less than ten per cent. There was also an increase in body size, which perhaps conferred a small advantage in the cold.

By contrast, if a highly inbred strain were transferred to the cold, no such progressive change could be expected. A highly inbred strain is genetically fairly uniform, and so gives little scope for improvement by artificial selection.

Nevertheless, two inbred strains transferred to the cold did in fact display a substantial improvement over a number of generations. Each began with a nestling mortality of fifty per cent or more; but in both the rate declined to a figure similar to that of their cousins maintained at 21°C. The number of young born to each pair did not change over the generations.

A detailed study of one of the inbred strains has begun, and has revealed a number of other changes. The mice of long ancestry in the cold are heavier than mice of the same strain recently introduced, and, in fact, weigh much the same as the controls at 21°C. Moreover, newly introduced mice have more body water and less fat than the controls, but this does not



Decline of nestling mortality under cold conditions. (Adapted from Royal Society, Proceedings, *1961)*

apply to the mice of long ancestry in the cold: indeed, the latter have more fat than the controls.

Maternal performance of the various kinds of mice has been studied by transferring litters at birth from one kind of female to another. The fostered young are afterwards mated. These experiments, still in progress, confirm that old stock females are particularly efficient mothers. What is more, fostered mice of the old stock, regardless of the kind of female by which they have been reared, are themselves exceptionally good mothers; that is, they tend to resemble their true mothers. But this difference does not persist into a second generation, as it would if it were genetically determined.

The strange, almost bizarre, modification in the mice kept for many generations in a cold environment suggests at first sight a genetical change; but it cannot be one. Evidently it is some sort of cumulative maternal effect. It may be that a disease organism is present; that it is transmitted from mother to young; and that it has become progressively less virulent over the generations. This is only one possible kind of maternal effect.

The principal fact is that inbred mice have become better adapted to a cold environment as a result of breeding there for

a number of generations. This unexpected observation, whatever the detailed explanation, suggests that the cold-adaptation of mammals has features which cannot be fully revealed by short-term experiments.

• The Sense of Smell and Animal Color

THE OLFACTORY SENSE IN WHITE ANIMALS

Anonymous; *English Mechanic,* 24:7-8, 1876.

Some very curious physiological facts bearing upon the presence or absence of white colours in the higher animals have lately been adduced by Dr. Ogle. It has been found that a coloured or dark pigment in the olfactory region of the nostrils is essential to perfect smell, and this pigment is rarely deficient except when the whole animal is pure white. In these cases the creature is almost without smell or taste. This, Dr. Ogle believes, explains the curious case of the pigs in Virginia adduced by Mr. Darwin, white pigs being poisoned by a poisonous root which does not affect black pigs. Mr. Darwin imputed this to a constitutional difference accompanying the dark colour, which rendered what was poisonous to the white-coloured animals quite innocuous to the black. Dr. Ogle however, observes, that there is no proof that the black pigs eat the root, and he believes the more probable explanation to be that it is distasteful to them, while the white pigs, being deficient in smell and taste, eat it and are killed. Analogous facts occur in several distinct families. White sheep are killed in the Tarentino by eating Hypericum criscum, while black sheep escape; white rhinoceroses are said to perish from eating Euphorbia candelabrum; and white horses are said to suffer from poisonous food where coloured ones escape. Now it is very improbable that a constitutional immunity from poisoning by so many distinct plants should in the case of such widely different animals be always correlated with the same difference of colour; but the facts are readily understood if the senses of smell and taste are dependent on the presence of a pigment which is deficient in wholly white animals. The explanation has, however, been carried a step further, by experiments showing that the absorption of odours by dead matter, such as clothing, is greatly affected by colour, black being the most powerful absorbent, then blue, red, yellow, and lastly white. We have here a physical cause for the sense-inferiority of totally white animals which may account for their rarity in nature. For few if any, wild animals are wholly white. The head, the face, or at least the muzzle or the nose, are generally

black. The ears and eyes are also often black; and there is reason to believe that dark pigment is essential to good hearing, as it certainly is to perfect vision. We can therefore understand why white cats with blue eyes are so often deaf---a peculiarity we notice more readily than their deficiency of smell or taste.

If then the prevalence of white colouration is generally accompanied with some deficiency in the acuteness of the most important senses, this colour becomes doubly dangerous, for it not only renders its possessor more conspicuous to its enemies, but at the same time makes it less ready in detecting the presence of danger. Hence, perhaps, the reason why white appears more frequently in islands where competition is less severe and enemies less numerous and varied. Hence, also, a reason why albinoism, although freely occurring in captivity never maintains itself in a wild state, while melanism does. The peculiarity of some islands in having all their inhabitants of dusky colours--as the Galapagos---may also perhaps be explained on the same principles, for poisonous fruits or seeds may there abound which weed out all white or light-coloured varieties, owing to their deficiency of smell and taste. We can hardly believe, however, that this would apply to white coloured butterflies, and this may be a reason why the effect of an insular habitat is more marked in these insects than in birds or mammals. But though inapplicable to the lower animals, this curious relation of sense-acuteness with colours may have had some influence on the development of the higher human races. If light tints of the skin were generally accompanied by some deficiency in the senses of smell, hearing, and vision, the white could never compete with the darker races, so long as a man was in a very low or savage condition, and wholly dependent for existence on the acuteness of his senses. But as the mental faculties became more fully developed and more important to his welfare than mere sense-acuteness, the lighter tints of skin, and hair, and eyes, would cease to be disadvantageous whenever they were accompanied by superior brain-power. Such variations would then be preserved; and thus may have arisen the Xanthochroic race of mankind, in which we find a high development of intellect accompanied by a slight deficiency in the acuteness of the senses as compared with the darker forms.

• Water Breathing

MAMMALS CAN BREATHE WATER

Kylstra, Johannes A.; *Sea Frontiers*, 10:209–214, 1964.

Mice and dogs, under certain conditions, can be submerged

in water for considerable periods of time without drowning. Under these circumstances the animals actually breathe the way a fish does; their lungs, acting as gills, extract oxygen from the water.

Submerged adult mammals usually drown because water in contact with them does not contain enough dissolved oxygen. But if the oxygen pressure above the water is raised, more oxygen dissolves. The rate of increase is such that under eight atmospheres of pressure the amount of oxygen dissolved in water is roughly equal to the amount of oxygen in air. Under these circumstances water now "resembles" air a bit more closely and experiments have shown that. With some salts added, mice and dogs can breathe water and survive.

. . . .

Mice Breathe One Mile Down. Adult "Swiss" mice of both sexes were used. A small chamber was partly filled with water to which various salts had been added in similar amounts as those present in blood. Air in the chamber was replaced by compressed oxygen. The fluid was agitated to hasten solution of the gases. After approximately thirty minutes, a mouse was introduced into the chamber via a lock. The mouse was prevented from surfacing by a grid.

This basic procedure was followed with a number of mice at an oxygen pressure of eight atmospheres. The mice continued to breathe for several minutes and, in some cases, hours. The longest survival times occurred when the temperature of the solution was 68°F. The mice were initially active and alert while submerged and did not appear to be in severe distress. The procedure was repeated in another chamber which could be pressurized up to 160 atmospheres equivalent to the pressure of sea water at a depth of one mile. Even this extraordinary pressure did not kill the mice.

Mice submerged in the oxygenated solution usually appeared to lose consciousness within one-half hour but when a chemical "buffer" (A substance which tends to hold the acidity of a solution constant) was added, the animals responded to stimuli such as shaking the tank or knocking on the walls for up to six hours.

Mice that have breathed fluid for ten to thirty minutes have subsequently been kept alive for over two hours in the unflooded portion of the chamber. Unfortunately, the transition from fluid breathing to air breathing has not been successful in mice.

. . . .

Dogs Survive. Under these conditions, six dogs lived submerged while breathing an oxygenated saline solution for from twenty-three to thirty-eight minutes. The duration of the fluid breathing was limited by the long decompression time needed by the attendants rather than by ill effects to the animals. One of the dogs which breathed water for twenty-three minutes has been adopted as a mascot by submarine rescue vessel H.M. Cerberus of the Royal Netherlands Navy. In another series of experiments a dog which survived twenty-seven minutes of fluid breathing has recently given birth to nine healthy puppies.

Many animals did not fare so well, however, on being returned to the surface. Ironically, it appears that the return from water to air breathing is much more hazardous than the initial submersion or prolonged fluid breathing in itself.

When an animal is brought to the surface, its lungs are drained mechanically but some of the fluid remains in the alveoli or tiny sacs at the ends of the branches of the bronchial tubes. Until the fluid is absorbed, a process which takes hours, gas exchange in the lungs is seriously impaired. The reverse occurs when the animal is first immersed. Then, gas exchange is initially impaired until the nitrogen in the alveoli, left over from the animal's recent air breathing, has been absorbed and replaced by the oxygenated fluid. The absorption of nitrogen, however, is much faster than that of the liquid and so the period of impaired gas exchange in the lung is fairly brief upon submersion.

It may be that readaptation of terrestrial animals to a gaseous environment after prolonged liquid breathing will require some means of aiding the normal process of gas exchange until the residual fluid in the lungs has been absorbed and the alveoli are normally expanded again with air.

. . . .

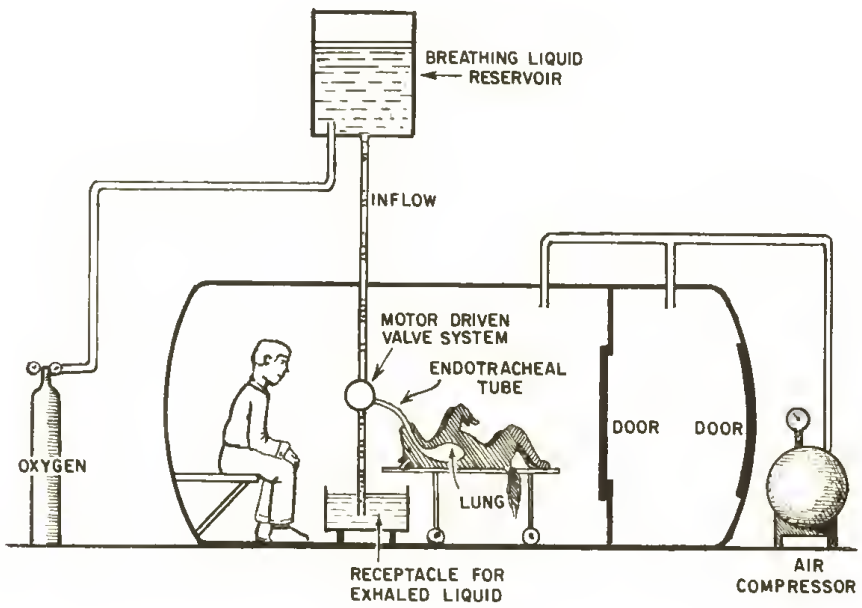


Diagram of J. A. Klystra's experiments with dogs. (Adapted from New Scientist, *24:566, 1964)*

• Sleep Curiosities

SLEEP IN RUMINANTS

Balch, C. C.; *Nature,* 175:940-941, 1955.

During digestion experiments with cattle, it was observed that the animals never appeared to sleep and always used the same lying position. In recent years the behaviour of cattle and sheep has been carefully studied by numerous workers, particularly under grazing conditions; the subject has been reviewed by Tribe and Hancock. In spite of this attention, the almost complete absence of reference to sleep is a striking feature of the reports. Brownlee could find no evidence that healthy cattle ever lost consciousness either by day or night. Similarly, Hancock found that it was debatable if cows ever sleep; even at times of total rest they kept their eyes open except for very short periods--a few minutes at most--when they generally rested their heads on the flanks.

It is difficult to find an exact definition of sleep acceptable by all authorities; in the present work a search has been made for the marked relaxation and loss of consciousness (especially of vision) which are the more obvious manifestations of sleep in other animals. In the vast majority of cattle seen at night, the eyes were open and clearly watching the observer. In the few instances where they were closed the movement of an ear would often betray consciousness, or slight noises such as rubbing together of the fingers would evoke an immediate reaction.

In order to confirm that the absence of mention of sleep from published reports did in fact mean that none was observed, I have communicated with a number of observers. Their replies were in close agreement with observations here, namely, that sheep and cattle sleep little if at all, rarely closing their eyes and seeming even then not to lose consciousness. When lying, they keep the thorax upright and only very rarely and for short periods lie out flat. Observation and inquiry in zoos has confirmed that the apparent lack of sleep and the typical lying position are general characteristics of ruminants.

. . . . .

CURIOSITIES OF BEHAVIOR

• Apparent Intelligence in Mammals

INTELLECT IN BRUTES

Greenock; *Nature,* 20:196, 1879.

During the recent severe winter a friend was in the habit of throwing crumbs for birds outside his bedroom window. The family have a fine black cat, which, seeing that the crumbs brought birds, would occasionally hide herself behind some shrubs, and when the birds came for their breakfast, would pounce out upon them with varying success. The crumbs had been laid out as usual, one afternoon, but left untouched, and during the night a slight fall of snow occurred. On looking out next morning my friend observed Puss busily engaged scratching away the snow. Curious to learn what she sought, he waited, and saw her take the crumbs up from the cleared space and lay them one after another on the snow. After doing this she retired behind the shrubs to wait further developments. This was repeated on two other occasions, until finally they were obliged to give up putting out crumbs, as Puss showed herself such a fatal enemy to the birds.

INTELLECT IN BRUTES

Gunning, W. D.; *Nature,* 20:30, 1879.

A few months ago I made the acquaintance of a dog, which, I think, is worthy of a place among the dogs, and cats, and rats, and mules that are helping the pages of Nature to determine the degree and kind of animal intelligence.

"Priests" is a hotel on the way from the Calaveras Grove of Big-trees to the Yosemite. In former years, on the arrival of the stage, the landlady would send the dog to the poultry yard to catch chickens for the tourists' dinner. New the dog "takes time by the forelock." The stage is due at six o'clock. About five o'clock the dog saunters leisurely down the road till he meets the stage, he then bounds back to the poultry-yard, catches chickens, bites their heads off, and takes them to the

cook! The number of chickens he kills bears a relation to the number of passengers he saw in the stage.

A gentleman who was stopping at the hotel for a few days went into the woods one afternoon with a gun. When he returned the dog came to him in much excitement to see what game he had taken. Finding his hands and his bag empty the dog ran into the forest and returned in less than an hour with a bird, which he gave with an air of compassion to the unskilful hunter.

INTELLECT IN BRUTES

Tait, Lawson, et al; *Nature*, 20:147, 1879.

My wife and I are devotedly fond of our cats, so much so as to afford amusement to our fiends, and we are never tired of expatiating on their indications of intelligence. A pedigree book is kept, and any reader of Nature desirous of possessing a kitten of an intelligent stock is welcome to one on three or four months' notice.

I wish to give one other story of them which seems to show that they are apt to indulge in revenge and to act in systematic co-operation to accomplish it. They are of very cleanly habits, and, save under the circumstances about to be narrated, have never given any trouble in this respect. But some time ago we had a visitor who had a strong and very badly-concealed dislike to them. The dislike was quite mutual. Very soon after the arrival of this visitor, the cats became very objectionable on account of messes, and these were concentrated in and near the bedroom occupied by the object of their aversion. Their insanitary proceedings became so pronounced that it almost appeared as if they had invited all their feline friends in the neighbourhood to join in the establishment of a "night-soil tip." No amount of correction, aded by pepper of the most pungent kind, could stop it, and I most reluctantly determined upon a wholesale felicide. The visitor departed, however, before this was carried into effect, and immediately the nuisance ceased, and our cats resumed their original cleanly habits. (Lawson Tait)

About thirteen years ago, a now deceased medical man residing near Edinburgh, possessed a favourite collie, "Cheviot" by name. The incident I am about to relate, I may mention, was related to me by the son of the gentleman in question, both father and son, along with a perfectly disinterested party, having corroborated the facts. The then provost of the burgh in which "Cheviot" resided, had issued an interdict against unmuzzled dogs, during the "dog days," and "Cheviot" submitted with no good grace to the operation of securing his jaws. Frequently "Cheviot's" master and the members of the family spoke in the dog's hearing, in no measured terms of the cruelty of the provost's order. But the end of the "dog days" came, and

"Cheviot's" muzzle was removed. On the afternoon of the day of liberation, the provost called on "Cheviot's" master, to say, that in the morning he had heard a dog whining at his front door. The provost opened the door; "Cheviot" was in waiting, his muzzle in his mouth. One look at the provost, and the muzzle was dropped at his feet, "Cheviot" scampering off in the highest glee, as if delighted to have had the opportunity of laying the cause of his grievance at the door of his enemy.

The details were vouched for the the provost himself, also a medical practitioner in the burgh.

Here it seems to me you possess an incident of dog character explicable only on the supposition that there are germs in the canine philosophy of acts and traits fully developed in ours.

Incident number two deals with the doings of a retriever, some four or five years old, who, whilst bearing an implacable enmity to felines at large, had struck up a close friendship with a household cat, which, from kittenhood, had been associated with him. For sanitary reasons the cat was condemned to die. According to the orthodox method, puss was placed in a sack weighted with stones, and carried to the sea, "Keeper," the dog, following in the wake of the procession. The cat was duly thrown into the sea, "Keeper" waited to see if it would rise, but on seeing no signs of his feline friend, he at once dived for the sack, and landed it at an adjacent pier. Being met by the executioners and divining that puss was yet in danger, "Keeper" re-entered the water, sack in mouth, and swam across the bay to a point of safety, and landed his burden. Puss was spared in deference to "Keeper's" anxiety.

I can find still another example of extreme unselfishness in a mongrel dog, who, for some years before the death of an old deaf and blind companion, was accustomed to proceed to his resting-place, and bark in his ear to warn him of the presence near at hand of the milk which the kindly hand of the mistress of the house was accustomed to place for the delectation of both. This proceeding was repeated day by day, with unvarying regularity, and in its nature suggests strongly that the exercise of self-denial--amidst the obvious temptation of an easy acquirement of luxury--has to be placed to the credit account of the canine race. (Andrew Wilson)

With reference to the article in Nature of 5th June, permit me to narrate an instance of "abstract reasoning" in a retriever that I was witness to last autumn.

Having shot a hare so slightly as to make it a long chase for the dog (a young one), I watched the retriever follow the hare over the open hills of Aberdeenshire for upwards of two miles until the chase was lost to view under a stone dyke. In a few moments the dog was observed to carry something in his mouth with which he disappeared over the dyke into a turnip field. "He has killed the hare and he is too tired to bring it back, so he is burying it," quoth the keeper, "we shall come up with it in the evening." The day's sport over, we made for the dog's burying ground, but the retriever, if you please, knew nothing about it;

and careered wildly about in every direction except the right one. The keeper, Henry Ledingham of Tarland, Aboyne, having a remarkable gift of spotting fallen game, actually put his foot on the very spot among the turnips where the burial had occurred. After immense affectation of surprise the retriever was forced to unearth the hare. The hare, however, was a rotten old carcass of a hare, with no eyes and teeth, that the retriever had picked up and buried to save himself the pains of following the live hare. Perfectly conscious of his misdeed, the dog had given evidence of abstract reasoning in each stage of the transaction. (Charles Baillie Hamilton)

INTELLIGENCE IN ANIMALS

Anonymous; *Knowledge,* 1:28-29, and 1:46-47, 1881.

Few of the questions raised in Darwin's "Descent of Man" are at once more difficult to deal with satisfactorily, or more important in their bearing on the subject of that volume, than the question how far animals possess mental powers akin to those of man. It is somewhat singular, we may remark in passing, that Darwin and Huxley, whose views in some respects are so similar, and who are regarded by the general public as standing side by side in their advocacy of the theory of the relationship of man to the lower animals, should seem to uphold almost exactly opposite opinions respecting the cerebral qualities of animals,--one maintaining that in some cases animals reason, while the other (if we rightly apprehend what Huxley has said about animal automatism) will scarcely allow that animals even possess consciousness.

We propose here to consider some cases in which animals have seemed to reason. The importance of the subject will be recognised if we remember Darwin's admission that had no organic being except man possessed any mental power, or if man's powers had been of a wholly different nature from those of the lower animals, we should never have been able to convince ourselves that our high faculties had been gradually developed. Darwin expresses his belief that there is no fundamental difference of this kind. "We must also admit," he says, "that there is a much wider interval in mental power between one of the lowest fishes, as a lamprey or a lancelet, and one of the higher apes, than between an ape and a man; yet this immense interval is filled up by numberless gradations." But this has not been so generally admitted, despite the evidence advanced by Darwin, as might have been expected. The feeling is still commonly entertained that a distinction exists between the mental qualities of the cleverest ape and the dullest and stupidest savage, which is utterly unlike any that exists among animals. In this essay we shall have to consider cases in which rats, cats, dogs, &c.,--animals all inferior in mental faculties though not all in equal degree, to

the more intelligent apes,--have acted in ways which seem to imply reasoning. We shall treat these cases rather from the point of view of an opponent of Darwin's thesis above quoted than of a supporter, endeavouring in every case to find explanations not involving the exercise of reasoning faculties. But we must admit at the outset, that we find ourselves led to precisely the conclusion which he has indicated.

In the first place, we must recall to our reader's recollection those instances which have been selected by Darwin as so satisfactory, that in his opinion any one not convinced by them would not be convinced by anything that he could add.

Rengger states, says Darwin, "that when he first gave eggs to his monkeys, they smashed them, and thus lost much of their contents; afterwards they gently hit one end against some hard body, and picked off the bits of shell with their fingers. After cutting themselves only once with any sharp tool, they would not touch it again, or would handle it with the greatest care. Lumps of sugar were often given them wrapped up in paper, and Rengger sometimes put a live wasp in the paper, so that in hastily unfolding it they got stung" (the tenderness of some of these students of science towards animals is quite touching). "After this had once happened, they always first held the packet to their ears, to detect any movement within." These were not monkeys of the higher orders, but American monkeys, none of which are so near man in cerebral development as the orang, the chimpanzee, the gibbon, or the gorilla. The next cases relate to the dog, and are important, first, because two independent observers give evidence in the same direction; and secondly, because the action of the dogs can hardly be explained as resulting from the modification instinct. "Mr. Colquhoun winged two wild ducks, whch fell on the opposite sides of a stream; his retriever tried to bring over both at once, but could not succeed; she then, though never before known to ruffle a feather, deliberately killed one, brought over the other, and returned for the dead bird. Colonel Hutchinson relates that two partridges were shot at once, one being killed, the other wounded; the latter ran away, and was caught by the retriever, who on his return came across the dead bird. 'She stopped, evidently greatly puzzled, and after one or two trials, finding she could not take it up without permitting the escape of the winged bird, she considered a moment, then deliberately murdered it' (the winged bird), 'by giving it a severe crunch, and afterwards brought away both together. This was the only known instance of her having wilfully injured any game.' "Here," proceeds Darwin, "we have reasoning though not quite perfect, for the retriever might have brought the wounded bird first, and then returned for the dead one, as in the case of the two wild ducks." If the dog had followed the wiser course, it would not have been quite so clear as in the actual case that he had reasoned, though the pause for consideration after an attempt to take both together, would have gone far to suggest that explanation. But the action of the dog in killing the bird seems quite decisive, because such an act was entirely opposed to the instincts of the breed, and to the train-

ing which retrievers receive.

To these cases Darwin adds the statement that "the mulateers in South America say, 'I will not give you the mule whose step is easiest,' but la mas racional--the one that reasons best"; on which Humboldt has remarked, "this popular expression, dictated by long experience, combats the system of animated machines better, perhaps, than all the arguments of speculative philosophy." Here, although Darwin only quotes Humboldt, he manifestly expresses his own view, and we find him opposed in a very definite manner to the theory of Kepler, afterwards supported by Descartes, and recently advocated by Huxley and others, that animals are automata, not possessing consciousness (or at any rate that this theory is admissible).

The next case to be considered is one which was described a year of two since in Nature. It was not one which in reality demonstrated, or even strongly suggested, the exercise of reasoning faculties by animals. We quote it, however, because it illustrates well the mistakes into which want of care may lead the student of our subject. During the cold weather of last January, the writer of the letter in question put bread on the window-sills of his drawingroom for the benefit of the birds. These, finding food there, were constantly fluttering about the windows. "One day a large water-rat was seen on the window-sill, helping himself to the bread. In order to reach the window he had to climb to a height of about 13 ft.; this he did by the help of a shrub trained against the wall. Neither instinct nor experience," proceeds the correspondent of Nature, "will easily account for his conduct, since he never found food there before. If neither experience nor instinct, what, save reason, led him? His action seems to have been the result of no small observation and reasoning. He seems to have said to himself: I observe the birds are thronging the window all day; they would not be there for naught; it may be they find there something to eat; if so, perhaps, I, too, might find there something I should like. I shall try." The way in which this story is told singularly illustrates the difficulty which we, as speaking animals, find in understanding how a process of reasoning can be carried on without the imagined use of words. Probably few men whose mental powers have been well trained carry on a process of pure ratiocination, without clothing with words the thoughts successively suggested to their minds. It almost seems to a mind thus accustomed to reason with a verbal accompaniment (audible to the mind's ear only) that any mental process not thus accompanied must be to some degree instinctive, and any actions resulting from such a process automatic. But it is certain that even the most intellectual sometimes act in a manner which, if noticed in an animal, would suggest the exercise of reasoning power, not only without putting their thoughts into mental language, but without, in reality, noting what they are doing. However, the point to be specially noticed about the above story is that the narrator overlooks the most obvious, and probably the true, explanation of the rat's behaviour. The rat could not see the food, but most probably he could smell it. If so, his adventuring up the wall to get

it was not the result of reasoning, or, at least, not necessarily so, for that was the shortest path to the much needed food. Possibly the birds themselves may have been an attraction to him. Certainly the case is not one which compels us to believe that water-rats reason.

This objection was so well urged, in company with other points neccessary to be considered in such inquiries, by a German writer, Herr H.T. Finck, that we quote his remarks almost in full. "Before we ascribe to a rat such complicated reasoning powers," Herr Finck remarks, "it is necessary to ask if there is no other simpler way of accounting for the phenomena. I think there is. It is well-known that different species of animals vary greatly in the acuteness of their senses. To man, sight is the most important sense, and the same is true of many other animals and most birds. The cat is a representative of another smaller class of animals, whose most perfect organ of sense if the ear; while the dog lives in a field of sensatives, the most important of which are contributed by the sense of smell." This point, as dogs afford many of the most striking illustrations of reasoning, or of what looks like reasoning, in animals, must be carefully remembered. Few are aware, we believe, how imperfect a sense is sight with all dogs, as compared with our own sense of sight. We believe that there could not be cited a single instance tending to show that a dog has been able to see as well as a very short-sighted man would, while in the great majority of cases, it can be shown by a few easily-tried experiments that dogs scarcely see at all in the true sense of the word. Our sense of smell is probably not more completely inferior to the same sense with dogs, than is their sense of sight to ours. To return, however, to Herr Finck. After pointing out that the rat belongs to the class of animals who are guided by the sense of smell, he says, "It is evident, therefore, that the water-rat in question was led to the window-sill by his nose, which in his case was a more trustworthy guide than his eyes would have been. I do not wish to deny, by any means, that animals have reasoning powers. On the contrary, I am convinced that human and brute intellect differ only in degree, not in kind. But what we have to guard against is not to ascribe [he obviously means the reverse, that we are to guard aganst ascribing] to animals reasoning powers of a higher type than is consistent with the development of their brain, especially when te actions which seem to postulate such powers can be readily accounted for by simply bearing in mind the extraordinary acuteness of one or more of their senses. We are altogether too prone to judge the intellectual life of animals by the human standard--to imagine that the eye is everywhere, as with us, the leading source of knowledge. The neglect of the important role which the sense of smell plays in animal life has been particularly fruitful of errors in philosophical speculation. It has, among other things, helped to give a longer basis of life to the old theory of instinct, regarded as a mysterious power of nature." In passing, we may remark that at the very beginning of our own life the sense of smell is stronger and more useful than the sense of sight; as though during those first few days,

before the eyes acquire power to recognise objects or to do much more than to distinguish light from darkness, we belonged for the time being to that inferior class of animals with whom the predominant sense is that of smell. In that part also, of their lives, human beings seems so far to resemble the lower animals that their actions appear to be governed by instinct solely. In reality, probably, a sense of smell much keener then than during the subsequent years which alone we can remember, governs the actions in the same way, though not so obviously, as sight governs them in most of the actions of later years ..... other abstract, which brutes never acquire; but the boy will as his intelligence develops."

Certainly the next case cited in the correspondence suggests practical rather than abstract reasoning. "In Central Park, one very hot day, my attention," writes Mr. James J. Furniss, of New York, "was drawn to the conduct of an elephant which had been placed in an inclosure in the open air. On the ground was a large heap of newly-mown grass, which the sagacious animal was taking up by the trunkfull, and laying carefully upon his sun-heated back. He continued the operation until his back was completely thatched, when he remained quiet, apparently enjoying the result of his ingenuity. It seems to me that instinct should have prompted the elephant to eat the grass, and that it was reason which caused him to use it for the purpose of diminishing the effect of the sun's rays." Undoubtedly, had hunger been the prevailing sensation at the time, instinct would have caused the elephant to eat the grass. But, as he was probably much troubled by the heat, it was not more wonderful that he should throw grass on to his back, than it would have been if, had there been any shadow, he should have withdrawn under it. Doubtless, however, the true explanation is that the elephant reasoned in a practical way. The effect of the grass as a protection from the heat was obvious to his senses, so he continued to add more and more grass to his covering until he felt comfortable. If the use of the grass for food occurred to him at all, it would have appeared obvious enough that even if all the grass were used for shelter, it would be none the less suitable for food when hunger began to be troublesome.

The next case cited also relates to the apparent exercise of reasoning faculties by rats, and is interesting, because probably their action was guided by the sense of hearing, rather than by that of smell. "Some years ago," says the narrator, "a plumber told me that he had, on several occasions, been called in to examine into the cause of leakage of water-pipes under the flooring of houses, and had found that the rats had gnawed a hole in the leaden pipe to obtain water, and that great numbers of them had made it a common drinking-place, as evidenced by the quantity of dung lying about. The plumber brought me a piece of leaden pipe, about three quarters of an inch in diameter, and one-eighth of an inch in thickness, penetrated in two places, taken by himself from a house on Haverstock-hill. There are the marks of the incisors on the lead as clear as an engraving; and a few hairs and two or three of the rat's whiskers have been pinched

into the metal in the act of gnawing it. This crucial proof of brute intelligence--for a rat will not drink foul water--interested me so much that I ventured to send an account of it to Dr. Charles Darwin, asking his opinion on the means by which the rats ascertained the presence of water in the pipe. To this he replied: 'I cannot doubt about animals reasoning in a practical fashion. The case of the rats is very curious. Do they not hear the water trickling?'" This explanation would go far, it would seem, to do away with the idea that the rats in this case had reasoned, seeing that if they recognised the presence of water by the sense of hearing, their action in biting their way through to what they wanted would correspond precisely with what we have been taught (erroneously, in all probability, but that is a detail) to regard as instinctive. The narrator, however, did not read Dr. Darwin's reply in this sense. "It may be conceded," he says, "that this explanation is the most probable, and if it be the true one, we have an example of an animal using his senses to obtain the data for a process of reasoning leading to conclusions about which he is so certain that he will go to the trouble of cutting through a considerable thickness of lead. Obviously man could do no more under the same conditions." If the rats had shown in their boring operations some special aptitude for securing most conveniently, with the least possible overflow, the water they required, this would be a just inference. But as we know no more than that, having found, probably by the sense of hearing, that water was present in the pipe, no more proof of reasoning power than is afforded by the familiar action of mice in biting their way through the wooden or card casings of boxes of edibles they like, of whose presence within such boxes the sense of smell has convinced them.

. . . . .

THINKING ANIMALS

S., C.; *Nature*, 94:426-427, 1914.

About ten years ago, it became known that "Clever Hans," an Arab stallion owned by a Herr von Osten in Berlin, was able to answer arithmetical and other questions, tapping out the reply with his fore-foot. Notoriety led to heated controversy, and the appointment of committees to investigate. The second of these, under Prof. Stumpf, resulted in Pfungst's book, explaining everything in terms of signals consisting in slight movements made unconsciously by some person present knowing the answer. This seemed to have solved the problem finally until the appearance of Krall's book in 1912. The author, a wealthy jeweller of Elberfeld and friend of von Osten's, had after the latter's death continued to experiment, obtaining results which, he claimed, refuted Pfungst's explanation. This claim found support in a report signed by the zoologists, Kraemer, Sarasin, and Ziegler, assert-

ing that signalling was excluded since correct answers were given even when none of the human participants was visible to the animal. The opinions expressed on Krall's book vary from that of Prof. Dexler--"a shameful blot on German literature," to that of Prof. Ostwald, who foresees that it will "as clearly mark the beginning of a new chapter in the doctrine of man's place in nature as Darwin's chief work did in its day."

As to the problem itself, a definitive solution could result only from a free and impartial testing of the animals; as it is one can only indicate probabilities. Intentional deceit is almost certainly too simplicist an explanation, and is in any case inadequate. On the other hand, the probability of obtaining correct answers by chance has been underestimated in view of the number of unsuccessful attempts and the greater frequency with which certain numbers occur. Very much must be allowed for this and other weaknesses of testimony, the demonstration of which has been one of the successes of applied psychology, but which, as every newspaper now shows, are seldom given weight in practice. They particularly affect some at least of the would-be crucial tests. Nevertheless much remains, of which the following main explanations have been offered.

The answers are evidence of mathematical intelligence. This, although a highly developed "number-sense" has been found in persons of low general ability, and even in the feeble-minded, conflicts with all that we know from other sources about the animal mind. Detailed scrutiny of Krall's account of his teaching shows that the problem often could not have been understood from his exposition. Again, the correcting of a single false figure is done quickly and certainly, as might be expected if signals were being given, since these would be facilitated by concentration of the signaller's attention; if the errors are mistakes of calculation it is odd. Finally, the inability of the animals to prove their understanding by action, compared with their eloquence in the language of taps, is extremely suspicious.

The answers are due to memory. The horse's memory is, no doubt, excellent for some things, and the theory has advantages, but also serious difficulties. To associate eight taps with one symbol and nine with another, the horse must be able to distinguish the two series. But it seems probable that animals cannot distinguish numbers beyond four or five. Rothe trained his dog to come only at the fifth whistle--but this only if the whistles were at regular intervals: his horse would take four lumps of sugar in preference to three, but confused four and five. Again, the horse's eye, while very sensitive to movement, is probably unsuited to the clear perception of complex visual forms such as written numbers, and, as a matter of fact, the animals seem to attend to the questioner more than to the blackboard. Finally, the mistakes in cube root, etc., questions strongly suggest the use of tips.

The animals are responding to unconscious signals. Krall claims to have refuted this by "ignorant" experiments, but these are relatively few and seem all to have some weak spot. Thus Mackenzie reports that Rolf, the Mannheim dog, described a pic-

ture on a card held so that the holder could not see it; unfortunately, the picture was a red and blue cross, and there is reason to think that dogs are nearly colour-blind. Nevertheless, the fair number of "peep-hole" experiments and the case of the blind horse, Berto, seem to stamp as inadequate Pfungst's theory of visually perceived movements. Yet no other one mode of signal seems sufficient for all cases, while Hacker did actually get answers by moving his foot. Again, it is unlikely that the many individuals who have obtained answers should all make precisely the same unconscious movements. These difficulties disappear if we suppose the animals not to be blindly reacting to one specific stimulus, but to be interpreting more or less intelligently a general type of unconscious emotional or ideomotor expression--movement, variation of respiration, etc.--possibly always complex and varying with the individual and occasion. Both horses and dogs are notoriously sensitive to shades of emotional expression, and recent work by the Pawlow school indicates that dogs can hear sounds so faint as the beating of the heart. It is true, any theory of unconscious signalling presents difficulties. Units, tens, etc., are tapped with different feet; the spelling of verbal answers is phonetic, and spontaneous utterances are recorded, including a letter dictated by Rolf! Can the subconscious be credited with so much? The solution, if it ever comes, can scarcely fail to illuminate, if not the animal mind, at least that of man.

• Collective Action

ANIMAL RETRIBUTION

Anonymous; *Popular Science Monthly*, 20:861, 1882.

The Boston papers tell a curious story of the retribution which recently came upon a buck, which, by virtue of his superior strength and sagacity, had exercised a tyrannous lordship over the herd of deer on the Common, and had thereby excited the hatred of the younger bucks. The time came when he had to shed his horns. The other bucks gained knowledge of the fact with a marvelous quickness, gathered around him, made a concerted attack upon him and speedily disabled him, despite the gallant resistance he tried to make. He was knocked down, butted and kicked until his head and sides streamed with blood, shoved this way and that, with all the fury accompanying each action that the pent-up spite of years could render itself capable of, and, finally, was reluctantly compelled to give up the ghost.

Several of the men employed on the Common and public grounds witnessed the affray, and attempts were made to drive off the old fellow's assailants, but it was of no use. Each attempt was resented by the infuriated deer, and every man who entered the inclosure with pacific intentions was obliged to flee for his life. The murder having been consummated, the fury of the animals became appeased, and the dead carcass was removed from the arena.

RATS ATTACK CATS

Anonymous; *Nature*, 225:784, 1970.

A portent unparalleled in the annals of biology has become the talk of Palermo. The Sicilian city is home to over a million rats which have recently begun to dispatch the local cats by "tearing out their throats and nibbling at them." The rats of Palermo, which outnumber the humans by three to one, are thought to consume some 200,000 tons of foodstuffs a year not counting the cats.

COOPERATION AMONG CETACEA

Keller, D. A.; *Marine Observer*, 35:118–119, 1965.

ss. Maidan. Captain D.A. Keller.

20th August 1964 at 2120 GMT. "In thirty years at sea, I have repeatedly seen and watched the antics of porpoises, but until this evening I have never before witnessed them doing anything other than what appears to be playing. Today though, things were different and I record the following for your information and interest. I sighted the school on my starboard bow about two miles away and although many were seen to be jumping the most striking aspect of the scene were the dozens of dorsal fins sticking out of the water with their owners completely motionless. As we drew nearer it was observed that the visible and stationary dorsals belonged to about 50 or 60 porpoises lying half awash. The whole group was formed into a perfect semi-circle with the open side facing the NW. They were spaced no more than 2-3 feet apart, were keeping their positions like soldiers on parade and all appeared to be working their upper jaws as if eating. Across the open side of the semi-circle to the NW were another 50 or more leaping and jumping in the usual manner, but although the ship passed only a quarter of a mile away, the formation held and only two of the 'outer guard' left the school and gambolled alongside us and even they quickly left and returned to their companions. As a ship normally passing so close to a shoal usual-

ly acts like a magnet in drawing them alongside, this disregard was quite remarkable. They were observed in the same posotions until lost sight of some 20 min later. Presumably they were feeding, but there were no signs of fish life and hadn't been all the afternoon.

"Weather at the time was light variable winds, slightly more than a rippled sea very low s'ly swell and heavily overcast. Air and sea temps. at the time were 73°F although the latter had been varying alarmingly between 60° and 80° all day. Perfectly smooth oily patches of sea were observed all round and large amounts of gulf weed in small bunches had been seen all day. Sea birds visible at the time were Wilson's Petrels, Leach's Petrels and Cory's Shearwaters."

Position of ship: 41° 26'N,54° 38'w.

Note. Dr. F.C. Fraser, Keeper of Zoology at the Natural History Museum, was very interested in this observation and sent a copy of the Museum's publication Stranded Whales to enable Captain Keller to give further details.

Captain Keller then wrote:

"After due thought and examination of the illustrations in the booklet, I would say that the beasts forming the motionless semi-circle were almost certainly Blackfish. Their sizes were definitely very much more than the 5-6 ft of ordinary porpoises and could well have been 20 ft. or slightly more. Indeed, had I not seen some of the 'out riders' jumping, I might well have taken them for basking sharks, as their obvious great length plus the marked concavity of their dorsals, had immediately registered that thought in my mind. Their heads were definitely bulbous, as my attention was particularly drawn to them by the steady 'munching' motion of their jaws, but as only the upper portion was visible from an end-on direction, I would not stress this, although I feel positive about it. Pigmentation was almost certainly pure black. As regards the jumping out of the water--it now becomes apparent that I wasn't observing as closely as I might but after reading Dr. Fraser's remarks, the memory picture is that those beasts which were doing the jumping were most definitely not as long as the ones forming the semi-circle."

Dr. Fraser then commented:

"It is clear from Captain Keller's second letter that two different kinds of cetacean were involved, the bigger ones "2-3 ft apart" forming the semi-circle and the smaller ones closing the semi-circle behaving as small dolphins are known to do. The bigger ones seem pretty certainly to have been Blackfish; the smaller ones are not identified. It is interesting to have Captain Keller's information of what appears to have been an interspecific co-operation in feeding. Association between different kinds of cetacean is known to happen, for instance the little cruciger dolphin with the fin whale, and the southern right whale dolphin with another having no common name--Lagenorhynchus obscurus. I am sure there are others but these come to mind."

Dr. Fraser mentions that the upper jaw does not work inde-

pendently of the rest of the head, so that it must have been either the head that was seen to move or the lower jaw.)

THE ENIGMA OF MARINE MAMMAL STRANDINGS

Geraci, Joseph R.; *Oceanus,* 21:38–47, Spring 1978.

The reasons why marine mammals become stranded either singly or in large groups of 200 to 300 or more are as bewildering now as they were when Aristotle recorded such events more than 2,000 years ago. Some current theories take into account common denominators, such as sites of repeated strandings, shoreline configuration, and the social behavior of the species that come ashore. Other theories have a less solid foundation, some bordering on mysticism.

. . . .

Mass Strandings. One of the most informative accounts of a mass stranding is contained in a report by W.K. Fehring and R.S. Wells. Early on the evening of August 19, 1971, three

Mass stranding of short-finned pilot whales on the Florida Gulf Coast in 1971. (William K. Fehring)

short-finned pilot whales, Globicephala macrohynchus, came ashore in less than 3 feet of water along a gently sloping beach in Sarasota, Florida (Gulf Coast). Through the struggling efforts of local residents, the whales were pushed into deeper water and eventually rejoined a large herd 150 yards offshore. At about the same time, six other whales from the herd beached less than a mile south of the Sarasota site. They too were pushed off and rejoined the herd, which slowly moved south, parallel to the coastline. Shortly after dawn the following day, 44 whales stranded, again on a gently sloping beach, 11 miles south of the original site. They were grouped into several pods of from three to seven tightly clustered animals. A large crowd of people gathered, some of whom made several attempts to push the whales off, but the animals immediately stranded again. During these activities, one whale gave birth to a stillborn. It was finally decided to tether some of the larger animals to boats anchored 400 yards offshore. With a minimum of assistance, the stranded herd then left the beach and moved to a point 2 miles offshore. Later in the day, an aquarium exhibitor entered the herd, captured an immature animal, and towed it to shore. The herd then followed slowly and deliberately toward the beach. The whales spread out as they struggled in shallow water, emitting low-intensity whistles. Repeated attempts to return them to sea failed. Finally, observers again tethered the largest animals to boats anchored offshore.

> There was a noticeable change in the behavior of the remainder of the herd. After being pushed seaward, each whale seemed less apt to return, although some still did. Once the majority of the herd was off the beach at the same time, the remainder joined them and all headed diagonally away from the shore. The herd moved slowly southward in three pods with the two largest animals, still trailing tail ropes, leading them about 50 yards to seaward.

A storm prevented further observations, but five days later, 13 pilot whales stranded near the Marquesas Keys, 20 miles west of Key West, and more than 160 miles south-southeast of the original stranding site. Among them was one of the large whales (marked by rope burns around the tail) who had decoyed the herd out on two occasions in the Sarasota strandings. He was successfully returned to deep water along with five others; the remaining whales died on the beach.

Had the whales wandered ashore, or was there an element of intent, suicide perhaps; were they feeding in shallow water and simply ran aground on the outgoing tide; were they harassed or chased by a shark or prankster, or frightened by strange underwater sounds; could they have been misled by errant echo signals from an unfamiliar shoreline, or disoriented by disease or by disturbance in the social order; did they follow an ailing leader to shore; were they travelling an ancient migratory route used by their ancestors, perhaps a land bridge now submerged; or had they reverted to some primitive social behavior that led their

shore-dwelling ancestors to retreat to land when faced with a menacing sea?

. . . .

Recent attention has focused on the role of disease in mass strandings. Just a few months ago, a friend brought a syndicated cartoon strip to my attention: the scientist/author had laid bare the answer to strandings (and his and our naive acceptance of the fragmentary data)--parasites lodge in the middle ear, interfere with echolocation, and the herd comes ashore. His skit had two elements of truth: animals come ashore, and many species have small roundworm parasites cramming the head sinuses and middle ears (that part of the hearing structure in which three small bones transmit vibrations [in terrestrial mammals] from the eardrum to the internal ear). Attempts to tie the two together have been purely hypothetical, and yet the tale has reached a wide audience and gained acceptance among some who study strandings.

. . . .

A New Hypothesis. Certainly, there are flaws in all the theories about strandings that we have thus far examined. In August 1977, during the first full-scale workshop on marine mammal strandings held at the University of Georgia, F.G. Wood, well known for his valuable contributions to cetacean husbandry and behavior attempted to explain all the known facts. In his words:

> It is commonly accepted that cetaceans are descended from land-living ancestors. At some time intermediate ancestral forms must have been amphibious, and during that time it was presumably critically important for them to seek safety on land when injured, afflicted with disease, attacked by aquatic predators, or otherwise subjected to severe stress. There is evidence that organisms under stress may regress, or revert, to fundamental and primitive behavior (e.g., eating, sleeping, sexual). In the case of organisms with a highly developed central nervous system, behavior under stress may be dominated by responses associated with the primitive and very basic motivations and emotions regulated by subcortical systems, instead of following a more rational course under control of the cerebral cortex. The basic drives (hunger, thirst, sex, etc.) and emotions (anger, fear, pleasure) are evolutionarily very conservative, i.e., resistant to change.
>
> It is suggested that the requirement to seek safety on land early in cetacean history became a response that also was mediated by subcortical structures, that the response manifests itself under conditions of stress, of whatever nature, and has persisted to the present time despite the fact that long ago it became maladaptive. No other hypothesis appears to account for as many of the known facts of stranding behavior.

The same arguments that were used against the suicide theory will probably be raised against this hypothesis. Wood recognizes that the trait is maladaptive; a major point of contention. It is

self-destructive, and should have been whittled from the gene pool long ago. In fact, one might argue it was a reversal of this once beneficial trait that made the transition from amphibious to purely aquatic forms possible.

Wood's hypothesis is one of the few concepts, however, that tries to explain the apparent determination with which animals come ashore. If this theory is not accepted, it will be because, like the others, it lacks hard supporting data, which would take years of costly effort to procure.

. . . .

• Unusual Uses of Tools

THE SWALLOWING OF STONES BY SEALS

Lucas, F. A.; *Science*, 20:537-538, 1904.

So far as I am aware no satisfactory reason has been advanced for the swallowing of stones by seals, and this statement may probably be extended to other animals. They are not taken in for ballast, for the empty seals keep down as easily as the others; they are not swallowed for the purpose of grinding up food, for they occur in the stomachs of nursing pups; they are not to allay the irritation caused by parasitic worms, since the two are by no means found together; they are not taken in with food, not merely because they are found in the stomachs of young seals, but in those adults that have fed on squid caught in the open sea. On page 68, Vol. III., 'Report of Fur Seal Investigation,' it says: "It is evident that these things are not swallowed haphazard, but are selected with considerable care from among the articles strewn along the shore, and that a preference is exhibited for rounded objects. This is shown by the fact that, as a rule, only articles of one kind are found in one stomach, although seals do not discriminate between fragments of barnacles and fragments of gastropods. Moreover, pebbles of serpentine and chaleedony are now and then found on the hauling grounds under conditions indicating that they were brought there by the seals, while the pup seen gathering pebbles on Lukanin did so with great care, by no means taking the first that came to hand. The most striking example of this discriminative selection is, perhaps, shown by the pup which had swallowed a buckshot, while the chance of finding such a thing must be, at a guess, about one in a million."

Furthermore, it may be said that as the fur seal regurgitates bones and other indigestible things, the supply of stones must be renewed from time to time.

That there is any connection between the presence of stones and the presence of a gizzard does not follow.

• Fascination or Animal Hypnotism

RABBIT APPARENTLY FASCINATED BY A STOAT

Bond, Henry; *Zoologist*, 18:7273, 1859.

As I was walking on the hill-side above West Creech, Farm, in Penbeck (the down was scattered with very low furze bushes), my attention was arrested by a cry of distress; it proceeded from a rabbit which was cantering round in a ring, with a halting gait. I watched it form some minutes, but as the circle became smaller and the rabbit more agitated I perceived a stoat turning its head with the rabbit's motion, and fixing its gaze upon it. I struck a blow at the stoat and missed it; its attention was thus withdrawn, and the rabbit ran away with great vigour in a straight direction.

FASCINATION

Anonymous; *English Mechanic*, 56:34, 1892.

Mr. J. McNair Wright, an American naturalist, has called attention to a case of "fascination" by a cat which came under his observation. The cat was sitting on the sill of his window near a pine tree, wen a bird alighted on a tree. The cat fixed his eyes on the bird with a peculiar intensity of expression, and the fur on his head stood on end, but otherwise he remained motionless. The bird quivered, trembled, looked fixedly at the cat, and, finally, with a feeble shake of the wings, fell towards the cat, who pounced upon it. Mr. Wright regards such fascination of snakes and other animals as a form of hypnotism.

FASCINATION

Anonymous; *English Mechanic*, 62:269–270, 1895.

Between 50 and 60 years ago, the Chief of Ulwar (not far from Delhi) boasted to the Resident that he could charm the wild antelopes in his domain and compel them to follow him! As he had often made this assertion, he was one day asked to exhibit his powers to a party of Englishmen staying at the Residency. Accordingly, on the appointed day, these visitors and a number of

courtiers, mounted on elephants, went off to the jungles. I think they had some plaintive music playing in advance, and soon the animals assembled in a vast herd. "Now," said the Rajah, "select a fine specimen, and on his horns I will put a ring." Giving one to a dancing girl in his suite, she slowly approached the chosen buck on foot, and put the metal circle on one of his spiral appendages. There was a general shout of applause as she accomplished the promised feat.

The hereditary huntsman at that Court possessed a great secret, which he would not divulge for offers of money. It was this--he could make any kind of dogs attack a tiger, and though many were killed and maimed on each occasion, he always had a fresh pack. The Rajpoot horsemen, armed with spears, turn out a trapped tiger or panther on a plain, and bait him with dogs, giving him the coup de grace with a spear-thrust.

A new book, called "Rifle and Spear with the Rijpoota," by Mrs. Alan Gardner, contains a very interesting account of some wild animals attracted by a conjurer. Mrs. Gardner and her party rode out on elephants to an endless uncultivated plain; the man told them to remain quiet, while he went forward a hundred yards and hid himself in a bush. He then began a muffled chuckling kind of call, which he kept up without ceasing. In about two minutes a fox came out of a little ravine close by, and, looking suspiciously about him, trotted towards the noise. Then came another, and presently two or three more ran in from different directions; soon four or five appeared in the distance, followed by several others; and, finally, two big jackals, quite half a mile away, came galloping up, as if afraid of being too late for the fun. At last, there must have been thirty or forty foxes and jackals clustered like a pack of hounds not a hundred yards from the bush. They all looked frightened, and seemed to come against their will, and the instant the man stopped his chant, every one of the animals fled, as if the spell was broken. The Maharajah of Jeypoor was delighted.

• Singing Mammals

SINGING MOUSE

Farr, John; *Zoologist*, 15:5591, 1857.

Some years ago I remember having heard of a singing-mouse: I fancy it was exhibited somewhere in London; I certainly then had doubts as to the truth of the report, but must confess since then my opinion is greatly changed. Some short time since I met,

at a friend's house, a gentleman who asked me the question, "Did you ever hear a singing-mouse?" I said "No." He answered, "I have one: I will tell you all about it. Two or three months ago I had occasion to go into my game-larder one morning, and was surprised to hear a singular, low, warbling kind of note proceed from the corner of the room; I asked one of my servants, who was there, if he knew what it was: his answer was, 'No, sir; it is something in the wall: we often hear it.' I suggested 'Surely, it might be a singing-mouse,' and it appeared to be in a meal-tub, which I was told was quite full; however, I had the tub moved, and, from underneath it, started a mouse, which escaped by getting into a hole in the wall. That night I had a trap set, and caught the mouse, which proved to be the singing-mouse, for, on being brought into my room, he commenced his warbling, and has continued to do so (of course with intervals of rest) ever since." Last Monday a brother clergyman and myself drove to Captain Mead's, of Ersham Hall, Bungay, Suffolk, to see his mouse, and moreover we heard it sing, that is, upon being stirred up and driven from its nest of wool, it commenced a low, warbling, chirping kind of noise, quite loud enough to be heard across the room; it reminded me much of the note one sometimes hears of the hedgesparrow when singing or commencing to sing in early spring, buried in the ivy of an old fence. The butler informed me that, as the day was cold, the mouse would not sing half so loud as he did sometimes. I must confess I was agreeably surprised; the mouse certainly did what I never heard a mouse do before. Now my impression is, this extraordinary noise is made by the grinding of the teeth, and yet, on looking close, you might observe a heaving of the throat, as if it were a guttural sound, yet I was told he sings particularly when feeding. It strikes me it is not caused from any pleasurable account, for when driven from his nest, and evidently frightened, still the singing continued, but that is just as curious with regard to some of our warblers: if you want to find whereabouts the nest of the reed warbler is situated, a stone or two thrown into the reeds soon causes the male bird to sing, and the nest is sure to be within ten or a dozen yards of the place. But I must conclude, as I am running away from my subject: first, I must just say another mouse has been heard in the same place. In appearance it is, I should say, rather larger than the common mouse; the tail longer, thicker and more blunt at the tip, and the nose less pointed, more snoutish or pig-fashioned.

SINGING MICE

Hiskey, W. O.; *American Naturalist,* 5:171–172, 1871.

A communication in the Naturalist some time ago in regard to musical mice, prepared me for a phenomenon which recently came under my observation, which otherwise would have astonished me

beyond conception. I was sitting a few evenings since, not far from a half-open closet door, when I was startled by a sound issuing from the closet, of such marvellous beauty that I at once asked my wife how Bobbie Burns (our canary) had found his way into the closet, and what could start him to singing such a queer and sweet song in the dark. I procured a light and found it to be a mouse! He had filled an over-shoe from a basket of pop-corn which had been popped and placed in the closet in the morning. Whether this rare collection of food inspired him with song I know not, but I had not the heart to disturb his corn, hoping to hear from him again. Last night his song was renewed. I approached with a subdued light and with great caution, and had the pleasure of seeing him sitting among his corn and singing his beautiful solo. I observed him without interruption for ten minutes, not over four feet from him. His song was not a chirp, but a continuous song of musical tone, a kind of to-wit-to-wee-woo-woo-wee-woo, quite varied in pitch. While observing him I took for granted that he was the common house-mouse (Mus musculus), but when he sprang from the shoe to make his escape he appeared like the prairie mouse (Hesperomys Michiganensis), a species I had not, however, observed before indoors. I have thus far failed to secure this little rodent musician, but shall continue to do all I can in the way of pop-corn to entertain him, and if his marvellous voice gives him the preeminence in mousedom which he deserves, by the aid of Natural Selection I shall presently have a chorus of mice, in which case you shall receive their first visit.

MUSICAL MICE

Lockwood, Samuel; *Popular Science Monthly*, 1:323-327, 1872.

That our little cosmopolite, the Old-World mouse, whom Linnaeus, on account of its smallness among its fellows, named Mus musculus, has achieved some distinction in the musical line, almost everybody knows. Indeed, these musical house-mice are almost ceasing to be uncommon. Even his less graceful, big relative, the rat, has tried his hand at the pipes, and not wholly without success. And, among these little erratics, some have been known that might be called more comical than entertaining---certain eccentrics, known as hiccoughing-mice. But these and the above are all, wherever found, directly or indirectly, of the Old-World race. That any New-World species had done aught of this sort was to naturalists unknown. A late friend of ours had a domestic mouse---"a singer, that is," as the old man said---"not much, but it would whistle a little---chirrup, you know." Now, it happened that, one day, our friend caught two wood-mice, real natives---delicate, white-footed things, that looked too innocent to do any thing else than step mincingly around in their delicate white-satin slippers. So they were put into the cage with the singing-mouse. Whether, like some other folks, they had no ap-

preciation of foreign airs, we have no means of answering; but alas! in spite of their silken ways, they at once set upon and murdered the little musical mouse.

These wood-mice are often called white-footed mice. They belong to a genus of the Sigmadontes, known as the Hesperomys, or Vesper-mice, and are indigenous to this our Western Continent. There is a number of species in the genus; but those best known are diminutive things, not so large as the house-mouse, their sides are yellowish-brown, the back considerably darker, the abdomen and feet almost snowy-white. Their home is the woods. With but little sympathy for man, they will occasionally intrude for a time into his dwelling, when, as I believe, the domestic mouse withdraws. My friend Philip J. Ryall, Esq., in the spring of 1871, when at his Florida home, near St. Augustine, was disturbed, at night, by what he supposed to be the chirping of birds in the chimney. The mystery was cleared up in an unexpected way. A very small mouse came up from a crevice in the sitting-room floor. Here it sat up on its hind-feet, and looked around with the utmost confidence, all the time singing in a low, soft, yet really warbling style. This visit became a daily business, until it paid the penalty of its temerity by being captured. About a month after, this prodigy was intrusted to the custody of the writer. Of course, it came introduced as a "singing house-mouse." What was our astonishment at recognizing, in the little stranger, a true Hesperomys, and no house-mouse at all! It was one of the wood-mice, and among the smallest of the species. It is a female, and fully grown, yet not so large as a domestic mouse. Every pains was taken to secure the comfort and well-being of my little guest.

And what an ample reward I reaped! For a considerable time she carolled almost incessantly, except when she slept. Day and night she rollicked in tiny song, her best performances being usually at night. To me it was often a strange delight, when, having wrought into the late hours, and the weary brain had become so needful and yet so repellant of sleep, I lay down, and gave myself up to listening to this wee songster, whose little cage I had set on a chair by my bedside. To be sure, it was a low, very low, sweet voice. But there was, with a singular weirdness, something so sweetly merry, that I would listen on, and on, until I would fall asleep in the lullaby of my wingless and quadrupedal bob-o'-link. The cage had a revolving cylinder or wheel, such as tame squirrels have. In this it would run for many minutes at a time, singing at its utmost strength. This revolving cage, although ample as regards room, was not over three and a half inches long, and two and a half inches wide. Although I have now been entertained by these pretty little melodies for a year, yet I would not dare redescribe them. In the American Naturalist, for December, 1871, the music is given with that elaboration which was possible under impressions so novel and delightful. She had two especially notable performances. I called these roles---one the wheel-song, because it was usually sung while in the revolving cylinder, and the other the grand role. A remarkable fact in the latter is the scope of the little creature's musical powers.

Her soft, clear voice falls an octave with all the precision possible; then, at its wind-up, it rises again into a very quick trill on C sharp and D.

I must quote from the above a paragraph entire. Let me simply premise that in our household this little creature goes by the pet name "Hespie."

> "Though it be at the risk of taxing belief, yet I must in duty record one of Hespie's most remarkable performances. She was gambolling in the large compartment of her cage, in intense animal enjoyment. She had just woke from a long sleep, and had eaten of some favorite food, when she burst into a fulness of song very rich in its variety. While running and jumping, she carolled off, what I have called her grand role; then, sitting, she went over it again, ringing out the strangest diversity of changes, by an almost whimsical transposition of the bars of the melody; then, without, for even an instant, stopping the music, she leaped into the wheel, sent it revolving at its highest speed, and, while thus running in the wheel, she went through the wheel-song in exquisite style, giving several repetitions of it. After this, without at all arresting the singing, she returned to the large compartment, sat upright, resumed again the grand role, and put into it some variations of execution which astonished me. One measure, I remember, was so silvery and soft that I said, to a lady who was listening, that a canary able to execute that would be worth a hundred dollars. I occasionally detected what I am utterly unable to explain---a literal dual sound (a rollicking chuckling), very like a boy, whistling as he runs, drawing a stick along the pickets of a fence. So the music went on, as I listened, watch in hand, until actually nine minutes had elapsed! Now, the wonderful fact is, that the rest between the roles was never much more than for a second of time; and, during all this singing, the muscles could be seen in vigorous action, through the entire length of the abdomen. This feat would be impossible to a professional singer; and the nearest to it that I have heard was the singing of a wild mocking-bird in a grove."

The point which I think I have demonstrated elsewhere in this matter is, the invalidity of the position taken by some, that the singing faculty of these little creatures is due to a diseased condition. The specimen above dwelt on has been for a whole year at least in perfect health. It now appears, from a late number of the Naturalist, that a gentleman in Maryland amused himself in breeding white mice, in the hope of raising a singer. After raising several hundred, he procured one that manifested a little musical ability. It sang in six months about half a dozen times. He says that it is in perfect health, and that its offspring are the largest and the finest and that it is an amiable, playful little pet. This was a domestic mouse, and at best but a very moderate singer. But Hespie differs in all respects. She is the wild wood-mouse, and an incessant singer, and one of very remarkable parts in musical ability. She has also many interesting differences per-

taining to habits and food. Cheese is not relished by her; but insects and grass are choice morsels. Her greatest luxuries are worms, and maggots out of nuts and fruit. She will take an earthworm into her little hands, and, holding it up to her mouth at one end, will cause it to gradually shorten and disappear, as some bipeds from Faderland might dispatch a favorite sausage. Her agility in catching flies is wonderful; she leaps at the object, and rarely misses a catch.

. . . . .

A SINGING MARMOT

Anonymous; *Popular Science Monthly,* 1:509-510, 1872.

Dr. Lockwood's theory of the latent singing capacity of the Rodents has received an interesting item of confirmation from an article in the June number of the American Naturalist, by Dr. R.A. Kellogg, librarian of the Academy of Natural Sciences, San Francisco. He says that Dr. Lockwood's recent articles call out again a statement of his having a young Maryland marmot or woodchuck, when a boy, that "sung like a canary-bird, but in a softer, sweeter note." His impression was, that it was a female. "I used to watch the pet very closely to see how it sang, as children are apt to do. There was a slight motion of the nostrils and lips, and consequently of the whiskers, with an air of unmistakably happy or serene enjoyment."

SINGING WHALES

Anonymous; *Nature,* 224:217, 1969.

Strange cases of animal communication continue to be discovered. One of the highspots of this year's International Ethological Conference, held at Rennes in France from September 1 to 10, was Roger Payne's account of the "song" of the hump-backed whale. Payne, from Rockefeller University, New York, has been analysing deep sea recordings made in the vicinity of Bermuda during the annual spring migration of these animals, and has found that the whales not only produce many different squeals, squeaks and groans, ranging from ultrasonic to very low frequency sounds, but also that these sounds are arranged into a highly complicated "song". The song of the whale is in many ways like a bird song, but differs in that it lasts not for a few seconds, but for eight or nine minutes. At the end of this time, the whale starts again at the beginning of the song, and spectral analysis has shown that the sequence of sounds is repeated almost exactly each time.

Payne believes that the singing whales are communicating with each other, and suggests that they may do so at distances as great as tens or even hundreds of kilometres. The singing of the whales has been picked up by hydrophones between 2,000 and 4,000 feet under the surface, where there is a layer of water so constituted that sounds spread within it in two dimensions rather than three. This has the effect of carrying sound over enormous distances, and by means of this "sound channel" it is theoretically possible that one hump-backed whale could hear the singing of another hundreds of kilometres away.

• Hedgehog Idiosyncracies

THE CURIOUS CASE OF THE CONCENTRIC RUNNER

Anonymous; *New Scientist,* 36:743, 1967.

Cambridge is a place where almost anything can happen around midnight, especially in June, but the sight of the animal running round and round the Master's garden night after night was reckoned peculiar, even by Cambridge standards. It was brought to the attention of the Reverend J. S. Boys Smith, Master of St. John's College. For a month he, too, became an ardent after-dark hedgehog-watcher---in his own garden. And then the animal disappeared.

Reporting all this with fine academic detachment in the current Journal of Zoology, the Master says that apart from its behaviour the animal appeared to be a normal, alert specimen of Erinaceus europaeus, the common European hedgehog. It was first noticed on 6 May, shortly after midnight. Thereafter, as soon as darkness fell it left the secluded southern part of his garden where it spent the day and ran round the northern portion which is more public, exposed to the noise of traffic and illuminated by street lamps. The animal invariably ran in an anti-clockwise direction. Widdershins? The circle varried to some extent. Normally it was about 15 yards in diameter which it accomplished in about 20 seconds, that is at a speed of about 4.5 mile/h. Typically, there was a steady rapid trot round and round, snout held forward well above the ground. Starting at dusk it kept this up for stretches of two hours or even longer and was seen until half past three in the morning.

On several occasions the Master walked into the centre of the circle while the animal was trotting round. It sometimes paused as it passed on his down-wind side, sniffing the air, but if he remained still it lost interest and resumed its concentric noctambu-

lations.

The Master fed it with bread and milk which it ate until it was apparently too gorged to run far or fast. But after what seems to have been the Erinaceid equivalent of an after dinner nap it resumed its running. It kept this up for a month and then was seen no more. The master's explanation? The Master has no explanation.

HEDGEHOGS WIDDERSHINS

Wainwright, B. H.; *New Scientist,* 37:322, 1968.

I believe the lodger in the Marten of St. John's garden (New Scientist 28 December, 1967 p. 743) to be no exhibitionist trend setter in the animal kingdom but a follower of hedge-pig fashion.

On a hike in Caernarvonshire in the summer of 1952 I myself observed the same phenomenon---in a pasture in broad daylight. The hedgehog ran left handed in a circle perhaps 10 yards across through the short meadow grass. Now and then he stopped momentarily as if cocking an ear to something in the ground. I cautiously approached the circle taking a step each time he was at the far side until he was lapping about two feet from the toe of my boot but stopping more often as he came close to me to sniff the air. I watched for half an hour before I had to resume my journey and I had not the heart to interrupt the game.

Seriously, we should ask the zoologists to explain this behaviour if they can.

PUZZLING HABIT OF THE HEDGEHOG

Burton, Maurice; *New Scientist,* 4:1071-1073, 1958.

The idea that an animal may do something merely because it likes doing so is foreign to present-day biological thought. Nevertheless, there are certain lines of conduct that come perilously near to this. One of the most outstanding of these is the self-anointing of hedgehogs. This was first recorded as recently as 1912, by Ludwig Heck, who named it selbstbespuchen. Since the German equivalent is clumsy when rendered into English, I have proposed "self-anointing." Since 1912 there have been only three or four further records of this remarkable behaviour, and with one exception these have been in the German literature. Judging solely from this, it was natural to assume that the behavior was abnormal or at least unusual. Subsequent experience has shown me that it is widespread among hedgehogs and frequently

indulged.

A typical display of self-anointing is as follows. The animal starts to lick a substance and continues to do so for perhaps half-a-minute. During this time a copious foam, presumably of saliva, appears on the lips. Then, the hedgehog raises itself to the full on its front legs, and turns its head to one side in what appears to be a severe muscular effort, the apparent convulsion being communicated to the rest of the body. Then it turns the head to the rear and transfers the foam to some of the spines by vigorous flicking action of the tongue. Following this, the animal returns to the normal posture, once more licks for up to half-a-minute, and repeats the process, either on the same side of the body or on the opposite side. While the foam is being transferred to the spines, the skin of his back tends to be thrown into corrugations, and, such be the vigour of the action, the hedgehog may topple over on one side and lie apparently paralysed, its limbs rigid and the body contorted. In due course the body is bedecked with patches of foam, mainly on the spines but sometimes on the hairy flanks also.

. . . . .

A wide variety of substances are known to have induced self-anointing. They include leather, polished or raw varnished wood, cigar-ends, a clean handkerchief, a cigarette packet, the skin of a toad, bare earth, various animal droppings, soot, slugs, earthworms, porcelain tiles, human sweat, well-cleaned human skin, a wool rug, various vegetable substances cooked or raw, cheese, tap water and even distilled water. This list is not complete, and the full range suggests that almost anything may be calculated to trigger off the behaviour, provided the animal is in the mood. At the same time, there have been individuals which have had favourite substances and have shown every sign of seeking these out specially to use them as stimulants. Others have favoured circumstances, like the young hedgehog which, after having been fed, was habitually picked up by its owner and cradled in her hands. It was noticed that this young animal would always self-anoint when so cradled after a meal, licking its owner's fingers to do so. Another, also young and tame, which would come at its owner's call, greeting her by licking her shoes and self-anointing.

. . . . .

HEDGEHOGS USE TOAD VENOM IN THEIR OWN DEFENSE

Brodie, Edmund D., Jr.; *Nature*, 268:627-628, 1977.

I report here that toxic secretions evolved in prey organisms such as toads (Bufo) as chemical anti-predator mechanisms are used by hedgehogs (Insectivora, Erinaceidae) to enhance their

own mechanical anti-predator adaptations---the spines, modified hairs, of the back. The secretions are taken into the mouth and licked on to the spines. In spite of interest in chemical, mechanical and behavioural anti-predator adaptions of both invertebrates and vertebrates there have been no reports of the behavioural utilization of the anti-predator secretions of one species by another for its own defense. Toad skin is one of many irritating substances used by hedgehogs in self-anointing, and the presence of any of these, fresh or dried, on the spines, would probably increase the pain or potential of infection to the would-be predator.

. . . . .

• Rodent Kings

KING OF THE RATS

Ley, Willy; *On Earth and In the Sky,* Ace Books, New York, 1957, pp. 27-33.

A Rat King is not a separate species. One is tempted to say "on the contrary," for it consists of a number of rats which cannot be separated.

The average number of rats forming a "king" is twelve, but larger and smaller numbers have been found. The tails of the animals are intertwined in such a manner that the individuals cannot pull free. The whole thing is about two feet in diameter. All the heads point outward, naturally, since the tails are stuck together. Being handicapped in this manner, the animals are not very mobile and in a few cases a dead and dried-out "king" has been found. Evidently the rats had starved to death. But in the majority of the cases the "king" when found was still alive. It often was found because the members composing it were squealing with hunger.

. . . .

During December, 1822, two Rat Kings were found by three farm hands engaged in threshing grain in the barn of the local forester. The place was the hamlet of Dollstedt, not quite ten miles from the city of Gotha. While doing their work, the men heard the squealing of rats and started a search, aided by the forester's helper. They finally found that one of the main cross beams had a six-inch-deep hollow in its topside, evidently one that the rats had gnawed. They saw that this hollow was full of rats, and they were somewhat surprised that the rats made no at-

tempt to run away.

The men pushed the rats out, and saw that one was a Rat King of twenty-eight rats and the other one, one of fourteen rats. Both sets were described as consisting of fully grown rats, quite clean but also very hungry. Strangely enough they are also described as having been very docile, which probably means that the rats were in an advanced state of weakness from hunger.

After the whole village had marveled at the find, the public spectacle ended by the execution of both Rat Kings, the execution being carried out with the flails. Then two of the men took pitchforks and tried to pry the bodies of the smaller assembly apart. After much effort they succeeded in pulling out a few of the rats. The forester himself looked at them and wrote down that the tails had not been torn off in the process and even had their skin, but they "showed the impressions of the other tails, just like leather straps that had been plaited together for a long time."

A later case has the advantage of having been examined by a man with scientific knowledge. The Rat King was found on February 2, 1880 by a drayman in a room of the abattoir of the city of Dusseldorf. It was a room which was used for the storage of skins and which was, for this reason, not much frequented by people. The Rat King consisted of eight black rats, of which one was killed when it was discovered. The other seven lived for some time. A Mr. Wilhelm Deckers acquired it and went to a taxidermist to have it preserved. The taxidermist, naturally, cleaned it up so that one cannot tell whether the intertwined tails were also held together by anything that might act like glue.

After Mr. Deckers died, the specimen was donated to the local High School and examined by the school's science teacher, Prof. Ahrend. Ahrend wrote that "Guntermann [the taxidermist] had cleaned the tails of the animals carefully so that only their intertwined positions were preserved . . . but just looking at the intertwined tails makes it clear that even the smallest amount of a sticky substance would render the animals inseparable."

The latest case which I could find in a quick survey was reported by the German zoologist Dr. Ludwig Heck, who later became the director of the Zoological Garden in Berlin. It refers to a Rat King found in January, 1907, in the village of Capelle, near Hamm in Westphalia. The local pastor, Wigger by name, informed the zoologist Reeker, who tried to acquire it for the Provincial Museum of Westphalia. But before Reeker could move, the Zoological Institute of the University of Gottingen had secured it for its own collection.

It consisted of ten specimens of the black rat. The director of the Zoological Institute, Privy Councillor Ehlers, wrote to Dr. Heck in reply to a question that he could not explain how such a thing could have happened and that the rat's tails do not show any visible pathological changes. And he added that the Institute's taxidermist considered it impossible that this Rat King might have been made by some pranksters.

Well, this is the story.

All the cases have two things in common. It is always the black Mus rattus which produces a Rat King, and they have all

been found during the coldest time of the year, in weather which would make the animals huddle together for warmth.

THREE INSTANCES OF TANGLED SQUIRREL TAILS

McClung, Robert M.; *Animal Kingdom,* 55:46-47, 1952.

Animals in the wild often become imprisoned by various types of natural traps or snares. Spider webs are a good example of these, with insects, of course, as their primary victims. However, small birds, mammals and even snakes have been known to be trapped by them. The water-filled, flask-shaped leaves of the Pitcher Plant is another example of a natural insect trap. The oozing gum or sticky sap of various plants are often effective traps for insects, and there are many instances of small animals having been caught by burrs or impaled on thorns. But until lately we had never heard of such strong and active creatures as squirrels being trapped by having their tails knit together by natural causes. Not one, but three, instances of this peculiar accident have recently come to the attention of the Zoological Park staff.

The latest occurred on the morning of December 31 when one of our keepers noticed seven Gray Squirrels huddled closely together on the ground at the north end of the Zoological Park. Their bodies extended in different directions and they seemed to be bound by their tails so tightly that any coordinated movement was impossible. They were netted and taken to the Animal Hospital, where the photograph that accompanies this note was made by an amateur photographer.

Close examination at the Hospital revealed that the seven tails were tightly tangled and knotted together like twisted strands of twine. Individual hairs were matted and interwoven to such an extent that a great deal of fur had to be cut away in order to separate the animals.

All seven squirrels were adults. Two of them, both females, were dead when they were picked up and another was so weak that it died the same day. The other four, once released, were able to scamper away under their own power. They were slightly wobbly and their tails were ragged, but otherwise they seemed little the worse.

From their general condition, it would seem that they had been tied together for several days.

We have no way of knowing actually how the squirrels became entangled, but a possible explanation comes to mind. Many trees in the Zoological Park contain bulky leaf nests or holes in the trunk which are inhabited by Gray Squirrels. Often several individuals live in the same nest, sometimes an adult female with several of her grown-up young of the preceding summer. The nest is their home base, used for sleeping and for shelter during bad weather. Presumably these seven all lived in the

same nest and their tails became entangled as they curled up together and stirred and burrowed under each other for additional warmth and shelter.

It may be significant that the week preceding the discovery of the seven was damp, with much rain and some snow. Possibly the squirrels returned from foraging for food with their tails wet, which might make matting easier.

A similar occurrence in the Zoological Park was reported early in December by Head Keeper Scott of the Bird Department, who found five Gray Squirrels tail-tied on the lawn in front of his apartment in the Park. He separated them and they ran off with their tails considerably frazzled.

These two instances within a few weeks of each other reminded Staff Photographer Dunton that in late March, 1948, while he and Brayton Eddy, our late Curator of Reptiles and Insects, were collecting near Switzerland, South Carolina, they found three squirrels whose tails were not only matted together but were additionally tangled with Spanish moss. The moss is used by squirrels of that area in the building of their nests and would, of course, be an excellent contributory factor in tail-tangling. Here, again, the weather had been very damp in the period before the squirrels were found.

• The Nightly Resurrection

A "NIGHTLY RESURRECTION"

Bonavia, E.; *Nature*, 20:505, 1879.

Yesterday, in the Pall Mall Budget of July 11, 1879, p. 22, in a review of Mr. Stevenson's---"Travels with a Donkey in the Cevennes," I read the following, which is an extract of Mr. Stevenson's book. It is a very interesting observation. He slept a good deal under trees at night, and he says: "And there is one stirring hour unknown to those who dwell in houses, when a wakeful influence goes abroad, and all the out-door world (meaning animals and men who sleep in the open) are on their feet. It is then that the cock first crows....Cattle awake in the meadows, sheep break their fast on dewy hill-sides, and change to a new lair among the ferns; and houseless men, who have lain down with the fowls, open their dim eyes and behold the beauty of the night ....Even shepherds and old country folk, who are the deepest read in these arcana, have not a guess as to the means or purpose of this nightly resurrection. Towards two in the morning they declare the thing takes place, and neither know nor inquire further."

This is a very curious and interesting fact, but Mr. Stevenson is mistaken when he states that this "stirring hour," when a wakeful influence goes abroad," between the hours of one and two in the morning, is unknown to those who dwell in houses. I have been aware of it for a long time, and have noticed it year after year on myself, although I dwell in a house. In the winter I usually go to sleep at 9 P.M., and then feel cold and require a good deal of bed covering to keep me warm; but between one and two in the morning I feel uncomfortable, wake, and feel hot, and am obliged to throw off some of the bed clothes. Afterwards this discomfort passes away, I pull over me the blankets again, and go to sleep till daylight. This occurs morning after morning as regularly as possible.

In the summer I awake as regularly as possible about the same hour, and feel uneasy and toss about for some little time, although at this season no blankets are used, and then go to sleep again.

Since I have been at Fyzabad I have been able to test more accurately the hour in which this wakeful influence begins to occur. I used to awake at the usual hour, and while awake I invariably heard the railway whistle of the train which leaves for Lucknow at 12.50 A.M. Latterly I have not been noticing this whistle, and I am not aware that I wake at that hour, but there has been and is plenty of rain during this rainy season, saturating the soil and atmosphere with moisture. Probably this moisture may prevent that subtle "wakeful influence" from reaching the nervous system. Again, I am rather subject to an occasional neuralgic pain on the left side of my forehead. When this occurs at night, it goes on increasing to its maximum between one and two o'clock in the morning, and afterwards it begins to subside. I often suspected that some change in the terrestrial magnetism some time after the passage of the sun across the meridian, on the other side of the earth, may be the cause of this "subtle influence." Perhaps those who take observations on terrestrial magnetism may throw some light on this subject. Whatever may be this "subtle influence" which acts on the nervous system of animals between one and two o'clock A.M., there is a similar influence in the day, between one and two P.M., although it may not have been noticed. I have observed it, because when I get the before-named neuralgic pain in the day, it goes on increasing till between one and two o'clock P.M., when it begins to subside. This question arises: are the periodical exacerbations in fever and neuralgias, & c., due to some similar cosmical influence? Statistics on these points are worth collecting. It is natural to suppose that the nervous system of animals--a most sensitive tissue--would be readily influenced by any magnetic change of the earth, or by other subtle cosmical influence.

• Gorillas and Man

THE GORILLA AND MAN

Phillipps, Tracy; *Man*, 28:56, 1928.

Referring to a letter of mine, dated 15th February, to The Times, concerning the carnivorous habits of some gorillas: there has been some hesitation in England in accepting the statements of du Chaillus, so many of which have, however, since been verified, regarding the attraction of the human female for the larger anthropoids.

It may be of interest to record the fact that a muHutu woman living at the foot of the Birunga Mufumbiro mountains was seized, violated and killed by gorilla in recent years. Her "father" was alive in 1919 and described the incident to me. It took me a year and a knowledge of the local language to elicit the facts.

In 1920, a muHutu woman was dragged off in Bufumbira by a male gorilla, of a family which had just been stoned while raiding a banana plantation. He only dropped her about half a mile from the village when closely pursued.

I took down the details from the woman herself and recorded her name and family. The baTwa pygmies, who live a normal existence on the mountains, were able, after months of hesitation, to recount other specific cases within their knowledge.

Incidentally, in 1920, the professional tanners of the tribe refused to clean a gorilla skin for the purpose of curing it, on the ground that, unlike the (other) apes, he was "a human, though of an abominable kind."

It should perhaps be recollected that the Birunga range has been erupted in such a way as to form a bridge athwart the Great Divide or Albertine Rift. The gorilla (beringeri) has presumably crossed eastward upon it from the steamy forests of the Congo to the intensely cold end of the promontory which juts out into Eastern Africa under the names of Sabinio-Muhavura. There, in the totally different conditions and surroundings, it is not surprising that these few families should have developed very distinct physical and temperamental characteristics.

The primeval tangle of the equatorial forest gives way to clear tree bamboos, and temperate and even alpine flora.

Here the gorilla is emerging gradually from the primeval forest into more open country. In the woods he may frequently be seen moving in the semi-erect position from glade to glade. His tropical food is almost unobtainable. It will perhaps be easier to visualise the environment when it is realised that my first knowledge of the existence of gorilla here was the sight of their footprints at 13,000 feet on fleeting snow.

Here then he has begun to develop at certain seasons a dawning taste for warm flesh. The family is visibly tending to stabil-

ise itself as such, with an apparent tendency towards the social organisation of humanity.

It may be noted incidentally that in parts, of the baNgala country of the Congo Basin there is an almost standing feud between the men of the tribe and the gorilla on account of the attacks of the latter upon their women.

• Chemical Transmission of Intelligence

TRANSFER OF A RESPONSE TO NAIVE RATS BY INJECTION OF RIBONUCLEIC ACID EXTRACTED FROM TRAINED RATS

Babich, Frank R., et al; *Science*, 149:656-657, 1965.

Abstract. Rats were trained in a Skinner box to approach the food cup when a distinct click was sounded. Ribonucleic acid was extracted from the brains of these rats and injected into untrained rats. The untrained rats then manifested a significant tendency (as compared with controls) to approach the food cup when the click, unaccompanied by food, was presented.

TRANSFER OF ACQUIRED INFORMATION BY BRAIN EXTRACTS

Ungar, G., and Irwin, L. N.; *Nature*, 214:453-455, 1967.

We have previously found that mice injected with extracts of brain taken from rats habituated to a sound show a significant loss of their startle responses when submitted to the same stimulus. We afterwards showed that, this transfer of habituation is stimulus-specific: recipients given extracts of the brains of rats habituated to sound were habituated only to sound and responded normally to air puff; those injected with extracts from donors habituated to air puff had significantly decreased responses to air puff but not to sound.

In other types of experiments, attempts were made to transfer conditioned avoidance and escape training. In both cases the recipients of brain extracts from trained animals performed significantly better than the control animals which received extracts of untrained brains.

It seemed, however, that increased rate of learning was not an entirely satisfactory criterion for an actual transfer of information.

Brains of trained animals may contain factors which could stimulate learning without conveying any specific information to the recipients. Experiments have therefore been devised in which changes in the behaviour of the recipients could be attributed only to the information contained in the brain extracts. This was accomplished by testing the recipients without any reinforcement and comparing their performances before and after administration of the brain material taken from the donors. [Experimental details omitted.]

The present experiments indicate that the success of transfer depends on variables that are still poorly understood. Experiments under similar conditions may give quantitatively different responses, as for example the third group in Table 2 and the third group in Table 3 in which recipient bias was not controlled. [Tables omitted.] The present experiments, however, indicate that there is an optimum amount of brain to be administered and that the training of the donors has to be of the appropriate duration. Furthermore, a significant positive correlation has been found between the number of errors made by the donors in the course of training and the percentage of correct turns made by the recipients: brains of slow learners gave consistently better results than those of fast learning donors. Recipient bias was shown to be a critical factor in transfer; other conditions concerning the selection of recipients are yet to be explored.

Lack of information on the variables involved in transfer has been a factor in the controversy over the transfer of learning induced by RNA. In this case, however, the main source of confusion is probably the chemical nature of the transfer material. The factors operative in the present experiments, as well as in our previous work, have consistently been found to be peptides or proteins, not nucleic acids. Their further characterization and isolation are in progress.

• Possible Inheritance of Acquired Characteristics

ARE THE EFFECTS OF LONG-CONTINUED ROTATION IN RATS INHERITED?

Detlefsen, J. A.; *American Philosophical Society, Proceedings,* 62:292-300, 1923.

I have been asked to report briefly and yet critically on the present status of those unique experiments on long-continued rotation begun by the phychologists, Dr. Bentley and Dr. Griffith. Perhaps my only justification in presenting, among other data, the

data accumulated largely by Dr. Griffith, lies in the fact that both Dr. Griffith and Dr. Bentley have thought important zoological and genetic implications were involved in their experiments, and because they have been kind enough to speak of me as cooperating in these experiments since they were housed in my laboratory, where I had ample opportunity to follow them at first hand. Furthermore, it has been possible for me to make additional observations at the Wistar Institute.

Every one is familiar with the sensation of dizziness that usually results from a rapid spinning upon one's heels, or an experience in the merry-go-round, or even with the revolving chair or turntable of the laboratory. Associated with this is a twitching back and forth of the eyes and oscillatory movements of the head to which we apply the term nystagmus. Such responses are really compensatory adjustments, the object of which is to retain the same field of vision and original bodily position which the subject had prior to the application of the stimulus. Thus in a rat rotated clockwise or to its right, say ten turns in twenty seconds, on the turntable in a horizontal plane around a vertical axis, the two eyes make coordinated movements in a direction opposite to rotation during rotation, slowly moving to the left and snapping back to normal, while the head swings to the left---i.e., versus rotation. If the rotation is then suddenly arrested, the head swings to the right or in the direction of former rotation, while the eyes show the slow compensatory movement to the right with the quick return movement to the left---to which phenomenon we apply the term "after-nystagmus." This latter group of compensatory adjustments in after-nystagmus is therefore, in general, the opposite of the former adjustments of rotational nystagmus. A similar set of sequelae, but opposite in direction, would accompany rotation to the left or counter-clockwise. All such readjustments which an animal tends to make to disturbances of equilibrium are in part dependent upon speed and duration of rotation, negative and positive acceleration, bodily condition of the animal, and finally upon the amount of rotation which the animal has previously endured. Indeed, if the stimulus is intense, the bodily manifestations may be profound and involve pronounced muscular, glandular, respiratory, and vascular responses.

This fact, that after-nystagmus persists for some seconds after rotation, makes it possible to measure the response to a given unit of stimulus in terms of time. Thus the rotation of a rat on a turntable or cyclostat, when the unit stimulus is a rate of twenty revolutions in ten seconds, produces an after-nystagmus in which the eyes oscillate for about 9.5 seconds. If the position of the rat and speed are constant, then the results of a given series of such unit stimuli applied at one time and under one set of conditions show much uniformity---varying perhaps only a second on either side of a mean. However, while the results of a series of cyclostat tests given today may be fairly uniform in themselves, they may as a whole differ from a similar uniform set of cyclostat tests administered tomorrow. Certain causes, obscure as yet (perhaps such as temperature, hunger, and excitement), may modify the averages of given sets of tests, although the results within a

given set show considerable uniformity. In any event, it is of some value to be able to measure the response and know the ranges under various conditions, for large differences due to differential treatment become significant. Throughout my discussion the unit of stimulus to call forth a measurable after-nystagmus will be understood to be twenty rotations in ten seconds.

During the late war, when men were taking to the air in far greater numbers than ever before, it was necessary to gain more knowledge of the mechanics of equilibration, concerning which there was much divergence of opinion, particularly relative to a long-continued and repeated stimulus such as rotation. In an attempt to solve some of these problems an experiment was devised for rotating white rats continuously over long periods of time. Individuals were placed in 18 circular cages, about 10 inches in diameter, revolving at speeds of 60, 90, and 120 revolutions per minute, some in a clockwise and some in a counter-clockwise direction. The animals, to be sure, made definite and pronounced attempts to adjust themselves to their new environment by running in a direction opposite to rotation and by the characteristic behavior that goes with rotational nystagmus, in the same manner that a human being does when enduring the rolling motion of a boat at the beginning of a sea voyage. Eventually they became accustomed to their novel experience and learned to eat, sleep, play, reproduce, in a word to carry out their entire life history in the rotating nest. Many animals have endured from 6 millions to 80 millions of rotations, which, put into terms of time, means two to eighteen months. Upon removal from the rotating nest, the subjects had to readjust themselves to our prosaic universe and they showed a marked after-nystagmus in much the same manner (to continue our simile) as a human being does when returning to terra firma after a long sojourn on a rolling sea vessel. The rats would often run or walk in a direction parallel to the specific former rotation for several weeks. If the random movements were counted, they bore the definite earmarks of being related to the specific previous rotation---clockwise or counter-clockwise. Two results followed:

(1) Such subjects showed increased muscular incoordination, ocular movements became modified, they declined rapidly, and died; or

(2) They recovered after several weeks and appeared normal; but after several months deep-seated effects apparently cropped to the surface, and the subjects showed modified muscular and ocular movements, the necks and heads were twisted, and signs of labrinthine or vestibular disturbances became apparent. These disturbances were again related to the specific type of former rotation; for counter-clockwise rotation was followed by a right twist of the head and a shorter counter-clockwise after-nystagmus; while clockwise rotation gave the opposite result, a left twist with short clockwise after-nystagmus. Dr. Griffith mated individuals removed from rotating nests both inter se and to normal unrotated stock, and in the results of these matings appeared the bewildering facts which he found difficult to compromise with prevalent genetic conceptions, and which I am purporting to reinvestigate so

that the facts may stand either corroborated or refuted, and in any event, at least, we hope explicable.

Briefly stated, Griffith maintains:

(1) That individuals with a long history of rotation in their ascendants showed a peculiarly high incidence of disequilibrated progeny among the descendants, although the descendants were not rotated; and

(2) That the type of disequilibration in these descendants is specific in that it shows a one-to-one correspondence to the type of ancestral rotation. (pp. 292-295)

MCDOUGALL'S EXPERIMENTS WITH THE INHERITANCE OF ACQUIRED HABITS

Drew, G. C.; *Nature,* 143:188-191, 1939.

In a series of papers extending over a period from 1926 until 1938, Prof. William McDougall brought together evidence which led him to suppose that the hypothesis of Lamarck concerning the inheritability of acquired characteristics is substantially correct.

In designing his experiments, McDougall kept in mind two principles which previous investigators have overlooked. These are, first, that "the adaptation investigated should be achieved by the intelligent purposive efforts of the organism concerned", and, second, that it should be of such a nature that slight degrees of adaptation should be measurable, for it has already been shown by Lamarck's opponents that acquired characteristics are not transmitted as "perfected wholes of structure or function". Hence, if Lamarckian transmission is to be found at all, it should be looked for as some "slight increase of facility in the acquisition of the modification acquired by the parent". Many generations would probably be necessary, therefore, before any very marked changes appeared.

While rats were chosen as subjects for this experiment since it was possible to obtain an initially pure strain (Wistar), and since the breeding habits of rats allow a large number of generations to be studied in a reasonable length of time. A preliminary experiment showed deterioration in performance with successive generations, due, apparently, to an increasing fear of electric shock. The final form of the experiment, therefore, was designed to develop such a phobia. To this end, an apparatus was constructed consisting of three parallel alleys, the central one serving as an entrance and the outside ones as exits to a platform. The apparatus was filled with water, and the motivation was escape from the water to a dry platform. Of the two exit alleys, one, the shocked pathway, was brightly illuminated while the other, the free pathway, was kept dim. The task confronting the animals, therefore, was to escape from the water through the dim alley, the 'error' of choosing the bright being punished by an electric shock of tetanizing strength. The strength of the

shock was varied from animal to animal to obtain this behavioural effect with all of them, and tetanization lasted for three seconds. To avoid position habits, the 'correct' alley was alternated from left to right of the central. This alternation technique, however, allows no control of any alternation habits adopted by the rats. Rats were placed in the apparatus in batches of about six at a time and were given six trials daily. The primary measure of any Lamarckian inheritance was the total number of errors made in reaching a criterion of mastery of twelve successive errorless trials, though note was also taken of any pre-training preferences for the dim path.

McDougall's conclusions rest, in the main, on the results obtained with three groups of rats, an experimental group (or Tank group), a control group, and an experimental group obtained by adverse selection from the control stock. The Tank rats were originally obtained from Wistar stock, and subsequent generations were obtained by mating trained members of this stock according to a random selection. The only conscious selection of breeding stock was in the rejection of obvious runts. The control stock does not show the same continuity and inbreeding. McDougall's early intention was to use half the original Wistar stock as controls and breed from members trained in some task other than that of the experimental group. This stock, however, seems to have been abandoned, and subsequent controls were obtained direct from the Wistar Institute, or, in some cases, from other laboratories. The adversely selected experimental group represents an attempt to balance any unconscious favourable selection in the Tank rats by breeding from the worst animals in each generation.

The results with the Tank rats show a progressive improvement from generation to generation, until by the thirty-fourth, the average total number of errors per rat had decreased from an estimated 120 for the first generation (McDougall's records are incomplete before the fourteenth), to 36 for the thirty-fourth. This improvement in error scores seems to be correlated with an increase in timidity, cautiousness or hesitancy. 'Normal' learning shows an initial period of rapid, chance reactions, followed by a pre-solution period of hesitancy with oscillations from one exit to the other. This hesitancy developed earlier in the training period with successive generations of Tank trained rats, and is regarded as the probable cause of the decrease in errors. The adversely selected stock similarly show a very marked improvement, the total average error decreasing from 215 to 43 in fourteen generations. However, the control groups also show a decrease in errors. Groups obtained from Wistar stock in 1926, 1927, 1930 and 1932 show a decrease from 149 to 102. Admittedly, this decrease is by no means as large as those of the experimental groups, but neither the number of generations tested nor the environmental conditions of the groups are at all comparable. McDougall interprets the improvement in the control groups, which had had little contact with his laboratory and whose only relation with the experimental groups was that they too were Wistar rats, as indicating a probable improvement in the entire Wistar stock, for reasons which he is unable to suggest. The possibility that

there was an improvement in his experimental technique should also be taken into account.

McDougall's experiments have been repeated by Crew and Agar, neither of whom have obtained any evidence of Lamarckian inheritance. Both these experimenters have attempted to duplicate McDougall's apparatus, but have changed the procedures somewhat to obtain what they consider to be more trustworthy results. Thus, Agar standardized the strength and duration of the shock, and abandoned McDougall's group method of experimenting in favour of the more usual individual trials. Crew, on the other hand, retained the group method and used McDougall's tetanizing criterion of the strength of shock, but introduced marked changes in training, giving his animals a hundred trials before the introduction of shock instead of McDougall's six. All these changes, however, though likely to produce marked changes in the rate of learning of any given group of rats, should be relatively unimportant in producing different rates of learning in successive generations, provided the same technique is rigidly adhered to throughout any given experiment.

Explanations of McDougall's results as due to variations in the strength of shock used imply a gradually increasing strength with successive generations. Since there is no evidence either that such a gradual increase did take place, or that the rate of learning is affected by such small variations as would be necessary with each generation to keep within the limits of variation admitted by McDougall, such explanations cannot be regarded as satisfactory.

Other criticisms have, in the main, accepted McDougall's technique as sufficiently consistent throughout the experiment, but have endeavoured to explain his results in terms of factors other than that of the inheritance of acquired characteristics. One obvious criticism, advanced by Sonneborn and Agar and examined by Crew, is that McDougall unwittingly selected the brightest animals in his experimental group for breeding. That this is possible in apparently random selection is known to everyone who has worked with animals for any period. However, Crew was satisfied that in his case there was no such favourable selection and thinks it unlikely, therefore, that there would be in McDougall's experiment, and, furthermore, McDougall specifically controlled for such selection in his group of adversely selected animals. The fact that these animals also gave evidence of increased facility with successive generations makes it relatively unimportant whether or not there was favourable selection in his Tank rats.

Both Agar and Crew have criticized McDougall for failing to maintain adequate control groups in his experiment. Agar points out that over a long period of intensive inbreeding there may be progressive changes in the general constitution of the animals, and that such changes might facilitate the performance of McDougall's task. Since all these experiments agree that less vigorous animals learn more quickly on this task, this criticism may be valid. McDougall states in answer to it, that he observed no such progressive change, but the only really satisfactory control

would be the maintenance of a group from the same original stock, subjected to the same laboratory conditions, and inbred in a similar manner.

Crew criticizes McDougall for not having maintained adequate pedigree charts, so that he is unable to say which of his original stock are represented in his later generations. Crew's analysis of his pedigree and record charts led him to postulate the existence of 'quick' and 'slow' rats, 'quickness' being a Mendelian dominant and 'slowness' a recessive. On these grounds, he explains the improvement in the average number of errors per rat in McDougall's experiment as showing merely an increasing number of 'quicks' per generation. As he further states that a 'quick' may make a poor score owing to the action of numerous chance factors, adverse selection may result in a heterogeneous group of 'slows' and 'latent quicks'. If now unconsciously favourable selection is exerted, the 'latent quicks' may be chosen for breeding, and hence a decrease in average errors could be expected. While it may be that rats do differ in some general factor, especially in some such factor as 'photo-phobia', Crew's explanation of the results of McDougall's adversely selected group seems somewhat improbable, particularly as 'latent quicks' keep appearing for fourteen generations.

None of these experimenters seem to have considered the work of Tryon in breeding 'bright' and 'dull' strains as measured by maze performance, or Krechevsky's analysis of these strains. Tryon has bred rats selectively according to their ability on the California automatic maze, and, in a very carefully controlled experiment, has shown clearly that the offspring of 'bright' parents contain more 'brights' than 'dulls', and the offspring of 'dulls' more 'dulls' than 'brights'. The interesting point here in connexion with Lamarckian inheritance, however, is that both strains, 'dulls' as well as 'brights', became progressively better at learning this maze. Tryon inclines to the belief that increased vigour was responsible for the improved performance, but admits the possibility of Lamarckian inheritance.

Krechevsky has analysed the performance of these strains on various problems, and has concluded that they do not differ in any general factor; that is, 'brights' are not 'brights' on all problems, but rather that the difference is one of specific abilities. 'Brights' are good at maze problems because they utilize 'spatial hypotheses almost entirely, that is, their reactions are governed by spatial cues, while the 'dulls' use primarily visual hypotheses. This penalizes them on a maze, but might assist them in any brightness discrimination test. On McDougall's test, one would assume that Tryon's 'dulls' would be Crew's 'quicks' because they would be 'photo-phobic' and would react on a basis of the differential illumination of the two paths, while the 'brights' would more readily adopt spatial hypotheses such as position or alternation habits.

The position regarding Lamarckian inheritance of acquired habits, on the basis of these experiments with rats, seems to be as obscure as ever. McDougall and Tryon have obtained evidence of an increased facility on a specific task with successive, trained

generations; Crew and Agar, under conditions resembling McDougall's, have obtained no such evidence. The reasons for this discrepancy are not clear from the published accounts. It may well be that faults in the experimental techniques are responsible, and that this particular technique will not give trustworthy results concerning inheritance.

McDougall's task is open to criticism on two lines: (1) that an avoidance habit is not really suitable for an inheritance problem, since avoidance reactions are closely knit with such innate factors as fear, timidity, etc., the inheritance of which may certainly not be Lamarckian; and (2) that it provides a very untrustworthy measure of learning ability. All the experimenters who have used it comment on the large element chance plays in producing behaviour which meets the criterion of learning adopted. Crew, in fact, states that thirty-nine of his animals never received a single shock, and, therefore, obviously cannot have 'learned' the differentiation. Others may satisfy the criterion merely by adopting an alternation habit which happens to coincide with the alternation of the 'correct' alley. If they get 'out of step' they may then make 100 per cent errors. That such an occurrence is possible betrays a poor experimental technique, and raises serious doubts as to the validity of any results obtained with it. Furthermore, the practice of allowing animals to escape by the 'wrong' alley, differentiating it as an error merely by the presence of shock, is poor from the point of view of speed of learning, and trustworthiness of results. Apart from the chance factor as to when and how often the animal encounters both conditions of escape-shock and escape-no-shock, a violent shock can produce marked neurotic symptoms, which may result in purely random activity, or may, in some cases, result in a stereotyping of the error. Crew, in fact, gives a vivid description of violent, emotional and purely random activity lasting for as long as a hundred trials resulting merely from one violent shock.

Without enumerating more of such experimental artefacts here, it is obvious that trustworthy comparisons between successive generations would be a very hazardous task where chance factors can have so marked an effect on the rate of learning. Future research would do well to use a more trustworthy task and to control not only the constitutional factors so rightly emphasized by Crew and Agar, but also that type of factor shown by Krechevsky to be responsible for the differentiation between Tryon's strains.

• Mammal Behavior and Astronomy

MOONLIGHT AND THE PREGNANCY OF MALAYAN FOREST RATS

Harrison, J. L.; *Nature,* 170:73–74, 1952.

During the past four years, records have been kept of the state of pregnancy of a large number of rats and squirrels of various species collected and killed in connexion with scrub-typhus investigations. While studying the relation between rainfall and breeding season, it was found that the rates of pregnancy of the nocturnal forest rats showed a bimonthly rhythm which appeared to be in phase with the moon, and which suggested that the greatest number of conceptions occurred near the time of full moon.

. . . . .

LUNATIC BATS HIDE FOR SURVIVAL

Anonymous; *New Scientist*, 80:111, 1978.

Douglas Morris, from the Section of Neurobiology and Behaviour, has been studying the nocturnal wanderings of the bat Artibeus jamaicensis, a bat disappointingly unvampire-like in its diet, comprising such bloodcurdling goodies as figs and other soft fruits. He trapped the bats and fitted them with the transistorised device now de rigeur in ethological fieldwork, the tiny backpack radio-transmitter. Their feeding activities were then recorded on 100 nights, over a period of eight months.

When Morris plotted out the bats' activities over the lunar cycle, it became clear that the bats were exhibiting a marked "lunar phobia". Invariably, as the Moon reached its full-Moon proportions, the bats interrupted their night shifts and returned to their roosts, only to come out again after the Moon set.

It's clear that this lunar phobia is not triggered directly by the light levels, because it still occurred on cue when the Moon was totally veiled in cloud. That said, one has now to join in the evolutionary guessing game as to the reason why such a behaviour pattern, very wasteful in time and energy, could have arisen.

. . . . .

ANOMALOUS DISTRIBUTION IN TIME AND SPACE

• Recent Survival of Mammoths and Other Mammals

RECENT EXISTENCE OF THE MAMMOTH

Anonymous; *Popular Science Monthly,* 21:138-139, 1882.

Professor Collett's "Geological Report of Indiana for 1880" mentions some new facts that seem to indicate that the mastodon existed in our country at a more recent date than is commonly supposed. In nearly all the specimens that have been found, generally in places where the animal has been mired, the skeletons are in a greater or less state of decay. In a skeleton discovered a few years ago, in Fountain County, the marrow of the larger bones was used by the workmen to grease their boots, and the place of the kidney-fat was occupied by lumps of adipocere. During the summer of 1880 a mastodon was found in Iroquois County, Illinois, that gave every evidence of having lived among the same life and vegetation as prevail today. A mass of fibrous, bark-like material was found between the ribs, filling the place of the animal's stomach, which proved to be composed of crushed herbs and grasses, similar to those that still grow in the vicinity. In the same beds of miry clay, a multitude of small fresh-water and land shells were observed and collected, of mollusks which prevail all over the States of Illinois, Indiana, and parts of Michigan. These facts afford strong evidence that animal and vegetable life, and consequently climate, are the same now as when the mastodon lived.

THE MAMMOTH STILL IN THE LAND OF THE LIVING

Newman, Edward; *Zoologist*, 2:8:3731-3733, 1873.

The 'New York World' describes an interview between one of its correspondents and Cheriton Batchmatchnik, a Russian convict, lately pardoned by the Government in consideration of his wonderful discoveries in Siberia, to which territory he had been banished

for smuggling. Cheriton had escaped from the mines of Nartchinsk, and having reached the mountains struck southward for the Amoor River, intending to get to China. Meeting Cossacks, he again turned north, and essayed what seemed to be a pass in the great Altai range. For thirty days he scrambled about seeking in vain for an outlet. At length he got out to the north, together with one of the branches of the Lena river. He then turned eastward, and was entering the gorges of the Aldan mountains when winter overtook him. Across the snow, however, came vast troops of animals, all going the same way. Cheriton followed them, arrived at the summit of the range, and there, far below his feet, land-locked between great ranges of hills on all sides, lay a valley fifty miles wide by one hundred and fifty long, and centred by a blue lake. He descended and found the valley warm and fertile, and full of animals. At night he made a fire and lay down beside it to sleep. During the night---"Dark shadowy forms came over the water, splashing towards him, and seemed to seek what his fire might mean. The trampling of great beasts, that crushed the willow-stalks like pipe-stems, on their way to the water's edge, and that came and stood over him, breathing heavily and slow as they seemed to gaze at the fire with stupid wonder, made him afraid each moment of being overrun. Wild eyes, reflecting the fire-light, shone around him out of the gloom upon all sides, and wilder cries and howls gave new horrors to his position. He sprang to his feet almost paralysed with fright, and fired off his pistol at the nearest intruder. The echo of the shot rang long around him, and it seemed the signal for the cries of a thousand new monsters to burst forth. There were mad, plunging rushes of frightened beasts around him that made the ground tremble; a peculiar long, shrill, quavering shriek sounded over the lake, and was answered by a harsh, full-guttural bellow near at hand. When Cheriton awoke it was broad day, and there were no traces of the animals that had disturbed him over night, except the paths they had worn going down th the water. In these paths he saw the deep-planted spoor of some animal larger than anything of the sort he had ever before beheld. His first care was to seek some place to pass the next night, where he could be free from the alarms that had made the past night so terrible. He re-crossed the meadow, and followed the edge of the slope around in the direction in which he saw some rocks. Among them he found the wide and lofty entrance to a cave. He entered with some pre-cautions, for the rocky pavement was worn as if by use, and within he heard a slow, measured movement as of an animal gently ruminating, and heavily breathing with great calm inspirations and expirations like the sigh of a smith's bellows. One turn, then an-other, he heard a heavy startling snort, and there in the half light of the cave, standing full before him, alive, chewing the cud and waving its proboscis to and fro with a slow, gentle, majestic motion, he saw---a mammoth! 'I did not know then,' said Cheri-ton, 'what I have since been told, that Siberia was an old habitat of these animals, and that some of the best scientific judges are uncertain whether to look upon the remains found on the shores of the Arctic Ocean as fossil animals or as the remnants of wand-

ering herds caught and perishing in storms, individuals of which may still exist under favourable circumstances. Without intending it I have solved that doubt.' Cheriton describes the mammoth as being a very imposing looking animal, covered with reddish brown wool and long black hair. During his stay in the valley he was close to five of them, all of which were nearly of a size, being about twelve feet high, eighteen feet long, with tusks projecting about four feet, and being eight to ten feet counting the curve. The skin which was bare on the upper surface of the ears, on the knees, and rump, was of a mouse-colour, and seemed very thick. The animal was nocturnal in its habits, frequenting caves or forest depths by day, and feeding at night and early morning. Cheriton thinks there might be some fifteen or twenty of these monsters in the valley altogether, but that all these are aged, and that very few are born now-a-days. At any rate, he saw none that had the least appearance of being young. They were very peaceable animals, torpid and sluggish as old oxen, never disturbing Cheriton, nor indeed taking much notice of him. The lake was inhabited by a monster of which Cheriton was in constant dread, a sort of saurophidian, which he described as being thirty feet long, and armed with scales and horrible fangs. This monster--he never saw but the one--was master of the lake, and lived by devouring the animals which came by night to its brink to drink. Cheriton gives a graphic and exciting description of a contest which he witnessed one morning at early dawn between this crocodile-serpent and one of the mastodons. The battle, which lasted more than an hour, ended in the discomfiture of the mammoth, which could barely limp away after having been constricted in the serpent's folds."

Although fully aware of the advantages of what the late Thomas Moore called a "heliacal rising," I postponed the publication of this extraordinary narrative for a month for two reasons: first, because my notice of Mr. Moggridge's volume had already been deferred for an unreasonable time; and secondly, because I thought it possible some counter-statement about the mammoths might appear, showing that the narrative itself was one of those jeux d'esprits for which our transatlantic brethren are so deservedly celebrated. A month, however, has elapsed, and not only have we received no official contradiction, but the statement has been thought worthy of reproduction by Professor Feuchtwanger, before the "American Association for the Advancement of Science." The learned Professor says:---"The discovery of the mammoths in Siberia in the deep gorges of the mountains near the Lena Viner, which was lately published as having been made by a scientific Russian convict, who had seen five living animals, twelve feet in height and eighteen feet in length, with projecting tusks four feet long, excites some discussion in Europe. I think it worthy of inquiry whether the mammoth of the post-tertiary period, discovered during this century in Siberia, near the same river, can have any relation to the convict's discovery. Thousands of these animals have been found buried in the ice, with their well-preserved skins, and thousands of tusks are brought to England to this day for the use of the turner. These are of

nearly the same dimensions as those seen by the Russian. The convict has received an unconditional pardon, on the recommendation of scientific men who have investigated his statements and believe them to be true." I must confess to feeling some hesitation in receiving as true Zoology such an astounding statement; there are also certain of the details, particularly that relating to chewing the cud, which could scarcely emanate from a "scientific" observer, and so the learned Professor calls him. I would avail myself of this opportunity for inviting attention to a paper of my own on the Siberian mammoth, published at page 1 of the 'Zoologist,' under date of January, 1843. From this it appears that M. Klaproth published a note on the same subject in the 'Memoirs of the Academy of St. Petersburgh' in the early part of the present century (see vol. v. p. 409). In this there is abundant evidence of the head, tusks, legs, and even entire bodies of mammals having been repeatedly found, at the beginning of the present century, in so recent a state that bears and dogs fed on the flesh: portions of skin and hairs are preserved in the Museum of the London College of Surgeons. The extract from the 'New York World' is reprinted from the 'Standard' newspaper.

MASTADON OR BUFFALO?

Anonymous; *American Antiquarian,* 11:65, 1889.

When Mr. H. von Beyer, an engineer from Washington, D.C., stayed at Port Townsend, Puget Sound, W.T., in December, 1887, a mysterious rumor spread in that locality about some monstrous animals seen in the interior of Alaska. He read from white people settled in the vicinity, that Indians had gone into Alaska, ascended the Yukon River to a great distance from the coast, when they discovered tracks on the ground. They followed these tracks for miles and miles, and finally they came in sight of hairy animals, which they supposed to be mammoths. They were frightened at the enormous size of these creatures, whose tracks always described circles. The story has passed through so many mouths before they reached Mr. von Beyer that he is doubtful about its veracity; the report probably came from some Vancouver Island Indians, and reached Port Townsend through these, who often undertake long travels at sea. Similar stories of a huge animal going in a circle are found among the Iroquois of New York and referred to the buffalo.

CHINESE ACCOUNTS OF THE MAMMOTH

Anonymous; *American Naturalist*, 24:847–850, 1890.

The gradual cooling of the Asiatic climate may be supported by the existence of the bones of the mammoth in northern Siberia. This hairy elephant lived in that country when the air was temperate, and when abundant forests supplied it with the young twigs on which it lived. Since that time northern Siberia has become an intolerably cold desert. The ground there is constantly frozen to a depth of more than two feet below the surface, and produces only moss, with a few modest-looking flowers. The mammoth very early drew the attention of the Chinese. It is first mentioned in the Er-ya, and next in Chuang-tse, in the third century before Christ. The enormous quantities of valuable ivory which the remains of the mammoth in Siberia furnish made known to the ancient Chinese the existence of the animal through their trade with Tartary. On account of its being found in very many localities imbedded in the soil and in rocks, old books always speak of it as a monstrous mole living underground. It was found, they tell us, in China and in Tartary. Chuang-tse wrote as a poet, and pictures it (yen shu) as drinking a river of water before its thirst was satisfied. He had been told of the fossil bones or had seen them, and filled up the picture by the aid of imagination, either his own, or that of those from whom he heard the story. Seven centuries afterwards a medical writer, Tao Hung-king, says: "It is found in forests, and is as large as a water buffalo. It is in form something like a pig. Its color is a greyish-red. Its feet are like those of the elephant. Its breast and upper tail are white, and blunt though powerful. Its flesh is eaten, and is like that of the cow. It is known by the name 'King of the Shu tribe.' In calamitous years this animal often appears."

In the seventh century this account of the animal was discredited. Its great size was not believed. Its hiding and walking in the earth were thought absurd. These disparaging criticisms were made by Chen T'sang-chi, an eminent writer, who does not seem to have been shown any of the bones of the animal. Yet in the eleventh century Su Sung defended the statements of early writers on the subject. Bones of some large unknown animal had been found at T'sangchou, near Tientsin, just as the Tsin History states that at Siuencheng, a little way southwest of Nanking, there had been found similar remains in the third century. It was also related that the same animal existed in Tartary, where the larger specimens weighed one thousand catties, and was fond of living in water. It was like an elephant in the legs, though it had the hoofs of a donkey. Another place where it was found was at Tsiuning, near Pingyang Fu in Shansi. The people called it the "recumbent cow." It used to wander among the

mountains at times, and drop its hair in the fields. Each one became a rat, and great was the damage to the crops. The Liang history says that in Japan there is a large animal like a cow of the Shu class, which is eaten by a great serpent. These are all instances of the mammoth ("hidden, shu") and prove the correctness of Tao's words. Tao has been blamed without reason by men who had not themselves inquired into the truth of his statements. The name by which this animal is known in Sensi is "the small donkey." Such are the testimonies of the existence of the mammoth collected by the author of the Pent'sao. The Chinese accounts of a monster animal as given in the Pent'sao could not, if taken alone, be regarded as agreeing with the Siberian mammoth except in a rough way, yet they are very important. Early in this century the remains of that animal were found in so many parts of Siberia, and the ivory was of such great commercial value that the whole scientific world was interested. Cuvier in France was absorbed in the contemplation of the remarkable bones submitted to him, and decided that as the mammoth was met with often with the flesh undecayed, there must have been a sudden change of climate from temperate to extremely cold to account for the frozen condition in which the remains were found. Klaproth, who was then at Kiachta, visited the Chinese drug shops and found that the bones were known to the Chinese there. They gave him the name of the animal as it was recorded in the Pent'sao. It was he that suggested that the throne of ivory of the Mongol Emperors was formed of the tusks and teeth of the Siberian mammoth, and that Chinese traders for two thousand years would be ready to buy on any occasion the ivory which was from time to time discovered and brought away. He went home to Berlin, and made known to the learned world that the Chinese had accounts of the animal. The passages he translated are apparently those which are found in the Pent'sao, in the chapter on the class Shu, which includes the Rodentia with the squirrel, sable, ermine, and weasel. There can be no doubt that the mammoth, and possibly other fossil animals known to the Chinese, are assigned to the class Shu, because they were supposed to hide themselves in the soil of cultivated fields, and to have died underground in the position where their bones were afterwards found.

. . . .

MYTHS HINT INDIANS KNEW MAMMOTHS

Anonymous; *Science News Letter,* 17:31, 1930.

Did the early Indians on this continent know the great hairy mammoth, monstrous survivor of the Ice Age? Did they hunt him for his meat and hide and ivory?

Prof. William Duncan Strong of the University of Nebraska thinks it possible. Before anthropological section of the American

Association for the Advancement of Science, he told of evidence which he has found in the myths of certain remote Algonkian tribes that seems to point that way.

The first was secured from the Naskapi Indians of northern Labrador. The Naskapi are an extremely isolated and conservative group who retain to the present time many primitive customs and beliefs. In the quite widespread Djakabish legend the Naskapi version tells of a huge primal monster that destroyed the parents of the hero.

"The animal is described as very large, with a big head, big teeth and a very long nose with which he hit people," said Prof. Strong. "His tracks in the snow were deep and round. After a considerable struggle Diakabish killed this monster with his arrows, cut him up and used the ears for his bed. The native name for the creature is 'Katcitoask' and one Indian who had seen pictures of the elephant translated it by the latter name. The same native term is used by the Rupert House Cree in their version of the legend.

"The second reference was secured by Dr. F.G. Speck from the Penobscot Indians in Maine," Prof. Strong continued, "Here Snow Owl, a Penobscot culture hero, was searching far to the south for a wife. He noticed that the water courses were drying up and followed up the valleys to find what caused it. He noticed what seemed to be hills without vegetation moving slowly about. Upon closer scrutiny he saw that these masses were really the backs of great animals with long teeth, animals so huge that when they lay down they could not get up. They drank half a day at a time, thus taking up all the water from the land.

"Snowy Owl went on and after many adventures secured his wife. Then he returned to the place where these animals had their 'yard'. Here he cut certain trees upon which the monsters were accustomed to lean for rest at night so that when they did so the trees would break. Thus the animals fell upon the sharpened stumps and Snowy Owl shot them all. The water courses filled up and his people rejoiced.

. . . .

MEN AND ELEPHANTS IN AMERICA

Johnson, Ludwell, H., III; *Scientific Monthly*, 75:215-221, 1952.

Elephant remains and Indian legends relating to them have provoked interest and inquiry from early colonial times. Possibly the earliest account of such inquiry comes from Cotton Mather (1663-1728), famous Congregational divine who told of an Ohio Indian story about the giant bones. The animals were described as having once been abundant. They fed upon the leaves of the lime (basswood) tree and when they slept they did not lie down but leaned against a tree. Pierre Francois Xavier de

Charlevoix (1682-1761), Jesuit traveler and writer, recounted a most remarkable Algonkian legend: "Il a, disent-ils, les jambes si hautesque huit pieds de neiges ne l'embarrassent point: sa peau est a l'epreuve de toutes sortes d'armes, & il a une maniere de bras, qui lui sort de l'epaule, & dont il se sert, comme nous faisons de notres. The Indians called this beast the "grand original;" usually "original" referred to a moose. This is, to say the least, a singular tale. What else could the "sort of arm which comes out of his shoulder" be except an elephant's trunk? What but the sight of a living elephant could have inspired such an account?

Thomas Jefferson was deeply interested in paleontology, geology, and, in fact, in all phases of natural history; his was an omnivorous mind. He was especially fascinated by the elephant bones that were being found even in that period, and in the Indian stories concerning them. In his Notes on Virginia Jefferson transcribes such a story:

> Our quadrupeds have been mostly described by Linnaeus and Mons. de Buffon. Of these, the Mammoth, or big buffalo, as called by the Indians, must certainly have been the largest. Their tradition is, that he was carnivorous, and still exists in the northern parts of America. A delegation of warriors from the Delaware tribe having visited the governor of Virginia, during the revolution, on matters of business, after these had been discussed and settled in council, the governor asked them some questions relevant to their country, and among others, what they knew or had heard of the animal whose bones were found at the Saltlicks on the Ohio. Their chief speaker immediately put himself into an attitude of oratory, and with a pomp suited to what he conceived the elevation of his subject, informed him that it was a tradition handed down from their fathers, "That in ancient times a herd of these tremendous animals came to the big-bone licks, and began an universal destruction of the bear, deer, elks, buffaloes, and other animals which had been created for the use of the Indians: that the Great Man above, looking down and seeing this, was so enraged that he seized his lightning, descended on the earth, seated himself on a neighboring mountain, on a rock of which his seat and the print of his feet are still to be seen, and hurled his bolts among them till the whole were slaughtered, except the big bull, who presenting his forehead to the shafts, shook them off as they fell; but missing one at length, it wounded him in the side; whereon, springing round, he bounded over the Ohio, over the Wabash, the Illinois, and finally over the great lakes, where he is living at this day."

Writing on the same general subject, Jefferson continues:

> It is well known that on the Ohio, and in many parts of America further north, tusks, grinders, and skeletons of unparalleled magnitude, are found in great numbers some lying on the surface of the earth, and some a little below it. A Mr.

> Stanley, taken prisoner by the Indians near the mouth of the Tanissee, relates, that, after being transferred through several tribes, from one to another, he was at length carried over the mountains west of the Missouri to a river which runs westwardly: that these bones abounded there; that the natives decribed to him the animal to which they belonged as still existing in the northern parts of their country; from which description he judged it to be an elephant. Bones of the same kind have lately been found, some feet below the surface of the earth, in salines opened on the North Holston, a branch of the Tanissee, about the latitude of 36½° north. From the accounts published in Europe, I suppose it to be decided, that these are of the same kind with those found in Siberia.

Albert Koch published in 1841 an account of an Indian legend roughly similar to that related by Jefferson. It is an Osage tale.

> There was a time when the Indians paddled their canoes over the now extensive prairies of Missouri, and encamped or hunted on the bluffs....That at a certain period many large and monstrous animals came from the eastward, along and up the Mississippi and Missouri rivers; upon which the animals which had previously occupied the country became very angry, and at last so enraged and infuriated, by reason of these intrusions, that the red man durst not venture out to hunt any more, and was consequently reduced to great distress. At this time a large number of these huge monsters assembled here, when a terrible battle ensued, in which many on both sides were killed, and the remainder resumed their march towards the setting sun.

This story and that related by Jefferson are probably in large part "myths of observation" efforts by the Indians to account for the existence of the great bones about which they had no real knowledge. On the other hand, Stanley's report indicates that some of the Western tribes may have retained traditional descriptions of the mammoth with considerable accuracy. It is interesting to note that William Berryman Scott wrote of

> a widely spread legend among the tribes of the Northwest British provinces, that their ancestors had built lake dwellings on piles like those of Switzerland, "to protect themselves against an animal which ravaged the country long, long ago... I find the tradition identical among the Indians of the Suogualami and Peace Rivers, who have no connections with each other...." So suggestive were these Indian tales that on some of the early maps of North America the mammoth is given as an inhabitant of Labrador.

Strong, in his valuable article, reproduced a Naskapi account concerning a creature called Katcheetohuskw. The story is too long to be set down in full here, especially as the body of it is not particularly significant; the really important point is the In-

dians' description of the monster. "When asked to describe Katcheetohuskw, the informants said he was very large, had a big head, large ears and teeth, and a long nose with which he hit people". "His tracks in the snow were described in their stories as large and round." Strong did not believe, as some skeptics do, that this description was a result of leading questions asked by whites because, as he said, "the older Indians questioned were unanimous in declaring that such had always been the description of Katcheetohuskw so far as they had any knowledge." Strong's interpretation of the story has been strongly questioned by several gentlemen who maintain that in its true translation Katcheetohuskw (other spellings are put forward) denotes a mythical bear, a stiff-legged, hairless, anthropophagous being endowed with supernatural powers. Their contention, although certainly impressive, does not come to grips with the Indians' picture of the beast. A bear may well be said to have a big head and large teeth. But by no coruscation of the imagination can he be said to have "a long nose with which he hits people."

A Penobscot "Snowy Owl" legend mentioned by Strong is dealt with more at length in a later article by F. G. Speck, one of those who disagreed with Strong's interpretation of Katcheetohuskw. The following is an abstract from this tale:

> Snowy Owl proceeded then to find the monsters which he had seen before. He went to where the animals had their "yards." He cut certain trees, where he had observed the monsters were accustomed to lean for rest at night, almost through, so that when the monsters would lean on them they would break. When the creatures went to rest at night leaning against the trees, they fell upon the sharpened stumps when the top bent over and broke, and could not get up again; and Snowy Owl shot them all.

It is an amazing fact that in the Sixth Book of Caesar's Gallic Wars there appears an almost identical account of an animal of the German forests which the Roman called an elk.

> In outward form and in the parti-coloration of its skin, this has much in common with the Italian wild goat, though in build it is slightly heavier. Its horns are mutilated and its legs are marked by an absence of any natural protuberance or joint; it never lies down to rest, and if accidentally cast, is powerless to recover its position or to raise itself from the ground. Its lairs are the trunks of trees, leaning against which, with its body slightly out of the perpendicular, it will go off to sleep. On discovering from their tracks any of these animals' favorite haunts, the huntsmen either undermine all the trees of the immediate neighborhood, or else cut them through just far enough to leave them apparently still firmly standing. The elks then return and lean against these as usual, and their weight proving too much for the weakened trunks, they and their supports come crashing to the ground.

It is submitted that this was not a description of a living animal, as Caesar believed, but that Caesar's informant was relating a very old German tradition about the mammoth. At first glance it might seem rather difficult to make an elephant out of an animal which is favorably compared to a goat. It can be pointed out, however, that a method of hunting is by its nature more easily described and understood than is the animal itself, and that Caesar may very well have got a clear comprehension of the former and a distorted picture of the latter. Little imagination is required to see that the curling tusks of a mammoth may in the course of a double translation, have become the "mutilated horns" ascribed by Caesar to the so-called elks. Moreover, no animal at all comparable to a goat in size could be hunted in such a way as that used by the Germans (and by Snowy Owl).

Sir Thomas Browne, in his Common Errors, had the following to say about the elephant:

> The first shall be of the Elephant; whereof there generally passeth an Opinion, it hath no joynts; and this absurdity is seconded with another, that being unable to lie down, it sleepeth against a Tree; which, the Hunters observing, do saw almost asunder; whereon, the Beast relying, by the fall of the Tree, falls also down it self, and is able to rise no more. Which conceit, is not the daughter of later times, but an old and gray-headed Error, even in the daies of Aristotle.....

Here again is the same tradition, and this time it is explicitly applied to elephants. Bertha L. Loomis, in "The Elephant in the Literature and Art of Greece and Rome," gives an ancient method of capture which "consisted of noting a tree in the forest against which one of the giant beasts was wont to lean for support when he slept, hew into it from the opposite side and await his return. His mammoth bulk would fell the tree and carry him down with it to a position from which he was unable to rise." As early as the fifth century B. C. Ctesias, Greek physican and historian, stated that since the legs of an elephant were jointless it had to sleep in a standing position, "tied to a tree." Aelian (d. 222) was most surprised at the dancing of the trained Roman elephants, for he also believed that their legs had no joints. Philip Matthew Paris, Shakespeare, and John Donne ? ? fell into the same error. The persistence of this belief was such that its currency continued to the ninteenth and twentieth centuries, and doubtless many people could be found today who subscribe to this ancient error. For example, Charles John Andersson wrote in 1873 that "it is a commonly received opinion . . . that the elephant always keeps standing, or reclining, it may be against a tree or rock; or, as regards Southern Africa, against one of those gigantic ant-hills one there so frequently meets with...." Captain Francis A. Dickinson, a big-game hunter, believed that the elephant, if he ever lay flat on the ground, would be unable to get up. By 1923, however, George A. Chamberlain reported that modern hunters had exploded the hoary assertion that elephants rarely lie down, although it is unusual for them to do so.

The essential elements in this complex of traditions are: (1) stiff, jointless legs; (2) the inability to rise from a prone position; (3) always, as a consequence, maintainenance of a standing position; (4) the habit of leaning against a tree when sleeping; (5) hunting the animals by weakening the trees against which they sleep. All these elements are present in the Penobscot story of Snowy Owl, in Caesar's account of the German "elk," in Browne's Common Errors, and, to a lesser degree, in the method of hunting as reported by Bertha Loomis. Mather's Ohio Indian legend and the story of the stiff-legged Katcheetohuskw fall into the same pattern. The only conclusion to be drawn is that all these traditions and beliefs, American as well as European, are derived from a common source---elephants. (pp. 217-219)

THE JEMISCH, OR GREAT GROUND SLOTH

Anonymous; *English Mechanic*, 72:118-119, 1900.

It is curious that the wandering Englishmen who ransack countries, old and new, in search of strange adventures and stranger beasts, so few turn their steps to Patagonia. The forests of Central Africa are yielding up their secrets, the temples of Palenque have been measured and photographed, but the plains where the guanaco and ostrich roam, the mysterious beechwoods that overhang the icy waters of the nameless fiords beyond Magellan's Strait, the untrodden peaks where the hugest of the puma tribe make their dens, all these have been seen but by some half-dozen travellers, one of whom, as we are proud to remember, was an Englishwoman.

And yet no one can say what wonders that unknown land may not hold. When Chaworth Musters was encamped with a party of Tehuelche Indians on a branch of the Rio Senguer, a couple of ostriches which the hunters had killed, and left outside the camp disappeared in the night, and their remains were discovered next day, torn and half devoured, in a shallow of the river to which led the tracks of a mighty beast of prey. The puma of the Cordillera is large---one of 9 ft. from tip to tip has been shot;---but this mysterious beasts's tracks were far too large for a puma's. The trembling Indians declared that they knew what it was that made those tracks; it was the "tigre del agua," or water tiger, a ferocious yellow monster that haunted their rivers and lay in wait for the unwary swimmer to seize and devour him.

It is more than 20 years since Musters was there, but now come further tales of this mysterious animal brought by Don Carlos Ameghino, who also heard of it from the Tehuelches. Their name for it, according to him, is the "Jemisch"; they believe it to be invulnerable, and do not like even to speak of it. Evidently a monster which can kill them but which they cannot kill, is not

quite "canny." They broke through their reserve, however, so far as to reveal to him that the Jemisch lives in caves on the shores of rivers or lakes, coming out only by night. They describe it as having a short broad head; a skin covered with short dun hair; short strong legs with long-clawed feet, like a bear's but webbed between the toes---which last detail seems to be partly confirmed by a story that has reached Don Francisco Moreno, the director of the La Plata Museum, of fresh footprints of a bear-like animal having been recently observed in the forests of Southern Patagonia.

But the most surprising particular of all is still to come:-- Don Carlos Ameghino was actually shown what was said to be a piece of the skin of the Jemisch, in which were embedded little "ossicles" similar to those already known to occur with the fossil remains of the grypotherium. If the water-tiger carries a horny armour under its hair, no wonder it is invulnerable to the attacks of the Indians.

And to make the mystery more complete, Don Francisco Moreno has now brought to England not only the bones, but also the skin of a grypotherium which, he found in a cave in Patagonia. This grypotherium, or giant ground sloth, has always been reckoned among the extinct monsters, and Don Francisco himself considers that the specimen he discovered may have been preserved in the ice-cold cave ever since the Glacial period. But the skin resembles the skin said to be that of the Jemisch in this, that it contains a layer of small armour-like plates underneath a dense coat of hair. What, then, is the Jemisch? Is it a sloth, is it a bear, is it a water-tiger? Or is it, after all, only an Indian bogy-beast, and did the piece of skin shown to Don Carlos really belong to a monster of the Glacial epoch? Here is a chance of scientific immortality for any man who will go and find out. If the Jemisch exists, its horny armour may prove to be not invulnerable to a Lee-Metford bullet.

IS THE GIANT LEMUR A "LIVING FOSSIL"?

Anonymous; *New Scientist*, 20:589, 1963.

Although giant lemurs are generally supposed to have become extinct during te Pleistocene, there is no apparent reason for this, since they would not have been threatened by carnivores and their food supply remained unchanged. It seems far more likely that man was the guilty party. A 17th century French explorer in Madagascar described enormous animals with a "human" face that terrified the natives and it is possible that giant lemurs may yet be found in the dense forests of the interior. The sites from which the bones were collected are marsh and lacustrine deposits and are all superficial; an old report states that "white pulpy matter" came out of one "extinct" lemur's skull. Many of the bones have a suspiciously recent appearance

and have been subjected to nitrogen analysis. Bones from one site contained 2.6 per cent nitrogen compared with less than 1 per cent for those from two other localities, a result that suggests that the bones are very recent, at least in this case. Their absolute age is to be determined by carbon-14, so whether Megaladapis is a "living fossil" should soon be determined.

• Mammal Population Explosions

HORDES OF SQUIRRELS

Anonymous; *Science News Letter,* 14:134, 1928.

For a time after the pioneers began making clearings in the forest and raising crops, the squirrels increased because of more abundant food and fewer wild enemies. According to Dr. Kirtland, red squirrels first made their appearance on the Western Reserve in northern Ohio early in the nineteenth century. By the middle of the century they had become abundant there.

Gray squirrels, too, doubtless increased in numbers when food became plentiful. In northern Ohio a hunter is said to have killed 160 in a day; 21 gray and black squirrels were seen in one tree in 1855. In White River township, Johnson County, Indiana, in 1821, gray squirrels were so numerous that on four farms not a single bushel of corn was saved from them. In one four-acre field, every ear was taken within two days after the corn had ripened. In Bartholomew County, Indiana, in 1834, there was a contest among the squirrel hunters in which one hundred persons participated. The champion killed 900 squirrels in three days; the second largest number killed was 783.

At various times and places gray squirrels have been observed to migrate. These migrations were seen in southern Wisconsin in 1842, 1847, 1852, and 1857, one each five years. Their cause is not well understood, but is thought to be due to a superabundance of the squirrels in the region from which they migrate. While migrating "many are drowned in attempting to cross streams; not a few are destroyed by man; some die from utter exhaustion, and when forced to travel in an unnatural manner upon the ground they fall an easy prey to rapacious birds and mammals, all of which feast when the squirrels migrate. During one of these migrations innumerable squirrels swan across the Niagara near Buffalo, landing so exhausted as to be easily taken by hand or knocked down with sticks."

Referring to migrations of gray squirrels in the Ohio Valley in an early day, Col. James Barker speaks of their "coming in

millions from the north to the south, destroying whole fields of corn in a single day."

MIGRATION OF GRAY SQUIRRELS

Jackson, Ralph C.; *Science,* 82:549-550, 1935.

At the present time considerable amount of speculation is passing through the minds of the zoological world in an endeavor to solve the migration of gray squirrels. It is reported that a horde of these animals are migrating from New England to New York covering an area of 100 miles wide. Recently a number of dead squirrels were picked up on the shore of Gilboa aqueduct. Between Albany and the Rip Van Winkle Bridge 2,000 remains were found along the west shore of the Hudson River.

This migration is not the result of campers invading their habitat or being attacked by parasites, as some writers have postulated. The slaughter of hawks, owls and foxes under the guise of conservation has made it possible for the red squirrels to multiply to the extent of breaking up the balance of nature, thus creating living conditions which would lead to extermination if migration did not take place.

It is not generally known that a large percentage of gray squirrels are emasculated annually by the pugnacious reds. In years past the writer has shot and trapped alive a large number of gray squirrels and close observation showed 98 percent, mutilation. The technique used by the red squirrel is simple but most efficacious. Contact is made by leaping from a limb of a tree, a distance of fifteen feet of more, making a perfect landing on the back of the unsuspecting gray engaged in digging seeds. With a quick body twist the unfortunate victim is pinned on its side and in less than ten seconds emasculation is effected.

SQUIRRELS ERUPT IN APPALACHIA

Anonymous; *Science News,* 94:359-360, 1968.

The area involved consists of the mountains of northwestern North Carolina, all of Tennessee, northern Georgia and the Ozarks in southeast Missouri.

The action has been quite sudden, with areas that a few days earlier were teeming with squirrels suddenly emptied, and adjacent empty areas suddenly swarming.

At first, observers were puzzled by the fact that immediate hunger seemed to play no part in the mass movement. Walter Edmunson, district game protector for North Carolina's Western District, reports that there is ample food and that squirrels that

died on the highway were "well fed, nice and fat, in good shape." Yet the animals were even entering buildings, evidence they were desperate for something.

The tentative explanation: In 1967 the mountain weather was good and there was a bumper crop of mast come fall. Mast is a collective term for beechnuts, acorns, walnuts and the like, the forest harvest on which squirrels absolutely depend in winter. Since they don't hibernate, they must have some kind of storable food to survive.

Because of the good mast crop, a large breeding population survived the winter in good condition and produced a bumper crop of young this year. Unfortunately this spring there was a late frost which caught the mast-bearing trees in bloom, and the crop was heavily damaged.

Throughout the summer the large population of squirrels fed on perishable summer food, knowing and caring nothing about the mast crop. When the first signs of coming winter awoke their nut-gathering instinct they found there were very few nuts to gather. Something akin to panic apparently set in.

The squirrels began moving out of their home territories, not driven by hunger but by an urge to find storable food. The urge is strong enough apparently to produce lemming-like determination to cross any obstacle even the three mile width of Fontana Lake, N.C., a journey for which they are completely unequipped.

LEMMING HORDES

Hill, John Eric; *Natural History,* 54:180, 1945.

In 1868 a ship was coming into Trondhjem Fjord, Norway. The captain noticed that the water was covered with small brown objects, like fallen leaves, but these on closer inspection proved to be lemmings, swimming out to sea. For fifteen minutes the ship ploughed through the mass of rodents.

Every three or four years the lemmings overrun parts of the Scandinavian Peninsula. They are stout-bodied relatives of our meadow mice, about five inches long, with very short tails. In color they are rusty brown, with a black stripe down the back and yellowish sides. Normally they live above timberline on the mountains or on the arctic tundra, far from cultivated fields, feeding on grass, moss, and dwarf birch. Periodically, however, the lemming population reaches a density far beyond the food supply in their homeland. Since each pair may produce five or six young several times a year, and the offspring reproduce when only a few weeks old, this is not surprising. The animals that normally prey on lemmings are few--weasels, foxes, hawks, and owls. They do not reproduce nearly so rapidly as the rodents, and once the lemmings gain a head start their enemies cannot catch up with them.

When conditions in their homeland become too severe, millions of young lemmings emigrate, moving down to the valleys and eating up nearly every green thing as they go. The hordes journey chiefly at night, but sometimes they continue their march by daylight. They multiply as they go, but now their destruction begins. Snowy owls and arctic foxes from the high mountains and tundra follow them; skuas leave the seacoast to nest where the lemmings abound; and almost all the animals of the lowlands are their enemies. Even those that usually eat plant food--pigs, cattle, goats, and reindeer--turn carnivorous and eat lemmings. The larger fishes in the rivers and lakes they cross also gorge on the multitude of lemmings.

Then disease strikes the lemming hosts, and hundreds of thousands die of "lemming fever," probably tularemia. This is sometimes transmitted to the people of these areas, perhaps through contaminated water. Wells and streams become filthy with the waste products and dead bodies of the rodents.

. . . .

THE RISE AND FALL OF THE LYNX

Dewey, Edward R.; *Cycles, The Mysterious Forces That Trigger Events,* Hawthorn Books, New York, 1971, pp. 24-28.

The Rise and Fall of the Lynx. The Canadian lynx is another prime example of one of the most baffling aspects of animal life---its rise and fall in population . . . the cycle of abundance. Patrolling the northernmost regions of Canada in search of his favorite food, the snowshoe rabbit, the lynx moves with huge running strides on padded feet large enough to prevent him from sinking into the soft snow. But while he is a hunter, he is also the hunted, for his skin is instantly convertible to cash at the nearest trading post.

Unless we are trappers, hunters, or fishermen, we normally think of animal populations as relatively stable, a notion that is far from actuality. Animal populations vary tremendously from year to year, even from month to month. Since the lynx is a favorite of north-country trappers, year-by-year records of its population are available over a long period of time, and it thus makes excellent study material.

Of course, there are no actual lynx censuses, but there are records of the offerings of lynx skins by trappers, particularly to the Hudson's Bay Company. As the efforts of trappers to earn a livelihood are fairly constant, biologists feel that the records of skin offerings constitute a reasonably reliable index of the abundance of the animal in its wild state.

. . . . .

Except for the fact that during the last fifty to sixty years the catch has been considerably lower, the most notable features of this record are the tremendous fluctuations that characterize these figures and the amazing regularity of the fluctuations. The graph shows a range from under 2,000 skins in a poor year to over 70,000 in a good one. Intervals between one high and the next, or one low and the next, normally vary from eight to ten years. Over the span of the record they average precisely 9.6 years.

Because of the wide fluctuation in skins from a high year to a low, and because of its regularity, the Canadian lynx cycle has received wide attention. Although there is general agreement that it has not continued to fluctuate in such a regular rhythm for over two hundred years purely by chance, there is little agreement as to the cause.

One attempted explanation is based on a similar cycle in the rise and fall of abundance of snowshoe rabbits, the most important item of food in the lynx diet. But this raises an obvious and unanswered question. What causes the 9.6-year cycle in the snowshoe rabbit?

The 9.6-year cycle in population is characteristic of much wildlife. The coyote, red fox, fisher, marten, wolf, mink, and skunk have abundance cycles of the same period (average wavelength), all reaching their highs and lows in abundance at about the same time on the calendar.

In Illinois, and in much of the Midwest, a pesky white-winged insect called the chinch bug also has a 9.6-year cycle, at the peak of which up to 70 million have been known to cover one acre, wreaking havoc on cereal crops. Since it is rather difficult to imagine 70 million of anything, this reduces to 1,600 bugs per square foot!

. . . . .

The Rodent Who Dies in a Cycle. The Norwegian word for "destroying" is lemmus, or lemming. On the average of every 3.86 years a six-inch rodent by the same name sweeps down from the hills of Norway in hordes, destroys everything in its path, and continues on until it reaches the sea. But it doesn't stop at the water's edge. It continues on, destroying itself by drowning. A few, who for some inexplicable reason remain behind, become the nucleus for the new horde that will migrate toward the sea, on the average, 3.86 years later. The cause of the lemming's rush to death on such a regular schedule is not known.

Norway also has a 3.86-year cycle in the abundance of foxes, and in the United States the growth of limber pine seems to have an identical cycle length. What obscure force could possibly affect the growth of certain trees in America and also influence the lemmings and foxes of Norway? (pp. 24-28)

UNRECOGNIZED SPECIES

• The Loch Ness Monster and Other Unrecognized Mammals

THE LOCH NESS "MONSTER"

Anonymous; *Nature*, 132:921, 1933.

Since early in the past summer, newspaper references have become more frequent and more precise, relating to the presence of an aquatic "monster" in Loch Ness. After its reported occurence, the writer spent a forenoon in July by and on the Loch without seeing anything unusual, and up to the present the "creature" has been chary of exhibiting its paces to a skilled naturalist. Experience of alleged wonders and the results on investigation--where investigation was possible--lead to deep scepticism concerning sports by inexpert observers describing phenomena with which they are unfamiliar, and in the present case the variations in the description, or the observation of different phenomena.

Descriptions vary: several observers have "seen" an eel-like monster with humps upon its back, a few a creature with small head and long neck attached to an enormous body, and others something that looked like an upturned boat. As a rule, it is said to move swiftly through the water, sometimes to the accompaniment of a "flurry" of foam. Guesses at identity have varied even more than descriptions: from possibilities, such as large eel, grampus, porpoise, whale-shark, seal, otter, to improbabilities such as sunfish, crocodile, "some amphibian", and so to impossibilities which include Plesiosaurus, "sea-camel" and the sea-serpent of the Middle Ages. Two professional zoologists have ventured identifications: one suggested an unstable mass of drifting peat, the other a white whale or beluga, but for various reasons neither is likely to be correct.

During the last fortnight, the "monster" has become insistent upon attention, mainly through its advocates. The Secretary of State for Scotland, in reply to questions, has written that he has communicated with the Chief Constable of Invernesshire, who had already stationed five constables at different places on the loch. None has yet seen the monster; but it is guaranteed protection should it appear and be threatened.

In the Times of December 8, Lieut.-Commander R.T. Gould, who has taken much trouble to examine the evidence, considers

that the creature is a "specimen of one of the rarest and least known of all living creatures", in fact as near as may be a relative of the traditional "sea-serpent", with large body supporting a small head perched upon a long neck, the base of which is fringed with appendages, "possibly gills"--he suggests a giant marine form (hitherto unrecognised by science) of the common newt. To a zoologist, Commander Gould's acceptance and analysis of at any rate some of the evidence appears to be uncritical and even credulous, and his conclusion unjustified.

By far the most important piece of recent evidence has been the publication of a photograph said to be of the "monster". The photograph lay undeveloped for four weeks, notwithstanding its possible interest, and its first reproduction in the newspaper in which the writer saw it was cancelled on the following day because the print had been "slightly retouched to throw up the details of the monster and its shadow". A second "wholly untouched reproduction of the photographic print" was produced, and if it is correct, it suggests a creature quite different from the animals it is alleged to represent. But to the writer there still seem to be on the print indications that it is not a direct contact print from an untouched negative and until he has critically examined the negative, he is not prepared to hazard a suggestion.

So far, one can say that although the evidence is not consistent, and that although much that has been said about the "monster" is, to put it mildly, uncritical, if the evidence is to believed at all, there may be in Loch Ness a creature, which if not unusual in its own habitat, is unusual in its surroundings in a Highland fresh-water loch.

LOCH NESS MONSTER: A REAPPRAISAL

Burton, Maurice; *New Scientist,* 8:773-775, 1960.

Never before, in any single year, has the Loch Ness Monster received such publicity, including two television programmes reaching millions. Nor have there previously been so many organized attempts to study the problem on the spot. Yet the hard fact remains that we are no nearer to knowing whether there is some large unknown animal in the loch, and even less have we any further clue to its identity. From supposing there might possibly be an animal there, with tentative suggestions as to its order, we have passed this year to positive assertions on its existence and some have even named the genus to which it belongs. Indeed, if we are to believe all we read, see and hear, it belongs to no fewer than three distinct genera of plesiosaurs!

As one who has studied this problem as closely as anyone now living, and who has for the last 27 years maintained a belief in the possiblity that it might be a large animal, perhaps I may be permitted to draw attention to some of the flaws in the arguments now being offered, if only to apply a brake to some of the un-

realistic suggestions which are gaining currency. In December, 1933, I was wrongly quoted in the press as saying that the Monster was a walrus. In September, 1951, in a broadcast programme, I propounded the giant eel theory, publishing evidence in support of this in August, 1953. After reading Constance Whyte's book, More than a Legend, published in 1957, I became converted to the plesiosaur idea, or at least to the idea that the animal might be of the plesiosaur type. Today, I am less sure that any of these ideas hold water.

My doubts arise from three independent lines of study. A year ago I made a fairly complete analysis of the accumulated eye-witnesses' depositions. From this 1 attempted to construct my hypothetical model. They, in the hope of resolving some of the difficulties, 1 made a close study, by direct observation and by accumulating photographs of the behaviour of aquatic animals, to test my hypothetical model. Finally, this last June, I had the opportunity to go to Loch Ness with a party of observers who kept a close and unremitting watch on a fair stretch the loch for all the daylight hours during a period of eight days, a total of not less than 162 hours of continuous watching.

From my own experiences and those of other observers, there is one statement more true than another: it is that the Loch Ness Monster comes to the surface with surprising infrequency. We know that some aquatic reptiles can remain submerged for long periods without coming up to breathe, and a turtle had, under experimental conditions, remained submerged for eight days. But this is not normal. And if the Monster were a mammal, as some people claim, its visits to the surface would be more frequent.

That there is a Monster in Loch Ness 1 have no doubt. 1 am, however, less sure than formerly of its identity. In fact, having since my return from Scotland gone over once again my analyses of the eye-witnesses' depositions, 1 can only regard the Monster as the most versatile animal 1 have ever met, in colour, size, shape and general behaviour. That, no doubt, was why 1 had experienced some difficulty in constructing my hypothetical model. But 1 would go further and say that at least 75 per cent of the eye-witnesses' descriptions relate to a body whose behaviour fails to conform with that of any aquatic animal 1 have been able to study.

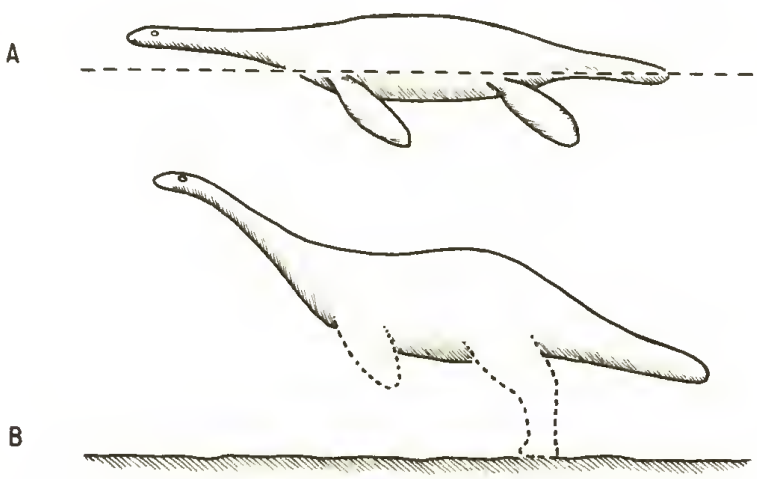
. . . . .

One of the strong supporting arguments for the reality of the Monster--and it is one 1 have used myself on occasion--is that there are similar stories to those told for Loch Ness, from other Scottish lochs, from western Canada, Iceland and the Norwegian lakes. The descriptions are so similar--the "upturned boat", the several humps, the tremendous disturbances of the water, even the head and neck and the large eyes--that one is tempted to say that "fifty million people can't be wrong". Unfortunately, as Elizabeth Skjelsvik has shown in a recent paper, on the two occasions that the Norwegian couterpart of the Loch Ness Monster have been investigated they have been found to consists of large

mats of rotting vegetation brought to the surface by marsh gas generated by their putrefaction. It would be rash to say that this same explanation can be advanced for the sightings elsewhere in the world, but it does suggest a more feasible and plausible explanation as well as a new line of enquiry. Certainly, some three-quarters of the eye-witnesses' depositions for Loch Ness would seem to fall into line with this. From all accounts it now appears that the combined universities' team at Loch Ness this summer had an object answering to this description under observation for two hours, without attempting to photograph it or to take a boat out to investigate it at close quarters--a great opportunity missed.

It has been often said that even if the Loch Ness Monster is a myth it would be a pity to prove it so because people like a mystery, and they like a monster. I agree with this, but I also find it an exciting possiblity that mats of vegetable rubbish could be the cause of it. In addition, even in my new and chastened mood, I cannot reject more than 90 to 95 per cent of the evidence. There still remain a number of convincing accounts of a long neck and head seen at close quarters, at distances of about thirty yards; there is still the photograph by the London surgeon showing a long neck protruding from the surface of the water; and there are several accounts of the Monster seen on land which, accompanied in one instance by sketches, cannot readily be dismissed as motor-boats, vegetable mats or even otters.

When we have reduced the quasi-acceptable evidence to these dimensions, however, the plesiosaur theory begins to look a bit tattered. It is worth while re-stating the verbal description of the most convincing of the sightings on land, that made by Mr. Arthur Grant in January, 1934. He described it as 18 to 20 ft



(A) Typical form of the plesiosaur. (B) Reconstruction of an animal seen on the road by Loch Ness in 1934.

long--a far cry from the 60 ft length we are now asked to accept --with a long neck, small head, long tail, small and flipper-like fore-limbs and apparently powerful hind-linbs (they were not clearly seen). It crossed the road in two bounds like a kangaroo and entered the water "with a huge splash".

This may or may not have been a plesiosaur, but if it was it would be difficult to say from this description, or any other we can piece together from the available evidence, to which genus of plesiosaurs it belonged. And it may not even have been a plesiosaur at all.

As I have said, there are monsters in Loch Ness. They may be of several kinds. They may be animal, vegetable or mineral--or all three. We have no more than started to investigate the problem and there are sufficiently exciting possibilities to make work on it amusing as well as scientifically profitable. This does not mean that our approach to it need be immoderate nor our claims extravagant.

MONSTERS BY SONAR

Anonymous; *Nature,* 220:1272, 1968.

The Loch Ness monster has once more been disinterred from its proper habitat in the underworlds of fable. Dr. Hugh Braithwaite and Professor D. Gordon Tucker announced in the Christmas number of New Scientist that they have detected echoes from the Loch which they find a "temptation to suppose must be the fabulous Loch Ness monsters, now observed for the first time in their underwater activities!" The furtive kraken has flown into the headlines on the viewless wings of sonar. A new type of equipment developed at Birmingham University was set up to monitor the Loch during two weeks in August. Two objects, one about 50 metres in length and the other much shorter, are believed by their fast rates of ascent and decent--up to 7.5 metres a second--to be groups of large animals. On the advice of their biological colleagues the two engineers exclude the possibility that shoals of fish could attain these speeds, but they say that studies with more refined equipment are needed before definite conclusions can be drawn.

The Loch Ness monster seems to have been invented in 1933, the date when it first allowed itself to be photographed. Before this time few, if any, people claimed to have seen it. The most famous photograph of the monster was taken by a London surgeon in 1943. Dr. Maurice Burton, a zoologist with a long-standing interest in the monster's affairs, believes that this photograph shows an otter in the act of diving, and that all other films and photographs so far published are of commonplace objects such as otters, birds or floating debris. The monster legend holds that Loch Ness, once an arm of the sea, was pinched off by movement of the land, so trapping the monsters in its salty depths. In

fact the Loch was excavated by Pleistocene glaciers and has always been a fresh water lake. Although parts of Scotland have subsided some 100 feet in Recent time, there is little chance of marine life having been washed into the Loch, let alone surviving there.

The sonar equipment used by the Birmingham engineers has been tested by the Fisheries Laboratory at Lowestoft, where it seems to have been discovered to be prone to ambiguities. For example, two targets at the same range but different bearings tend to appear on the screen as a single object while, under certain circumstances a shoal of fish swimming horizontally through the sonar beam could give the impression of a rapidly diving object. In other words, there is little reason to take seriously the claims of Dr. Braithwaite and Professor Tucker to have found a monster.

NAMING THE LOCH NESS MONSTER

Anonymous; *Nature,* 258:466-468, 1975.

On August 8, 1972, a team from the Academy of Applied Science, Boston, Massachusetts, working in conjucton with the Loch Ness Investigation Bureau of London, obtained what seems to be the most precise evidence on which to base such a description.

Two consecutive underwater photographs were taken by a stationary time-lapse camera with strobe flash, operating automatically at a depth of 45 feet in Loch Ness, along with a simultaneous sonar trace. The photographs have been computer enhanced at the Jet Propulsion Laboratory in Pasadena, California, a technique which can 'improve' the image by comparing adjacent grains electronically so as to remove haziness, but cannot alter shapes or otherwise falsify the record.

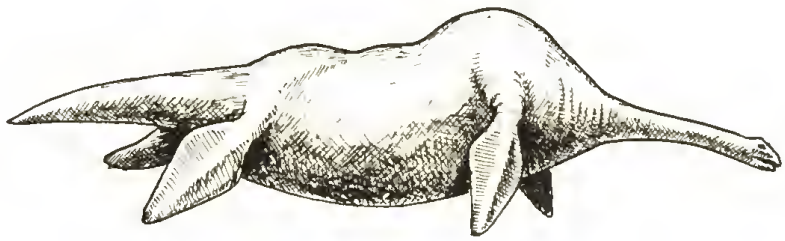
A black triangle in one corner of the photograph is caused by the edge of the strobe flash apparatus, and should be disregarded. The pictures show a flattened, diamond-shaped fin, flipper or paddle, in which the limb structure is not quite central. Calculations from optical data corroborated by simultaneous sonar recordings suggest that the paddle is about 2m long. Given its function, the 'main spar' of the paddle is likely to be nearer the leading, rather than the trailing edge, suggesting that it is a right-sided paddle.

A neck would be likely anterior to a forelimb, and a wider body posterior to it; since the opposite appears to be the case the photographs are assumed to show a right hind limb. The strobe light illuminates an area of the animals' back and belly with a rough skin-texture. In the upper photograph there is what may be some suggestion of ribs.

Although these two photographs of the hind flipper are the main basis of the description, the flipper-length is thought

to be some 2 m, it is possible, using the evidence from other photographs and from sightings, to indicate some further features and dimensions of the animal. A total body length of 15-20 m seems possible including a neck of 3-4 m with a rather small head which may have some horn-like protuberances. Moving-target-discriminating sonar displays have provided body length measurements of the order of 15 m, and the underwater automatic strobe photography has provided support for the reports of a long neck.

Frequent descriptions liken the back to 'an up-turned boat', and both still photgraphs and films show this configuration. Further underwater photgraphs taken in June 1975 may show other aspects of the same species, including a view of the head, neck and body. (p. 466)



Possible configuration of the Loch Ness monster as suggested by photographs and eyewitness accounts.

SEARCH FOR THE LOCH NESS MONSTER

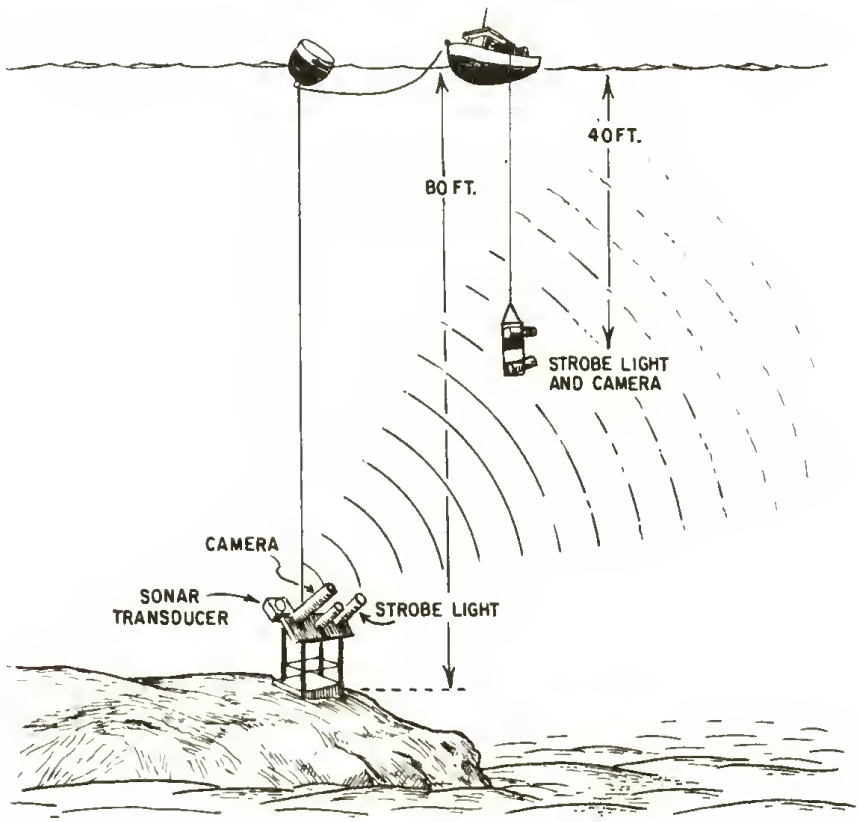
Rines, Robert H., et al; *Technology Review,* 78:25-40, March/April 1976.

A Computer Expert on the Loch Ness Photos. The following is from a report on the Loch Ness photographs by Alan Gillespie, a computer expert with the Jet Propulsion Laboratory, California Institute of Technology, who performed the computer enhancement on the photographs. It was presented at the symposuim in Parliament by Isaac S. Blonder of the Academy of Applied Science.

"1972: Three frames taken while sonar showed large animals in or near the camera field of view themselves showed unusual shapes. These shapes were not artifacts, and did not appear in hundreds of frames taken when no sonar echoes were reported . . . Assuming ranges indicated by the sonar were correct, the size of the animal or animals seen in these pictures agreed with size estimated from the sonar record. One animal may have had a 16-foot body.

"One frame (the 'flipper') showed a fairly distinct, coloured object which I inferred was the animal or a portion of the animal. A final frame showed two objects, an interpretation which was consistent with the sonar record. One was much farther from the camera than the other, according to the sonar . . . the distant profile was about 12 feet long. The 'flippers' hanging down from it were about 4 feet long.

"1975: One picture showed a body with a long neck and two stubby appendages . . . the second frame appeared to show a neck and head, with the head closer to the camera than the body . . . the neck was reticulated. The head supported projections . . . I see no evidence that they are pictures of a model, toy . . . or whatever. I emphasize: I detect no evidence of a fraud. These objects are not patterns of algae, sediment of gas bubbles." (p. 30)



Equipment arrangement during the 1975 American expedition to Loch Ness

CAMERAS, SONAR CLOSE IN ON DENIZEN OF LOCH NESS

Wiley, John P., Jr.; *Smithsonian Magazine,* 7:96-102, June 1976.

Unaware of media and scientific controversy over their existence, a "population of large animals" seems to live in the cold, peat-dark waters of Scotland's Loch Ness, eating fish and occasionally breaking the surface. No remains have ever been found, there are no living specimens in zoos or dead ones in museums, but new evidence has persuaded a number of expert observers that a Loch Ness "monster" is there.

The views of George R. Zug, a curator of reptiles and amphibians at the Smithsonian's National Museum of Natural History, are significant. Summarizing the evidence before a packed auditorium, Zug said:

"I started as a skeptic. Now I believe there is a population of large animals in the loch. I don't have any idea of what they are; I have no way of identifying them. And I'm not going to try to identify them until I meet one eye to eye."

Some of the evidence that is changing minds in the scientific world is on these pages. These photographs were taken simultaneously with sonar readings that revealed moving objects 20 to 30 feet long. The pictures appear to show the head, neck and upper body of an animal moving vertically; a diamond-shaped flipper attached to a body; and a close-up of a head. They are from 16-millimeter film. The frame showing the flipper was computer-enhanced at the Jet Propulsion Laboratory in Pasadena, using the same techniques developed for pictures returned by planetary probes.

. . . .

Surface surveillance continues under the coordination of the Loch Ness Phenomena Investigation Bureau (the sort of title that could identify a Smithsonian activity), headed by a Member of Parliament, David James. Coverage of a lake 24 miles long and a mile or more wide is not easy, however, especially when some of the shoreline is inaccessible. In addition to its own visual and photographic surveillance, the bureau also investigates and compiles data about other sightings.

Underwater photography is an obvious answer, but that is not simple either. The lake plunges to depths of 700 feet (975 in one spot). More inhibiting, the water is full of peat particles that quickly attenuate artificial light and make it impossible to photograph anything more than about 30 feet away.

The best answer so far seems a combination of underwater photography and sonar, a sort of radar using sound waves that was perfected to find submarines. That was the system used by Rines and his team to obtain these pictures. Rines began to investigate Loch Ness in 1969. In 1970 he returned with a sonar rig and recorded moving objects that were 10 to 50 times larger than the fish detected many times before.

In 1972 he first used both an underwater camera with a strobe light and sonar. About 1:40 A.M. on August 9, when he could

see salmon jumping to get away from something, the sonar showed a large object moving through its beam, followed shortly by a second large object. A photograph taken at exactly the same time shows a blurred object. Experts who looked at the sonar records were not told of the photograph, yet identified one part of the sonar image as an "appendage," about ten feet long, on one of the two objects. That appendage is believed to be the flipper in another photograph. In a third photograph, taken 45 seconds earlier, the flipper appears in a different position. It had moved during those 45 seconds. Based on focusing considerations, the Boston group estimated the flipper to be four to six feet long.

(It was at this point that Zug became directly involved. He and a colleague were asked to look at the pictures and see if they could see anything that would match up with the large sonar blips. He said they could clearly see salmon, eels and crustaceans in some of the pictures, but could not make out what was on the "monster" frames. When the same pictures were computer-enhanced, Zug said, he then saw what he calls a "flipperlike structure associated with or attached to a robust body.")

For the next two years the Rines team came home empty-handed. Then last year they obtained more photographs, enough to set off a furor. The torso shot . . . . was made at 4:32 A.M. on June 20; the head picture was made at 11:45 A.M. the same day. Believers see in the torso picture a long, curving neck and the front part of large body with two flippers showing. The middle part of the neck is curved so far back that it is shadowed and does not show up well. Critics are not so sure. Some have dismissed the picture as merely a cloud of bubbles. Others argue that is is the prow or stern of a sunken Viking ship; the "flippers" then become the oars. The same critics contend that the "head" shot could just as well be a carved dragon head of the kind used to embellish Viking vessels.

Whatever was caught in the head photograph was close enough to move the camera in the preceding and following frames the camera has been flipped up, so that it photographed the bottom of the boat from which it was suspended, and the water surface.

In that picture the head is about five feet from the camera and appears to be about two feet long. Is it really a head? Zug thinks so, "I see bilateral symmetry--matching left and right sides," he said, and quickly added, "I hear snickers" from unconvinced colelagues. Others have suggested it is the decomposing head of a Highland steer or a stag.

The Rines pictures have suffered something of the same fate as the 1934 surgeon's picture. A scientific conference to discuss them was to have been held in Edinburgh last December. But premature publication of a chapter from a popular book and hyped-up publicity in the press caused the sponsors to withdraw. Only the hearing in the House of Commons saved the results from the limbo reserved for UFO reports.

KOSKOLTERAS RHOMBOPTERIX?

Anonymous; *Nature,* 270:656, 1977.

A Loch-Ness-type monster, 15 m long with a head 2 m long by 1 m wide has been reported in Lake Kos Kol, Kazakhstan. An unnamed commentator on Moscow radio observed that since several "extinct" species have recently been discovered to be still surviving, he considered it quite possible that "unknown creatures of the kind reported in both these lakes" might, indeed, exist. What he did not suggest, however, is any kind of joint Soviet-British (or Kazakh-Scottish) study project. This seems a strange omission; joint scientific research is a major feature in all discussions of mutual cooperation, and a possible monster investigation would enliven what too often become dull routine talks on oil drilling equipment and fertiliser plants. Indeed, should the two creatures prove to be of the same species, an interbreeding project might not be beyond the bounds of all possibility!

THE FLATHEAD LAKE MONSTER

Church, Tim; *Pursuit,* 8:89-92, 1975.

The following report is the result of an extensive, but as yet incomplete, survey of reports of unidentified marine monsters in Flathead Lake, Montana. The area round the lake was opened to settlement by whites in 1910, and reports of the creatures have accrued steadily since that date. The lake was navigated by steamboats as early as 1880 and Indians inhabited the vicinity prior to that. The earliest reports made by whites originated with the steamboat crews, but it appears that the Indians recognized the existence of a large and unusual creature in the lake long before that. Descriptions of the creature reveal similarities with reports of other unidentified lake monsters.

In the traditions of the Kutenai Tribe of American Indians there is preserved a legend of a time long ago when food supplies became dangerously scarce during the freezing winter months. The elders of the tribe determined that the best chance for survival was to leave the islands in the midst of Flathead Lake which had been their home for generations. While the famished band fought across ice toward the shore, two maidens noted what they took to be an antler protruding from the frozen surface of the lake. Ignoring the protests of her companion, one of the women began to saw away the antler. As she was half through, the ice erupted around the scene and a huge monster emerged from the icy water, shaking its injured antlers in rage. The two maidens, who were well versed in the arts of witchcraft, turned themselves into a ball and a round buckskin target, respectively, and by this clever artifice made good their escape.

Another Indian legend, in the tradition of the Kalispell Indian

tribe, tells of the adventure which befell a group of braves who were camped on the shore of the lake when a great monster rose out of the water and attacked them. The braves shot arrow after arrow at the beast, but the arrows merely bounced off. Seeing this the Indians turned and fled. All, that is, except for one particularly courageous warrior who stayed to fire his last arrow. The monster caught him and carried him down to the depths of the lake. From that time on the Indians preferred to fish in other lakes.

In 1885 James Kern, skipper of the U.S. Grant, the first steamboat to operate on the lake, spotted what he thought to be another boat coming toward him. As it drew nearer, he was startled to discover that what he thought was a boat was in fact a large animal with an appearance similar to a whale. A passenger grabbed a rifle and fired at the animal, but it immediately submerged and disappeared from sight.

Thirty four years later, in 1919, another steamboat nearly collided with an unknown marine animal. The captain of the City of Polson, on a run between Somers and Polson, saw what he supposed at first to be a giant log floating in the lake. He wheeled about to avoid it, and was astonished to see the "log" suddenly come to life and maneuver away from the steamboat. Fifty passengers caught a glimpse of it before it swam out of view.

In the 1920's commercial salmon trawling was started in the lake. There were frequent reports from the fishermen that their nets, which were designed for capturing 40 pound bull trout, were found shredded.

The May 27, 1937 issue of the local newspaper Flathead Courier carried a story concerning the experience of a certain Mr. L.J. Elkins. It seems that Mr. Elkins was fishing from a bridge over the Flathead River below Polson. While intently watching the river for signs of fish, he spotted a strange animal or fish swimming downstream. The creature was described as six to seven feet long, with long brown hair on its neck and broad head. The head was held out of the water as it swam. Elkins said that his first impression was that it was a large dog. He said that it most definitely was not a bear, beaver or otter, and it is probable that a sportsman living in this area could make such an identification easily.

The following is a chronological summary of monster sightings: 1949 - A group of tourists reported seeing a "seal" near Bigfork. Later, probably in August, a tourist told people in Somers that he had watched something as big as a Japanese two-man submarine pass close to the boat from which he was fishing. Several days later the H.W. Black family from Polson were returning when they spotted a "big fish", described as about 20 feet in length and very dark in color. In September several witnesses saw a similar creature, but gave it a length of 12 to 18 feet. A few days earlier Mr. and Mrs. Bob Racey of Big Arm struck an unknown object as they were boating on the lake. They turned back to investigate, but could not determine what their boat had struck.

1950 - two men riding in a speedboat saw the monster and gave chase. The monster threw up so much spray that the boat's engine was flooded. (This one sounds a bit far-fetched, but is included here for the record.)
1951 - On August 1, a "big fish" surfaced and sped in front of a boat filled with people on Skidoo Bay. The "fish" was reported to have thrown a spray of water 3 feet high and 20 feet long, which continued until it finally submerged in the middle of the bay.
1952 - In July two vacationers from Spokane reported a large "fish" breaking water in the Narrows, and thought that it was chasing something.
1955 - June, a Mr. and Mrs. Howard Gilbert said that while driving along the eastern shore of the lake they spotted what appeared to be an overturned boat. They stopped the car, and upon closer inspection discovered that the appearance was actually two large "fish", the larger of the two being about 10 feet long. They observed the fish until they submerged. On the same day the Degolier family noticed what they also took to be an overturned boat off the east shore. Stopping to investigate, they found it to be a "large fish" rolling in the water. In September, a large group of fish were observed by a crowd of about 100 people. The witnesses apparently presumed that these fish were sturgeon, and gave the size of the fish as ranging from three to 12 feet.
1960 - Two men fishing in Indian Bay saw a big dorsal fin streaking through the water, and said that the fish had a length of about six feet. Mrs. C.E. Blankenhorn together with her father and her daughter, saw "something large in size swimming against the current" in a creek near the Polson golf course. The next Friday the Zigler family was lounging near the lake at the Polson country club when Mrs. Zigler noticed waves crashing against the shore. Because there was no wind or boats in the vicinity, they thought it strange and decided to investigate the source of the waves. Walking to a nearby pier, they saw "something" rubbing against the pilings as if scratching itself. Mr. Zigler then started back to the house for his rifle. Mrs. Zigler, alone but curious, went closer and was confronted with a "thing" which raised its head out of the water. "It was a horrible looking thing, with a head about the size of a horse... and about a foot of neck showing." She screamed at this sight, and her husband returned on the run, arriving just in time to see the creature swimming in an easterly direction at a "great rate of speed" with a very large wake. Mr. Zigler was familiar with sturgeon and stated that the creature he saw was definitely not that species. The following Friday the creature was reported in three locations. Lorin Nostad and Dick Schlatter reported "some kind of water going outfit" cavorting near Koss Landing at about 6:30 p.m., and several minutes later it was reported at other nearby docks. The Zigler family provided the third sighting, this time seeing it in the vicinity of the country club.
1962 - In September a group of five women were reported to have seen a large, fast moving object in Polson Bay. Maynard Nixon

and his brother watched "some kind of swimming object, not a sturgeon, about 25 feet in length." They watched this creature, which had a triple hump, for 30 seconds as it swam past the old Polson City docks. Mr. Nixon stated that he had been very skeptical about the monster stories until that day. Two days later Mrs. A.D. Bartlett and her daughter sighted a large black object swimming off the old Polson city docks. They described it as jet black, with no fins, and not undulating in the water as a fish might.

1964 - July saw several attempts to catch and/or observe the monster in a fairly systematic fashion. A local fisherman attempted to catch the creature by using whole chickens and hunks of liver as bait, and a scuba diver four days searching the lake. Neither effort met with any success.

1965 - On August 1, Earl House and his son, visiting the lake from their home in California, were registering at a lakeside hotel at Polson when the receptionist noticed something passing an orange buoy in the lake. The receptionist followed the object with binoculars and saw that it swam with "up and down" movements and had a silvery dorsal fin. There were no boats in the area at the time.

On August 19, the E.E. Funke family were surprised to notice a strange body moving in the water past their dock. The creature had a large fin on its back and had a shiny black color. One of Mr. Funke's sons attempted to photograph it with a movie camera, but the "fish" disappeared before he was able to start filming. See p-32. Two days later an Oregon woman sighted a similar object in Indian Bay, and estimated that it was about 20 feet long, with a shiny black, long and narrow body.

1969 - In May, a Mrs. Verne Thomas of Dayton reported that she spotted a wide wake on the lake. Apparently the waves were being formed by a part of the creature protruding about 18 inches above the water. On June 1 two men who were crossing Polson Bay saw what initially appeared to be a log in the water. Approaching it slowly to avoid a collision, the men were amazed when the "log" suddenly shook and sped away. The men followed it until it submerged. On August 24 the Ron Buchanan family saw a 20 foot "fish" swim by their vantage point on the shore. They said it was gray in color, with a three foot top fin and a large tail.

1971 - On July 31, Ray Love spotted a 10 to 12 foot creature swimming near his dock. It was black on top, with yellow or buff underneath, and no scales were visible; the animal swam with an undulating motion.

1972 - A very brief local report of a large "fish" in Peaceful Bay.

1974 - In September an eight foot long "fish" was seen off Angel Point by several Lakeside residents. See O-11. An undated report, probably made in 1965, tells of a sighting by a Mr. and Mrs. Dean Howell, who saw a creature near Bird Island. "It was huge and it was black. But it couldn't have been a boat, it went up and down in the water, splashed around and circled."

In 1951 the inevitable happened. The Polson Chamber of Com-

merce got the brilliant idea of using the monster as a publicity gimmick to lure tourists and fishermen to the lake area. The potential tourists were led to believe that it was indeed a monster alive and well in the lake, and in a flash of genius, the Chamber tried to convince potential fishermen that the lake contained nothing less than a giant sturgeon. A promotional group calling itself Big Fish, Unlimited was formed to beat the drums for all this, and a $1000 reward was offered for the Giant Sturgeon. A mock Grand Jury investigation was held to determine if the creature really existed. Unfortunately, all this nonsense tends to taint the authenticity of the reports made during the period from 1950 through 1955, when the promoters quietly folded their gaudy tents.

Flathead Lake is the largest lake in the northwestern United States, lying in Montana, where it was gouged out some two million years ago by the glaciers. As the ice receded it left a 500 foot high moraine which is now at the foot of the lake, and at the same time the ice barriers damming Lake Missoula melted and released the lake's waters. Over the ages erosion and earthquakes transformed the lake into its present state.

The lake is 27 miles long and 15 miles wide, with a total area of 188 square miles and 123 miles of shoreline. It is nearly 350 feet deep in some places and maintains a temperature of 37 degrees. At the northern end the glacier-fed Flathead River and Swan River flow into the lake. At the southern end the Flathead River flows out, and about five miles downstream is the Kerr Dam. The river ultimately joins the Columbia River and continues to the Pacific coast.

The lake is surrounded by a number of small towns and summer homes dot the shore. Roads run fairly close to the lake throughout its circumference, with the exception of the north end where the terrain is marshy.

A number of suggestions have been made by way of explaining these puzzling reports, and more will be said on this further on. The most persistent explanation is that the witnesses have seen a giant sturgeon. This seems to make some sense but it poses almost as serious a dilemma as does the possible existence of a totally unknown marine animal in the lake.

There are 22 species of fish common to Western Montana, the largest being the white sturgeon, Acipenser Transmontanus. This, the authorities insist, is the only sturgeon found in Western Montana. The White Sturgeon grow slowly, but can become 16 feet in length and weigh from 300 to 1000 pounds. They are mainly an ocean fish but can remain land-locked, in which event they feed primarily on other fish and crustaceans. They are dark gray in color, with a pale belly, appear armored due to their bony plates, and have one dorsal fin, two lateral fins, and two ventral fins. The sturgeon's habits are not too well known, but it has been established that their spawning season runs from spring to early summer.

Whether the sturgeon inhabit Flathead Lake is the subject of intense controversy. In his book Fish Guide to Intermountain Montana, George F. Weisel states that "There is no possibility

that sturgeon can swim up the Columbia to the lake (Flathead Lake)." A sturgeon was allegedly caught in the lake in 1955. This fish was seven and a half feet long and weighed 181 pounds, but there is reason to believe that it was a spurious product of the publicity generating operation mentioned previously. Mr. Weisel puts it thus; "...and it is hardly conceivable that this apparently young specimen could have attained its size landlocked in a lake so poor in food." So it seems that everywhere we turn the Flathead Lake mystery becomes murkier.

Other fish in the lake include several species of trout, catfish and salmon, the largest of these being the salmon which attain a maximum weight of about five pounds. I have no definite information of microorganisms in the lake, but because the lake is glacier-fed it is unlikely that it is rich in these. Beaver and otter inhabit the area, but there seems no reasonable connection between these animals and the monster reports.

The "monster" is almost always described as black in color, with some reports telling of a yellow underbelly. Only the 1937 report gives the color as brown. The reported size varies from three to 25 feet, with the most widely reported size being six to eight feet. Witnesses have been disappointingly vague about the body shape. Four reports show a dorsal fin colored either black or silvery, but no other fins have been mentioned. Only the Zigler sighting provides any idea of the creature's head, and in this case the witnesses said it was the size of a horse head, leaving the shape of the head to conjecture. A tail appears in only one report.

A rather common characteristic is the animal's great speed and playfulness in the water. Only two reports describe more than one creature: in one instance two were seen, in another case a total of 36 were sighted. Sightings predominate in the month of September, although reports have been made throughout the year. Apparently the most active year for sightings was 1960.

It is natural that a great deal of speculation has been advanced to explain the phenomenon giving rise to all these reports in Flathead Lake. Those who are familiar with the variety of ingenious theories attempting to explain the Loch Ness sightings will not be surprised to learn that some people in the lake area are convinced that a miniature submarine is being piloted by persons unknown, and for purposes unknown, other than perhaps the befuddlement of the populace. A seal may very well have been seen by the fisherman in the 1937 report. Sturgeon would seem to be a fairly good candidate for the monster reports, or at least the most reasonable suggestion thus far, but there are many discrepancies here in addition to the failure of anyone to catch a sturgeon over the very long period during which the lake has been extensively fished. Even if we can credit the sturgeon mentioned previously, it would seem very strange that only one should be brought up in all these years. Of course, no one has yet hooked or otherwise captured a "monster" in the lake, either.

The Montana newspapers have linked the Flathead monster sightings to similar reports from Lake Winnebago in Wisconsin, where a large sturgeon was said to be the cause. Without fur-

ther investigation it would seem a bit premature, however, to accept this explanation at face value. Since the giant sturgeon would have to subsist mainly on other fish, I thought that the peak of "monster" sightings ought to occur in the salmon spawning season. This, however, occurs in November and December, while the greatest number of monster sightings take place in September, with August the month in which sightings occur with the next highest frequency.

It should be emphasized that this is only a preliminary report, based in the main on newspaper accounts. In the near future I intend to interview as many witnesses as possible to further document the phenomenon and to obtain more detailed descriptions. I would very much appreciate receiveing comments from the readers of Pursuit.

• Unicorn Tales

AN ATTEMPT TO PROVE THE EXISTENCE OF THE UNICORN

Laterrade, J. F.; *American Journal of Science,* 1:21:123-126, 1832.

To say that it is impossible that there should be, or, at least, should have been such an animal as the land unicorn, would be to go astray from acquired knowledge, to credit an absurd fable, in a word, to affect singularity. Meanwhile, if we can show that the account of this animal has in it nothing remote from the ordinary laws of nature, that several authors have made mention of it, and that there is found no proof, that can overthrow the ideas, that have been formed respecting it, its existence is thereby established. Let us endeavor to illustrate our threefold proposition.

1. The account of the unicorn has in it no appearance of the fabulous. Let us hear our opponents themselves. "It is said," says the Dictionnaire des Sciences, "that this is a timid animal, inhabiting the depths of the forests, of the size of the horse, bearing in front a white horn five hands in length, and with brown hair hanging over that, which is black." The difficulty can fall only on the long horn, with which the front of our quadruped is armed. Its horizontal direction, its position, its being single, the form of the animal, that carries it; these, it is said, are by no means natural. But then the defense of the narwal, which has a horn fourteen feet in length, that has a horizontal direction, proceeding from the upper jaw, and finally, belonging to an inhabitant of the waves, is certainly far less natural. Yet this is a cetaceous animal, concerning whose exis-

tence there is no doubt, and which is common in the northern seas; and the armed fox, which M. Duhamel, after M. de Mannevillette, made known to us, presents a phenomenon still more extraordinary, since it has a horn, small indeed, but placed on the backside of the head; a most singular character, and altogether peculiar to this species.

2. Several authors have spoken of the unicorn. First, if we open the sacred scriptures, we shall see that David and the prophets were well acquainted with it. But as the commentaries speak of this animal only in a figurative manner, we respect their silence, and pass over a proof which alone would, perhaps, be sufficent for our purpose. It satisfies us to know that they have made mention of it.

Pliny, whom none will suspect of connivance with the sacred writers, gives a description of the unicorn in his eighth book, adding that it cannot be taken alive.

Accordingly, Hyeronymus Lupus and Bathasar Tellez found, in Abyssinia, a quadruped of the size of a horse, and whose front was armed with a horn.

Finally, the respectable Leibnitz announces, in his Protogea, on the authority of the celebrated Otho Guerike, that, in 1663, there was dug up, from a quarry of limestone in the mountain of Zeuniqueslberg, in the territory of Quedelimburg, the skeleton of a land quadruped, flat on the back parts of the head, but the head itself elevated, and bearing in front a horn about ten feet in length and terminated in a point. This skeleton was broken up by the workmen; nevertheless, the head and some of the ribs were sent to the princess Abbesse. These details are accompanied with an engraving.

3. As yet there is no sufficient proof found of the nonexistence of the unicorn. The account of it has no appearance of fable, and several authors, at different times and among different people, have mentioned it in a positive manner, as we have just seen. What further objection then is there? That the ancients attributed to the horn of our quadruped properties so extraordinary and ridiculous, that every thing relating to it can be no more than a fable. What! it would be deemed sufficient then that falsehood or ignorance should add to real facts, compared with which they should be regarded as mere tales! it would suffice that malice should spread the poisonous venom of calumny over the sacred truth, for which it ought henceforth to have no affinity! Where then shall we be? But, without straying from our subject, what animal is there a little extraordinary, concerning which there have not been suspicions, when the night of time has removed it a little distance from us? The giraff is an example as striking as it is recent; and the mammoth, whose remains have been discovered, has fairly overthrown such reasonings; and the shells, the inhabitants of which we have not yet been able to determine, will tell us with silent but irresistible eloquence that nature loses nothing by growing old. Besides, the bezoards, to which have been attributed properties scarcely less ridiculous than to the horn of the unicorn, do they not exist? Do not such things occur still with respect to animals, that live in parched countries,

where heat gives to vegetable juice a power, that is unknown in temperate regions? Nevertheless, it is unnecessary to dissemble that it would be in vain for all antiquity to testify in favor of this singular production, it would be in vain that the cabinets should furnish it to the curious, these recitals would be false, these productions would be the work of imposture, if the fact were not still repeated, or if our weakness could not perceive it!

Will it be objected that the moderns have never seen this animal? How many other species are there, which they have not noticed! New discoveries sufficiently prove this. Besides, the unicorn inhabits the interior of Africa, and precisely that part of it, of which we know the least; and in Africa, as well as in other countries, certain animals might well appear, at first, even on the coasts, and afterwards, when the number of inhabitants was increased, be confined to the center of the forests. A countless number of similar facts, sufficiently well known, may well excuse us from enlarging upon this. In short, let us, without being detained by unimportant discussions, come to the grand proof of the nonexistence of the unicorn; let us examine attentively and judge with impartiality.

For a long time there was exhibited a defense resembling ivory, white and channeled, of a very considerable length, and terminating in a point. It was asserted that it was the horn of a quadruped. Of this, however, notwithstanding all the researches, that were made, nothing could be discovered; from time to time these defenses became more numerous, no other part of the animal being united with it; finally, there was brought to Wormius the head of the narwal; then the question was decided, and because some too credulous persons had said that the tooth of a cetaceous animal was the horn of a quadruped, it was thence concluded that the unicorn had never existed, and consequently that it was only a fabulous animal, whose nonexistence was mechanically demonstrated by Kamper. Without detracting from the celebrity of this great anatomist, we do not cite his demonstration, persuaded that the beauties of nature and her admirable secrets cannot be explained by the laws of mechanics only.

Nevertheless, we may remark that Wormius, cautious in his inferences, is always in doubt; that he speaks of unicorn as he had heard it described before the king of Denmark, by an embassador from Congo; that Gmelin is not sure that the fossil unicorn, which is sometimes found in the earth, is the defense of the narwal; that, finally, if the narwal were unknown till of late, the unicorn, after being seen by the ancients, may not yet have been discovered by us.

Finally, is it not the hight of error and blindness to maintain the nonexistence of our quadruped by the existence of the narwal? It must be confessed that this would be to disguise the process of nature, that seems to delight in repeating the particular animals in each class, and that it is to regard as favorable to an opinion that, which is almost sufficient to overthrow it. Thus, as the ostrich among birds, and the highbunched coffre, among the inhabitants of the seas, are the representatives of the camel, and the fish zebra is of the quadruped zebra, so the unicorn of the

sea seems to prove the existence of the land unicorn.

We conclude, therefore, that we have satisfactory evidence, to say the least, that this animal may have existed, that it is possible that he exists still, and close by saying with the immortal Buffon: "It is not by contracting the sphere of nature and confining her within a narrow circle that we shall be able to understand her; it is not by making her act according to some preconceived ideas that we shall be able to judge of her or comprehend her; and we shall not be able to fathom the designs of the Creator by furnishing him with our ideas; instead of confining the limits of his power it is necessary to extend them even to immensity; it is necessary to consider nothing as impossible, to look for everything, and to suppose that whatever can exist, really does."

THE UNICORN

Anonymous; *Scientific American*, 4:150, 1848.

M. Antoine d'Abbadie, says the London Atheneum, writing to us from Cairo, gives the following account of an animal new to European science, which account he received from Baron Van Muller, who had recently returned to that city from Kordofan. "At Melpes in Kordofan," said the Baron, "where I stopped some time to make my collections, I met on the 17th day of April, 1848, a man who was in the habit of selling to me specimens of animals. One day he asked me if I wished for an A'nasa, which he described thus:--

It is the size of a small donkey, has a thick body and thin bones, coarse hair, and tail like a boar. It has a long horn on its forehead and lets it hang when alone but erects it immediately on seeing an enemy. It is a formidable weapon; but I do not know its exact length. The A'nasa is found not far from here (Melpes), towards the S.S.W. I have seen it often in the wild grounds; where the negroes kill it, and carry it home to make shields from its skin. This man was well acquainted with the rhinoceros, which he distinguished under the name of Ferit from the A'nasa. On June the 14th, I was at Kursi, also in Kordofan, and met there a slave merchant who was not acquainted with my first informer, and gave me spontaneously the same description of the A'nasa; adding that he had killed and eaten one not long before, and that its flesh was well-flavoured." Herr Rippell and M. Frosnel, adds M. d'Abbadie, have already spoken of a one-horned African quadruped; and I have with me some notes which tend to the establish the existence of perhaps two different kinds.

UNICORN DRAWINGS

Anonymous; *Nature*, 6:292, 1872.

In a communication from Natal, Mr. G. R. Blanche states that Mr. B. Bouwer had seen, in a stone cave in Namaqua-land, about twelve days from Lake Ngami, pictures of all sorts of animals, drawn by Bushmen, in which the unicorn was distinctly delineated. Mr. Bouwer added that an old Bushman at Ghanze told him that he had many years ago seen the animal, that it was very fierce, but that it had now gone away. He had heard, besides, other Bushmen speak in similar terms, of the reputed fabulous beast. Mr. Blanche concludes:---"My opinion is, that the unicorn existed recently in Africa, and that it is not proved to be extinct now, but that the probability of its being in existence now is not very great." He rests this conclusion on the general accuracy of such rude sketches by savages in other parts of the world besides Africa, asking, if the unicorn never did exist, why should drawings of it be made in Namaqua-land, Natal, the Transvaal Republic and Cape Colony, possessing the same general and one particular characteristic.

CONCERNING THE REAL UNICORN

Trotter, Spencer; *Science*, 28:608-609, 1908.

In a certain issue of Science (February 2, 1906, Vol. XXIII., p.195) Mr. C.R. Eastman contributed an exceedingly interesting article under "Notes on the History of Natural Science," on "The Real Unicorn." In setting forth the facts as to the origin of this fabulous animal, brought to the notice of the western world by Ctesias, Mr. Eastman concludes that the source of this strange creature of the medieval mind is to be traced to certain relief profiles described by Ctesias as graven on the walls of the Persian court at Persepolis and figuring some "Asiatic ruminant new to the Greeks with the two horns appearing in side-view as one." To the animal so depicted Ctesias gave the name of "unicorn" or "monoceros."

Unquestionably Mr. Eastman's view as to the unicorn's zoology position is probably close to the real facts. It remains to determine, if possible, what species of "Asiatic ruminant" can stand sponsor for the fabulous creature. Some horned beast known to the ancient Persians, the horns of which would appear as a single horn in profile and would point forward when the animal's muzzle was held downward as in the defense attitude or when grazing, could be the only one so pictured as to give rise to the idea of a "unicorn" or "monoceros." Such a beast, I think, may be seen in the male Nilghai (Boselaphus tragocamelus), an Indian antelope, ranging at present from the southern foothills of the Himalaya to beyond Mysore, though most abundant in the central parts of

Hindustan. Any one standing alongside of a Nilghai can see at once how the spikelike horns spring straight upward, bending slightly forward, and how the near horn hides its fellow.

The knowledge of this animal would undoubtedly have reached the ancient Persian civilization from the trans-Indus region, and the artists of the period would very naturally have graven but a single horn in bas-relief profile. Further evidence that this animal was known to the ancient Persians is to be found in the name itself--"Nilghai," or "Nylghan," being of Persian origin and meaning "blue bull." The species first became known to the modern world of Western Europe about 1745, and was described and figured in Philosophical Transactions for that year by Dr. Parsons, in a paper entitled "An Account of a Quadruped brought from Bengal, and now to be seen in London." In Philosophical Transactions for 1770 Dr. William Hunter published a very full account of the animal from living specimens brought to England, and bestowed upon it the native name "Nylghau."

As the unicorn of Ctesias failed to materialize in the fauna of any country, it was relegated to the land of fabulous creatures, and became conventionalized in the art of the ancient and medieval world. If, as Mr. Eastman points out, its origin is to be found in the bas-reliefs on the walls of Persepolis, then, undoubtedly, it must have been a figure from some living prototype, and this prototype could, it seems to me, be none other than the Nilghai, the only Asiatic ruminant with horns so placed as to give rise to such a conception.

• Rumors of Mermaids

MERMAID

Swift, Asa; *American Journal of Science,* 1:2:178–179, 1820.

First part of the day light variable winds and cloudy; at two P.M. on the larboard quarter, at the distance of about half the ship's length, saw a strange fish. Its lower parts were like a fish; its belly was all white; the top of the back brown, and there was the appearance of short hair as far as the top of its head. From the breast upwards, it had a near resemblance to a human being and looked upon the observers very earnestly; as it was but a short distance from the ship, all the afternoon, we had a good opportunity to observe its motions ahd shape. No one on board ever saw the like fish, before; all believe it to be a Mermaid.

The second mate Mr. Stevens, an intelligent young man, told me the face was nearly white, and exactly like that of a human person; that its arms were about half as long as his, with hands resembling his own; that it stood erect out of the water about two feet, looking at the ship and sails with great earnestness. It would remain in this attitude, close along side, ten or fifteen minutes at a time, and then dive and appear on the other side. It remained around them about six hours. Mr. Stevens also stated that its hair was black on the head and exactly resembled a man's; that below the arms, it was a perfect fish in form, and that the whole length from the head to the tail about five feet.

Communicated by Mr. Elisha Lewis of New-Haven, a respectable merchant.

MYSTERY OF THE MERMAIDS

Carrington, Richard; *Science Digest*, 42:33-39, July 1957.

The Times (London) of Friday, Sept. 8th, 1809, contained a letter sandwiched inconspicuously between the Ship News and the Price of Stocks that must surely have provided the highlight of the day's news. It was from a Mr. William Munro, a schoolmaster of Thurso in Scotland, and was headed The Mermaid Seen on the Coast of Caithness. The following is the substance of this singular communication:

> Dear Sir--About 12 years ago, when I was Parochial Schoolmaster at Reay, in the course of my walking on the shore of Sandside Bay, being a fine warm day in summer, I was induced to extend my walk toward Sandside Head, when my attention was arrested by the appearance of a figure resembling an unclothed human female, sitting upon a rock extending into the sea, and apparently in the action of combing its hair, which flowed around its shoulders, and was of a light brown colour.
>
> The forehead was round, the face plump, the cheeks ruddy, the eyes blue, the mouth and lips of a natural form, resembling those of a man; the teeth I could not discover, as the mouth was shut; the breasts and abdomen, the arms and fingers of the size of a full-grown body of the human species; the fingers, from the action in which the hands were employed, did not appear to be webbed, but as to this I am not positive.
>
> It remained on the rock three or four minutes after I observed it, and was exercised during that period in combing its hair, which was long and thick and of which it appeared proud; and then dropped into the sea, from whence it did not reappear to me.
>
> I had a distinct view of its features, being at no great distance on an eminence above the rock on which it was sitting, and the sun brightly shining.
>
> If the above narrative can in any degree be subservient towards establishing the existence of a phenomenon, hitherto

almost incredible to naturalists, or to remove the scepticism of others, who are ready to dispute everything which they cannot fully comprehend, you are welcome to it, from

Dear Sir
Your most obliged, and most
humble servant,
(Signed) Wm. Munro

Mr. Munro was not alone in his observation of this mysterious and romantic phenomenon. His letter to The Times had itself been prompted by the experience of a certain Miss Mackay and another girl, who had seen a creature of equally unusual physique bathing in a rough sea off the coast of Caithness earlier in the year. According to Miss Mackay, this creature's face was "round and plump and of a bright pink hue."

Although not actually observed to have a comb, every now and then it would lift a slim white arm above the waves and toss back a mane of long green hair.

Thus the two descriptions agreed together excellently and for a time there seemed every reason to hope that mermaids would be recognized as a valuable if unexpected addition to the marine fauna of Scotland.

Unfortunately, however, this romantic possibility was never to be realized. No further Scottish mermaids were seen, and in the absence of proof in the shape of a live specimen, or at least a corpse, naturalists preserved an aloof and dignified silence.

This story is a good example of the persistence into comparatively modern times of a legend that is nearly as old as the written records of man.

. . . .

The earliest naturalist to deal with mermaids in any detail was Pliny the Elder, whose famous Natural History appeared in the 1st century A.D., Philemon Holland, Pliny's 17th-century English translator, records his views:

"And as for the Meremaids," he says, "it is no fabulous tale that goeth of them: for looke how painters draw them, so they are indeed: only their bodie is rough and skaled all over, even in those parts wherein they resemble a woman.

"For such a Meremaid was seene and beheld plainely upon a coast neere to the shore: and the inhabitants dwelling neere, heard it a farre off when it was a dying, to make piteous mone, crying and chattering very heavily."

No one in 17th-century England would have regarded these words of Pliny's as particularly surprising. The existence of mermaids was as firmly established as the existence of shrimps. They were regularly seen off the coast of Britain, and travelers brought back tales of encounters with them from every corner of the seven seas.

One example is the following description taken from the voyages of Henry Hudson, published in London in 1625:

This evening (June 15) one of our company, looking overboard,

saw a mermaid, and, calling up some of the company to see her, one more of the crew came up, and by that time she was come close to the ship's side, looking earnestly on the men. A little after a sea came and overturned her.

From the navel upward, her back and breasts were like a woman's, as they say that saw her; her body as big as one of us, her skin very white, and long hair hanging down behind, of colour black. In her going down they saw her tail of a porpoise, speckled like a mackerel. Their names that saw her were Thomas Hilles and Robert Rayner.

. . . .

Many of the mermaid stories from the coasts of Africa and Asia have almost certainly been inspired by a distant or indistinct view of a dugong or manatee in the act of submerging. But the animal most likely to account for the various appearances of mermaids in cold and temperate latitudes is probably one of the world's 30 species of seals.

Not everyone will agree that seals could be mistaken for mermaids, and certainly when they are seen at close quarters in a zoo or under similar controlled conditions, the theory is a little difficult to swallow. But in a storm-tossed sea off the Scottish coast, or seen at a distance from a cliff top, the likeness is not really so very far-fetched.

The body of the phocid seals tapers to a point in the same way as that of the Sirenia, and their hind limbs are permanently extended backwards, so that the rear part of the trunk strongly resembles the typical mermaid tail.

Moreover, the plump, rounded, expressive face of the seal, its soft, intelligent eyes, and handlike foreflippers give it a most human character, which is still further emphasized when, as often occurs, it poises itself in the water with only the upper part of its body protruding.

The natural history of mermaids, therefore, seems in many of its aspects to be safely comprised within the natural history of seacows and seals. But the natural history of mermaids cannot be understood by the methods of natural science alone. These hauntingly beautiful goddesses of the sea, full of mystery and danger, were surely conjured from the chaos of the waters in answer to some primal human need. Their reality in terms of poetic truth is firmly established in the impassioned imagination of men.

For best photo of this ape, see:
The Scientific Monthly 29:276 (1929) (best) (2 photos)
or Cortiss' Biological Anomalies: Mammals II p. 257 (ok quality)
OR Natural History 60:289 (1951) (poor qual. and "air-brushed")

- **The Controversial South American Ape**

THE DISCOVERY OF A NEW ANTHROPOID APE IN SOUTH AMERICA?

Ashley-Montague, Francis M.; *Scientific Monthly*, 29:275–279, 1929.

The discovery of new monkeys or apes, apart from their importance, is always of the greatest interest. Not alone do such discoveries provide systematists, anthropologists and other scientists concerned with the study of the Primates--the order of mammals to which man belongs--with much pleasurable labor, but that great public which is interested in the past and future evolution of man, and which attentively follows the newest developments in the fields of spiritual and physical humanism, is ever stirred to the most wholesome enthusiasm on such occasions.

On the eleventh of March, 1929, Dr. George Montandon, of the Museum National d'Histoire Naturelle, Paris (a well-known anthropologist, and the author of an ingenious theory, the Ologenic theory of anthropogenesis, which holds that the anthropoids and man originated independently over the whole of the earth), announced to the scientific world the discovery of a new and hitherto unknown anthropoid ape. This announcement in itself was sufficient to engender the liveliest interest among scientists. When, however, it was learned that his ape was discovered in South America, a continent in which anthropoids were hitherto completely unknown and in which it was considered extremely unlikely that they should exist, the find took on the dimensions of an epochmaking discovery.

Dr. Francis de Loys, the discoverer of this Primate, and Dr. George Montandon, who was entrusted with the task of presenting the facts to the scientific world, have been kind enough to send me the material upon which this paper is based, and I wish here to express my cordial thanks to them. The facts are as follows.

Dr. de Loys, a geologist, was exploring in the neighborhood of the "Tarra River, an affluent of the Rio Catatumbo, in the Motilones districts of Venezuela and Colombia, at a bend of a western minor affluent of the Tarra River, when two huge monkeys, one male, the other female, suddenly broke out upon the exploring party, which was then at rest. Owing to the violence of their attitude, the animals had to be received at the point of the rifle. One of the two was instantly shot dead at very close range, the other, which was unfortunately wounded, managed to get away in the thick growth of the jungle and make good its escape.

The dead animal, which was found to be an adult female, was immediately set up on a box and photographed, certain measurements were then taken, the animal was skinned and its bones cleaned. The subsequent hardships encountered by the party on their long and hazardous journey across the forest unfortunately prevented the final preservation of either the skin or the

bones.

When measured, the height of the animal was found to be 157 ems (approximately five feet two inches), and its weight was roughly estimated to be somewhat over eight stone (say 115 pounds). The body, which was entirely covered with a thick coat of coarse, long, grayish-brown hair, was, according to Dr. de Loys, entirely devoid of any trace of a tail. "The jaw, carefully examined, revealed the presence of thirty-two teeth only, without on the back portion of the mandible, any protuberances hinting at the possibility of a greater number of embryonic molar teeth.

All these features, "size, absence of tail, number of teeth and ground habits, together with the strongly humanoid aspect of the face and the ruggedness of the build," lead Dr. de Loys to believe that this creature is a hitherto unknown anthropoid ape.

The two photographs which I am here able to reproduce [not available] by the courtesy of Dr. de Loys should convey a good idea of the creature. The object depending from the puberal region between the legs, and which looks like a male copulatory organ, is, in fact, the enormously enlarged clitoris, which, possibly owing to a local hyperemia, corresponding in the male to a condition known as priapism, has been forced out of the vagina. Whatever its cause may be, it is none the less an extraordinary phenomenon.

Careful inspection of these photographs reveals the following facts:

(1) The human-like rounded head presents (a) a prominent forehead, and (b) there are no markedly overhanging brow-ridges; the nose is wide and presents a broad septum between the outwardly deflected nostrils---characters which are peculiar to the New World monkeys generally, and specifically to the genus Ateles.

It is a curious fact that none of the Old World monkeys and apes possess a forehead as prominent as that found in many New World monkeys. The high forehead, which is so distinctively human a characteristic, is primarily what lends so human an appearance to the head of this creature, whose face is identical in appearance with most species of the genus Ateles. In no Old World monkey and in no ape, however, are the nostrils separated by a wide septum, nor are the nostrils so flaring and deflected in an outward and upward direction--this condition is peculiarly South American, there being only three New World genera in which there is an approximation to the Old World arrangement of a narrow septum and inwardly directed nostrils, namely, Alouatta, Aotus and Brachyteles.

(2) With the aid of a magnifying glass one may perceive that the thumb is a much reduced, nail-less tubercle, the merest excrescence upon the side of the hand. This is a characteristic which is specifically associated with Ateles, for no other South American monkey possesses so reduced a thumb. None of the Old World monkeys and apes possesses such a character; in only the Orang-Utan, in which the thumb is the most reduced but is quite large compared with this creature's, is the thumb occasionally lacking in a nail. It is clear enough from the photograph

that this creature's hands are adapted to an extreme arboreal existence.

(3) The feet are evidently of the quadrupedal grasping type, normally associated with an arboreal life. Doubtless, this creature could support itself on its hind legs, but the structure of its foot renders it quite impossible that its habitual gait is bipedal rather than quadrupedal, or that it spends more time upon the ground than in the trees. This foot is identical in appearance with that of Ateles.

(4) The fact that the body was entirely devoid of any trace of a tail (an appendage which is possessed by all South American monkeys) would certainly convince us that we are here dealing with a new species of monkey, but, unfortunately, we can not quite eliminate the possibility of this particular creature having lost its tail in early infancy, thus accounting for there not being even the "trace of a tail." Monkeys have often been known to bite off the tail of some other monkey, the males not infrequently injuring their young in this way. There are, however, a number of other ways in which it is possible to explain the loss of the caudal element in any monkey, but with them we need not concern ourselves here.

(5) Dr. de Loys is quite convinced that the jaw held only thirty-two teeth--a number associated with the Old World Primates and man only. Dr. de Loys does not, however, give the formula for these teeth. In the New World monkeys the formula is $\frac{M3, PM3, C1, I2}{M3, PM3, C1, I2}$, that is, three molars, three premolars, one canine and two incisors, for each half of the jaw, in all, thirty-six teeth. The Old World monkeys have lost one pre-molar in each half of the jaw, thus they possess thirty-two teeth only. If the creature we are here dealing with has lost a premolar, is it not curious that Dr. de Loys should have looked for it on the back portion of the mandible, where only the true molars grow? The bare statement that the jaw held only thirty-two teeth conveys very little to us, for such a statement may mean either that there were only two premolars, or that there were three premolars and two molars (the third having failed to erupt, or to develop), and how are we to decide which is true? The latter condition is the most unlikely one, so that we must provisionally accept the former as the true one.

(6) As far as the stature is concerned, I am not aware of any South American monkey which reaches a height of more than 90 cms (three feet), although this height may conceivably be exceeded in some cases. Certain it is that the height of five feet two inches and the weight of 115 pounds of this monkey are quite unknown in any South American monkey. Nor would it appear from an examination of the photographs that these features are due to any anomalous or pathological causes, although such a possibility can not be altogether eliminated. Assuming, however, that there does exist a species of monkey of which that figured here is a normal representative in the matter of height and weight, it becomes certain that we are here dealing with at least a new subspecies of monkey.

The only characters, then, which distinguish this creature from all other South American monkeys are (1) the absence of a tail, (2) the presence of only thirty-two teeth, (3) its height and weight. The number of teeth alone is a character sufficient to justify the creation of a new genus to receive this animal; the absence of a tail, and the height and weight present ancillary reasons for such a procedure but these features are quite insufficient to justify the appellation of Anthropoid which has been applied to it by Dr. Montandon. Dr. Montandon creates a new genus (as he calls it, but which is really a new sub-family), Amer-anthropides, comprising the single species (Loysi), whilst reserving the possibility, however, of this being a new species of the genus Ateles.

Now exceptional weight and height can at best be regarded as subspecific characters. The absence of a tail in any New World monkey would endow it with specific, but not with generic rank, whilst the loss of a premolar would serve to separate it from all other New World Primates. Neither taillessness nor the loss of a premolar would, however, distinguish this creature, in these respects, from those monkeys of the eastern hemisphere which lack tails. Since, as far as we know, no Old World monkey attains a height of 175 cms nor a weight of 115 pounds, it seems that Dr. Montandon has seen in this (on the basis of its taillessness and its dentition), an adequate reason for endowing this creature with the rank of an anthropoid. That is to say, he has converted a subspecific into a generic character having a sub-family rank, a procedure which is quite unjustified by the facts, and contrary to any natural system of classification.

The most, I think we can say of this creature is that it is a new genus of monkey, possibly an aberrant type, but at any rate, a close relative to Ateles.

It is to be deeply regretted that Dr. de Loys was unable to make a number of photographic records of the skull and other bones; doubtless he little thought of the contingency of losing the actual bones. If a photograph had been made in this case of the skull and the teeth alone, we would have been saved a considerable amount of trouble in speculating about the possibility of this being a new genus of monkey or not; as it is, we have only the observation of Dr. de Loys to rely upon, which, even if he were the most accomplished observer in the world, is not sufficient to satisfy scientific standards of accuracy.

It has ever been the custom to deride the new discovery of apes---the classic case of Du Chaillu and the gorilla is yet fresh within living memory (1859-65), whilst the story of the acrimonious battles which have raged over the discovery of the remains of extinct races of man is too well known to require any amplification here. Let us here bear these events in mind, and endeavor not to commit a like injustice; let us also, however, be cautious, lest in our anxiety to be just we grant too much, and say, until further real evidence is forthcoming, the only attitude to adopt in this matter is that of suspended judgment.

THE ALLEGED DISCOVERY OF AN ANTHROPOID APE IN SOUTH AMERICA

Keith, Arthur; *Man*, 29:135–136, 1929.

Let me say at once that on the evidence submitted regarding the nature of Ameranthropoides loysi--the alleged anthropoid of South America--it is only possible for those familiar with the anatomy of apes to come to one conclusion, namely: that a mistake has been made and that the animal in question belongs to the genus Ateles; in brief, it is a spider monkey--whether of a known species we cannot say, owing to a lack of evidence.

Four communications have been published concerning its characters and nature: (1) A statement prepared by Dr. George Montandon and communicated to the Academy of Science, Paris, by M. Bouvier on March 11, 1929 (C.R. de la Acad. des Sc. 1929, t.188, p.815. (2) A brief note by Dr. Montandon which was published in Revue Scientifique, 1929, t. 67, p.269. (3) A critique of the discovery by Professor Joleaud of the Sorbonne (ibid., p. 269). (4) An article in the Illustrated London News, June 15, 1929, p. 1040, written by the discoverer--Dr. Francis de Loys, B.Sc., D.Sc., F.G.S., a geologist of Lausanne.

From these various sources we learn that the discovery was made fully ten years ago--between 1917-1920--when Dr. de Loys was travelling in jungle country on the west of Venezuela--almost on the frontier of Colombia. He and his company rested one day on the bank of a stream when the breaking of branches made them peer into the jungle. Two animals, which were mistaken at first for bears, advanced, arming themselves with branches and, I am sorry to relate, behaving shamelessly for they defaecated into their hands as they advanced and threw their excrement at the invaders ("excrementant enfin dans leurs mains et "jetant ces excrement contre les hommes"). Thus attacked, the party flew to their guns, with the result that one animal was shot--said to be a female--and one escaped--said to be a male. That was all that was seen of this new species of anthropoid in a live condition--a momentary encounter.

Dr. de Loys apparently made no notes at the time of the characters of the animal which was shot; his recollection is that the hair of the animal was thick, coarse, long and of a greyish-brown colour. He told Dr. Montandon that he measured the stature and found it to be 4 feet 5 inches. English measurement which is 134.6 ctm., but in his own published account Dr. de Loys gives the stature "from sole of the feet to apex of the skull" as 157.0 ctm.--so that the animal apparently grew 23 ctm.--over 9 inches--after original measurement was made. He also states in his own account that "the jaw, carefully examined, revealed "the presence of 32 teeth only," and although no mention is made of the characters of these teeth, the further statement is volunteered that "on the back part of the mandible there were not any protuberances hinting at the "possibility of a greater number of molar-teeth"--the latter statement making experts suspect that Dr. de Loys's knowledge of teeth is not deep. Further, Dr. de Loys simply

mentions--as if it were a matter which required nothing more than mention--that the tail was completely absent.

A photograph of the animal from behind would have clinched matters, but the only photograph taken was one of the front--the animal being placed in a sitting position on a box of unknown size and with no standard object in or near the body of the animal to give a clue to the dimensions of its parts. The problem of identification is further complicated by the discoverer, who has, after removing skin and skull, lost them; his party met with hardships and had to abandon them. Thus the only original document at the disposal of zoologists who seek identify the kind of ape shot by Dr. de Loys--some ten years ago--is the photograph on which Dr. Montandon's description is based and which is reproduced in the Revue Scientifique and in the Illustrated London News.

Professor Joleaud has made a minute examination of the characters exhibited by the photograph and recognises in it all the features which are associated with spider monkeys, but--instead of drawing the logical conclusion that the "new anthropoid" is a spider monkey--accepts Dr. Montandon's diagnosis because of the alleged size of the animal and because in shape of body and of limb it shows certain resemblances to the gibbon and orang. He forgets that in all these respects the new ape resembles spider monkeys more than it does either the gibbon or orang.

Now all the characters of the nose, mouth, eyes, orbits, skull and scalp--the form of body and proportion of limbs--so far as can be judged from the photograph--are those of a spider monkey; the animal is alleged to have the large clitoris of spider monkeys; Venezuela is the home of one of the larger spider monkeys. The hand and foot are those of a spider monkey. Dr. de Loys asserts that it is a ground-living form; the hands and feet, as shown in the photograph, are shaped as in purely arboreal apes. Clearly Dr. Montandon has himself a suspicion of the truth, for in the final paragraph of his original communication he makes the following observation: "Reservant la possibilite que nous nous trouvions en presence d'une nouvelle espece do genre Ateles, nouvelle espece geante."

Nevertheless Dr. Montandon goes on to name the animal, not Ateles loysi, but Ameranthropoides loysi. He would have shown greater zoological acumen if he had stuck by his reservation. Thus we fear that the latest discovery, which ascribes to South America a higher or anthropoid kind of ape, is doomed to go the way of so many others which have been announced from that continent. Since the beginning of the present century there have been many alleged discoveries of human ancestors, but all of them have proved to be other than what they were originally supposed to be.

THE "APE" THAT WASN'T AN APE

Tate, G. H. H.; *Natural History*, 60:289, 1951.

This fantastic story of an alleged South American anthropoid began in 1929, in a variety of publications, Dr. George Montandon attested to the fact that Dr. Francis de Loys, a French traveler in western Venezuela between 1917 and 1920, had reported the presence of such animals. Montandon reproduced in support of his contention a photograph of a large dead spider monkey seated on a kerosene case with its tail parts concealed. Montandon proceeded to solemnize this "discovery" by giving the creature the technical name of Ameranthropus loysi.

Rebuttal was quickly forthcoming. Sir Arthur Keith in Man, vol. 29, pp. 135-136, easily demolished the "new anthropoid." Keith stated flatly that the animal was an Ateles (Spider Monkey) and objected to the offhand way in which Dr. Loys remarked that the tail was completely absent---"as if it were a matter that required nothing more than mention . . .

"A photograph of the animal from behind would have clinched matters," Sir Arthur Keith continued. "The problem of identification is further complicated by the discoverer, who has, after removing skin and skull, lost them . . . Thus the only original document . . . is the photograph on which Dr. Montandon's description is based on . . ."

Other scientists, including Ashly-Montagu, expressed equal doubts.

In 1930, Dr. Montandon answered his critics with a lengthy summary (Archivio Zoologico Italiano [Torino], vol. 14, pp. 441-459), attempting to bolster up his argument on the basis of the large size of the monkey. He cited no less than 20 papers on Ameranthropus in his bibliography.

No further response was forthcoming.

• Phenomenal Footprints

PHENOMENAL FOOTPRINTS IN SNOW, S. DEVON

Busk, R. H.; *Notes and Queries*, 7:8:508-509, 1889.

Staying lately in S. Devon, I was asked what solution 'N.&Q.' had supplied for a phenomenon which seems to have convulsed England in general, and S.Devon in particular, some five-and-thirty years ago. I remember nothing about it myself, but I am

told that on occasion of a deep fall of snow somewhere in the years 1852-4 an extraordinary track, consisting of a clawed footmark of unclassifiable form, alternating at huge but regular intervals with (seemingly) the point of a crutch-stick, and vaulting over walls, hedges, rivers, even houses, and obstacles of every sort, appeared over a surface of thirty-five miles, all produced in one night; that the track was followed up by hounds and huntsmen, and crowds of country folk, till at last, in a wood (I think it was said over Dawlish), the hounds came back baying and terrified. This was the moment when one would think the real excitement would begin. Nevertheless no one seems to have had the courage to rush in where the dogs feared to tread, and the matter ended in a battle of conjecture on paper. The most general local impression seems to have been that it was the devil put his foot in it, though so widespread a belief in so useless and partial a manifestation of a personal devil seems incredible. Now what did 'N. & Q.' contribute to the inquiry? I have looked in the General Index of the decade named, under all the headings under which I can conjecture that the matter might have been classified--"Fantastic," "Phantom," "Phenomenal," "Mysterious," "Footprints," "Snow," "Devon," "Devil's Walk," "Diable boiteux," "Hooky Walker"--but all in vain. Can any contributor better versed in back numbers assist me?

PHENOMENAL FOOTPRINTS IN SNOW, S. DEVON

Busk, R. H.; *Notes and Queries,* 7:9:70, 1890.

Some one, I am told, repeated my query in the Western Morning News (published at Plymouth, but circulating over the whole of S.Devon) of the 31st ult. A large number of answers were elicited by this, some of which have been forwarded to me, as well as a number of private communications. From all these it appears that the exact date was February, 1855. Mr. St. David Kemeys-Tynte, Balnageith, Torquay, partly from childhood's memory and partly from a book called 'Country Essays,' supplies an account very similar to my first report. Mr. E. Spencer, dating from Tavistock, disposes thus of the badger theory:--

> "For years I had a tracing of the footprints taken by my mother in her garden, Montpellier House, Exmouth. It represented half a dozen hoof-like marks, such as would be made by a small donkey, only they were those of a biped; moreover, after reaching the gate of the garden which was of close wood, they continued in the road outside. Prof. Owen, on being consulted, assuming that they must have been made by a quadruped, replied that it must have been a badger, which places its hind foot exactly where the fore-foot had stood, and so left a trace like a biped. But, unluckily, he had not been told that the same tracks were found on the flat tops of some buildings, and on that of a church tower [another correspon-

dent adds "hayricks"].

Mr. Spencer goes on to suggest ingeniously that the tracks might have been caused by herons driven from their usual haunts by strong frosts, "a slight thaw having obliterated the thin wedges of snow in each footstep, and given it the rounded, hoof-like form." He adds that he was led to this guess by seeing on a subsequent occasion some marks like a heron's track on a snowdrift over the Bransen Tor Brook. But I think it difficult to imagine that the "slight thaw"--if there was one at all, and there is no contemporary evidence of the fact, but rather the contrary, as many speak of the snow remaining firm all the next day--could have so uniformly, over such a large tract of country as thirty or forty miles, transformed the appearance of a claw into that of a hoof.

Mr. Charles Taylor, dating from Tavistock, is one who points this out. He also has taken the trouble to collect from the Illustrated News of the moment various accounts, which exactly agree with that I sent you, supplying the further detail that the hoof impression measured 4 in. by 2-3/4 in., the distance between each tread being rather over 8 in., exactly the same in each parish, and that one wall the track passed over was 14 ft. in height. He goes on to quote that, besides the badger theory, the otter, bustard, and crane were all guessed at. It was also adduced that two kangaroos had escaped about that time from the Sidmouth menagerie. Mr. C.B. Mount, Norham Road, Oxford, also supplies the reference to the Illustrated London News. But all fail in some point or other.

Another correspondent writes:--

> "I addressed communications to the British Museum, the Zoological Society, the keepers in the Regent's Park, and the universal reply was that they were utterly unable to form any conjecture on the subject."

My friend the Rev. J.J. Rowe, Marychurch, writes: The episode of the hounds, & c, I well and distinctly remember."

Christophine Goddard, Willow Bank, Paignton, writes:--

> "No allusion has as yet been made to the mysterious footprints having extended to Dorsetshire. We were at Weymouth at the time, at Gordon Place, on the Greenhill. I remember a creepy feeling on seeing the hoof-prints in the snow, which passed from Greenhill over the high wall of our garden...I have a very distinct recollection; it was like the cloven hoof of a calf, one immediately in front of the other. I remember also the theory of their being caused by a badger...But be it bird or beast...why should these marks have simultaneously appeared over so wide an area, and never been observed before or since?"

G.E. Garvey, 23 Walker Terrace, Plymouth, writes to similar effect, but apparently it was in Lincolnshire that he observed them.

• An African Pot Pourri

AFRICAN MYSTERY BEASTS

Hichens, W.; *Discovery,* 18:369-373, 1937.

Every white hunter who has trekked the African big-game trails has heard tell of strange marauding beasts of a kind that never figures on his game-licence, but which, so the natives say, prowl the dark trackways of the bush around the kraals, or lurk in the forest ways and swamps. In one's hunting camp, when the safari porters squat around the scrub-wood fire at night, they tell queer tales of these fearsome brutes; of the ndalawo, that grim, howling man-eater of the Uganda forests; of the mbilintu, a gigantic hippo-elephant of the Congo swamps; of the dreaded mngwa, that furry, silent-padded, purring lurker in the coconut-groves of the coast; of the lau and the kukwata, monstrous beasts whose hideous calls are heard booming through the grey night-mists of the lakes. And someone is sure to set the whole camp peering fearfully into the shadows with a tale of that grim night in the Masai country, when the kerit, ravenous and awful, raided the sleeping-huts--under the very nose of the white man--and dragged away its shrieking victim. These tales lose nothing in the telling. The flickering flames of the camp fire light up with furtive shadows the dark, mysterious wall of the surrounding bush; a slinking hyaena moans dismally, or the sharp starting yap of an inquisitive jackal punctuates the story; while tipsy little komba, the galago, breaks in with cackling insane cries from his hiding in some camp-side tree. The porters huddle closer to the fire; the white man casts a reassuring glance upon his guns and pours himself a chota peg...on such nights prowls kiret, the devil-beast of darkness!

. . . .

Typical of them and their manner of raiding stock or human quarry is an unknown beast which, for some time past has been wreaking havoc over a large tract of the north Cape Province and Transvaal. No one has seen the animal, but its spoor is known and its savage depredations have caused widespread alarm. The natives call it the khodumodumo, or "gaping-mouthed-bush-monster." In stealthy silence, under cover of the darkest nights, this marauder invades the kraals and farms clambers over the six-foot palisades which pole in the cattle-byres and stock-pens and then, seizing a sheep, goat, or calf, leaps back over the fence, to disappear with its quarry.

Its spoor on the kraal pathways and bushveld tracks only serves to shroud the marauder in deeper mystery. Its footprints are "round, saucer-like spoor, with two-inch toenail marks," a pug which has so far puzzled hunters to identify, since it does not fit the paw of any known wild beast that raids stock. The

khodumodumo's attacks were especially predatory in the Graaffreinet area, where a posse of over a hundred settlers turned out to hunt the beast down, a large reward having been placed on its head. Views as to what it was varied widely; some held that it was a "freak" hyaena; but others objected that hyaenas always drag their quarry; and, certainly, no one has ever heard of a hyaena leaping a six-foot fence with a calf in its jaws. More, the hyaena is a noisy thief, moaning before a kill and shrieking like a demon afterwards; and this beast is silent. The leap-and-grab attack pointed to a lion or a large leopard. Some lions do raid silently, and I have more than once known them jump a six-foot kraal fence and carry off a beast; for their strength is colossal. But they always grunt in a husky undertone during the kill and often roar later; often they try to stampede coralled stock by roaring at them. This beast was silent.

Many of the hunters in the posse, too, were old hands who would at once recognise lion or leopard spoor, quite apart from scent and other clues. But the khodumodumo has not yet been caught and a useful reward awaits the hunter lucky enough to get this uncanny raider. It is not impossible that the khodumodumo may yet prove to be an animal hitherto unknown. The nsui-fisi was a brute of a similar kind.

Its name means "leopard-hyaena," and many hair-raising tales are to be heard of it in Rhodesian kraals. For many years natives have told white hunters of this beast, averring that it was incredibly cunning, swift and ferocious, as one would expect of a hybrid "killer" combining a leopard's ferocity with the hyaena's slinking guile. It always attacked, the kraalsmen said, at night, and smashed its way through the flimsy doors or roofs of stock-pens, making off with goats and sheep, and often turning the pens into a veritable shambles. It was like a leopard, the natives declared, but instead of being spotted, it was barred, white and black, like a zebra, and not unlike a striped hyaena. But no such beast was known to white hunters and so the nsui-fisi was pooh-poohed into the limbo of "it's just native superstition, of course!"

In this case, however, the native was right. No less an authority than Mr. R.I. Pocock was able to lay on the table of the Zoological Society not long ago, a skin of the nsui-fisi, one of a number obtained in Rhodesia. It was shown to be a new species of cheetah (Acinonyx rex), not spotted, but striped like a zebra, as the kraalsmen had been saying for many years! As Mr. Pocock remarked, it was "most extraordinary that so large and distinct a species should remain for so long unknown." The natives were wrong in supposing the nsui-fisi to be a leopard-hyaena cross, but that is certainly what it looks like to anyone other than a skilled zoologist. It would thus be rash to assert that other "mythical beasts" like the nsui-fisi cannot exist, and it is by no means impossible that the mngwa, kerit, and ndalawo may yet prove to be as real. By description all these beasts are well known.

. . . .

The kerit is another monster which, in some form or other, unquestionably exists and remains to be discovered. It is sufficiently notorious under the name, "The Nandi bear." On the Kenya coast the natives call it the dubu; the Lumbwa, up-country, call it the getet, and the mere mention of it evokes cries of horror throughout the East African kraals as far west as Ruanda, where it is known as the ikimizi and, elsewhere, as the kibambangue. It would be stupid to assert that this widespread native belief in the kerit is mere baselsss superstition. The kerit is the author of numerous raids of the most frightful description. I have heard it described as a beast, half-man half-gorilla, breathing fire, with one flaring eye in the centre of its head, and emitting a fearful yowling howl. That is the kerit as terror sees it. But as to the howl I can testify, having heard it and having shared the experience of many other white men in hunting the monster. Though it does not always howl, it always attacks under cover of dark, moonless nights and with the swiftness and ferocity of a veritable devil. It is certainly not a lion or a leopard. The kerit will plunge into the thick of a six-foot thorn zareba (a "wall" of piled spiked and hooked thorn-scrub), whereas lions and leopards are very chary of tackling such a defence, the tangled thorns in which painfully lacerate their tender pads and muzzles. I have known man-eating and cattle-snatching lions leap over zarebas; but I have yet to hear of a lion boring through one as the kerit does, like a mole through earth.

Again, the kerit's spoor is nothing like a lion's or leopard's pad. Opinions vary upon it, but there is a body of evidence that this astounding beast leaves a pug-mark with six pads and six claws showing on each paw. I was assured of that as long ago as 1912, and since then, with other hunters, have seen this unbelievable spoor at more than one kraal where the kerit has raided. Many white hunters have actually seen and shot at what has been thought to be a kerit. One of the best accounts is that of Major Braithwaite and Mr. C. Kenneth Archer, two well-known Keyna colonists, whose experience and word are not lightly to be inputed in such matters. They saw the animal in grass and scrub and took for a lioness; later, a side-view of its head gave the impression of a snout, the head being very large while the beast stood very high forward, 4 ft.3 ins. to 4 ft.6ins. at the shoulder. "The back," they say, "sloped steeply to the hindquarters and the animal moved with a shambling gait which can be be compared with the shuffle of a bear. The coat was thick and dark brown in colour. Finally, the beast broke into a shambling trot and made for a belt of trees near the river, where it was lost." Many other observers have given similar accounts of the kerit.

. . . .

In quite a different class of mystery animals are the water-monsters, the lau and the lukwata. These may be one and the same animal. The lau is an immense water-serpent, which is said by the natives to haunt the swamps of the Nile, around Lake No, and the depths of other lakes and marshes. They describe it as an enormous snake, up to a hundred feet in length,

with the body-girth of a donkey. Here again, terror of the monster has adorned native stories of it and one hears that its eyes flash deadly fire and that it feeds on men and large animals, which it seizes with monstrous bristling tentacles protruding from its muzzle. At night it makes a loud, booming cry and a rumbling noise like the typical after-dinner rumble of a herd of elephants. There are, of course, large water-snakes in Africa, and one shot in Tanganyika a short time back by a Greek settler is said to have measured forty feet in length, though this is questionable. Natives declare that the lau takes heavy toll of men and cattle, and various white men have recorded both seeing and hearing monsters that may be laus. The late Sir Clement Hill has described how, in the gulf near Mount Homa on Victoria Nyanza, a monster rose up from the lake and tried to grab the native who was on look-out on the prow of the steamer; it was the man's cry which attracted Sir Clement's attention.

He particularly noted the monster's long neck and small head, and it was, the natives averred, the lukwata, the lake monster that attacks fishermen. Grant, the explorer, saw a similar beast near Jinja, and only recently Mr. E. G. Wayland, Director of Geological Survey in Uganda, recorded that he was shown a fragment of alleged lukwata bone. He found belief in the animal very strong in the Kavirondo country, where the natives said that the lukwata fought with crocodiles and thus lost pieces of its body, which were highly prized as charms. They, too, asserted that its booming voice can be heard at great distances, and Mr. Wayland states that he has himself heard it and can offer no other explanation than the native one, that it is the voice of the lukwata, whatever the monster may be.

. . . .

Lastly, there are mystery men-beasts, such as the agogwe, little furry men, which are said to lurk in the Ussure and Simbiti forests on the western side of the Wembare Plains. Some years ago I was sent on an official lion-hunt to this area and, while waiting in a forest glade for a man-eater, I saw two small, brown furry creatures come from the dense forest on one side of the glade and disappear into the thickets on the other. They were like little men, about four feet high, walking upright, but clad in russet hair. The native hunter with me gaped in mingled fear and amazement. They were, he said, agogwe, the little furry men whom one does not see once in a lifetime. I made desperate efforts to find them, but without avail in that well-nigh impenetrable forest. They may have been monkeys, but if so, they were no ordinary monkeys, nor baboons, nor colobus, nor Sykes, nor any other kind found in Tanganyika. What were they?

The natives of the local villages told me strange tales of them; how, if one put out a gourd of ntulu-beer and a bowl of food in the grain-gardens, these little folk would take the food and do some hoeing and weeding at night, as thanks. That, I can well believe, is myth; but my little brown men were real enough. They may yet be found. One could tell as yet other mysterious creatures, the irizima of the Congo; the ngagia, the chiruwi, the

kitunusi and the ngojama; some are definitely mythical, but it would be rash to aver that all are so. One must not forget that the okapi was once a "mythical beast" and once no one believed in the platypus or in Tibet's giant panda. Yet all these have been proved to be "real." So with the mystery beasts of the African bushveld and forest-ways, they may be improbable, but they are by no means impossible; and the afternoon may well be near when at the hair-raising hour when the Zoo broadcasts its jungle voices on the wireless, we shall hear in our homes the hideous snarl of the mngwa and the spine-freezing howl of the kiret.

Chapter 3

BIRDS

Chapter Contents

ANOMALOUS DISTRIBUTIONS IN TIME AND SPACE

UNRECOGNIZED SPECIES

MORPHOLOGICAL PHENOMENA

• Reports of Luminous Birds

LUMINOUS BIRDS

Anonymous; *Scientific American,* 103:411, 1910.

Birds with luminous or phosphorescent plumage were mentioned by Pliny and other ancient writers and form the subject of several notes recently published in the Revue francaise d'Ornithologie. Phosphorescence has been observed most frequently in herons and owls. It has been suggested that the luminosity is due to particles of phosphorescent decaying wood adhering to the feathers. In the case of the heron this hypothesis can hardly be sustained, and the white barn owl, which exhibits the phenomenon of phospherescence, frequents buildings rather than holes in trees. Besides, the phosphorescence of the feathers is more intense than that of rotten wood. The luminosity of birds' plumage has also been attributed to photo-bacteria living in decomposing animal matter, but this theory appears to fail in the case of a screech-owl which was observed by Spencer and Purdy, in England, to phosphoresce during a period of several months. Luminosity due to putrefaction would vanish on the drying of the feathers.

The most plausible theory is that of Pycraft, who attributes the phosphorescence to a specific fungous growth on the plumage. The greater luminosity of the breast feathers is probably caused by the mechanical and chemical action of the air current encountered during flight; that is, to concussion and excessive oxygenation. This view is supported by the well-known fact that agitation of air or of water containing phosphorescent organisms usually increases the luminosity.

THE EXISTENCE OF LUMINOUS BIRDS

de Sibour, L.; *Knowledge,* 10:321-322, 1913.

Few students delve deeply in natural history without encountering the topic of luminous birds, and the pros and cons of the subject are developed by the reader with a frequency that tests the credulity of any superficial investigator. That birds having

the quality of luminosity have long existed seems a fact beyond dispute. Especially true is this in England. In 1907 Sir Digby Piggott called the attention of ornithologists to the appearance of luminous birds in Cambridge, and these unusual members of the feathered family had already been noticed by others in the same county, and especially by Mr. J. H. Gurney, of Norwich, who spread the news on the Continent through the French ornithnologist, M. Ternier, in The Ornithological Review of France.

It seems that as early as 1866, in the same county, Mr. J.A. Harvie-Brown had mentioned "moving lights" frequently seen at night. But no special attention had been paid to these reports, as they were believed to have originated in the credulous minds of country folk.

The more frequent apparitions in 1907 at last aroused the attention of naturalists, especially in France, where similar cases had been observed in the Vosges and in the Pyrenees.

According to Sir Digby Piggott, a couple of luminous birds were seen near Twiford, Norfolk, in February, 1907, by a gamekeeper, who, having killed one, identified it as a common barn owl (Strix flammea). In October, 1907, Mr. B.J. Purdy and Mr. Spencer saw another which was seen again on the 19th and 22nd of December. On the first occasion it seemed to have attained the maximum of luminosity, as the branches of the tree upon which it had perched were visible in the pale yellow glow. This light did not frighten the mice; for the bird was seen to drop upon them several times.

The power of the light was that of bicycle lamp seen three or four hundred yards off, and its strength diminished considerably when the bird's flight was in a direction away from the observer. This pointed to the inference that the luminosity was confined to the breast.

According to Mr. C.L. Harman, a luminous bird was seen by him in the marshes of Haddiscoe, on the 25th day of December, 1907. Similar apparitions were recorded during the years 1907 and 1908; but in 1909 they ceased, and none has since been observed.

The glow on the breast of the barn owl is undoubtedly due to phospohrescence, and the moulting of the feathers explains its sudden extinction.

Two theories were given as to the origin of this unusual luminosity.

Mr. Gurney, who had the opportunity of observing several specimens of these birds, thought it probable that the owls had been in contact with phosphorescent wood, and that phosphorescent bacteria had attached themselves to the feathers. This opinion at first was generally accepted, especially in Norfolk, where many birds had been seen, and was apparently confirmed when Lord Lindley announced that on his property there was a beech tree showing a patch of phosphorescence eight inches square. It was therefore surmised that the birds had inhabited holes infested by this bacterium. The other theory was that dampness and uncleanliness of the covering of the breast had favoured a sudden growth of luminous fungi peculiar to feathers.

This explanation appears to have been more plausible; for in the contact theory it would seem that the wings and head, rather than the breast, would be likely to touch the sides of the hole. Yet these parts produced little or no light. Again, it would necessitate the bird being a tree-hole dweller, whereas similar cases have been observed on Canadian blue herons, for which this kind of life is impossible.

The phenomenon is not confined strictly to wild birds. Cases are also found among domestic pigeons. The locating of the light on the breast can be explained by the fact that the feathers are finer and thicker on that part of the body than on any other, except the neck. It is also a part that the bird cannot thoroughly clean, and will therefore retain the greater part of the germs and dust gathered in flight. The peculiar increase of light during flight is probably due to a chemical action of the air producing superoxygenation, as it is well known that the agitation of a medium containing phosphorescent particles intensifies the luminosity of the latter.

The balance of argument is thus in favour of the fungi theory, and the latest observations of Senor Elorza in Spain are a confirmation of it. On several nights he was a couple of luminous birds. Upon inquiry he was informed that they had been noticed for several years, that they lived in cliffs near by, and that they disappeared in the month of May. The description given by him did not answer to that of the Barn Owl, and it is to be supposed that we are in presence of another bird, one of nocturnal habits, offering a similar case of phosphorescence. These specimens did not live in trees, but in cliff-holes. Their disappearance in May is accounted for, as in the other cases, by the spring moulting of the feathers.

It might be of interest to look back to the works of the earlier naturalists and note that several observers were aware of the existence of luminous birds. The first to record their appearance was Pliny. He mentions them in his account of the Hercynian forest ("Historia Mundi," X, 47). Two hundred years later Solin, in the twentieth chapter of his "Polyhistoria," alludes in much the same way to what the great Latin naturalist had observed: "Soltus Hercynius aves gignit, quarum pinnae per obscurum emicant et interlucent, quamvis densa nox denset tenebras." It is probable that he was not unacquainted with Pliny's works.

The first work solely devoted to luminous animals was written in 1555 by Conrad Gessner: "De raribus et admirandis herbis, quae sive quod noctu loceant, sive quod alias ob causas lunariae nominantur, et obiter de aliis etiam rebus, quae in tenebris lucent." He speaks of plants and grass shining at night, and seems to have an obscure idea of the origin of this phenomenon, which he calls "res naturae luscentes."

Finally, in 1647, Thomas Bartholin published his great work, "de luce animalium." This is a compilation, in three volumes, of observed (and some problematical) cases of luminous animals. The third book is entirely devoted to birds, and in it are mentioned the Phoenix, the birds of Diomedes, the "Incendiaria avis," which set on fire any tree or house on which it perched; the

cock "cum luce consensum alit," whose feathers had robbed from the sun their brilliant metallic shine.

But among these quaint beliefs one finds observations very probably true. In 1641, at Montpellier, in France, during a short period of famine, many fowls were brought to market. Several of these birds attracted wide attention by their unmistakable phosphorescence, and Henri de Bourbon, Prince de Conde, was called to admire them. A cock was killed "who shone on all parts of his body with a remarkably strong light," "veram totius corporis lucem . . . aperte exserint." The same year, at Montebello, according to the author, there was a hen which "shone like a ball of white fire." And Thomas Bartholin, comparing these two birds, ingeniously adds: "It is a pity that the cock did not meet the hen; for we might then have obtained a breed of incandescent fowls."

BIRDS THAT SHINE BY NIGHT REPORTED BY OBSERVERS

Anonymous; *Science News Letter,* 52:296, 1947.

Birds that shine in the dark with a phosphorescent light like that given off by some fish and other sea creatures are the near-incredible rarity reported by Dr. W.L. McAtee of the U.S. Fish and Wildlife Service. He has gathered the statements of a considerable number of reliable observers, who declare that they have seen the phenomena in such diverse birds as barn-owls, night-heron and Australian finches.

Dr. McAtee has not yet been able to discover what causes the light, but he suspects it comes from luminous bacteria or fungi eaten by the birds or attached to their tissues.

• Four-Footed Birds

A "FOUR-FOOTED BIRD"—MORE LINKS IN EVOLUTION

Anonymous; *English Mechanic,* 40:211, 1884.

Within the last three months two announcements have been made which when duly verified will help materially to complete the chain of Evolution, and to settle the subsidiary question whether the Mammalia have arisen by a modification of the Batrachia or the Reptilia. The sensational announcement at the recent meeting of the British Association was the telegram from Mr.

Caldwell, in Australia, in which he stated that he had found monotremes to be oviparous with mesoblastic ovum, and now we have another in a paper read before the Chicago Academy of Sciences, in which Mr. Edward M. Brigham describes his discovery, a four-footed bird which inhabits the island of Marajo, on the small river Anabiju, lower Amazons. Mr. Brigham has, it appears, already made two journeys to the Amazon, and during one of them he discovered the "quadruped-bird, as he calls it, in 1881; his excuse for the delay in making it known being that he desired to investigate further before reporting. The Chicago Academy of Sciences will publish his paper as a bulletin of the society, with drawings illustrating the peculiar characteristics of the bird, the Opisthocoma cristata, which we may say at once if four-footed only during early life. The announcement that the Ornithorhynichus, a warm-blooded, milk-giving, furred quadruped, lays eggs was of such importance that Mr. Brigham determined to wait no longer before laying before a scientific society such knowledge as he has of the four-footed bird. The egg-laying mammal occupies a place at the foot of its class, and the quadruped-bird fills a place similarly low in its class, for both are instances at variance with what is called the natural order of things, and are in some senses missing links in the chain of evolution. While pursuing his embryological studies in the island of Maranjo, Mr. Brigham examined several specimens of the four-footed bird at various ages, and found that from what corresponds to about the embryonic state of development of the common fowl at the 10th day of incubation the fore feet showed their characters unmistakably throughout their egg development, and to a period of several days after hatching the fore feet, toes, claws, &c., were as clearly characterised as similar parts in the posterior members. Later on a progressive modification manifested itself by reducing the digits, exfoliating the claws, and developing these anterior members into those characteristics of a bird. Mr. Brigham says that among the higher vertebrate animals, so far as he knows, there is no other example of post-natal metamorphosis in such fundamental organs to anything like the extent witnessed with Opisthocoma cristata. The animal progressing in its embryonic course passes into its reptilian ancestral type, and before its evolution had carried it through this, its reptilian phase, it emerges from the egg. For several days after hatching it retains its quadrupedal character; then under the influence of open air and sunlight one pair of legs develop into wings. An adult speciment resembles a pheasant, but it is not a relative of the pheasant; in fact, has no near relatives living, being the sole survivor of its genus, its family, its order---an order of which geologists have found numerous representatives in a fossil state. The Cigana (gipsy), as the natives call the bird, has a limited geographical range, being found only about the estuary of the Amazon and its lower tributaries. It is gregarious, being rarely found in less number than half a dozen together, while its habitat is peculiar, and is limited, for the most part, to a single species of plant, called the aninga by the natives---a tall, semi-aquatic aroid, with large leaves, calla-like flowers, and pineapple-like

fruit, which grows in dense masses on the low, flat, muddy margins of the islands. Mr. Grigham says he has never seen the Cigana alight on the ground; and though the aninga leaf is a frail and yielding perch, the bird seems to prefer it to all others. To steady itself on its swinging perch, the bird rests on its breastbone, and the habit causes a thickening or piling up of epidermal tissue in a hard callosity, which becomes so firmly anchylosed to the bone that in skinning it must be separated with a cartilage knife. This seems to imply, says Mr. Brigham, a long association of the Cigana and the aninga, and the bird is so much of its time on the swinging aninga that even when seen on other perches it is rarely erect. It is only on occasions when one is tormented by its fellows that the Cigana ventures into the forest, and then not so far but that it can quickly return with a few strokes of its wings. The nest is built of twigs and other coarse vegetable matter in the aninga tops, or the low overhanging branches of trees or matted vines, and the bird rarely takes a longer flight than across a narrow stream. It is quarrelsome, and when its pugnacity is aroused it loses the pheasant-like appearance and resembles no other bird. The noise of the flapping wings is accompanied by hissing, grunting, and shrieking, and when there is a general disturbance the jargon is indescribable and demoniacal. On the other hand, however, the Ciganas utter a most doleful sound, and seem says Mr. Brigham, as if they were suddenly moved to mourning for all the extinct Opisthocomidae. The doctrine of purposelessness, or dysteleology, is one of the most interesting subjects in connection with evolution---in fact, evolution perhaps received its greatest popular stimulus from the inability of other theories to explain the origin of organs to all intents and purposes useless. In the case of the Cigana it has a remarkable exemplification, for in the course of embryonic development a foot appears in a state morphologically finished, indicating its purpose by the well-developed toes, each terminating by a claw; but the developmental activities are modified, and instead of a foot a wing is the final post-natal result; hence it is held by bologists that these fore feet (a bird has no use for fore feet) are the link which unmistakably connects the Cigana with the Reptilia.

. . . . .

• A Fowl Sport

A FOWL MONSTROSITY

Michels, John; *Scientific American,* 38:89, 1878.

An interesting instance of a strange malformation in the head of

a fowl has been exhibited alive at the New York Aquarium, and Professor Fr. Stengel of Columbia College vouches for its authenticity, it may be presumed to be a genuine specimen.

The illustration will convey an excellent conception of the peculiarities of the fowl in question, which is said to have a monkey's face. It will be noticed that the ordinary beak of a bird is absent, and that the nose and lips of an animal are fully developed.

Fowl with mammalian head features

The nose appears to be formed by an extension of the comb, which at the point of junction suddenly changes from a bright red to a pale fleshlike color; the lips, which are large and protruding, have the same hue.

Both lips and nose are formed of a moderately hard cartilagenous substance, having a smooth surface, the nostrils being very similar to those observed in many species of monkeys.

The tongue is also modified in form, rounded at the point, and having unusual power of lateral motion.

With the exceptions I have named, or shown in the illustration, the general appearance of the specimen is normal, and indicative of its being of the Cochin China breed.

We have doubtless here an interesting specimen of one of

those strongly marked and abrupt deviations of structure which occasionally occur without any apparent cause.

Such cases are rare with birds in a state of nature, but happen with greater frequency with those which have become domesticated.

This monstrosity probably arose from an arrest of development rather than arrest of growth, and is doubtless capable of being transmitted. Breeders take advantage of such freaks of nature to produce what is called a variety.

Speaking generally, it is conceded that changed conditions and external influences produce variation from type, and considable effect upon organisms of all kinds.

There are, however, instances in which decided variation arises without any apparent exciting cause, and Darwin with his usual caution "provisionally" calls it "spontaneous;" he attributes such variations, whether consisting of slight individual differences or of more strongly-marked deviations of structure, as depending much more on the constitution of the organism than on the nature of the condition to which it has been subjected.

• Parthenogenesis in Birds

VIRGIN BIRTH IN VERTEBRATES

Burton, John; *New Scientist,* 59:334-335, 1973.

Birds and mammals are so far not known to have produced parthenogenetic populations in the wild. But remarkably enough, experimenters working with turkeys in the laboratory have had considerable success in producing parthenogenetic strains. Using over 42000 eggs American researchers were able to increase the number of eggs which started to develop from 16.7 per cent in 1952 to 41.7 per cent in 1959. This improvement was a direct result of selective breeding--female birds which produced a high proportion of eggs with parthenogenetic tendencies were mated with males descended from other such birds. Towards the end of the experiment 67 embryos (all males) were reared to hatching, a few survived to maturity, three produced spermatoazoa, and one actually produced offspring. More recently, Patricia Sarvella of the US Department of Agriculture (Nature, vol 243, p.171) reported the birth of four male diploid parthenogenetic chickens, hatched from 8532 eggs. All four reached maturity, and one mated with a normal female to produce fertile eggs. But interesting as these experiments may be, they do not really affect the picture as far as parthenogenesis in the wild is concerned.

• Effects of Electricity and Magnetism

ELECTRIFIED CHICKENS—ELECTRICITY AS A GROWTH STIMULATOR

Anonymous; *Scientific American,* 109:287, 1913.

Some time ago Mr. H. G. Wells wrote a delightful fantasy called "The Food of the Gods," in which he imagines a food which stimulates growth to such an extent that the dimensions of all living things who feed upon it are increased six or seven times. Mr. Wells probably had no suspicion that such a stimulating agent would ever be discovered. But it appears as the result of some very recent researches, that living beings may be greatly increased in size when subjected to proper conditions. It is true that no mysterious food has been discovered which stimulates growth in this way. The stimulating agent is electricity, to whose powers, both beneficent and harmful, there appears to be no end.

High frequency currents have long been used by the medical profession with beneficial results, but until recently the mere fringe of the subject has been touched. Medical men have been contented with a comparatively limited range of application. It has been reserved for Mr. T. Thorne Baker to show what great possibilities are inherent in the application of high frequency currents to living things. For some time Mr. Thorne Baker has investigated the effect of electricity upon the growth of bacteria and mosses, but his latest work, apart from its scientific interest, promises to have results of great commercial importance. The latest experiments are concerned with the influence of high frequency currents upon the growth of chickens, and they are being conducted upon a truly colossal scale. Meeches Farm, Poole, England, is probably the greatest chicken farm in the world, and is the scene of these new experiments. On this farm about four thousand chickens are being grown under the influences of the electric waves. The results are truly astonishing. The chickens live in flats and over the whole building is wound an insulated wire which is traversed by the high frequency currents. The apparatus which generates the high frequency currents presents distinct peculiarities from the electrical point of view, and is the outcome of numerous experiments. Careful adjustments have to be made of the various electrical quantities entering into the circuit, before any effect is produced, but with a proper adjustment of the apparatus, most marked effects occur.

Chickens living in the electrified flats reach, in five weeks, the normal weight of chickens three months old. And out of four hundred chickens treated in this way only six and those obviously doomed from birth, died. In view of the fact that a fifty per cent deathrate is usual at this period of the year, it will be seen that this result is sufficiently startling. Chickens so weak that they could not stand up, and who in the ordinary course would infallibly have died, have been put in the electrified flats and become healthy and strong.

It is not only that the output of a chicken farm is doubled by this process, but a considerable saving in food is effected. Only two thirds of the usual quantity of food is required by the electrified chickens.

The chickens are charged to such a high potential that a spark discharge occurs on presenting a finger to the beak. From the scientific point of view the most interesting part of this work is the theory which explains how high frequency currents stimulate growth in this way. Mr. Thorne Baker is of the opinion that the high frequency currents stimulate the blood circulation by lowering the viscosity of the blood. He has conducted experiments on the effect of high frequency currents on the flow of viscous fluids, and he finds that the time of flow is decreased. The viscous fluid loses some of its viscosity and becomes more mobile.

It is not at present known whether prolonged electric action increases growth up to maturity, or whether its whole effect is to cause the maximum size to be sooner reached.

In certain quarters the application of high frequency currents to the growth of children is being contemplated, and the results in this field will be awaited with interest.

It is evident that we have here a method pregnant with possibilities, and its further developments will be rich in interest both in scientific and commercial aspects.

EFFECTS OF MAGNETISM ON HEN'S EGGS

Anonymous; *Science*, 5:182, 1885.

The effect of magnets upon artificially incubated hen's eggs formed the subject of some very interesting experiments, of which an account was given by professor Carlo Naggiorani in a recent paper before the Academy dei lincei. During the hatching-process he kept one set of eggs under the influence of powerful magnets, while another set was incubated away from all such influence. Cases of arrested development were very numerous among the first set, and after birth the rate of death among these was four times as great as in the naturally incubated chickens. Only six chickens out of a hundred and fourteen eggs arrived at maturity. Of these, two were cocks of a splendid stature, and endowed with an insatiable reproductive appetite. With the four pullets the case was quite the reverse. One of these never laid at all, and the three others generally produced very minute eggs without yolks, without germinal spot, and, in a word, sterile.

• Problems in the Development of Flight

THE ALLEGED EVOLUTION OF BIRDS

Cousins, Frank W.; in *A Symposium on Creation, III,* Donald W. Patten, ed., Baker Book House, Grand Rapids, 1971, pp. 89–93.

Archaeopteryx ought to be a true connecting link between the birds and reptiles. As such it would be a striking proof of evolution. It appeared to be so good an intermediate form that people even argued over whether it should be attributed to the reptiles or to the birds.

Is it really the case that Archaeopteryx exhibits features intermediate between the birds and reptiles that do not occur in present-day birds? Is it not possible to attribute the form to one of these classes?

That Archaeopteryx is very bird-like is indisputable from one primary bird characteristic, namely, "the presence of a regularly developed and well-constructed coat of feathers.

It might appear as though the nature of the body covering is of small importance where so important an issue as evolution is concerned. But the form of the coat of feathers is not conceivable unless one assumes a quite special constitution of the whole organism. The perfect display of all parts of the plumage in Archaeopteryx presupposes that this plumage was planned, nourished, and renewed just as in the recent birds. It follows from this with the highest probability that the animal was warm-blooded, and hence again that its heart must have been four-chambered and the circulation completely developed. Only after these conditions are assured does the coat of feathers take on its importance.

Since the work of Linnaeus, feathers, the specialized body covering of the bird, have been regarded as the essential characteristic of birds. This opinion is also strongly held by present-day ornithologists such as Lambrecht. Lambrecht also refers to several other properties which speak for the bird-nature of Archaeopteryx:

(1) the extensive growing together of the skull-bones into a cranium
(2) the long, slim form of the shoulder-blade
(3) the growing together of the collar bones to one forked bone, the merrythought
(4) the backward-tilted pubic bone
(5) and the bird-like form of the rear extremities

All these are indeed important characteristics with respect to the whole constitution of the bird.

Against these very pronounced bird characteristics, Lambrecht sets several characteristics that he regards as reptilian. Strangely, as one such characteristic, he puts forward the presence of a sclerotic ring in Archaeopteryx. This seems incomprehensible,

since this bone-plate circle of the sclera is even more typical of birds than of reptiles. For among the latter, the ring is only found in lizards and tortoises, not in snakes and crocodiles. Therefore, Archaeopteryx is also quite bird-like with respect to this characteristic.

. . . . .

The presence of neck ribs in Archaeopteryx is considered by Lambrecht as a further reptile-like property. Yet in many cases the neck vertebrae of the recent birds actually carry short ribs. In the adult bird they generally fuse with the vertebrae; but this property, too, is neither a pronounced reptilian characteristic nor one that occurs only in the primitive birds.

Citing an important reptilian characteristic of the skull, Lambrecht regards the fact that the pre-orbital gap is completely separate from the pre-orbital hole. However, he calls attention to the fact that this property is also to be found in recent birds, e.g., in the parrot. Thus, this argument is deprived of effect.

The absence of pneumaticity of the skeletal parts in the Archaeopteryx points to the reptiles. But this property, too, is present in some recent birds. Thus, in dealing with the above-mentioned Aepyornix-like late-fossil birds, Lambrecht states:

> They are highly specialized forms with a massive skeleton and are partly pneumatic [emphasis his]. Thus, in particular, the vertebrae and the femur are pneumatic, the latter only, among the ratites, still containing the air in the case of struthio [the ostrich].

While the primitive bird, Archaeopteryx, is not pneumatic, it is also striking that Jurassic groups of reptiles, such as the Dinasaurs and Pterosaurs, do exhibit pneumaticity of the skeleton. The evolutionary argument with regard to pneumaticity is, therefore, confused.

There still remain a few characteristics, those that most meet the eye, which require discussion. It is emphasized again and again that Archaeopteryx cannot be a "true" bird since it has teeth, a vertebral tail and wings with claws. No bird has that! Now is this correct? Let us investigate this proposition with respect to recent birds.

The transformation of the forward extremities to an organ of flight is one of the most typical characteristics of birds. It follows that the fingers are reduced in number as are the number of finger joints. Also their claws disappear and the middle bones of the hand, as well as the bones of the wrist, fuse together. This is generally the case, but not always. With regard to the number of fingers, Archaeopteryx has three, all of which bear or have claws.

Now what is the position in this respect in the present-day birds, when we consider not only their average appearance, but their variability? It is peculiar that one must go back to the year 1889 to obtain information on this question. In that year, there appeared a paper by W. Kitchen Parker about the development of

birds and the wing structure of many birds, in particular, the Gallinaceous birds (poultry) and ratites (ostriches, Kiwis, emus, etc.).

Parker finds that in the embryo, the occurrence of a claw on the first finger (thumb) is usual. On the second finger it occurs in the majority of gallinaceous birds and in many birds of prey, waders and swimming birds. But not only the embryos and newly-hatched birds exhibit claw structures. The thumb claw and, in some cases, a claw on the second finger, can be seen in the adult gallinaceous birds.

However, the ratites are of quite special interest. For in the ratites we find genera in which not only the first and second fingers bear claws, but also the third. This is so in the case of Rhea (Mandu) and Struthio (Strauss). Rhea in the adult bird also has a very strong claw on the thumb and rudimentary claws on the second and third fingers. Struthio has well-developed claws on the first two fingers and a rudimentary one on the third. Thus Struthio, as with Archaeopteryx, has three developed fingers with a claw on each.

In addition, the central bones of the hand in Struthio are completely free just as in Archaeopteryx. The Struthio the joints of the finger are also unusually numerous. The thumb has two phalanges, the second finger three, and the third, two, i.e., just as many as in Archaeopteryx. All this is very beautifully shown in one of Parker's figures. Therefore, it is ironic that Struthio has not aroused the same amazement as Archaeopteryx, since with regard to the skeleton of the hand, the ostrich is just as primitive and reptile-like as Archaeopteryx. Indeed, why does not the ostrich cause even greater astonishment, since it is still living?

If one carefully reviews the recent birds, Archaeopteryx scarcely can be regarded as unique with respect to the primitive nature of the hand. In the fauna of present-day birds we find forms that are as primitive as the "ancestral bird" itself. Conversely, we have no corresponding transitions to the reptilian hand. The hand of Archaeopteryx is a true hand. Only the claws seem remarkable.

With regard to the structure of the bird hand, we find some recent birds that completely surpass the "ancestral bird" in primitive traits. In a later paper Parker describes one such bird under the title, "On the Morphology of a Reptilian Bird, Opisthocomus Cristatus." In the embryo form, this bird, the Hoatzin, or Cigana, from tropical South America, has strong claws on both the first and second finger, just as strong as those of the toes. (pp. 89-93)

ARCHAEOPTERYX AND THE ORIGIN OF FLIGHT

Ostrom, John H.; *Quarterly Review of Biology*, 49:27-47, 1974.

Abstract. Reexamination of the specimens of Archaeopteryx, which constitute the only direct evidence pertaining to the habits

and mode of life of the earliest stages of avian evolution, indicates that neither the highly favored arboreal theory nor the much criticized cursorial theory offers adequate explanation for the origin of avian flight. The osteology of Archaeopteryx, in virtually every detail, is indistinguishable from that of contemporaneous and succeeding coelurasurian dinosaurs---especially in the details of the manus, forelimbs, and pectoral arch. It is proposed that these conditions reflect a highly predaceous mode of life for Archaeopteryx, rather than being arboreal adaptations. Plumage, in the form of contour feathers, is believed to have arisen in response to the need for controlling heat loss (and gain) and was secondarily modified on the fore limbs to enhance the prey-catching function of the hands. Enlargement of the primordial "primaries" and "secondaries" transformed the forelimbs of "proto-Archaeopteryx" into large, continuous, trapping surfaces--natural insect nets--activated by powerful ventral adductor muscles (the pectoralis group). These adaptations were admirably preadaptive for active, flapping flight.

The primordial insulative function of contour feathers and the predatory hypothesis for the enlargement of the remiges seem to account for the otherwise paradoxical presence in Archaeopteryx of essentially modern "flight" feathers in the absence of virtually all of the skeletal specializations that are associated with (or required for ?) modern bird flight, whereas those skeletal specializations that are present in Archaeopteryx are the same, or nearly the same, as those that are preserved in various (presumed) predaceous coelurosaurian dinosaurs.

THE EVOLUTIONARY ORIGIN OF FEATHERS

Regal, Philip J.; *Quarterly Review of Biology,* 50:35-60, 1975.

Abstract. Previous theories relating the origin of feathers to flight or to heat conservation are considered to be inadequate. There is need for a model of feather evolution that gives attention to the function and adaptive advantage of intermediate structures. The present model attempts to reveal and to deal with, the spectrum of complex questions that must be considered.

In several genera of modern lizards, scales are elongated in warm climates. It is argued that these scales act as small shields to solar radiation. Experiments are reported that tend to confirm this. Using lizards as a conceptual model, it is argued that feathers likewise arose as adaptations to intense solar radiation. Elongated scales are assumed to have subdivided into finely branched structures that produced a heat-shield, flexible as well as long and broad. Associated muscles had the function of allowing the organism fine control over rates of heat gain and loss: the specialized scales or early feathers could be moved to allow basking in cool weather or protection in hot weather. Subdivision of the scales also allowed a close fit between the elements of the

insulative integument.

There would have been mechanical and thermal advantages to having branches that interlocked into a pennaceous structure early in evolution, so the first feathers may have been pennaceous.

A versatile insulation of movable, branched scales would have been a preadaptation for endothermy. As birds took to the air they faced cooling problems despite their insulative covering because of high convective heat loss. Short glides may have initially been advantageous in cooling an animal under heat stress, but at some point the problem may have shifted from one of heat exclusion to one of heat retention. Endothermy probably evolved in conjunction with flight. If so, it is an unnecessary assumption to postulate that the climate cooled and made endothermy advantageous.

The development of feathers is complex and a model is proposed that gives attention to the fundamental problems of deriving a branched structure with a cylindrical base from an elongated scale.

THE SIGNIFICANCE OF FLIGHTLESS BIRDS

Edelson, Edward; *Mosaic,* 11:10–15, May/June 1980.

Ever since they were discovered, the flightless birds--the ratites--have fascinated ornithologists and other scientists. Visually, these birds are spectacular: the ostrich of Africa, at eight feet tall and two-toed, the world's largest living bird (but nonetheless smaller than two extinct groups of ratites, the moas and the elephant birds); the two three-toed, ostrich-like South American rheas; the emu of the Austrialian plains; the aggressive, forest-dwelling cassowary of northern Australia and New Guinea with its two-inch, razor sharp claw; the three species of kiwi (or apteryx) of New Zealand--nocturnal birds whose five-inch egg weighs up to a quarter of the bird's body weight.

But another reason for fascination is the series of scientific questions presented by the existence of the ratites. For well over a century, the debates have gone on: Are the ratites living fossils, descendants of ancient species that never evolved flight? Or did they lose their wings because there was no selections in their environment against individuals with reduced powers of flight? Did these birds, scattered across more than half the planet, have a common ancestor and somehow disperse despite their lack of useful wings? Or did they develop from different stocks, growing to look alike through the evolutionary phenomenon called convergence, in which unlike species evolve common characteristics in response to similar spectra of environmental conditions?

Are the ratites related to the tinamous, the ground-dwelling Latin American birds that share many ratite characteristics but still can fly? And finally, if the ratites did have a common ancestor, what are their interrelationships?

Today, the common ancestry of the ratites has been estab-

lished with a high degree of certainty. But, with a twist that is worthy of a Hitchcock movie, the last question has become the focus of the kind of keen controversy that often develops at the cutting edge of a science. ("It's almost the flailing edge of this science at the moment," comments one observer.)

The attempt to construct a phylogenetic tree that would establish the relationships among the ratites has become a major testing ground for an emerging science, with lively arguments about both which data to use for such an effort and which mathematical techniques are best for analysis of the body of data and its representation in a phylogenetic tree or branching diagram.

The debate about the phylogenetic branching order of the ratites thus has greater significance than might ordinarily be attached to a discussion of whether the ostrich is related more closely to the kiwi or to the cassowary. It is a gripping case history of the demanding effort being made to change the process of species classification from a subjective, ill-defined discipline to a modern, quantitative science. "This is one of the first good cases where different kinds of data can be applied to the same group," says Walter M. Fitch of the University of Wisconsin about the ratites. "That's a plus because it's an extremely interesting group."

Those interested in permutations and combinations should know that there are at least three different techniques being used to gather data about the relationships among ratites: two that examine molecules from the birds and the third, a modern version of the classifical method of studying morphological differences. Additionally, several different methods can be used on the different sets of data to construct phylogenetic trees. As a result, despite a plethora of often conflicting conclusions, each proposed phylogenetic tree (and its supporting data) is thus assured the kind of close scrutiny that, ultimately, helps hone a research strategy to perfection.

Clearing the underbrush. To get to where they are, the scientists studying phylogeny via the ratites first had to clear away the underbrush by answering some basic questions. It is now generally agreed that the ratites are not primitive birds left over from an earlier evolutionary stage. Instead, they appear to be highly evolved descendants of an ancestor that had the ability to fly.

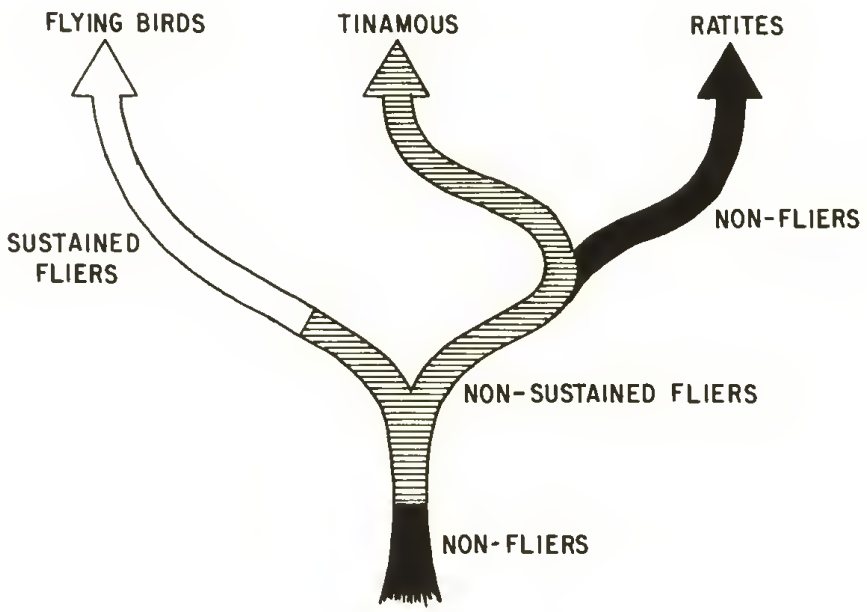
Although the ratites share many anatomical characteristics, there are some notable differences. The rhea and the ostrich have feathers that have one central shaft, for instance, while the feathers of the emu, kiwi and cassowary have two: a major one, called a rachis and a secondary "aftershaft." And, while most ratites are large birds, the kiwi is small. Further, and most obviously, the ratites are not geographic neighbors.

To those who once held to the belief that the ratites had to be an example of convergent evolution--the development of like characteristics by divergent species subjected to similar evolutionary opportunities--the bio-geographic question was the clincher: How could nonflying birds spread across the vast

oceanic distances between continents, virtually in all directions?

Anatomical study of the ratites, however, finally convinced scientists of the flightless birds' common ancestry. (That the ratites must have had a common ancestry, bio-geography notwithstanding, had been concluded as long ago as 1867, by Thomas Henry Huxley, from comparison of physical features. And parasitologists, who consider the fact that ratites share parasites that do not plague other birds as clear evidence of common ancestry, have argued it for many years; parasites and their hosts are an example of coevolution [see "'Feedback' Produces a Theory of Ecology," Mosaic, Volume 10, Number 6].) But it took geology, and the evolution of the theory of plate tectonics (see "What Drives the Earth's Plates," Mosaic, Volume 10, Number 5) to solve the biogeographic riddle: The birds had merely to stand still while the protocontinent called Gondwanaland broke into raftlike fragments that drifted apart over tens of millions of years, carrying all kinds of life forms that shared common ancestries along with them.

With that question answered, the question of phylogeny--how



A hypothetical model for the development of flight in birds assuming that the loss of flight in ratites occurred only once in a common ancestor.

the ratites relate to their common ancestor and to each other--came into sharper focus. And getting a correct answer to this question is surprisingly important, since the methods used to develop an accurate phylogenetic tree for the ratites can also be used to place the ratites in their proper relationship to other

birds, to develop an overall phylogenetic tree for all the orders of birds, which has not yet been done to everyone's satisfaction, and, indeed, to perfect techniques for the development of quantitative phylogenies in general.

The approaches. There are three major efforts to gather data on the ratites, all three being carried out in the wider context of a study of all birds. At Yale University, Charles G. Sibley and Jon E. Ahlquist are studying the quantitative differences between the genetic material of the various ratites. Allan C. Wilson of the University of California at Berkeley has made a similar biomolecular study of one blood protein, transferrin, from the different ratite species. And at the University of Illinois, ornithologist Joel Cracraft, taking a comparative approach to the study of morphological characteristics shared by different avian species, is employing a method designed to bring more testable procedures into a process long ruled by subjective judgments.

The two molecular techniques are based on the same principle: that evolution goes on not only among physical features but also in molecules (see "Molecular Evolution; A Quantifiable Contribution," Mosaic, Volume 10, Number 2). The DNA molecule in which genetic information is coded is hypothesized to experience mutations at a measurable rate. The proteins produced from the DNA instructions also show the change. By comparing either DNA or proteins from two species, an investigator can determine the extent of the difference between them and thus how far, if not when, the two species have evolved from their common ancestor. By comparing DNA and proteins from several species, data for a phylogenetic tree can be gathered.

Sibley's method is to look directly at the DNA. If purified DNA is dissolved and heated, the two strands of the DNA molecule can be separated. If the solution is then allowed to cool, the strands will reunite as complementary bases on each strand, "recognize" each other and join together. Sibley puts single strands of DNA from different species into the same solution and measures the extent to which they join. The closer the match, the closer the genetic relationship between the two species.

It is not nearly as simple as it sounds. Early work gave confusing results which were not cleared up until the discovery that the genetic material of higher species includes long stretches of repetitive DNA sequences whose function is still unclear. Sibley and Ahlquist now fractionate the DNA to remove the repetitive sequences, leaving behind the single-copy sequences of DNA that give meaningful results. The single-copy DNA is labeled with radioactive iodine and added to a solution of nonradioactive DNA from another species. (Most of the analysis is now done automatically, by an ingenious machine designed by the Yale investigators.)

The mixture first is incubated to allow the DNA from the two different species to form hybrid duplex molecules, composed of one strand from each of the two species. Then the mixture is heated again in a series of carefully controlled steps. The temperature is raised a degree or two, and the DNA which has dis-

sociated is removed for measurement; the amount of radioactivity in the sample indicates the degree of dissociation. By comparing the difference between the temperature required to dissociate the duplex DNA of a single species used as a control and the temperature that dissociates a hybrid duplex, Sibley and Ahlquist get a numerical reading on the similarity of the DNA strands of the two species.

A great virtue of the technique, Sibley says, is that it looks at the entire genome of a species, all the genes that count. Individual genes are known to evolve at vastly different rates; there is a 400-fold difference between the slowest-changing and fastest-changing genes, Sibley explains. By studying the whole genome, his method gives an average, overall, quantified rate of change, from which he derives estimates of genetic distance between species.

Sibley describes the relationship as rather straightforward: a difference of one degree Celsius in melting point equaling approximately a difference of one percent in DNA pairing. The results are expressed in a mathematical matrix of what Sibley calls "delta modes," each delta mode expressing the genetic difference between two species. Though his colleagues are far from unanimous in agreement, Sibley maintains that the analysis of such a matrix to produce a phylogenetic tree is relatively straightforward. "The readout we get out of the computer requires very little interpretation," he declares. "The data speak for themselves."

Protein comparisons. A major biochemical role for DNA is the synthesis of proteins, and nucleotide replacement in DNA should be reflected as amino acid substitution in a protein. So the comparison of proteins from species to species, as Allan Wilson, working with Ellen Prager at the University of California at Berkeley, is doing, should also reveal phylogenetic links and distances.

Wilson's method is to purify the chosen protein--transferrin in the case of the ratites--inject it into a rabbit and harvest the antibodies produced by the rabbit. Those antibodies then are added to a mixture containing transferrin from another species and a substance called complement, which helps the antibody to join with the protein. By measuring the amount of free complement left after the antibody-protein reaction has occurred, Wilson and Prager get a measure of the differences between the two proteins.

While the method measures mutations in only a single protein, Wilson points out, "this protein is a large one, containing 700 amino acids. The number of substitutions by which the birds under consideration differ from each other is... about 100. So we really are looking at a large number of traits at once."

The method is not quite precise, Wilson acknowledges; it detects no more than about 85 percent of the changes between proteins of two closely related species. The advantage of the method, he says, is that the validity of the tree obtained with one protein can be tested by investigating additional proteins.

The technique proved itself, says Wilson, with work on a Mexican bird, the chachalaca, which had been classified as one of the galliforms, and order including such game birds as the quail and the pheasant. The study of one protein, lysozyme, indicated that the chachalaca was distant from the galliforms, a result so startling that Wilson decided to check it further. "We looked at five different proteins. All show that the chachalaca forms a group as distinctive from gallinaceous birds as ducks are." Wilson reports.

Nevertheless, Sibley disagrees. He says that his analysis of DNA shows that the chachalaca is indeed a galliform--although he agrees with Wilson that it is distant from the other gallinaceous birds.

Wilson says, nonetheless, that his conclusions about the ratites are not very different from those of Sibley. Both have the tinamous as the closest relatives of the ratites, and both have the ostrich rather closely related to the kiwi and the rhea. The differences are reconcilable, Wilson says, because imprecision that still exists in any available molecular method leaves room for changes in the resulting phylogenetic tree; "the two molecular methods agree on the whole."

A new morphology. The Wilson and Sibley constructions, however, in addition to differing in some ways from each other, differ in apparently small but nonetheless significant ways from the phylogenetic tree drawn by Cracraft. The Illinois ornithologist is using a technique of comparing morphological differences--variations in the shapes of bones and other physical traits--that is little more than a decade old in North America. The historic method of assigning relationships between species by looking at physical traits was once hopelessly subjective, Cracraft notes. But help for the anatomists, he says, came with the development some 25 years ago of a more sophisticated technique, called the cladistic method, often credited to a German entomologist named Willi Hennig.

The essence of the cladistic method, Cracraft says, is that the anatomist looks for derived characteristics the group under study might share rather than for primitive characteristics--for features that are evolutionary novelties rather than those that many species have in common. To give a most basic example, a primitive feature of birds is the presence of feathers; a derived characteristic would be their absence or a modification of the feather shared with one group of birds but not another.

There are two ways to do such an analysis, Cracraft explains. One is "outgroup comparison," a study of how a group that is under study compares with a presumably closely related group. For horses, outgroup comparison could focus on the foot structure of the order of ungulates that includes the horse (perissodactyls) or of mammals in general. The modern horse has just one toe, and outgroup comparison indicates that it descended from primitive ancestors having five toes. A phylogenetic tree might be constructed by studying the character transformations presented by many such morphological features.

A second method is to study ontogenetic transformation, the development of an individual from fetus to adult. In the flounder, for example, the eyes are on either side of the head at first, but migrate until both are on the same side of the head. This ontogenetic transformation is evidence that the primitive ancestor of the flounder had symmetrically placed eyes and that asymmetry is a derived characteristic.

The important point is that the cladistic method can make comparative morphology more rigorous and testable. The further importance of the ratites for Cracraft is that his study of them was the first application in birds of Hennig's methods.

Cracraft has found evolutionary trends in a number of morphological characteristics of the ratites. For example, studying the tibiotarsus, one of the bones in the lower leg, he finds that the base of the cnemial crest, a feature at the upper end, is broad in the tinamous, narrower in the cassowaries and the emus and extremely narrow in the rheas and ostriches. As do Wilson and Sibley, Cracraft believes the tinamous to be the closest relatives of the ratites. He conclude from his study that the ostrich is closest to the rhea among the ratites--that they are more closely related than either Wilson or Sibley would agree. Further, Cracraft and Sibley both have the ostrich and rhea more closely related to each other, so far, than does Wilson.

Which analysis? The differences are important because they involve a critical issue in the discipline: analysis of the data. Drawing up a table of the differences, molecular or anatomical, between species is only the first step in constructing a phylogenetic tree. The next step is to apply to the data one method or another of comparative analysis to construct the tree.

Several methods of analysis can be applied to the different sets of data. One method, developed by Fitch at the University of Wisconsin, can be used on the DNA hybridization data of Sibley or the protein differences of Wilson. It starts with a matrix that lists, in tabular form, the differences between species. Constructing a phylogenetic tree can be regarded as building another matrix, read directly from the tree, which also gives the distances between species. The basic idea of this analysis is to minimize the difference between the first matrix and the second.

A different technique has been used by James S. Farris of the State University of New York at Stony Brook. Using the same table of differences between characteristics Farris's system produces a phylogenetic tree that meets what are called parsimony criteria by having branches that are as short as possible.

That the application of one system or another is far from straightforward at this stage, however, is illustrated by what happened when Sibley distributed some of his unpublished data to other investigators for comment. Farris, at the request of Cracraft, did an analysis of the Sibley data, using the method developed by Fitch, Farris and Cracraft then announced their results: Sibley's data fit Cracraft's tree better than they did Sibley's.

Sibley is unable to understand how a rigorous analysis could have produced such a result. His own subsequent analysis, he says, employing both Fitch's and Farris's techniques as well as others, resolves the distance between the emu and cassowary as well as quantifies distances from the nearby kiwi and distant tinamou. He has been unable to examine Farris's procedure, he says, but his own analyses produce not only distances between species but quantitative measures of the reliability of the distance estimates as well.

To compound the difficulties, Wilson argues further that the Fitch type of analysis "doesn't give a unique solution. You try a whole bunch of trees and estimate the goodness of the fit. You get several solutions that are almost equivalent. Our tree is a blend of the best ones."

The reason these points are important, Wilson says, is that "we are seeing a big revolution in the way people are looking at evolutionary biology. Biology is being unified in a way it never was before, and taxonomists now have to know molecular biology, as do paleontologists."

For the first time, Wilson continues, ornithologists can talk about constructing a quantitative phylogenetic tree for all the 27 orders of birds. "Despite more than a century of research," he says, "morphologists did not succeed in producing such a tree. But the main outlines of the avian family tree are now evident.... "Such a biochemical tree for birds was included in an article Wilson and Ellen Prager have in the proceedings of the 1978 International Ornithological Congress.

The controversies are probably just beginning as systematic biology enters a new era of numerical measurement and analysis. "We're going to fight about methodology for a long time," Fitch says. "The truth is that our methods are just not good enough right now."

As a consequence, different actors in this drama can and do point out the shortcomings of the various approaches. Critics of Sibley's DNA hybridization technique contend that, by looking at the entire genome of a species--the method's strong point, according to Sibley--the approach smooths out the distinctive differences that could be important. Cracraft's technique draws the same criticism: "Morphology is the consequence of many genes acting on structure," Fitch says. "So the morphological data could be subject to a smoothing caveat similar to that raised against Sibley's data." Wilson himself points out the imprecision of his own data. And, against both protein and morphological analysis is raised the epigram: the farther from the gene, the farther from the truth.

The debate about ratite phylogeny thus can be seen as the forerunner of future controversies that will arise as the new methods spread through systematic biology and produce new information coming out of our ears," says Fitch. "It will be marvelous, tantalizing information. The data are going to tell us a lot. But we don't know what."

• Possible Inheritance of Acquired Characters

THE INHERITANCE OF ACQUIRED CHARACTERS

Spencer, Herbert; *Nature,* 41:414-415, 1890.

Without expressing any opinion upon the question recently discussed in your columns under the above title, I think it may be as well to recall the belief of one whose judgment was not without weight, and to give some of the evidence on which that belief was founded.

In the first chapter of the "Origin of Species" (p.8 of the sixth edition), Mr. Darwin says, respecting the inherited effects of habit, that "with animals the increased use of disuse of parts has had a more marked influence"; and he gives as instances the changed relative weights of the wing-bones and leg-bones of the wild duck and the domestic duck, and, again, the drooping ears of various domestic animals. Here are other passages taken from subsequent parts of the work:--

"I think there can be no doubt that use in our domestic animals has strengthened and enlarged certain parts, and disuse diminished them; and that such modifications are inherited" (p.108). And on the following pages he gives five further examples of such effects. "Habit in producing constitutional peculiarities, and use in strengthening and disuse in weakening and diminishing organs, appear in many cases to have been potent in their effects" (p.131). "When discussing special cases, Mr. Mivart passes over the effects of the increased use and disuse of parts, which I have always maintained to be highly important; and have treated in my 'Variation under Domestication' at greater length than, as I believe, any other writer" (p. 176). "Disuse, on the other hand, will account for the less developed condition of the whole inferior half of the body, including the lateral fins" (p.188). "I may give another instance of a structure which apparently owes its origin exclusively to use or habit" (p.188). "It appears probable that disuse has been the main agent in rendering organs rudimentary" (pp.400-401). "On the whole, we may conclude that habit, or use and disuse, have, in some cases, played a considerable part in the modification of the constitution and structure; but that the effects have often been largely combined with, and sometimes overmastered by, the natural selection of innate variations" (p.114).

In his subsequent work. "The Variation of Animals and Plants under Domestication," he writes:--

"The want of exercise has apparently modified the proportional length of the limbs in comparison with the body" [in rabbits] (p.116). "We thus see that the most important and complicated organ (the brain) in the whole organization is subject to the law of decrease in size from disuse" (p.129). He remarks that in

birds of the oceanic islands "not persecuted by any enemies, the reduction of their wings has probably been caused by gradual disuse." After comparing one of these, the water-hen of Tristan D'Acunha, with the European water-hen, and showing that all the bones concerned in flight are smaller, he adds:--"Hence in the skeleton of this natural species nearly, the same changes have occurred, only carried a little further, as with our domestic ducks, and in this latter case I presume no one will dispute that they have resulted from the lessened use of the wings and the increased use of the legs" (pp.286-87). "As with other long-domesticated animals, the instincts of the silk-moth have suffered. The caterpillars, when placed on a mulberry tree, often commit the strange mistake of devouring the base of the leaf on which they are feeding, and consequently fall down; but they are capable, according the M. Robinet, of again crawling up the trunk. Even this capacity sometimes fails, for M. Martins placed some caterpillars on a tree, and those which fell were not able to remount and perished of hunger; they were even incapable of passing from leaf to leaf" (p.304).

Here are some instances of like meaning from vol.ii.:--

"In many cases there is reason to believe that the lessened use of various organs has affected the corresponding parts in the off-spring. But there is no good evidence that this ever follows in the course of a single generation. . . .Our domestic fowls, ducks, and geese have almost lost, not only in the individual but in the race, their power of flight; for we do not see a chicken, when frightened, take flight like a young pheasant. . . With domestic pigeons, the length of sternum, the prominence of its crest, the length of the scapulae and furcula, the length of the wing as measured from tip to tip of the radius, are all reduced relatively to the same parts in the wild pigeon." After detailing kindred diminutions in fowls and ducks, Mr. Darwin adds, "The decreased weight and size of the bones, in the foregoing cases, is probably the indirect result of the reaction of the weakened muscles on the bones" (pp.297-98). "Nathusius has shown that, with the improved races of the pig, the shortened legs and snout, the form of the articular condyles of the occiput, and position of the jaws with the upper canine teeth projecting in a most anomalous manner in front of the lower canines, may be attributed to these parts not having been fully exercised. . .These modifications of structure, which are all strictly inherited, characterize several improved breeds, so that they cannot have been derived from any single domestic or wild stock. With respect to cattle, Prof. Tanner has remarked that the lungs and liver in the improved breeds 'are found to be considerably reduced in size when compared with those possessed by animals having perfect liberty' . . . .The cause of the reduced lungs in highly-bred animals which take little exercise is obvious" (pp.299-300). And on pp.301,302, and 303, he gives facts showing the effects of use and disuse in changing, among domestic animals, the characters of the ears, the lengths of the intestines, and, in various ways, the natures of the instincts.

Clearly the first thing to be done by those who deny the in-

heritance of acquired characters is to show that the evidence Mr. Darwin has furnished by these numerous instances is all worthless.

. . . .

ANOTHER LOOK AT LAMARCK

Gould, Stephen Jay; *New Scientist,* 84:38-40, 1979.

Another phenomenon, passing under a variety of names, including the "Baldwin effect" and "genetic assimilation", seems more Lamarckian in character but fits just as well into a Darwinian perspective. To choose the classic illustration: Ostriches have callosities on their legs where they often kneel on hard ground, but the callosities develop wthin the egg, before they can be used. Does this not require a Lamarckian scenario: Ancestors without callosities began to kneel and developed them as a non-genetic adaptation acquired during life, just as we, depending on our profession, grow writer's callouses or thickened soles. These callosities were then inherited as genetic adaptations, now forming well before their use. (Excerpt)

• Edibility of Birds and Their Eggs

THE EDIBILITY OF BIRDS

Cott, Hugh B.; *Nature,* 156:736-737, 1945.

Here follows an excursion in taste and its possible consequences in evolution.

In the autumn of 1941 I happened to be preparing some bird-skins of Beni Suef, Middle Egypt, when a casual observation led to what has proved a somewhat fruitful and little-explored line of inquiry. A palm dove (Streptopelia senegalensis) and a pied kingfisher (Ceryle rudis) had been skinned and the carcases thrown aside. Hornets were very plentiful in the garden where I was working and soon collected on the meat; but they were seen to concentrate their visits on one carcase, leaving the other almost neglected. Closer inspection showed that it was the dove that was receiving their attentions: with the kingfisher they would have little to do.

This apparent preference for one bird of the pair suggested

further investigation, and it was decided to test the edibility of a wide range of birds, using Vespa orientalis as an indicator. Innumerable observations and experiments have hitherto been made on the palatability of various groups of animals---notably insects---with special reference to the theories of concealing and warning coloration and mimicry; but so far as I am aware, birds have remained largely neglected in this respect. The experimental method here adopted is an inversion of that so frequently used, in which birds have played the part of taster in investigations on the palatability of insects.

During the period 1941--44 many experiments were carried out, both in Egypt and the Lebanon, as a result of which it has been possible to assess the relative edibility of some forty species of birds in forms of hornet-preference. As the work developed, it became clear that the preferences of the insects were not dissimilar from those of other and quite unrelated meat-eaters such as the domestic cat and men. Details of this research will be published elsewhere: it is only possible here to refer briefly to some points of interest.

Among the birds tested were members of several Passerine families, including larks, wagtails, bulbuls, fly catchers, warblers, finches, buntings and swallows, together with birds of other orders including wryneck, hoopoe, buff-backed heron, white stork and little owl. One broad conclusion to be drawn from this work is that in the birds used there appears to be a relationship between edibility of the flesh and visibility of the plumage.

When arranged in order of preference, it is found that the species heading the list are those with the most effective concealing coloration, wryneck (Jynx torquilla) and crested lark, (Galerida cristata) taking first and second place. Reading upwards from the bottom of the series we have the following: white-rumped black chat (Oenanthe leucopyga), pied king-fisher, mourning chat (O. lugens), masked shrike (Lenius nubicus), hoopoe (Upupa epops), hooded chat (O. monacha), golden oriole (Oriolus oriolus), redbacked shrike (L. collurio), lesser grey shrike (L. minor), European swallow (Hirundo rustica), common kingfisher (Alcedo atthis), red-rumped swallow (H. daurica)---all birds which in the field are highly conspicuous, the chats referred to being black-and-white deserticolous species.

Two striking exceptions to this general statement were provided by the buff-backed heron (Ardeola ibis) and white stork (Ciconia ciconia), both of which are extremely conspicuous and at the same time relatively palatable. It will be noted that both are large birds, which by reason of gregarious habits in the first case and powers of defence in the second are presumably not much subject to predacious attack in Nature.

This inverse relationship between edibility and visibility appears to occur in certain cases even within the limits of a family. For example, the finches and a bunting tested are found to stand in the following order of preference: greenfinch, house sparrow, Spanish sparrow, Cretzschmar's bunting, goldfinch, the last being rated least acceptable. Similarly, the blue-headed wagtail appears to be preferred to the white wagtail.

Experiments with the preferences of the cat for birds of various species have not yet reached a stage which would justify drawing any general conclusions; but it may be mentioned that in some instances the cat seems to be in close agreement with the hornet in his notion of what is fit to eat. For example, cats rate the pied kingfisher, hoopoe and a black-and-white chat (O. finschii) as relatively distasteful compared with sparrows, doves and other birds.

When we turn to the preferences of man, it becomes apparent that the birds---of many groups---which are most prized for the table are those which are specialists in cryptic camouflage; it is only necessary to mention a few: Passeres (skylark, meadow lark); Columbiformes (various fruit pigeons such as Osmotreron pompadora and Vinago delalandii); Charadriiformes (American golden plover and grey plover, woodcock, common snipe, stone curlew, bronze-wing courser); Gruiformes (bustards and floricans); Galliformes (notably grouse and quail); Anseriformes (teal, mallard, gadwall and others); Caprimulgiformes (European nightjar).

Conversely, there is some evidence that numbers of highly conspicuous birds belonging to many different orders are definitely unfit for the table: sheld-duck, crocodile bird, magpie, and swallows being examples, though there are, of course, notable exceptions, such as the rook and starling, which make tolerable eating. Furthermore, here again within certain natural groups of birds the general correlation obtains. To take one example, among the Ralliformes, landrail, clapper rail, and Carolina crake are highly palatable and cryptic in coloration and habits; coot and moorhen, though 'eatable', are scarcely accountable as table-birds and are among the most conspicuous members of the order.

Thus there would seem to be some evidence that the correlation above referred to is fairly general among birds as a class, and these facts would appear to bring birds into line with other groups of animals such as the Anura and Insecta, in which the relationship between edibility and cryptic coloration on one hand and between certain classes of conspicuousness and unpalatability on the other, has been well established.

I am at present collecting further information on the above subject and would be glad to hear from members of expeditions, travellers, sportsmen and others who have tasted birds---of whatever kind---which are not usually considered as game. Such information is infrequently included in ornithological literature and would be welcomed as a contribution to the present investigation.

EDIBILITY OF THE EGGS OF BIRDS

Cott, Hugh B.; *Nature,* 161:8–11, 1948.

In a previous communication, a preliminary account was given of observations and experiments on the relative edibility of the

flesh of birds. Full details of this research have now been published elsewhere. During the past two years, this work has been extended to an investigation of the relative edibility of birds' eggs. Final results of the inquiry, which is still in progress, will not be available for some time; but since, with the exception of a paper by C.F.M. Swynnerton published in 1916, the subject appears hitherto to have remained almost unexplored, it would now seem appropriate to summarize the new observations and to indicate certain conclusions to drawn from them.

The bulk production and purchase of various foodstuffs (including eggs) during the War led to a widespread use of tasting panels (together with other methods for the assessment of quality) for the purposes of standardization and grading. At the Low Temperature Research Station, Cambridge, a tasting panel was already in existence in 1936; and I take this opportunity of expressing my indebtedness to the three members of this panel--Dr. J. Brooks, Mr. H.P. Hale and Dr. J.R. Hawthorne--whose skill and experience have made possible the reliable assessments of palatability which form the basis of the present inquiry. Sincere thanks are also due to the Superintendent of the Station for allowing me storage and many other facilities in his Department.

The method of assessment was similar to that used in routine examination of fowls' eggs. Each sample was tested in the form of a scramble, prepared over steam, a numerical score being awarded on a scale ranging from 10·0 (excellent flavour) to 2·0 (inedible). In general, seven or eight species were tested at one session. Members of the panel were in all cases unaware of the identity of the samples to be examined (except in the case of a fowl's egg used as a control); and they recorded their results independently. The majority of species listed below were examined on two occasions by each taster, so that in general the mean edibility rating for a species is based upon six independent assessments. Eggs of eighty-one species, belonging to eleven orders and thirty-five families, have now been examined. In the following list these are arranged in order of palatability.

GROUP A (RELATIVELY PALATABLE)

Domestic fowl	8·8
Coot	8·3
Moorhen	8·3
Lesser black-backed gull	8·3
Kittiwake	8·2
Herring-gull	7·9
Common tern	7·8
Fulmar petrel	7·7
Greater black-backed gull	7.7
Guinea-fowl	7.6
Razorbill	7.6
White-naped crane	7.5
Chaffinch	7.5
Hedge-sparrow	7.5
Partridge	7.3

Lapwing	7.3
Spur-winged plover	7.3
Stock-dove	7.3
Jackdaw	7.3
Spotted flycatcher	7.3
Domestic turkey	7.2
Red-legged partridge	7.2
Bullfinch	7.2
Southern guillemot	7.1
Great skua	7.1
Rock-dove	7.1
House-sparrow	7.1
Common gull	7.0
Dipper	7.0

Group B (INTERMEDIATE)

Hobby	6.9
Cirl bunting	6.9
Black-headed gull	6.8
Wood-pigeon	6.8
Little owl	6.8
Kestrel	6.7
White-fronted goose	6.7
Carrion-crow	6.7
Magpie	6.7
Pheasant	6.6
Domestic duck	6.6
Robin	6.5
Rook	6.3
Skylark	6.3
Catbird	6.3
Sparrow-hawk	6.2
Little tern	6.1
Eider	6.0
Red-backed shrike	6.0
Swallow	6.0
British jay	5.8
Long-tailed tit	5.8
Blackbird	5.8
Greenfinch	5.7
Chiffchaff	5.7
Meadow-pipit	5.7
Sand-martin	5.7
Capercaillie	5.5
Song-thrush	5.5
Cowbird	5.4
Tree-sparrow	5.3
Yellow bunting	5.3
Gannet	5.3
Oyster-catcher	5.2
Bar-headed goose	5.2
Dabchick	5.0
Turtle-dove	5.0

GROUP C (RELATIVELY UNPALATABLE)

Puffin	4.8
Sedge-warbler	4.7
Blackcap	4.7
Lesser whitethroat	4.5
Shag	4.4
Sandwich tern	4.3
Little crake	4.0
Linnet	3.9
Ringed plover	3.8
Whitethroat	3.8
Great tit	3.5
Reed-warbler	3.4
Blue tit	3.3
Eastern house-wren	3.2
Wren	2.0

It might be expected, in the absence of contrary evidence, that the palatability of an egg would show some relation to that of the bird which laid it, and that an acceptable or repugnant flavour in the former might be a reflexion of palatable or distasteful properties of the parent. Indeed, it is true that some birds with high-grade flesh lay palatable eggs---as is notably the case with Galliformes, and with some Charadriiformes and Columbiformes. Similarly, in the oyster-catcher both eggs and flesh are unpalatable. Nevertheless, in general there is a wide discrepancy between the quality of the flesh and eggs of various species. Thus, the relatively distasteful common tern, jackdaw, and bullfinch lay palatable eggs. Conversely, many of the species the eggs of which are bitter and repugnant, including turtle-dove, little crake, ringed plover, yellow bunting, tree-sparrow, sedge-warbler and reed-warbler, are themselves good or excellent as food. Such divergences no doubt follow from the fact that while there is a general correlation between coloration and palatability, both for eggs (see below) and for birds, the coloration of the egg itself has no relation to that of the parent bird.

We have also to note that there is no close or obvious relationship between the food-habits of a bird and the flavour of its egg. For example, the eggs of many maritime or shore-feeding species such as kittiwake, common gull, herring-gull, lesser and greater black-backed gulls, common tern, great skua, razorbill guillemot and fulmar petrel, are good in flavour and free from 'fishiness'. The same is true of other outside the series examined. Thus, as I am informed by Mr. G. A. Gibson-Hill and Dr. L. Harrison Matthews, the eggs of king (Aptenodytes p. patagonica) gentoo (Pygoscelis p. papua), jackass (Spheniscudemersus), macaroni (Eudyptes chrysolophus) and rockhopper penguins (E. c. crestatus) are quite palatable and without any unpleasant or fishy flavour. The last-named is described by Earle (in Murphy) as "particularly fine and delicate in flavour". Similarly, Gibson-Hill and Matthews considered the eggs of black-browed (Diomedia-

melanophrys) and wandering albatross (D. exulans) very good eating and nearly as palatable as those of domestic fowl. Conversely, the eggs of some species (see below) which do not eat fish or feed along the shore yet have a more or less pronounced and unpleasant fishy flavour. This apparent lack of correlation between edibility status of the egg and feeding-habits of the parent is again illustrated by insectivorous species, which may have high-grade (spotted fly-catcher, dipper), intermediate (sand-martin), or low-grade eggs (sedge-warbler, reed-warbler, wren). Similarly, the eggs of mixed (seed and insect) feeders may vary widely: thus we have the palatable chaffinch and hedge-sparrow; the intermediate greenfinch and yellow bunting; and the unpalatable linnet and blue tit. It appears, therefore, that no simple explanation of distastefulness, in terms of food-derived deterrents, is admissible.

There appears to be a broad relationship between relative edibility and egg-size. The shell-length of the species so far examined ranges from 104.0 mm. (white-naped crane) to 14.0 mm. (eastern house-wren). The five most distasteful eggs, those of the wren, eastern house-wren, blue-tit, great tit and reed-warbler, are all among the smallest tested, and range from 14.0 to 18.3 mm. Conversely, the twelve species heading the list as most palatable are mostly large eggs, ranging from 41.0 (common tern) to 104.0 mm.---seven of these species laying an egg larger than the domestic fowl's.

. . . . .

There also appears to be a general correlation between the degree of conspicuousness of the egg and its palatability. Among nineteen species laying cryptic eggs in open situations, twelve occur in the palatable group (A) and five in the intermediate group (B), only two being recorded as unpalatable (C). Moreover, within Group A, cryptic eggs predominate at the head of the list, the first six species (omitting domestic fowl) all falling into this category. Twelve other species, in which non-cryptic eggs are covered by a cryptic parent on open nests, are also distributed (with one exception) in Groups A and B. Of the remaining fifty species, in which the eggs are more or less conspicuously coloured, and are not incubated by a cryptic parent, twelve are unpalatable, representing no less than 80 per cent of all species in Group C.

. . . . .

The various species rated as least palatable owe their distastefulness to a variety of unpleasant or foreign flavours: such terms as acid, rancid, soapy, sour, oily, salty, fishy, and bitter occurring in the reports of the panel. Eggs of the oyster-catcher (Haematopus ostralegus occidentalis) (5.2) are markedly different from those of all other eggs tested, being remarkable both for their strong aromatic odour and flavour, and for their wide individual variation: thus of three samples examined, the first was rated by the three observers at 4.0, 4.0, 5.0, each member of the

panel recording a strong smell and taste of onions; the second, at 8.5, 7.5, 7.0, with normal or nutty flavour; the third, at 4.0, 4.0, 3.0, inedible, with a strong odour and flavour of hemp. Species rated as 'fishy' were numerous, those with a markedly rank and fishy smell and taste being shag (4.4), bar-headed goose (5.2), eider (6.0), sparrow-hawk (6.2), ringed plover (3.8), sandwich tern (4.3) and blue tit (3.3). It will be noted that some of these, such as the sparrow-hawk and blue tit, are not themselves fish-eaters.

But by far the most widespread and important distasteful quality is bitterness. This is a frequent attribute of eggs in the lowest palatability grades. In nineteen species this flavour was more or less pronounced (being noted in at least 50 per cent of tests on the species concerned). Fifteen of these bitter-tasting eggs were those of passerine birds: greenfinch, linnet, yellow bunting, tree-sparrow, meadow-pipit, long-tailed tit, red-backed shrike, sedge-warbler, reed-warbler, chiffchaff, common and lesser whitethroats, eastern house-wren, swallow and sand-martin. It will be noted that all these are small species, laying eggs between 23.0 and 14.0 mm. long. Moreover, the remaining four species recognized as markedly bitter, namely, ringed plover, little tern, little crake and turtle-dove, are in each case the smallest eggs examined within their respective orders or families. To this we may add that the two smallest eggs of Corvidae (jay and magpie) were both recorded (in two observations out of six for each species) as bitter; the same being true of the puffin---the smallest of three auks examined.

This incidence of bitterness among the smaller eggs laid by members of four different orders, Passeriformes, Charadriiformes, Gruiformes and Columbiformes, raises an interesting ecological question, and suggests that this quality may have been developed as a deterrent to attack by potential egg-eating predators. Obviously, the point requires further investigation, especially in the light of field observations, but the suggestion appears to be supported by three types of evidence.

In the first place, while distasteful properties of the egg appear to be neither directly related to the feeding-habits nor to the palatability of the parent bird, they are, on the other hand, often associated with small size and with conspicuousness. Size is an important factor in the vulnerability of a species in Nature. In the absence of compensating adaptations, smaller birds are liable to attack by a wider range of predators, and are less able to protect themselves and their eggs during the incubation period than larger species. Consequently any deterrent property such as relative distastefulness in the eggs of such birds is likely to be of selective value in the interspecific struggle for existence---especially if it is associated with conspicuous coloration easily recognized and remembered by potential predators.

Secondly, Rensch has shown, in his work on the sensitivity of birds to various tastes, that the lower limit of (externally ascertainable) taste perception is not markedly different from that of man; and that while relative insensitivity to bitterness is characteristic of birds feeding on seeds or insects which often have a

sharp or bitter flavour, other species, and in particular birds of prey, such as the goshawk (Accipiter gentilis) and tawny owl (Strix aluco), are especially sensitive to bitterness.

A third approach to the question is through feeding experiments with egg-eating predators. We have already noted that there is a fair agreement between the palatability ratings of man and the apparent preferences of the ferret, rat, hedgehog and mongoose, and that this agreement is closest for the eggs rated as least edible by man. The series of thirty-five egg-species used in the rat tests includes eleven species rated as bitter, and all these occur in the lower half of the palatability sequence. Among eighteen species used in the hedgehog experiments, the three which appear at the end of the list as least palatable to these animals are the only three markedly bitter eggs offered.

In conclusion, it may be noted that previous work on the relative edibility of the flesh of birds affords a series of facts parallel in many respects to those here considered. Thus cryptic coloration, whether of plumage or egg-shell, appears to be correlated with palatability; and conspicuousness, in otherwise vulnerable species, often to be associated with a distasteful or deterrent property. I hope to continue the present investigation next season, and should be glad to hear from anyone (a) who has tasted eggs of tropical or other species not usually eaten by man, or (b) who might be willing to submit newly laid eggs (of species not included in the above series) for examination.

• Incredible Adaptations of Woodpeckers

MIRACULOUS DESIGN FEATURES IN WOODPECKERS

Sunderland, Luther D.; *Creation Research Society Quarterly,* 12:183, 1976.

The woodpecker has a number of specially designed features which distinguish it from other birds, and permits it efficiently to function as a living "jackhammer".

One of the most amazing features is a tongue that extends three to five times the normal length to extract worms and insects from hollow trees. One might think that the woodpecker would need to have a tongue rooted in the tail to perform this neat trick; but in reality, the tongue originates in the right nostril. It exits the right nostril, splits into two segments, wraps around the skull beneath the skin passing on either side of the neck bones, joins together and comes up through the lower mandible (jaw). It is composed of an elastic tissue and the root portion is stiffened by five tiny bones called the hyoid apparatus.

These bones can be seen clearly in the cover illustration which is an untouched picture of a flicker woodpecker's skull found in this exact condition in the woods by Mrs. Clair Gotshall in Susquehanna County, Pennsylvania. Insects had apparently picked the bones clean of all flesh.

As can be seen, two of the hyoid bones exit the right nostril and are connected to the next two bones by miniature ball joints. A third ball joint appears at the fork as it passes through the jaw where the fifth bone is joined which stiffens the outer portion of the tongue.

Charles Darwin's Omission. Charles Darwin wrote that the thought of how anything as complex as the eyeball could have evolved turned him cold. Well, the woodpecker's tongue must have literally frozen him up completely; for he wrote an entire page in The Origin of Species about the marvelous adaptations of the 179 species of woodpeckers, but he didn't even discuss the most marvelous feature of all, the origin of the tongue in the right nostril. Neither the Encyclopedia Britannica nor Americana contains any mention of it either, although the 1972 edition of the World Book Encyclopedia contains a sketch of the complete tongue with hyoid.

Other special features are: A tough beak that can drive through hard wood which would bend a nail; strong neck muscles that deliver jackhammer blows to the head and beak; a thick skull that withstands continuous shocks that would kill or give a bad headache to other birds; a glue "factory" in the tongue which makes the surface sticky; stiff tail feathers with sharp spines which brace the bird for hammering; and viselike toes with two in front and two in back, rather than three and one like other birds. All these features are integrated into a perfect living, flying jackhammer.

Admitted Insurmountable Problems. Think of the insurmountable problems this bird poses for evolutionists. They would need to start their evolutionary speculations with a normal bird like a robin. Think how many millions of robins would need to have bashed their brains out trying to play jackhammer before, not just one, but a pair of them accidentally and gradually mutated a tongue like that. Even if two were lucky enough to come up with the right tongue, it would have been of no adaptive advantage without the other features.

This woodpecker's skull has been more effective in convincing scientists of the inadequacies of the evolution "theory" than perhaps any book in the author's library. Other birds have hyoid bones, also, but it would seem obvious that some sort of miracle would be needed to get them rooted in the right nostril.

One prominent evolutionist on the editorial staff of a prestigious scientific magazine confided to the author after examining the skull, "There are certain anatomical features which just cannot be explained by gradual mutations over millions of years. Just between you and me, I have to get God into the act too sometimes."

Another scientist observed, while examining the skull under a microscope, "It is very easy to tell the difference between man-

made and God-made objects. The more you magnify man-made objects, the cruder they look but the more you magnify God-made objects the more precise and intricate they appear."

• Utility of Bird Colors

BIRD COLOURS

Krebs, John R.; *Nature,* 282:14-16, 1979.

Most biologists, if asked to hazard a guess about the possible significance of the brilliant plumage adornments of many bird species, would probably say that bright colours evolved in the context of ritualised display between rival males or as attractants to lure females. They would point to the fact that male birds are usually brighter than females, and exotic male plumage is especially highly developed in polygamous birds. Males generally compete for females, and this competition is more intense in species where one male can acquire a larger number of mates, so sexually selected colours are expected to be found in males, and especially in polygamous males. This was the line of argument put forward by Darwin, and although a number of alternatives have been proposed from time to time, the first serious attempt to examine Darwin's idea by means of a large scale comparative survey has recently been published by Baker and Parker (Phil. Trans. R. Soc., B287, 63-120; 1979). As a result of their analysis, Baker and Parker reject the sexual selection hypothesis and replace it with what at first sight may seem a surprising idea: that bright colours have evolved in response to selection pressures exerted by predators.

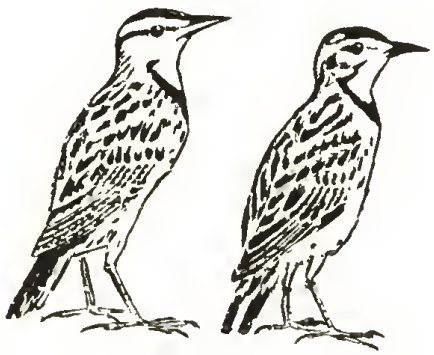
. . . . .

Baker and Parker point out that distastefulness is not the only basis for aposematic colouration: if a prey item is hard to catch, bright colours may evolve as a signal of unprofitability. They suggest that this might be especially applicable to birds, which once they have seen a predator can manoeuvre fast enough to be almost certain of escape. Thus the bright colours of many birds might signal not only "I am distasteful" but also (or alternatively) "I am hard to catch" or "I have seen you".

. . . . .

Baker and Parker restrict their analyses to overall differences between species in conspicuousness of particular body regions.

Equally intriguing are remarkable convergences in the fine detail of plumage between unrelated species occupying similar ecological niches in different parts of the world. The African yellow throated longclaw (Macronyx croceus) and the North American meadowlark (Sturnella magna) constitute perhaps the most remarkable pair, but there are many others. It is tempting to conclude that ecological selection pressures must influence the fine detail of plumage as well as overall conspicuousness, and a comparative survey like that of Baker and Parker could be a way of testing this conclusion.



Remarkable convergence in plumage between two unrelated species occupying similar ecological niches. (Left) North American meadowlark; (right) African yellow-throated long claw.

INSTANCE OF A BLACKBIRD TURNING WHITE FROM FRIGHT

Smith, Alfred Charles; *Zoologist,* 10:3576-3577, 1852.

Every one at all cognizant of the manners and customs of the English in olden time, knows that it was an ancient custom in this kingdom, at the time when the humane sports of bull-baiting, cock-fighting, and the like, were in vogue with our merry forefathers, for the enlightened populace of England to collect together in every village on Shrove Tuesday, for the purpose of throwing stones and other missiles at some unfortunate cock, doomed to destruction on that day, and that he who first knocked over the miserable bird claimed him as his lawful prize: this was the sport known as "cocking." But perhaps it may not be so generally known that the remains of this ancient and barbarous custom still exist in some of the villages of Somersetshire, though in quite a different form, under the name of "crocking." It is the habit at this day, in some parts of that county, if there be any especially disagreeable person in the village, for his or her neighbours to throw down, with most discordant and crashing

noise, at the door of their victim, all the broken pots and pans and such-like crockery that they have collected during the preceding twelvemonth; this takes place on the night of Shrove Tuesday, and as the actors in the sport are usually pretty numerous, and their object is to make as much clatter and din as possible, we may imagine the disturbance and noise created at such a time to be of the most deafening description. In a certain parish of Somersetshire, of which a relative of mine was the curate, an old lady, for some cause or other which it matters not to relate, became very obnoxious to her neighbours, and they, following the ancient custom of the place, saluted her on a certain Shrove Tuesday evening with all the broken crockery they could collect; indeed they carried out the pastime of "crocking" to the full. Now it chanced that hard by this persecuted individual was a publichouse, the owner whereof had a tame blackbird, whose sleek black coat and rich notes had charmed many a lazy loiterer, and the bird was well known to all the inhabitants of the parish: but though the blackbird had doubtless listened to many a deep argument in the tap-room, and had his own peculiar opinions about protection, I fear he had never been initiated into the mysteries of "crocking," and so his mind had never been prepared for the horrors he had to undergo. But time went on, the end of the Carnival set in, and the blackbird without doubt, as an orthodox bird, ate pancakes to the full, and went off to roost, when suddenly he awoke with a start, his ears were assailed with the most discordant and crashing sounds, such a diabolical noise as he had never before heard so flurried and confounded him, that for two whole days he seemed to be panic-stricken, hopping about his cage incessantly, refusing his food, and apparently overcome with fright, and unable to shake off the terror with which the noise of the "crocking" had inspired him. So tremulous and confounded had the poor bird become, that fears were entertained for his life; however, at the end of two days, he began to be more tranquil, and gradually forgot his late fright, and became once more the same jovial merry fellow he had been before. But soon the season for moulting came on, and now, instead of the black coat for which he was so famous, he assumed a piebald dress of black and white, to the great amazement of all who saw him. I have known more than one instance where the hair of a human being has been turned white in a single night from sudden and extreme terror; but this is the first instance that ever came to my notice of such an effect being produced on any irrational creature: however, I can vouch for the truth of the above statement, as my friend, who told me the story, knew all the circumstances well, and had constantly seen the bird in question, both before and after his fright.

• Implications of Convergence

PREDETERMINED EVOLUTION

Cockerell, T. D. A.; *Science,* 13:311-312, 1901.

The American Redstart (Setophaga ruticilla) is structurally very widely separated from the true Redstart (Ruticilla phoenicurus) of Europe, and yet outwardly resembles it to an extraordinary degree. This fact has caused Professor Alfred Newton (Ency. Brit., XX., 318) to write as follows: "The wonderful likeness, coupled of course with many sharp distinctions, upon which it would be impossible to dwell, between the birds of these two genera of perfectly distinct origin, is a matter that must compel every evolutionist to admit that we are as yet very far from penetrating the action of creative power, and that especially we are wholly ignorant of the causes which in some instances produced analogy."

Cases of this sort may excite our wonder, but they are much more common than is often realized. In New Mexico and Arizona we have a series of numerous species of snails, which possess shells in no way distinguishable, except in a specific sense, from those of the genus Polygyra, which is dominant in the eastern States. During the last two years the anatomy of several of these species has become known, and it turns out that they are not even closely allied to Polygyra, but represent a peculiar genus which has been named Ashmunella (Pilsbry and Cockerell). In Arizona and southern New Mexico there is another series of snails, which has nearly the shell of Epiphragmophora, a genus of the Pacific coast. The species were always referred to the last-mentioned genus until Professor Pilsbry recently dissected one of them, E. hachitana of Dall. It then appeared that we had here another perfectly distinct genus, which was named Sonorella (Pilsbry). But not only do these interesting resemblances occur between species of our continent; they are seen equally between species of different continents. Some of the California species of Epiphragmophora so closely resemble the European Arionta that naturalists were for a long time deceived. I have recently had occasion to notice the extraordinary resemblance between certain Japanese snails and those of the United States. Thus Eulota connivens (Pfr.) of Japan might easily be taken for Sonorella hachitana of Arizona; and Eulota mercatoria (Gray) is remarkably similar to Epiphragmophora fidelis (Gray), the first being from Japan, the second from Oregon.

Is it possible that we may find a real, if imperfect, parallel between this independent development of similar species and the development of diverse cells in the metazoa? A human being, for instance, contains innumerable cells of very diverse nature, all descended directly from the ovum or germ-cell. If these cells

were not parts of an organic whole, but lived separate lives, we should speak of their descent from a primitive common ancestor (the germ-cell) and their evolution in the course of countless generations into distinct genera and species. Coues, in fact, has gone so far, in writing of bird-anatomy, as to treat the different kinds of cells as pertaining to several genera and species, which he names.

But we are here met by the extraordinary fact that all this complicated development and evolution is repeated anew in every individual, and that, speaking broadly, the course of cellular evolution is predetermined in the germ. This fact is so commonplace to us that we have ceased to realize the wonder of it, or its possible significance as a hint of the method of evolution among species.

Why may it not be that the evolution of species, to a greater or less extent, is similarly predetermined, and that here is to be found the explanation of the phenomena described in the beginning of this note? If life exists in Mars, a knowledge of it would go far toward answering such a question. How much similarity would there be between creatures evolved on two planets, with all the diversity of conditions which this implies?

• Amazing Mimicry in Parasitic Birds

A MORE CUNNING BIRD THAN THE CUCKOO

Anonymous; *New Scientist,* 28:355, 1965.

When a young cuckoo pushes its foster brothers and sisters out of the nest, it kills potential hosts which might care for its own offspring, and puts a biological limit to its own population increase in relation to the host population. The cuckoo partly overcomes this difficulty by placing his eggs in the nests of a variety of species.

The African Viduinae also put their eggs in the nests of other species. But they have adapted themselves to a group of very colourful finches (Estrildidae) in a very specialized way. There are 64 species of Estrildidae and apparently the Viduinae have adapted to each one of these species. The Estrildidae nestlings have very obvious coloured patterns on and inside their beaks and on the larynx, different for each species. The nestlings of the Viduinae copy these patterns perfectly. The young Viduinae do not push the young of their hosts out of the nest, but peacefully live together with them: even when fully grown they live for some time with their foster family.

Male Viduinae include a stanza of the song of their hosts in their own song. This helps them to find females which were brought up among the same species of Estrildidae and whose offspring will therefore have the same beak pattern. Female Viduinae begin to ovulate only when they have noticed a male of their own foster family starting to build a nest (Naturwissenschaft und Medizin, Vol. 2, No. 3).

THE EVOLUTION OF THE CUCKOO

Bulman, G. W.; *Knowledge,* 9:449-450, 1912.

Those who hold the theory of natural selection, looking back in time, see the Cuckoo as a bird with the normal instincts of its kind. Darwin was persuaded that even to-day it still sometimes shows traces of those ancestral instincts. "It has also recently been ascertained," he wrote, "on sufficient evidence, by Adolf Muller, that the Cuckoo occasionally lays her eggs on the bare ground, sits on them, and feeds her young." How, then, in the struggle for existence did it attain its present strange and unique position among the birds of our country? That instinct which leads it to hand over the care of its eggs and young to foster-parents is doubtless a great advantage from the Cuckoo's point of view. There results for it a life of ease, no troublesome nest-building, no trying work of brooding over eggs, no voracious young to be fed at the expense of much time and trouble. The Cuckoo is free to speed off for Africa's sunny shores early in July, while other birds are still toiling over their broods. For the individual cuckoo the advantages are obvious; its life becomes a "primrose path of dalliance." The advantages to the race, however, are not so obvious. It is not clear that the Cuckoo leaves a more numerous and stronger progeny than it would it if reared its brood in the usual way, or than it did while it was still respectable. This, of course, is mere speculation, but the fact remains that the Cuckoo is not specially numerous in this country. It is less so than many birds which rear their young in the ordinary way. But, waiving this doubt as to the possible ultimate advantage of the habit, let us go back to the commonplace respectable Cuckoo, or Cuckoo-like bird building its nest and hatching and rearing its own young. Some abnormal twist in the brain of a certain Cuckoo led it to pick up one of its eggs and place it in the nest of a Titlark. We will suppose it thus disposed of all its eggs, though we are not quite sure that the etiquette of natural selection would not bind us down to one in the first instance. Let us suppose that this brood is more successfully reared than those treated in the ordinary way. This might arise from the fact that the Cuckoos were careless nurses---though this is not a quality that could be evolved by natural selection. Or an adult Cuckoo may have been unable to feed a full brood as well as a Titlark could feed a single young Cuckoo. But, however it may have arisen,

let us assume the advantage to have been with the Cuckoo which got its young reared by the Titlark. The quality or instinct which led the female Cuckoo to act thus would probably appear in some of the descendants, the rest inheriting normal instincts from paternal sources. But in the descendants of those which did inherit the new instinct, this would run a heavy risk of being swamped by intercrossing with others of normal instincts. This, however, is a very common difficulty in the case of an incipient new species, and we will suppose the new instinct managed to survive the flood. It might then be reinforced by the sporadic appearance of the like in other individuals. Those possessing the instinct would leave more numerous and stronger progeny, and those which did not adopt it would be finally weeded out by natural selection. Thus the Cuckoo race sauntered down a curious bypath of evolution to the idle life.

But if this be accepted as the general outline of the evolution of the Cuckoo there are also certain special points which call for attention. There are, for example, the strange instincts and actions of the young Cuckoo in the nest. In the Cuckoo's respectable days it cannot have been the little demon it now is. It cannot have been in the habit of turning its brothers and sisters and eggs out of the nest. So it probably had not then the convenient hollow in its back for holding the eggs. And yet these habits, instincts and structure seem absolutely essential to the well-being of the young Cuckoo. Only by turning everything else out of the nest can it obtain sufficient nourishment for itself. And yet the first Cuckoo hatched in a Titlark's nest cannot be supposed to have had these characters. It would get no advantage in the strange nest, and would probably be starved.

The eggs of the Cuckoo have suggested another evolutionary problem. More than a hundred years ago, the naturalist Salerne gave currency to the idea that the egg of the Cuckoo resembles those beside which it is placed. He was himself hardly a believer in the assertion, but gave it on the authority of an inhabitant of Sologne. In 1853, Dr. Baldamus brought forward the same idea, and supported it by a series of eggs in his cabinet. But English ornithologists were slow to accept this view. They saw for example, that in the case of the Hedge Sparrow, in whose nest the Cuckoo so frequently places its eggs, there was no resemblance at all. On the basis of the collection of Cuckoos' eggs in the Natural History Museum Sir Wm. Flower gives the following summing up of the question:---

> "We have now a fine series of Cuckoos' eggs, with those of the birds in whose nests they were laid, showing in many cases a great resemblance in colour, in others none at all. In some Hedge Sparrows' nests the Cuckoos' eggs are as blue as the others; but in some they are of the more usual speckled-brown. It has been doubted whether the blue eggs were really those of the Cuckoo, but Mr. Seebohm set the question at rest by taking an undoubted young Cuckoo (with its very different feet from the Sparrow's) from one of them. The Cuckoos' eggs vary much in colour, and, generally speaking (though with many exceptions), show some conformity to the eggs of the

bird in whose nest they are laid."

Professor Newton was inclined to accept the theory of the resemblance with the reservation that there are "numerous instances in which not the least similarity can be traced." Granted the resemblance, then, it is an obvious suggestion that its object is the more successfully to deceive the foster-parents. And this being so it admits of an explanation on the lines of natural selection, since those Cuckoos which most successfully palmed off their eggs on their dupes would succeed best in the struggle for existence. Thus the Cuckoo which laid a robin-like egg and placed it in a Robin's nest would succeed better than one whose egg had no resemblance to those of its host. And having once confided its egg to a Robin, a Cuckoo would be likely to continue doing so, and the daughters would be likely to inherit the habit. Thus there would be a tendency to produce separate races of Cuckoos, one laying blue eggs in Hedge Sparrows' nests, another blue brown-speckled eggs in Robins' nests and so on. It is easy to suggest objections to this explanation. In the first place it is not obvious that any deception is necessary. As a rule birds seem ready to sit on any sort of eggs. Our domestic hen will hatch ducks, turkeys or pheasants as readily as her own chicks; in Sir John Sinclair's classical attempt to introduce the nightingale into Scotland the eggs of this bird were hatched by Robins; the Hon. Daines Barrington reared linnets under Skylarks, Woodlarks and Titlarks; Starlings have been known to sit on and hatch bantams' eggs. Professor Newton, however, suggested that while some species of birds are thus easily deceived there may be others which are not. And it would be only in the nests of the latter that we should expect to find the Cuckoos' eggs approximating closely to the others. This would explain the numerous exceptions. But to test the view thoroughly we should require to have figures showing the relative frequency of the resemblance in the case of a number of different species of small birds. There is some evidence that the Reed Warbler is one of the objectors, but in the one case in which we have seen---in a museum---a Cuckoo's egg in the nest of this bird it was very different in size and markings. We suggest, however, that those Cuckoos which habitually placed their eggs in the nests of the easily deceived Robin and Hedge Sparrow would succeed so much better in the struggle for life than those which went with their not yet perfectly matching eggs to the nests of the more fastidious birds, that the latter would, according to the principles of natural selection be weeded out.

Let us suppose, however, that the Cuckoo species is divided into races laying eggs of different colours, blue, dark grey speckled, blue speckled with brown, and so on. Should we not expect this race segregation as regards eggs would be accompanied by some differences in plumage and other characters? In the case of the domestic fowl we know that differences in the eggs are associated with variations in other characters. No case of two varieties laying different eggs without variation in plumage, and so on, can be brought forward. But no such racial differences can be pointed out in the Cuckoo.

And then we must remember that the male Cuckoo has his part to play in the matter. Is there any evidence that a male hatched in a Wagtail's nest, for example, usually seeks for its mate a female laying Wagtail-like eggs, or reasons why it should? What marks are there by which he could recognise the right female, supposing his tastes were orthodox? And if he did not choose the right partner would not the variation in the direction of laying Wagtail-Cuckoo eggs be swamped? Nay, further, might it not happen that if a Hedge-Sparrow-Cuckoo mated with a male hatched from a Robin-Cuckoo egg, the blue-egg layers among the offspring would inherit the instinct of placing their eggs in Robins' nests, and those which laid eggs like the Robin's the instinct of choosing the Hedge Sparrow as foster-parent?

And then must we not also consider the question of the evolution of what we may call the receptivity in the foster-parent? Professor Newton points out that this varies in different species, and thus it becomes a quality subject to the action of natural selection. In the beginning, again, it must have been variable among individuals of the same species. Some would receive the Cuckoos' eggs, and some would reject them. The latter would succeed best in rearing their own offspring, while those which reared young Cuckoos would leave no inheritors of their---from the Cuckoo's point of view---virtues. Thus the quality of receptivity could never be evolved on the lines of natural selection: those possessing it would be weeded out.

The evolution of the Cuckoo by natural selection in fact, bristles with difficulties.

UNUSUAL PHYSICAL ABILITIES

• A Magnetic Sense in Birds

THE ROBIN'S COMPASS CAN'T TELL NORTH FROM SOUTH

Anonymous; *New Scientist*, 54:118, 1972.

There is growing evidence that birds make use of the Earth's magnetic field for direction finding when they migrate. If that is the case, it poses something of a teaser for evolutionists, for the

ancestors of today's birds would have had to adjust their compasses to 180° reversals of the field every half million years or so. Two West German zoologists have now performed experiments with European robins (Erithacus rubecula) which reveal that they employ a direction-sensing mechanism which depends on recognising the angle or inclination of the magnetic field instead of its polarity (Science, vol. 176, p.62).

. . . .

Especially interesting was the case where the field was northwards but with zero inclination. Here the birds' distribution was random, indicating that they lack a direct north-south sense. Hence it is clearly the axis, not the polarity, of the magnetic field to which they are sensitive.

However, the inclination of the Earth's magnetic field changes with latitude so that some other information is needed by the robins before they can determine direction. Obviously they would get lost at the magnetic equator where the inclination is zero; and it is significant that European robins only migrate within the northern hemisphere. The two Frankfurt workers suggest that robins combine their magnetic sense with their sense of up and down, selecting "north" as the direction in which the magnetic field axis makes the smaller of its two angles with downwards direction.

Most bird migrants, nevertheless, do cross the equator twice a year so that what serves for robins is scarcely an adequate means of navigation for other long-distance avian flight. On the other hand, robins were evidently cunningly able to circumvent the problem of field reversals during their evolution.

THE MYSTERY OF THE MISGUIDED PIGEON

Anonymous; *New Scientist*, 60:529, 1973.

As one recent consequence of man's insatiable curiosity, pigeons have been flying around upper New York State encumbered with Cochran-type 145-MHz radio transmitters, glued to their backs with veterinary branding cement and sending back signals to a receiver through directional antennae attached to a truck some 90 miles distant. One or two birds even found themselves pursued diligently by light aircraft. Why? Because although we know how to guide a rocket to the Moon and back, we still do not know what guides a pigeon home. William Keeton, at Cornell University, however, is prepared to go to considerable lengths to find out (Journal of Comparative Physiology, vol.86, p.1). The Sun seems to play a part; and so does the Earth's geomagnetic field; but there is reason to believe that there is some other more mysterious factor determining the setting of the pigeon's private map; and this is what Keeton has been pursuing, not only with radiotransmitters and aircraft, but with an experimental design of considerable subtlety.

The mystery factor manifests itself in an anomaly associated with one particular release site. Normally, when a number of pigeons from the same loft are released from some distant site, the average direction of flight is towards the home loft. But Keeton found that birds released from the Castor Hill Fire Tower, 89 miles north north east of the home loft at Cornell, consistently set out in a direction on average a few degrees clockwise of the home direction. Exactly the same error was seen in birds from other lofts, and it made no difference whether the birds were experienced or inexperienced, or whether the sky was sunny or overcast.

. . . .

MAGNETIC DIRECTION FINDING, EVIDENCE FOR ITS USE IN MIGRATORY INDIGO BUNTINGS

Emlen, Stephen T., et al; *Science,* 193:505-507, 1976.

Abstract. The orientational capabilities of caged migratory indigo buntings were studied under differing magnetic field conditions. When tested in a situation allowing minimal exposure to visual cues but in the presence of the normal geomagnetic field, the birds demonstrated a significant orientation in the appropriate migratory direction (to the north). When the horizontal component of the magnetic field was deflected clockwise 120° by activation of Helmholtz coils surrounding the cage, the orientation of the buntings shifted accordingly (clockwise to geographic east-southeast). These results suggest that indigo buntings are not only able to detect the geomagnetic field, but also can use this information in the finalization of their migratory direction.

MAGNETIC FIELD TURNS BIRDS

Anonymous; *Science News,* 111:153, 1977.

Migrating birds react to low-intensity electromagnetic fields during nocturnal flight, two Rockefeller University biologists report. Ronald P. Larkin and Pamela J. Sutherland used radar to track birds flying 80 to 300 meters above the suspended antennas of the Navy's Project Seafarer at the Wisconsin Test Facility. They found that birds changed direction and altitude more frequently in the presence of the alternating current than when the antennas were off. Direction changes were most frequent when the current of the antennas was changing.

Because the magnetic field created by the antennas is less than

1 percent of the earth's magnetic field, the data indicate "a heretofore unsuspected degree of sophistication in the birds' use of magnetic fields in orientation," the researchers write in the Feb. 25 Science.

. . . .

PIGEONS HAVE MAGNETS

Walcott, Charles, et al; *Science,* 205:1027-1029, 1979.

Abstract, Research on pigeon homing suggests that magnetic field information is used for orientation. The ability of pigeons to sense magnetic fields may be associated with a small, unilateral structure between the brain and the skull which contains magnetite in what appears to be single domains.

FERROMAGNETIC COUPLING TO MUSCLE RECEPTORS AS A BASIS FOR GEOMAGNETIC FIELD SENSITIVITY IN ANIMALS

Presti, David, and Pettigrew, John D.; *Nature,* 285:99-101, 1980.

Abstract. Over the past decade several investigators have provided convincing evidence that the orientation of pigeons and other birds during homing and migrational activities is significantly affected by Earth-strength (<0.5G) magnetic fields. The presumed mediator of such effects would be a highly sensitive magnetoreceptor which the birds would normally use to extract navigational information from the geomagnetic field. The recently reported measurement of magnetic remanence in honeybees and in homing pigeons has stimulated interest in the possibility that the magnetically sensitive structure may be constructed from permanently magnetic material. Here we report the detection of permanently magnetic material in the neck musculature of pigeons (Columba livia) and migratory white-crowned sparrows (Zonotrichia leucophrys). We propose that a magnetic field detector might involve the coupling of magnetic particles to a sensitive muscle receptor such as a spindle. A detection mechanism of this kind could account for the difficulties encountered in conditioning immobile homing pigeons to magnetic field changes and for the puzzling requirement of movement in other behavioural experiments involving pigeons and magnetic fields.

• Unexplained Bird Navigation

STRANGE BUT TRUE

Anonymous; *Nature Magazine*, 33:334, 1940.

This is the story of a homing pigeon that did things in the reverse. It is a story of a happening in Nature that does not conform to accepted ideas. It defies explanation, but it has been carefully investigated and authenticated.

The story starts in the fall of 1939 when Hugh Brady Perkins, twelve-year-old son of Sheriff F.C. Perkins of Summerville, West Virginia, befriended an exhausted homing pigeon bearing a pigeon-racing band. Boy and bird guest became close friends. A few months after this friendship started, Hugh Brady was taken to the hospital in Phillipi, West Virginia, 105 miles from his home. The pigeon, now named Billy, was left at home, and was still there ten days after the boy left.

The scene shifts to the hospital room at Phillipi. The nurse came into Hugh's room one morning and her young patient said that there was a pigeon on the window sill. It had awakened the boy at one o'clock by scratching on the pane. The nurse opened the window. The pigeon was still there, and walked into the room.

"Why, that's my pigeon--Billy," declared the boy.

The nurse suspected delirium. But the boy insisted that she look at the leg band and see whether it did not have 167 on it. It did--AU 39 C&W 167. It was Billy. When Sheriff and Mrs. Perkins came, a couple of days later, they did so with the task of reporting that Billy had gone.

"He's all right," said Hugh Brady, "he's here with me." He pointed to a pasteboard box where Billy had taken up his temporary hospital abode.

. . . .

• Enhanced Sense of Hearing in Birds

THE HEARING OF PHEASANTS

Anonymous; *Science*, 58:sup xii-xiv, November 9, 1923.

Do pheasants and other birds hear sounds which are inaudible

to human ears? That is a question brought up by Dr. Charles Davison, leading seismologist, in a discussion of the distance at which great explosions may be heard. Pheasants during the great war showed evidence many times of being greatly disturbed by air waves resulting from explosions or naval battles which were not heard by human beings, and their behavior has thrown light on the problem of the so-called "zones of silence" around great detonations which beyond these zones are again audible.

These "inaudible sound waves" cause the pheasants to crow, scream and flutter about as if greatly frightened. Such behavior was observed at many points in England at the time of the naval battle of the Dogger Bank on January 24, 1915. At a distance of 216 miles from the action, pheasants "shrieked themselves hoarse" and smaller birds were terrified, although not a sound was heard by human ears. The greatest distance at which the birds were affected was 320 miles. The same effect was produced by the explosion of Zeppelin bombs, the birds reacting to explosions 80 miles away, beyond ordinary human earshot.

In some cases people did hear the noise of the cannonading 200 miles or more away, but if this were on the far side of a silent zone the pheasants were affected a little before any one heard a sound indicating that the inaudible waves traveled a little faster than those which were heard. But if the point of observation were on the near side of a silent zone, the audible waves arrived first. It is suggested that the inaudible waves travel across the silent zones close to the ground while the waves which are heard farther on make a detour upwards across the zone and arrive a little later because of their longer course.

Just what effect these "silent waves" have on pheasants is not known. The theory that they "hear" them is opposed by the fact that the audible waves produce no effect upon the birds. The inaudible waves are of long wave length and set up vibrations in loose articles, and it is thought more likely that the birds are frightened by the quivering of the branches upon which they may be resting.

• Sonar Also Developed by Birds

THE OILBIRD: HEARING AND ECHOLOCATION

Konishi, Masakazu, and Knudsen, Eric I.; *Science,* 204:425-427, 1979.

Abstract. Oilbirds can navigate in total darkness by echolocation. The sound energy in their sonar cries is unevenly dis-

tributed over the range from about 1 to 15 kilohertz, with a dominant frequency range of 1.5 to 2.5 kilohertz. This corresponds to the most sensitive range of their hearing as determined by neurophysiological methods. Behavioral tests in their home cave indicate that the smallest object avoided by them is a disk 20 centimeters in diameter.

CURIOSITIES OF BEHAVIOR

• Collective Action in Birds

SYNCHRONY IN FLOCK WHEELING

Gerard, R. W.; *Science,* 97:160–161, 1943.

The problem of the synchronization of action of cells and organisms has long attracted the interest of biologists. Separate units often respond with such simultaneity or under such other conditions that, in the case of neurones, ordinary conducted nerve impulses seem to be excluded. Similarly with the behavior of organisms---insect flashes, plant blooming, fish wheeling, and the like.

Some time ago an opportunity presented which enabled me to time the wheeling of a small bird flock with considerable precision. As I drove along a Vermont hilltop road at 35 miles per hour, the speed noted by chance just at the right time, a flock of something under half a hundred birds flew parallel to me not over fifteen feet from the car window and at precisely my speed. After a few seconds of this the flock wheeled away---not columns left, but each individual left face. I could not detect the slightest shift of position of one individual relative to the group. Surely none continued forward anything like its own length, perhaps 9 inches, after the others had turned.

Flying at 35 miles per hour, a bird lagging only ten milliseconds behind its fellows would have shot six inches ahead before making the turn. Clearly all the birds swerved simultaneously, within a maximum variation of less than five milliseconds.

What the cue or signal was which initiated the group maneuver, I do not know. No leader moved first and was followed by the

others---I could not have failed to see the sequence. And any optic or even auditory stimulus, with a probable minimal reaction time of at least 100 milliseconds, must have acted with extreme constancy on the separate individuals. Even the Rockettes, elaborately trained to pretimed movements, can not approach such precision.

THE CONTROVERSIAL ROOK

Fitter, R. S. R.; *New Scientist*, 3:18-19, April 10, 1958.

Another rook controversy, on quite a different plane, relates to the so-called "rook parliament." I have on my files nearly a score of eyewitness accounts within the past thirty years of assemblies, or "parliaments," of rooks which ended in the death of one or more birds. Most of them happened some years before they were reported in print, and hardly any of the observers could be called trained naturalists--it is not sufficient that they should be trained observers in some other walk of life. It is impossible to believe either that all these people were mistaken, or that they all saw what some of them thought they had seen--a criminal trial of a rook or rooks by a flock of others.

. . . .

The rook parliament in its classic form, as first related by that indefatigable Victorian populariser, the Rev. J.G. Wood, sounds a most unlikely story. The fortunate witness of the whole scene hears a large concourse of rooks approaching. He reins his horse and awaits their arrival in a large open field nearby. Here the rooks arrange themselves, still cawing vigorously, in a rough circle, while the judges, two or three venerable rooks of evident authority, stand apart within the circle. The prisoner now arrives, in charge of two or more warder rooks, and the trial is noisily conducted, ending usually in his condemnation. Then, on a signal from the presiding judge, the whole concourse of rooks falls on the unhappy prisoner, and when they fly away he is left dead on the field.

Put like this, of course, the phenomenon both sounds and is fantastic. Moreover, I can neither find evidence that Wood himself saw a rook parliament, nor trace the source of his information. His Illustrated Natural History of Birds, published in 1869, seems to contain the first printed reference to the rook parliament. Before that such goings-on among British birds were confined to "craa's courts"--a much more suitable term for the supposed phenomenon, incidentally--among the hooded crows of the Shetlands; and there has been no eyewitness account of a craa's court for well over a century.

. . . .

There is no doubt that vast noisy concourses of rooks do take

place and that the birds often do arrange themselves in rough circles or sometimes in parallel lines. Nor is there any doubt that individual birds are sometimes set upon by some or many of the assembled birds and left dead on the ground. All the dead birds for which I have any information were in poor condition, some of them emaciated or heavily parasitised, and I have little doubt that the phenomenon hitherto attributed to meting out justice to a criminal is in fact of the nature of a mercy killing. The victim must be an old bird which has become a liability to the colony.

. . . .

BATTLE OF RAVENS

Anonymous; *American Naturalist*, 17:897, 1883.

The Frankfurt (Germany) Journal writes: The gardener, Mr. Georgius, from Ginnheim, called at our office today with a chest full of dead ravens, victims of a battle which was fought high in the air among a flock of over four hundred of these birds near the above-mentioned village. The ravens formed together into three detachments, and as if at a given signal flew at each other, and with savage cries seemed as if they would tear each other's eyes out or their heads off with their beaks. The ground was soon covered with the bodies of over fifty birds, which were picked up by observers. Wounds on other parts of the body except the head could not be found. The blows on the head appeared on close observation to have been given with such force that one was sufficient to destroy life. The cause of the battle was doubtless the fact that the pairing season of the birds was near at hand. Not only the males but also the females participated in the fight, as bodies of the latter were found among the slain.

REMARKABLE FLIGHT OF CROWS

Endicott, W. E.; *American Naturalist*, 2:381, 1869.

An account of a remarkable flight of crows I once witnessed may, perhaps, be of interest to some of your readers. The organization of which I was a member, was stationed in March and April, 1863, at Poolsville, Md., on the Upper Potomac, midway, or nearly so, between Washington and Harper's Ferry. One afternoon in April I was posted as sentinel "between the guns," with instructions there to walk until six; it was then four.

Soon after being posted, I saw two or three crows fly over, and soon five or six more, followed by nine or ten more; seeing them so increase I thought to count them, and for half an hour

or so was able to do so with some degree of certainty; after that they formed one continuous stream, flying east by south in perfect silence. After that I could only estimate their number by calculating how many passed a given point in a minute. There was no apparent diminution in their numbers as the time passed on; but the line shifted towards the north, as though they were advancing "in echelon," and when it finally grew dusky, they still presented the appearance of a low black cloud to the northward, their motion visible only when a break occurred in the line. I estimated that their number was eighty thousand up to the time that darkness prevented farther observation. Some weeks after I spoke with Dr. Thayer, Surgeon of the 14th N.H. Volunteers, on the subject, and found his estimate to be--if I remember rightly--ninety-five thousand. The species was the common C. Americanus.

• Fascination by Snakes and Other Animals

THE FASCINATION OF SNAKES

Nash; *American Journal of Science,* 1:12:368-370, 1827.

I have often heard stories about the power that snakes have to charm birds and animals, which, to say the least, I always treated with the coldness of skepticism, nor could I believe them until convinced by ocular demonstration. A case occurred in Williamsburgh, Mass., one mile south of the house of public worship, by the way side, in July last. As I was walking in the road at noon-day, my attention was drawn to the fence by the fluttering and hopping of a robin red-breast, and of a cat-bird, which upon my approach flew up, and perched on a sapling two or three rods distant; at this instant a large black snake reared his head from the ground near the fence. I immediately stepped back a little, and sat down upon an eminence; the snake in a few moments slunk again to the earth, with a calm placid appearance, and the birds soon after returned and lighted upon the ground near the snake first stretching their wings upon the ground, and spreading their tails, they commenced fluttering around the snake, drawing nearer at almost every step, until they stepped near or across the snake, which would often move a little or throw himself into a different posture, apparently to seize his prey, which movements I noticed seemed to frighten the birds, and they would veer off a few feet, but return again as soon as the snake was motionless. All that was wanting for the snake to secure the vic-

tims seemed to be, that the birds should pass near his head, which they would probably have soon done, but at this moment a waggon drove up and stopped. This frightened the snake, and it crawled across the fence into the grass; notwithstanding, the birds flew over the fence into the grass also, and appeared to be bewitched to flutter around their charmer, and it was not until an attempt was made to kill the snake that the birds would avail themselves of their wings and fly to a forest one hundred rods distant.

The movements of the birds while around the snake seemed to be voluntary, and without the least constraint, nor did they utter any distressing cries, or appear enraged, as I often have seen them when squirrels, hawks, and mischievous boys attempted to rob their nests or to catch their young ones; but they seemed to be drawn by some allurement or enticement, (and not by any constraining or provoking power;) indeed, I thoroughly searched all the fences and trees in the vicinity to find some nest or young birds, but could find none.

What this fascinating power is, whether it be the look or effluvium, or the singing by the vibrations of the tail of the snake, or anything else, I will not attempt to determine; possibly this power may be owing to different causes in different kinds of snakes. But so far as the black snake is concerned, it seems to be nothing more than an enticement or allurement with which the snake is endowed to procure his food.

P.S. Since this case occurred, I have heard several respectable people, who have also seen birds charmed, observe that they have heard music occasioned by the vibrations of the snake's tail, which, they being near, could see. That snakes make music thus I know; and also that birds are extremely captivated with music--but whether this is the only means that the snake uses, or whether all kinds of snakes use it, I am not prepared to say.

FASCINATION

Chatel; *Nature,* 22:484, 1880.

I expected some of your readers to refute the explanation of Mr. Stebbing on "Fascination." I see in Nature, vol. xxii, p.383 another paragraph which is not more to the purpose. Want of presence of mind and stupefaction are not fascination. In 1859 (twenty-one years ago) I followed in the rocks of Avon, close by the park of Fontainebleau, the fairy paths of Denecourt, when the approach of a storm induced me to leave the blue arrows, indicating the right path, for a short cut. I soon lost my way and found myself in a maze of brambles and rocks, when I was startled by seeing on my left hand, at a distance of about ten yards, a snake, whose body lifted up from the ground at a height of about a yard, was swinging to and fro. I remained motionless,

hesitating whether to advance or to retreat, but soon perceived that the snake did not mind me, but kept on maintaining its swinging motion, and some plaintive shrieks attracted my attention to a greenfinch perched on a branch of a young pine overhanging the snake, with his feathers ruffled, following by a nod of his head on each side of the branch the motions of the snake. He tottered, spread his wings, alighted on a lower branch, and so on until the last branch was reached. I then flung my stick at the snake, but the point of a rock broke it and the snake disappeared with the rapidity of an arrow. On approaching the spot, a real abode of vipers, which I did with the greatest precaution, knowing by observation that death may be the result of the bite of a viper, I saw the greenfinch on the ground agitated by convulsive and spasmodic motion, opening and shutting his eyes. I put him in my bosom to try the effect of heat, and hastened to reach the park of Fontainebleau, The little claws of the bird opening and shutting, perhaps as an effect of heat, made me think that he might perhaps be able to stand on my finger, and he did clutch it and held on with spasmodic squeezes. In the park I got some water, and made him drink it. In short, he revived and finally flew off in the lime-trees of the park.

Now whilst following the motions of the snake and bird I experienced a singular sensation. I felt giddy; a squeezing like an iron hoop pressed in my temples, and the ground seemed to me to be heaving up and down. In fact the sensation was quite analogous to that experienced on a beginning of sea-sickness.

From these facts would it not seem probable that fascination is nothing more nor less than an extreme fatigue of the optic nerve, produced by a rapid gyratory motion of a shining object and resulting in a nervous attack and a coma? Curiosity rivets at first the attention of the bird, unconscious of any danger, and when giddiness warns him of his peril it is too late. The snake is as well aware of this as the Lophius piscatorius is of the effect of his membrane.

In this system the fact of the bird coming down from a higher to a lower branch would be explained by the supposition that, giddiness overtaking him, he opened instinctively his wings and clung to the next support that he found, the motion having partially removed the giddiness so as to enable him to hold fast.

Observe, that nothing hindered the bird from flying away, and that the snake being at most five feet long, could never have reached even the lowest branch.

Besides he could have no nest to protect, for in the rocks of Avon there is no water save rain-water in the hollows of the rocks, and this is not potable on account of microscopic leeches which people it, the instinct of birds teaching them to avoid it.

"FASCINATION" OF BIRDS BY A SNAKE

Poulton, Edward B.; *Nature*, 100:244, 1917.

I have just received the following record of an observation made on September 19 by Capt. G.D.H. Carpenter, at Itigi, about 150 miles east of Tabora, on the Central Railway of late German East Africa. Capt. Carpenter's account recalls the behaviour of small birds to a cuckoo or an owl, and suggests that they were "mobbing" an enemy rather than fascinated by it. The observation may supply the clue to the interpretation of all cases of supposed "fascination" by snakes.

"Yesterday afternoon I witnessed what I have always found difficult to believe, namely, the strange 'fascination' of birds by a snake. I came upon a party of very pretty little finches hopping about among thick dead twigs of a fallen branch on the ground. I came on them quite suddenly from round another bush, and stopped dead when I saw them to watch them. Though I was within a couple of yards they did not fly away, but continued to hop about, all gradually coming closer and uttering faint chirps, I thought I had never seen such tame birds, and admired their beauty. While looking at the birds I quite missed an Elapine snake, which suddenly attracted my attention by striking at a hen finch just in front of me! It fluttered back a foot or two, and the snake got a mouthful of feathers among its teeth, which seemed to incommode it, for it went down among the thin grass at the foot of the clump of twigs, where I could still see it. The birds none of them made any attempt to get away, but actually several of them, including the one already struck at, hopped further down to get another look at the snake! The latter bird did show some signs of agitation, as every now and then she spread out her tail fanwise and kept on chirping but still went nearer. However, after a bit the birds flew away one by one, without any excitement, and I crept up and found the snake had gone. I wished I had seen the snake before it struck, to see which way its head was pointing. Of course, I do not believe in the mesmeric theory, but it was not a question of a snake pursuing a victim which was too frightened to run away."

FASCINATION

Ochsenius, Carl; *Nature*, 23:508-509, 1881.

In the interior or the province Valdivia, South Chili, a species of wood-snipe (Paipayen inc.) is often caught by the natives in the following manner:---When the bird flies into one of the low bushes, which in spots of about three to six metres diameter are found frequently in the wood meadows there, two men on horseback go round it in the same direction, swinging their lazos over the bush. After ten or more rounds one man slips down from his

horse, whilst the other continues, leading his companion's horse behind. Carefully then the first man creeps on to the point, where the paipayen is sitting nearly motionless or stupefied with the rider's circular movements, and kills it by a quick blow of a stick.

When I first was told so I would not believe it; but in 1853 or 1854 I took part myself in this kind of capture in the hacienda San Juan, in Valdivia, belonging to my chief, Dr. Philippi, now professor in the University and director of the museum in Santiago. I had left the house without gun, accompanied by a native servant, when, in a part of the wood called Quemas, I observed a paipayen falling into a dense but low bush of the above-mentioned kind. Desiring to obtain a good specimen of this not very common bird for our collection, I expressed my regret at not having the gun, but the servant replied: "never mind, if you wish, we will get the bird, And he caught it with my assistance in the above way without injuring it.

• Tumbling in Flight

"TUMBLING" IN A WILD MOURNING DOVE

Metcalf, Maynard M.; *Science,* 61:444, 1925.

Thirty years ago my wife and I observed "tumbling" in a wild mourning dove. A description of the observation was sent to Dr. C.O. Whitman and its receipt acknowledged by him, but, so far as I know, he never made use of the data or made these known. I will give the data here from memory.

We were driving north over flat open country. Mourning doves were flying from the northwest to a "pigeon roost" southeast of us, in a dense apple orchard which had grown up from an abandoned nursery. The tumbling bird was first noticed a sixth of a mile in front of us and to the left of the road. I thought the bird had been shot and hit and I waited for the report of the gun, but none came. The bird balked, fell over backward fluttering, dropped say fifteen feet, recovered and flew on. When no gun report was heard we wondered if the bird might have struck a wire, but saw none in the field where the bird had been. The bird crossed the road an eighth of a mile in front of us and flew diagonally past us at a distance of about a tenth of a mile and disappeared behind and to the right of us. Twice more it tumbled in full view, though at distances of one tenth and one eighth of a mile. Each time the behavior was the same--balking, a fluttering backward fall, recovery while still

well above the ground and a renewal of flight. I had not at that time seen tumbler pigeons in flight. When later I did see them their behavior impressed me as similar to that of the wild dove.

• Anting with Fire

A POSSIBLE EXPLANATION OF THE PHOENIX MYTH

Burton, Maurice; *New Scientist,* 1:10–12, June 27, 1957.

It is only within comparitively recent years that bird anting has been accepted by the ornithologists, and therein lies what I consider another very remarkable story. Before reaching this, however, let us take a closer look at the procedure we call bird-anting. Under certain circumstances, a bird will pick up an ant in its pair of forceps, go into a very special posture. The bird brings its endeavoring to protect something in front of it. At the same time the tail is thrown to one side and slightly drawn under the hinder end of the body. Holding this posture, the bird lowers its head to bring the beak under one wing and appears to rub the ant rapidly up and down the bases of the primary feathers.

If the anting posture is fantastic, it is also widespread. It has been positively recorded for over fifty species, and careful observation coupled with deliberate experiment may yet reveal that it is more common than this figure would suggest; for the process is most erratic in occurrence. An aviary bird that ants readily one day may fail to do so, in the presence of all the appropriate stimuli, the next day. It may go for weeks or months stubbornly refusing to perform, and then for a spell ant repeatedly day after day at the slightest provocation.

. . . .

And if we trace the references to anting and its related phenomena back into the historic literature, we gather material enough for a dozen romantic stories, all relating to birds playing with fire. To show how closely this is linked with anting, let me detail the idiosyncrasies of one of my tame rooks.

This rook, while with its former owner, Diana Ross, the novelist, took to opening boxes of matches of the non- "safety" kind. He quickly learned that by holding a match in his toes and pecking at its red head he could cause it to burst into flame. The moment this happens Corbie, as the rook is known, picks the match up in his beak, goes into a magnificent anting posture and rubs the lighted match up and down the inside of his arched wings. In the anting position a rook's spread plumage takes on

"Anting" with a lighted match

a bizarre but altogether magnificent appearance. I have demonstrated Corbie's abilities dozens of times to people visiting my aviaries. The only time it fails is when Corbie is courting or frightened.

This, however, is not the end of Corbie's tricks. He will ant in front of a fire in an open grate or fly up into the smoke from it, going into the anting posture in mid-air. He has also anted in front of an electric fire and even learned--a trick entirely self-taught--to turn the fire on by depressing the switch in the skirting, an interesting instance of a bird recognising cause and effect. He has a fondness for the hot water from a tap in the kitchen, and will try to turn the tap by pecking at it with his beak. Corbie ants with lighted cigarette-ends. When, purely as an experiment, I lighted a small heap of straw in his aviary he carried away the flowing embers. He also ants with ants--and so does a pet jay, who performs better, however, with hot cigarette-ends and matches, but has not yet learned to strike the latter.

• A New Skill: Milk Bottle Opening

OPENING OF MILK BOTTLES BY BIRDS

Hawkins, T. H.; *Nature,* 165:435-436, 1950.

In 1921 birds described as tits were observed to prise open the wax-board tops of milk bottles on the doorsteps in Swaythling, near Stoneham, Southampton, and drink the milk. This is the first known record of an act which has now become a widespread habit in many parts of England and some parts of Wales, Scotland and Ireland, and which has to date been practised by at least eleven species of birds.

The spread of the habit is interesting, because of the problems of behaviour involved. How far did the individual birds learn the habit from each other, or invent it for themselves? If most of them learnt it, by what process did they do so? How did, and how do, they detect the presence of food inside the bottle?

These are the questions which James Fisher and R.A. Hinde pose in a recent article in British Birds (42, No.11; November 1949), and although the writers agree that proper answers to the questions can be obtained only from carefully controlled experiments on birds of known history, it is submitted that much useful information can be derived from the collection of facts about the spread of the habit from members of ornithological societies and from the general public.

Some four hundred records have already been obtained about bottle-opening by species of tits while to a lesser extent observations have also been made with house-sparrows, blackbirds, starlings, robins, chaffinches and hedge-sparrows.

Occurrence of the habit is naturally limited primarily by whether or not milk is distributed in bottles in the district. There is, however, good evidence in many cases that the habit may not develop in a district for several years after the use of bottles has become almost universal there--this is known to have been the case in at least twenty-three out of thirty districts where the year in which milk bottles were actually introduced is more or less accurately known.

Most British tits, and certainly the British species known to open milk bottles, namely, the great tit (Parus major), the blue tit (P. coeruleus) and the coal tit (P. ater), are resident and do not normally move, even in winter, more than a few miles from their breeding place. It would seem, therefore, that new centres and records more than fifteen miles distant from any place where the habit has been recorded previously probably represent new 'discoveries' of the habit by individual birds. The distribution of the records is consistent with the view that the new source of food was actually discovered de novo by only a small proportion of a local tit population and then passed on in some way to other

individuals. In England and Wales, it seems likely that the habit has arisen de novo at least once per vice-county and may have arisen more often than this. It might be argued that the pattern of distribution of the observations reflects simply the view of the observers. In densely populated areas there are, of course, more observers; but there are, similarly, more milk bottles. Further, there are many densely populated areas from which no record of the habit has yet been received or from which negative records have been received. Moreover the time interval between the introduction of milk bottles and the first occurrence of the habit makes this argument unsound. The evidence that the area in which the habit occurred, as well as the actual number of records, increased more rapidly each year, is enough to support the view that when the habit has been acquired by one tit it can then be spread through the population by some form of imitation or learning.

Many observations have been made about the actual method of opening the bottles. Although the habit occurs throughout the year, it is more prevalent during the winter months than in the summer. This may be due to the increased need which the birds have for fats during severe weather; but many observers record that tits are much more common in winter in urban areas than they are in summer, and this is of importance. The bottles are usually attacked within a few minutes of being left at the door. There are some reports of parties of tits following the milkman's cart down the street and removing tops from bottles in the cart while the milkman is delivering milk to the houses. The method of opening the bottles varies greatly. When the milk bottle is closed by a cap of metal foil the bird usually first punctures the cap by hammering with its beak and then tears off the metal in thin strips. Sometimes the whole cap is removed and sometimes only a small hole is made in it. Cardboard caps may be treated in a variety of ways. The whole top may be removed, or only the press-in centre, or the cardboard may be torn off layer by layer until it is thin enough for a small hole to be made in it; the milk may be taken through this hole or the bird may insert its beak in the hole and flick off the remainder of the top. The records show that several different methods may be used in any one district, and that more than one method may be employed by one individual.

In many areas bottles containing milks of different grades are distinguished by having caps of different colours. No less than fourteen observers who had milk of more than one type delivered at the house reported that the tits attacked only bottles of one type, and four others reported an almost invariable preference for one type.

Without experimental evidence it is impossible to decide which senses are of use to the bird in indicating the presence of food. Several correspondents have found that bottles filled with water or even empty bottles are still attacked; but this conveys nothing if the previous history of the birds is not known.

To what extent it would be correct to refer to the behaviour of those individuals which 'invented' the habit for themselves as

'insight learning' is a problem which cannot be resolved with the present inadequate data.

• Use of Tools by Birds

A REMARKABLE CASE OF TOOL-USING IN A BIRD

Montagu, Ashley; *American Anthropologist*, 72:610, 1970.

To the growing list of tool-users among animals other than man should be added the Northern Territory kitehawk or, as he is called among the aborigines of that part of Australia the firehawk. In the fascinating book about his life, I, The Aboriginal (Adelaide: Griffin, 1962), written down by Douglas Lockwood, Waipuldanya, an aboriginal of the Alawa tribe at Roper River, says, "I have seen a hawk pick up a smouldering stick in its claws and drop it in a fresh patch of dry grass half a mile away, then wait with its mates for the mad exodus of scorched and frightened rodents and reptiles. When that area was burnt out the process was repeated elsewhere. We call these fires Jaluran" (p.93).

Is this, possibly, the first recorded case of the use of fire by a nonhuman animal?

• Wire Nests

WIRE NESTS OF CROWS

Warren, Ernest; *Nature*, 132:29-30, 1933.

The nest illustrated in Fig. 1 [not reproduced] was removed from one of the ends of the top horizontal framework of an electricity transmission tower near Colenso, Natal, in April, 1933. These towers are some thirty feet in height. The nest proper, consisting of branches, twigs and dried grass, was built in a stout wire basket, some twenty-three in. in diameter. The crows (pied crow, Corvus scapulatus, Daud.) picked up odd scraps of

wire to form the basket, and they bent some of the pieces round the 2-in. angled iron of the tower in such a manner as to fix the nest very securely. The kinds of wire so used were: No.8 hard-drawn copper; Nos. 8 and 6 galvanised iron; No. 14½ baling; No. 14 2-strand barb-wire. The total weight of the nest is 20 lb.

It is especially to be noted that wire is about the only thing which could have been used to attach the nest firmly to the iron framework of the tower, and the birds had the wonderful intelligence to utilise this artificial material for the purpose. The nest was, in fact, so securely attached to the iron bars of the tower that the greatest difficulty was experienced in removing it. Also, the mere manipulation of such heavy gauge wire by a bird is a surprising feat of strength.

If nest-building is the non-intelligent and purely instinctive action that some regard it, how is it that these crows definitely met a difficulty by utilising wire, since twigs and branches by themselves would have been insufficient to wedge the nest securely on the narrow and open framework of the tower, and an artificial material like wire could not lie within the purview of instinctive action?

. . . .

• Foresight?

FORESIGHT OF BIRDS

Anonymous; *Zoologist*, 2:10:4536, 1875.

The foresight of birds of a coming epidemic of cholera has been occupying the attention of a German journal, the 'Jardin Zoologique.' The journal notes that a few days previous to the terrible ravages of cholera in Gallicia in 1872, all the sparrows suddenly quitted the town of Przemysl, and not a single bird returned until the end of November, when the disease had entirely disappeared. The same circumstance was remarked in Munich and in Nuremberg. During the attacks of cholera at St. Petersburg and Riga in 1848, in Western Prussia in 1849, and in Hanover in 1850, every swallow and sparrow forsook the town and remained absent until the eradication of the scourge. Might not such a coincidence serve as a warning and give time for measures to be taken against the disease? Sanitary Boards should take note of this.

• Imitative Powers of Birds

ANOTHER TALKING CANARY

Newman, Edward; *Zoologist,* 16:6240-6241, 1858.

Mr. Sotheby relates, in the 'Zoological Proceedings,' some particulars of a canary whose conversational powers made an approach to those of the little bird which, a few years ago, proved so attractive an exhibition in Regent Street. This bird was the only one hatched out of a nest of four eggs, and was forsaken immediately on escaping from the egg-shell; it was found nearly dead in the nest, placed by the fire in flannel, eventually restored to life and health, and brought up by hand. Separated completely from all other birds it became familiar with those who fed it. Its singing notes were totally different from those of a canary: at three months old it repeated the words "Kissie, kissie," and imitated the sound of kissing; and afterwards for hours together repeated "Dear, sweet Titchie; Kiss Minnie; Kiss me then dear Minnie; Sweet, pretty little Titchie," & c.: it also whistles the first bar of "God save the Queen," and imitates the sound of a dog-whistle occasionally used about the house. The object of my quoting these particulars is to throw an additional ray of light on that highly interesting subject, the natural source of instruction in songbirds: the facts recorded by Mr. Sotheby seem to show that song in birds, as in human beings, is entirely educational.

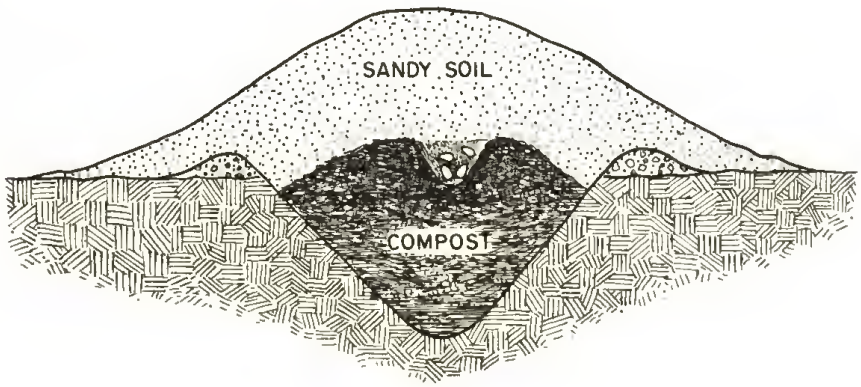
• Incubator and Bower Birds

THE INCUBATOR BIRD

Anonymous; *Popular Science Monthly,* 48:858-859, 1896.

An account in the Geographical Journal of a trip to the little island of Niuafou, in the South Pacific, describes a native bird called the malau, which is apparently of the same order as the Megapodes; it makes no nests, but buries its eggs in the soft, hot sand of this volcanic island. It is about the size of a small domestic fowl. Its eggs are large and of a dull reddish color. The strange nesting habit and the large size of its eggs recall

that curious bird the jungle fowl of Australia (Megapodius tumulus). Great numbers of high and large mounds exist in some parts of Australia, which were for a long time thought to be the tombs of departed natives; the natives, however, disclaimed the sepulchral character, saying that they were artificial ovens in which the eggs of the jungle fowl were laid, and which, by the heat that is always disengaged from decaying vegetable substances, preserved sufficient warmth to hatch the eggs. The size of some of these mounds is quite marvelous. One which was measured proved to be fifteen feet high and sixty feet in circumference.



Typical nest of an incubator bird

The whole of this enormous mass was made by the jungle fowl. If the hand be inserted into the interior of the heap it will always be found quite hot. In almost every case the mound is placed under the shelter of densely leaved trees. This precaution is probably taken to prevent the rays of the sun from evaporating the moisture. The bird seems to deposit its eggs by digging holes from the top of the mound, laying the egg at the bottom, and then making its way out again, throwing back the earth it had scooped away. The holes are not dug perpendicularly, so that although they are six or seven feet in length they may be only two or three feet from the surface. The leipoa, or native pheasant of Australia, like the preceding, lays its eggs in a mound of earth and leaves, but the mound is not nearly so large. Another bird having this curious nesting habit is the brush turkey of New South Wales. In the Guide to the Gardens of the Zoological Society of London an interesting account of the construction of the mound by some captive birds is to be found. "On being removed into an inclosure with an abundance of vegetable material within reach, the male begins to throw it up into a heap behind him by a

scratching kind of motion of his powerful feet, which projects each footful as he grasps it for a considerable distance in the rear. As he always begins to work at the outer margin of the inclosure, the material is thrown inward in concentric circles until sufficiently near the spot selected for the mound to be jerked upon it. As soon as the mound is risen to a height of about four feet, both birds work in reducing it to an even surface, and then begin to excavate a depression in the center. In this in due time the eggs are deposited as they are laid and arranged in a circle about fifteen inches below the summit of the mound at regular intervals with the smaller end of the egg pointing downward. The male bird watches the temperature of the mound very carefully; the eggs are generally covered, a cylindrical opening being always maintained in the center of the circle for the purpose of giving air to them, and probably to prevent the danger of a sudden increase of heat from the action of the sun or accelerated fermentation in the mound itself. In hot days the eggs are nearly uncovered two or three times between morning and evening. On the young bird chipping out of the egg it remains in the mound for at least twelve hours without making any effort to emerge from it, being at that time almost as deeply covered up by the male as the rest of the eggs. On the second day it comes out. Early in the afternoon it retires to the mound again and is partially covered up for the night by the assiduous father. On the third day the nestling is capable of strong flight."

WE ARE BEGINNING TO UNDERSTAND THE BOWER BIRDS

Marshall, A. J.; *Animal Life,* 58:34-43, 1955.

Miles from the homestead of an inland Australian sheep station (ranch), a man returned to his automobile and found its ignition key missing. It seemed that he was stranded. He knew that there could be no practical joker in the neighborhood. So, familiar with the habits of the Spotted Bower-bird (Chlamydera maculata), he walked half a mile to a bower whose location he knew. The shiny key and ring were there on the display ground.

Another bushman used to leave his glass eye in a cup of water at his bedside every night. In camp he awoke one morning to find the cup upset and the eye gone. The bushman was very attached to this particular eye and took a low view of its disappearance. The camp dog was suspected and had a poor time of it for the next few days. An autopsy was projected, but fortunately postponed. A fortnight passed. Then one of the bushmen, short of lead with which to make bullets to shoot a troublesome brumbly stallion (wild horse) went to a nearby bower-bird's bower hoping to find some. He found not only the desired lead, but, in addition, his mate's glass eye. It stared vacantly up from among the bleached bones, shells and odd pieces of shining metal and glass that the birds had accumulated for use in their display.

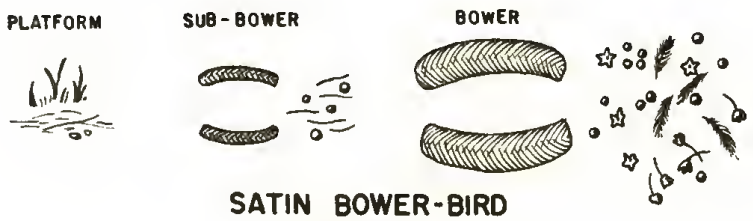
In the Ord River country of West Australia a gentleman named Edward Delaney lost his spectacles. They were found at the bower of a Great Gray Bower-bird (Chlamydera nuchalis). The collector S. W. Jackson had to retrieve stolen table silver and his entomological forceps from a bower. He reported also that "anything bright" in his camps had to be kept shut up in a box. Precious opal has been found on display ground in opal-bearing country as well as small glistening nuggets of gold embedded in pieces of quartz. Bower-birds have been known to steal jewelry from dressing tables near open windows. After country race meetings Australian aborigines sometimes make a tour of nearby bowers to collect odd silver coins found and taken there by the scavenging birds.

Another writer reported that quantities of metal staples were stolen from along a line of newly sunken post holes. Nuts, bolts, thimbles, screws, penknives, wire, broken glass (especially) have been found at bowers. In the old days, when unpainted metal buttons were worn, bower-birds sometimes hopped close to bushmen in order to inspect them.

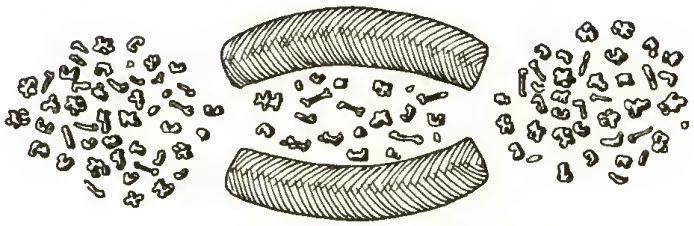
These stories, of course, seem wildly improbable --- but they are, in fact, true. They all relate to the Spotted and Great Gray Bower-birds. These species inhabit the dry inland, and lightly forested northern areas of Australia. They habitually accumulate at their bowers great collections of bleached and reflecting articles.

Most people believe that bower-birds simply accumulate indiscriminately a heap of varied, colorful rubbish. In actual fact, the different species select their bower decorations with a remarkable discrimination. The Satin Bower-bird (Ptilonorhynchus violaceus) of the eastern Australian rain forests and their fringes collects only objects colored blue, greenish-yellow, gray and brown. At one country hotel I know, Satin-birds regularly invade the laundry and purloin washing bluebags. A display ground nearby was always smothered with bluebags, together with the usual array of freshly picked bluebells, greenish-yellow Billardiera blossoms, pieces of gray sloughed snakeskin, brown snail-shells and other objects. Flower growers in districts where Satin-birds live usually have to shoot them if they are determined to keep them out of their gardens. The birds become so bold that they will wrestle with "spikes" of delphinium blossoms larger than themselves within a few yards of the aggrieved gardeners. They like also petunias, cinerarias, cornflowers, irises and hyacinths. These are quickly wrenched off and carried to bowers in the rain forest to join the odd blue and greenish-yellow bush flowers and the motley accumulation of blue parrots' feathers, bus tickets, glass, fragments of blue-bordered china, discarded hair ribbons and other miscellanea there. An aviculturalist who unwisely tried to keep blue finches in the same aviary as a male Satin-bird found that the finches were killed one by one and taken as decorations to the display ground. Finches of other colors were not molested.

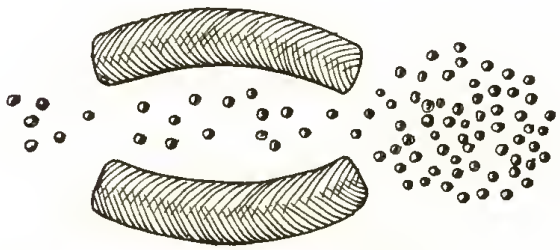
Bower-birds (Ptilonorhynchidae) are passerine (perching) birds not unrelated to the birds of paradise. They exist only in Australia and New Guinea. The males are often very beautiful. Their bowers are built of twigs, always on the ground, and have



SATIN BOWER-BIRD
Ptilonorhynchus



SPOTTED BOWER-BIRD
Chlamydera maculata



FAWN-BREASTED BOWER-BIRD
Chlamydera cerviniventris

Some bower bird "decorations"

no direct connection with the nests. These are placed in trees, often some hundreds of yards away. Early Australian explorers who first observed bower-birds' display grounds thought that they were a kind of toy built by native mothers for the amusement of their children. Very soon, however, it became known that birds, not aborigines, made them. The first bowers found were of the avenue type --- a pair of symmetrical walls of twigs with just enough room in between for a single bird about as big as a small pigeon to pass through. At one, or sometimes both, ends

of the central avenue is always arrayed the pile of distinctive display things. Some species scatter their decorations through the avenue as well. One avenue-builder, the Yellow-breasted Bowerbird (Chlamydera lauterbachi) of New Guinea, has extended the simple double-wall plan by adding two more walls at right angles and so constructs three avenues. In all of these passages it strews blue, green and red berries, pieces of blue clay and slate-colored pebbles.

Apart from the avenue-builders there exists a group that I have called the maypole-builders, about which very little is known. With one exception all are confined to the dense rain forest of tropical New Guinea. In each case the fundamental features of the bower are a rigid supporting central tower (a thin sapling growing in the forest) and a cone of fabric that is packed about its base. The tower sometimes supports a spherical (as built by Amblyornis inoratus), or a hemispherical (A. subalaris) waterproof hut or a secondary tower of fabric (A. macgregorii). These maypole bowers are sometimes fronted with a living lawn of transplanted moss and this display ground may be decorated with fresh flowers, fruit, fungi, colored beetles' wingcases or snail-shells. Thus the maypole-builders have come to be known as the Gardener Bower-birds. The Golden Queensland Gardener (Prionodura newtoniana) decorates the spacious fabric of its double-towered maypole bower with living orchid plants. These continue to grow and flower on the new site. Very little is known about the maypole-builders because all inhabit tropical jungles remote from heavy European settlement.

. . . . .

At least two of the avenue-builders are known to orientate their bowers in a special way. Thus, both the Satin-bird and the Great Gray Bower bird point the avenue along the path of the sun. Other avenue-builders (e.g. Spotted Bower-bird) site it in any direction. So faithful is the Satin-bird to the north-south direction that when I experimentally altered the bearing of bowers the owner demolished one wall, altered the bearing of the other, and then built a new third wall from the twigs taken from the first. Soon it was pointing almost along the original line.

All three avenue-builders that are at all well known have been found to paint the inner walls of their bowers. One of the first two recorded observations of this habit was made in the New York Zoological Park. In 1922 Lee S. Crandall was told of a blue male Satin-bird that nibbled particles of soft wood from a sieve carried by a keeper. It mixed the wood fragments with saliva and coated the inside walls of its bower with the resultant paste. Mr. Crandall placed a piece of dry rotten wood in the cage. The paint was now on the house, so to speak, and "an orgy of plastering followed." A year or two before this, Nubling had seen bush birds use what was probably fruit pulp and several years later Gannon and several other Sydney naturalists saw birds crush in their beaks charcoal (from logs charred by bush fires) and then wipe a sticky black mixture on the bowers. In the bush the Satin-bird

often shapes with its beak a flat pellet of bark. This it holds towards the end of its bill while applying the paint. The manufactured tool acts as a combination sponge and stopper. As the bird wipes each twig the black suspension adheres and results in an embellishing black band on each side of the avenue.

. . . . .

• Remarkable Mutual Adaptations

COEVOLUTION OF CALVARIA AND THE DODO

May, Robert M.; *Nature*, 270:204–205, 1977.

Stanley Temple has drawn attention to the fact that the large monoecious tree Calvaria major, which according to historical records was once abundant and exploited for lumber on Mauritius, also seems to have undergone a trauma dating back to the dodo's demise. There were by 1973 only 13 Calvaria major trees surviving in the remnant native forests of the island and all 13 were estimated to be more than 300 years old. No younger specimens are known to exist, even though the surviving trees produce well-formed, apparently fertile seeds each year. None of these seeds now germinate naturally, even if planted in nursery conditions.

Temple notes that, although there are several examples where the elimination of a plant species has been deleterious for an associated animal species, the Celvaria-dodo system may be the first documented instance where disappearance of the animal partner has doomed a mutualistic plant species. The mechanism suggested by Temple is that the seed inside the Calvaria fruit developed a coat (endocarp) thick and strong enough to resist crushing in the powerful gizzard of the dodo. As a result, 'these specialised, thick-walled pits could withstand ingestion by dodos, but the seeds within were unable to germinate' without the coat first being battered and abraded by the stones in the dodo's gizzard.

In support of this hypothesis. Temple observes that the Calvaria fruits are today consumed by some frugivorous animals, notably a parakeet and a flying fox, but that none is large enough to eat the fruit whole, and thus disperse the seed. The dodo was large enough, and therefore it was presumably the customer for whom the tree designed its fruit. There is fossil evidence that dodos ate Calvaria fruit. Furthermore, measurements of the crushing forces generated by the stones in the gizzards of granivorous birds from several taxonomic groups

(ranging in body weight from 0.8 to 3.2 kg) give a formula which, when applied to the 12-kg dodo, suggest crushing forces of around 10 kg m in its gizzard. Calvaria pits are strong enough to withstand these forces, but only just: Temple suggests that once the pit had been sufficiently abraded to reduce its size by 30%, the seed would be crushed. But by then some of the seeds would have passed through the dodo, albeit after a battering which would weaken the endocarp and facilitate germination of the infant plant within.

As an experimental test of his ideas, Temple force-fed Calvaria seeds to turkeys, whose gizzards generate crushing forces around 4,000 kg m, fairly close to those inferred for dodos. Of 17 seeds, seven were crushed. The remaining 10 were either regurgitated or defaecated, some after up to 6 days. These 10 seeds were planted in nursery conditions, and three subsequently germinated. Temple concludes that 'these may well have been the first Calvaria seeds to germinate in more than 300 years.'

ANOMALOUS DISTRIBUTION IN TIME AND SPACE

• Late Survival of the Moa

NOTE ON THE POSSIBLE EXISTENCE OF A HUGE BIRD ALLIED TO THE MOA

Newman, Edward; *Zoologist*, 20:7847, 1862.

The following extract from the 'Nelson Examiner,' of June 12, 1861, will afford ground for much speculation on the possible existence of one of those gigantic birds of the southern hemisphere which all authorities have agreed in supposing to be extinct:--- "About three weeks ago, while Mr. Brunner, chief surveyor of the province, and Mr. Maling, of the Survey Department, accompanied by a native, were engaged in surveying on the ranges between the Riwaka and Takaka valleys, they observed, one morning, on going to their work, the footprints of what appeared to be a large bird, whose tracks they followed for a short distance, but lost them at length among rocks and scrub. The size of the foot-

prints, which were well defined wherever the ground was soft, was fourteen inches in length, with a spread of eleven inches at the points of the three toes. The footprints were about thirty inches apart. On examining the bones of the foot of a moa in the Museum, we find the toe to measure, without integuments, eight inches and a half, and these evidently form part of a skeleton of a very large bird: the length of the impression of the toe of the bird in question was ten inches. The native who was in company with Messrs. Brunner and Maling was utterly at a loss to conjecture what bird could have made such a footprint, as he had never seen anything of the kind before. On a subsequent morning similar marks were again seen, and, as a proof that they had been made during the night, it was observed that some of them covered the footprints of those the party made the preceding evening. The size of these footprints, and the great stride of the supposed bird, has led to a belief that a solitary moa may yet be in existence. The district is full of limestone caves of the same character as those in which such a quantity of moa bones were found, about two years ago, in the neighbouring district of Aorere. We believe it is the intention of the Government to take steps to ascertain the character of this gigantic bird, whether moa or not, which keeps watch in these solitudes." A tale has found some currency that huge apes have been seen in the interior of Australia, but this seems very apocryphal.

REPORTED DISCOVERY OF THE MOA

Webber, Hulton J., et al; *Zoologist*, 21:8558–8560, 1863.

Explorers in New Zealand report that they have found traces of the gigantic bird called by the natives the "moa," which induce them to think it is not extinct. The bones found in the earlier days of the colony, though not very recent, were not fossil. Some, indeed, which I have seen had cartilage about them which Papin's digester would have been capable of converting into soup. The bones found by the last explorers were much more recent, and were on the surface; and it is said that they found footmarks which they believe to have been made by some large bird very recently. I see no reason to believe that the moa does not exist, and I think the probability is that it does. The Middle Island was never very thickly peopled, and it is nearly thirty years since it was almost depopulated by Te Ranperaha's tribe. The natives were confined to the eastern side of the island, and never penetrated to the mountains on the east coast. Their superstitions militated against it. They believed that a race of wild men inhabited the mountains, which were also infested by the dreaded taniwa, a great lizard which ate men. They also had a tradition that the moa still lived in the ranges. Since the Europeans have inhabited New Zealand no lizards have been seen larger than about eighteen inches, and certainly no wild men have

been met with by the explorers; so that the superstition of the natives only proves their ignorance. The bones discovered during the last twenty years prove that the moa lived at no very distant day. Why should he be extinct? We know of no enemy likely to exterminate him; and if the untrodden wastes of the Middle Island furnished him with food at a period not distant enough to fossilize his bones, we know of no change which has altered the condition of the islands on that score up to the present day. There are birds which could not so easily preserve their existence, and which have not as yet become extinct. The kiwi-kiwi (apteryx) for instance, and the kaka-po (night parrot), which is also wingless. Both these birds are small, and have numerous enemies, especially native dogs, which would be powerless against the moa. It is, therefore, by no means impossible---I even think it probable---that Professor Owen may yet be gratified by a recent specimen of this gigantic bird.

REPORTED DISCOVERY OF LIVING MOAS

Anonymous; *Popular Science Monthly,* 6:765, 1875.

A report is published in an Auckland newspaper, of October 3d, of the finding of two live moas at Browning Pass, New Zealand. The story runs that one R.K.M. Smyth, on September 26th, while hunting, saw his dog set off suddenly at a great pace, barking furiously. He followed, and soon saw two large birds, one of gigantic height, the other smaller. Seeing the dog getting the worst of the fight, Smyth ran back and called his mate to assist him. They got a leather rope, and, under shelter of a small patch of bush, got behind the larger bird and roped it at the first cast. He then took a turn round a birch tree with the rope. The large bird did not show fight to any great extent, and the smaller one remained quietly by it. After this they had very little trouble to secure the legs of the large bird, and they left it fastened to the tree two days, the young one making no effort to leave its mother. With the assistance of some shepherds the old bird was taken to the camp, the young one following. The old bird is eight feet high, and the young one five feet. The story needs confirmation; it is almost too good to be true.

• Passenger Pigeon Sighting

PASSENGER PIGEON

Anonymous; *Science News Letter,* 17:136, 1930.

The passenger pigeon, long believed to be totally extinct, has been reported alive in the wilds of northern Michigan.

Prof. Philip Hadley, bacteriologist at the University of Michigan, has notified the journal, Science, that while hunting in an uninhabited wilderness in the northern peninsula of Michigan, his companion, familiar with the country, pointed out a bird which he declared to be a passenger pigeon, such as he had seen in vast numbers in his younger days. As Prof. Hadley looked, the bird took flight, and all he could see was its pigeon-like form and its pointed tail.

. . . .

• Land Birds in Mid-Ocean

LAND-BIRDS IN MID-OCEAN

Grim, George W.; *Popular Science Monthly,* 22:207–209, 1882.

The appearance of some of the smaller varieties of migratory birds, such as sparrows, swallows, doves, etc., several hundred miles away from the nearest land is by no means an unusual occurrence on the ocean. About these little erratic visitors there are some curious and interesting facts. Their appearance is almost always one at a time, though I have known a considerable number, representing, perhaps, as many different varieties, to accumulate in the course of a day. It is usually, though not always, in stormy or unsettled weather.

The first curious fact about these birds is, that they never appear to be tired out; whereas birds are often met with near the land with their strength quite exhausted. A second curious fact about them is their preternatural tameness where there is no cat or dog on board, and the crew show no disposition to molest them, as exhibited by their apparently seeking rather than avoiding the presence of man.

Another curious fact about them is the recovery of all their

native wildness and their instinctive avoidance of man's presence on approaching the land. The first time I noticed this fact was with a pair of olive-colored ring-doves, which, from their remarkable tameness and familiarity, I was led to believe had been bred in a domestic state and perhaps on shipboard. I kept them in the skylight in the cabin, where they seemed to be quite contented; but on approaching the land they became the wildest of the wild. One of them escaped and flew away. I succeeded in taking the other into port, where I gave it its liberty. Now, I am certain that these birds could not have been apprised of the approach to the land through the medium of any of their ordinary senses. This curious circumstances led me to notice more particularly the conduct of other varieties of these little wanderers upon the ocean so far from their native habitat, and I find that they nearly all exhibit to a greater or less extent the same curious characteristics. Here the observant mariner with a smattering of science may find something to cogitate upon.

. . . . .

UNRECOGNIZED SPECIES

• Some Possible Enormous Birds

ON A GIGANTIC BIRD SCULPTURED IN THE TOMB OF AN OFFICER OF THE HOUSEHOLD OF PHARAOH

Bonomi; *American Journal of Science*, 1:49:403-405, 1845.

In the gallery of organic remains in the British Museum, are two large slabs of the new red sandstone formation, on which are impressed the footsteps or tracks of birds of various sizes, apparently of the stork species. These geological specimens were obtained, through the agency of Dr. Mantell, from Dr. Deane, of Massachusetts, by whom they were discovered in a quarry near Turner's Falls. There have also been discovered by Captain Flinders, on the south coast of New Holland, in King George's Bay, some very large nests measuring twenty six feet in circumference and thirty two inches in height; resembling, in dimen-

sions, some that are described by Captain Cook, as seen by him on the northeast coast of the same island, about 15° south latitude. It would appear, by some communications made to the editor of the Athenaeum, that Prof. Hitchcock, of Massachusetts, had suggested that these colossal nests belonged to the Moa, or gigantic bird of New Zealand; of which several species have been determined by Prof. Owen, from bones sent to him from New Zealand, where the race is now extinct, but possibly at the present time inhabiting the warmer climate of New Holland, in which place both Capt. Cook, and recently Capt. Flinders, discovered these large nests. Between the years 1821 and 1823 Mr. James Burton discovered on the west coast or Egyptian side of the Red Sea, opposite the peninsula of Mount Sinai, at a place called Gebel Ezzeit, where for a considerable distance the margin of the sea is inaccessible from the Desert, three colossal nests within the space of one mile. These nests were not in an equal state of preservation; but, from one more perfect than the others, he judged them to be about fifteen feet in height, or, as he observed, the height of a camel and its rider. These nests were composed of a mass of heterogeneous materials, piled up in the form of a cone, and sufficiently well put together to insure adequate solidity. The diameter of the cone at its base was estimated as nearly equal to its height, and the apex, which terminated in a slight concavity, measured about two feet six inches, or three feet, in diameter. The materials of which the great mass was composed were sticks and weeds, fragments of wreck, and the bones of fishes; but in one was found the thorax of a man, a silver watch, made by George Prior, a London watchmaker of the last century, celebrated throughout the East, and in the next or basin, at the apex of the cone, some pieces of woollen cloth and an old shoe. That these nests had been but recently constructed was sufficiently evident from the shoe and watch of the shipwrecked pilgrim, whose tattered clothes and whitened bones were found at no great distance; but of what genus or species had been the architect and occupant of the structure Mr. Burton could not, from his own observation, determine. From the accounts of the Arabs, however, it was presumed that these nests had been occupied by remarkably large birds of the stork kind, which had deserted the coast but a short time previous to Mr. Burton's visit. To these facts, said Mr. Bonomi, I beg to add the following remarks:--Among the most ancient records of the primeval civilization of the human race that have come down to us, there is described, in the language the most universally intelligible, a gigantic stork bearing, with respect to a man of ordinary dimensions, the proportions exhibited in the drawing before you, which is faithfully copied from the original document. It is a bird of white plumage, straight and large beak, long feathers in the tail; the mail (sic) bird has a tuft at the back of the head, and another at the breast: its habits apparently gregarious. This very remarkable painted basso-relievo is sculptured on the wall, in the tomb of an officer of the household of Pharaoh Shufu, (the Suphis of the Greeks,) a monarch of the fourth dynasty, who reigned over Egypt, while yet a great

part of the Delta was intersected by lakes overgrown with the papyrus,--while yet the smaller ramifications of the parent stream were inhabited by the crocodile and hippopotamos,--while yet, as it would seem, that favored land had not been visited by calamity, nor the arts of peace disturbed by war, so the sculpture in these tombs intimate, for there is neither horse nor instrument of war in any one of these tombs. At that period, the period of the building of the great pyramid, which, according to some writers on Egyptian matters, was in the year 2100 B.C., which, on good authority, is the 240th year of the deluge, this gigantic stork was an inhabitant of the Delta, or its immediate vicinity; for, as these very interesting documents relate, it was occasionally entrapped by the peasantry of the Delta, and brought with other wild animals, as matters of curiosity to the great land-holders or farmers of the products of the Nile,--of which circumstance this painted sculpture is a representation, the catching of fish and birds, which in those days occupied a large portion of the inhabitants. The birds and fish were salted. That this document gives no exaggerated account of the bird may be presumed from the just proportion that the quadrupeds, in the same picture, bear to the men who are leading them; and, from the absence of any representation of those birds in the less ancient monuments of Egypt, it may also be reasonably conjectured they disappeared soon after the period of the erection of these tombs. With respect to the relation these facts bear to each other, I beg to remark that the colossal nests of Capts. Cook and Flinders, and also those of Mr. James Burton, were all on the sea shore, and all of those about an equal distance from the equator. But whether the Egyptian birds, as described in those very ancient sculptures, bear any analogy to those recorded in the last pages of the great stone book of nature, (the new red sandstone formation,) or whether they bear analogy to any of the species determined by Prof. Owen from the New Zealand fossils, I am not qualified to say, nor is it indeed the object of this paper to discuss; the intention of which being rather to bring together these facts, and to associate them with that recorded at Gezah, in order to call the attention of those who have opportunity of making further research into this interesting matter.

ENORMOUS FOSSIL EGG FOUND IN MADAGASCAR

Anonymous; *Scientific American,* 6:33, 1850.

A singular discovery has been made in Madagascar. Fossil eggs of an enormous size have been found in the bed of a torrent. The shells are an eighth of an inch thick, and the circumference of the egg itself is 2 feet 8 inches lengthwise, and 2 feet 2 inches round the middle.

AUSTRALIA'S EMBARRASSING EGG

Butler, Harry; *Science Digest*, 65:70-73, March 1969.

For six years, the Western Australian Museum in Perth has had a giant embarrassment on its hands in the shape of an egg almost a foot long. Found by a farmer in the 1930s on Australia's south coast, the foot-ball-sized egg baffles scientists who study it. They report that it is, indeed, an egg and a bird's egg, but beyond these meager facts they're not willing to venture.

There are good reasons for their caution. Australia is the home of a giant bird, the emu, but emu eggs are relatively small--only about one-thirteenth as large as the giant egg. There is a giant extinct bird that lived until fairly recent times and laid eggs as large or larger than the Australian egg, but this huge creature, the Aepyornis, is known only from the island of Madagascar off the east coast of Africa. Madagascar is some 4,000 miles west of Australia.

. . . . .

First, the facts are these:

(1) It is a bird's egg, 11½ inches long, 8½ inches wide, 6½ litre volume. It is not a water-worn boulder, a shark's egg or other misidentification.

(2) The discovery site is in Aeolean dunes on the south coast within 500 yards of the sea. The find was witnessed.

In light of these facts, here are the possibilities:

(1) The egg belonged to an extinct Australian bird of Pleistocene time, a hypothesis borne out by the footprints, the possible skull and egg fragments of a similar type reported found in Queenland and South Australia.

(2) It is a Madagascar Aepyornis egg, either drifted or carried across the Indian Ocean and left on this drift area. Madagascar was the base for French whalers and sealers operating on the southern coast in the early days of Australian settlement and a sealer could have an Aepyornis egg from Madagascar in his possession. If for any reason the egg was jettisoned or lost overboard, it would be likely to float and drift ashore. The Indian and Southern Oceans meet in the immediate area of the egg find, and much drift material does come ashore there.

As for the skull and footprints, the finding of the beaked skull was witnessed, too, and the large, four-toed footprints are contemporary with those of such recognizable modern fauna as the emu and kangaroo. The skull, I suggest, may be either the skull of a bird similar to that which laid the egg or it could be the skull of one of the strap-tooth whales, since marine material has been found in the deposit. The prints may be those of a large extinct bird or they may be part of an aboriginal petroglyph, or carving, corroded with age.

And there, at present, the case of the embarrassing egg rests. New evidence may turn up that will clinch matters for one of these possibilities---or that will indicate that an entirely new explanation is correct.

Chapter 4
REPTILES AND AMPHIBIANS

Chapter Contents

UNRECOGNIZED SPECIES

MORPHOLOGICAL PHENOMENA

• Reptile and Amphibian Flight

FLYING REPTILES

Tweedie, M. W. F.; *Animal Kingdom*, 59:11-15, 1956.

Most naturalists are familiar with the appearance of the "flying" squirrels and with their capacity for gliding from one tree to another, and so for traversing large areas of forested country without descending to the ground. Quite a number of mammals possess this limited power of flight, such as the flying squirrels and the so-called flying lemur. All of them are adapted for gliding in the same way, by the development of a web of skin between the fore and hind limbs which, when the limbs are fully stretched out, acts as a parachute to support the animal in the air. In the equatorial rain forests of Southeast Asia several kinds of arboreal reptiles are also endowed with this power of gliding, but the nature of the adaptations whereby they achieve it are curiously diverse and make an interesting study. During my association with the Raffles Museum I have had the opportunity of seeing most of the Malayan species of these gliding reptiles in life and some of them in actual flight.

Most highly modified, probably most efficient and surely the most attractive of all of them are the wonderful little lizards of the genus Draco, the "Flying Dragons" of the older textbooks. These have the hinder five, six or seven pairs of ribs extending, in the form of cartilaginous rods, outside the body, and supporting a membrane of skin. The ribs are horizontally hinged at their bases and the whole apparatus can be folded up like a fan and laid along the sides, when it is scarcely visible and no encumbrance to the lizard when it is running or climbing.

As a rule the parachute is only spread when the lizard takes to the air, but occasionally it is half opened when two lizards are courting, chasing or sparring with each other. When it is closed and the animal stands motionless on a tree trunk the color of the body blends perfectly with the bark.

In flight these little reptiles are graceful and buoyant and glide at quite a small angle from the horizontal. The "wings" are brightly colored, those of the common Draco volans being blue below in the male and orange with darker markings above. The first one I saw in flight I quite genuinely mistook for a butterfly and was most startled when it landed on a tree and

was transformed to a drab, skinny little lizard.

All of them possess a scaly pouch on the throat with a smaller "wattle" on each side of it. Rods of cartilage (branches of the hyoid bone) project into these and the pouch can be flicked backwards and forwards. In most of the species it is brightly colored, yellow or red, and so is very conspicuous and its movement often betrays the presence of a lizard which one would not otherwise have noticed. Both sexes have the pouch, but it is larger and brighter in males. It is certainly used in courtship, but unaccompanied lizards can often be seen displaying it, and I think it also serves the purpose of a signaling device to inform individuals of the presence of another member of their species.

The "flying dragons" belong to the family Agamidae. The only other lizards which are adapted in this way belong to the genus Ptychozoon, members of a totally different group, the geckos. Of the two Malayan species of gliding geckos Kuhl's Flying Gecko, Ptychozoon kuhli, is the more common. Its gliding mechanism, when compared with that of Draco, affords an interesting example of how the same object can be achieved in two quite different ways. On each flank of the gecko there is an outgrowth of rather thick skin, stiffened with fibrous tissue, strongly near the line of union with body, distally progressively less so. It is not under any sort of muscular control and, when not in use, lies curled round the body, the two flaps not quite meeting in the mid-ventral line. From below it looks a little as if the lizard were wearing a waistcoat, fitted in its younger and slimmer days and no longer susceptible of being buttoned up. On each side of the head is a smaller outgrowth of skin similarly disposed. The tail has a scalloped fringe of skin on each side, permanently extended, and the fingers and toes are broadly webbed.

When Ptychozoon takes to the air it starts with a sort of swallow dive. As it accelerates the pressure of the air on its underside forces open the two flaps of skin so that they project on each side. The degree and distribution of their stiffening is such that, under the falling lizard's weight, the air pressure and their own natural tendency to downward curvature are balanced when they lie in the horizontal plane of the body. These, together with the flaps on the head, the outstretched webbed feet and the frilled tail, enable the lizard to glide at an angle a little flatter than 45 degrees. It is a larger, more heavily built animal than Draco, and its total speed of air-resisting surface, although so diversely constituted, is considerably less, relative to its weight. It is, accordingly, much less buoyant in the air.

. . . .

"FLYING" SNAKES

Sherbbeare, E. O.; *Malaysian Nature Journal,* 1:157-162, 1941.

Several people have observed snakes gliding from tree tops to

other trees or into the undergrowth at a lower level after the manner of Flying Squirrels. I have come across the following records:--

1. On 4th February 1899 somewhere in Thailand (presumably Northern Thailand) Mahon Daly, and his Kareen interpreter, saw a snake about 2½ feet long "sail from a very high tree on one side of the road to a lower one on the opposite side". He remarks, rather plaintively, that no one in Thailand or Burma would believe him. (Bombay Nat. Hist. Journal Vol. XII. p. 589).

2. In the same year S.S. Flower describing the snakes of Malaya records having seen at 2½ foot specimen of Chrysopelea ornata, the Golden-headed Tree-snake, "take a flying leap from an upstairs window downwards and outwards on to the branch of a tree and then crawl away among the foliage." (Proceedings of the Zoological Society, London 1899 p. 684.)

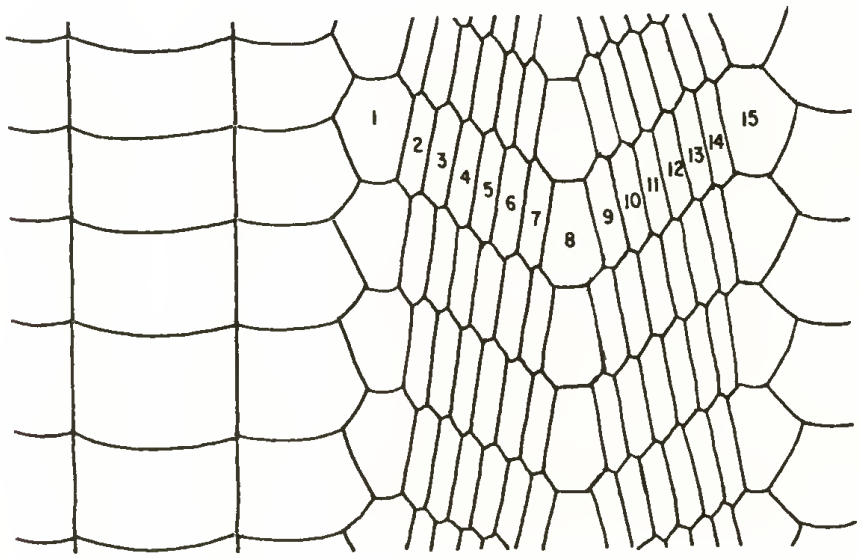
3. In 1906 Robert Shelford wrote a note on the gliding powers of three species of snakes (Chrysopelea ornata, C. chrysochlora and Dendrophis (now Ahaetulla) pictus--the Eastern Bronzeback). He tells of how he had had reports of these snakes "flying" from his Dyak collectors, of how, at first, he disbelieved them and how he finally convinced himself by experiments with live snakes. What is still more to the point he found out how the feat was accomplished. (Proc. Zool. Soc. 1906 p. 227.)

4. The latest record comes from still nearer home as the scene was only a few miles from where I am writing this and the observer was my friend Mr. H.M. Pendlebury of the Selanger Museum, Kuala Lumpur. On January 12th 1930, on the road between the Ginting Sempak pass and Bentong, he saw a snake, which of course he could not identify at that range, float from near the top of a tall tree into rank undergrowth on the hillside below. He estimates the angle of glide at about 45 degrees, the distance about 100 feet and the time taken roughly 7 seconds. What is still more interesting is that the snake held itself in a double S bend (Shelford's snakes all kept straight and rigid throughout their glide) and what is most interesting of all "during descent if lashed it tail several times vigorously to the right as though trying to steer itself to some particular spot. These tail lashings became more pronounced immediately before it landed when it dropped the tail end of its body, under complete control, on some large-leaved plant and disappeared into the undergrowth." It had apparently been disturbed by some monkeys. (Bulletin Raffles Museum No. 5, August 1931 p. 75.) In a private letter Pendlebury writes:--"A man on an estate near Port Dickson has more than once seen Chrysopelea gliding off trees and thatched roofs and tells me he was once made an unwilling 'blanket'". The species of snakes which can glide are common in Malaya and it would be interesting to hear from readers who have witnessed the phenomenon.

So much for the records, now for how it is done, and which snakes can do it.

There are two small groups of snakes which have what the books call a "lateral keel along the ventral shields". This pecu-

liarity, though not easy to describe, is easily recognized from the figure below.



The figure represents a short length of the skin of such a snake which has been slit, not up the middle of the belly, but along the right flank so that the left side of the picture shows the belly and the right side of the back. The lines and notches on the ventral shields (on the left of the figure) are peculiar to snakes of these two groups. There are six species of snakes in this country, three species of "Bronze-back" (Ahaetulla formerly Dendrophis) and three tree-snakes (Dryophiops and two species of Chrysopelea) which have this type of ventral shields. It is probable that all six have the power of gliding to some extent but at any rate one of them, the beautiful "Goldenheaded Tree-snake" Chrysopelea ornata) is known to be an expert.

I had often wondered, vaguely what was the significance of this curious type of ventral shield but, as far as I know, Shelford was the first to discover the thrilling secret of their use. He describes them not as "keels" but, much more sensibly, as "hinges" and, after he had decided that there might be some truth in these gliding stories and got hold of a live specimen of Chrysopelea ornata, he was soon on the track of his great discovery. As soon as he had the snake in his hands he observed that it was able, by a muscular effort, to change the cross-section of its body from the normal more or less circular shape to a very decided crescent, convex on the back and concave on the belly the horns of the moon being represented by the "hinges". This change of shape was, as he puts it, the difference between an ordnary length of bamboo and one half of a split one and, whereas the former will fall straight to the ground,

the latter will glide downwards at an angle to the perpendicular. He therefore took the snake to an upstairs verandah and threw it in the air. The snake fell in writhing coils and hit the ground "with a distinct thud." This was not so good but neither Shelford nor Chrysopelea were the sort to be disheartened by the failure of a first attempt and "after one or two false starts eventually felt it glide rapidly through my hands, straightening itself out, and hollowing in its ventral surface as it moved; this time it fell not in a direct line to the ground but at an angle, the body being kept rigid all the time."

Shelford's Dyak collectors told him that some other snakes possessed the same power of gliding; another species of Chrysopelea (C. chrysochlora), and Dendrophis pictus, the Eastern Bronzeback, which is described in one book as "perhaps the commonest species of snake in Selangor." He experimented with a live specimen of the last named species which unfortunately would not play, possibly because it was just not in the mood for gliding or perhaps because, for a shy and delicate little snake like a Bronzeback, his methods were, to say the least of it, rather rough. He noted however that even the Eastern Bronzeback assumed the concave shape when held by the tail and that all the species reputed to glide had hinged ventrals. Pendlebury and I had a similar experience when we experimented with this species--with a cine camera at the ready.

It seems at least probable that all snakes of the bronzeback type are capable of gliding when circumstances demand it and, as our readers will no doubt wish to experiment I will give a few notes as to what these snakes look like.

Chrysopelea ornata.--Is rather a variable snake and I will therefore not bother you with a description of if beyond saying that it is such a gorgeously coloured snake that it is not likely to be overlooked by anybody and that the outstanding feature that will distinguish it from any other of our snakes is a row of pink, red or orange blotches all along the back. The head is very handsomely marked in black and gold and the general colour, as a rule, green with black markings.

The Bronzebacks themselves, three species in all belonging to the genera Dendrophis (now called Ahoetulla and Dendrelaphis), are all very much alike. They are slender creatures in shape very like the common Malayan green Whipsnake (Dryophis prasinus), but they lack the whip-snakes' "greyhound" head and snout. In colour they are like old bronze above, the illusion being sometimes heightened by a few verdegris-green scales especially near the neck. The belly is greenish-blue to bluish-green, the part between the "hinge" and the outer costal rows being the brightest and often the bluest. There may or may not (according to the species) be a dark flank-stripe through the eye and along the junction of costals with ventrals to the end of the tail. They live almost entirely in trees and low bushes.

Having got your snake you can either experiment straight away or get him used to being handled bently so that he will not be afraid of you; I am not sure which will be best. You

need not be afraid of any of these snakes yourself, they are of course not poisonous and I have always found them gentle and never known them bite, and the bite of a harmless snake is a thing well worth avoiding. When you are ready to make the experiment I should suggest taking the snake to a point of vantage, such as an upstair verandah, within easy reach of a shrubby-topped tree--an isolated one if you do not want to lose him--and encourage him to do his gliding act voluntarily.

Malaya seems to be nearly central in the habitat of gliding snakes if I am right in thinking that "hinged" ventrals are confined to four genera-- Dendrophis (now Ahoetulla), Dendrelaphis, Chrysopelea and Dryophiops, and, further, in assuming that all species of these genera, and only they, are "gliders".

I have blackened in the habitat of these gliding snakes on the accompanying map, to the best of my knowledge and with the above assumptions, because it seems to include rather strikingly the habitat (or what I think is the habitat) of the majority of the "gliders" of the world--flying lizards, flying lemurs, flying phalangers (marsupials mark you, from Queensland and New Guinea) as well as most of the flying squirrels. (These flying squirrels are much too widely distributed for the liking of theory-mongers like myself. There is a single species in America and another in Europe, both of them related to some of our Malayan species, and a whole separate group in equatorial Africa. Except for these flying squirrels I believe no gliding creatures are found outside the corner of the world shown in this map). It seems strange that an accomplishment like gliding, which one might have thought would come in handy for tree-dwellers in any part of the world, should have such a restricted range; but speculations of this sort are dangerous for amateurs, if there is anything in it it has no doubt been commented on by experts long ago.

Before leaving the subject of flying snakes I must do what I can to clear our reputation (Shaw's and mine) by quoting from our original series of the "Snakes of Northern Bengal". Though I certainly knew nothing of these gliding powers before I came to Malaya and never heard Shaw mention it, Colonel Wall, who knows all there is to know about Indian snakes, most of it at first-hand, had evidently witnessed a short flight though he wrote of it more as a jump. Under Dendrophis pictus we say "is able to spring from one tree to another", and, under Chrysopelea ornata "has been known to take flying leaps from branch to branch. One mentioned by Colonel Wall leapt from a tree 20 to 25 feet. It was fully extended till it reached the ground."

It would be nice to have a tame gliding snake to amuse one's friends but, though Bronzebacks will become friendly and unafraid, I have found it difficult to get them to feed in captivity and I think I remember others telling me the same.

A FLYING BATRACHIAN OF MALAISIA

Anonymous; *Scientific American,* 56:233, 1887.

All the vertebrates have representatives that are endowed with the faculty of flight. This gift has not been refused to the bat any more than to the galeopitheci, or flying lemurs, and in various regions we find squirrels whose skin extend along the sides in wide folds, and form a large parachute. The flying squirrels (Pteronys) are the most remarkable representatives of these aerial rodents, and, among marsupials, the charming Belidae of the Austo-Malaisian region, are not the least graceful of the flying mammals.

It has been the privilege of all who have sailed in tropical waters to see flying fishes describing graceful parabolas over the water, and endeavoring to escape the bill of the bird or the teeth of the bonito. The reptiles have their flying dragons, and the batrachians include the Rhacophori among their number.

The celebrated English traveler A.R. Wallace has, in a remarkable work--a true vade mecum for every naturalist visiting Malaisia--embodied the fruit of his researches and observations made during a stay of about eight years in the islands of Malaisia and New Guinea. During one of his trips to the island of Borneo he was enabled to procure one of those batrachians of the curious genus Rhacophorus, and he thus describes it:

"One of the most curious and interesting reptiles which I met with in Borneo was a large tree frog, which was brought me by one of the Chinese workmen. He assured me that he had seen it come down, in a slanting direction, from a high tree, as if it flew. On examining it, I found the toes very long and fully webbed to their very extremity, so that when expanded they offered a surface much larger than the body. The fore legs were also bordered by a membrane, and the body was capable of considerable inflation. The back and limbs were of a very deep shining green color, the under surface and the inner toes yellow, while the webs were black, rayed with yellow. The body was about four inches in length, while the webs of each hind foot, when fully expanded, covered a surface of four square inches, and the webs of all the feet together about twelve square inches. As the extremities of the toes have dilated disks for adhesion, showing the creature to be a true tree frog, it is difficult to imagine that this immense membrane of the toes can be for the purpose of swimming only, and the account of the Chinaman, that it flew down from the tree, becomes more credible."

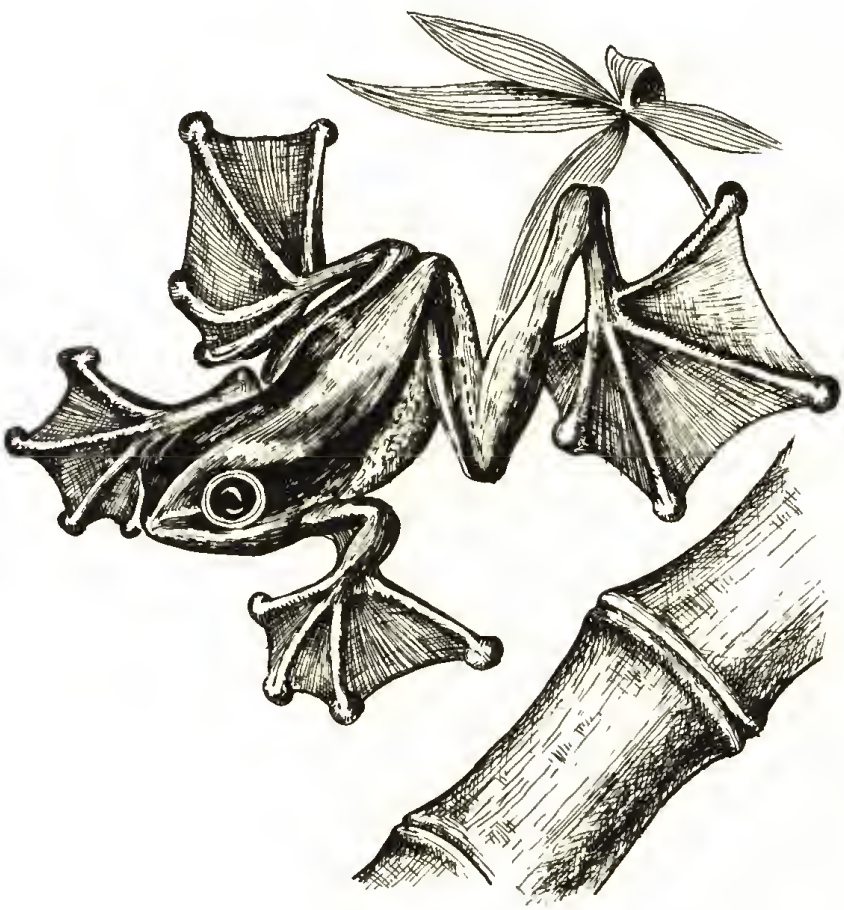
The naturalist Kuhl, who perished in Java, a victim to his devotion to science, assigns the following general characters to these toads, some of the forms of which he groups under the title of Rhacophorus:

Interdigital membranes long and extensible, folded longitudinally when the fingers are not extended; head short, tongue wide, and developed lengthwise, narrow in front, and forked, free behind; tympanum apparent; vomerian teeth situated between the wide spaced posterior nares; the skin of the arm forming

along the latter a crest-like expansion. While resembling tree toads in their general external characters, the Rhacophori, in their internal organization, recall the frogs, among which many naturalists are inclined to class them.

We herewith give a figure of a large species, drawn by Mr. Clement from a specimen in the Museum of Natural History of Paris. This species, which is known as R. rheinwardti, has a green back, speckled with black, and an orange-yellow belly, marked with black dots. Blue blotches are found upon the palms of the four limbs, between all the fingers except the first and second. The general form is well shown in the engraving. The eyes are protuberant, and the snout is rounded in front. Although the dorsal skin, as well as that of the upper surface of the limbs, is smooth, the belly is very grannular, as is also the lower surface of the thighs; but the breast and throat are smooth. At the extremity of the very large and long fingers are observed spongy disks of considerable size. The hand has one peculiarity: its fingers are provided in the center with a long, tubercular appendage.

. . . . .



FROGS OF MEXICAN FORESTS LEARNING TO FLY AND CLIMB

Anonymous; *Science News Letter,* 22:51, 1932.

A curious species of frog which can fly, or, to be more accurate, glide through the air by spreading its elongated hind legs, thus coming gracefully to earth from heights of as much as 90 feet, is among those found in the tropical forests of Mexico by Dr. Remington Kellogg of the United States National Museum and described by him in a Smithsonian Institution report.

These tree frogs are difficult to find, for they keep out of the way of the human explorer and can even change the color of their bodies like chameleons to match their surroundings.

Various members of the family of tree frogs show different stages of evolutionary adaptation to their home in the trees. Besides the "flying" species, there is another which is learning to climb and cling to limbs of trees through the development of adhesive disks on the ends of the fingers and toes.

• Two-Headed Reptiles

AMPHICEPHALOUS REPTILES: ANIMALS REPUTED TO HAVE A HEAD AT EACH END OF THE BODY

Cunningham, Bert; *Scientific Monthly,* 37:511-521, 1933.

Monsters, animals having abnormalities, have always held a strange fascination for man. Those occurring among lower animals have been the source of much of our mythology, while those occurring among humans have given rise to some of our demonology.

While there are many authentic monsters of strange and bizarre appearance, many others are figments of the imagination which are either remotely based upon fact or are wholly without foundation. It is difficult to determine the origin of some of the ideas, and their wide-spread appearance seems to argue against a common source.

Among recurring stories concerning reptiles is one which portrays animals with two heads, one at each end of the body. In this paper such animals are called amphicephala. It is my purpose to examine such reports and to determine, if possible, their bases.

Aminals of the foregoing type, whether mythical or not, were early called by the group name Amphisbaena, and stories concerning them range from the days of Nicander (185-135 B.C.) to

recent times, and in regions as widely separated as Europe, Asia, Africa and Mexico, and also the United States. However, it is doubtful if any modern scientist believes in the existence of such monsters.

If one were forced to select animals in which such phenomena might occur, one would most naturally select worms and snakes, and it is here, for the most part, that one finds such records and traditions. Except for a single mention of the condition in worms, the reports thus far found center upon the reptiles and include three of the four common orders of reptiles, namely, serpents, lizards and turtles.

Snakes. It is not at all surprising that most of the reports of amphicephalic animals, or animals that closely approach this condition, should relate to reptiles, since among them are two groups of animals without legs which have certain characteristic habits that lend aid to the deception on which such ideas may be based. There are certain snakes (Cylindrophis rufus and others) which are described and figured by Barbour as running with the tail erect, and, in some cases, swaying it back and forth. Such specimens would serve as exceptional evidence, to the superficial observer, of a snake which seems to be crawling backwards. It follows naturally, therefore, that this snake is capable of crawling in both directions, and amphicephala are thus created from misinformation. There are also legless lizards whose bodies are nearly uniform in diameter throughout the whole length and whose eyes and mouths are very inconspicuous. These lizards move with celerity either backward or forward and often run with the tail erected, which give them somewhat of the appearance of snakes crawling with heads raised from the ground. The general character of this group of animals may be better understood by examining Fig. 2 [not reproduced], which is a photographic copy of the drawing of Amphisbaena alba from Leunis. To lizards of this group the name Amphisbaena has been applied in modern literature, and, in order to avoid confusion, it is proposed in this paper to use the word Amphicephala to indicate specimens which are presumed to have a head at each end of the body. This term was used by Galen in describing Amphisbaena, which was thought by him, as well as by Nicander, to be amphicephalous.

. . . . .

Thus far the stories concerning serpents have been heresay, and apparently none of the foregoing have been eyewitnesses of this type of monster. However, in 1679, a catalog of the Swammerdam collection was published, in which, among other curious creatures, was listed an Amphisbaena serpens. In the following year, Blancartus commented on this specimen as follows:

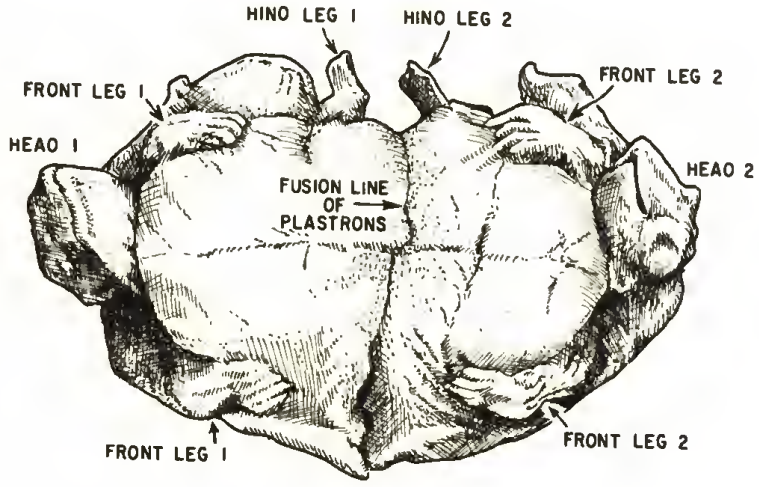
> Some may not believe in two-headed snakes, but they do actually exist, for I have actually handled one which is among the Swammerdam rarities. The specimen is a span and a half long, normal in color, and has a head on each end of the body with small scales.

The figure which accompanied his report is reproduced herewith as Fig. 6. There can be no doubt about what he aims to show. This case is of exceedingly great interest, since two reputable scientists and a well-known scientific publication are involved. Furthermore, if this specimen did actually exist, it must stand alone, as no other cases of exactly this type have been found in the considerable literature that has been examined.

Taken as a whole, it would seem that all cases reported in reptiles, with the exception of Blancartus, might easily be explained upon some other basis, and, since in no case are they observations of eye-witnesses, they may at least be held in grave doubt, if not totally rejected. The case of Blancartus, on the other hand, must be considered, since an actual specimen in the collection of a scientist, observed by a scientist, and recorded in a scientific publication, can not be ignored, even though one is justified in wishing for additional confirmation.

Turtles. The occurrence of amphicephalous monsters is not incredible. Twins, among humans, have been found fused together in almost every conceivable position, and, among turtles, a case has been reported which approaches, although it does not parallel, the condition described by Blancartus for the snakes.

Townsend has figured a pair of turtles which were fused together at the posterior ends of the carapace. This figure is reproduced here as Fig. 7. If the two posterior ends of the



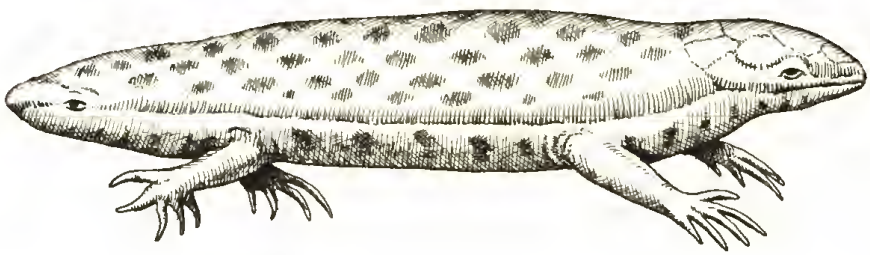
Two "fused" turtles (Fig. 7)

serpent twins should become grafted in a similar manner but with a little more efficiency, just such a condition as that described by Blancartus would arise. Such an animal might well grow to hatching size, be hatched and live for a few days, even in the complete absence of an anus. The occurrence would naturally be exceedingly rare, possibly even more so than our present records indicate.

. . . . .

Lizards. So far as I have been able to discover, the two lone cases of amphicephalous lizards are presented by Aldrovandi; one of which he reports from another source; the second came under his own observation. Both specimens are here figured.

The first of Aldrovandi's figures is taken from an earlier work by Torquatus Bembus, and represents two lizards with the bodies fused, the heads pointing in opposite directions. It does not seem reasonable to call this animal an amphisbaena (or amphicephala) as has been done by Bembus. There is nothing to indicate that these are any more than very simple Siamese twins, fused in an unusual fashion, to be sure, but quite probably without any of the internal organs in common, a thing which would not be so incredible as that shown in his second figure, where it seems evident that the two bodies are fused end to end in what should be about the middle region of each animal. In this latter he does not say whether or not an anus is present. The animal was alive when taken, and "walked in the direction of each head." The figure is said to have been drawn from the living specimen.



A two-headed lizard (Aldrovandi's second figure)

Since Aldrovandi claims to have seen this specimen while it was living, and also made the figure from the living animal, one can not do otherwise than accept it. This provides, therefore, a good case of amphicephaly among lizards.

Possible Source of Traditional Cases. Since this monstrosity

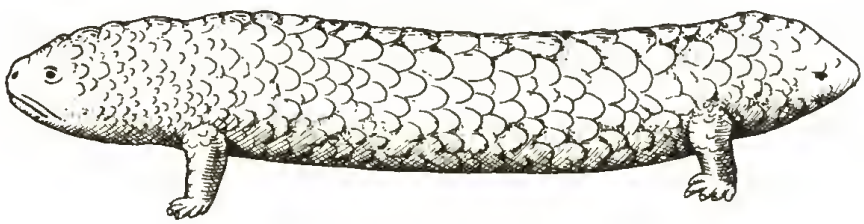
is so uncommon, and since the stories, especially those concerning snakes, are so wide-spread, it is unreasonable to suppose that all the stories of Amphicephala have originated from the actual observation of an amphicephalous type of monster. There are other ways in which this idea may have originated concerning serpents. The credulous may have been fooled by the native Hindu who paints eyes and a mouth on the posterior end of the rubber boa to give it the appearance of being amphicephalic; or by the same treatment accorded the Gila monster by natives. In some cases even artifical decoration is unnecessary, for Ditmars writes as follows:

> The Brown Sand Boa Eryx johnii . . . owing to its stubby tail is also called the two headed snake, and, when coiled in a mass with the head hidden and the tail protruding as is a common position, the tail might readily be mistaken for the anterior end.

It also seems quite possible that the idea may have originated from snakes similar to those described by Barbour (l.c.) If one, unaware of the habit, should see such an animal crawling with the tail erected, one would immediately conclude the animal was running backward. From that point it is not far to the conclusion that the animal has a head on each end of the body. In animals so spectacularly marked as those shown by Barbour, the deception would be complete.

. . . . .

Except for the fact that Aldrovandi specifically states that he observed the lizard specimen, his figure might well have originated from a more or less careless observation of some such animal as the stump-tailed lizards, which appears depicted in Fig. 14, redrawn from an original photograph of the animal published in the "Book of Popular Science." This particular form is a native of Australia.



A single-headed stump lizard (Fig. 14)

It is evident that many of the reported cases are based upon hearsay, and that others may be based upon misinterpretation, but, nevertheless, that there are a few cases in which the actuality of existence can not well be doubted must be conceded. It is reasonable, therefore, to conclude that amphicephaly does occur in reptiles, but that it is exceedingly rare.

TWO-HEADED SNAKES

Cunningham, Bert; *Scientific Monthly,* 25:559-562, 1927.

The two-headed specimen of the spreading adder which initiated this study came into my possession in an unusual manner. It was discovered by men who were making a run of illicit liquor when the small reptile came up to the fire either from curiosity or to warm itself. The men, fearing that their senses were fooling them, captured and preserved the reptile in their brew in order that they might observe it under more favorable conditions. The specimen was lost to science when it had to be returned to its owner to settle a bet. The route by which the specimen came to me was so indirect that I have been unable to trace the original ownership.

However, good use was made of the specimen while in our possession. Photographs were made of the external appearance and X-rays were made of the skeleton. These are shown in the accompanying figures.

Through the kindness of Dr. E.W. Gudger, of the American Museum, and Mr. C.S. Brimley, of Raleigh, N.C., some references to the literature of the subject were secured. With these as a basis, the writer has been able to secure a rather extended bibliography. Although the many references to this phenomenon are scattered, there have been two rather exhaustive papers published upon this condition--the first by Johnson in the Transactions of the Wisconsin Academy in 1901, and the other by Strohl, which was published in 1925 in the Annales des Sciences Naturalles Zoologie. Both of these articles carry good bibliographies which are remarkably supplemental.

Animals possessing two heads have been described in two ways. Among these there are those which are reputed to have a head on each end of the body, and there are those having multiple heads on the anterior end of the body.

Among the first are the snake-like lizards, that is, lizards without legs, to which belongs the genus Amphisbaena. These animals are nearly the same diameter throughout their entire length. When disturbed, they have the peculiar habit of elevating the tail in much the same manner that a snake does its head. Ditmars in describing the animal says "The tail is just as thick and stumpy as the head; unless one examines the animal closely, the two extremities can not be told apart," and further, "When a specimen was annoyed it would elevate the stubby tail in a

fashion that caused the organ to look like a head reared in threat; at such times the head remained flat upon the ground unless the body was pinched; then the head swung about for the offender." They are also reported to have the habit of running backward with the same agility as they go forward. Hence the name. With these two habits combined, one is not surprised to find that some observers thought the animals possessed of two heads. There is also a group of snakes having somewhat similar characteristics, the Eryx johnii. The Hindoos painted a mouth and eyes on the blunt end to make the appearance of two-headedness more conclusive.

The Amphisbaenas (the snake-like lizards) were considered as extremely poisonous according to Bancroft. This writer pointed out the improbability of the Amphisbaena having two heads since "there is no animal in nature that is thought to have two heads." In a footnote to this sentence he reports and figures a genuine dicephalous case occurring in a specimen from the Lake Champlain region, which he designated as an Amphisbaena. This animal is in no wise similar to Amphisbaena, but is rather to be grouped with those having anomalies of development which are now to be considered.

Of the snakes having the dicephalous anomaly, there have been from twenty to thirty cases reported, ranging from those having skulls but slightly separated to those having necks of considerable length. In some the ribs from one vertebra fuse with those of the vertebra lying opposite. This may be observed in our specimen as well as in Pityophis sayi. It can also be seen that in our specimen the bifurcation begins with a Y-shaped vertebra.

The internal anatomy of some of the specimens has been studied. In all cases it is to be supposed that the alimentary canal is double for some distance, although we did not demonstrate this in our specimen. Both mouths are reported as functional, and it has been observed that when one mouth has eaten, the other ceases to be hungry. However, both heads will attempt to swallow the same morsel. In a case reported by Lessona, there were two sets of lungs present although three of the four were atrophied; there were also two livers, one of which was larger than the other; the heart also was paired.

Although most of the specimens reported were small and presumably young, some virtually newly born, Lessona cites Redi as having had in his possession an adult snake having two heads.

Since nature does not often produce such monsters, it is legitimate to ask why does she produce any. It is comparatively easy to understand why placental animals become twin monsters, and the spina bifidal effects of pressure on frog eggs, but what can happen to a hen's egg that would make it produce a four-legged, three-winged monster is a mystery. The same might be said with a reasonable degree of assurance about the snake egg.

One of the more common explanations is that the phenomenon is due to a double yolk. Double yolks may be responsible, but I have seen two turtle embryos (Chrysemys marginata) on a single embryonic disk. They were small, about 4 mm., but apparently perfect. It would not take much imagination to see them fuse into

a two-headed monster with a single body.

Another explanation is that the condition is due to pressure. Pressure effects would in all probability depend upon the rigidity of the shell. Certainly in chicks this would not be the cause, for it has been shown that chick embryos produced under increased atmospheric pressure show no more anomalies than those produced under normal pressure. In such animals as Chrysemys marginata where the shell is flexible the turgidity is so great that considerable external pressure would be required to produce any effect.

The possibility of two germ nuclei in a single embryonic disk, or the separation of the blastomeres produced in first cleavage with independent development for a time and later a partial fusion, seems more reasonable, although neither of these explanations is wholly satisfactory.

• Chameleon Bilateral Independence

COMPOUND ANIMALS

Beard, J. Carter; *English Mechanic*, 64:9, 1896.

As we descend in the scale of animated beings the confusion of identities, exceptional and even monstrous among mammals, becomes the established order of existence. Here, indeed, we find distinct and divided personalities tenanting the same body, and on the other hand a number of independent bodies composing one animal.

As an example of the first may be adduced the chameleon, an animal that in a certain sense seems to be constructed like one of those Chinese swords which, though occupying one scabbard, and though to all appearances a single weapon, is, in fact, a double one, composed of two hilts and two blades. The physical person of each chameleon seems to accomodate two complete mental outfits, and is governed by two wills, as different and distinct from each other in most respects, as those of two separate animals. The movements of organs and members, subjects to voluntary motion, on one side of the chameleon, have no necessary connection or co-ordinated action with those on the other side. One side may be asleep while the other is wide awake, one eye may look forward and the other backward, one side change its colour while the other remains as it was, and it seems to be shown that some sort of mutual consultation between the two halves of the animal is necessary before any orderly or regulated movement can take place, by the fact that if the chameleon drops

into the water, the struggles of the limbs on each side of its body are entirely unrelated, and as a consequence the creature forms a very rare exception among quadrupeds of being unable to swim. In such a case the chameleon, if left to itself, invariably drowns.

. . . .

• Frog Cocoons

COCOON SURROUNDING DESERT-DWELLING FROGS

Lee, A. K., et al; *Science,* 157:87-88, 1967.

Abstract. A cocoon formed from a single cell layer of shed stratum corneum may reduce water loss from the skin of desert-dwelling frogs while these estivate in soil-filled burrows. In several Australian examples, the cocoon is a single layer of cells, and thus differs from the multilayered structure obtained from an American species, Scaphiopus couchi.

• Parthenogenesis and All-Female Populations

VIRGIN BIRTH

Mittwoch, Ursula; *New Scientist,* 78:750-752, 1978.

The importance of parthenogenesis in a given animal species is quite variable. On one hand, it may occur sporadically in a species which normally reproduces by fertilisation. Or it may occur regularly in a certain part of the life-cycle, whereas fertilisation occurs in a different part of the life-cycle. Alternately, parthenogenesis may be the sole mode of reproduction of species which consists exclusively of females. This last phenomenon appears to be more important than had been realised. In his recent book Modes of Speciation (Freeman, 1978), Professor Michael White states that there are about a thousand animal species which reproduce exclusively by parthenogenesis. Thus,

all-female species form a small, but by no means negligible proportion of animals. Furthermore, biologists used to think that parthenogenesis was confined to animals which were less advanced than vertebrates. However, 20 years ago, Professor Ilya Darevsky, of the Zoological Institute of the Academy of Sciences of the USSR, put forward the view that the Caucasian rock lizard, Lacerta saxicola, reproduces parthenogenetically. At first, this claim met with considerable scepticism, but it has since been fully confirmed. Indeed, in at least 25 species of lizards males are either entirely absent or are absent in certain populations which are maintained by parthenogenesis.

The final proof that parthenogenesis can be a viable reproductive process in lizards are brought by Dr. Charles Cole at the American Museum of Natural History in New York. He captured in New Mexico a few female chihuahua whiptail lizards, Cnemidophorus exsanguis, and these have become the founding mothers of a lizard colony which is now in its fourth generation. All the lizards are female, and they have been maintained in complete isolation from males.

ALL-FEMALE SPECIES OF THE LIZARD GENUS CNEMIDOPHORUS, TEIIDAE

Maslin, T. Paul; *Science*, 135:212–213, 1962.

Abstract. Six species of lizards in the genus Cnemidophorus, teiidae, have been found to be all-female or virtually all-female. The species are C. costatus exsanguis Lowe, C. deppei cozumelus Gadow, C. inornatus Baird (western population), C. perplexus Baird and Girard, C. tessellatus (Say). and C. velox Springer. It is hypothesized that the explanation of these virtually all-female populations is genetic.

• Mysteries of Dinosaur Evolution

MYSTERIES OF EARLY DINOSAUR EVOLUTION

Cox, Barry; *Nature*, 164:314, 1976.

Although many pages have been written discussing the mystery of the extinction of the dinosaurs, almost as much uncertainty

surrounds their origin or origins. The large Mesozoic archosaurian reptiles known as dinosaurs belong to two separate groups, the Saurischia and the Ornithischia, which differ in such characters as the structure of the pelvic girdle and the possession of a predentary bone (present only in ornithischians). A diversity of saurischians is known from the late Triassic, as are about six genera of ornithischians. But the relationships of the two groups to each other or to the earlier Triassic archosaurians known as pseudo-suchians, remain uncertain.

. . . .

The possible ancestors of the different types of dinosaurs are thus scattered through the Triassic, and the pseudosuchians persisted until the end of that Period. Throughout the Late Triassic, at least, a variety of pseudo-suchians therefore coexisted with a variety of their presumed descendants, the dinosaurs, as has been pointed out by Sill. The perhaps multiple evolution of dinosaur lineages therefore did not lead to the rapid ecological replacement of their pseudosuchian ancestors. These instead persisted, together with their relatives the semi-aquatic phytosaurs and the pig-like rooting aetosaurs, until the end of the Triassic, when all became extinct together--though no new ecological replacements for them appeared at that time.

So the poor palaeontologist searching for answers is therefore, in the origin of the dinosaurs, confronted with complexity where he hoped for simplicity, while in the replacement of the pseudosuchians by their varied offspring he meets a sudden (if delayed) simple event where he expected complexity! Today, the fashionable deus ex machina for explaining extinctions is continental drift, but even rising sea levels resulting from the beginning of the opening of the Atlantic in the Late Triassic seem unlikely to provide a solution to these puzzles.

• Problems in Explaining Turtle Ancestry

NOT A TURTLE AFTER ALL

Anonymous; *Nature,* 244:1057-1058, 1969.

The ancestry of turtles and tortoises is one of the most persistant mysteries of vertebrate evolution. These most aberrant of reptiles comprise the group known as the Chelonia and are classified with the most primitive of known fossil reptiles, the cotylosaurs, as the subclass Anapsida. But this seems an inadequate response to the unique anatomy that has developed along with the shell which is carried both dorsally and ventrally.

The earliest known turtle in the fossil record occurs in the Upper Triassic and is a definitive if primitive turtle in its anatomy. The earliest known cotylosaur reptiles are of early Upper Carboniferous (Pennsylvanian) age and no fully satisfactory intermediates in time or morphology are known.

In 1892, Seeley described a fossil reptile from the Middle Permian of South Africa, roughly half way in geological time between the early Pennsylvanian and Upper Triassic. He remarked that its slender, elongate vertebrae, lacking transverse processes, were reminiscent of those of Chelonia and that the expanded leaf-like ribs "simulate the ribs and costal plates of chelonians". The animal was named Eunotosaurus.

In 1914, D.M.S. Watson attempted a reconstruction of the characteristics of an ancestral chelonian from a study of fossil and living forms and concluded that Eunotosaurus was sufficiently close to his ideal that "it is by no means improbable that it is an actual ancestor of the Chelonia". Since Watson's study the tendency has been to place Eunotosaurus within the Chelonia in vertebrate classifications, despite dissident voices.

A recent study by C.B. Cox (Bull. Brit. Mus. (N.H.), 18, No. 5) should settle the status of Eunotosaurus beyond reasonable doubt at least as far as postulated turtle ancestry is concerned. Watson's study was based only on the four specimens in the British Museum; Cox has also assembled together all the known specimens of Eunotosaurus from museums in the United States and South Africa, giving a grand total of eleven fragmentary individuals.

A detailed and well illustrated study of the material suggests that Eunotosaurus is not a chelonian nor yet an anatomical antecedent to that group. The characters which suggested chelonian affinity, the small number of elongate trunk vertebrae and the expanded ribs, now suggest evolutionary convergence of Eunotosaurus to a turtle-like habit, a phenomenon also seen in an unrelated group of marine reptiles, the placodonts.

Other characters cited by Watson were not confirmed. The Single fragmentary skull does not have a turtle-like snout, as revealed by further treatment of the matrix with acetic acid, and no trace could be found of the reported dermal bone from a shell. Cox concludes that Eunotosaurus is a cotylosaur, but a very aberrant one, with characters which may actually proclude it from chelonian ancestry. It is to be hoped that it will now cease to be considered for the latter role.

• Convergence and Mimicry

A DARWINIAN PARADOX

Gould, Stephen Jay; *Natural History*, 88:32+, January 1979.

Consider my candidate for the most astounding convergence of all: the ichthyosaur. This sea-going reptile with terrestrial ancestors converged so strongly on fishes that it actually evolved a dorsal fin and tail in just the right place and with just the right hydrological design. The evolution of these forms was all the more remarkable because they evolved from nothing---the ancestral terrestrial reptile had no hump on its back or blade on its tail to serve as a precursor. Nonetheless, the ichthyosaur is no fish, either in general design or in intricate detail. (In ichthyosaurs, for example, the vetebral column runs throgh the lower tail blade; in fish with tail vertebrae, the column runs into the upper blade.) The ichthyosaur remains a reptile, from its lungs and surface breathing to its flippers made of modified leg bones, not fin rays.

Koestler's carnivores tell the same tale. Both placental wolf and marsupial "wolf" are well designed to hunt, but no expert would ever mistake their skulls. The numerous, small marks of marsupiality are not obliterated by a convergence in outward form and function. (p. 40)

A LIZARD IN BEETLE'S CLOTHING

Anonymous; *Science News*, 111:54, 1977.

Juvenile lizards of the species Eremias lugubris in southern Africa walk stiffly and jerkily with arched backs in an apparent attempt to imitate a neighboring insect. And researchers now have evidence that the lizards' mimicry is successful.

According to an important hypothesis of evolutionary theory, palatable and unprotected individuals can gain protection from predators by imitating an unpalatable or well-armed species. "The resemblance of juvenile E. lugubris to oogpister beetles represents not only the first substantive case of mimicry involving a quadrupedal lizard, but also, to the best of our knowledge, the first case of a terrestrial vertebrate mimicking an invertebrate," say zoologists Raymond B. Huey of the University of California at Berkeley and Eric R. Painka of the University of Texas.

The oogpister beetle is well equipped to discourage attackers. The beetle can eject a pungent fluid of formic acid and assorted

other acids and aldehydes.

The juvenile lizards are certainly in need of protection, being palatable and relatively defenseless. Against the pale sand, a lizard's jet-black skin with white broken stripes makes it an obvious target for birds, snakes, foxes and jackals. Paradoxically, because it is conspicuous and looks like a noxious beetle, predators avoid the lizard.

. . . .

• The Possible Inheritance of Acquired Characters

ADAPTATION AND INHERITANCE IN THE LIGHT OF MODERN EXPERIMENTAL INVESTIGATION

Kammerer, Paul; *Smithsonian Institution Annual Report, 1912,* pp. 421-442.

I shall now cite a series of examples in which the transmission of an intentionally produced variation in higher plants and animals has been demonstrated. The offspring necessary for these demonstrations were produced by the usual bisexual methods--that is, through the fertilization of an ovum; both parents may have been subjected to the change-producing environment, or only one of the parents may have been changed, the other being normal. The nature of the experimental variation was as variable as the organism in which it was produced--small and large butterflies, flies, beetles, water and land salamanders, frogs, toads, lizards chickens, dogs, guinea pigs, rabbits, rats, and mice. Various kinds of grain and higher flowering plants have yielded positive answers in our experiments: to say nothing of the lower plants or animals, the bacteria, yeast, and smut fungi, the algae, and flagellates. The characters which could be changed or newly formed in the higher organisms include size, form, color, development stages, habits of locomotion, food, reproduction, and nidification. Of all these groups of acquired changes, I can give only one or two examples. In many instances several of the mentioned groups of variations are combined in a single case, so that in spite of all the necessary concentration, quite a comprehensive survey of the field will result.

. . . .

And now to the experiments themselves, for I intend to give you a somewhat more complete account of my rearing of the midwife toad (Alytes obstetricians). In order to understand this, it becomes necessary to say a few words about the ordinary

method of reproduction in most of the toads and frogs. They deposit their small eggs, hundreds in number, in water. The eggs are surrounded by a jelly-like substance that unites them into bunches or strings. Here the jelly-like covering swells at once about each dark-colored egg as a sharply-differentiated translucent ball. The eggs remain without attention from the parents after deposition. The young escape from the eggs. The free larvae, so-called tadpoles, which at first are not provided with any special breathing apparatus, for they breathe with the outer skin, develop external gills after a few days, which are later retracted and give way to inner gills. But still for weeks the larva remains without feet. It develops first the posterior, then the anterior extremities, after which the tail becomes shriveled and the narrow, horny jaw is replaced by the deeply cut mouth of the frog. Then the small complete frog jumps to land.

There is only a single exception to this rule in Europe, the so-called midwife toad, or egg-bearing toad. In this the deposition of eggs takes place on land and a comparatively small number of eggs (18-83) are produced, but which, on account of their great yolk mass, appear large and light colored. The jelly capsules which connect these eggs into a chain can not swell in the air, but, on the other hand, become contracted and fit closely to the surface of the egg. The male assists the female during oviposition by drawing the string of eggs from the cloaca. He also assists in the brooding of the eggs, winding the eggs about his thighs and carrying them about in this manner, until the young are ready to emerge. At this time the male with his burden enters the water, where the larvae break their capsules. They do this not in the stage unprovided with special breathing organs, for this and the following stage, that with external gill, are passed over in the egg. The larva is still footless, but has internal gills. The succeeding developmental stages agree with those of other frogs and toads--two-legged, four-legged, shriveling of the tail, and habitat change from water to land in the completed toad.

I was able to change the above-mentioned process considerably in four directions. In the first, the aquatic existence of the larva was much prolonged. I gradually learned to what extent factors like darkness, cold, richness of oxygen in the water, overfeeding after previous starvation, and the early removal of the embryo from the egg, played in prolonging the metamorphic period of the toad. With each one of these factors I obtained larvae which did not transform at the proper time, and which already in the larval stage attained considerable size, still, what is most important, developed into adult toads at the advent of the reproductive period. The progeny resulting from these toads, in which the metamorphic changes had been delayed, underwent metamorphosis within the usual normal time. They therefore had failed to inherit the varying developmental process. It required the combination of all the above-mentioned factors to produce a sexually fertile toad larva. Their progeny, although produced by the mating of the unique sexually fertile female larva with an ordinary completely developed mate, for years did not progress beyond the stage with

developed hind legs and displayed little metamorphic energy.

Secondly, the independence of water, which is already expressed in the normal reproduction of the midwife toad, by the phases passed over out of water during development was pushed to the limit. If one hastens all the processes of development by the employment of warmth and retards the hatching by withdrawing light and placing them in comparatively dry surroundings, then one obtains gigantic eggs in which the embryos remain until they have well-developed hind legs. The toads developed from these are dwarfed and eggs laid by them are of lesser numbers than in the normal toad, and possess from the very beginning a great amount of yolk, much more than in the usual eggs, and it is a curious sight to see a dwarfed male perform his brooding function with the very large few eggs. If one continued to apply the same stimuli to the eggs, then one again obtained larvae, with completely developed hind legs; but if one transferred them into normal conditions as far as requirements of temperature, light, and moisture are concerned, larvae were obtained, which at hatching possessed bud-like hind legs.

Thirdly, one can develop the larvae to the two-legged stage, away from water, simply on moist ground; in this, if danger of death threatens, one must return them to their normal element. The land larvae possess a thicker skin than the aquatic larvae, which is easily observed by the fact that in the aquatic larvae only the rump musculature is visible through the skin. The land larvae possess also a narrower fin, but are stronger in the bones and musculature of the tail. The lungs are subjected to a curious modification in the aquatic larvae; they are simple, smooth-walled tubes; in land larvae of the same age they have already been separated into lobules, alveoli, and sacs, which approach, both in form and structure, those of the completed toad. The toads developed from terrestrial larvae are dwarfed. If one keeps the larva produced by these again out of water, then the ability to exist out of water is increased. A further stage is therefore reached than in the preceding generation, which extends to the time when the fore limbs are about to erupt, and all adaptations to a terrestrial existence now appear emphasized.

The fourth cycle of adaptation and inheritance. If one keeps gravid midwife toads in a high temperature (of 25° to 30°C), then they omit the brooding stages above described and return to the primitive methods of reproduction peculiar to the rest of the toads and frogs. The unusual heat forces the animals to seek coolness in the water basin, which is at all times at their disposal. Here the sexes meet and fertilization and oviposition take place. But the moment the gelatine capsule of the egg comes in contact with the water, it swells and loses its viscosity and therefore its property to draw itself tightly about the thighs of the male later upon drying. The male is therefore unable to fasten it to its posterior extremities. The string of eggs therefore remains in the basin, in which a few of them develop inspite of the changed conditions.

In the same proportion in which the seeking of the water and the completing of the reproductive processes without brooding

become a habit so that the animals even without the stimulus of high temperature conduct themselves in this manner, do certain changes occur in the eggs and larvae which correspond to a closer approach to the original methods of reproduction of toads. The number of eggs and their ability to develop in water becomes decidedly increased. The aquatic eggs possess a lesser amount of yolk than the terrestrial eggs, and are therefore smaller and different in color, but owing to their swollen gelatine layer, they appear just as large as formerly. From these eggs emerge larvae which belong to an earlier stage than those normally produced, representing an intermediate stage between this and that of the rest of the toads. They possess external gills, of which the midwife toad has only a single pair (the anterior). Toads produced from such larvae are distinguished from normally produced individuals by being considerably larger.

In order to test the inheritance of these reproductive adaptations I permitted aquatic eggs derived from animals which had become accustomed to this mode of oviposition to develop under normal conditions, in a room in which the control animals are kept and in which these have kept normal. If the reproduction adaptation had become a changed instinct, then the transmission left nothing to be desired in the way of distinctness. The sexually matured young midwife toads sought the water with the beginning of their first reproductive period and deposited there their strings of numerous small, dark-colored eggs without bestowing any additional attention upon them. The aquatic eggs of later generations are still smaller and possess still thicker investments, the additional gelatine being obtained by a shortening of the spaces between the eggs in the string. The larvae of later generations, derived from aquatic eggs show an increase in dark pigment, a reduction of the yolk to its complete retrogression as well as changes in the gills, which become shortened, simplified, and coarser, and while usually only the first of the gill arches of the skeleton bears a gill, these appear on three of the free arches in the fourth generation. The males, probably on account of the difficulty of clasping the female in the water, have developed as an adaptation coarse swellings on their thumbs, and also strengthened the musculation of the arms, which lend the forelimb a more inwardly flexed appearance. These are external sexual characters which hold good in all toads and frogs that mate in water, but which are not normal, for the midwife toad normally mates on land. (pp.430-434)

THE INHERITANCE OF ACQUIRED CHARACTERS

MacBride, E. W.; *Science Progress,* 15:392-405, 1920.

But an ounce of fact is worth a ton of theory, and the answer of Weismann and the Mendelians to the supporters of the Lamarckian doctrine has always been that no experimental evidence has

been forthcoming to prove the effects of use and disuse can be inherited. Indeed Weismann has gone further, and has claimed to have produced evidence that these effects cannot be inherited.

This evidence is not a little extraordinary. If we turn to his Essays on Heredity (English translation by Poulton, Schonland, and Shipley) we read: "It can hardly be doubted that mutilations are acquired characters...they are merely the reaction of the body to external circumstances." He then proceeds to recount how he cut the tails off a series of white mice, bred from them, found that the young were born with tails, and triumphantly concludes that he has proved that "acquired characters" cannot be inherited. It seems incredible, but it is, nevertheless, true that on the foundation of childish experiments like these are based the confident assertions in many textbooks (especially American), that Weismann had demolished the case for the inheritance of acquired characters. Comment is superfluous!

During the last ten years, however, Kammerer, working under Przibram in the Institute for Experimental Zoology in Vienna, has carried out a series of experiments which, if confirmed, settles the question of the inheritability of acquired characters for ever in the affirmative. Naturally his results have aroused a storm of opposition, but his most keen sighted opponents have seen clearly that the results are far too clear to allow of the supposition of genuine mistakes in interpreting the evidence. Either Kammerer has proved his point, or else he is acting in scandalous bad faith, and this second alternative, more or less concealed in diplomatic language has been adopted by many.

The subjects of these experiments have been chiefly Amphibia. A few of them may be briefly outlined here. There exist in Europe two salamanders, one the yellow and black form (Salamandra maculosa), which inhabits the lowlands, and the other a black species (Salamandra atra), which lives at high altitudes. Both are viviparous; but S. maculosa gives rise to 30-40 gilled young, which live in water for six weeks before losing their gills and metamorphosing into the land form. S. atra, on the other hand, gives birth to only two young, which are at birth land animals, and ready at once to take up the parental mode of life. But if we cut open a pregnant black salamander, we find inside her at least a dozen embryos. Of these, however, only the two situated furthest back and nearest the openings of the oviducts are destined to survive; all the rest in the course of development degenerate into a kind of soup, which is devoured by the hindermost and serves to feed them. These two lucky embryos have long gills, but these gills are absorbed before birth.

Now, Kammerer asserts that, if S.atra be gradually accustomed to live under warmer and moister conditions, she will begin to produce first three and ultimately four young at a birth, that these young will enter the world at an abnormally early period of development--even before the gills are fully absorbed; that if these young be reared to maturity under the same conditions, they will give rise to still more young at a birth, and these young will be provided with gills, and will take to the water--in a word, that S. atra can be induced to assume the habits of

S. maculosa, and that these habits will be transmitted to posterity.

Some of Kammerer's critics, while accepting this result, have maintained that it is not a genuine case of the inheritance of acquired characters, because the habits to which S. atra reverts under the influence of more favourable conditions are the ancestral habits of the race. But this objection involves a confusion of thought. When a habit and the structures associated with it have completely vanished, if by exposure to different conditions the old habit is reacquired; and descends to subsequent generations, the case is just as genuine an instance of the inheritance of an acquired character as if the new character were one totally foreign to the past experience of the race. It is a superstition to think that evolution is bound to go forward; it may go backward as well. But Kammerer has a neat way of outflanking this silly objection. He has performed the reverse experiment. If S. maculosa be subjected to conditions of increasing cold and dryness, it produces fewer at a birth, and these are born in a more advanced stage of development. In three generations a state of affairs is reached when only three to four are born at one time, and in these the gills are mere stumps, the gill clefts closed, and the animals can at once take up their life on land; in a word, S. maculosa has acquired the habits of S. atra. Now, if S. maculosa be taken as representing the ancestral condition of S. atra, S atra cannot be ancestral to S. maculosa.

Another experiment of Kammerer's which has raised an enormous amount of discussion has for its subject the midwife toad Alytes obstetricians. Most toads, though they spend a good deal of their time on land in cool damp spots, nevertheless resort to the water in order to pair. Here the male embraces the female round the waist, maintaining his hold on her by means of a roughened horny pad situated on the ball of the apparent thumb. (The real thumb is absent; the apparent thumb is the modified index finger.) After a period of sexual enjoyment which may extend over weeks, the female emits the eggs, which issue from her in two long strings, held together by a clear jelly-like substance. As the eggs appear, the male fertilises them by emitting the spermatozoa; the fertilised strings sink to the bottom of the water, and in due time small tadpoles hatch out, each provided with three feather-like gills at the side of the neck. As the tadpoles grow, a fold of skin, termed the operculum, grows back from the head and covers up the gills, and the body of the tadpole then assumes the round plump form familiar to all of us. But Alytes differs from all other toads in the fact that it pairs on land; and as the skin of the female is comparatively rough and dry, the male does not develop the horny pad on his hand, since he can hold her without it. The eggs are much larger than those of ordinary toads, and fewer in number. As they are emitted and fertilised, the male winds the egg-strings round his legs and hops away encumbered with them. Some weeks later he visits the water, and the eggs are hatched; tadpoles emerge which have already covered up their gills, and resemble the later stages of the tadpoles of other toads.

Now, Kammerer found that, if Alytes was accustomed to warm

dry conditions, it would still flourish if a tank was provided in which the toads could bathe themselves when they so desire. Under these circumstances they begin to pair in the water, and the egg strings become slippery and fall off the male's legs and lie in the water. Most of them perish, but if the water is kept perfectly sterile, a few will survive. These, when they reach maturity, will pair in the water--and the eggs produced by the female will be much smaller than normal, and more like those of ordinary toads, and the tadpoles will emerge in an earlier stage of development, showing the external gills.

Of these, however, there will only be one on each side--and only one external gill is found in the embryos of the normal Alytes, if the eggs are opened before hatching. If, however, a third generation of Alytes is raised under these conditions, eggs will be produced from which tadpoles will hatch out showing three external gills on each side as in ordinary toads; and these eggs when reared will yield males with horny patches on their hands. As such Alytes had never previously been recorded, this point was fixed on by Dr. Bateson, the leading English Mendelian, to test the reliability of Kammerer as an investigator. He visited Kammerer's laboratory and demanded to be shown these abnormal toads, and this wish Kammerer was either unwilling or unable to comply with. Hence Dr. Bateson felt justified in regarding these results with great suspicion; this was a few years before the war. In 1919, however, Kammerer published a further paper on the subject, giving the results of renewed experiments. He succeeded in rearing these modified Alytes through six or seven generations, and found that the horny pad increased in size and definiteness up till the fifth generation, after which it remained stationary. He explained his inability to satisfy Dr. Bateson by saying that, as in ordinary toads so in the modified Alytes, the horny pad was only developed in the breeding season, and was a temporary phenomenon. He said that in this, as in other experiments, he had had to decide whether to retain his specimens alive for further work, or to sacrifice them in order to satisfy objectors, and that he had decided on the former course.

Since publishing this paper, however, Kammerer has sent sections through the skin of the hand of the abnormal Alytes to Dr. Bateson, and these Dr. Bateson has been good enough to show to the author of the present article, and in these sections the horny pad with its prickles can be clearly made out.

It will occur to the reader that it is of cardinal importance to the theory of heredity that these experiments should be repeated, and indeed it was partly with the hope of stimulating naturalists everywhere to undertake this task that this article was written. But the difficulty of such work should be clearly realised. It is necessary to find an animal that will respond to a change of environment by a change of habit or structure, and this is by no means easy. If, however, this difficulty is surmounted, and the animal is reared to maturity under the new conditions, and is induced to breed, then the young have to be divided into two lots, one portion being allowed to remain under

the new conditions, and the other portion restored to the old conditions. It most frequently happens that those offspring which are thus replaced in the ancestral environment lose the modification produced by the new conditions, and this is often triumphantly referred to by Mendelians as a proof that the modifications was merely "somatic," and not "germinal." But this reasoning is fallacious. If the constitution of the animal is so labile that a change from condition A to condition B produces a certain modification, the reverse change from condition B to condition A should undo it. The utmost that we could expect to find--and what, indeed, Kammerer claims to have found in his salamander experiments--is that the offspring of the modified parents, when replaced in ancestral conditions, should in their early development still show traces of the modification (this is the essence of "recapitulation"), and that young exposed to the further action of the changed environment should exhibit the modification in an intensified degree.

In the Zoological Gardens in London attempts are being made to repeat Kammerer's experiments with the salamander. These experiments were begun a year ago, but we must wait between two and three years before the young exposed to the altered conditions become sexually ripe, and three or four years must elapse subsequent to this event before it is possible to determine whether the altered condition is transmitted to the offspring. Meanwhile, new and startling evidence of the inheritability of acquired changes has come in from a totally new quarter.

Two American observers (Messrs. Guyer and Smith) have published in the Journal of Experimental Zoology (vol. iii, 1920) an account of their experiments on rabbits. They took the lenses of the eyes of rabbits, pulped them in Ringer's solution, and injected small portions of the resulting fluid into domestic fowls. After the lapse of a few weeks the serum of the fowl's blood developed an "antibody," which tended to dissolve and disintegrate the lens of the rabbit. Small quantities of this serum were then injected into pregnant rabbits. The mothers were unaffected, but some of the young which they bore showed on one or both sides of the head diminished or completely aborted lenses and correspondingly developed retinae, for, as we have already seen, the development of one of these elements of the eyeball depends on that of the other. Many of these young with imperfect eyes died, but some survived, and these mated together, gave rise to young some of which showed the defect, and this defect was observable through six generations, without any further injection of serum. Two instances also of inheritance through the male alone were noted--that is to say, that an affected male mated with a normal doe gave rise to young some of which showed the defect.

Messrs. Guyer and Smith say: "It is a noteworthy fact that once the defects were established, without any subsequent treatment, they became more and more pronounced in successive generations." The authors suggest that "the degenerating eyes are themselves directly or indirectly originating antibodies in the blood-serum of their bearers--which in turn affect the germ-cells."

If this conclusion be accepted, the cardinal principle of the

theory of the inheritability of acquired characters, viz. the influence of the soma on the germ, is conceded.

We conclude, then, by saying that a very strong prima facie case for the inheritability of acquired characters has been made out, and that no serious efforts have yet been made to combat the evidence. We are entitled to use the principle of use-inheritance in endeavouring to explain the facts of Palaeontology and Embryology, and when we do so, we find a flood of brilliant light thrown on these subjects, and a whole variety of puzzling phenomena become susceptible of rational explanation.

THE LATE DR. KAMMERER

MacBride, E. W.; *Science Progress*, 22:305-307, 1928.

The tragic death of Dr. Paul Kammerer of Vienna last September was one of the saddest events which has happened in the realm of zoology for many years.

As readers of Science Progress know, Dr. Paul Kammerer was celebrated for a series of brilliant researches on the adaptation of lizards and amphibia to changes in their surroundings, which were carried out over a long period of years in the University of Vienna previous to the War. Dr. Kammerer asserted that he had found that changes in habit and skin colour produced in young animals by long-continued exposure to altered environment were hereditary, and that the century-old problem of the inheritability of acquired characters was thereby solved.

This assertion of Dr. Kammerer led to his being assailed with a storm of abuse and vilification by the followers of Weismann and Mendel, and those who for no obvious reason dubbed themselves "Neo-Darwinians," oblivious of the fact that Darwin himself had admitted the inheritability of the effects of use and disuse. In this conflict the late Dr. Bateson in England and Dr. Baur in Germany took leading parts.

The War put an end to Dr. Kammerer's experiments, but as soon as the War was concluded he published a paper replying to his opponents (1919). The University of Vienna was terribly impoverished by the War and by the wasteful socialistic government which succeeded to it, and Kammerer's type specimens, which were deposited in the Museum, were allowed to macerate and become ruined.

One of Kammerer's results which attracted the special interest of his critics, though he himself regarded it as of no particular importance, was his reproduction in the male midwife toad (Alytes) of a horny nuptial pad on the hand similar to that found in normal toads from which Alytes is undoubtedly descended, but this pad is never found in normal Alytes.

In 1923 the enthusiastic younger naturalists of Cambridge collected funds and induced Dr. Kammerer to visit England. He lectured before the Cambridge Natural History Society and

the Linnean Society, and he brought with him such of his specimens, including a male Alytes, as had survived the War. These specimens were exhibited in Cambridge and in London and demonstrated by Dr. Kammerer. Dr. Bateson received a pressing invitation to visit Cambridge and see them, which he contemptuously refused. He did see them in London, but he made no request to Dr. Kammerer to discuss them or to demonstrate them, and after Dr. Kammerer's departure to Austria he published a bitter attack on Dr. Kammerer in the columns of Nature. It is, however, not too much to say that every fair-minded person who attended the long demonstration of these specimens in Cambridge was convinced of the uprightness of Kammerer and the bona fides of his experiments, whether his conclusions were justified or not. In 1924 Dr. Kammerer toured in America, and his reception by the Mendelian School there was described to me by the Nestor of American Zoology as disgraceful. When he returned to Vienna he was utterly unable to support himself on the meagre salary allowed by the University, so he resigned and supported himself by journalism and popular lecturing.

In 1925 a prominent light of the American Neo-Darwinian School, Dr. Noble of New York, visited Europe and attended the meeting of the British Association at Southampton. There he attacked Dr. Kammerer, introducing into his speech statements about the nuptial pads of Amphibia which were certainly new to his hearers and which would not be accepted by Amphibian experts on this side of the Atlantic. In 1926 he went to Vienna and obtained From Dr. Kammerer permission to examine his Alytes and found that the specimen had been "doctored" with Indian ink. Dr. Noble's description left no doubt in the mind of anyone who had seen the specimen in 1933 that it had been tampered with since its return to Vienna. The horrified Dr. Kammerer, when he heard Dr. Noble's report visited the Museum and found not only that his Alytes had been interfered with, but that may other of his precious and irreplaceable type-specimens had been similarly treated. In a fit of deep depression, concluding as he expressed it in his last letter, that his life's work had been destroyed, he ended his life by shooting himself in one of the parks of Vienna.

This tragedy was but the culminating point of a load of discouragement and depression resulting from the years of abuse and misrepresentation which he had endured. In a letter to me written some months before he expressed the view that his work would receive recognition only long after he was dead. But the irony of fate was that it occurred just after he had been appointed Professor in Moscow, and his conclusions as to the inheritability of acquired characters had been confirmed by the results of Durkhen, Pavlov, McDougall, and Heslop Harrison. His last work, published in the spring of 1926, on the causes of variation in the island races of lizards in the Adriatic, is one of the finest contributions to the theory of evolution ever written, and deserves to rank with the Island-life of Wallace.

Dr. Hans Przibram, head of the Institute of Experimental Biology in Vienna, who was his teacher and under whose supervision his experiments were conducted, has and still does uphold his

reliability and bona fides, and indeed is endeavouring to get up a fund to perpetuate Kammerer's memory. In a letter to me Dr. Przibram states that Kammerer was worn out by the campaign against him. This campaign is a proof, if one were needed, that once a popular biological theory has hardened into a dogma, opposition to it awakens a quasi-theological fury, and that scientific men in this respect are no better than their religious opponents whom they so often bitterly condemn.

UNUSUAL PHYSICAL ABILITIES

• Frog Navigation Capabilities

HOMING INSTINCT OF FROGS

Anonymous; *Science News Letter,* 61:130, 1952.

Illinois Natural History Survey biologists in Urbana have found some correlation between a well-developed homing instinct and size and sex of bullfrogs from a 10-year study made at 18-acre Ridge Lake, near Charleston, Ill. Frogs captured around the lake shore were marked and released at the Natural History Survey laboratory pier. More of the large than of the smaller-sized frogs returned to their original area of capture. Two-thirds of the frogs, all females, demonstrated a good homing instinct. Of four bullfrogs showing poor homing tendencies, three were males.

• Extraretinal Light Detection

EXTRARETINAL LIGHT PERCEPTION .....

Underwood, Herbert, and Menaker, Michael; *Science,* 170:190-192, 1970.

Abstract. The circadian activity rhythm of the iguanid lizard Sceloporus olivaceus can be entrained by light cycles whether or not the animals have eyes. Removal of the pineal organ and parietal eye in blinded lizards does not prevent entrainment. Our data demonstrate the existence of an extraretinal photo-receptor which can mediate entrainment of a biological clock in reptiles.

CURIOSITIES OF BEHAVIOR

• Unusual Snake Congregations

SERPENTS IN A PILE IN SOUTH AMERICA

Anonymous; *Scientific American,* 3:147, 1848.

In the Savannahs of Izacubos in Guiana, I saw the most wonderful, and most terrible spectacle that can be seen; and although it be not uncommon to the inhabitants, no traveller has ever mentioned it. We were ten men on horseback, two of whom took the lead, in order to sound the passages, whilst I preferred to skirt the great forests. One of the blacks who formed the vanguard, returned at full gallop, and called to me--"Here, sir, come and see the serpents in a pile." He pointed out to me something elevated in the middle of the Savannah or swamp, which appeared like a bundle or arms. One of my company then said, "this is certainly one of the assemblages of serpents, which heap themselves on each other after a violent tempest; I have heard of these but have never seen any; let us proceed cautiously and not go too near." When we were within twenty paces of it, the

terror of our horses prevented nearer approach, to which none of us were inclined.

On a sudden the pyramid mass became agitated; horrible hissing issued from it, thousands of serpents rolled spirally on each other shot forth out of their circle their hideous heads, presenting their envenomed darts and fiery eyes to us. I own I was one of the first to draw back, but when I saw this formidable phalanx remained at its post, and appeared to be more disposed to defend itself than attack us, I rode around it in order to view its order of battle, which faced the enemy on every side. I then thought what could be the design of this numerous assemblage, and I concluded that this species of serpents dread some collosean enemy, which might be the great serpent or cayman, and that they re-unite themselves after having seen this enemy, in order to resist him in a mass.

BUNDLES OF SNAKES

L., E.; *American Naturalist,* 14:206–207, 1880.

The statements made by Humboldt as to the piles of snakes he saw in Guiana, can be verified here in our northern woods and swamps. I personally had the pleasure of observing it twice, both times very early in spring, and in locations which could be called wildernesses. I first saw such a bundle of snakes in the neighborhood of Ilchester, Howard Co., Md., on the stony bank of the Patapsco river, heaped together on a rock and between big stones. It was a very warm and sunny location, where a human being would scarcely disturb them. I reasoned that the warmth and silence of that secluded place brought them together. Some hundreds of them could be counted, and all of them I found in a lively state of humor, hissing at me with threatening glances, with combined forces and with such a persistency that stones thrown upon them could not stop them nor alter the position of a single animal. They would make the proper movements and the stone would roll off. All the snakes in this lump were common snakes (Eutoenia sirtalis L.). The second time I noticed a ball of black snakes (Bascanion constrictor L.) rolling slowly down a steep and stony hillside on the bank of the same river, but about two miles above Union Factory, Baltimore county, Md. Some of the snakes were of considerable length and thickness, and, as I noticed clearly, kept together by procreative impulses.

It is surely not agreeable to go near enough to such a wandering, living and hissing hundred-headed ball to examine the doings and actions, and search for the inner causes of such a snake association. As, furthermore, the localities for such mass-meetings of snakes are becoming rarer every year, and our rapidly increasing cultivation of the country must make it hotter for snakes everywhere, only a few naturalists could see such a

sight, even if they should look for it in proper time, which, as stated above, seems to be the first warm days in spring.

• Fascination of Reptiles and Amphibians

A PHYSIOLOGICAL PUZZLE

Mangold, Ernst; *Nature*, 94:338-339, 1914.

What the magicians did in ancient days before Pharaoh in the way of turning sticks into snakes has often been done since, or its converse of turning snake into stick, but it remains in great measure a physiological puzzle. If the cobra in its threatening attitude be deftly caught behind the head and gently pressed, it soon becomes stiff, and will remain so for a considerable time, either coiled up or drawn out straight. It had passed into the strange state of animal hypnosis. In 1646 the Jesuit father, Athanasius Kircher, described the famous experiment, "de imaginatione gallinae." He laid a hen on the table, held it firmly for a little, and drew a chalk line in front of its eyes, with the result that it remained as if in katalepsy. Czermak showed in 1872-3, that this could be done with many birds, and that the chalk line was quite unnecessary. The veteran entomologist, Fabre, tells us that he and his school companions used to put a whole flock of turkeys to sleep with their heads tucked under their wings. Animal hypnosis can also be induced in mammals (guinea-pig, rabbit, mouse, squirrel, bat, dog, cat), and this is usually effected experimentally by fastening them to a board and turning this suddenly upside down. Frogs are readily susceptible, and newts will also submit.

. . . . .

HYPNOTISM IN ANIMALS

Prentiss, D. W.; *American Naturalist*, 16:715-727, 1882.

Mesmerism, or more properly hypnotism, has been recognized under various names in the history of all nations.

The same influences which work the modern phenomena of hypnotism are undoubtedly identified with the manifestations of magic found described in ancient history. The magic of Zoroaster, the wonderful performances of the magi of the East--among the ancient

Persians, Hindoos and Egyptians--the spells and incantations of the Grecian and Roman oracles, the methods of divination, the remarkable feats of the snake charmers of India and Egypt, all belong to the same category.

And so also might we include the more recent wonderful manifestations of religious mania which swept Europe in the seventeenth century as an epidemic, known as the "dancing mania," and was literally a national calamity. In our own country it was represented at the close of last century by the witchcraft of New England, and still later within the memory of men and now living, by the "convulsive" and "laughing" mania among the Methodists--notably in the State of Kentucky, where it is said that on one occasion as many as 5000 persons in camp meeting were under the "influence" at one time. The victims fell in convulsions and soon passed into a state of ecstatic trance, and were laid out on the grass in rows to recover themselves.

. . . .

It is necessary to state that I have no pet theory of my own to propound or uphold, but it is my hope in reviewing and briefly analyzing the history of hypnotism in the lower animals, to develop facts known to naturalists that may have an important bearing upon the subject. The practice of magic on the lower animals has a somewhat parallel history in ancient nations to that already referred to in connection with man. All nations and tribes have their conjurors, more or less expert. Probably the most expert are the "serpent charmers" of India and Egypt. Of these mention is made in the most ancient writings as well as in modern books of travel. The serpent is the favorite animal on which to exhibit the influence of these charms--for what reason may possibly be explained by modern herpetologists. In Dr. Spry's "Modern India," published in 1837, is a description of the method of operating of one of these Indian magicians. He says: "An eminent physician, skeptical on this point (serpents and birds being drawn and held as by a charm), in company with other English gentlemen thus tested the fact. Taking a serpent charmer alone, they brought him to a distant heap of rubbish, and causing him to lay off all his raiment that there might be no deception practiced upon them, they watched his movements.

"Approaching the pile with a serpent-like hiss and nervous working of the features and limbs, which became more and more excited and violent, presently serpent after serpent of the most venomous kind, showed their heads and gradually moved towards their charmer, until reaching out his hand he took them as so many lifeless withes, and deposited them in his basket." Numberless attested incidents of a similar kind might be given, the operator winding the serpent about his neck and pressing coil after coil into his mouth, and rendering it rigid as a stick or pliant as a cord at pleasure.

In a report on the "Manners and Customs of the modern Egyptians," by E.W. Lane, 1836, is an almost identical account of snake charmers of Egypt, and their method of drawing serpents out from the houses.

In 1646 Athanasius Kircher, an Italian monk, described what

he termed the "Experimentum mirabile." It was an experiment which has since become sufficiently familiar to all of us, but which appeared to the old monk little less than miraculous.

He tied the feet of a hen together and laid her on the ground, where after cries and violent struggling she became quiet, "as if," says he, "despairing of escape through the fruitlessness of her motions, she gave herself up to the will of her conqueror."

Kircher then drew a chalk line in a diagonal direction from one eye to the other, loosened the ribbon, and the hen, although left perfectly free, remained immovable, even when he attempted to rouse it. Kircher believed that the hen thought the chalk line was a string by which it was bound as at the feet, and attributed its quiet state to this idea.

The most extended observations upon hypnotism in animals have been made by Czermak in the private physiological laboratory of the University of Leipsic. The results obtained were reported in two lectures delivered by him in January, 1873, and published (in translation) in Popular Science Monthly for Sept. and Nov., 1873.

Czermak dwells upon the unreliability of untrained observation in such matters, and says that the usual reports, while honest and technically true from the observer's standpoint, are in their conclusions generally false.

To such inaccurate reports he applies the term "events viewed unequally." From this view the "experimentum mirabile" of Kircher is characterized as inaccurate, it is an "event viewed unequally." Czermak repeated the experiment, tying the legs of the fowl, pressing it down upon its side and making the chalk mark in front of its bill. It laid quiet, panting just as Kircher has described. The chalk line was then dispensed with, and still the animal remained quiet; and finally the tying of the feet was left out, and still the same result. It was not therefore the imagination of the hen, produced by the chalk line, as Kircher supposed, that rendered the animal lethargic, but some other cause not yet explained.

A friend of Czermak's told him a story about mesmerizing crawfish; that by making certain passes in the direction of the body, the animal became stiff and soon stood on its head, and after a while by reverse passes it resumed its natural position and crawled off. Czermak was skeptical, but a capture from the neighboring brook dissipated his doubts. Just what has been described took place. Not only this but Czermak himself found he also possessed the same magic power over the Crustacean. He was not satisfied to stop here. Pursuing his experiments, he found that passes were not needed either to put the crawfish on end or to bring him down again. He obtained a basketful of the animals, turned them out on the table, stirred them up a little, and lo! all of them turned tail up and stood so for a short time, when they gradually descended and crawled away. It was further observed that the crawfish would remain motionless in any position in which forcibly held until struggling ceased. Czermak repeated his experiments with ducks, geese and swans with similar results; but whether he is justified in claiming, as he does, with the

positiveness of italics, "that he has proved the appearance of hypnotism in animals," I think is open to doubt, and it may be, in the light of the naturalists' knowledge, that even he has not viewed his "events equally."

. . . .

THE RATTLE SNAKE DISARMED BY THE LEAVES OF THE WHITE ASH

Woodruff, Samuel; *American Journal of Science,* 1:23:337-339, 1823.

Last evening while perusing your very interesting Journal, I found in Vol. iii, p. 85, a communication to you by Prof. Jacob Green, giving an account of a large quantity of rattle snake skeletons, found in a cave near Princeton College; Prof. Green closes his communication with a passing notice of a popular story, among the former inhabitants of that town, that the leaves of the white ash were obnoxious to those reptiles.

This brought to my recollection an occurrence connected with this subject, of which I was a witness, and now proceed to relate.

During the summer months of 1801, I resided in the north eastern part of the state of Ohio. Rattle Snakes were then very numerous in that region. I found the opinion universally prevalent among the inhabitants there, that the leaves of the white ash were highly offensive to the rattle snake. Several persons of respectability assured me that the rattle snake was never found on land where the white ash grows, that it was the uniform practice among hunters, as well as others, whose business led them to traverse the woods in the summer months, to stuff their shoes and boots, and frequently their pockets also, with white ash leaves, as a preventive of the bite of the rattle snake, and that they had never known or heard of any person being bitten who had used this precaution.

Sometime in the month of August, I went with Mr. T. Kirtland and Dr. C. Dutton, then residing at Poland, to the Mahoning, for the purpose of shooting deer, at a place where they were in the habit of coming into the river, to feed on the moss attached to the stones in the shoal water. We took our watch station on an elevated part of the bank, fifteen or twenty yards from the edge of the water. About an hour after we had commenced our watch, instead of a deer, we discovered a large rattle snake, which, as it appeared, had left his den, in the rocks beneath us, and was slowly advancing across a smooth narrow sand beach towards the water. Upon hearing our voices, or for some other cause, he stopped and lay stretched out with his head near the water. It occurred to me, that an opportunity now offered to try the virtues of the white ash leaves. Requesting the

gentlemen to keep, in my absence, a watch over our subject, I went immediately in search of the leaves, and on a piece of low ground thirty or forty rods back from the river, I soon found, and by the aid of my hunting knife, procured a small white ash sapling eight or ten feet in length, and with a view to make the experiment more satisfactory, I cut another sapling of the sugar maple, and with these wands returned to the scene of action. In order to cut off a retreat to his den, I approached the snake in his rear. As soon as I came within about seven or eight feet of him, he quickly threw his body into a coil, elevated his head eight or ten inches, and brandishing his tongue, "gave note of preparation" for combat. I first presented him the white ash, placing the leaves upon his body. He instantly dropped his head to the ground, unfolded his coil, rolled over upon his back, writhed and twisted his whole body into every form but that of a coil, and appearing to be in great anguish. Satisfied with the trial thus far made, I laid by the white ash. The rattle snake immediately righted, and placed himself in the same menacing attitude as before described. I now presented him the sugar maple. He lanced in a moment, striking his head into a tuft of the leaves, "with all the malice of the under fiends," and the next moment coiled and lanced again, darting his whole length at each effort with the swiftness of an arrow. After repeating this several times, I again changed his fare, and presented him the white ash. He instantly doused his peak, stretched himself out on his back, and writhed his body in the same manner as at the first application. It was then proposed to try what effect might be produced upon his temper and courage by a little flogging with the white ash. This was administered. But instead of arousing him to resentment, it served only to increase his troubles. As the flogging grew more severe, the snake frequently stuck his head into the sand as far as he could thrust it, seeming desirous to bore his way into the earth and rid himself of his unwelcome visitors.

Being now convinced that the experiment was a satisfactory one, and fairly conducted on both sides, we deemed it ungenerous to take his life after he had contributed so much to gratify our curiosity; and so we took our leave of the rattle snake, with feelings as friendly at least as those with which we commenced our acquaintance with him, and left him to return at leisure to his den.

• Toads and Hot Charcoal

TOADS AND RED-HOT CHARCOAL

Woodland, W. N. F.; *Nature,* 106:46, 1920.

Toads are associated with some wonderful myths, and my scepticism was naturally great when my friend Mr. H. Martin Leake assured me, while on a visit to Cawnpore in October of 1915, that toads would eat red-hot charcoal. An after-dinner demonstration, however, soon dispelled my doubts. Small fragments of charcoal heated to a glowing red were thrown on the cement floor in front of several of the small toads (usually Bufo stomaticus) which so commonly invade buagalows at that time of year, and, to my surprise, the glowing fragments were eagerly snapped up and swallowed. The toads appeared to suffer no inconvenience, since not only did they not exhibit any signs of discomfort, but, on the contrary, several toads swallowed two or even three fragments in succession. A probable explanation of the picking-up is that the toads mistook the luminous pieces of charcoal for glow-worms or fireflies, the latter being numerous in the grounds of the Agricultural College at Cawnpore in October; but this does not account for the swallowing of the hot particles--the absence of any attempt to disgorge. I repeated the experiment at Allahabad in August, 1916, with the same results (the toads even attempting to pick up glowing cigarette-ends), though I have never observed glow-worms or fireflies in Allahabad at any time of year.

The fact that some toads seized several hot particles in succession would seem to imply either that the heat was not felt (which seems incredible), or that the memory is entirely absent in toads; but since toads most certainly come to associate a given time of day with the supply of food, i.e. remember, this latter explanation seems to be equally incredible. The truth must be that the incentive to seize an object (a luminous point in this instance) usually associated with an insect is so strong that even acute pain is no deterrent when the experience is limited--the lessons of experience out of the ordinary require to be "burned" into the toad intelligence by sheer repitition, just as the imprisoned shark which repeatedly bruises its snout against the glass of its tank has the lesson "knocked" into it in time. I may add that I unfortunately neglected to examine the toads post-mortem, and that I have recently repeated these experiments with Bufo vulgaris in England with entirely negative results.

• Pretense of Death

TRAGIC DEATH FEINT OF A SNAKE

Bartlett, W. E.; *Nature*, 106:503, 1920.

On Sunday morning, May 30, about 10 o'clock, I noticed a common western hog-nosed viper, about 20 in. in length, basking on the lawn in the warm sunshine. I approached the serpent in company with a friend to make some investigation of it, and only to interfere with it enough to keep it from crawling away. The creature went through the usual feint of being a dangerous snake that is peculiar to this species, and quickly began to coil and recoil and to hide its head under its body. After it had done this a short time it turned on its back, but continued to writhe as though injured severely. Gradually it assumed a position simulating that of a dead snake lying on its back, with its mouth completely inverted and bleeding. This was done in such a way that the head appeared to be completely mashed or severed. The exudate of blood from the entire surface of the mouth was perfect. It was the most complete and well-carried-out feint of a tragic death that I have ever witnessed, and all without the least torture or stroke of any kind from me. I only detained the snake by placing my foot in front of it and turning it back once at the beginning. We left the creature in this apparently killed condition, only to see that is disappeared in a very short time.

My observation of this genus Heterodon (hog-nosed viper), which is not a viper at all, has shown me that it always puts up the tamest kind of bluff before hiding its head, but never before have I observed this complete performance with a bloody exudate from the inverted mouth. I am convinced that it will not often be carried out thus completely unless the conditions of season, the weather, and the development of the snake are just right. In other words, I think that it must be a peculiarity of some maturity of growth, and that the full vigour of a warm day in late spring or early summer must enter into it.

• Might Snakes Swallow Their Young?

A FEMALE ADDER SWALLOWING HER YOUNG

Bond, Henry; *Zoologist*, 18:7278, 1859.

Walking in an orchard near Tyneham House, in Dorsetshire, I came upon an old adder basking in the sun, with her young around her; she was lying on some grass that had been long cut, and had become smooth and bleached by exposure to the weather. Alarmed by my approach, I distinctly saw the young ones run down their mother's throat. At that time I had never heard of the controversy respecting the fact, otherwise I should have been more anxious to have killed the adder, to further prove the case. As it was she escaped, while I was more interested in the circumstance I witnessed than in her destruction.

NOTE ON YOUNG VIPERS ENTERING THE MOUTH OF THE PARENT REPTILE

Gurney, J. H.; *Zoologist*, 21:8856-8857, 1863.

The late John Galley, shepherd, of Cossey, in Norfolk, related the following anecdote to his son, who repeated it to me, and having full confidence in my informant's accuracy, I have taken down his statement, which is as follows:---John Galley saw a viper at Swannington, in Norfolk, surrounded by several young ones; the parent reptile, perceiving itself to be observed, opened its mouth, and one of the young ones immediately crept down its throat; a second followed, but after entering for about half its length, wriggled out again, as though unable to accomplish an entrance. Upon this Galley killed and opened the viper, and found in the gullet, immediately behind the jaws, the young one which he had seen enter, and close behind that a recently-swallowed mouse. Galley was of opinion that the first young viper which entered was unable to pass the mouse, and that consequently there was not sufficient room for the second young one; which endeavoured unsuccessfully to follow in the wake of the first.

ON THE QUESTION "DO SNAKES SWALLOW THEIR YOUNG?"

Goode, G. Brown; *American Association for the Advancement of Science, Proceedings,* 22:176-185, 1873.

It has long been a popular belief that the young of certain snakes seek temporary protection from danger by gliding down the open throat of the parent. This has been doubted by many naturalists, and the general disposition has been to class the belief among the popular superstitions. Ths paper is intended to sum up the evidence, which will show, it is hoped conclusively, that the popular idea is sustained by facts.

. . . . .

In Browne's "Vulgar Errors" may be found the following account of the Viper:---"For the young ones will upon any fright for protection run into the belly of the Dam; for then the old one receives them in at her mouth, which way, the fright being past, they will returne againe; which is a peculiar way of refuge, and though it seems strange is avowed by frequent experience and undeniable testimony."

Gilbert Wite refers to the prevalent belief in this habit of the viper, and though rather inclined to favor it, he is evidently shaken in his faith by the adverse testimony of the London viper-catchers.

M. Palisot de Beauvois, an eminent French naturalist, published in 1802 some very important observations on the rattlesnake, which will be quoted hereafter.

S. John Dunn Hunter, an early traveller in the United States, says:---"When alarmed, the young rattlesnakes, which are generally eight or ten in number, retreat into the mouth of the parent and reappear on its giving a contractile muscular token that the danger is past." A few years later a long discussion occurred in the "Gardener's Chronicle" which, however, reached no satisfactory conclusion.

In a note to the eighth edition of "Selborne," Sir William Jardine says:---"The question remains, we believe nearly as it did in White's time. The supposed habit is so much at variance with what we know of the general manners and instincts of animals, that without undoubted proof of its occurrence we are inclined to consider it as a popular delusion."

In 1865 Mr. M. C. Cooke, editor of "Science Gossip," made a strong argument in the affirmative.

Mr. F. W. Putnam published in the year 1869 a very thorough discussion of the question. He speaks of it as still unsettled and, though sympathizing fully with Mr. Cooke, asks for additional proof.

During the past year an animated discussion has been carried on in the London "Land and Water." Mr. James Simson and others have argued for the affirmative but Frank Buckland, the editor, classes the belief among the numerous popular delusions and persistently refuses to believe until he or some other natural-

ist has personally investigated the subject.

The feeling of the majority of naturalists at the present time seems to be well expressed in these words:---"The cumulative testimony of many witnesses would compel us to receive this supposed habit as an established fact, did not experience warn us of the extreme liability of untrained observers to be misled by preconceived opinions. The fact that no competent naturalist has found young vipers in the stomach or oesophagus of the mother raises a strong presumption, on the doctrine of probabilities, of its being a mere delusion. The habit moreover would be contrary to the ordinary laws of animal instinct which lead both parent and offspring to adopt the best available means for the preservation of the race.

Theorizing upon this question has proved useless, and it is obvious that it can only be settled by the statements of persons who have seen the act. Believing that none would be so likely to supply the desired facts as those whose vocation brings them into daily contact with snakes in their native haunts, I wrote a short note to Mr. Orange Judd, Editor of the "American Agriculturist," which he kindly inserted in the issue of that magazine for February, 1873.

As a result over eighty letters were received, from persons in twenty-four states and provinces, almost every one containing valuable evidence. Many of the writers seem indignant that a fact so well known to them should be questioned. On the depositions of these witnesses, together with those collected by diligent personal inquiry, the case must rest.

A farmer living in Mechanicsburg, Ohio, writes:---"In 1835 I saw on the bank of Deer Creek a large water-snake. I procured a pole for the purpose of killing her. One stroke slightly wounded her and she immediately made for the water; after she had swam about her length she wheeled, placing her under jaw just out of the edge of the water, then opening her mouth to the fullest extent. Some dozen young snakes, three to four inches long then seemed to run or rather swim down her throat, after which she clumsily turned in search of a hiding place. I opened her and found about twenty living young snakes, two or three seven or eight inches long."

A gentleman in Georgetown, S.C., writes:---"I had for several days noticed a very large moccason coiled around the limb of a small tree near the pond. I concluded to capture it and accordingly procured a large rabbit and placed it some way up from the pond to toll her away from the water. She soon came down and disappeared under a large log; when next seen she was near the bait, having traced it along the log on its opposite side. When she had nearly swallowed the bait we made an advance; quickly disgorging it she gave a shrill whistling noise, and five young snakes ran from under the log and ran down the throat of the old one. We cut off her head and found the five young, which made efforts to get away."

A farmer in Rosendale, N.Y., writes:---"I was one day mowing and coming close to a smooth flat rock. I thought I saw as many as a dozen snakes on it. I ran for a fork which was standing

within a few yards and when I came back there was only one snake on the rock. I struck it on the back and seven snakes ran out of the mouth."

A letter from Chesterfield, N.H, says:---"I saw a striped snake on the hillside, and noticed something moving about her head, and counted twenty little snakes, from one and a half to two inches long. I made a move and the old one opened her mouth and they went in out of sight. I stepped back and waited and in a few moments they began to come out. Then I made for the old snake and killed her and forced out several."

A farmer in Newburyport, Mass., writes:---"Riding through a large corn field, in the centre of which was a large shelving rock I observed on the top a curious commotion, but on near approach found nothing. My curiosity was excited, and the next day I repaired to the spot very cautiously, and on the top of the rock saw an enormous striped snake sunning herself, surrounded by a bevy of young four to six inches long. After viewing them to my satisfaction I made a demonstration, and to my surprise the old snake opened her mouth very wide, the little snakes ran down her throat and then she disappeared in the shelving rock. I repeated the experiment a number of days to the same effect."

The total number of testimonies in my possession is one hundred and twenty. Sixty-seven witnesses saw the young snakes enter the parent's mouth; twenty-two of these heard the young warned by a whistle or hiss or click or sound of the rattles; five were considerate enough to wait and see them reappear when danger seemed over; one seeing the act repeated on several days.

Three saw young snakes coming out of a large one's mouth, and not having seen them enter were naturally much astonished. Five struck the parent and saw the young rush from its mouth; eighteen saw the young shaken out by dogs or running from the mouth of the dead parent. Thirty-six of those who saw the young enter the parent's mouth, found them living within its body. Only twenty of the sixty-seven allowed the poor, affectionate parent to escape. Thirty-three who did not see the young enter, found them living within the parent's body. Testimony of this character concerning the ovo-viviparous species is, however, to say the least, dubious.

It may be objected that these are the testimonies of laymen, of untrained observers, of those who might be influenced in their observations by their prejudices. I reply that the letters are from a class of well-informed farmers, mechanics and business men, intelligent readers of a practical agricultural magazine. The act of swallowing the young is of such a character as to admit little room for error in the observations, and I find that, as a general rule, opinions on the subject are current only among those who have had it brought to their notice by their own experience or that of their friends. Due weight should be given to the wide distribution of the witnesses, and the remarkable concurrence in their statements.

Let us not, however, trust entirely to the statements of the untrained observer. Says Mr. Cooke:---"Clergymen, naturalsts, men of science and repute, in common with those who make no

profession of learning, have combined in this belief." We add the statement of gentlemen, the accuracy of whose observations in other departments of natural history would surely not be doubted. Prof. Sydney I. Smith, of the Sheffield Scientific School, saw a ribbon-snake (Eutoenia saurita), about two feet long, accompanied by two young ones of three of four inches; on a hiss from the parent they disappeared down its throat. The parent was killed and two ran out of the mouth, while a third was found alive in the body. Dr. Edward Palmer, a well known traveller and collector, assures me that when in Paraguay with the "Waterwitch" expedition, he saw seven young rattlesnakes (Caudisona terrifica) run into their parent's mouth. After it was killed they all ran out. These snakes, parent and brood, are preserved in the U.S. National Museum, Washington.

Rev. Chauncey L. Loomis, M.D., of Middletown, Conn., a keen and enthusiastic observer, saw a black snake (Coluber, Allegheniensis?) open its mouth, allow seven young ones to enter and then glide away.

D.L. Phares, M.D., of Woodville, Miss., writes:--"A few years ago a gentleman, directing some hands at work on my lawn, heard a low, blowing noise, and on looking saw a large water moccason (Toxicophis piscivorus, I believe) and a large number of young hurrying to her head and disappearing so rapidly that he first thought they ran under her. He soon discovered that they went into her slightly opened mouth, which was held close to the ground till they had all entered. She then attempted to escape, but was cut in two with a hoe. We took from her a large number of young, eight or ten inches long."

I might take from Mr. Cooke's work several statements equally to the point. I quote from the "Zoologist" a note concerning the scaly lizard (Zootoca vivipara), which has an important bearing upon the question. Says the editor, Mr. Newman:--"My late lamented friend, William Christy, Jr., found a fine specimen of the common scaly lizard with two young ones; taking an interest in everything relating to natural history, he put them into a small pocket vasculum to bring home, but when he next opened the vasculum the young ones had disappeared, and the belly of the parent was greatly distended; he concluded she had devoured her own offspring. At night the vasculum was laid on a table and the lizard was therefore at rest; in the morning the young ones had reappeared and the mother was as lean as at first."

Mr. Putnam has kindly put into my hands a note from Thomas Meehan, of Philadelphia, containing strong affirmative testimony in the case of the English viper as observed by him in the Isle of Wight; also a note from Herman Strecker of Reading, Pa., who says:--"Some years ago I came across a garter snake (Eutaenia saurita) with some young ones near her. Soon as she perceived me she hissed and the young ones jumped down her throat, and glided beneath a stone heap. Another time I caught a snake of the same species, but as I thought of immense size, which I took home and put in a case; on going to look at her some short time afterwards I discovered a great number of young ones (about thirty if I recollect rightly) and whilst I was still looking at the

sudden increase, two more crept out of the old one's mouth, and finally after a little while a third one did likewise."

Prof. C.F. Brackett, of Princeton College, sends me a note which, besides throwing light upon the question under consideration, gives a very interesting instance of hereditary instinct: he writes:--"About twenty-five years ago I saw the following things. A workman who was mowing in my father's hay-field came upon a moist, moss-grown knoll, and his scythe cleft off a portion of the thick moss and sphagnum and revealed several (at least a dozen, I should say) small soft bodies which he declared to be snake's eggs. I at that time having no knowledge of such matters was incredulous, and proceeded to tear one of them open, when, to my surprise, there appeared a small, perfectly formed milk adder, which immediately assumed a pugnacious attitude, and brandished its tongue as defiantly as an old snake would have done. Other eggs were torn open with like results. Soon the old snake appeared and after endeavoring, apparently to encourage the young family, thus suddenly initiated into the world, it put its mouth down to the ground, and every one that had been liberated from the egg voluntarily and hastily disappeared within the abdomen of the old one (mother?). Last of all I put the point of a pitchfork through the old snake and fulfilled the scriptural injunction of bruising its head, when with a pocket knife I opened the abdomen and found the young ones still active."

The snake referred to by Prof. Brackett is apparently the common milk-snake (Ophibolus triangulum).

Col. Nicolas Pike, late U.S. Consul at the Mauritius, assures me that he has seen the garter-snake (Eutania sirtalis) afford its young family temporary protection in its throat, from which they were soon noticed to emerge.

Our last witness is one who appears to have been overlooked throughout this discussion, one whose statement, it would seem, ought of itself to have decided the question long ago. M. Palisot de Beauvois, an eminent French naturalist, member of the Institute and Councillor of the University of Paris, thus details an observation made near the close of the last century:--"When making my first excursion into the Cherokee country, I happened, while botanizing, to see a rattlesnake in my path. I approached as softly as possible, but, just as I was about to strike, imagine my surprise to see it, after sounding its rattle, open a very large mouth and receive into it five little serpents, each about the size of a goose-quill. Astonished at this singular spectacle I retired some distance and hid behind a tree. After some minutes, the animal, believing itself out of danger, again opened its mouth and allowed the little ones to escape. I advanced, the little ones retreated to their stronghold, and the mother, carrying her precious treasure, disappeared among the underbrush where I was not able to find her."

We have the opinion of Dr. Jeffries Wyman, Prof. Gill and other physiologists, that there is no reason why the young snake may not live for a time within the parent. It would be very difficult to smother a reptile, even in such close quarters, and lizards, toads and snakes have often been rescued, unharmed,

after a sojourn in a snake's stomach. It is a well known fact that living tissues are acted upon very feebly by the gastric juice.

The supposition that the serpents swallow their young for food is manifestly absurd, for the act is purely voluntary with the young snakes. If the habit is not protective in its design, it must be destructive to a degree that will in time exterminate the species which practise it.

An analogous case is found among certain South American fishes of the genera Geophagus, Arius and Bagrus, the males carrying the eggs in their mouths, depositing them in places of safety and removing them on the approach of danger.

I have been told of two instances where a large snake was found to contain one of smaller size, which in its turn had within it a number still more diminutive. This may be easily explained by supposing the parent snake, after affording the usual protection to its young brood, to have been swallowed by some hungry reptile of larger size.

The American Indians seem to have had some knowledge of this peculiar habit of the rattlesnake. Among the many legends collected by Maj. J.W. Powell, U.S. Geologist, in his researches among the Pai Utes, is one giving the origin of the echo. An old sorceress was suspected of wrong doing and was pursued by her enemies until in desperation she sought aid from her grandfather. "Takoa," the rattlesnake. His only resource was to open his mouth and allow the old witch to crawl in out of sight and out of danger. She was so well pleased with her safe retreat that she could not be induced to leave it, so the rattlesnake had to crawl out of his skin and leave her within. And there, say the Pai Utes, she remains to this day, and when any one calls she mockingly repeats their words from her hiding place in the cast off snake-skin.

This curious tradition, even if it cannot be counted as evidence, shows in an interesting way the wide prevalence of this belief.

There is much need of other observations, to determine what species of American snakes have this singular habit. Thirty-four of the observations relate to Eutenia; the habit is probably shared by all the species, but is only well attested for the garter snake (Eutaenia sirtalis) and the ribbon-snake (Eutoenia saurita). Seventeen refer to the water-snake (Tropidonotus sipedon). Nine refer to the banded rattlesnake (Caudisona horrida), two to the copperhead (Ancistrodon contortrix), three to the moccason (Ancistrodoa piscivorus) and one to the massauga (Crotalus tergeminus). Does the habit extend throughout the Crotalidoe? One instance is given for the blowing-adder (Heterodon playtyrhinos). Six relate to the so-called "black snake," but this name is too indefinite. With all deference to Mr. Buckland, I believe the case of the viper (Pelias berus) to be settled, as well as that of Zootoca. Whether the male snake ever protects the young in this way has not been observed.

It is a noteworthy fact, which may or may not prove an important one, that the snakes mentioned above are all ovo-vivip-

arous with the exception of Ophibolus. There is nothing to indicate that the habit is shared by the oviparous snakes of the genera Liopeltis, Cyclophis, Storeria, Diadophis, and Pityophis. the case of Bascanion, which is oviparous, is still quite problematical, and it remains to be shown whether the "black snake" of my correspondents is Coluber Alleghaniensis, or Bascanion constrictor. Mr. Gosse gives facts which make it seem quite probable that the Jamaica boa (Chilabothrus inornatus) may share the habit.

The breeding habits of North American snakes deserve careful investigation, as they are totally unknown in more than twenty-five of the genera.

• Gastric Brooding

GASTRIC BROODING: UNIQUE FORM OF PARENTAL CARE IN AN AUSTRALIAN FROG

Corben, C. J., et al; *Science,* 186:946-947, 1974.

Abstract: The recently described leptodactylid frog Rheobatrachus silus of Queensland, Australia, exhibits a unique form of parental care. The female carries embryos and young in the stomach, propulsively ejecting the juveniles.

• The Hoop Snake Story

THE HOOP SNAKE STORY

Schmidt, Karl Patterson; *Natural History,* 25:76-80, 1925.

One of the most persistent and widespread snake myths in the United States tells of a large serpent which takes its tail in its mouth and rolls like a hoop. It is further reputed to have a poisonous sting in its tail, which is launched at its enemy from the rolling position. This story has come to be associated with

various snakes in different parts of the country. My interest in the story was aroused during a stay in Louisiana, where I could gather eyewitness testimony regarding one of the "hoop snakes" (Abastor erythrogrammus) and the "stingin' snake" of the genus Farancia. It appears that the supposed habit of rolling like a hoop is an elaboration of the more fundamental belief in a snake with a poison sting in its tail.

My first impression was that this must be one of the universal snake myths, and I thought of the familiar "snake" bracelet or finger ring, in which the head of the snake meets the tail. On inquiry, however, there appears to be no classical or European analogue of the American hoop snake story. Herodotus, the earliest source of some still current misinformation concerning reptiles, is not the fountainhead of this legend, and it does not find place in Pliny, whose voluminous Natural History would surely have included a story so much in his own vein, had he known of it. My search of possible European sources was slight, but it gave no clue to a European prototype of the hoop snake yarn.

Turning to American sources, our search is at once rewarded by finding references to the horn or hoop snake in early accounts of travel in this country. These accounts may speak for themselves. The earliest is in a letter dated 1688 that was written by John Clayton to the Royal Society of London:

"There is another sort of deadly snake, the Red-Snake; I once narrowly escaped treading on the back of one of them. They are of an ugly dark brown Colour, inclining to red; their bellies are of a more dusky white, with a large streak of vermilion Red on either side; this too is of the Viper kind, but is not so short, but its tail is more taper and small. The Horn snake, is as they say, another sort of deadly snake; I never saw any of them unless once, shortly after my Arrival in that Country, which I cannot attest to being the Horn-Snake, for I could not distinctly view it, being in a thicket of sumach; it was perched up about two feet high in a Sumach Branch, its Tail twisted about the Shrub, and about a quarter of a yard stood bolt forward, leaning over the forked branch thereof: I could not see the Horne, with which it strikes, and if it wounds, is as deadly as the Rattle-Snake's Bite. The Gentleman that was with me told me it was the Horn snake; but being in hast, and on Horseback, and the Snake in a Thicket, I could not see the Horn; but had I thought I should never have seen more of them, I should have took a little Pains to have been better satisfied. This I think may not improperly be referred to the Dart Snake."

Here are a number of elements that to to make up a typical snake story. A description of the mud or rainbow snake (it is impossible to be sure which is meant); a "horn snake," with the horn in its "front," apparently one of the tree snakes, or possibly a black snake; and not least, the abundant excuses for not making a more thorough investigation.

The next account is that of Robert Beverly, in a History of Virginia published in London in 1722. He writes: "They have likewise the Horn snake, so called from a sharp horn it carries

in its tail, with which it assaults anything that offends it, with that Force, that as it is said it will strike its tail into the But end of a Musquet, from whence it is not able to did engage itself."

In this short note are two distinct additions that are familiar in later accounts: first, the transference of the horn to the tail; and second, the characteristic of striking with such force as to remain fast in the object struck. No mention is made of the horn being poisonous--an oversight which is supplied by our next reference.

Alexander Hewatt, writing in 1779, describes the fangs of the rattler and other genuinely poisonous snakes; he then goes on to say: "The horn snake is also found here, which takes his name from a horn in its tail, with which he defends himself, and strikes it with great force into every aggressor. This reptile is also deemed very venomous, and the Indians, when wounded by him usually cut out the part wounded as quickly as possibly so as to prevent the infection spreading through the body.

In this account the name of "Hoop snake" if reenforced, and the quality of venom added. This is the only reference to a belief in stinging snakes on the part of the North American Indians which has come to my attention. It would be highly interesting if these legends should be found in North American Indian folklore, and in this case my hypothesis, subsequently stated, of an African origin could be discarded.

The next account in our list is supplied by one J.F.D. Smyth, in 1784. As this is the first account--had almost said authentic account--which introduces the "hoop," I shall quote it in full. Referring to a stay in western North Carolina, he writes:

"While I was at Sawra Town, one day a little lad of Mr. Bayley's came to acquaint us that he had killed a horn-snake, which being a curiosity that I was extremely desirous of observing and examining with particular attention, I accompanied him to the place where he said he had left it but when we arrived there, to my great disappointment, it was not to be found. He assured me that it must not have been quite dead, and had recovered so much as to be able to crawl from the spot on which he had left it, and had secreted itself somewhere among the leaves.

"However, everyone, and all the inhabitants, with the greatest confidence asserted, and avowed their having seen such snake, though very seldom.

"They represented them to me as the most formidable and direful foes in existence to the human race, and to all animation; poisonous and fatal to a degree almost beyond credibility.

"He is described as something resembling a black snake, but thicker, shorter, and of a colour more inclining to dark brown. He never bites his adversary, but has a weapon in his tail, called his sting, of a hard horny substance, in shape and appearance very much like to a cock's spur: with this he strikes his antagonist, or whatever object he aims at, when he least expects it, and if it penetrates the skin it is inevitable and sudden death.

"So very virulent is his poison that it is reported, if he should miss the object he pointed at, and should strike his horn through the bark of a young sapling tree, if it penetrates into the sap or vital parts, the bark or rind will, within a few hours, swell, burst, and peel off, and the tree itself will perish.

"As other serpents crawl upon their bellies, so can this; but he has another method of moving peculiar to his own species, which he always adopts when he is in eager pursuit of his prey; he throws himself into a circle, running rapidly around, advancing like a hoop, with his tail arising and pointed forward in the circle, by which he is always in the ready position of striking.

"It is observed that they only make use of this method in attacking; for when they fly from their enemy they go upon their bellies, like other serpents.

"From the above circumstance, peculiar to themselves, they have also derived the appellation of hoop snakes."

This account may be considered the first in which the hoop snake myth appears in full flower. It is to be noted that the hoop snake story is added to that of the horn snake. It is difficult to avoid the conclusion that some village genius has invented the hoop snake and blended his creation with the horn snake, the reputation of whose venomous tail sting was a really current snake "myth."

In a View of South Carolina, by John Drayton, published in Charleston in 1802, appears a list of the snakes of South Carolina that includes the horn snake. Robert Mills, in Statistics of South Carolina, 1826, also mentions a species under this name.

John Lee Williams, in a View of West Florida, published in 1827, lists various snakes and writes: that "... a livid looking mud asp, that has sometimes been mistaken for an eel, has in several instances proved fatal to those who expose themselves by wading in muddy creeks." This looks very much like a reference to the horn snake under a new name, for this snake really lives in mud and its sides and belly are a "livid" red.

The single case of scepticism on the part of a person mentioning the horn snake that has come to my attention in works of the nature thus far quoted is that of J.H. Hinton. In his History and Topography of the United States, published in London, 1832, he writes that "The accounts of the deadly venom of the Horn-snake being without actual attestation by fact, are considered as unfounded."

There is no doubt that this list of references to the hoop snake could be much extended. I have not examined current sources, for there is no difficulty in gathering any number of contemporary accounts of the hoop snake and the horn snake from alleged eyewitnesses, especially in any one of the southern states. These accounts are not to be confused with yarns spun for the misinformation of the traveler. If one expresses doubt, he runs real danger of seriously offending the narrator; and the stories are attached to perfectly definite and well-known species of snakes, which are greatly dreaded and shunned. The first specimen of the "stingin' snake" that I collected, I brought in alive to a camp in Louisiana, and I had difficulty in convincing

my camp mates that it was not by virtue of occult power over snakes that I escaped the predicted death. I had already gathered accounts from a considerable number of eyewitnesses, of the death of various animals from the sting of the "stingin' snake," and when I confronted my informants with the living source of their fears, they were forced to make the difficult choice between their traditional belief and the evidence of their eyes and of common sense.

In the locality in question (Natchitoches Parish), it was Farancia abacura, the horn snake or mud snake of other sections, to which the stinging powers were attributed. There was no hoop snake story current in this section of Louisiana. Only one family, which had come from Georgia, knew of it. All the members of this family would have been glad to take oath to having seen the veritable hooping of the hoop snake in their native state. They apparently referred their experience to the rainbow snake (Abastor erythrogrammus), which in many parts of the South is known as the "hoop snake."

Both of these snakes lend themselves well to their legendary roles. They are large, brilliantly colored serpents, which because of their habits are very rarely seen, for they burrow in soil, mud or soil in wet localities, or frequent swampy areas which are sparsely inhabited. The terminal scale of their tail is considerably enlarged and is spinous or horn-like, so that the examination of a dead snake, the only kind ever examined, lends apparent support to the theory of a sting. Further support is derived from the actions of the living snake, though it is doubtful if the authors of bearers of the "stingin' snake" stories ever observed the reptiles alive. When held in the hand, the mud snake (which is the more familiar to me) coil around the hand, and explores or feels about with the tip of the tail with sufficient force to give a considerable prick, though I doubt if even a large snake would penetrate the skin with its tail spine. This is the normal, or slightly modified, habit of constricting snakes in general, which attempt to tuck the tail beneath a coil or otherwise secure a purchase for it, to enable them to constrict.

An account by T.G. Dabney, who was a good observer both of snakes and of human reactions to them, illuminates the problem of explaining the many eyewitness accounts. He writes concerning a specimen which was brought to him:

"It had just been killed, but had enough vitality for tail movements. It was carefully carried on a fire poker to the porch for good light. The poker was pressed on the tail, which set up a lively oscillation, and the observer distinctly saw a sting, protruded and withdrawn 'in a flash' but saw no repitition of the exposure. A dissection showed the tail vertebrae descending in a diminuendo to the fine pointed extremity of the tail, and no place for a 'sting.' This shows that we are very likely to see what we expect to see, when snakes are involved; and the average person is prone to accept first impressions, and any extravagant statement about snakes, without any inclination to verify, or disprove them."

The most pronounced development of a tail spine in snakes

that has come to my notice is that of some of the blind burrowing snakes of the family Typhlopidae. Living specimens held in the hand make the same exploration with the tail spine, which is very sharp, as I have described above in the case of the horn snake. Mr. Herbert Lang, leader of the American Museum Congo Expedition, informs me that the African natives believe that the tail of these burrowing snakes is used as a sting, and that these snakes are relatively abundant in Central and West Africa. It seems a plausible hypothesis, therefore, that the stories of stinging snakes were brought from Africa by the negroes imported as slaves. The transfer of this reputation from the burrowing snakes of Africa to American burrowing snakes offers no difficulty to anyone familiar with the permutations and combinations of popular names for animals.

The habit of the common black snake of eastern North America of gliding along at great speed over the tops of bushes, without descending to the ground, may have a bearing on the origin of the belief in the hoops snake's rolling method of progression. Where the horn snake and rainbow snake do not occur, there seems to be a tendency to identify the hoop snake with the black snake or blue racer.

The only remaining hypothesis for the origin of the stories of the stinging snake and the hoop snake that has come to my attention rests on the comparison with scorpions. Scorpions do have a veritable sting in their tails, and they do advance with the sting raised over the back "in the ready position of striking." The remoteness of the scorpions from snakes in zoological classification seems to offer little difficulty to the popular imagination. In the same section of Louisiana where I collected my first hoop snakes, I heard repeatedly about "stingin' lizards." My informants thought they had scored a clear triumph against my scepticism when they showed me scorpions to prove their assertion that there were "stingin' lizards" that really could sting!

ANOMALOUS DISTRIBUTION IN TIME AND SPACE

• Population Explosions

EIGHTY SQUARE MILES OF TURTLES

Anonymous; *Scientific American*, 80:106, 1880.

The Galveston News, of June 29, reports that between Sabine and Calcasieu, in the Gulf of Mexico, June 22, the schooner James Andrews encountered a vast multitude of green turtles, many of them very large, and all of them on their backs. Captain J.B. Rodgers, owner of the schooner, states that the schooner was lying on and off, and from observation it was estimated that the water covered by these turtles formed an area of eight miles in width and ten miles in length. They were all sizes, and not one being seen in a natural position. The water was literally covered with them. During the passage among the turtles, Spanish mackerel were leaping high in the air in every direction, as if determined to escape from the sea, giving evidence that either the water underneath was in a dreadful commotion or the sea monsters had come down on them from some strange sea. Captain Rodgers is anxious to have nautical men explain these odd phenomena of the turtles on their backs and the excitement among the mackerel. During his nautical career he never saw anything similar to it, nor did he ever before lay eyes on as many turtles and Spanish mackerel.

SNAKES & THE SEA

Minton, Sherman A. Jr., and Heatwole, Harold; *Oceans*, 11:53–56, April 1978.

A still enigmatic aspect of sea snake behavior is the tendency of some species to aggregate in large numbers at the surface. Early Spanish explorers, sailing off the coasts of Mexico and Central America, reported vast numbers of the pelagic or yellow-bellied sea snake (Pelamis platurus). This sea snake is unique in

that it captures its food and spends most of its life floating on the surface. It has the widest range of any sea snake and is the only species found in American waters where stragglers have been reported as far north as extreme southern California. Recent observations on this species indicate the snakes are carried more or less passively by wind and currents and accumulate in great numbers along drift lines. Aggregates of several thousand have been reported by Chaim Kropach in Panama Bay. The most spectacular aggregation of sea snakes was sighted in the Malacca Straits on May 4, 1932. The snakes were disposed in a line about 10 feet wide and some 60 miles long. They were not definitely identified but were a larger species than the yellow-bellied sea snake. We have heard of similar but much smaller masses of sea snakes spotted by helicopter pilots off the coasts of Pakistan and Vietnam. (p.54)

. . . . .

A PLAGUE OF FROGS

Fortey, F. H.; *Nature,* 60:246, 1899.

This afternoon, as I was walking into Lickey Village from King's Norton, I came across innumerable frogs. They lined the hedges and covered the road so thickly that I had to walk on tiptoe. I thus proceeded quite 400 yards, where the phenomenon ended as sharply defined as it had begun. Nowhere else along the road was a frog to be seen. I was particularly astonished, as I knew the nearest water to be the Little Reservoir---quite 1/5 mile away. The frogs were about ten days old, very small. A cottage stood about 300 yards from the beginning of this swarm. Upon inquiry I ascertained that the frogs had thus congregated since noon on Monday, that they had literally besieged the house, jumping all over the ground-floor rooms, that the garden and its paths were full of them. The present occupants had lived there 4½ years, but had never experienced anything like this. They have sometimes seen a few frogs cross the road in wet weather. They are now occupied with brushing them out of doors. Can any of your readers explain the cause of this extraordinary spectacle?

• Reptiles and Amphibians in Unexpected Places

A CURIOUS SALAMANDER

Blackford, Charles M.; *Nature,* 60:389-390, 1899.

A few years ago a station was established near the town of San Marcos, Texas, for the culture of black bass and "crappies." A prime essential for fish hatching is a copious supply of water, and the supply should be an uniform in amount, temperature and composition as it is possible to otain. If there be much sediment in the water, it will be deposited on the eggs and suffocate them; and sudden variations in temperature may also be fatal. As the rainfall in western Texas is untrustworthy, the Commission determined to bore an artesian well to supply the water for its new station.

The well was bored successfully and a flow of twelve-hundred gallons per minute obtained from a depth of 188 feet. There are several such wells in this region that give this amount or more, but soon after the San Marcos well was opened a number of living animals began coming up with the water. So far, four kinds of Crustacea and a salamander have been seen, and of these quite a number have been obtained. The Crustacea are new to science and were described by Dr. James E. Benedict, of the Smithsonian Insititution. They are white and perfectly blind. Most of the shrimps and crab-like animals have eyes set on the extremities of stalks that project above the surface. The shrimps from this well have the stalks, but the eyes have disappeared.

The most remarkable creature that has come from the well is the blind salamander, the Typhlomolge Rathbuni. The name is compounded from the Greek typhlos, blind, and molge, a kind of salamander; while the second term was given in honour of Mr. Richard Rathbun, the Assistant Secretary of the Smithsonian Institution, and for many years the Chief of the Division of Scientific Inquiry or the Fish Commission. This animal is a new species and a new genus. It was described by Dr. L. Stejneger, of the Smithsonian Institution. The Typhlomolge is from three to four and a half inches in length. It has a large head, protruding forward into a flattened snout that bears the mouth. The eyes are completely covered by the skin, and are visible from the outside only as two black specks. Just behind the head are the gills. These are external and stand out in festoons about the neck, instead of being covered by a lid as in fishes. The skin is a dingy white, and the sharp contrast between the colourless skin and the vivid scarlet of the exposed gills makes the appearance of this subterranean visitor striking in the extreme. It has four long, slender legs, that are gruesomely human in appearance, and are supplied with feet that are startlingly hand-like. The fore feet bear four fingers or toes and the rear ones

have five, and though the legs are extremely slender, they possess a considerable amount of strength. Behind, the body terminates in a flattened tail that bears a fin like that of an eel.

In April 1899, two living specimens of this strange being were shipped by mail from San Marcos to the head office of the Fish Commission in Washington. They bore the journey of nearly 1800 miles, and reached their destination in good condition. They excited great interest, and for some time after their arrival a wondering group of spectators crowded about the aquarium into which they were put. These living specimens corrected several errors that had been made from observations of the dead bodies only. The legs are used for locomotion, and the animals creep along the bottom with a peculiar movement, swinging the legs in irregular circles at each step. They climb easily over the rocks piled in the aquarium, and hide in the crevices between them. All efforts to induce them to eat have been futile, as has also been the case with blind cave fish in captivity and they are either capable of long fasts or live on infusoria in the water.

From whence do these strange creatures come? The well is sunk in limestone, and that renders it likely that there may be some great cavern of subterranean lake communicating with it, but the rock through which the hole is bored is solid, except for a single channel two feet in diameter. The fact that the water rises nearly two hundred feet shows it to be under great pressure, and altogether this well affords material for study to geologists as well as zoologists.

ALLIGATORS-IN-THE-SEWERS: A JOURNALISTIC ORIGIN

Coleman, Loren; *Journal of American Folklore,* 92:335–338, 1979.

The story of alligators haunting the sewers of major American cities is a modern urban legend. But what of its origin? Thomas Pynchon has written:

> Did he remember the baby alligators? Last year, or maybe the year before, kids all over Nueva York bought these little alligators for pets. Macy's was selling them for fifty cents; every child, it seemed, had to have one. But soon the children grew bored with them. Some set them loose in the streets, but most flushed them down the toilets. And these had grown and reproduced, had fed off rats and sewage, so that now they moved big, blind, albino, all over the sewer system. Down there, God knew how many there were. Some had turned cannibal because in their neighborhood the rats had all been eaten, or had fled in terror.

With those words, Pynchon propelled the persistent rumors of alligators in the sewers of New York City into a major work of

fiction. Pynchon's retelling of this folktale is at once concise, complete, and elaborate. He envisioned an Alligator Patrol going into the depths of the sewers, working in teams of two, with one man holding a flashlight while the other carried a twelve-guage repeating shotgun. Pynchon's fascinating novel wove the fabric of the alligators-in-the-sewers motif throughout the pages of his book, and thus brought this tale into modern popular culture as no one before him had.

Folklorists to herpetologists have acknowledged the widespread distribution of this peculiar alligators-in-the-sewers story, but many researchers have been vague as to the origins of the tale. Without giving a citation, Richard M. Dorson noted marijuana harvesters, in pursuit of the elusive strain "New York White," had difficulties

> because, according to a newspaper story, full-grown alligators prowled the sewers of New York. It seems that Miami vacationers returning to New York in the winter brought back baby alligators as pets for their children. The more the alligators grew the less ideal they appeared as playmates, and their owners, too tenderhearted to skin them for their hides, mercifully flushed them down the toilet. Some survived in their new environment and confronted sewer maintenance workers, who publicly protested at this unnecessary additional hazard to their occupation. The newspaper published the matter, and the tales began to circulate.

In Jack Horn's 1975 review of Urban Folklore from the Paperwork Empire, the "blind white alligators that live in New York sewers" are mentioned as an example of urban folklore, but Horn's "source" of the tale does not go further than the Florida souvenir and the flushing toilet.

In the world of zoology, we find the same meanderings, for the herpetologists Sherman and Madge Rutherford Minton have written:

> One of the sillier folktales of the 1960s was that the New York sewers were becoming infested with alligators, presumably unwanted pets that had been flushed down the toilet. In some accounts, these were growing to formidable size from feeding on rats. We have been unsuccessful in tracing the source of these legends but would assure New Yorkers that alligators are not among their urban problems.

My search, therefore, was for an apparently "real" account of an alligator in a sewer--a journalistic vehicle--as the possible origin of the tale. As I have written elsewhere

> crocodilians fall from the sky, and materialize inside cotton bins and in washrooms from Texas to France. They slither and slink to the horror of humans from basement drains and sewers anywhere from Kansas to New York City. Unlike some mystery animals, alligators are caught, killed and placed in museums. Although actual alligators seem to appear and per-

sist in northern winters (e.g., sightings and finds for Oakland County, Michigan, 1953-1957) to the dismay of herpetologists, random out-of-place finds seem to be the rule.

Some of these alleged discoveries are unusual, as, for example, the "alligator five and a half feet long...found near the bank of the Rock river, at Janesville, Wis., frozen to death," in 1892.

I have compiled a list of seventy-plus encounters with erratic alligators for the years 1843-1973, but actual, supposedly true recording of an alligator in a sewer proved to be a rare occurrence. I was able to discover just such an event, nevertheless, recorded as fact from, not surprisingly, New York City.

The incident may or may not have taken place, but its publication in a no nonsense fashion in a highly regarded and respected newspaper must have lent much credibility to the story. In contrast to the common notion that the alligators-in-the-sewers motif is a product of the sixties, the following article is from the New York Times of February 10th, 1935, and is given here in its entirety:

ALLIGATOR FOUND IN UPTOWN SEWER

Youths Shoveling Snow Into Manhole
See The Animal Churning In Icy Water
SNARE IT AND DRAG IT OUT
Reptile Slain by Rescuers When It Gets
Vicious-Whence It Came is Mystery

The youthful residents of East 123rd Street, near the murky Harlem River, were having a rather grand time at dusk yesterday shoveling the last of the recent snow into a gaping manhole.

Salvatore Condulucci, 16 years old, of 419 East 123rd Street, was assigned to the rim. His comrades would heap blackened slush near him, and he, carefully obsering the sewer's capacity, would give the last fine flick to each mound.

Suddenly there were signs of clogging ten feet below, where the manhole drop merged with the dark conduit leading to the river. Salvatore yelled: "Hey, you guys, wait a minute," and got down on his knees to see what was the trouble.

What he saw, in the thickening dusk, almost caused him to topple into the icy cavern. For jagged the surface of the ice blockage below was moving; and something black was breaking through. Salvatore's eyes widened; then he managed to leap to his feet and call his friends.

"Honest, it's an alligator!" he exploded.

Others Look and Are Convinced.

There was a murmur of skepticism. Jimmy Mireno, 19, of 440 East 123rd Street, shouldered his way to the rim and stared.

"He's right," he said.

Frank Lonzo, of 1743 Park Avenue, looked next. He also confirmed the spectre. Then there was a great crush about

the opening in the middle of the street and heads were bent low around the aperture.

The animal apparently was threshing about in the ice, trying to get clear. When the first wave of awe had passed, the boys decided to help it out. A delegation was dispatched to the Lehigh Stove and Repair Shop at 441 East 123rd Street.

"We want some clothes-line," demanded the delegation, and got it.

Young Condolucci, an expert on Western movies, fashioned a slip knot. With the others watching breathlessly, he dangled the noose into the sewer, and after several tantalizing near-catches, looped it about the 'gator's neck. Then he pulled hard. There was a grating of rough leathery skin against jumbled ice. But the job was too much for one youth. The others grabbed the rope and all pulled.

Slowly, with its curving tail twisting weakly, the animal was dragged from the snow, ten feet through the dark cavern, and to the street, where it lay, non-committal; it was not in Florida, that was clear.

And therefore, when one of the boys sought to loosen the rope, the creature opened its jaws and snapped, not with the robust vigor of a healthy, well-sunned alligator, but with the fury of a sick, very badly treated one. The boys jumped back. Curiosity and sympathy turned to enmity.

"Let'im have it!" the cry went up.

Rescuers Then Kill It.

So the shovels that had been used to pile snow on the alligator's head were now to rain upon it. The 'gator's tail swished about a few last times. Its jaws clashed weakly. But it was in no mood for a real struggle after its icy incarceration. It died on the spot.

Triumphantly, but not without the inevitable reaction of sorrow, the boys took their victim to the Lehigh Stove and Repair Shop. Ther it was found to weigh 125 pounds; they said it measured seven and a half or eight feet. It became at once the greatest attraction the store ever had had. The whole neighborhood milled about, and finally, a call for the police reached a nearby station.

But there was little for the hurrying policemen to do. The strange visitor was quite dead; and no charge could be preferred against it or against its slayers. The neighbors were calmed with little trouble and speculation as to where the 'gator had come from was rife.

There are no pet shops in the vicinity; that theory was ruled out almost at once. Finally, the theories simmered down to that of a passing boat. Plainly, a steamer from the mysterious Everglades, or thereabouts, had been passing 123rd Street, and the alligator had fallen overboard.

Shunning the hatefully cold water, it had swum toward shore and found only the entrance to the conduit. Then after another 150 yards through a torrent of melting snow--and by that time it was half dead--it had arrived under the open manhole.

Half-dead, yes, the neighborhood conceded. But still alive

enough for a last splendid opening and snapping of its jaws. The boys were ready to swear to that.

At about 9 p.m., when tired mothers had succeeded in getting most of their alligator-conscious youngsters to bed, a Department of Sanitation truck rumbled up to the store and made off with the prize. Its destination was Barren Island and an incinerator.

The article makes exciting reading and probably had people of its day talking about alligators-in-the-sewers for some time. Indeed, the reported encounter may have, for years, spawned similar reports from New Yorkers and other readers of the paper. As far as this writer can establish, this account is the first documented source of this particular motif.

[1]Thomas Pynchon, V. (New York: Bantam Books, 1964), p.33.
[2]Richard M. Dorson, America in Legend: Folklore from the Colonial Period to the Present (New York: Pantheon Books, 1973), pp. 291-292.
[3]Jack Horn, "White Alligators and Republican Cousins--The Stuff of Urban Folklore," Psychology Today, November 1975, pp. 126, 130.
[4]Sherman A. and Madge Rutherford Minton, Giant Reptiles, (New York: Charles Scribner's Sons, 1973), p. 34.

MORE ON ALLIGATORS IN THE SEWERS

Fergus, George; *Journal of American Folklore*, 93:182, April-June 1980.

In a recent note published in the Journal of American Folklore (92[1979]. 335-338), Loren Coleman described his efforts to discover the origin of the legend about alligators in the sewers of New York City. He found a 1935 newspaper article about some kids discovering an alligator in a sewer, but was unable to come up with any further documentation, or any explanation for why the folktale did not surface until the 1960's. I think I can provide this information.

In 1959 a book entitled The World Beneath the City was published by Lippincott. Written by Robert Daley, it is a history of the problems involved in the development of the network of utilities underneath Manhattan Island. And in the midst of the stories of engineering problems and political deals is a chapter entitled "Alligators in the Sewers" (see pp. 187-189). It is based on the author's interviews with Teddy May, who had been Commissioner of Sewers in New York for some thirty years.

According to May, sewer inspectors first reported seeing alligators in 1935, but neither May nor anyone else believed them. "Instead, he set men to watch the sewer walkers to find out how they were obtaining whisky down in the pipes." Persistent reports,

however, perhaps including the newspaper item discovered by Coleman, caused May to go down to find out for himself. He found that the reports were true. "The beam of his own flashlight had spotted alligators whose length, on the average, was about two feet."

May started an extermination campaign, using poisoned bait followed by flooding of the side tunnels to flush the beasts out into the major arteries where hunters with .22 rifles were waiting. He announced in 1937 that the 'gators were gone. Reported sightings in 1948 and 1966 were not confirmed.

However, there is no mention of "blind, albino" alligators, and May suggests that the baby alligators were dumped down storm drains rather than "flushed down the toilet."

NOTICE OF THE APPEARANCE OF FISH AND LIZARDS IN EXTRAORDINARY CIRCUMSTANCES

Muse, Joseph E.; *American Journal of Science,* 1:16:41-44, 1829.

While on the subject of mysterious nature, I will introduce, as concisely as possible, a case, where she reconciled animals of the coldest and most meagre habits to the enjoyment of the warmth and luxuries of the human stomach; for these facts, though not personally conversant with them, I have the authority of a medical gentleman of unquestionable veracity, to vouch for their rigid truth; in reply to my request to be informed of the habits, food, drink, employment, &c. of the patient, I received the following account. "On my arrival, I found that she, (the patient,) had puked up two ground puppies and was labouring under a violent sick stomach, with pain, and syncope: the first was dead when ejected, the second was alive when I arrived, and ran about the room; they were about three inches long; she informed me, that on the road that morning she had thrown up two others; the case occurred in the summer, and had made gradual progress, from the first of April, and as she described it, with a peculiar sickness, and frequent sensation of something moving in her stomach; with slight pain and loss of appetite, which increased till her illness. She was about twenty years of age, and had enjoyed good health; her employment had confined her in the swamp, during the winter and spring, and she had from necessity, constantly drunk swamp water." The physician administered an emetic in quest of more puppies, but being disappointed, he gave an opiate; she was relieved, finally, and has been, since, in health.

These animals have, since, been shown to me: they are not the ground puppy, (gecko,) as they are vulgarly called; they resemble it very much, but are easily distinguished from it; they belong to the same genus, (lactera, or lizard,) but are of the species "salamander;" their habitudes too, are essentially different; the gecko is found in houses and warm places; the salamander in cold damp places, and shaded swamps, and by the streams

of meadows; these animals though oviparous, hatch their eggs in the belly, like the viper and produce about fifty young at a birth. The inference is irresistible, that the patient had, in her frequent draughts of swamp water, swallowed, perhaps thousands of these animals in their nascent, or most diminutive state of existence, and a few only survived the shock; but it is matter of astonishment, that from the icy element in which they had commenced their being, and for which, they were constituted by nature, they should bear this sudden transportation to a situation so opposite in its character, and grow into vigorous maturity, unannoyed by the active chemical and mechanical powers to whose operation they were subjected.

• Animals in Rocks: A Few Tales Among Many

ON THE VITALITY OF TOADS, &C. ENCLOSED IN FIRM MATERIALS

Thompson, Wm. A.; *American Journal of Science,* 1:25:41–47, 1834.

Having observed in Vol. XXIII, No.2, of the Journal of Science, an account of some experiments made by Professor Buckland of Oxford, on the long continued vitality of toads, and having reflected on these experiments, and their results, together with the conclusions drawn from them, I have been led to doubt whether the object which Dr. Buckland had in view, could be obtained in the manner and under the circumstances in which his trials were made. The reptiles were enclosed in two different pieces of stone; in the one case, in cells twelve inches deep and five in diameter, and in the other, the same number of toads were enclosed in sandstone, in cells of smaller dimensions. It appears, that at the end of a year or more, those in the smaller cells were all dead, while most of those in the sandstone were alive, although much diminished in weight. After the cells were taken up and examined, the surviving toads were enclosed again until the end of another year; they were then taken up, and all found dead. At the same time that the other toads were enclosed in the stone, four others were shut up in three holes cut in the trunk of an apple tree, five inches deep and three in diameter, and the holes were carefully closed with plugs of wood, so as to be apparently tight; at the end of a year they were all found dead.

Upon a review of these facts, it does not appear surprising, that these reptiles should, all or most of them, be found dead at the expiration of one or two years, and I doubt whether these experiments will be regarded as satisfactory. The very circum-

stances of these trials appear to admit the persistent vitality of these animals; for if there was any doubt as to the great length of time, that the principle of life remains unextinguished in these reptiles, Dr. Buckland would not have made the experiments.

In this country, toads and frogs have been found in three different situations.

1. Toads have been frequently found in sandstone of the secondary formation, and in secondary limestone.

2. In digging wells, where the workmen have come to beds of clay twelve or fifteen feet below the surface of the ground.

3. In the trunks of trees, which were apparently closed so as to be impervious to the air.

In the first place, the toads that have been found in the sandstone and limestone, were enclosed in cells just large enough to contain their bodies; and from every appearance, must have remained in that situation ever since the formation of the surrounding sandstone or limestone, in the water under which they appear to have been deposited.

The cells that enclose these reptiles were evidently accommodated to their shape and size, and of course, the materials of which they are composed were then in a plastic or yielding condition, so as to suit the form of any body that might become enclosed in them. Now it is obvious, that if a living healthy toad or other reptile were enclosed in a cell of the size in which they are usually found, it would not live one half of the time that those did which were immured by Prof. Buckland; for food and air are absolutely necessary to every animal that has the use of its natural organs.

But it is a well known fact, that toads, frogs, and other reptiles, have remained in a torpid state for many years, without any signs of life, and have revived, on being exposed to a higher temperature, with access of air. It thus proved that respiration and the circulation of the blood are not necessary to the vitality of the cold blooded animals, during their hybernation; it appears, also, that the food taken into the stomach remains, unaltered and undigested, even at the end of three or four years, the same as if it had not been in the stomach more than a minute, provided, however, that the torpidity of these hybernating animals remains the same, and that they continue in the same low degree of temperature.

We have a right to suppose, that from the earliest formation of our globe, there has been a succession of seasons of heat and cold, of tides, &c. as now; and that the constitution of animals has always been regulated by the same principles as at present; if, therefore, any of these reptiles, during a state of torpidity, should be imbedded in sand or calcareous matter, we know of no reason why their vitality should not continue for thousands of years. If food, respiration, and the circulation of the blood, are not necessary for the continuance of the vitality of these reptiles, the lapse of a thousand years is the same to them as that of one day. A free circulation of air, and a higher temperature, are both equally necessary for the revivescence of these torpid animals. We have no account of the toad and other reptiles being

found enclosed in sandstone or marble, in Europe or America, except in latitudes where the cold renders these reptiles torpid; it therefore appears probable that they might be enclosed in the substance when it was soft, and the reptiles in a torpid state. If it is objected that the animals should have been quickened into life by the annual return of a higher temperature, it may be answered, that a rock at the depth of fifteen or twenty feet remains at a much lower temperature than the incumbent air, and there appears great reason to doubt, whether, if a reptile should be enclosed in a rock, at the depth of fifteen or twenty feet, without a free circulation of air, it would become quickened. Frogs and toads at the south part of Hudson's Bay, and in Canada, have remained frozen and torpid for years, and afterwards revived.

Toads, in this latitude, remain torpid from the first of November until the first of May; in the summer, they usually burrow about eight or ten inches under the ground, or under some stone at a less depth; in the winter they continue in a torpid state, and remain so even until May, at which time the small insects begin to emigrate from their winter quarters, to furnish them with food.

In this climate, the earth is usually frozen during the winter season, from fifteen inches deep, and every thing enclosed by it appears to be congealed and lifeless.

The hybernating warm blooded animals, such as the marmot, hedge-hog, pole-cat and bat, although they remain torpid during the cold season, yet the cold operates very differently on them, from what it does on the cold blooded animals, in which the circulation of the blood can be carried on, independently of the action of the lungs.

When the temperature of the air sinks below 50° Fahr. the cold blooded animals begin to lose their sensibility; when reduced to 40° they become torpid, and if continued in that temperature they might remain unchanged for any length of time, as repeated experiments seem sufficiently to prove.

As respects the toads and frogs, that, in digging wells, have been found in the clay, at the depth of twelve or fifteen feet, I see no reason to think that they may not have lain there in a torpid state ever since the deluge, as most of the materials above the solid rock strata were, at that time, removed by the violent action of the water, and these frogs and toads might have been inclosed at that time with the materials that were every where in motion; and if they were not deposited in the earth at that time, but have been since covered deep in the earth by some violent irruption of the waters, so as to deprive them of air and food, their case will still be similar, for, obviously, at the depth of fifteen feet there could be no supply of food or air, and yet when taken out of the clay, they have soon become quickened so as to move; it is therefore possible that these toads and frogs may have remained in this situation many years, or even ages,--indeed, for a period incomparably longer than any person will attribute to the life of these reptiles.

As to the toads found enclosed in the trunks of trees, it is a case much more within our comprehension; there is no direct

necessity for supposing a very long continued vitality in them; it is not surprising that a toad, having crept into a hollow place in the trunk of a tree, should not be able to get out of his confinement, and that the place should, in the course of three or four years, become closed up, in the natural process of vegetation; and it is easy to admit that there might be some crevice in the wood, through which insects might enter and supply the animal with food. Again, as it is well known that our trees that are not more than two feet in diameter, are not unfrequently frozen completely through in winter, and the toads might thus become so torpid, that, having no free circulation of air, their torpidity might continue until they were extricated from their confinement. That the lives of these reptiles, when supplied with food and air, in the ordinary way, do not usually continue beyond twelve or fifteen years, we have every reason to believe when we consider their diminutive size, and that they attain maturity in two or three years; whereas, if we look at man--the number of years before he arrives at maturity--the long continued labor he is capable of sustaining, and the great effect which the mind has upon the body, so that, whether from physical or mental causes, his life is occasionally protracted to one hundred years or more; and if we contemplate particularly the large size of the elephant, the great number of years requisite to enable him to acquire his full growth, and finally his maturity of two hundred years or more,--from all these circumstances we must be led to suppose, that the reptiles that have been found immured in sandstone and marble must have remained in that situation longer than we can reasonably attribute to the life of reptiles of any kind, and that the concretions (as some have supposed) that have assisted to enclose them, would be longer in forming than can be allowed for the usual term of their natural lives. Professor Buckland concludes, from his experiments, that when the natural organs of the animal are in continual action, the vitality of the toad has no extraordinary continuance, and that therefore life most terminate in a short time; but we are, on the contrary, led to believe that the vitality of the toad may be continued to an interminable length of time, provided the animal has become torpid by cold, so as to stop respiration and the circulation of the blood, and provided he remain at a low temperature, and without a free circulation of air, adapted to produce revivescence.

We may presume that the internal parts of the rock strata from which cold springs issue, are of about the same temperature as the water that issues from them; it may therefore be admitted as probable, that the toad, if enclosed in a rock, would not become quickened until after that rock has become warmer than the water that issues from it in summer; and that, under ordinary circumstances, the toad does not issue from his torpid state in the spring, until after the air becomes warmer than the spring water that issues from the rocks in summer.

That in every instance where toads are found immured in stone, there should be a crevice or aperture in the rock, to admit air and insects for food to the tenants, and that it has escaped the notice and observation of the inquisitive observer,

is contrary to all probability, especially as the occurrence has always excited the most intense curiosity, and more particularly as the opening must have been originally large enough to admit the body of the reptile.

I have been led to make the foregoing reflections, partly, from observing the custom of taking the large pike from frozen ponds and lakes in this country, and carrying them, in a frozen state, into other ponds, for the sake of propagating their species, where they appear to revive and to suffer no damage, except the loss of some of the scales; likewise, by seeing snakes that have apparently been frozen stiff, so that three or four inches of the tail have been broken off, like an icicle, and yet the snakes have revived, on being exposed to the warm air.

Toads are often ploughed up, early in the spring, when no signs of life appear, until after being exposed for some time to the warm air; these facts appear to bear on the case in hand, and I might add a number more of a similar character that have fallen under my observation.

No person is more willing to pay homage to the distinguished character of Professor Buckland than myself, or has a more exalted opinion of the great service he has done to science; but I cannot forbear (notwithstanding his deserved celebrity) to think that his experiments are very inadequate to settle the question of the long continued vitality of reptiles found in the different rock strata.

I was in hopes that some more able pen than mine would have discussed this subject in the Journal of Science, but as I find it is not yet noticed, I have ventured to give you a sketch of my ideas on this subject.

P.S. Not long since, as a number of laborers were digging a well in this town, and after penetrating five or six feet through the gravel, they came to the hard pan, and entering it about five feet more, they found a live toad about two thirds the size of a full grown toad. It was enclosed in a cell somewhat larger than the animal, but suited, in every way, to his shape. The discovery naturally occasioned much surprise, and they examined the surrounding materials and endeavored to put them into place, but they were so broken by the pick-axe, that they found it impossible to put them correctly together. The toad, on being exposed to the air, soon began to move, but died within the space of twenty or thirty minutes afterwards.

I have to remark that this well is situated on elevated ground, and that the hard pan, common in the United States, is composed of clay and gravel, cemented with iron, and is so firm that it cannot be broken up without a pick-axe and crow-bar, which are the implements commonly used by laborers in digging wells and cellars. It is to be observed, also, that the hard pan is free from fissures and seams, and equally impervious to air or water as the sandstone of the country. This reptile, beyond all doubt, was excluded from air and the means of acquiring food; below the effects of the warmth of the sun in summer, and below, also, that of the rain water that sinks into the earth, whereby it is warmed.

It appears to me that this instance furnishes a case, that is

directly opposed to the inferences drawn by Prof. Buckland. In The absence of any direct evidence on the subject, he raises a presumption against the long continued vitality of toads; whereas, in all the acounts furnished in Europe and America, the evidence goes to prove, that the presumption he has made is opposed by well authenticated facts.

TOAD FOUND IN A FLINT

Anonymous; *Zoologist*, 9:3265-3266, 1851.

The Academie des Sciences, in its last sitting, was occupied with a grave question of what, in homely language may be called a "toad in a hole." In digging a well at Blois, in June last, some workmen drew up from about a yard beneath the surface, a large flint, weighing about 14 lbs., and on striking it a blow with a pickaxe it split in two, and discovered, snugly ensconced in the very centre, a large toad. The toad seemed for a moment greatly astonished, but jumped out and rather rapidly crawled away. He was seized and replaced in the hole, where he settled himself down very quietly. The stone and toad, just as they were, were sent to the Society of Sciences at Blois, and became immediately the subject of curious attention. First of all, the flint, fitted together with the toad in the hole, was placed in a cellar and embedded in moss. There it was left for some time. It is not known if the toad ate, but it is certain that he made no discharge of any kind. It was found that if the top of the stone were cautiously removed in a dark place he did not stir, but that if the removal were effected in the light, he immediately got out and ran away. If he were placed on the edge of the flint, he would crawl into his hole and fix himself comfortably in. He gathered his legs beneath his body, and it was observed that he took especial care of one of his feet, which had been slightly hurt in one of his removals. The hole is not one bit larger than the body, except a little where the back is. There is a sort of ledge on which his mouth reposes, and the bones of the jaw are slightly indented, as if from long resting on a hard substance. Not the slightest appearance of any communication whatsoever between the centre and the outside of the stone can be discovered, so that there is no reason to suppose that he could have drawn any nourishment from the outside. The committee, consisting of three eminent naturalists, one of whom has made toads his peculiar study for years, made no secret of their belief that the toad had been in that stone for hundreds, perhaps thousands of years; but how he could have lived without air, or food, or water, or movement, they made no attempt to explain. They accordingly contented themselves with proposing that the present should be considered another authentic case to be added to the few hundreds already existing, of toads being found alive embedded in stone, leaving it to some future savant to explain what now appears the wonderful miracle by

which nature keeps them alive so long in such places. But the distinguished M. Magendie suggested that it was just possible that an attempt was being made to hoax the Academy, by making it believe that the toad had been found in the hole, whereas it might only have been put in by the mischievous workmen after the stone was broken. Terrified at the idea of becoming the laughing-stock of the public, the Academy declined to take any formal resolution about the toad, but thanked the committee for its very interesting communication; and so the subject dropped. One word, however: ---if the toad had really not been embedded in the flint, how comes it that after being taken out, he always fixed himself exactly in the cavity, that the cavity fitted him to a hair's breadth, and that the hardness of the stone had made an impression on his jaw?

TOADS IN THE HOLE

Cumming, Alexander P. G., et al; *Zoologist,* 21:8641-8642, 1863.

Sir A.P. Gordon Cumming writes to the 'Elgin Courier':--- "In cutting the Inverness and Perth Railway through Lochnavandah ark, on Altyre, we have unceremoniously trespassed on the privacy and retirement of a numerous colony of ancient toads. The cutting is here from twenty to twenty-five feet deep, the lower part being through from ten to sixteen feet of freestone and red conglomerate. The interesting old residents are found in the red freestone about fifteen to twenty feet below the surface, where they certainly must have seen several nineteen years' leases out on the land above them. They are sometimes turned out by the heavy hand-pick or the great iron crowbar; but a blast of powder, of which a vast amount is here expended, seems to cause the greatest upset in the establishment, as a shot is sometimes the means of exposing as many as a dozen sleepy old fellows. They seem none the worse for their long repose, but after giving a few winks at the new light thus suddenly let in upon them, and taking several gasps of the unwonted air, they leisurely and deliberately proceed to hop and crawl down in the line along the small watercourse towards the lower fields. I have seen them in numbers, and some of the men have counted above forty at once."

PROLONGED TORPIDITY OF TOADS

Anonymous; *Scientific American,* 40:87, 1879.

Professor J.A. Allen, of Cambridge, states that he saw a large number of toads taken from the mud of a well which had been

closed for twenty years. The animals were apparently lifeless, being quite motionless, but after being drawn up and exposed to the air their legs began to twitch after a few moments, and their eyes slowly to open and close. In three or four minutes they so far recovered as to hop about, and shortly after became as bright if they had not been sound asleep for the last score of years. The temperature of the mud in which they were found was about 45 degrees, which was no doubt maintained throughout the year; and, as this corresponds very nearly to that of ponds where they hibernate in winter, Professor Allen thinks that this prolonged torpidity was caused by a continued uniformity of temperature, and that he sees no reason why it should not have been protracted indefinitely.

A TOAD IN THE COAL

Anonymous; *English Mechanic,* 73:260, 1901.

The London, Stereoscopic Company, 54, Cheapside, E.C., are now publishing photographs of a "Toad" discovered by Mr. W.J. Clarke, Bath-street, Rugby, in the centre of a lump of coal, after having been on the fire about an hour and a half. Mr. Clarke broke the coal with a poker, and noticing something moving, he picked it up, and found it was a living toad. It had no mouth, and was nearly transparent. It lived five weeks after being liberated, and is now on view in Cheapside.

THE EASTLAND HORNED "TOAD"

Hewatt, Willis G.; *Science,* 67:348, 1928.

Much attention has been attracted recently to a Horned "Toad" (Phrynosoma cornutum) which is alleged to have been placed in the corner stone of the Eastland County courthouse, Eastland, Texas, in the year 1897. The animal, it is claimed, remained entombed in the granite corner stone until February 18, 1928, a period of thirty-one years. On the latter date it is said to have been removed from the stone alive, before a large crowd of spectators which had gathered for the occasion.

On February 22, 1928, the writer had the opportunity to go to Eastland and make an examination of the external features of the animal in question. It appeared to be a perfectly normal specimen which had undergone winter hibernation. It was probably an old one for the horns about the head region were considably worn and the right hind leg had been broken but was healed. Otherwise it appeared no different from a normal Horned "Toad" at this season of the year.

EXTENDED HIBERNATION IN THE TOAD

Davies, P. A.; *Science,* 81:617, 1935.

The writer believes he has an example of extended hibernation in the common toad, Bufo americanus. In 1908, the W.E. Caldwell Company, Louisvill, Kentucky, constructed a structural steel plant over some filled swampy land. On November 1, 1934, twenty-six years later, while digging in one of the buildings for the placing of a new furnace, two toads were exhumed. The first was about four and one-half feet and the second nearly eight feet below the clay floor of the building. An examination of the walls of the pit showed the fill to be of yellow clay with an occasional small air space, none over one-half inch in diameter. There was no indication of any type of passage-way by which the toads may have entered. The closest distance from the pit to the wall of the building was twenty-five feet. The foundation of the building is fourteen inches thick and extends four and one-half feet below the clay floor of the building.

The workmen placed the first toad on the edge of the pit, believing it to be dead, but in a short time it hopped away. The second toad was saved. It was so thin that little remained but skin and bones. It revived to such an extent that it was able to hop and to turn over when placed on its back. When it was brought into the warm laboratory, it died within two hours.

• Far-Wandering Alligators and Crocodiles

ALLIGATORS IN THE BAHAMAS

Gardiner, John; *Science,* 8:369, 1886.

Catesby, in his 'Natural history of Carolina, Florida, and the Bahama Islands,' published about a century ago, speaks of having seen alligators on the Island of Andros in this group. At present there are none, and, with the object of finding out if there was any tradition current bearing upon the subject, I made inquiries through the medium of the Nassau guardian. In answer to my questions, I lately received from the rector of Inagua, at the extreme south-east of the group, a letter, in which he mentions that stories of alligators having been drifted on logs of mahogany, and thrown up on the shores of the island, are common, but that he had not been able to verify any of them. However, a few days previous to the date of the letter, while on a visit to one of the settlements, Mr. de Glauville (the rector) was shown the skin

of an alligator eight feet long from tip to tip, which had been shot on shore a day or two before by a man whose name is given. Many logs of mahogany had been cast up on the shores of Inagua about that time; but the alligator had not been observed to land, and had been seen on shore several times before it was shot.

There seems, however, to be no reasonable doubt that the alligator was drifted by the current from the south-east to Inagua, on a log of mahogany, from San Domingo, the nearest place in which alligators are found. This means that it travelled a distance of from one hundred to one hundred and fifty miles. With regard to the occurrence of alligators on Andros, Catesby was a very accurate observer, and there seems to be no reason for doubting his statement. These alligators would appear to have been carried on drift-wood from the north-west coast of Cuba, a distance of three hundred miles, by the Gulf Stream, and cast on the edge of the Great Bahama Bank, whence local currents, aided by the wind, might have carried them to the west coast of Andros. The absence of traditions on the subject may be owing to the fact that the present inhabitants of Andros are principally descendants of persons who settled there at a period subsequent to Catesby's visit.

These instances of the dispersion of large animals by means of oceanic currents may be of interest to those of your readers who study the question of the geographical distribution of animals.

While on the subject of Andros, may I be allowed to mention two rather curious superstitions current among the inhabitants of that interesting island? The interior of the northern part of the island consists of swamps and lakes, interspersed with patches of rocky ground on which the Bahama pine (P. bahamensis) grows thickly. The negroes have a great dislike to entering these pine-woods alone, or even in small companies; for they say that a peculiar race of malevolent beings, called 'little people,' inhabit the trees. These creatures are said to be like tiny men covered with hair. They sit on the pine-boughs, and if a man notices them, and points them out to his companions, the whole party is rendered immovable for a day and a night; but, if fire is thrown at the 'little people,' they disappear without doing any harm.

The other superstition also relates to the pine-woods. Creatures like enormous hairy men, called by the negroes 'Yayhoos,' are said to march about the woods in 'schools,' the largest coming first; and 'when dey cotch you, dey tear you.' These beings are naturally much more dreaded than the 'little people.' It looks as if their name had been given by some traveller familiar with 'Gulliver's travels,' and struck with the resemblance between them and the terrible creatures of Swift's imagination.

Both of these superstitions would appear to be traditions of the land from which the negroes originally came. The 'little people' are probably a recollection of the small, arboreal monkeys, while the 'Yayhoos' represent the gorillas, of West Africa.

ALLIGATOR MISSISSIPPIENSIS IN OKLAHOMA

Lane, H. H.; *Science,* 30:923-924, 1909.

The occurrence of any species of reptile at a considerable distance beyond its usual or recorded range is a matter of general zoological interest. The capture of Alligator mississippiensis Daudin in central Oklahoma under circumstances which render it very improbable that the individual had ever been in captivity seems, therefore, worthy of record. The specimen was taken in a "lake" or bayou of the South Canadian River within five miles of the State University of Oklahoma, at Norman. It was secured by a farmer, who presented it to the university museum through Dr. A. H. Van Vleet. Its skeleton has been preserved, and a life-like model, full size, was made and is now in the university museum. The specimen was a female, slightly over four and one half feet in length from tip to tip. It had been seen in the locality where taken, by a number of persons at various times for at least three years previous to its capture.

There is no record nor tradition of an alligator ever having escaped or been liberated (or even held captive) in this vicinity, and it is quite unlikely that such a thing could have occurred in what was formerly the Indian Territory. It seems altogether probable, therefore, that this individual had traveled up the Arkansas River to the mouth of the Canadian, and thence up the latter to the vicinity of Norman, some three hundred and fifty to four hundred miles west of the Arkansas-Oklahoma state line. The Canadian River is not a navigable stream and for most of the year is only a small meandering creek in a wide valley well filled up with sand. It is subject to numerous freshets, and frequently changes its course, so that "lakes" or bayous are numerous. The larger of such bayous apparently would make an admirable habitat for this species.

It is useless to speculate on the causes which impelled this individual to make such a journey, but it is important that it was apparently successfully maintaining its existence in its new station until man's interference put an end to one of nature's experiments.

ON THE EXISTENCE OF A CROCODILE IN FLORIDA

Wyman, Jeffries; *American Journal of Science,* 2:49:105-106, 1870.

It has been shown by different paleontologists, especially by Dr. Leidy and Prof. Cope, that several species of Crocodilians existed in North America during the Cretaceous and Miocene periods, all of which became extinct. At the present time two living species of true Crocodiles, viz: C. acutus and C. rhombifer, are known in South America, and both range as far north as Cuba and San Domingo, but we have not been able to find a record of the presence of either of them within the limits of the United

States, the Alligator being the only representative of the family to which it belongs.

While a guest of J. M. Forbes, Esq., on board the yacht Azalea, I had an opportunity of visiting Key Biscayne Bay in March, 1869, and while there Mr. William H. Hunt, of Miami, presented me the cranium here described. The animal to which it belonged, as I was told by the person who killed it, was shot near the mouth of the Miami river, which opens into the above mentioned bay. I was also informed that another had been killed in the same neighborhood.

The length of the head (from the alveoli of the incisors to the end of the occipital condyle) is 462mm, and the greatest breadth 191mm. The whole number of teeth is 68, viz: $\frac{19}{19} = \frac{19}{19}$; in the upper jaw the 4th and 10th, and in the lower the 4th and 11th are the longest. The first 12 teeth above and the first 11 below have the enamel fluted, and in both jaws the teeth behind these are marked with finely reticulated grooves. These hinder teeth are shorter and blunter, and thus more closely resemble the teeth of the alligator. The upper jaw is strongly convex and protuberant along the back of its middle portion.

. . . . .

In the South American specimen the markings of the surface of the bone consist chiefly of pits, while in the Florida specimen the markings are more in the form of grooves. In the first the parietal foramina are separated by a flat surface of bone 15 millimeters wide, while in the second there is only a blunt ridge. In this last also the lower jaw is somewhat longer, and its hinder part less recurved. The Florida specimen being the smallest is assumed to be the youngest; the differences of proportion above referred to may therefore depend upon age, as in young individuals the breadth is relatively greatest. In view of this we find no sufficient reason for considering the Florida specimen as belonging to any other than the sharp-nosed species (C. acutus). This conclusion is still further supported by a portion of the skin of the neck, which is preserved, and in which the nuchal plates are the same as in the species just named.

NOTES ON A YOUNG CROCODILE FOUND IN A FARM-YARD AT OVER-NORTON, OXFORDSHIRE

Wright, George R.; *Gentlemen's Magazine,* 5:2:149–154, 1866.

Whilst on a visit in Oxfordshire, at the farm-house of a then tenant of mine at Over-Norton, near Chipping Norton, I first saw the little reptile already referred to, in a glass case, where other specimens of animals and birds were well arranged and kept, the whole having been preserved by my tenant, Mr. William Phillips, who is well known in that part of the world as a keen sportsman

and good naturalist. On noticing at once the peculiarities of the little animal, I asked Mr. Phillips how and where it was found, when to my great surprise, as well as increasing interest, he told me, as well as I can now recollect, the following story of its discovery:---

He said, that one morning, in the year 1856 or 7, I can hardly now say for certain which, as he was walking in his farm-yard at Over-Norton, his attention was attracted by the sight of, as he at first thought, a lizard, lying in the gutter, evidently but lately killed, its bowels protruding from a wound in its belly. Upon, however, taking it up, he soon discovered that the animal was not a lizard, and he immediately asked his labourers, who were close by, unstacking some faggots for the use of the house, if they knew anything about it. The answer was that they had killed it as it ran out of the stack of wood, I think the day before; and on Mr. Phillips expressing his regret at their having done so without bringing it to him alive, they replied they could easily get him another, as at the place where the wood was cut a few miles from the farm, near to Chipping Norton Common, and not far from the village of Salford, at the "Minny" Pool---which I presume is a shortened form of Minnow---they saw them frequently in the water and on the land, and often running up the trees. Upon this statement, Mr. Phillips offered his wondering workmen a guinea for another specimen, adding the remark that they had killed an animal of a most rare character, and one he thought, in spite of all they said, they would have some difficulty in meeting with again. Mr. Phillips then proceeded to preserve the little reptile, which he did by carefully skinning it, and setting it in the position I subsequently saw it, and which the drawing annexed faithfully depicts. Seeing how much interest I took in the affair, Mr. Phillips presented the little animal to me to bring to London, as I told him I should be able, through some of my friends in town, to find out more about it. My friend in reply remarked that it had already been to London, and been shown at the British Museum, but to whom he could not say; and that the opinion he had received of it was to the effect that it was a young crocodile, and had very likely been dropped in a rain shower, or perhaps had escaped from some travelling menagerie. As both these ideas or suggestions were in my mind entirely out of the question, and as Mr. Phillips strengthened my belief, especially as regarded the latter suggestion, by saying that the "Minny Pool" was several miles away from any high road, I resolved on my arrival in London to consult my old schoolfellow and attached friend, Dr. Vesalius Pettigrew, on the subject of the little animal's history and habits, as I well knew I could not refer the matter to a more safe or competent naturalist to determine all that I wished to know, respecting the little fellow's birth, parentage, and education.

The result of my inquiry was that Dr. Pettigrew pronounced the reptile a young crocodile, with a mother and father, as he laughingly remarked, as long as the hearthrug in his room, or even longer, but how it had been found alive and killed in this country, he could not venture an opinion upon. At his request I

left the young reptile with him, to show to his friend Mr. Frank Buckland, who afterwards not only confirmed Dr. Pettigrew's views as to the character of the reptile, but subsequently, in a description of it in the Field newspaper, narrated the circumstances attending its discovery, giving it as his opinion that it had escaped from some travelling show---a thing not uncommon, as he attested by the instances of several such escapes that had come under his notice with little animals of a similar description, although he did not venture to say he had ever heard of a young crocodile being found alive some time after, in the country or town. This letter I replied to at the time, and I then gave the account of the discovery of the little creature in a similar way to which I have done now. I forget the date of these letters, but they will be found in the Field newspaper for, I think, the years 1861 or 1862. No further correspondence appeared on the subject, nor have I ever heard from my friend Mr. Phillips of the finding another specimen in or about his farm, although, in addition to his reward of one guinea, I offered two, for another specimen, dead or alive.

Thus far I have traced the narrative of this still, to me, very singular matter, and as I yet believe more information is to be obtained from the publicity of the story, I gladly avail myself of the opportunity offered for the publication of these notes in the pages of so renowned and national a work as The Gentleman's Magazine, reserving to myself the right of making, at some future time, further comments upon the subject, as I hold some very strong opinions in relation to the existence of what may be generally termed debased reptiles, descendants of a former greater race of which geology teaches us there were many types in our own country, although these "debased specimens" from many circumstances are very rarely to be met with at the present day, and of which this little crocodile may be one.

Since writing the foregoing, I have had the pleasure, through the introduction of a friend, of calling upon Professor Owen at the British Museum, with the little reptile in question, and he at once proclaimed the animal to be a crocodile, "not long from the egg," but would not in any way entertain the belief that the little creature had been found alive in this country. This he did, however, very courteously, merely intimating his idea that my friend Mr. Phillips, of Over-Norton, had been practised upon by his workmen, who had possibly thrown the little reptile in the farmyard for the purpose of deceiving their master. Nor would the Professor allow the notion of the escape of the animal from a travelling menagerie, nor that it had ever been hatched in this country; his notion being that it had been brought, preserved in spirits, from some distant spot, where crocodiles were common, and that it had been given to a friend in this country, and so had found its way at last to Oxfordshire. To strengthen his views upon the idea he possessed, of some deception having been practised upon the finder, he related the following circumstances, which only a few days before had occurred to himself. A gentleman came to him with an "Ornithorhyncus paradoxus," beautifully preserved, in a large glass case, and told him he had just bought

it of a man who was present, this man having asserted that he had caught and killed the animal in a pond somewhere in Hertfordshire, where it had been noticed alive for some time. The gentleman entirely believed the man, who roundly persisted in the truth of the story, even before the Professor; but who at last was so beaten by the questions put to him, and the flat contradictions of the Professor, who did not hesitate to accuse him of a gross deceit, that, to use the Professor's own words, he at last was glad to get out of his presence, and "slink away, evidently ashamed of himself."

This story is all very well, as far as it goes, and may serve to put people on their guard, especially where a money transaction is at the bottom of it, as was the case with the "Ornithorhyncus," the gentleman having given the man some forty shillings for the animal; but 1 cannot think it at all affects my friend's story, the truth of which 1 can vouch for, and which has never been doubted before. It is certain Mr. Phillips did not see the little creature alive, but the labourers who killed it would gain nothing by telling a deliberate falsehood in the matter, nor is it at all likely they would attempt the trick upon their master for no reason at all. Had they even called his attention to the dead reptile, there might have been a slight cause for suspicion; but they did nothing of the kind, and my full belief is that the creature might have remained where Mr. Phillips found it for many a long day, for any interest that the workmen in question would have taken in it.

Of course, after Professor Owen's opinion, 1 am bound to alter some of my pre-conceived ideas about this little reptile, as, till he decidedly said so, 1 was inclined to hope that it was not a true crocodile, as several worthy travelled friends of mine have thought, on looking carefully at it; but at the same time there are so many wonders and unaccountable things taking place in this busy world of ours, every day, which even Science and Philosophy cannot entirely unravel or grapple with, that I cannot help being of the opinion that more will come out of this interesting subject than at present is or can be known. And so to the readers of this paper, be they Naturalists or otherwise, 1 now commend the consideration, in all its details, of this mysterious find of a living crocodile on a farm in the northern part of Oxfordshire.

CROCODILES IN ENGLAND

Parr, C.; *Gentlemen's Magazine*, 5:2:640, 1866.

In reference to Mr. Wright's communication on this subject in your August number, in which he says that a young crocodile had been found a few years since near Chipping Norton, allow me to corroborate his statement by the following fact, which has come under my notice:---

A person formerly resident at Chipping Norton, crossing a field (situated above a piece of waste ground called the Common) in company with some other friends, was pursued by an animal of the crocodile kind, which chased them across the field; they had some difficulty in escaping from it, but eventually one of the lads crushed its head with a large stone. They were afraid to touch it afterwards, in case it should not be dead. The person (a woman) described it as being about a foot long; and crossing near the same place some years after, she saw a smaller animal of the same species. The first occurrence took place about thirty years ago. A footpath led past the pond from which this animal followed them. The pond has now been filled up for some years. The field was formerly in the occupation of Mr. Clark, and was called the Primsdown, or Primsden, Farm. I can vouch for the truth of these facts.

A CROCODILE ON ROTUMA

Gardiner, J. Stanley; *Nature*, 103:264, 1919.

Capt. W.W. Wilson, formerly harbour-master of Levuka, Fiji, has sent me a photograph of a crocodile taken by Mr. G. Missen. This animal landed alive on Rotuma in July, 1913, being afterwards speared by the natives. Rotuma lies 260 miles due north of the Yasawas, the most westerly islands of the Fiji group, and 600 miles east of the New Hebrides and Santa Cruz groups; the nearest Solomon islands are upwards of 300 miles further west.

The photograph represents a full-grown adult crocodile. Dr. H. Gadow has identified it as Crocodilus porosus, Schneider, a species which has the habit of wandering out to sea. It is found from the Bay of Bengal to the Solomon Islands. The British Museum Catalogue of Reptiles mentions Fiji as within its area of distribution, but gives no precise record of any occurrence there. It certainly did not come from Fiji or any lands to the east, as crocodiles do not now exist on them, though native legends of live crocodiles landing were rife in Fiji when I was there in 1896-97. It must indeed have crossed from the west, and covered at least 600 miles of open, landless sea. This occurrence is sufficiently remarkable to be placed on permanent record.

UNRECOGNIZED SPECIES

• Selected Sea Serpent and Lake Serpent Reports

THE GREAT SEA SERPENT

Deinboll, P. W.; *Zoologist*, 5:1604-1608, 1847.

It has been the fashion for so many years to deride all records of this very celebrated monster, that it is not without hesitation I venture to quote the following paragraphs in his defence. A month only has elapsed since 1 had occasion to quote with approbation, a very marked passage from the pen of Sir J.W. Herschell, [Zool. 1586]: I may apply it with equal propriety to the enquiry of the era of the Irish deer, or the existence of the great Sea Serpent. Naturalists, or rather those who choose thus to designate themselves, set up an authority above that of fact and observation, the gist of their enquiries is whether such things ought to be, and whether such things ought not to be; now fact-naturalists take a different road to knowledge, they inquire whether such things are, and, whether such things are not. The 'Zoologist,' if not in itself the fountain-head of this fact movement, may at least claim to be the only public advocate of that movement; and it is therefore most desirable, that it should call the attention of its readers to the following remarkable paragraphs. They are quoted from one our daily papers, which give them as literal translations from the Norse papers, in which they originally appeared; the localities mentioned are intimately known to all travellers in Norway; and the witnesses are generally highly respectable, and of unimpeachable veracity. The very discrepancies in the accounts prove the entire absence of any preconcerted scheme of deception. The only question therefore for the fact-naturalist to decide, is simply, whether all of the records now collected, can refer to whales, fishes, or any other marine animals with which we are at present acquainted.

"In the neighbourhood of Christiansand and Molde, in the province of Romsdal, several persons, highly respectable and credible witnesses, have reported that they have seen this animal. In general they state that it has been seen in the larger Norwegian fjords, seldom in the open sea. In the large bight of the sea at Christiansand, it has been seen every year, though only in the warmest season, in the dog-days, and then only when the weather was perfectly calm and the surface of the water unruffled. The following persons whose names are here mentioned,

give the subjoined testimony:--Nile Roe, workman at Mr. William Knudtzon's, relates: I saw the serpent twice, once at noon, and two days afterwards towards the evening, in the fjord at the back of Mr. Knudzton's garden. The first time it was about a hundred feet distant. It swam first along the fjord, then afterwards direct over the spot where 1 stood. 1 observed it for above half an hour. Some strangers who were on the opposite shore fired at it, when it disappeared. The second time it was further from me. It was small, perhaps twice as long as this room (about forty-four feet); while swimming it made serpentine movements, some to the side, others up and down. I cannot state what thickness it was, but it appeared to be about as thick as a common snake in proportion to its length. It was thinner towards the tail. The head was several times slightly elevated above above the surface of the water. The front of the head was rather pointed; the eyes were sharp, and glistened like those of a cat. From the back of the head a mane like that of a horse commenced, which waved backwards and forwards in the water. The colour of the animal was a blackish brown. John Johnson (merchant, about sixty years of age): I saw the animal some years since in the fjord; it was about a thousand paces distant when nearest to me. It swam very swiftly; in the same time that we rowed about a quarter of a mile to the side from it, it had swam about double the distance. I saw it most plainly when it swam in a semicircle round a tolerably large rock that obstructed its passage; in doing this it partly raised itself above the surface of the water. Its colour was blackish brown, and about the length of this house (fifty-five feet). With the exception of the head, I did not remark much of its body, as that appeared but little above the surface. Judging from what I observed, I should say the thickness of the body was about that of a stout man. The agitation it caused in the water was very strong. Its movements were serpentine, up and down, like a leech swimming. Lars Johnoen (fisherman at Smolen, about fifty years of age): I have several times seen the sea-serpent, but some time since, twelve years ago, in the dog-days, in the fjord not far from here, one afternoon as I was fishing in my boat, 1 saw it twice in the course of two hours, and for some time quite near me. It came close to my boat, so that it was only about six feet from me. 1 became alarmed, recommended my soul to God, laid down in the boat, and only held my head so far over it that 1 could observe the serpent. It swam now past the boat, that was agitated by the ripple caused by its movement in the water, which was previously smooth, and afterwards removed itself. After it had swam a considerable distance from me, 1 began again to fish. Not long afterwards, the serpent came close the the boat, which was strongly agitated by its movements in the water. I laid down and remained quite still, and notwithstanding my fright, kept a watchful eye on the animal; it passed me, disappeared and returned, though not so close as previously, and disappeared entirely when a light wind arose, and ruffled the water. Its length was about five to six fathoms, and the body, which was as round as a serpent's, was about two feet in diam-

eter. The tail seemed to be very round. The head was about as long as a brandy anker (ten gallon cask), and about the same thickness, it was not pointed but round. The eyes were very large, round, and sparkling. Their size was about the diameter of the box here (five inches), and they were as red as my neckerchief (crimson). Close behind the head, a mane like a horse's, commenced along the neck, and spread itself on both sides, right and left, while swimming on the water; it was of tolerably long hair. The mane as well as the head, and the rest of the body, was brown as this looking-glass frame (old mahogany). Spots, stripes of other colours I did not observe, nor were there any scales; it seemed as if the body was quite smooth. Its movements were occasionally fast and slow, which latter was the case when it neared my boat; I could clearly observe it; it was serpent-like, and moved up and down. The few undulations which those parts of the body and tail that were out of the water made, were scarcely a fathom in length. These undulations were not so high that I could see between them and the water. When Lars Johnoen had given this explanation, he was shown the drawing which Pontoppidan has given of this animal. He looked at it with astonishment, smiled, and said he found a great resemblance between it and the animal he had seen. He likewise said, that some of the other sea-serpents he had seen were a great deal longer than the one above described.

Mr. William Knudtzon and Candidatns Theologiae Bochlum, gave the following written account: We together saw the sea-serpent in a narrow fjord, at a distance of about one-sixteenth of a mile (half an English mile), for about a quarter of an hour; afterwards it dived, and came up so far from us we could not see it plainly. The water was smooth as a mirror, and the animal had, as it moved on the surface, the appearance of a serpent. Its motions were in undulations, and so strong that white foam appeared before it, and at the side, which stretched out several fathoms. It did not appear very high above the water, and its length was quite discernible. Once it stretched its head quite erect in the air. The body was somewhat dark, and the head nearly black, it had nearly the form of an eel, or snake, and a length of about 100 feet, and in proportion to it an inconsiderable thickness. The breadth diminished remarkably from the head, so much so that the tail ended in a point. The head was long and small in proportion to the throat, as the latter appeared much greater than the former, probably as it was furnished with a mane. Foged (Sheriff) Gottsche made the following remarks: I saw the sea-serpent for some time in a small fjord, first from a boat, afterwards from the beach, several minutes, at a distance of from thirty to thirty-six feet. In the beginning it swam round the fjord at Torvig, afterwards it went into the deeps. I saw its head stretched considerably out of the water. I remarked as well two or three undulations of the fore-part of the body. Its motion was not like that of an eel, but consisted in waving undulations, up and down. They were excessively strong, and caused tolerable large waves; they were largest at the fore-part of the animal, and towards the back gradually lessened. The traces of

them I discerned in a length of eight to ten fathoms, and a breadth of two to three fathoms. The head seemed blunted, and had the size and form of a ten-gallon cask, the undulations of the body were round and about the dimensions of a good timber stock (twelve to fourteen inches sqaure). The entire length of the animal I could not judge, as it was not possible to observe the extremity. Its colour appeared to be dark gray. At the back of the head there was a mane, which was the same colour as the rest of the body.

The writer of this article received letters from Mr. Soren Knudtzon, stating that a sea-serpent had been seen in the neighbourhood of Christiansand by several people, and from Dr. Hoffmann, a respectable surgeon in Molde, lying on a considerable fjord to the south of Christiansand, Rector Hammer, Mr. Kraft, curate, and several persons, very clearly saw, while on a journey, a sea-serpent of considerable size.

The Rev. Mr. Deinboll, Archdeacon of Molde, gives the following account of one which was seen last summer near Molde. The 28th of July, 1845, J.C. Lund, bookseller and printer; G.S. Krogh, merchant; Christian Flang, Lund's apprentice; and John Egenses, labourer, were out on Romsdale-fjord fishing. The sea was, after a warm sunshiny day, quite calm. About seven o'clock in the afternoon, a little distance from above, near the ballast place and Molde Hooe, they saw a long marine animal, which slowly moved itself forward, as it appeared to them, with the help of two fins, on the fore-part of the body nearest the head, which they judged from the boiling of the water on both sides of it. The visible part of the body appeared to be between forty and fifty feet in length, and moved in undulations like a snake. The body was round, and of a dark colour, and seemed to be several ells (an ell two feet) in thickness. As they discerned a waving motion in the water behind the animal, they concluded that part of the body was concealed under water. That it was one connected animal they saw plainly from its movement. When the animal was about one hundred yards from the boat, they noticed tolerably correctly its fore-part, which ended in a sharp snout; its colossal head raised itself above the water in the form of a semi-circle; the lower part was not visible. The colour of the head was dark brown and the skin smooth. They did not notice the eyes or any mane or bristles on the throat. When the serpent came about a musket-shot near, Lund fired at it, and was certain the shots hit it in the head. After the shot he dived, but came up immediately. He raised his head in the air like a snake preparing to dart on its prey. After he had turned and got his body in a straight line, which he appeared to do with great difficulty, he darted like an arrow against the boat. They reached the shore, and the animal perceiving it had come in shallow water, dived immediately, and disappeared in the deep.

Such is the declaration of these four men, and no one has any cause to question their veracity, or imagine that they were so seized with fear, that they could not observe what took place so near them. There are not many here, or on other parts of the Norwegian coast, who longer doubt the existence of the sea-

serpent. The writer of this narrative was a long time sceptical, as he had not been so fortunate as to see this monster of the deep, but after the many accounts he has read, and the relations he has received from creditable witnesses, he does not dare longer to doubt the existence of the sea-serpent.

THE GREAT SEA SERPENT

Anonymous; *Scientific American*, 4:66, 1848.

When the Daedalus frigate, Capt. M'Quae, arrived at Portsmouth, England, was on her passage home between the Cape of Good Hope and St. Helena, her Captain and most of her officers and crew, at 3 o'clock one afternoon, saw a sea serpent. The creature was twenty minutes in sight of the frigate, and passed under her quarter. Its head appeared to be about four feet out of the water, and there was about 60 feet of its body in a straight line on the surface. It is calculated that there must have been under water a length of 30 or 40 feet more. The diameter of the exposed part of the body was about 16 inches, and when it extended its jaws, which were full of large jagged teeth, they seemed sufficiently capacious to admit of a tall man standing upright between them.

THE GREAT SEA-SERPENT

Woodward; *Zoologist*, 6:2028, 1848.

"I, the undersigned, Joseph Woodward, captain of the Adamant schooner, of Hingham, being on my route from Penobscot to Hingham, steering W.N.W., and being about ten leagues from the coast, perceived, last Sunday, at 2 p.m., something on the surface of the water, which seemed to me to be of the size of a large boat. Supposing that it might be part of a wreck of a ship, I approached it, but when I was within a few yards of it, it appeared, to my great surprise, and that of my whole crew, that it was a monstrous serpent. When I approached nearer it coiled itself up, instantly uncoiling itself again, and withdrew with extreme rapidity. On my approaching again it coiled itself up a second time, and placed itself at the distance of 60 feet at the most from the box of the ship. I had one of my guns loaded with a cannon-ball and musket-bullets. I fired at the head of the monster; my crew and myself distinctly heard the ball and bullets strike against his body, from which they rebounded as if they had struck against a rock. The serpent shook his head and tail in an extraordinary manner, and advanced towards the ship with

open jaws. I had caused the cannon to be reloaded, and pointed it at his throat; but he had come so near that all the crew were seized with terror, and we thought only of getting out of his way: he almost touched the vessel, and had not I tacked as I did he would certainly have come on board. He dived; but in a moment we saw him appear again, with his head on one side of the vessel and his tail on the other, as if he was going to lift us up and upset us. However, we did not feel any shock. He remained five hours near us, only going backward and forward. The fears with which he at first inspired us having subsided, we were able to examine him attentively. I estimate that his length is at least twice that of my schooner, that is to say, 130 feet; his head is full 12 or 14; the diameter of the body, below the neck, is not less than 6 feet; the size of the head is in proportion to that of the body. He is of a blackish colour; his earholes are about 12 feet from the extremity of his head. In short, the whole has a terrible look. When the coils himself up, he places his tail in such a manner that it aids him in darting forward with great force: he moves in all directions with the greatest facility and astonishing rapidity."

[The foregoing statement was formally signed and sworn to at Hingham, by Captain Woodward, on the 12th of May.--E.N.]

THE GREAT SEA-SERPENT

Davidson, R.; *Zoologist,* 7:2458-2459, 1849.

"I see in your paper of the 30th December, a paragraph in which a doubt is expressed of the authenticity of the account given by Captain M'Quhae of the 'great sea-serpent.' When returning to India, in the year 1829, I was standing on the poop of the Royal Saxon, in conversation with Captain Petrie, the commander of that ship. We were at a considerable distance south-west of the Cape of Good Hope, in the usual track of vessels to this country, going rapidly along (seven or eight knots) in fine smooth water: it was in the middle of the day, and the other passengers were at lunch; the man at the wheel, a steerage passenger, and ourselves, being the only persons on the poop. Captain Petrie and myself at the same instant were literally fixed in astonishment by the appearance, a short distance ahead, of an animal of which no more generally correct description could be given than that by Captain M'Quhae. It passed within thirty-five yards of the ship, without altering its course in the least; but as it came right abreast of us, it slowly turned its head towards us. Apparently about one-third of the upper part of its body was above water in nearly its whole length, and we could see the water curling up on its breath as it moved along, but by what means it moved we could not perceive. We watched it going astern with intense interest, until it had nearly disappeared, when my companion, turning to me with a countenance

expressive of the utmost astonishment, exclaimed, 'Good heavens! what that can be?' It was strange that we never thought of calling the party engaged at luncheon to witness the extraordinary sight we had seen; but the fact is, we were so absorbed in it ourselves that we never spoke, and scarcely moved, until it had nearly disappeared. Captain Petrie, a superior and most intelligent man, has since perished in the exercise of his profession: of the fate of the others then on deck I am ignorant, so the story rests on my own unsupported word, but I pledge that word to its correctness. Professor Owen's supposition that the animal seen by the officers of the Daedalus was a gigantic seal I believe to be incorrect, because we saw this apparently similar creature in its whole length, with the exception of a small portion of the tail, which was under water; and by comparing its length with that of the Royal Saxon (about 600 feet), when exactly alongside in passing, we calculated it to be in that, as well as in its other dimensions, greater than the animal described by Captain M'Quhae. Should the foregoing account be of any interest to you, it is at your service: it is an old story, but a true one. I am not quite sure of our latitude and longitude at the time, nor do I exactly remember the date, but it was about the end of July.

ANOTHER PEEP AT THE SEA-SERPENT

Harrington, George Henry, et al; *Zoologist,* 16:5989, 1858.

I beg to enclose you a copy of an extract from the meteorological journal kept by me on board the ship 'Castilian,' on a voyage from Bombay to Liverpool. I have sent the original to the board of Trade, for whom the observations have been made during my last voyage. I am glad to confirm a statement made by the Commander of Her Majesty's ship 'Daedalus,' some years age, as to the existence of such an animal as that described by him. (G.H. Harrington; 14 and 14½, South Castle Street, Liverpool, February 2, 1858.)

"Copy of an Extract from the Board of Trade Meteorological Journal, kept by Capt. Harrington, of the ship 'Castilian,' from Bombay for Liverpool.

"Ship Castilian, Dec. 12, 1857, north-east end of St. Helena, bearing north-west, distance 10 miles.

"At 6:30 p.m., strong breezes and cloudy, ship sailing 12 miles per hour. While myself and officers were standing on the lee side of the poop, looking towards the island, we were startled by the sight of a huge marine animal which reared its head out of the water within 20 yards of the ship, when it suddenly disappeared for about half a minute and then made its appearance in the same manner again, showing us distinctly its neck and head about 10 or 12 feet out of the water. Its head was shaped like a long nun

buoy, and I suppose the diameter to have been seven or eight feet in the largest part, with a kind of scroll, or tuft of loose skin, encircling it about two feet from the top; the water was discoloured for several hundred feet from its head, so much so that on its first appearance my impression was that the ship was in broken water, produced, as I supposed, by some volcanic agency since the last time I passed island, but the second appearance completely dispelled those fears, and assured us that it was a monster of extraordinary length, which appeared to be moving slowly towards the land. The ship was going too fast to enable us to reach the mast-head in time to form a correct estimate of its extreme length, but from what we saw from the deck we conclude that it must have been over 200 feet long. The boatswain and several of the crew who observed it from the topgallant forecastle state that it was more than double the length of the ship, in which case it must have been 500 feet; be that as it may, I am convinced that it belonged to the serpent tribe; it was of a dark colour about the head, and was covered with several white spots. Having a press of canvass on the ship at the time I was unable to round to without risk, and therefore was precluded from getting another sight of this levaithan of the deep.

ANOTHER SEA SERPENT

van Lennep, J. H.; *Zoologist*, 17:6492, 1859.

The 'Amsterdamsche Courant' of October 6, 1858, inserts the following letter from Captain L. Byl, of the Dutch bark "Hendrik Ido Ambacht,' to the 'Jorn-Bode:'---"Sailing in the South Atlantic, in 27°27' N. lat. and 14°51' E. long., we perceived on July the 9th, between twelve and one o'clock in the afternoon, a dangerous sea-monster, which during nine days constantly kept alongside of us to 37°55' S. lat. and 42°9' E. long. This animal was about 90 feet long and 25 to 30 feet broad, and, most of the time, it struck the ship with such a force as to make it vibrate. The monster blew much water, which spread an unpleasant stench over the deck. The captain, fearing lest the animal might disable the rudder, did his utmost to get rid of his fearful antagonist, but without success. After it had received more than a hundred musket-balls, a harpoon and a long iron bar, blood was seen to flow from various wounds, so that at last, from loss of strength, the monster could swim behind our vessel no longer, and we were delivered of it. By its violent blows against the copper the animal's skin had been damaged in several places."

THE GREAT SEA SERPENT

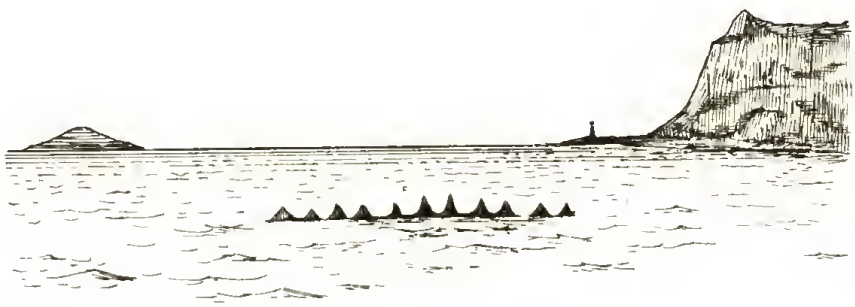
Anonymous; *Zoologist*, 21:8727, 1863.

The following is a copy of a letter from an officer of the African mail steamer 'Athenian,' addressed to a gentleman in this town:---"African Royal Mail Screw steamer 'Athenian," Cape Palmas, May 16, 1863.---My dear Sir,---All doubts may now be set at rest about the great sea serpent. On the 6th of May the African Royal Mail steamship 'Athenian,' on her passage from Teneriffe to Bathurst, fell in with one. At about 7 a.m. John Chapple, quartermaster, at the wheel, saw something floating towards the ship. He called the attention of the Rev. Mr. Smith and another passenger, who were on deck at the time, to it. On nearing the steamer it was discovered to be a large snake about 100 feet long, of a dark brown colour, head and tail out of water, the body sightly under. On its head was something like a mane or sea-weed. The body was about the size round of our mainmast. You are at liberty to publish this."

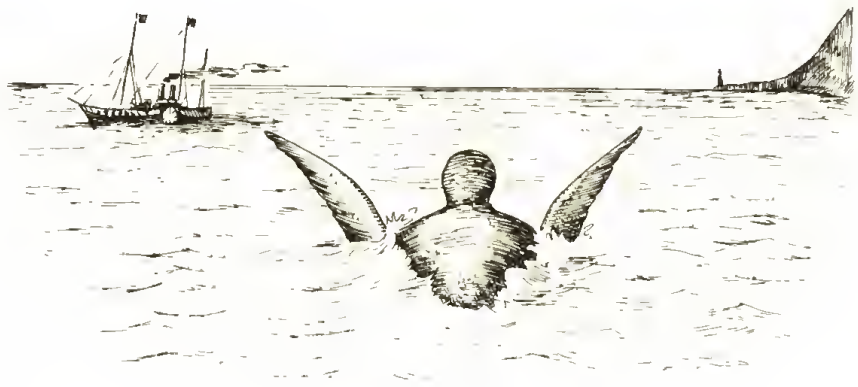
THE SEA MONSTER

Anonymous; *Scientific American*, 37:71, 1877.

We are indebted to Lieut. W. P. Haynes, of H.M.S. Osborne, for the sketch of the sea monster seen by the officers and crew of that vessel off the north coast of Sicily on the 2d inst., notice of which we gave in the Scientific American for July 14. In a letter accompanying the sketch he says: "My attention was first called by seeing a long row of fins appearing above the surface of the water at a distance of about 200 yards from the ship, and 'away on our beam.' They were of irregular heights, and extending about 30 or 40 feet in line (the former number is the length I gave, the latter the other officers); in a few seconds they disappeared, giving place to the fore part of the monster. By this time it had passed astern, swimming in an opposite direction to that we were steering; and as we were passing through



the water at 10½ knots, I could only get a view of it, 'end on,' which I have shown in the sketch. The head was bull-shaped, and quite 6 feet thick, the neck narrow, and its head was occasionally thrown back out of the water, remaining there for a few seconds at a time. It was very broad across the back or shoulders about 15 or 20 feet, and the flappers appeared to have a semi-revolving motion, which seemed to paddle the monster along. They were about 15 feet in length. From the top of the head to the part of the back where it became immersed, I should consider 50 feet, and that seemed about a third of the whole length. All this part was smooth, resembling a seal. I cannot account for the fins unless they were on the back below where it was immersed."



THE SEA SERPENT

Aldis, W. Steadman; *Nature*, 27:338, 1883.

In the summer of 1881 I was staying for some weeks at Veulettes, on the coast of Normandy. While there, on several occasions, several members of my party, as well as myself, saw, at a distance of three of four miles out at sea, what had the appearance of a huge serpent. Its length was many times that of the largest steamer that ever passed, and its velocity equally exceeded that of the swiftest. What seemed its head was lifted and lowered, and sometimes appeared to show signs of an open mouth. The general appearance of the monster was almost exactly similar to that of the figure in your correspondent's letter published on the 25th ult. Not the slightest appearance of discontinuity in its structure could be perceived by the eye, although it seemed incredible that any muscular mechanism could really drive such an enormous mass through the water with such a prodigious velocity. I carefully watched all that any of us caught sight of, and one day, just as one of these serpent forms was nearly opposite our hotel, it instantaneously turned through a right angle, but in-

stead of going forward in the new direction of its length, proceeded with the same velocity broadside forward. With the same movement it resolved itself into a flock of birds.

We often saw the sea-serpent again without this resolution being effected, and, knowing what it was, could with difficulty still perceive that it was not a continuous body; thus having a new illustration of Herschel's remark, that it is easier to see what had been once discovered than to discover what is unknown. Possibly this experience may afford the solution of your correspondent's difficulty.

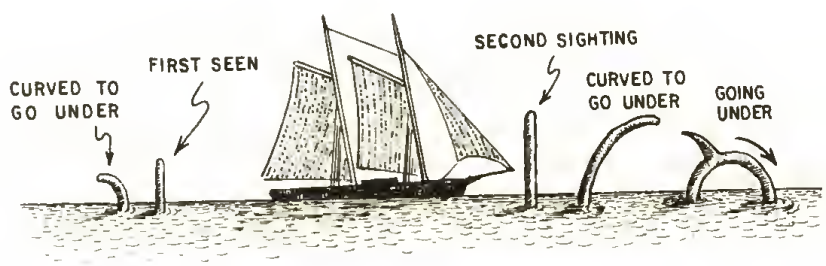
THE SEA-SERPENT

Colonna, B. A.; *Science*, 8:258, 1886.

With this please find an extract from an official report by Capt. Robert Platt, assistant coast and geodetic survey, with accompanying sketch of a 'sea-monster' seen by him near Cape Cod in October, 1878. Captain Platt is a trained observer, whose daily occupation at that time was to record just what he saw, and nothing more or less. I know Captain Platt so well that I have never doubted the existence of such a monster from the time his report was made known to me; and, if others have been sceptical, I hope that recent events have proven the matter beyond question.

[Extract from a report by Capt. Robert Platt, U.S. coast and geodetic survey, to the superintendent; written on board the U.S. coast-survey schooner Drift, Oct. 25, 1878.]

"I would also beg leave to state that Aug. 29, while becalmed off Race Point, Cape Cod, about four hundred yards from the vessel, we saw a sea-monster, or what I suppose has been called a sea-serpent. Its first appearance was that of a very large round spar two or three feet in diameter, from twelve to fifteen feet high, standing upright in the sea, but in a few minutes it made a curve and went down. It was visible about three minutes; the second appearance, about half an hour after the first, the monster came out of the water about twenty-five feet, then extended to about thirty-five or forty feet, and about three feet in diameter; when out about forty feet, it



curved and went down, and as it did so a sharp dorsal fin of about fifteen feet in length came up. This fin was connected to this monster, for the whole animal moved off with the same velocity. I looked at it with a good pair of glasses. I could not tell whether it had a mouth or eyes; it was of a brownish color. I enclose to you a rough sketch made by me, and submitted to all on board who saw the animal, and they all agree that it is a fair representation of the animal, as it appeared."

SEA SERPENT SIGHTING

Anonymous; *Zoologist,* 61:38-39, 1903.

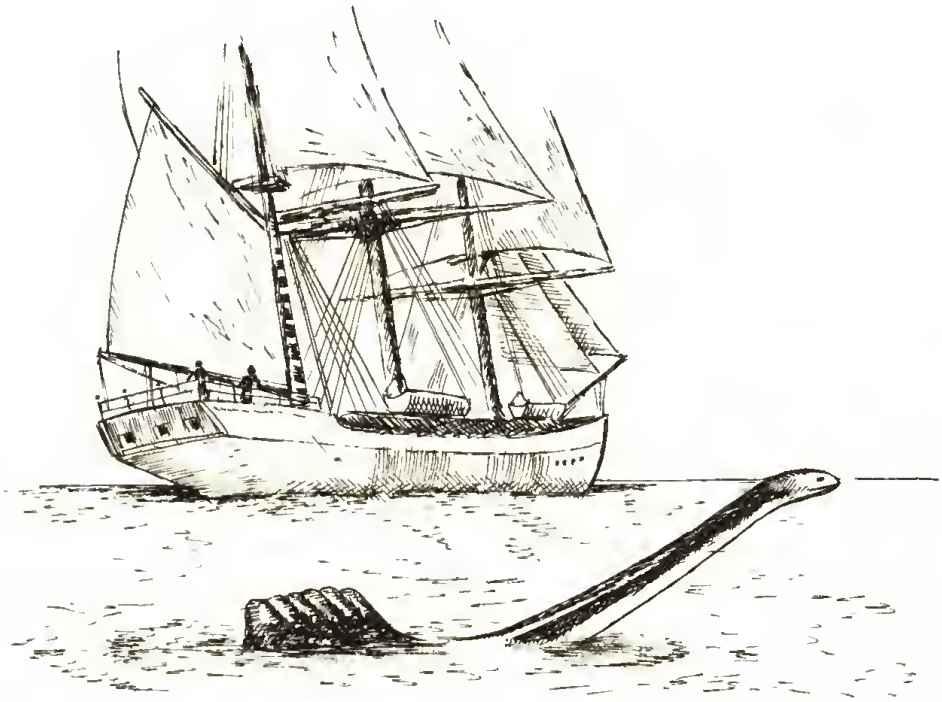
Extract from the log of the second officer of the s.s. Fort Salisbury:--October 28, 1902, 3.5 a.m.--Dark object, with long, luminous trailing wake, thrown in relief by a phosphorescent sea, seen ahead, a little on starboard bow. Look-out reported two masthead lights ahead. These two lights, almost as bright as a steamer's lights, appeared to shine from two points in line on the upper surface of the dark mass. Concluded dark mass was a whale, and lights phosphorescent. On drawing nearer, dark mass and lights sank below the surface. Prepared to examine the wake in passing with binoculars. Passed about forty to fifty yards on port side of wake, and discovered it was the scaled back of some huge monster slowly disappearing below the surface. Darkness of the night prevented determining its exact nature, but scales of apparently 1 ft. diameter, and dotted in placed with barnacle growth were plainly discernible. The breadth of the body showing above water tapered from about 30 ft. close abaft, where the dark mass had appeared to about 5 ft. at the extreme end visible. Length roughly about 500 ft. to 600 ft. Concluded that the dark mass first seen must have been the creature's head. The swirl caused by the monster's progress could be distinctly heard, and a strong odour like that of a low-tide beach on a summer day pervaded the air. Twice along its length the disturbance of the water and a broadening of the surrounding belt of phosphorus indicated the presence of huge fins in motion below the surface. The wet, shiny back of the monster was dotted with twinkling phosphorescent lights, and was encircled with a band of white phosphorescent sea. Such are the bare facts of the passing of the Sea Serpent in latitude 5 deg. 31 min. S., longitude 4 deg. 42 min. W., as seen by myself, being officer of the watch, and by the helmsman and look-out man.-- A.H. Raymer, Second Officer.

Mr. S.G. Stephens (master of the Fort Salisbury) writes in reference to Mr. Raymer's narrative: "I can only say that he is very earnest on the subject, and certainly has, together with look-out and helmsman, seen something in the water of a huge nature as specified."

SEA SERPENT—RIDDLE OF THE DEEP

Carrington, Richard; *Natural History,* 66:183–187 + , 1957.

The next important appearance of a sea serpent was in view of two expert naturalists who were actually taking part in a scientific expedition. The episode took place in the winter of 1905 off the northeast coast of Brazil and was witnessed from the Earl of Crawford's yacht Valhalla by E. G. B. Meade-Waldo and Michael J. Nicoll, both of the Zoological Society of London. Here is the story in Meade-Waldo's own words: "I saw a large fin or frill sticking out of the water, dark sea-weed-brown in color, somewhat crinkled at the edge. It was apparently about 6 feet in length and projected from 18 inches to 2 feet from the water. I could see, under the water to the rear of the frill, the shape of a considerable body. I got my field-glasses on to it and almost as soon as I had them on the frill, a great head and neck rose out of the water in front of the frill; the neck did not touch the frill in the water, but came out of the water in front of it, at a distance of certainly not less than 18 inches, probably more. The neck appeared about the thickness of a slight man's body, and from 7 to 8 feet was out of the water; head and neck were all about the same thickness. The head had a very turtle-like appearance, as had also the eye. I could see the line of the



In 1905 two scientists aboard the Valhalla *sighted this large unidentifiable marine creature.*

mouth, but we were sailing pretty fast, and quickly drew away from the object, which was going very slowly. It moved its neck from side to side in a peculiar manner: the color of the head and neck was dark brown above, and whitish below---almost white, I think. . . . Since I saw this creature I consider on reflection that it was probably considerably larger than it appeared at first, as I proved that objects, the size with which I was well acquainted, appear very much smaller than they really are when seen on the ocean at a similar distance with nothing to compare them with."

Michael Nicoll's report added that the head and neck, which he estimated as being about 6 feet long, lashed up the water with a curious wriggling movement. He also compared the fin to a gigantic piece of ribbon seaweed, and added that every now and then it disappeared entirely below the surface. In his book, Three Voyages of a Naturalist (1908), Nicoll observed, "I feel sure that it was not a reptile that we saw, but a mammal. It is, of course, impossible to be certain of this, but the general appearance of the creature, especially the soft, almost rubber-like fin, gave one this impression." (pp. 185-186)

SEA SERPENT SIGHTINGS

Anonymous; *Nature,* 125:469, 1930.

An interesting and important piece of evidence for the existence of the creature popularly called the 'sea-serpent' has recently come to hand in the shape of a letter in which Capt. F.W. Dean, R.N. (retired) describes a creature seen by himself and several of the officers and men of H.M.S. Hilary in May 1917. He relates the incident as follows: "About 9 a.m. on approx. 22/5/17, H.M.S. Hilary was some 70 miles S.E. of the S.E. part of Iceland, the day very fine and clear, the Iceland mountains in sight, flat, calm, and smooth sea. An object was observed on starboard quarter. The ship was turned round and steered straight for the object. When we were about a cable (200 yards) from it the creature quietly moved out of our way and we passed it on our starboard side at a distance of about 30 yards, getting a very good view of it....As we passed close to the creature it lifted its head once or twice as if looking at us. The head was in appearance black and glossy, with no protrusions such as ears, etc., in shape about that of a cow...the top edge of the neck was just awash, and it curved to almost a semicircle as the creature moved its head as if to follow us with its eyes. The dorsal fin was a black equilateral triangle which rose at times till the peak was estimated to be four feet above the water."

Three independent estimates made on board the Hilary gave the length of the neck of the 'sea serpent' (head to dorsal fin) as 15 feet or more, 20 feet, and 28 feet. The head appeared to have a patch of whitish flesh in front, "like that around a

cow's nostrils". The dorsal fin was thin and flexible, occasionally curving over at the top. The Hilary being on patrol at the time, the unfortunate creature was used as a target for anti-submarine practice with the 6-pounders, at about 1200 yards range. A direct hit having apprently been scored, it disappeared, no trace remaining. A few days later (May 25, 1917), the ship was torpedoed and sunk, taking with her all logs, journals, etc., recording the 'sea-serpent' incident. As described, the creature seems to have borne a most striking resemblance to that seen off the Brazilian coast, in December 1905, from the Earl of Crawford's yacht Valhalla (see Nature, June 28, 1906, p.202). This, also, exhibited a dorsal fin rising some four feet out of water, and a long, snake-like neck, terminating in a head described as resembling that of a turtle. In this case, the head and part of the neck were lifted well clear of the water, and not merely floating awash. In both cases there seems no doubt that the observers saw a single living sea-creature of unknown species.

GIANT WATER SNAKE

Brown, A. H.; *Marine Observer,* 12:5, 1935.

The following is an extract from the Meteorological Record of S.S. Port Bowen, Captain A.H. Brown, Fremantle to Perim, observer Mr. R. Bittess, 3rd officer.

On March 2nd, 1934, about 12.40 p.m. while on passage from Fremantle to Perim a large water snake was seen from the bridge of this vessel. When first sighted it was about three hundred yards away, one point aft side the beam on the starboard side. It was travelling along slowly in a direction approximately southwest or at right angles to the course, vessel steering 318°. The head was held about two feet clear of the water while the rest of the body was lying either along or close underneath the surface of the sea. After a few seconds the head dropped and the whole of the snake was then seen lying along the top of the sea. The body was of a dirty yellowish colour with black markings along the back which were not easily discernible even with the aid of binoculars. The forward motion of the snake through the water was evidently supplied by a continuous wriggling movement from the head to the tail, as no fins of any shape or form were visible. Its speed was estimated to about 2 knots, as the time taken from when it was first sighted to the position where it disappeared just clear of the vessel's wake, was approximately four minutes. The length of the creature was estimated by the observers to be about fifty feet, with a diameter of about two feet. The middle of the body was apparently of greater girth than the head or tail. After apparently diving on approaching the vessel's wake nothing further was seen of it, although a good lookout was kept in case it appeared again. Weather fine and clear with passing clouds. Visibility excellent. Slight sea and S.S.E. swell. Nearest land

Danger Island. Chagos Arch., 088°, distant 170 miles. Position of vessel, Latitude 6° 35' S., Longitude 68° 18' E.

OGOPOGO, CANADA'S LAKE MONSTER: OFT SEEN, NEVER SNARED

Moon, Mary; *Smithsonian Magazine*, 9:173+, November 1978.

Edward Fletcher of Vancouver, Canada, hastily shut off the engine of his high-speed runabout on August 3, 1976, to avoid colliding with "a garter snake blown up to 70 feet long." The boat bearing Fletcher and his daughter Diane, 14, across Okanagan Lake at Westbank, British Columbia, drifted to within 15 feet of "a strange creature" swimming with a flattened spiral motion. They were encountering the phenomenon called "Ogopogo," North America's version of the Loch Ness monster.

Fletcher stared at one four-foot hump as it passed, then raced back to shore for a camera. Back in the water, spotting "a strange wave" of the kind reported many times at Westbank, Fletcher shut off his engine and took photographs that recorded a giant serpent--or something. Filming bubbles it made when submerging, he got a disturbing picture showing something swimming under his boat, its long neck and blunt head raised in the distance.

Eleven months later, Erin Neely came too close to Ogopogo and went into shock. On July 2, 1977, she was waterskiing, saw Ogopogo as she approached it, dropped her towline and fell flat in the water. The thing underneath her, three feet away, gave her the distinction of tying Ernest A. Lording of Kelowna for first place in coming closest to Okanagan Lake's natural history mystery.

Other recent sighters (as with UFOs and Bigfoots or Sasquatches [Smithsonian, February 1974], Ogopogo encounters are called "sightings") were Betty and Harry Staines of Westbank. On November 5, 1977, they glimpsed it near the west end of the mile-long bridge from Westbank to Kelowna. They were the latest among thousands to see something strange in this narrow, 80-mile-long lake in southern British Columbia. (p. 173)

AN ANTEDILUVIAN MONSTER

Matters, Leonard; *Scientific American*, 127:21, July 1922.

Whatever may be the opinion in scientific circles--and it is one of scornful skepticism--the fact remains that the report of the existence in Argentina of a living specimen of a race of mammoth

reptiles, supposed to have been extinct for millions of years has excited intense interest. Every scientist of note has scoffed at the idea, but, headed by Dr. Clement Onelli, Director of the Zoological Gardens at Buenos Aires, there is a large body of Argentine public opinion strongly of opinion that a member of the plesiosaurus family will be discovered, alive or dead.

Dr. Onelli has disclosed to the press in Buenos Aires the fact that some time ago he received a letter from an American mining man down in Patagonia, reporting the existence of some monster, of a type well known to palentologists through fossil remains, but unknown to modern zoology. The American, a Mr. Sheffield, has been prospecting for gold along the Andes in the Territory of Chubut for many years, and Dr. Onelli says he knows him to be perfectly reliable. Mr. Sheffield wrote to the doctor stating that he had come across some enormous tracks near a lake. Bushes and undergrowth were trodden into the ground, and so deep were the tracks that they could only have been made by some living thing of ponderous weight. Continuing his account, Mr. Sheffield said: "I saw in the middle of the lake an animal with a huge neck like that of a swan, and the movement in the water made me suppose the beast to have a body like that of a crocodile."

Dr. Onelli has pointed out that Mr. Sheffield has given, for a layman, a remarkably accurate outline of the plesiosaurus, one of the mammoth reptiles that lived countless years before the ice-age, and became extinct during that geological epoch in the world's history. Moreover, the report recalls more than one similar story of the existence in Patagonia of some strange creature, never yet clearly identified. Patagonia was undoubtedly at some time the habitat of mammoth beasts, and prehistoric man. Extensive fossil beds have been located, and scientific knowledge has been widely enriched by the discovery of many kinds of extinct mammals and reptiles. Professor Frederic B. Loomis conducted an expedition through the Territory of Chubut in 1911, on behalf of Amherst College, and was successful in unearthing a petrified forest and, among other interesting fossils, the skull of a pyrotherium, an extinct elephantine creature.

There has long existed a romantic sort of idea that in the comparatively uninhabited parts of Patagonia, where certain forms of vertebrates are believed to have originated, a discovery would yet be made, of a nature that would stagger science--the finding of the "missing link," for instance, or more probably the capture of an actual living specimen of some supposedly extinct creature of antediluvian antiquity. Palentologists and scientists in general have never been associated with such a belief, but nevertheless it has been a popular fancy, and has not lacked some degree of support among men whose opinion carries weight. In announcing the latest report to the public Dr. Onelli has recalled this interesting speculation. He was personally first attracted by the idea 25 years ago when he was down in Patagonia. On the shores of White Lake there then lived a Chilean farmer who declared that often at night a strange noise could be heard, as though a cart were being dragged over the pebbly shore. It was asserted

that on moonlight nights a huge beast could be seen in the lake. It had a long reptilian neck which rose high above the water, but when distrubed it would at once dive and disappear in the depths.

Ten years after hearing of the apparition in White Lake, Dr. Onelli heard of the discovery made by a Norwegian engineer named Vaag, who was a member of the commission of demarcation of the boundary between Argentina and Chile. Mr. Vaag gave the name of River Tamango to a waterway flowing from the Andes, on whose banks he found the remains of some great beast, and the tracks of a creature that probably resembled the plesiosaurus. Later on, in a cavern, further south at Ultima Esperanza (Last Hope) there were found the remains of a hide and many bones of a great quadruped. The condition of the hide and the fact that the bones were conserved in a gelatinous condition, gave rise to the belief that it might be possible to find a similar beast alive. These interesting discoveries led to a number of expeditions, to which popular imagination at least attracted the objective of looking for a surviving specimen of the mammoth age, even if the scientists themselves had no such belief in the idea. These expeditions worked in different parts of Patagonia, and made interesting discoveries, though they found nothing to warrant any credence in the popular theory supposing the existence of zoological phenomena.

This, in brief, is the history behind the most recent report made on the authority of Dr. Onelli. Science in general utterly discredits the suggestion that a plesiosaurus, or anything like the extinct reptile, is still alive, and the many scornful comments on Dr. Onelli's report leaves it doubtful whether scientists admit even the possibility of there being in Patagonia some unknown animal of large proportions. It has been hinted that the creature with the long swan-like neck is in the same class as the sea-serpent, and is really born in a bottle. However, the refusal of scientists to take Mr. Sheffield's story and Dr. Onelli's theory as credible, has not prevented the doctor and the Argentine people getting deeply interested in a proposal to fit out an expedition for the capture of the creature, alive or dead.

So seriously has the idea been taken up that a rather amusing development has followed. Dr. Albarrin, President of the Society for the Protection of Animals, has demanded that the Minister for the Interior shall stop the expedition, under Law No. 2,786, which prohibits the hunting and destruction of rare animals. Dr. Albarrin urges that if there should still exist a plesiosaurus, then Argentina possesses a scientific marvel of immense value, and it should neither be killed nor captured, but should be allowed to enjoy to the full the liberty it has had over millions of years. The worthy doctor claims that it would be more humane and creditable to Argentine culture to leave the creature where it is and let it reproduce its species.

• The Minhocao

ON THE MINHOCAO OF THE GOYANES

de Saint Hilaire, Auguste; *American Journal of Science,* 2:4:130–131, 1847.

Luiz Antonio da Silva e Souza, whose acquaintance I made during my travels, and to whom we owe the most valuable researches on the history and statistics of Goyaz, says, in speaking of the lake of Padre Aranda, situated in this vast province, that it is inhabited by minhocoes; then he adds that these monsters--it is thus he expresses himself--dwell in the deepest parts of the lake, and have often drawn horses and horned cattle under the water. The industrious Pizarro, who is so well acquainted with all that relates to Brazil, mentions nearly the same thing, and points out the lake Feia, which is likewise situated in Goyaz, as also being inhabited by minhocoes.

I had already heard of these animals several times, and I considered them as fabulous, when the disappearance of horses, mules and cattle, in fording the rivers, was certified by so many persons, that it became impossible for me altogether to doubt it.

When I was at the Rio dos Piloes, I also heard much of the minhocoes; I was told that there were some in this river, and that at the period when the waters had risen, they had often dragged in horses and mules whilst swimming across the river.

The word minhocuo is an augmentative of minhoca, which in Protuguese signifies earth-worm; and indeed they state that the monster in question absolutely resembles these worms, with this difference, that it has a visible mouth; they also add, that it is black, short, and of enormous size; that it does not rise to the surface of the water, but that it causes animals to disappear by seizing them by the belly.

When, about twenty days after, having left the village and the river of Piloes, I was staying with the Governor of Meiapont, M. Joaquim Alvez de Oliveira, I asked him about these minhocoes: he confirmed what I had already been told, mentioned several recent accidents caused by these animals, and assured me at the same time, from the report of several fishermen, that the minhocao, notwithstanding its very round form, was a true fish provided with fins.

I at first thought that the minhocao might be the Gymnolui carapa, which according to Pohl is found in the Rio Vermetho, which is near to the Rio dos Piloes; but it appears from the Austrian writer that this species of fish bears the name of Terma termi in the country; and moreover the effects produced by the Gymnoti are, according the Pohl, well known to the mulattos and negroes who often felt them, and have nothing in common with what is related of the minhocao. Professor Gervais, to whom I mentioned my doubts, directed my attention to the description

which P.L. Bischoff has given of the Lepidosiren; and indeed the little we know of the minhocao agrees well enough with what is said of the rare and singular animal discovered by M. Natterer.

That naturalist found his Lepidosiren in some stagnant waters near the Rio da Madeira and of the Amazon: the minhocao is not only said to be in rivers, but also in lakes. It is, without doubt, very far from the lake Feia to the two localities mentioned by the Austrian traveller; but we know that the heats are excessive at Goyaz. La Serra da Paranahyba e do Tocantim, which crosses this province, is one of the most remarkable dividers of the gigantic water-courses of the north of Brazil from those of the south; the Rio dos Piloes belongs to the former, as does the Rio da Madeira. The Lepidosiren paradoxa of M. Natterer has actually the form of a worm, like the minhocao. Both have fins; but it is not astonishing that they have not always been recognized in the minhocao, if, as in the Lepidosiren, they are in the animal of the Rio dos Piloes reduced to simple rudiments. "The teeth of the Lepidosiren," says Bischoff, "are well-fitted for seizing and tearing its prey; and to judge of them from their structure and from the muscles of their jaw, they must move with considerable force." These characters agree extremely well with those which we must of necessity admit in the minhocao, since it seizes very powerfully upon large animals and drags them away to devour them. It is therefore probably that the minhocao is an enormous species of Lepidosiren; and we might, if this conjecture were changed into certainty, join this name to that of the minhocao to designate the animal of the lake Feia and of the Rio dos Piloes. Zoologists who travel over these distant countries will do well to sojourn on the borders of the lake Feia, of the lake Padre Aranda, or of the Rio dos Piloes, in order to ascertain the perfect truth--to learn precisely what the minhocao is; or whether, notwithstanding the testimony of so many persons, even of the most enlightened men, its existence should be, which is not very likely, rejected as fabulous.

A NEW UNDERGROUND MONSTER

Anonymous; *Nature,* 17:325-326, 1878.

A recent communication from Fritz Muller, of Itajahy, in Southern Brazil, to the Zoologische Garten contains a wonderful account of the supposed existence of a gigantic earthworm in the highlands of the southern provinces of Brazil, where it is known as the "Minhocao." The stories told of this supposed animal, says Fritz Muller, sound for the most part so incredible, that one is tempted to consider them as fabulous. Who could repress a smile at hearing men speak of a worm some fifty yards in length, and five in breadth, covered with bones as with a coat of armour, uprooting mighty pine trees as if they were blades of grass, diverting the courses of streams into fresh channels,

and turning dry land into a bottomless morass? And yet after carefully considering the different accounts given of the "Minhocao," one can hardly refuse to believe that some such animal does really exist, although not quite so large as the country folk would have us to believe.

About eight years ago a "Minhocao" appeared in the neighbourhood of Lages. Francisco de Amaral Varella, when about ten kilometres distant from that town, saw lying on the bank of the Rio das Caveiras a strange animal of gigantic size, nearly one metre in thickness, not very long, and with a snout like a pig, but whether it had legs or not he could not tell. He did not dare to seize it alone, and whilst calling his neighbours to his assistance, it vanished, not without leaving palpable marks behind it in the shape of a trench as it disappeared under the earth. A week later a similar trench, perhaps constructed by the same animal, was seen on the opposite side of Lages, about six kilometres distant from the former, and the traces were followed, which led ultimately under the roots of a large pine tree, and were lost in the marshy land. Herr F. Kelling, from whom this information was obtained, was at that time living as a merchant in Lages, and saw himself the trenches made by the "Minhocao." Herr E. Odebrecht, while surveying a line of road from Itajahy into the highlands of the province of Santa Caterina, several years ago, crossed a broad marshy plain traversed by an arm of the river Marombas. His progress here was much impeded by devious winding trenches which followed the course of the stream, and occasionally lost themselves in it. At the time Herr Odebrecht could not understand the origin of these peculiar trenches, but is now inclined to believe that they were the work of the "Minhocao."

About fourteen years ago, in the month of January, Antonio Jose Branco, having been absent with his whole family eight days from his house, which was situated on one of the tributaries of the Rio dos Cachorros, ten kilometres from Curitibanos, on returning home found the road undermined, heaps of earth being thrown up, and large trenches made. These trenches commenced at the source of a brook, and followed its windings; terminating ultimately in a morass after a course of from 700 to 1,000 metres. The breadth of the trenches was said to be about three metres. Since that period the brook has flowed in the trench made by the "Minhocao." The path of the animal lay generally beneath the surface of the earth under the bed of the stream; several pine trees had been rooted up by its passage. One of the trees from which the Minhocao in passing had torn off the bark and part of the wood, was said to be still standing and visible last year. Hundreds of people from Curitibanos and other places had come to see the devastation caused by the Minhocao, and supposed the animal to be still living in the marshy pool, the waters of which appeared at certain times to be suddenly and strangely troubled. Indeed on still nights a rumbling sound like distant thunder and a slight movement of the earth was sensible in the neighbouring dwellings. This story was told to Herr Muller by two eye-witnesses, Jose, son of old Branco, and a stepson, who formerly

lived in the same house. Herr Muller remarks that the appearance of the Minhocao is always supposed to presage a period of rainy weather.

In the neighbourhood of the Rio dos Papagaios, in the province of Parana, one evening in 1849 after a long course of rainy weather, a sound was heard in the house of a certain Joao de Deos, as if rain were again falling in a wood hard by, but on looking out, the heavens were seen to be bright with stars. On the following morning it was discovered that a large piece of land on the further side of a small hill had been entirely undermined, and was traversed by deep trenches which led towards a bare open plateau covered with stones, or what is called in this district a "legeado." At this spot large heaps of clay turned up out of the earth marked the onward course of the animal from the legeado into the bed of a stream running into the Papagaios. Three years after this place was visited by Senhor Lebino Jose dos Santos, a wealthy proprietor, now resident near Curitibanos, He saw the ground still upturned, the mounds of clay on the rocky plateau, and the remains of the moved earth in the rocky bed of the brook quite plainly, and came to the conclusion that it must have been the work of two animals, the size of which must have been from two to three metres in breadth.

In the same neighbourhood, according to Senhor Lebino, a Minhocao had been seen several times before. A black woman going to draw water from a pool near a house one morning, according to her usual practice, found the whole pool destroyed, and saw a short distance off an animal which she described as being as big as a house moving off along the ground. The people whom she summoned to see the monster were too late, and found only traces of the animal, which had apparently plunged over a neighbouring cliff into deep water. In the same district a young man saw a huge pine suddenly overturned, when there was no wind and no one to cut it. On hastening up to discover the cause, he found the surrounding earth in movement, and an enormous worm-like black animal in the middle of it, about twenty-five metres long, and with two horns on its head.

In the province of Sao Paulo, as Senhor Lebino also states, not far from Ypanema, is a spot that is still called Charquinho, that is, Little Marsh, as it formerly was, but some years ago a Minhocao made a trench through the marsh into the Ypanema River, and so converted it into the bed of a stream.

In the year 1849, Senhor Lebino was on a journey near Arapehy, in the State of Uruguay. There he was told that there was a dead Minhocao to be seen a few miles off, which had got wedged into a narrow cleft of a rock, and so perished. Its skin was said to be as thick as the bark of a pine-tree, and formed of hard scales like those of an armadillo.

From all these stories it would appear conclusive that in the high district where the Uruguay and the Parana have their sources, excavations, and long trenches are met with, which are undoubtedly the work of some living animal. Generally, if not always, they appear after continued rainy weather, and seem to start from marshes or river-beds, and to enter them again. The

accounts as to the size and appearance of the creature are very uncertain. It might be suspected to be a gigantic fish allied to Lepidosiren and Ceratodus; the "swine's snout," would show some resemblance to Ceratodus, while the horns on the body rather point to the front limbs of Lepidosiren, if these particulars can be at all depended upon. In any case, concludes Herr Muller, it would be worth while to make further investigations about the Minhocao, and, if possible, to capture it for a zoological garden!

To conclude this remarkable story, we may venture to suggest whether, if any such animal really exist, which upon the testimony produced by Fritz Muller, appears very probable, it may not rather be a relic of the race of gigantic armadilloes which in past geological epochs were so abundant in Southern Brazil. The little Chlamydophorus truncatus is, we believe, mainly, if not entirely, subterranean in its habits. May there not still exist a larger representative of the same or nearly allied genus, or, if the suggestion be not too bold, even a last descendant of the Glyptodonts?

UNDERGROUND MONSTERS

Anonymous; *Nature,* 18:389, 1878.

In a former number (vol. xvii. p.325) we gave some account of a curious underground monster, the Minhocao, supposed to exist in Brazil. Dr. Spencer Baird, of the Smithsonian Institution, sends us an interesting document, which shows that the belief in such a monster is not confined to Brazil, but is shared in by the people of Nicaragua. In the Gaceta de nicaragua for March 10, 1866, is a long letter signed "Paulino Montenegro," containing a circumstantial account of an object possessing very much the same attributes as the Minhocao. The letter is dated Jinotega, Nicaragua, February 21, 1866. The writer states that he went to Concordia on private business, when he heard on the 17th of a serpent having taken up its abode at a place called La Cuchilla, within the jurisdiction of the village. Along with some friends, M. Montenegro set out on the 18th to examine into the foundation of the report. A tradition concerning such a monster has existed from "time immemorial." After having travelled on that day about two leagues (1 league = 2.6 English miles) north-east from the village, they reached the spot where the inhabitants of the neighbourhood had traced signs and tracks, which, M. Montenegro states, positively prove the existence of such an animal.

The most detailed accounts stated that here, some five years before, a sort of platform of about fifty varas diameter had been formed at the foot of a large rock cropping out from a hillside. One of the neighbours had established there an orchard, though no one had been able to account for this new formulation. Three years before, however, people began to observe that this little piece of level ground was gradually deepening, and that in the

month of November the base of the rock adjoining it became exposed and worn from some agency, notwithstanding that there was not sufficient water to cause the phonomenon. At the same time mighty trees (robles) were observed to become uprooted and to fall in great disorder, while immense rocks were moved and shifted their foundations so much, that in the following month of December, during one night, the road from Chichiguas and Cuchilla to San Rafael del Norte was destroyed by a multitude of cracks and clefts, which had suddenly opened. At that time the ground was observed to be undermined, falling in at intervals. These occurrences were observed some three days before M. Montenegro and his friends visited the place, which they saw all to be in accordance with the statements. Immediately on examining the locality for themselves they came to the conclusion that there were signs not of one but of two animals, probably of the shape of huge fishes.

In commencing their work these animals seemed to pursue a kind of an upheaving movement. As the bottom of their hiding place was loose, shifting ground, the surface of this was seen to give way, while trees were shaken out and came down crashing. The noise of this seemed to scare the animals away. One of them--believed to have been the male on account of its larger size and greater strength--took to the left in descending, but always in a parallel direction with and along the slope of another hill, which here terminated. As it broke through the banks of a ravine, which measured about twenty varas in width and nine feet in depth at its greatest opening, he passed with his head underground. The thrown-up soil showed the tracks of the head, which left its marks both in the soil and on the roots of the trees, which were broken, the broken pieces being four inches thick. The main part of the body, which certainly must have passed here uncovered, left its traces at the bottom of the ravine. Passing out from this the animal entered upon ground more level and friable, which it went through at a depth of five quarters (1.25 varas), forming a furrow and leaving behind a ridge more than one vara high. Following the ravine for a distance of about sixty varas it encountered two deep ditches, then it turned and traced its way back, and, approaching the aforesaid ravine, took to the bed of a pond and disappeared perpendicularly.

The other animals, which left behind a smaller track, and therefore was believed to have been the female, went at once to the right, to the outlet of the pond of water before referred to, leaving behind it everywhere the same marks as the other. When it reached the two deep ditches it turned back also, and undoubtedly encountered its companion afterwards.

The whole ground had become irregularly disturbed and broken up, and the power of these animals is shown by their being able not only to throw up huge masses of soil but even to move rocks weighing more than thirty quintals.

The animals seem to be covered with a skin clad with scales or plates, the markings of which, imprinted on the soft clay or loam, bear much resemblance to those of the garrobo in the mud. It appears that the shape of these animals must be like that of

the guapote. The length of the body is at least twelve varas, the height three, and its thickness 1.5 varas.

A tradition about such an animal as this has been kept up unaltered, without contradiction, for more than a hundred years. It is described in general as a large snake, and called "sierpe," on account of its extraordinary size, and living in chaquites. One is said to have been once killed by lightning the moment it had left its hiding-place in the river "Sebaco viejo."

A BOLIVIAN SAURIAN

Anonymous; *Scientific American,* 49:3, 1883.

"The Brazilian Minister at La Paz, Bolivia, had remitted to the Minister of Foreign Affairs in Rio photographs of drawings of an extraordinary saurian killed on the Beni after receiving thirty-six balls. By order of the President of Bolivia the dried body, which had been preserved in Asuncion, was sent to La Paz. It is twelve meters long from snout to point of the tail, which latter is flattened. Besides the anterior head, it has, four meters behind, two small but completely formed heads (?) rising from the back. All three have much resemblance to the head of a dog. The legs are short, and end in formidable claws. The legs, belly, and lower part of the throat appear defended by a kind of scale armor, and all the back is protected by a still thicker and double cuirass, starting from behind the ears of the anterior head, and continuing to the tail. The neck is long, and the belly large and almost dragging on the ground. Professor Gilveti, who examined the beast, thinks it is not a monster, but a member of a rare or almost lost species, as the Indians in some parts of Bolivia use small earthen vases of identical shape, and probably copied from nature."

• Possible Giant Snakes

GIANT SNAKES

Anonymous; *Pursuit,* 2:36-37, 1969.

The following most excellent report came to us from Mrs. R. L. Pollard (97) who is resident in Venezuela. It comes from a news-

paper named The Daily Journal---"Venezuela's English-Language Newspaper," which is a most excellent publication, beautifully written and maintaining extraordinarily high journalistic standards. The account is datelined Rio de Janeiro and is an AP report with no less than Silvia Landau's byeline. This too is a model of journalism since, unlike even our best newspapers, all the facts, names, and figures check out; and this after translation from Portugese through Spanish, into English. And we Anglos still sneer at the Latinos! But then, how many of us know that they had universities two centuries before this country declared its independence. The story goes as follows:---

"An international hunt is underway in the wilds of the Amazon for a legendary giant snake which may be the last of its species. Brazilians and foreigners are chasing after the 'cobra grande' as several jungle expeditions try to outrace each other to capture the giant snake, which in captivity could be the main attraction of any zoo. Until two years ago, the snake was believed to be just a part of the Amazon jungle mythology, but then the huge animal was seen by Italian ichthyologist Bruno Falci in the south of the territory of Rondonia. As he did not have the equipment to capture it, he decided not to kill it either. He took some photographs of the animal while it was taking a siesta. The giant snake had apparently just eaten two calves for lunch. The snake is part of the Indian and regional legends about 'boiuna' and 'cobra grande' that would appear here and frighten men and animals. It would shake houses just by rubbing against them, while crawling by. Another story tells of six members of one family being killed by the giant snake.

"Sometime, later on, comparing the pictures taken by Bruno Falci to tracks left by the snake, it was estimated to measure between 100 and 130 feet (30 to 40 meters) and weigh one ton. It is believed to be of the 'securi' or anaconda species. They usually are much smaller, and hunters and scientists believe the giant might be, if not the last, one of the last of a species. In a museum in Belem there is one securi that although 12 meters long (38.4 feet) and weighing hundreds of pounds is less than half the size of the 'cobra grande' now being chased. Falci has returned to Brazil to join the hunt. This time he is equipped to capture the snake and take it to a European zoo. But he is only one among the several expeditions searching for the 'cobra grande'."

The matter of giant snakes stems from three tropical areas---Malaya, the Congo, and the Amazon Basin, and with particular emphasis on the last. The business was covered in considerable detail by Dr. Bernard Heuvelmans (58) in his book On The Track of Unknown Animals which was first published in 1958. Quite a lot has happened since that date and notably some extraordinary photographs taken from low altitude in the Congo by a Belgian military aviator. The Society owns the originals of these, and we had them blown up and submitted to the Eighth Reconnaissance Technical Squadron, U.S.A.F. experts in Massachusetts for analysis. A most interesting fact emerged; namely, that there were two possible interpretations, depending upon, of all things, certain botanical matters. The point at issue was simply identification of

the vegetation so clearly shown in the photographs. Were these little herbs, or forty-foot trees?

Now, there happened also to be a number of termites' nests of a very distinctive type in the photographs. These can be built as high as twenty feet by the insects but this particular species of termite has the uncanny facility of knowing how to orientate the longer axes---they are like four-sided pyramids but with two very wide sloping sides and two very narrow ones, and a flat top ---so that their larger faces receive the greatest amount of sunlight first in the morning and then in the afternoon. (These insects live precisely on the equator.) Of course, all termites' nests have to start small but it is astonishing how minute a percentage in any one area are small. By the same token, most are near maximum height. Judging from the intense cast-shadows from the nests in these photographs, the Air Force technicians estimated them to be between 15 and 20 feet tall and, moving then to the surrounding vegetation, they found that it must have been between forty and fifty feet tall. Trouble was, should this be so, the snake so clearly shown, (even to the sheen on its scales) would have had to have measured about 200 feet in length, with a girth-diameter of some five feet. And, we repeat; two hundred!

This snake looks like one of the burrowing snakes of central Africa and it is clear that its head is in the process of going down into the ground. Some of these burrowing snakes are termite-eaters and have been found only in termites' nests, but they are only a few inches long. If, then, it just so happened that this reptile was caught above ground in a little bare patch of soil with only "baby" termite nests and surrounded by some small herbs that look like trees, everything would fit neatly into the alternative pattern suggest---but for one fact. Photographs of that clarity would have had to have been taken from a tripod standing on the ground; but analysis of the shadows of the termites' nests made it almost, if not absolutely, certain that they had been taken from the air and from just about 500 feet altitude as the flier had stated!

There is then a rather nasty come-back to the assumption that there are 200-foot-long burrowing snakes. Just what do they eat? Despite the profuseness of termites and the ability of snakes to maintain health and growth on an astonishingly small amount of food with long intermediate periods of fast, the notion that they could be insectivorous is most dubious. Could they subsist on burrowing animals of which there are many of fair size even unto the pig-sized Aard-Vark and other purely subterranean dwellers? Almost all size-groups of life have appropriately sized predators to contend with, and the idea of an aard-vark-eating blind-snake is rather delightful. But there is a third alternative that we suggest might have been overlooked.

Might we, in fact, suggest that this snake was photographed from only about fifty feet up, and simply be a large python going back into its hole? In this case, the termites' nests would be only about a quarter built and the vegetation modest bushes. In fact, we have only the flier's word for the altitude from which the thing was photographed.

• Miscellaneous Unidentified Animals

2 EXPLORERS ACCEPT CONGO MONSTER STORY

Anonymous; Baltimore *Sun*, April 11, 1980, p. A3. (From the Chicago *Tribune*).

A shy monster, bigger than an elephant, that spends most of its time in the water but comes out at night to nibble fruit is a real denizen of the African Congo, not a myth, an explorer from the University of Chicago has concluded.

"They are a species unknown to science as a living form," Dr. Roy Mackal, energy coordinator and research associate at the university, says.

Dr. Mackal and James Powell, a Texas colleague, recently returned from a month of searching through the swamps and jungles of the Congo, where they sought out pygmies for accounts of the monster, known as Mokele-Mebembe.

Reports of the monster from European explorers and missionaries date back to the Eighteenth Century. They include recurrent stories told among the pygmies and sightings of footprints as big as frying pans with three claws to the print.

Dr. Mackal and Mr. Powell set out to determine if the monster was mythical or real, and after interviewing more than a dozen pygmies who claimed to have seen the beast, they concluded it is real.

"The descriptions were remarkably similar despite the fact that the witnesses lived long distances from each other," Dr. Mackal said.

From the accounts, Dr. Mackal and Mr. Powell told the Congolese government that the monsters may grow up to 50 feet long, have snakelike necks, heads that can be as long as 10 feet, and long tails. The animals were said to be reddish or gray, and some have rooster-like combs atop their heads and a long tooth or horn protruding from their mouths.

Despite the fearsome,dinosaur-like look of these massive creatures, they apparently are shy and vegetarian, eating the fruit of the Molombo vine that grows near rivers.

. . . .

A LARGE TURTLE

Anonymous; *Scientific American*, 48:292, 1883.

Captain Augustus G. Hall and the crew of the schooner Annie L. Hall vouch for the following: On March 30, while on the Grand Bank, in latitude 40° 10', longitude 33°, they discovered an immense live trunk turtle, which was at first thought to be a

vessel bottom up. The schooner passed within twenty-five feet of the monster, and those on board had ample opportunity to estimate its dimensions by a comparison with the length of the schooner. The turtle was at least 40 feet long, 30 feet wide, and 30 feet from the apex of the back to the bottom of the under shell. The flippers were 20 feet long. It was not deemed advisable to attempt its capture.

WHAT WAS THE NEW ZEALAND SEA MONSTER?

Koster, John; *Oceans,* 10:56-59, 1977.

On April 25, 1977, the Japanese fishing ship Zuiyo Maru, trawling for mackerel off the coast of New Zealand, snagged a rotting corpse at a depth of 900 feet and hauled in the remains of a beast that no one--anywhere--seemed to be able to identify.

"It's not a fish, whale, or any other mammal," said Professor Yoshinori Imaizumi of Japan's National Science Museum, in the Asahi Shimbun newspaper story that broke the news to the world. "It's a reptile, and the sketch looks very like a plesiosaur. This was a precious and important discovery for human beings. It seems to show that these animals are not extinct after all."

For the benefit of those who have never studied paleontology or seen the movie version of "The Land That Time Forgot," a plesiosaur is a marine reptile, a cousin of the dinosaurs which became extinct about 60,000,000 years ago, except in Hollywood and Japanese movies. Plesiosauri were probably fish-eaters--"they had very good teeth," one scientist observes--and were widely distributed over the world's oceans in the Mesozoic era, or age of the dinosaurs. To Japanese scientists who examined the available evidence left in the New Zealand monster's foul-smelling wake, the most likely candidate for identification seemed to be the plesiosaur.

Nonsense! shouted back the American and British scientific communities, and not a few people in Japan, where the New Zealand monster was front-page news for weeks. Rather than face the stinking carcass of a dinosaur apparently deceased not more than thirty days, paleontologists, mammalogists and marine biologists all over the world advanced their own theories--it was a seal, a whale, a basking shark...but no theory, whether prehistoric or mundane, was completely adequate to explain away the 4,000-pound, 32-foot body, which was examined, photographed five times, clipped for tissue samples, and then dumped back into the sea for fear it would contaminate the Zuiyo Maru's catch of fish.

. . . .

Besides taking pictures of the creature, Yano had snipped some tissue samples of fibrous material from one of the fins for analysis. As soon as he returned, tests began on these specimens. About a week after the news of the monster first broke on July

21, the early reports came in.

Using a method known as ion-exchange chromatography to determine the amino-acid structure of the protein in the fibrous strands Yano had given him, Dr. Shigeru Kimura, a biochemist at the Tokyo University school of fisheries, found that for every 1,000 units of amino acids in the monster tissue, 40 were of a type called tyrosine. The amino-acid structure of a blue shark's fin averaged 44 units of tyrosine per 1,000 of amino acids which, Kimura said, represented a remarkable correlation.

"Among fish, it is known that only sharks and rays have the type of protein called elastoidin," Dr. Kimura said, "But as for reptiles, I do not think there is relevant data, even abroad."

He added that the protein could not have come from a mammal's skin or hair. Thus, chemically, the monster may have been either a fish or, possibly, a reptile, but not a mammal.

. . . .

"Even if the tissue contains the same protein as the shark's, it is rash to say that the monster is a shark," said Prof. Tokio Shikama, a paleontologist at Yokohama National University. "The finding is not enough to refute a speculation that the monster is a pleisiosaur."

In the end, everybody's individual preconceptions won out. Those who were prepared to believe in living plesiosauri were convinced or nearly so, while those who refused to believe found nothing to change their minds. For the open-minded skeptics, or for those who were just plain curious, the New Zealand monster remains one of the most tantalizing enigmas of the sea.

IS THE CHINESE DRAGON BASED ON FACT, NOT MYTHOLOGY?

Irwin, J. O'Malley; *Scientific American,* 114:399+, 1916.

During the latter part of a holiday trip in the Yangtze Gorges undertaken by my wife and self in November, 1915, we met Mr. M. Hewlett, British Consul at Ichang, and his wife, and in their company spent a day in the Ichang Gorge, landing at various points to climb the cliffs and explore some of the numerous caves.

While exploring a large cave on the right bank of the river, about one mile above the Customs Station at Ping Shan Pa, we discovered the fossils about to be described. The cave is reputed by the Chinese to extend some 20 miles to a point near Ichang. It is reported that a party of bluejackets from H. M. S. "Snipe" spent three days in the cave some years ago and that they failed to reach the end. Evidence that this party penetrated beyond the point where the discovery was made exists in the name of their ship painted on the cave walls at a point considerably farther in. The Chinese name of the cave is Shen K'an Tzu, which means "The Holy Shrine," and one of the characters forming the word K'an is the Chinese character for "dragon." A large rock is seen

at the entrance, and some eight or ten yards behind this there is a peculiar piece of curved rock bearing some slight resemblance to a portion of a dragon's body; the resemblance is possibly suggestive enough to impress the Chinese mind, but altogether fails to impress the foreigner. After proceeding some hundred yards inside the cave we found ourselves walking on a peculiar ridge in order to avoid the surrounding pools of water. This ridge curved backward and forward across the width of the cave like the curves of a large serpent, the suggestion being so strong that we lowered our lamps in order to examine the ridge more closely. To our astonishment and delight, we found that we were in very truth walking along a perfect fossil of some huge reptile. Further inspection revealed the presence of six or eight of these enormous monsters. Having taken a few small specimens of loose portions of scale for examination in a better light, we left, planning to return the following morning for the purpose of measurement.

On our return the following morning we selected one of the largest fossils lying for a great part of its length isolated from the others--the coils of the remainder being rather entangled. The isolated portion measured 70 feet, so that it is absolutely certain that the length is at least 70 feet, and as far as we could ascertain, this same specimen extended for another 60 or 70 feet. However, 1 admit that error is possible here, owing to the interlacing coils of the reptiles. The depth of the body seen in the foreground of the first illustration is two feet. The head is partially buried in the cave wall and appears to be a large, flat head similar to that of the Morosaurus Comperi. About 12 or 14 from the head two legs are seen partially uncovered, and again two more about 50 feet from the head. The fact that several persons have penetrated this cave in former years beyond the point where the discovery was made seems to indicate the fossils have been but recently uncovered; probably by a heavy discharge of water through the cave. lt seems probable that these reptiles were trapped by some volcanic disturbance and starved to death; the size of the bodies compared to their length would indicate this. A point of peculiar interest is the resemblance to the Chinese dragon of these fossils. 1 believe that it has heretofore been supposed that the Chinese borrowed their idea of the dragon from Western mythology. The discovery has created a great stir among the local Chinese and foreigners, who are daily flocking to view the fossils. 1 am attempting to interest the Chinese authorities in Pekin and also the Chinese Monuments Society in order that the specimens may be preserved from damage.

Chapter 5

FISH

Chapter Contents

ANOMALOUS DISTRIBUTION IN TIME AND SPACE

UNRECOGNIZED SPECIES

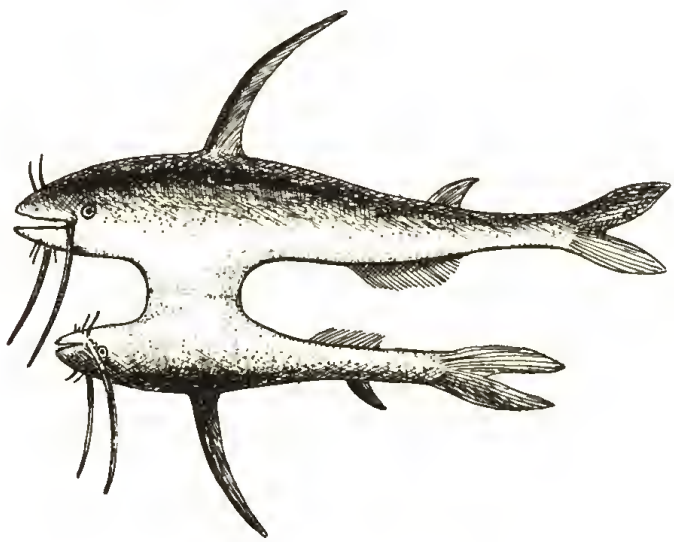
MORPHOLOGICAL PHENOMENA

• Sports

NOTICE OF A DOUBLE FISH

Churchill, Syl.; *American Journal of Science,* 1:26:116-117, 1834.

The annexed drawing represents a pair of cat-fish, (a species of Silurus? L.) which were taken alive in a shrimp net, at the mouth of Cape Fear river, near Fort Johnston, N.C., in August, 1833 and presented to Professor Silliman. One of them is three and a half, the other two and a half inches long, including, the tail--the smallest, emaciated and of sickly appearance. They are connected in the manner of the Siamese twins, by the skin at the breast, which is marked by a dark streak, at the line of union. The texture and color otherwise, of this skin is the same as that of the belly. The mouth, viscera, &c., were entire and



A double catfish

perfect in each fish, but on withdrawing the entrails, through an incision made on one side of the abdomen, the connecting integument was found to be hollow, and nothing resisted a flexible probe in passing through form one to the other. This operation was performed with great care, with the tender and soft end of a spear of grass drawn from a green plant. But there was no appearance of the entrails of one, having come in contact with those of the other, for the integument was less than one tenth of an inch in its whole thickness, and in length from the body or trunk, of one fish to the other, it was three tenths, and in the water, when the largest fish was in its natural position, the small one could, by the length and pliancy of this skin, swim in nearly the same position. It was not ascertained whether, they were of different sexes, or of the same.

When these fish came into existence it is probable they were of almost equal size and strength, but one "born to better fortune," or exercising more ingenuity and industry, than the other, gained a trifling ascendency, which he improved to increase the disparity, and by pushing his extended mouth in advance of the other, seized the choicest and most of the food for himself. Yet though he probably hated the incumbrance of his companion, and wished the "marriage tie cut asunder," he afforded protection to his "weaker half," and could not eat it without swallowing himself.

FISH TURNS CUT THROAT INTO EXTRA MOUTH

Anonymous; *Science News Letter,* 19:105, 1931.

A fish that apparently had its throat cut, or suffered some similarly bad accident, early in life and yet was not discouraged is the remarkable find described by Dr. E.W. Gudger of the American Museum of Natural History. The fish, a yellow perch now preserved in the Erie, Pa., Public Museum, simply turned the wound into a second mouth opening directly under the first one, and "carried on."

This second mouth had the disadvantage of having no closing muscles, so that it remained permanently open. Nevertheless the fish made use of it without any doubt, for it was hooked in this second mouth when it was caught.

Dr. Gudger describes several other similar cases that have been recorded. The earliest of these was in 1810, but the others are all of comparatively recent date. In all cases the second mouth seems to have been established as the result of accident.

THE MYSTERY OF THE TWO-HEADED SALMON

Anonymous; *New Scientist,* 18:478, 1963.

When the fish hatchery experts of the Avon and Dorset River Board found a few salmon parr with two heads in their artificial breeding troughs at Fordingbridge they were interested but not unduly surprised. Salmon are not uncommonly hatched with two heads and sometimes with two tails. The freaks are few and far between and they usually die quickly. But when more and more double-headed salmon began to appear they became alarmed. At the final count, some 20,000 monsters suffering from a defect called ciprosopus tetrotus in human babies had been hatched, River board officials described the harvest of freaks as catastrophic and even experts in abnormalities of birth (teratologists) seemed to think the affair was phenomenal. But, even after an intense inquiry, the officials of the river board are not able to offer an explanation for the mass deformity.

. . . . .

MACKEREL WITH RUBBER BAND

Anonymous; *Science,* 67:sup xiv, April 27, 1928.

A mackerel with an unbroken rubber band running over its back and right through the lower part of its body is the zoological puzzle recently examined and reported by Dr. E.W. Gudger, of the American Museum of Natural History. The fish was purchased on a market stall, so that its history is unknown. Dissection indicates that the band must have been snapped around the fish's body, and that its pressure and friction forced it upward through the tissues of the abdomen. These later healed shut beneath it, leaving it apparently "wove through" the living fish. Other mackerel have been reported with rubber bands placed around their tails by unknown persons, but this is the first case recorded where the band went through the body.

• Mouthless Fish

MOUTHLESS FISHES

Anonymous; *Popular Science Monthly,* 7:638-639, 1875.

Prof. Leidy lately exhibited at the Philadelphia Academy of

Sciences an apparently mouthless fish, found in the Ouachita River, Arkansas. The fish is the buffalo sucker (Catostomus bubalus), an inhabitant of the Mississippi and its tributaries. The specimen is fifteen inches long. The maxillaries, premaxillaries, and mandible, are absent, and the integument is tightly extended between the end of the snout, the suborbitals, and the articular ends of the quadrates. In the centre of this expansion of the skin there is a small oval aperture one-fourth of an inch fore and aft, and one-eighth of an inch in transverse diameter. The hole is sufficient to admit a current of water for the purposes of respiration; but it is difficult to understand how the fish had procured its food. The cyprinoids generally are remarkable for their small, toothless mouth, but it is nevertheless important in its prehensile capacity. The condition of the specimen is of course a deformity, but appears to be the result of a want of development of the jaws, and not of accidental violence. Such fishes are often caught in the Ouachita, and occasionally even they have been reported without a vestige of an oral orifice. If the latter condition really occurs, the fish can only supply itself with food and with water for respiration through the branchial fissures, by the alternating outward and inward movements of the opercula.

A MOUTHLESS CARP

Thomson, L. Arthur; *Knowledge,* 10:152, 1913.

It seems strange that a fish can live and thrive without a mouth. J. W. Fehlmann describes this apparent simplification of life on the part of a carp four years of age. Its mouth was absolutely shut and the same was true of the anus. Yet there were numerous mayfly larvae, crustaceans, pieces of plants and the like in the food-canal. The animal must have not only breathed but fed through its gill-clefts. We are not surprised to learn that the carp showed no trace of fat, but to live for four years without a mouth was certainly an achievement.

• Remarkable Eyes

A FISH WITH TWO PAIRS OF EYES

Anonymous; *New Scientist,* 27:738-739, 1965.

When a new species of fish was hauled out of deep water off the coast of Oregon a few years ago marine biologists noted its curious bulging eyes which seemed to have slipped down into protuberances below the main lenses and gave it the forbidding generic name of Bathylychnops, literally "the deep-lantern-eye". However, when they examined the specimen more closely they found that Bathylychnops had two pairs of eyes on each side of its head. The upper ones appeared to be capable of looking at objects upwards and backwards whilst a pair of very sensitive eyes below them were directed downwards.

As Professor William G. Pearcy, an oceanographer and Dr. Samuel L. Meyer, an ophthalmologist, both of Oregon State University and Medical School describe the four-eyed fish in the current issue of Nature (Vol 207, p 1260). Bathylychnops is equipped with organs for binocular vision supplemented by photoreceptors below them for hunting in deep dark water. It must be a formidable predator. This view is confirmed by their co-author, Mr. Ole Munk of the Institute of Comparative Anatomy in Copenhagen.

. . . . .

The secondary globes of the eyes of Bathylychnops appear to be unique in the world of vertebrates and closely related to the development of stereoscopic vision. This is supported by the fact that similar but structurally far simpler specializations are present in the tubular eyes of other deep-sea fish. But why Bathylychnops should have evolved two sets of contiguous organs, when one bulgy pair, represented by the lower eyes, would have sufficed to detect the luminous flashes of its prey, is at the moment quite inexplicable.

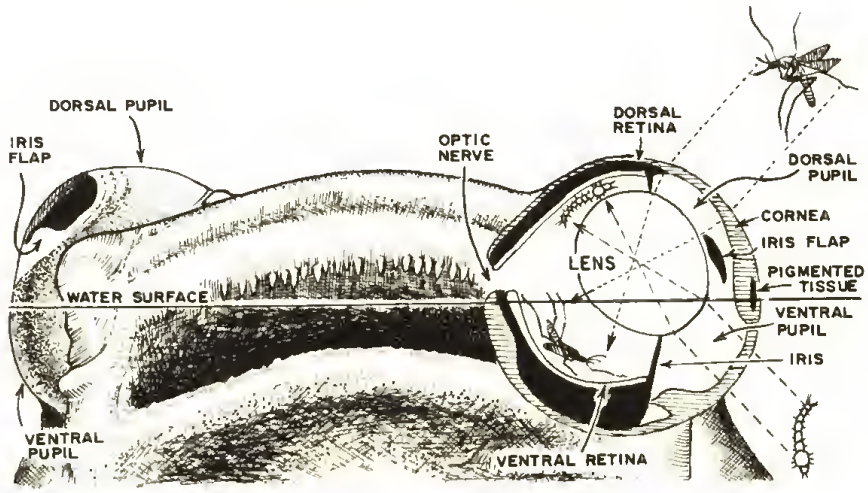
THE FOUR-EYED FISH SEES ALL

Zahl, Paul A.; *National Geographic Magazine,* 153:390-392, 1978.

Built-in bifocals provide the key to Anableps's remarkable dual vision. A speckled band of pigmented tissue and two iris flaps visible just above it divide each eye at the waterline, in effect creating two pupils, one for above water and one for below. Viewed from above, the iris flaps resemble projecting fingers in-

side the bulging eye.

Because the refractive natures of water and the surface of the eye, or cornea, are almost identical, light reflected from underwater objects passes straight through the cornea, to be bent and focused sharply on the retina by the lens, which has a higher refraction index than the cornea so that the light is bent twice---once by the cornea and again by the lens (diagram).



Anableps, using its unique egg-shaped lens, sees both images clearly. The part of the lens aligned with the lower pupil is rounded like a typical fish lens, so that an image of a swimming insect larva will be focused on the retina. The less rounded upper part, more like the human lens, compensates for double refraction when objects in the air are viewed. A mosquito can thus be clearly seen.

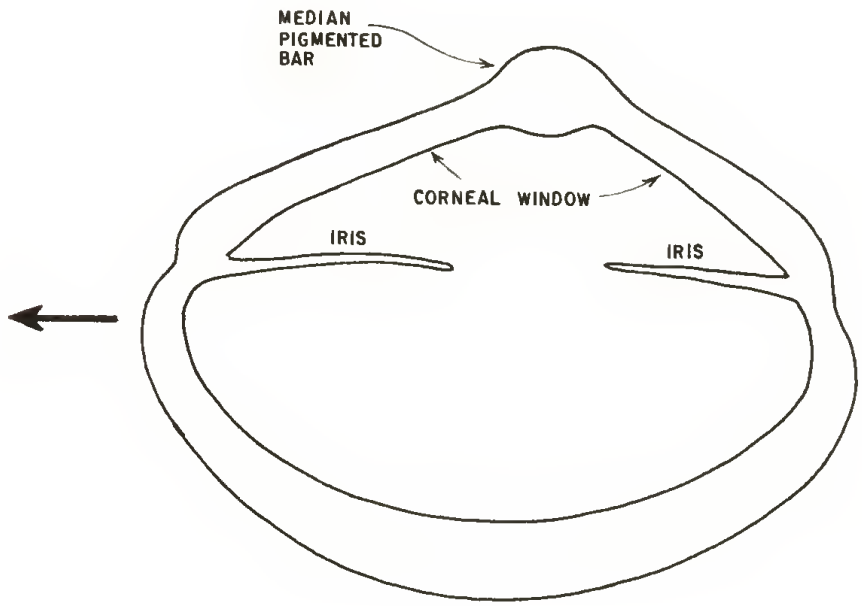
The four-eyed fish will lunge into the air to ambush flying insects or dip beneath the surface to catch swimming creatures. More commonly, however, it cruises the shallow water near a shoreline and captures crustaceans, algae, and insects that are trapped in the surface film.

Scientists have determined that Anableps relies mostly on its aerial vision, which can detect smaller objects at greater distances than the aquatic sight system. But the fish often dives to feed or escape predators. When on the surface, Anableps repeatedly bobs its head to moisten its "upper eyes." (p. 392)

AERIAL VISION: UNIQUE ADAPTATION IN AN INTERTIDAL FISH

Graham, Jeffrey B., and Rosenblatt, Richard H.; *Science,* 168:586-588, 1970.

Abstract. Mnierpes macrophalus, a clinid fish of rocky shores of the eastern tropical Pacific, makes frequent terrestrial sojourns. The normal fish eye is myopic in air because of curvature of the cornea. This is overcome in Mnierpes by the presence of two flattened corneal surfaces.



Section through the eye of Mnierpas macrocephalus

FISH MAY HAVE OPTICAL FILTERS TOO

Anonymous; *New Scientist,* 78:370, 1978.

Retinas, because of their curious evolutionary history, are organised in a thoroughly perverse manner: light that enters the eye has to plough through layers of nerves, junctions, and other bits and pieces of mechanical wizardry that makes the eye such an amazing piece of machinery. In some birds and reptiles an extra barrier that light encounters on its way through the retinal layers before it reaches the photosensitive cells (rods and cones) is made up of microscopic droplets of oil. They are there, not through some anatomical aberration, but because they

act as efficient filters to short wavelength light.

Now, fishes don't have oil droplets in their eyes, but some of them do have curious transparent spheres at the top end of some of their cones. (Cones usually come in three types; red-sensitive, green-sensitive, and blue-sensitive.) It is these spheres that have been named ellipsosomes, for the simple reason that the region of the cones where they sit is called the ellipsoid (Science, vol 200, p 549).

The ellipsoid is usually packed with mitochondria, tiny double-membraned organelles that generate the cell's energy by "burning" metabolic fuel. Although ellipsosomes are very similar to mitochondria in appearance (the authors of the Science paper suggest the former may have evolved from the latter), their principal characteristic is that they contain a home-based pigment that absorbs at relatively low wavelengths. The obvious conclusion is that, like the oil droplets in birds and reptiles, ellipsosomes are optical filters. It is possible, however, that they carry out some of the metabolic functions of mitochondria (they certainly have some of the requisite enzymes).

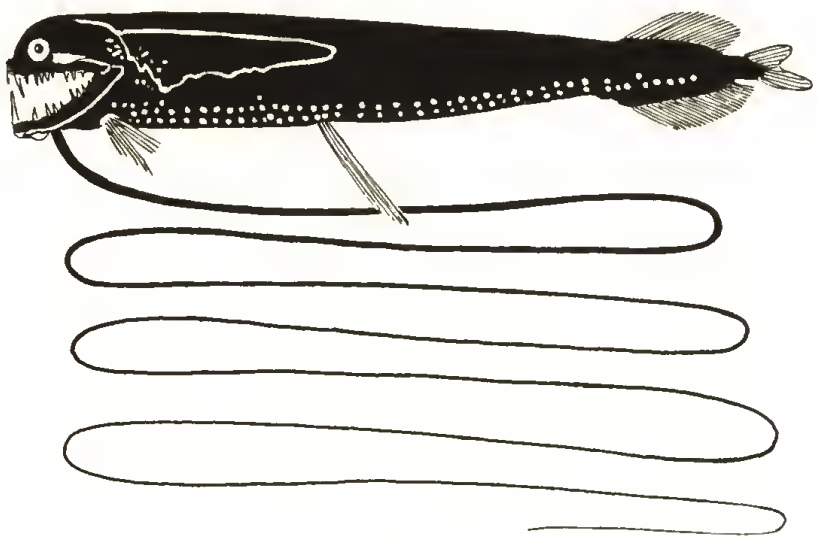
Stalked eyes are found in the young of some deep sea fishes but never in the adults. (Adapted from Sea Frontiers, *6:145, 1960)*

• Fish with Unusual Appendages

LITTLE MONSTERS OF THE DEEP

Smith, F. G. Walton; *Sea Frontiers,* 6:137–145, 1960.

From its lower jaw, Grammatostomias flagel' ibarba trails a barbel that is even longer than its name. In some cases this odd appendage is ten times the length of the fish--one of a group known as melanostomiatids. The white dots along its lower sides are luminous organs that the fish lights at willl. Scientists are not certain about the functions of the long, trailing barbel, but it may be sensory in nature, possibly helping to detect the presence of prey. (Zoologica, 1939)



• Warm-Blooded Fish

THE TUNA: A WARM-BLOODED FISH

Anonymous; *New Scientist,* 33:151, 1967.

The folk-lore of fishermen includes the idea that the bodies of

tuna fish are much warmer than the sea they come from. This rather surprising observation has been confirmed by zoologists at the Woods Hole Oceanographic Institute in Massachusetts---tuna fish can, in fact, maintain a body temperature up to 14°C above that of their environment.

This finding is the more surprising in view of the way in which the blood circulation of a fish operates. The animals' blood passes through the gills, where it encounters oxygenated sea-water. An exchange of gas takes place, and while this is happening the blood attains sea-water temperature; as a result most fish are cold blooded.

• Skeletal Structure That Varies Geographically

LATITUDE AND VERTEBRAE

Jordan, David Starr; *Popular Science Monthly*, 45:346–350, 1894.

In this paper is given an account of a curious biological problem and of the progress which has been made toward its solution. The discussion may have a certain popular interest from the fact that it is a type of many problems in the structure and distribution of animals and plants which seem to be associated with the laws of evolution. In the light of these laws they may be more or less perfectly solved. On any other hypothesis than that of organic evolution the solution of the present problem, for example, would be impossible. On the hypothesis of special creation a solution would be not only impossible but inconceivable.

It has been known for some years that in several groups of fishes (wrasse fishes, flounders, and "rock cod," for example) those species which inhabit northern waters have more vertebrae than those living in the tropics. Certain arctic flounders, for example, have sixty vertebrae; tropical flounders have, on the average, thirty. The significance of this fact is the problem at issue. In science it is assumed that all facts have significance, else they would not exist. It becomes necessary, then, to find out first just what the facts are in this regard.

Going through the various groups of nonmigratory marine fishes we find that such relations are common. In almost every group the number of vertebrae grows smaller as we approach the equator, and grows larger again as we pass into southern latitudes.

It would be tedious to try to prove this here by statistical tables, but the value of generalization in science depends on such evidence. This proof I have elsewhere given in detail. Suffice

it to say that, taking an average netful of fishes of different kinds at different places along the coast, the variation would be evident. At Point Barrow or Cape Farewell or North Cape a seineful of fishes would perhaps average eighty vertebrae apiece, the body lengthened to make room for them; at Sitka or St. Johns or Bergen, perhaps, sixty vertebrae; at San Francisco or New York or St. Malo, thirty-five; at Mazatlan or Pensacola or Naples, twenty-eight; and at Panama or Havana or Sierra Leone, twenty-five. Under the equator the usual number of vertebrae in shore fishes is twenty-four. Outside the tropics this number is the exception. North of Cape Cod it is virtually unknown.

The next question which arises is whether we can find other conditions that may affect these numbers. These readily appear. Fresh-water fishes have in general more vertebrae than salt-water fishes of the same group. Deep-sea fishes have more vertebrae than fishes of shallow waters. Pelagic fishes and free-swimming fishes have more than those which live along the shores, and more than localized or nonmigratory forms. The extinct fishes of earlier geological periods had more vertebrae than the corresponding modern forms which are regarded as their descendants. To each of these generalizations there are occasional partial exceptions, but not such as to invalidate the rule.

All these effects should be referable to the same group of causes. They may, in fact, be combined in one statement. All other fishes have a larger number of vertebrae than the marine shore fishes of the tropics. The cause of the reduction in numbers of vertebrae must therefore be sought in conditions peculiar to the tropical seas. If the retention of the primitive large number is in any case a phase of degeneration, the cause of such degeneration must be sought in the colder seas, in the rivers, and in oceanic abysses. What have these waters in common that the coral reefs, rocky islands, and tide pools of the tropics have not?

In this connection we are to remember that the fewer vertebrae indicates generally the higher rank. When vertebrae are few in number, as a rule each one is larger. Its structure is more complicated, its appendages are larger and more useful, and the fins with which it is connected are better developed. In other words, the tropical fish is more intensely and compactly a fish, with a better fish equipment, and in all ways better fitted for the business of a fish, especially for that of a fish that stays at home.

In my view the reduction in number and increase of importance of the individual vertebrae are simply part of this work of making a better fish. Not a better fish for man's purposes---but a better fish for the purposes of a fish. The competition in the struggle for existence is the essential cause of the change. In the center of competition no species can afford to be handicapped by a weak backbone and redundant vertebrae. Those who are thus weighted can not hold their own. They must change or perish.

The influence of cold, darkness, monotony, and isolation is to limit the struggle for existence, and therefore to prevent its changes, preserving through the conservation of heredity the more remote ancestral conditions, even though they carry with them disadvantages and deficiencies. The conditions most favor-

able to fish life are among the rocks and reefs of the tropical seas. About the coral reefs is the center of fish competition. A coral archipelago is the Paris of fishes. In such regions is the greatest variety of surroundings, and therefore the greatest number of possible adjustments. The struggle is between fish and fish, not between fishes and hard conditions of life. No form is excluded from the competition. Cold, darkness, and foul water do not shut out competitors, nor does any evil influence sap the strength. The heat of the tropics does not make the water hot. It is never sultry nor laden with malaria. The influence of tropical heat on land animals is often to destroy vitality and check self-activity. It is not so in the sea.

From conditions otherwise favorable in arctic regions the majority of competitors are excluded by their inability to bear the cold. River life is life in isolation. To aquatic animals river life has the same limitations that island life has to the animals of the land. The oceanic islands are behind the continents in the process of evolution. In like manner the rivers are ages behind the seas.

Therefore the influences which serve as a whole to intensify fish life, and tend to rid the fish of every character or structure it can not "use in its business," are most effective along the shores of the tropics. One phase of this is the reduction in numbers of vertebrae, or, more accurately, the increase of stress on each individual bone.

Another phase is the process of cephalization, the process by which the head becomes emphasized and the shoulder bones and other structures become connected with it or subordinated to it. Still another is the reduction and change of the swim-bladder and its utter loss of the function of lung or breathing organ which it occupied in the ganoid ancestors of modern fishes.

Conversely, as these changes are still in operation, we should find that in cold waters, deep waters, dark waters, fresh waters, inclosed waters, and in the waters of past geological epochs, the process would be less completed, the numbers of vertebrae would be larger, while the individual vertebrae remain smaller, less complete, and less perfectly ossified.

This, in a general way, is precisely what we do find in examining the skeletons of a large variety of fishes.

. . . . .

• Surprisingly Rapid Speciation

THE MYSTERIOUS ORIGINS OF THE FISH ON THE HILL

Anonymous; *New Scientist*, 54:543, 1972.

Barombi Mbo, a small crater lake in North-west Cameroon, and three other even more diminutive lakes in the same area, were the subjects two years ago of an intensive ecological investigation by Ethelwynn Trewavas of the British Museum and her colleagues J. Green and Sarah Corbet of Westfield College. The results of their labours have begun to appear in a series of papers in the Journal of Zoology. The second and most recent of these papers, a detailed account of the fish of Barombi Mbo (vol 167, p 4), provides a nice example of the origin of species--and also poses an intriguing puzzle in evolution.

The lake is 2·5 km across, over a hundred metres deep and roughly conicular in shape. Its fish population supports the 200 members of the Barombi tribe, whose village is nearby. Of the 17 species of fish found in the lake, 11 are cichlids (mouth breeders, carrying the fertilised eggs in the mouth or pharynx), and every one of these 11 species is unique to the lake. The London-based team, with the cautious cooperation of the Barombi fishermen, caught enough specimens to describe fully the anatomy and appearance of the fish, and also to determine their likely diet through analysis of stomach contents.

In an attempt to reconstruct the origin of these 11 species, Trewavas and her colleagues speculate that the lake--situated in its crater on the top of a high volcanic hill--was originally colonised by between two and four separate cichlid species. Finding themselves in an environment which--while probably different from their original home--offered plenty of food and space, the species would have increased their populations rapidly. Studies of man-made lakes have shown that under these golden circumstances, the living and feeding habits of a species can alter rapidly; but what made between two and four species become 11?

The classical mechanism by which speciation is initiated is the isolation of communities--often by simple geographical barriers. The in-breeding colonies that result then evolve their separate ways. But Barombi Mbo provides no barriers now, nor can it have done in the past. Clearly, the species present today are markedly different in appearance, eating habits and preferred habitat. They have achieved an "impressive ecological separation", allowing the dense overall fish population to exploit the fairly meagre food resources of the lake to the full. But how and why communities of the original colonising species started living and breeding apart remains a mystery.

ZOOGEOGRAPHY AND EVOLUTION

Briggs, John C.; *Evolution*, 20:282–289, 1966.

Those who have concentrated on the history of living populations have found that oftentimes speciation can proceed wth almost unbelievable rapidity. For example, in Lake Lanao on Mindanao Island in the Philippines, a body of water with an estimated age of 10,000 years, no less than 18 species and four genera of fishes have evolved from a single ancestral species. Lake Waccamaw in North Carolina is almost certainly of late Pleistocene origin, yet it contains four distinct endemic fish species. The postpluvial disruption of streams, lakes, and springs in the Great Basin area took place about 10,000-12,000 years ago and this time has been sufficient to allow the formation of some full species in the fishes. In Lake Nabugabo, Uganda, five endemic species of cichlid fishes evolved in about 4,000 years. Bananas were introduced into the Hawiian Islands about 1,000 years ago and, since that time, five species of Hedylepta moths apparently developed on this food source. In the Faero Islands, the house mouse (Mus musculus) introduced as recently as 300 years ago, has become so different that some authors consider it to be a full species.

Why are there such vast differences in the time that it takes for a species to develop? It has been suggested that terrestrial groups are liable to evolve faster than marine groups since the latter live under more equable conditions and are less effected by climatic fluctuations. However, it can be shown that, in some situations, evolution among marine animals has taken place very rapidly. Cocos Island in the eastern tropical Pacific is apparently of Pleistocene origin, yet there are now at least six endemic shore fishes, five endemic mollusks, and five endemic crustaceans. Bouvet Island in the Sub-Antarctic area was also probably formed in the Pleistocene, but now has endemic species belonging to three different marine groups. (pp. 285-286)

• Parthenogenesis and All-Female Populations

ALL-FEMALE STRAINS OF THE TELEOST FISHES OF THE GENUS POCCILIOPSIS

Miller, Robert Rush, and Schultz, R. Jack; *Science*, 130:1656–1657, 1959.

Self-perpetuating populations of unisexual vertebrates have been experimentally demonstrated only among the viviparous fishes

of the New World family Poeciliidae--of which the guppy (Lebistes reticulatus) is perhaps the most widely known representative. Nearly 30 years ago Hubbs and Hubbs announced the discovery of a remarkable fish which has only one sex. This is the so-called Amazon molly (Mollienesia formosa), which inhabits coastal and inland waters of northeastern Mexico and adjacent Texas. These females are able to maintain themselves as natural populations by utilizing the males of M. sphenops in certain parts of their range and the males of M. latipinna in others. Fourteen years of experimental work has demonstrated that this species, evidently of hybrid origin, produces daughters only, and that these are invariably like their mothers despite the fact that the mothers were mated with the males of many related species, including even the guppy. Development is clearly induced by sperm, though no evidence of paternal inheritence has been found in resulting generations. These authors subsequently concluded that "the most plausible explanation for the genetic behavior of Mollienesia formosa is that this unisexual species is a permanently fixed diploid," and that active sperm is essential to initiate embryonic development (gynogenesis).

. . . .

At this time we wish to announce the discovery of unisexual types in two other species belonging to the same family as M. formosa. These, members of the genus Poeciliopsis, inhabit coastal streams of northwestern Mexico. Unlike the Amazon molly, these two allopatric species each have two kinds of females--those which produce both sexes and those which, although mated to the same male as the former, produce daughters only. Four generations of these fishes have been reared in the laboratory, with consistent production of bisexual young from the "normal" females and of unisexual offspring from the "aberrant" females.

A FISH THAT'S NEITHER ONE THING NOR THE OTHER

Anonymous; *New Scientist,* 42:681, 1969.

A fish called Poecilia formosa, which lives in streams and coastal lagoons in southern Texas and north-east Mexico, has several peculiar features. To begin with, only females are known to exist. Perhaps not too surprisingly, they can reproduce without fertilization. But what makes them really odd is that where their geographical range overlaps with that of two other species of the same genus, P. latipinna and P. mexicana, the females P. formosa will forsake their enforced chastity and mate with the males of these species. Moreover, although P. latipinna and P. mexicana are structurally distinct, P. formosa is intermediate between them in several features, including its body proportions, colouring, size, and the number of rays in the fins.

All these peculiarities have suggested that P. formosa origi-

nated as a species from a fertile hybrid formed between the two species with which it now consorts. Until now, this has been merely a plausible hypothesis, but the case seems to have been clinched as a result of serological evidence fathered by P. Abramoff, R.M. Darnell and J.S. Balsano of Marquette University, Milwaukee (American Naturalist, vol. 102, p.555).

Abramoff and his colleagues have bred a laboratory hybrid of P. latipinna and P. mexicana and compared its blood plasma proteins with those of P. formosa by electrophoresis. The hybrid possesses two albumin bands; so does P. formosa, in identical positions. Moreover, P. latipinna and P. mexicana possess only one albumin band, each band corresponding to one of the two bands in the hybrid or P. formosa.

Poecilia formosa is therefore serologically as well as morphologically indistinguishable from hybrids between P. latipinna and P. mexicana. It is almost certainly the result of hybridization. What makes the work of much broader importance is that this method of speciation is often postulated, but has never previously been unequivocably demonstrated to occur within vertebrates.

• Flatfish Metamorphosis

MIRACLES OF FLATFISH METAMORPHOSIS

Dewar, Douglas; *The Transformist Illusion,* DeHoff Publications, Murfreesboro, 1957, pp. 212-213.

The flat-fishes, i.e. soles, plaice, flounders and turbot, etc., undergo a limited metamorphosis, quite unlike that of Sacculina or of the Sea-squirt. The fish when adult have both eyes on the same side of the head, and swim about and rest with one side of the body facing upwards and the other downwards. More than 600 species of these flat fishes have been described. Some consider them to be an order composed of five families.

When it emerges from the egg, the flatfish has a symmetrical body shaped like that of an ordinary fish, with an eye on each side of the head. As it grows the body becomes laterally compressed and the cartilage of the supraorbital bar above one eye (the left in some species, the right in others) becomes absorbed, leaving a gap through which the eye below will pass during its migration round the top of the head to the opposite side. The fish now begins to tilt over to the side from which the eye is moving. The migration of the eye is a matter of a few days. When the migrating eye reaches the supraorbital cartilage on the opposite side of the head, the other eye begins to move, and

the two eyes travel together until they reach the position they occupy in the adult. Their combined movement causes the intervening supraorbital bar to become distorted. Then ossification sets in.

In this connection it may be noticed that the turning over of the flatfish to one side does not seem to be a necessary consequence of the flattening of the body because some fish equally flat, such as the John Dory and the fresh-water fish, Pterophyllum scalare, remain vertical. Moreover, the Bream, which does not live on its side, is more flat than the Halibut, which is the least flattened of the flatfish.

The evolutionist apparently believes that some ancestor, or ancestors, of the flat-fish became rather flat and then, for some unknown reason, the supraorbital bar on one side conveniently became absorbed, and the eye below began to shift its position, and this shifting became greater in successive generations, until eventually both eyes were situated on the same side; that is for thousands of years these fish swam about in a position intermediate between the vertical and the horizontal.

The fossils afford no support to this theory, the earliest known turbot, which occurs in the Eocene, and the earliest sole, found in the Upper Miocene, are as flat as are any living flat-fish. All living fish assume in the water a perfectly vertical or a perfectly horizontal position; none of them swim with a list, as evolutionists suppose many kinds of fish to have done in the past. (pp.212-213)

EYE MIGRATION IN FLAT-FISHES AND LAMARCKIANISM

Hawkes, Arthur J.; *Nature*, 76:79, 1907.

Mr. R.H. Lock, in his recent book on "Variation, Heredity, and Evolution" reviewed in Nature of April 27), has, in common with many other writers, adduced the phenomenon of eye transposition in flat-fishes as a cogent argument in favour of the transmission of acquired characters, remarking that an alternative hypothesis is lacking." I venture to dispute this position, believing it to be decided evidence in support of the potency of natural selection to accumulate small mutations. I quote a part of the passage (p. 35) that my argument may be the better understood:--

"in the adult condition these fishes lie flat on one side; and during their development from the young condition that eye which, if it remained in its original position, it comes to lie quite upon the upper surface. . . . The very young fish whilst still symmetrical, are known sometimes to fall upon one side, and when in this position to twist the lower eye forcibly upwards. Darwin himself there supposed that the origin of the adult structure is to be attributed to the inherited effect of efforts of this kind."

This misinterpretation of the phenomenon seems to me to arise from an inadequate appreciation of the nature of the actual varia-

tion, i.e. the capacity to twist the eye, which is exhibited by the young fish. The young of some other fish are known to exhibit the same muscular control over the orbit ("Origin of Species," p.292), and we need only to suppose that the forerunner of the modern race of flat-fishes possessed it as a fortuitous variation to the extent of making vision just possible whilst in the recumbent position; and this would seem to be the case, for it is recorded in the "Origin of Species" that a young fish has been observed to "raise and depress the eye through an angular distance of about seventy degrees." In the transmission of the original variation to the offspring it is not the effect of the movement, which is passed on, but the structural arrangements which enabled it to initiate the movement, the amplitude being increased in successive generations by the aid of natural selection.

This contention may be supported by citing a peculiar muscular capacity possessed by myself. I am able to raise and depress the right eyebrow independently of the left, but I have no such control over the other. To test whether this power may not be induced by practice, I have striven to raise the left whilst holding down the right, but find myself quite unable to accomplish it. Herein we see that the capacity to make the movement is of itself a distinct mutation; and assuming that in the case of the flat-fish mobility of the optic aperture was so far possible as to be of advantage to it, natural selection would operate in preserving those of the progeny which were able to retain the eye in the advantageous position with the least possible effort.

I have ventured to tender this explanation to the readers of Nature because the phenomenon is very generally used as a good illustration of Lamarck's doctrine, and as being "inexplicable on the theory of natural selection."

REVERSAL IN THE WINTER FLOUNDER .....

Gudger, E. W.; *Science*, 102:672-673, 1945.

In 1935, a paper on "Reversal of Sides in Flat-fishes," I brought together all the accounts that I could find of reversal in flatfishes, and tabulated the specific data (dates, sizes, figures, etc.). After a careful search I found a solitary record of a reversed Pseudopleuronectes americanus.

No. I. In "Biological Notes" from Woods Hole, Mass., is this record from Vinal N. Edwards. "Pseudopleuronectes americanus: A male in spawning condition, 14 inches long, taken in a fyke net in Waquoit Bay, February 23, 1900, has eyes on the left side--the first of the kind I have taken." To those who know the meticulous care with which for over 30 years Vinal Edwards made his records, nothing more need be said. What became of this first recorded reversed winter flounder is not known, but it remained a unique record for over forty years.

No. II. The history of the second known reversed winter

flounder is as follows: Late in October, 1943, Charles Sciarini, of the Borough of Queens, New York City, hooked this fish in Shinnecock Bay on the ocean side of Long Island. He recognized it as an "oddity," took it home and with good judgement put it in the refrigerator. In December, his father, Louis D. Sciarini, in some way, learned that I was interested in abnormal fishes, and called me on the telephone to say that he had a "left-handed" flounder that ought to be right-handed, and that he wanted to know about it. I explained and said that I wanted this fish badly. He then said that he would keep it frozen until he could send it in to me.

Some time in January, 1944, Charles Sciarini brought to me this fish, at that time the only extant specimen of a reversed Pseudopleuronectes americanus in any collection. And with it, for comparison, he brought a small normal specimen. But for the good judgment of father and son in keeping this fish frozen, this precious specimen would have been lost to science. After being in alcohol for about 21 months, it measures 6.4 inches in standard and 9 inches in total length, and 3.3 inches in depth (body only). Its weight is 4 ounces.

Specimen No. III. Our second fish was caught on June 10, 1945, by Fred Sterzenback, of Ridgewood, Brooklyn, N.Y., out of Freeport on the ocean side of Long Island. The fish was taken on a hook baited with a clam, and behaved like the other and normal flounders. After nearly 5 months in alcohol, it measures 9.4 inches in standard length and 11.4 over all, and is 4.5 inches deep (body only). Its weight is 9.5 ounces. Except for being reversed it, like the other reversed fish, is entirely normal in all other respects.

Our specimens are young fish. In waters contiguous to New York, the winter flounder is known to reach a length of 20 inches and a weight of 5 pounds, but the average is about 15 inches and 1.5 pounds for a good-sized fish.

In a postscript to my article, "Reversal in Flat-fishes" (1935), I noted that, while that article was in press, two reversed ambicolorate halibuts, and two reversed flounders (summer and winter) had been received at the Museum, and that all these would presently be described. This was done for the two halibuts and for the summer flounder, but not for the winter flounder.

It being at that time the second known reversed fish of its species, and the easiest to describe, naturally should have been the first worked up. However, this was not done. And now this fish can not be found in the tank in which all abnormal fishes are kept, nor are any notes available.

How rare reversed specimens of Pseudopleuronectes americanus are may be gathered from the following statement from Dr. Daniel Merriman, Director, The Bingham Oceanographic Laboratory, New Haven, Conn.:

> During the detailed analysis of the Connecticut trawl fishery carried out by the staff of the Bingham Oceanographic Laboratory at Yale, in the last two years over 11,000 winter flounders have been examined for at least weight, length and

sex, and frequently scales and otoliths have also been taken on these specimens. Approximately 8,000 more have had lengths and weights taken on them, and approximately 3,200 more have been tagged and measured. So far as we are aware, none of those fish have been reversed, and I think that it can be stated with reasonable assurance that had any of them been reversed we could hardly have failed to notice it. With the exception of the tagged individuals these fish were all examined in the laboratory where they came under the scrutiny of at least several members of the staff. Apart from all the above mentioned fish, we have handled countless thousands aboard the commercial fishing vessels, and have not noticed any reversed individuals. Of course the handling of fish in the field means that they were not subject to such close scrutiny as those that passed through our hands in the laboratory, but it seems to me that the conclusion that the incidence of reversed winter flounders is extremely low is inescapable.

With reference to the areas from which theses winter flounders came, Dr. Merriman writes that:

> The vast majority of our catches came from the Block Island Sound area, a relatively small percentage came from Long Island Sound proper, and a still smaller lot from more distant localities such as the south side of Long Island and the Southern Rhode Island and Massachusetts coast.

The winter flounder is one of the flatfishes with the fewest known cases of reversal. On the other hand, the starry flounder, Platichtys stellatus, a right-sided cold-water species, of the north Pacific, has the greatest number of reversed individuals for any member of the order Heterosomata. Hubbs found the percentage of rights and lefts of this fish in California waters to be 50-50. In Alaskan waters 75 per cent were lefts, while in Japan the fish was 100 percent. left-handed. Why some flounders are reversed, and why there should be this great diversity of reversal in the starry flounder has not been explained so far as I know.

• Unusual Symbiosis: Flashlight Fishes

A POCKETFUL OF GLOW

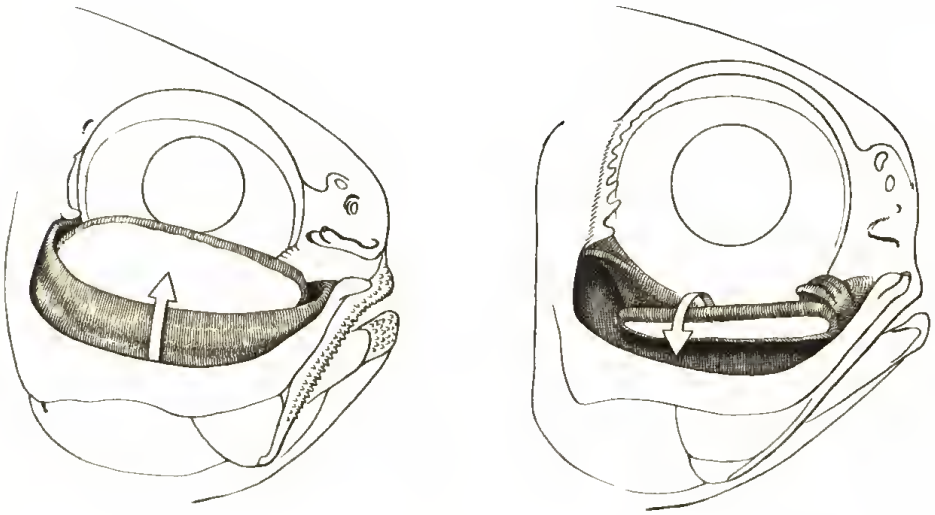
Miller, Julie Ann; *Science News*, 113:106-107, 1978.

A thrifty miner might consider saving bulbs and batteries by mounting a jar of fireflies on his helmet. Perhaps if history had gone differently, and all people lived in dark underground passages, we would have struck up an evolutionary partnership with such glowing organisms. Bacteria in our intestines help digest our food, so why not packets of luminescent organisms riding on our foreheads?

Such a symbiotic situation is common among ocean fish. Much light in the depths originates from bacteria specially nurtured in fish structures called light organs. For example, the "flashlight fish" swims above reefs in the Indian Ocean and Red Sea beaming light like an automobile on a country road.

. . . . .

The design of light organs is complex, and important aspects are not yet understood. "With canals and blood vessels and reflectors and shutters, they are as highly evolved as you can imagine," says Kenneth H. Nealson of the Scripps Institution of Oceanography in La Jolla. "Bacteria that we can't keep alive for a week in the lab, can live 20 years in the fish." Light organs are surprisingly specific as to what bacteria they will host. So far only three of the six known species of luminescent bacteria have been found in the light organs of more than 100 specimens of fish, representing 33 species. Although a sample of seawater might contain all six of these bacterial species, and seawater is continuous with the interior of the organs, each organ (and all light organs of one species of fish) contain only one species of bacteria. "Insofar as is known, there is absolute specificity with regard to the bacterial species cultured in the light organs of a given species of fish. Also, closely related species of fish, even when living or maintained in very different locations, appear to carry the same bacterial species," Nealson and J. Woodland Hastings of Harvard University say in a recent review (Annual Reviews of Microbiology 1977, 31:549-595.



Two types of flashlight fish switches: (left) Eyelid-like mechanisms roll up to obscure light; (right) light organ itself is rotated down into a pouch. (Adapted from Scientific American, *236:112, March 1977)*

FLASHLIGHT FISHES

McCosker, John E.; *Scientific American,* 236:106–114, March 1977.

Flashlight fishes are also unusual among bioluminescent organisms in that their light is on more than it is off. To turn off its light Photoblepharon simultaneously raises a black curtain over each light organ, completely blocking out the light. The rate of blinking varies with the water temperature and the conditions of the fish's environment. When live brine shrimp (a food for Photoblepharon) are added to an aquarium tank containing several of the fish, a rapid blinking sequence ensues, suggesting that the fish are somehow communicating the information to one another. (p. 110)

• Mimicry in Fish

FISHES THAT LOOK LIKE PLANTS

Atz, James W.; *Animal Life,* 54:130–136, 1950.

The fantastic shapes and forms assumed by some of these fishes are incredibly complicated and seem to represent the end-result of a process that operates down to the finest detail. Take the Sargassum Fish, Histrio gibba, for instance. Its whole body is irregularly decorated with tabs of flesh, and its fins, especially the first few rays of the dorsal one, also serve to break up the outline of its body and make it blend into the sargassum weed that is its home. Add to its weird form the ability to alter its mottled color pattern in order to match the general tone of the particular bunch of weed in which it finds itself, and you have camouflage par excellence.

But the Leafy Sea-dragon, Phycodurus eques, excels even the Sargassum Fish in the extravagance of its plant-like appendages. This relative of the seahorses and pipefishes is known from only a few specimens taken off southern Australia. It is believed to inhabit moderate depths of the sea, presumably among seaweed --- or why else should it be decked out so bizarrely? The fish boasts more than a dozen appendages, each with a full complement of

The plant-like Leafy Sea-Dragon from Australian waters

"leaves" --- enough foliage to be a complete plant in itself! Several less spectacular pipefishes also resemble plants, frequently eel grass, by holding their slender bodies upright among the tape-like leaves, swaying with water currents just as they do. Those species which have prehensile tails attach themselves to a stalk, heightening the illusion that they are growing out of the plant.

An entirely different fish achieves the same effect by fastening itself down by means of its mouth. Dr. William Beebe was the first to describe this peculiar behavior. While driving among the coral jungles of Haitian reefs, he noticed a young Orange Filefish, Alutera schoepfi, swim into a small clump of eel grass. "He took hold of a bit of coral with his sucker mouth and immediately set both vertical fins in gentle, undulatory motion, the other fins, especially the long caudal, being furled, so that the general body shape was tapering, which, together with the mottled green color, transformed it into a seaweed frond or eel-grass blade. Now and then the fish revolved on its base without letting go. The trigger spine, slightly elevated, conveyed the impression of a bit of shredded tissue. An additional aid in the deception was the considerable variation of color in these fish, shifting from plain dark cedar green to a mottled greyish or greenish white."
(pp. 135-136)

FISH MIMICS WITH DEADLY PURPOSE

Anonymous; *New Scientist,* 77:89, 1978.

Ivan Sazima, of the University of Campinus, Brazil, has recently uncovered a fascinating example of an unusual form of mimicry in the neotropical fish Probolodus heterostomus--it has developed aggressive mimicry.

Mimicry in the animal world is a well-known phenomenon. In Batesian mimicry a prey species evolves a body form or another characteristic which allows it to "pass" for a different species which is not eaten by the predator. Alternatively, it may look like a leaf or a twig. The monarch butterfly, for example, is rarely eaten by birds presumably because it tastes noxious. Another species of butterfly, the viceroy, is quite innocuous to the avian palate, but, because it looks like the noxious monarch, birds leave it alone. In Mullerian mimicry two or more species with a protective adaptation look alike. In this way a predator need only learn to avoid one warning pattern. One of the most dramatic defensive mimics, one type of South American butterfly, has evolved 12 different varieties in different regions corresponding to the 12 different varieties of its double.

The piscine aggressive mimic, Probolodus heterostomus, is a small scale-eating fish which lives in the coastal rivers of Brazil. Taxonomists have disagreed over its proper classification because of its remarkable resemblance to another genus of fish found in the same area. When Sazima set out to study Probolodus, he

noticed that they nearly always travelled near or in a shoal of their near double, the Astyanax fasciatus. Moreover, their stomachs were often crammed with the scales of Astyanax.

In order to see just what was happening, Sazima caught several of the mimics and put them in aquaria with Astyanax. As he suspected they immediately began to shoal with the Astyanax. Each Probolodus followed a single Astyanax, manoeuvring obliquely to the rear and quickly striking at its flanks. The mimic secured a few scales from the unsuspecting victim with each strike, quickly swallowed them, and resumed shoaling.

. . . . .

THE COMPLEAT ANGLER: AGGRESSIVE MIMICRY IN AN ANTENNARIID ANGLERFISH

Pietsch, Theodore W., and Grobecker, David B.; *Science*, 201: 369-370, 1978.

Abstract. A case of aggressive mimicry is described in which an anglerfish of the genus Antennarius (order Lophiiformes) utilizes a lure that mimics a small fish. The lure provides not only a highly attractive visual cue but presumably also a low-frequency pressure stimulus for potential prey with a minimum of energy expenditure.

FEMALE MIMICRY IN MALE BLUEGILL SUNFISH—A GENETIC POLYMORPHISM?

Dominey, Wallace J.; *Nature*, 284:546-548, 1980.

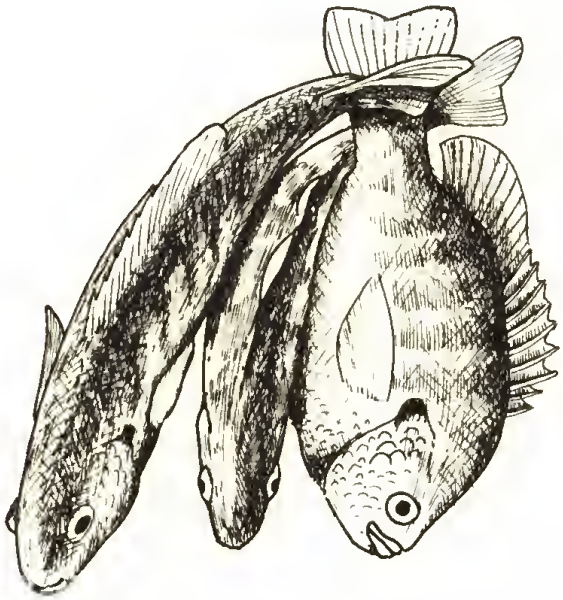
The behavioural observations reported here were made while snorkelling or scuba diving in Lake Cazenovia, New York, during the summers of 1976, 1978 and 1979, as part of a long-term study of the mating system of Lepomis macrochirus. Following all behavioural observations, individuals were captured with a hand net and stripped of their gonadal products to verify their sex. Because of the difficulty of identifying female mimics in the field, female mimicry in fishes may be much more common than the few reported instances indicate.

As is typical of the species, male Lepomis macrochirus in Lake Cazenovia occupy nesting territories in densely packed colonies which may contain several hundred nests. Males compete aggressively for position within these colonies, and the ability of a male to maintain residency on a nest is related to its size (unpublished

data). This is consistent with aquarium studies of Lepomis macrochirus and other sunfishes where body size has been found to be an important determinant of success in agonistic encounters. Males which mimicked female behaviour in 1978 ($\bar{x}$=126mm, s.d.=9mm,n=20) were much smaller than nesting males ($\bar{x}$=190mm, s.d.=13mm, n=45), and were never seen to compete aggressively with them. Although the size distribution of female mimics was disjunct from that of nesting males, it did overlap with the lower end of the distribution for spawning females ($\bar{x}$=156mm,s.d.=19mm, n=45).

An actively spawning Lepomis macrochirus colony is easily recognised because hundreds of females and males without nests typically aggregate in the water column. To initiate courtship a female leaves this aggregation and swims towards a nesting male while displaying a coloration pattern (dark background, dark eyes, and pronounced vertical barring) known to reduce aggression in the nesting males of another sunfish, Lepomis gibbosus. Once the female is in the nest, the male begins to swim in tight circles, turning, with the female following. Every few seconds as the pair turns, the female lies on her side, presses her genital pore against that of the male, quivers, and releases eggs that the male simultaneously fertilises.

The behaviour of female mimics is essentially the same as that of functional females. Female mimics do not attempt to construct or defend nest sites, are found in the predominantly female ag-



Bluegill nesting male (left), female depositing eggs (right), female mimic spawning (in between)

gregations, and solicit nesting males for courtship. These small males approach the aggressive, territorial males without hesitating or retreating, adopt the dark, barred coloration, and turn in the nest with the resident males. Surprisingly, in some colonies the majority of 'spawning' pairs were nesting male-female mimic pairs. Although this type of 'homosexual' behaviour would not seem to be immediately adaptive, the adaptive significance of female mimicry becomes evident when female mimics spawn as part of a trio (Fig. 1) or larger group which includes a functional female. During these spawning bouts, any eggs fertilised by female mimics are abandoned to the care of the nesting males.

. . . .

UNUSUAL PHYSICAL ABILITIES

• Homing Instinct in Fish

HOMING INSTINCT IN SALMON

Scheer, Bradley T.; *Quarterly Review of Biology,* 14:408-430, 1939.

Summary

1. Specimens of S. salar, S. gairdneru, O. nerka, O. tschawytscha, O. gorbuscia, and O. kisutch reared, marked, and liberated in a particular river have been shown to return, in the great majority of cases, to that same river.

2. There are very few cases in which it has been shown convincingly that individuals of any of these species have entered streams 100 or more miles from the one to which they were reared.

3. There is some evidence that fishes reared in small rivers will stray in small numbers to similar streams not more than 50 miles away.

4. When eggs spawned in a particular tributary are hatched and the young fishes reared in that same tributary, most of the adult fishes will return to the tributary (O. nerka, Fraser River; O. tschawytscha, Columbia River).

5. Demonstrable differences in life history, in certain anatomical features, or in both exist between salmon of the same species breeding in adjacent rivers (O. nerka, O. tschawytscha, O. ki-

sutch, Pacific coast of North America; S. salar, Scotland, Sweden).

6. Similar differences exist between the salmon inhabiting different tributaries of the same large river system S. salar, New Brunswick; O. nerka, British Columbia; O. tschawytscha, Columbia River).

7. Except for those cases in which the principal fishery depends upon the riverward migration of fishes, and centers around the mouth of the river, the majority of recoveries of marked fishes at sea have been at considerable distances from the river in which they were marked (S. salar, Sweden, Scotland, Canada; O. tschawytscha, California).

8. Practically all of the fishes recovered in the sea at considerable distances from their natal streams could have returned to them in time to enter with the normal run, granted the ability to find the stream.

9. Fishes tagged in the sea in certain areas will subsequently disperse to a variety of streams, some at considerable distances (O. nerka, O. keta, O. gorbuscha, Japan; O. nerka, Alaska; O. tschawytscha, British Columbia, California; S. salar, Newfoundland, Scotland, and Norway).

10. Young specimens of O. tschawytscha marked in the Columbia River may go north as far as southeastern Alaska; adults tagged in southeastern Alaska or farther south may go to the Columbia River.

11. Specimens of S. salar tagged in the sea off Scotland and of O. nerka tagged in Alaska have later appeared in streams remote from the region of tagging; scales from these fishes gave evidence of life histories otherwise known to be characteristic of the streams they later entered.

12. The hypothesis most compatible with these facts is the following: Anadromous salmon and trout hatched and reared in a particular region will, when seeking fresh water to spawn, seek out and return in the great majority of cases to the same region, even from considerable distances.

13. No mechanism is known by means of which fishes could so return.

• Electrical Navigation

ELECTRIC RECEPTORS IN A NON-ELECTRIC FISH (CLARIAS)

Lissmann, H. W., and Machin, K. E.; *Nature*, 199:88–89, 1963.

It is now well established that certain fish can detect objects electrically. These fish have weak electric organs, and set up an

electric field in the surrounding water. A nearby object distorts this field, and the distortions are detected by the fish. The electric receptors are sense organs communicating with the water by means of jelly-filled canals.

In an early examination of this 'electric sense' we suggested that the incoming electrical signals were 'smoothed' at the electric receptors before being coded into impulses in the afferent nerves. This hypothesis was tested in a series of experiments in which a Gymnarchus niloticus was trained to respond to an applied electric field. This hypothesis was fully confirmed; the smoothing time-constant was found to be about 25 msec. It was further found that the fish was unexpectedly sensitive to trains of pulses of low-repetition frequency; this suggested that adaptation of the receptors was taking place with a time constant of some tens of milliseconds. Each successive pulse of the train would then give rise to a separate sensory signal. If the time taken in the central nervous system to process the sensory data and release the response (the 'perception time') were sufficiently long, several of these separate signals might be centrally summated to form a larger stimulus.

All the species of the two families of fish examined so far (Mormyridae, Gymnotidae) possess electric organs and specialized electric receptors derived from the lateralis system. On evolutionary grounds there is reason to suppose that some non-electric fish may have electric receptors, and that knowledge of their properties may help towards an understanding of the mechanism of electric receptors in general

. . . . .

Gymnarchus niloticus, an electric fish possessing an exceedingly efficient object-locating mechanism, will respond to an electric field as low at 0·15 μV/cm. The sensitivity of Clarias, in which no continuous electric discharge has been found, is only a factor of five worse than this. What purpose, if any, does this remarkable sensitivity serve in the normal life of the fish? Perhaps, as has been suggested, the fish can detect action potentials from predators or prey. Alternatively it may locate nearby moving fish by detecting electric currents set up by the movement of water in the Earth's magnetic field. It is, in any event, almost inconceivable that this great electrical sensitivity is fortuitous, and not used by the fish in any way.

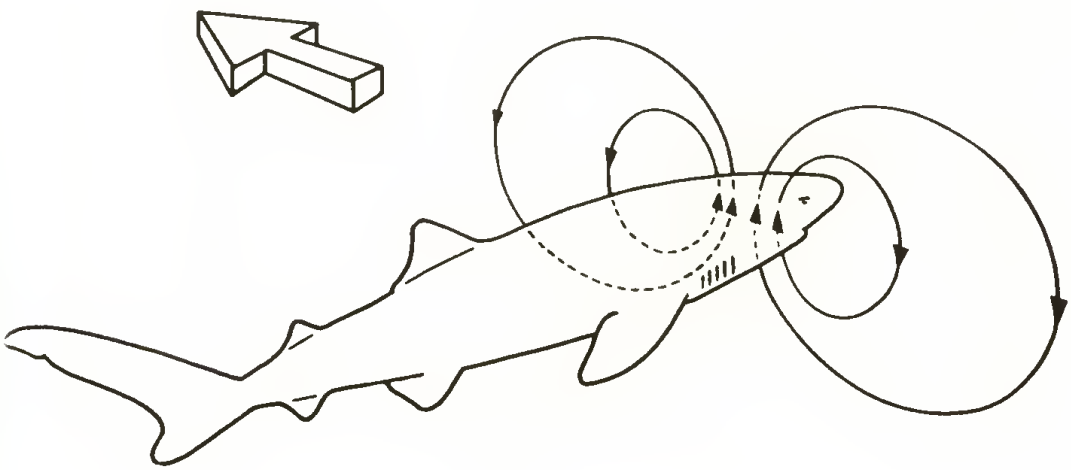
THE ELECTRIC AND MAGNETIC SENSE OF SHARKS, SKATES, AND RAYS

Kalmijn, Adrianus J.; *Oceanus,* 20:45–52, Summer 1977.

Scientists must know the sensory world of an animal if they are to understand its behavior. Certainly most aquatic vertebrates have good eyesight, hear well, and are endowed with a

sharp sense of smell and taste. Yet the physical characteristics of underwater light, sound, and odor fields are quite different from those on land. Moreover, animals use the sensory information in a variety of ways, depending on the particular interest of the species. Therefore, scientists cannot infer from man's own sensory experience how an animal perceives its environment but have to learn through behavioral tests and field observations. This point is dramatically emphasized by animals that have sensory capabilities that stretch man's imagination. One of the most interesting examples of this is the electromagnetic sensory performance of marine sharks, skates, and rays, commonly referred to as elasmobranch fishes.

In predation, sharks, skates, and rays cunningly cue in on the weak, bioelectric fields of their prey, even though it may be hiding under sand. These well-aimed feeding responses clearly demonstrate the remarkable acuity of the elasmobranchs' electric sense and testify to its biological significance in the animals' daily life. Recent research indicates that the elasmobranchs also detect the electric fields they induce when swimming through the earth's magnetic field, and thus sense their compass heading. Obviously, the electric sense of these ancient fishes has reached a high degree of sophistication.



By swimming through the earth's magnetic field (large arrow indicates the horizontal component), a shark causes electrical currents to flow in its body (small arrows).

Last year, the author and his student collaborators initated field experiments in Vineyard Sound off Cape Cod, Massachusets, to verify the results of earlier laboratory studies on the electrical aspects of predation. After chumming with chopped herring, we

observed the smooth dogfish Mustelus canis search the bottom and viciously attack a current source simulating the bioelectric fields motivated by odor, the predators made their final approach exclusively relying on their electric sense. Coming in with the sharks, the American eel Anguilla rostrata paid no attention to the field, whereas in comparative tests the catfish Ictalurus nebulosus repeatedly dug at a current source hidden in the mud along the banks of a local freshwater pond.

The magnetic orientation studies have been conducted on the stingray Urolophus halleri at our land facilities, set up in the woods of the quiet Quissett Campus of the Woods Hole Oceanographic Institution. The stingrays repeating the original experiment, this time, however, with a thin, electrically insulating polyethylene film covering the agar chamber. Now, the sharks and skate no longer responded to the prey, although they eagerly searched about and often passed directly over it (Figure 2d). This drastic effect could not conceivably be due to the mechanical properties of the thin (10 micrometer) and extremely pliable polyethylene film, since even the stiff, 1-centimeter-thick agar roof to which the film was added did not noticeably weaken the feeding responses. Hence, the sharks and skates evidently were able to attack a flounder hiding in the sand without the aid of visual, chemical, or mechanical cues.

The results of these behavioral tests suggested that the sharks and skates electrically located the agar-screened prey. This would also explain the all-or-none effect of the polyethylene film, which offered an extremely high electrical resistance, whereas the agar layer did not distort the bioelectric field of the flounder to any extent. To provide direct evidence for the electrical hypothesis, the presence of the flounder was simulated by passing electrical current between two salt-bridge electrodes buried in the sand. After odor motivation, the predators displayed the same characteristic feeding responses to the electrodes (whether or not covered with agar) as they did to the actual prey (Figure 2e). They dug tenaciously at the source of the field, responding again and again when coming across the electrodes. These results proved that the electric sense of sharks and skates plays an important role in the animals' lives. (pp. 45-48)

• Responsive Sex Changes

A FISH THAT CHANGES SEX

Anonymous; *New Scientist,* 19:372, 1963.

All individuals of the fish Monopterus albus start their lives as functional females but, as they grow older and larger, they turn into functional males, passing through an intersex phase in the process. In this eel-like, burrowing fish, which occurs in swamps and rice fields in SE Asia, sex reversal is a normal process, and is in fact the only way in which males can be produced. This remarkable situation has recently been studied by Dr. K. F. Liem, a zoologist at the State University of Leyden, who has given an account of his observations, in the latest issue of Copeia.

Dr. Liem first studied 96 fish collected in the field. These were measured, and the sex of each was found by examining sections of their gonads under a microscope. The sections were taken from several parts, since in some species of fish different parts of the gonads are of different sexes. The fish were classified as females if ripe eggs or oocytes were seen, and as males if testicular tissue was present. All the smaller ones were females, the percentage falling to about 90 at 28 cm, and then dropping rapidly, none over 40 cm being females. Some intersexes were found, these being between 28 and 46 cm long. A few small fish were males, and the proportion increased with larger fish, all over 46 cm being males. The evidence from these collected specimens, though suggestive, is not conclusive, but Dr. Liem clinched his case by collecting nine egg-masses of M. albus, and rearing the fish from them for three years. After one year, and at six-month intervals thereafter, some of the developing fish were examined. For the first two years all were females, and then at 30 months 67 per cent were females and 33 per cent intersexes. After three years, only 6 per cent were females, 66 per cent being intersexes and 28 per cent males.

It seems from these observations that sex reversal is a regular occurrence in the life of every member of this species, but it is not yet known how it happens or what use it is. It is probably premature to conclude M. albus possesses no chromosomal mechanism of sex determination, and presumably hormones and nutrition are involved, for when some of the fish being reared were starved for several months, males appeared after only 30 months. M. albus normally lives in places that are likely to dry up from time to time, and it is polygamous, females outnumbering males by three to one. So sex reversal may be useful since it will ensure that the males, being older, are in a minority, and that, despite having a shorter breeding period, M. albus can compete successfully with other fish which breed continuously. However, this is only a hypothesis, and there is a lot to find out about this intriguing animal.

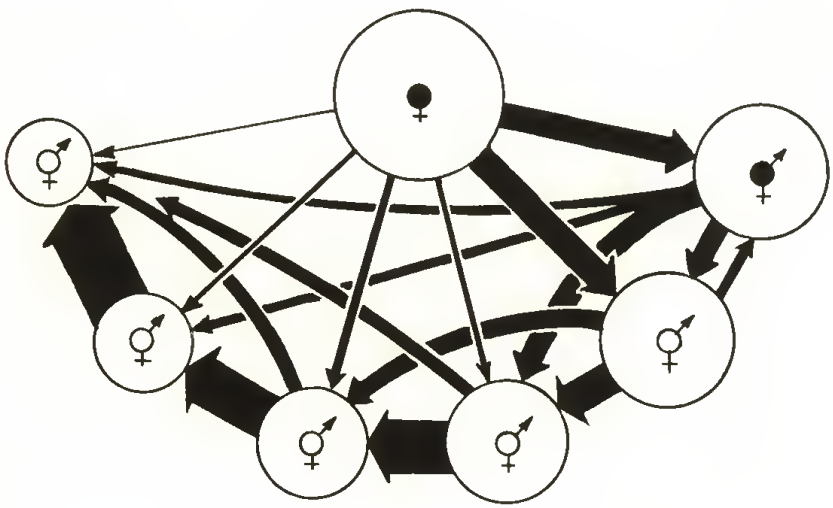
MONOGAMY AND SEX CHANGE BY AGGRESSIVE DOMINANCE IN CORAL REEF FISH

Fricke, Hans, and Fricke, Simone; *Nature,* 266:830-832, 1977.

In several sequentially hermaphroditic coral reef fish, of which individuals first function as females and then males (protogynous hermaphroditism), dominant males can control production of other males by aggressive dominance over females. Robertson suggested that socially controlled protogynous sex changes might operate only in species with a well defined polygynous social system "based on individual relationships. Further exogenous and endogenous factors may control sex change in species which school anonymously and in which the individuals lack special individual relationships. We describe here socially controlled proteandric hermaphroditism (individuals functioning first as males and then as females) in the anemone fish Amphiprion, in which females control production of females by aggressive dominance over males.

Amphiprion is widely distributed in the tropical and subtropical Indopacific region and is symbiotic with several sea anemones. A 38-month field study of the ecology and social behaviour of A. bicinctus in Eilat, Isreal (Red Sea) and a 2-month study of A. alkallopisos at Aldabra indicated that the breeding population of both consisted of large functional females with smaller functional males. Gonadal examinations of 170 A. alkallopisos and 41 A. bicinctus showed that both species are born as males and females, but during ontogeny undergo functional male then female phases. Immature oocytes are always present in the testes of functional males, while functional females show no sign of testicular tissue.

. . . . .



Rank order in a group of seven A. akallupisos. *Arrows show the direction of aggression. Filled circles indicate the two reproductive individuals.*

The typical social unit is a large female, a single smaller male and a varying number of subadults and juveniles, none of which is offspring of the adult pair. Both species live in closed monogamous groups based on individual recognition and individual pair bonding. Several pairs of A. bicinctus remained on their home anemone for 3 yr. without changing partners. Differences in group size result from differences in the ecology of occupied anemones. Occasionally groups of up to 10 adult A. akallopisos are found on a single anemone colony. The average group size of Amphiprion, however, is restricted by the dimensions of the anemones to no more than two adults. "Monogamy by force" and permanent pair formation is favoured under these ecological limitations. The largest and oldest individual of a group is always female and dominates the male, subadults and juveniles. Subadult males are attacked by both sexes, the dominant alpha-female and high ranking males. The beta-male particularly attacks the sexually competing gamma-male. In two groups of four A. akallopisos each, 53% of 861 and 64% of 559 observed, intragroup aggressive acts were performed by beta against gamma. The largest male prevents other males from spawning, thereby maintaining a monogamous social structure. The intragroup social pressure determines the gonadal development of the subdominants. Their testes are smaller and show little or no mature testicular tissue. Low ranking males are psychophysiologically castrated.

Sex reversal occurs in different social situations. After removal of a dominant female in an aquarium group of five mature A. akallopisos, the beta-male took the alpha-position and changed sex in less than 63 d. Sex change was also induced in field experiments by the removal of females from 24 pairs of A. bicinctus; six of the 24 males remained solitary while the remaining 18, joined by subadults of unknown origin, turned into females. The first of these females laid eggs after 26 d. To demonstrate the influence of dominance on sex change, different sized males were paired forcibly on empty anemones in the reef, and the gonads of six pairs were examined after 2 months and another 10 pairs after 6 months. The largest and dominant males always changed to females. Sex reversal also occurred in the absence of intragroup social pressure. Twelve functional males placed separately in the sea, six caged and six free, after 2 months showed various degrees of increased oocyte production. The uncaged males were more advanced towards becoming female in contrast to the caged males which were probably more stressed. Sex change was much slower in both experimental groups compared with dominant males which were paired forcibly with smaller males in the same conditions. The reversal of sex change from female to male could not be induced experimentally. Forcible pairing of functional females (n=7) resulted in death or severe injury to the subdominants. This study demonstrates socially controlled sex change by aggressive dominance. Females control the procreation of other females. They restrict the size of the breeding population and actively suppress males which are likely candidates for future females.

. . . . .

• Suspended Animation

STRANGE RESUSCITATION

Hame, John; *Knowledge,* 5:118, 1884.

A trawling vessel, the Tempus Fugit of Lowestoft, arrived at that port on Wednesday, Jan. 9, about 7 a.m., having made the last catch of fish about 8 a.m. the previous morning. At 8 a.m. on the 9th ult. these fish were landed in Lowestoft Market, twenty-four hours having elapsed since they were caught. I picked up a fine specimen of plaice ("platessa") which was frozen quite rigid. It accidentally dropped from my hand and fell with a smart blow on the vessel's deck, when it immediately commenced jumping about quite as briskly as a fish just out of the water, and continued doing so for upwards of an hour.

This phenomenon is well known by most trawl fishermen, and is by no means an isolated case.

As I cannot understand how a fish that has been frozen for twenty-four hours, and then, by receiving a sharp blow, will exhibit---at least as far as its movements are concerned---all the appearance of a freshly-caught fish, I shall be pleased if any of the readers of Knowledge can throw any light on this subject.

CURIOSITIES OF BEHAVIOR

• Unusual Collective Action

JUMPING RAYS

Addison, A., et al; *Marine Observer,* 46:11, 1976.

s.s. Bendoram. Captain A. Addison, Malta to Callao. Observers, the Master, J.M. Groat, 2nd Officer and Mr. H.S. Jeffrey, 4th Officer.

24 February 1975. At 2000 GMT large numbers of rays were seen jumping from the water. The rays varied in size from about 60-120 cm upwards to 240-300 cm across the wings. They were black on top with a white belly, and 'jaws' could be clearly seen. The jumps from the water appeared haphazard and some of the landings were quite spectacular. They did not appear to be in pursuit of fish. The ship passed through these rays for 20 minutes steaming at 15 knots and they were visible on both sides of the vessel for a distance of at least 2 nautical miles.

Position of ship: 4° 30'S, 81° 36'W.

m.v. Australind. Captain D.A. Dickinson. Pisco to Guayaquil. Observer, Mr. Lorne, 3rd Officer.

29 March 1975. A large shoal of ray-type fish, extending for several miles, was seen to be leaping about a metre in height out of the water, and they continued to do so as we sailed through the shoal. None of the fish was less than a metre from wing tip to wing-tip. Is there any explanation for this behaviour, please?

Position of ship: 4° 00'S, 81' 15°W.

Note. Mr. A. Wheeler of the Department of Zoology British Museum (Natural History) comments:

'The observations of ss. Bendoran and m.v. Australind of shoals of rays both in the same area are very interesting. They were probably Eagle Rays (Aetobatus) one of the surface-living species and although large shoals have been reported before, the reason for the behaviour is unknown. Presumably, they are migrating but whether it is a breeding aggregation or whether they are heading towards richer feeding grounds is unknown. No convincing explanation for their leaping has been advanced.'

EEL'S APPEARANCE PUZZLES

Anonymous; *Science News Letter*, 68:125, 1955.

Scientists were puzzled by the appearance of thousands of eels at the land-locked mouth of Lake Coila in southern New South Wales. Eel heads protruded above the water like a forest of blackened twigs.

The sea was calm and a big stretch of sand separated lake and eels from the ocean.

The eels, up to five feet in length and some weighing 30 pounds, began to churn back and forth across the waters of the lake, which is near the mouth of the Tuross river, 13 miles from Moruya. Within three days a storm lashed the coast, bringing waves across the beach, over the low-lying sandhills and into the mouth of Lake Coila.

Then a squiggling, squirming mass of eels made an exodus into the sea. When the waves receded, hundreds were left

stranded on the sandhills between the lake and the sea.

Harold Phippin, a local resident, said that it was a "fantastic sight" to see a lake about seven miles around full of eels.

The experts do not know what instinct brought the eels to the lake nor why it is that eels seem to carry built-in tide and storm predictors. They agree that the Lake Coila eels were off to their traditional breeding grounds at sea at an unknown site.

Australian Museum ichthyologist Gilbert Whitley said the breeding ground "is probably off New Caledonia," about 1,000 miles northeast of Australia.

He said that the females, fattening up in the fresh water streams, pick up the males from around the estuaries and head back to the breeding ground about every 13 years.

In the deep of the breeding ground, the eels develop large eyes. They lay an egg that floats to the surface, where the elver begins growing. The elvers then work their way back to the Australian coast.

• Ingestion of Foreign Objects

FOREIGN BODIES FOUND EMBEDDED IN THE TISSUES OF FISHES

Gudger, E. W.; *Natural History,* 22:452-457, 1922.

That the larger fishes, especially the sharks, do not discriminate in regard to the things they swallow, is known to all students of ichthyology and is not wholly unknown to the general public. From my own dissections of sharks I have made a list of all sorts of incongruous materials found in their stomachs: heads and horns, hoofs with iron shoes, bones of all kinds, the skeletons of birds and their feathers, the beaks of turtles and their scutes, tin cans, and a host of other preposterously indigestible things. Furthermore, to my own list I have added data from various other writers until the list is almost as large as it is varied and incongruous. But to the average student of fishes, as well as to the readers of Natural History, I suspect that the title of my article will seem strange and unusual.

Many years ago, while dissecting a fish in the laboratory of the United States Bureau of Fisheries at Beaufort, North Carolina, I found embedded in a fold of the mesentery a hard, fairly straight body from five to seven inches in length and with aproximately the diameter of a small lead pencil. On cutting it out and carefully freeing it from the enveloping tissues, I found it was mummified pipefish, which had been swallowed at some previous time, had worked its way out into the mesentery and had

there become preserved. The pipefish was very much shrunken, consisting of hardly more than the bony framework and the tough integument, and was very hard, offering considerable resistance to the scalpel; but there was no evidence that putrefaction had taken place, nor had the containing fish suffered any apparent injury. My notes made on the occasion having been destroyed and the mummy lost, further information unfortunately cannot be given.

In July, 1912, while dissecting a barracuda (Sphyraena barracuda) at the Marine Biological Laboratory of the Carnegie Institution of Washngton, which is located at Tortugas, Florida, I found a similar body embedded in the mesentery. Recalling my past experience, I at once suspected that this also was a pipefish, but when it had been freed from the mesentery, it proved to be merely the backbone of some unidentifiable fish. It was about five inches long and fairly straight save at the upper end, where it was bent in the fashion, shown in the figure on page 452. This phenomenon was reported and the figure published in my paper on the barracuda. How the pipefish referred to in the previous paragraph could have worked its way through the intestinal wall into the body cavity can at least be conjectured, but how this vertebral column could have done so is hard to conceive.

Becoming interested in this phenomenon and being informed that the late Vinal Edwards, the veteran collector at the station of the United States Bureau of Fisheries at Woods Hole, Massachusetts, had noted similar occurrences, I wrote to him for additional data. Under date of February 3, 1917, he answered that since 1908 he had noted three such occurrences; namely, in a hake, a swordfish, and in a sculpin. In the first two there was "a skeleton of a fish in the meat near the backbone." In the case of the hake the embedded skeleton was about ten inches long and in that of the swordfish about a foot long. In the sculpin he found what looked like a pipefish. These were all sent to the Bureau of Fisheries in Washington, but as the war was at the time engrossing men's energies, the specimens were mislaid and cannot now be found.

Bearing these things in mind, when I became joint editor with Dr. Bashford Dean, of the Bibloigraphy of Fishes, which is being issued by the American Museum, I noted down all similar occurrences in the course of our indexing the vast literature on fishes brought together in this work. The data thus gathered are added herewith to that presented above, in the belief that the readers of Natural History will find them of interest and possibly of value.

The first of these accounts is that given by Captain N.E. Atwood before the Boston Society of Natural History, April 1, 1857. He is quoted as saying that:

> "The cod often swallows alive the tant or sand-eel and the pipe-fish, both having heads very much elongated anteriorly and pointed. These fish sometimes pierce the stomach of the cod and escape into the abdominal cavity, and there they

are found in a perfect state of preservation, adherent to its walls, but changed in color to a dark red, and in substance so hard that they are not readily divided with a knife. They have to be cut away before the cod can be split open. The fish is always in good health apparently, and there are no marks of inflammation about the stomach or abdominal cavity, unless the material of attachment be considered as such."

The next account was also given by Captain Atwood before the Boston Society of Natural History on January 5, 1859, and is thus reported in the Proceeedings of that society:

"Fish are often swallowed by the cod, pass from their stomach into the abdominal cavity, and are there found mummified and adherent to the inner walls; he presented a specimen, apparently of the eel family, thus preserved and hardened, which he had taken from the adbominal cavity of a pollock. . . . He presented two large cod hooks, with portions of the line attached, which he had taken from the livers of apparently healthy cod; the greater part of the hooks was buried in the organ, and must have remained there, he thought, at least twelve months; they must have been swallowed, broken off, and have worked their way through the stomach into the liver."

Nine years later (1868) Captain Atwood again addressed the Boston Society of Natural History on this subject. The report, as recorded by the secretary, reads as follows:

"Captain Nathaniel E. Atwood exhibited a codfish which presented a curious appearance. A number of sand-eels were seen in the walls of the abdominal cavity; they were so hard as to resist the knife, not at all decomposed, and in many places with a sort of earthy crust or membrane of their own. Capt. Atwood said the occurrence was not an unusual one, and the cod, being in good condition, had apparently not suffered at all by this phenomenon."

Thereupon the presiding officer, Dr. Jeffreys Wyman, remarked that there were three fairly well defined features presented by this fish. In the first place, the eels were outside the cavity of the stomach; in the second place, they had an investing membrane of their own although they had apparently been embedded in the cod for some time, and finally--the most remarkable point of all--they showed no signs whatever of putrefaction. The fact that they had not decomposed he thought, on the theory of Pasteur, was due to the absence of any disturbing agency.

Apropos of the strange things swallowed by fishes and in anticipation of the embedded foreign body next to be noted, attention is here called to a letter from Capt. J.W. Collins to Prof. S.F. Baird, published in the United States Fish Commission Bulletin for 1884, p. 175. In this Captain Collins puts on record the finding in the stomach of a large codfish taken on Le Have

Bank, of a knife known as a "haddock ripper."

However, in 1886, Captain Collins recorded what is undoubtedly the most remarkable instance known of a foreign body embedded in the flesh of a fish. We will let him tell his story in his own words:

"While discharging a fare of codfish from the schooner Vinnie M. Getchell, at Gloucester, Mass., on September 15, 1885, Capt. John Q. Getchell, master of the vessel, found embedded in the thick flesh of a large cod a knife of curious workmanship, represented by the accompanying illustration, which is of full or natural size.

"The fish in which the knife was found was one of a fare caught in 75 fathoms of water on the northeast part of George's Bank; it was apparently healthy, being thick and 'well-fed,' and according to Captain Getchell, would weigh about 40 pounds after being split, or say 60 pounds as it came from the water. The general excellent quality of the fare of fish attracted considerable attention from people who saw them, and led to the discovery of the knife. Some remarks having been made concerning the fish, Captain Getchell lifted several of them from a tub (where they had been thrown to wash after being weighed) and exhibited them to the bystanders, commenting on the size and thickness of the specimens. Holding one across the edge of the tub in a semi-curved position, he ran his hand over the thicker portion of the fish to call attention to its fatness. In doing so, he felt something hard beneath his fingers, and further examination produced the knife. Of course much surprise was expressed by those present who had never before seen such a strangely formed implement, and speculation was rife as to how it came there. When found, the knife-blade was closed, and the small or posterior end of the handle was the part first felt by Captain Getchell, and was nearest the tail of the fish.

"The flesh of the fish where the knife was imbedded is estimated to have been 2½ inches thick. Unfortunately, the excitement attending the finding of the knife prevented any notice being taken of the fish, which was carried off and salted among the others; therefore nothing is known as to whether the implement was encysted or not.

"The handle of the knife is of brass, curved and tapering posteriorly, with a longitudinal incision, on the concave side, to receive the edge of the blade. The handle is remarkable in form, and is suggestive of the handiwork of some savage tribe or the scrimshaw work of a sailor. Its length, measured with the curve, is 3-5/8 inches, and its greatest diameter one-half inch.

"The blade is lanceolate in form, with the cutting edge curved outward, to fit into the handle, and the back nearly straight. It has been corroded a good deal and the extreme point is very thin. Its length, from handle to tip, is 2-3/4 inches; greatest thickness (near the handle), one-eighteenth inch; and its greatest breadth a little less than one-half inch.

The total length, from point to point in a straight line, is 6-1/4 inches.

"How did the knife get there? is the question that will be asked by those who are not too skeptical to credit the story of its being found as has just been stated. Personally, I neither doubt the finding of the knife, nor the probability of its being found as stated. It is a fairly common occurrence for fishermen to find the sand-launce, or lant, imbedded in the flesh or the liver of the cod, and dried very hard. I have many times seen lant thus imbedded, and in no case that I remember was the cod any the worse for it. It is therefore evident that it is possible for the stomach of a cod to be penetrated by a sharp-nosed fish or by an implement it has swallowed, and ultimately for either to work its way through and become imbedded in the flesh, while the would heals and the stomach goes on to perform its ordinary functions.

"As to where the fish got the knife we can only conjecture, unless some ethnologist can point out its origin. In any case, the finding of such a remarkable implement in such a strange place must be a matter of interest to the ethnologist and naturalist alike."

However, let us return to our sand eels, since they seem to be the piece de resistance of the cod family. In 1885, W.H. Barrett mentions the finding of a sand eel embedded in the liver of a haddock. It was four inches long and was firmly embedded, with its dorsal region toward the liver. The head and half an inch of the body lay in a groove formed by pressure in but were non-adherent to the liver. The other parts of the eel were, however, adherent to it. The little fish was partly covered over with white membrane, which in spots was apparently filled with cohesive matter. Because of this it was difficult to cut through the fish with a knife.

Barrett explains the penetration into the body cavity as follows: He notes that the sand eel penetrates the sand by using its sharp-pointed lower jaw as a wedge or drill. He thinks that the fish is swallowed head first and that head first it penetrates into one of the caecal diverticula just below the stomach. It then bores with its jaw until its head penetrates into the body cavity, but if the caecum is too small to let the larger shoulder region pass through, the fish is caught and held. However, the fish by vigorous wrigglings may tear the caecum off and pass out into the body cavity of its host, where it would die surrounded by its cuirass of caecum. Later, partly through pressure and partly

as a result of inflammatory processes, it would become embedded in and adherent to the liver.

The most extensive series of observations of the phenomenon ever made, illustrated by the only figures known (save that of the knife and my own of the backbone) we owe to H.C. Williamson, who published so lately as 1911. According to his records, also, sand eels were the chief offenders and the number of instances of their penetrating into the body cavity of their devourers was eight in all. Speaking of these cases in general, he says:

> "The sand-eels, after being swallowed by the fish, have excaped from the gut and passed into the abdominal cavity. There they have generally damaged the liver before they died. Sometimes they are found with the head or tail jammed tightly into the space between the reproductive organ and the peritoneum. They are covered with a material which resembles a hardened paste, and in some cases they are in part enclosed in a skin of connective tissue derived from the peritoneum. In this way they are reduced to a mummified condition. . . . One large sand-eel was commonly found in the cavity, but in one case three small sand-eels were present."

Some of Williamson's clearer figures are reproduced herewith, together with the gist of his remarks concerning each case. three cases are not figured, but of them he says:

> "A large sand-eel, 7-3/4 inches in length, was lying along the dorsal region of the abdominal cavity of a codling. It was thickly plastered with hardened paste. Its tail was twisted round the urinary bladder. The skin of the liver, which had evidently been destroyed, was attached to the peritoneum as a thickened wall along the ventral part of the abdomen.
>
> "Another codling had a very small sand-eel, 2-3/8 inches ling, coiled up at the anterior end of the abdominal cavity. The anterior third of the fish was buried in the liver, and the liver had grown attached to the peritoneum. A sand-eel was discovered in one saithe. It was adhering to the abdominal wall."

Now we come to the last, and, taking all things into consideration, the most remarkable case of an embedded body ever recorded. Those previously described, including the knife, are pointed bodies, which one might expect would under favorable conditions penetrate the walls of the stomach or of the intestine and pass into the abdominal cavity. But the intrusive animal to which attention is now called is bulky and, furthermore, is provided with five pairs of sharp-pointed legs that are capable of offering opposition. The case in point is that of a hermit crab, which, probably finding that it had outgrown its quarters in some marine snail shell, had left its safe home to seek new quarters, and while on this unprotected quest had been spied by a wandering codfish, pounced upon, and swallowed whole.

However, it revenged itself in true melodramatic fashion by penetrating the wall of the stomach and passing into the body cavity where it became transformed into a mummy, surely to the great discomfort of its former captor and present host.

Williamson agrees with Barrett that the sand eel might without great difficulty penetrate the walls of the stomach, caecum, or intestine, and thus get into the cavity of the abdomen, but that this could not have been the process by which the crab passed, he is sure. The crab, he thinks, must have passed through the wall of the stomach at a point where the tissue was weakened, possibly to some degree by the attacks of some intestinal parasite, such parasites being found only too frequently in fishes.

Some occurrences of intrusive bodies, such as those of pipefish and sand eels, in the body cavity are fairly easily explained, and even the presence of the hermit crab can be understood. Not so clear, however, is the presence of the spinal column which I found in the abdominal cavity of the barracuda; and I am entirely at a loss to explain how the knife made its entry and how the skeletons noted by Vinal Edwards could have penetrated into the great muscle of the back of the fishes in which they were found. Careful dissections of such specimens by a trained anatomist might show traces of the manner of penetration. Until such an opportunity offers itself, the matter must remain more or less of a mystery.

• A Cocoon-Weaving Fish

NIGHTGOWN-WEAVING FISH

Long, E. John; *Nature Magazine,* 52:187–188, 1959.

When scientists first saw it, they thought they were watching a very sick fish. But its breathing and other reactions appeared to be normal. And further more, it wrapped itself in a mucous envelope only at night, swimming out of it again in the daytime to lead a normal piscatorial life. What the surprised scientists were observing finally became apparent---it was a fish that actually wove its own nightgown when darkness fell, and shed it at daybreak!

While most fish do not sleep, it is well known that many species rest on the bottom or among reef and rock formations at night. Some of them lean against various objects, such as boulders, coral or shells, as they while away the hours of darkness, neither feeding nor, what is much more important, being eaten by midnight predators.

Unquestionably the strangest and most interesting of these sea-bottom resters are parrotfishes, notably the rare and brilliant rainbow-parrot, Pseudoscarus guacamai. As soon as the last trickle of twilight has faded from clear tropical seas, it glides down to the purple shadows of the sandy bottom. Resting lightly on the tips of its ventral fins and tail, the parrotfish begins an odd ritual.

From its blue-green beak exude long, gossamer threads which it weaves, by the motion of its body, into a cobweb-like envelope that soon encases the entire fish. A little flap with a hole in its center covers the open mouth, and moves in and out as the fish breathes. Behind the caudal--or rear--fin there is another tiny opening through which expired water leaves. Thus the necessary flow of liquid over the gills is assured.

At first, the thin, transparent folds are difficult to see unless light rays are directed at an angle. Later, as the folds become thicker, the parrotfish disappears from view, and the envelope looks like a mere gelatinous mass that might be anything from a discarded egg case to some other useless bottom debris.

The exact formation of this marvelous enevlope has not yet been observed, even by scientists working with captured parrotfish in an aquarium tank. During these aquarium experiments, it was found that an artifical light could not be left on for more than a few seconds--at intervals of five minutes--without the fish suspecting that daylight had returned and a nightgown would be superflous.

While a parrotfish may spend a busy half-hour to an hour, or more, preparing for bed, it can break out of its covers in less time. Even so, the escape is not easy. The mucous covering seems to be sufficiently resistant to slow the first early-morning swimming efforts. So, some backing and filling seem called for. In four out of seven observed attempts to free itself of the envelope, the fish succeeded by wriggling backward; twice it broke through the sides, and once it simply forged straight ahead.

With some species, the nightgown-weaving is apparently a normal, every-evening procedure, although one parrotfish, Scarus brachiale, does not form an envelope at all. And just to make it difficult for the fish experts, a few specimens were observed in their native haunts forming mucous envelopes around their bodies in broad daylight--possibly making practice runs!

Just what is the purpose of this nocturnal gown-weaving, a procedure, as far as is known, unique in Nature? Lacking a more complete scientific study of the creature, we can only surmise and wonder, says Dr. Howard E. Winn, of the University of Maryland, who made a detailed study of the phenomenon during the summer of 1954 at the Lerner Marine Laboratory, at Bimini, in the Bahamas. The diaphanous garments, thin as they are, may afford some sort of protection against night-prowling enemies. Also, Dr. Winn believes, they may protect the body from becoming surrounded by smothering silt. In some instances the mucus near the bottom had sand grains attached to it, and silt particles often settled on the upper parts.

The lack of any kind of light, which is usually necessary to

the formation of the mucous envelope, leads some scientists to believe that there is a nervous control, leading from the eyes to the nervous systems, and finally to the mucus cells. Another theory is that there may be less oxygen along the sea-bed, and this lack triggers the mucus cells. Obviously more research is needed here.

The nightgown-weaving habit is not confined to parrotfishes of the Bahamas area. Dr. John E. Randall, of the University of Miami's Marine Laboratory, observed the strange practice in mid-Pacific coastal areas, and in certain South Pacific island waters.

. . . . .

• Singing Fish

MUSICAL FISHES

Anonymous; *Scientific American,* 3:304, 1848.

Aquatic animals are generally supposed destitute of the means of making themselves heard; and if they communicate with each other, it is usually supposed that it must be otherwise than by sound. The seal, has, it is believed, a peculiar and distinct cry; and the grampus snorts as it attains the surface. Frogs and other amphibious animals croak loud and long enough; but in all these cases the sounds are emitted, not under, but above the water, and by creatures rarely more than half aquatic. The cetaceous races have warm blood, and suckle their young; and fishes, properly so called, are considered, as we shall presently show, erroneously, a silent race. The long-eared Baalamite is justly reckoned the strangest ass mentioned in history, and a scaly creature emitting sounds may truly be reckoned a very odd fish indeed. A party lately crossing from the promontory in Salsette, called the Neat's Tongue, to near Sewree, were about sunset, struck by hearing long distinct sounds like the protracted booming of a distant bell, the dying cadence of an Eolian harp, the note of a pitchpipe of pitchfork, or any other long drawn-out musical note. It was at first supposed to be music from Parell floating at intervals upon the breeze; the it was perceived to come from all directions almost in equal strength, and to arise from the surface of the water all around the vessel. The boatmen at once intimated that the sounds were produced by fish abounding in the muddy creeks and shoals around Bombay and Salsette; they were perfectly well-known, and very often heard. Accordingly, on inclining the ear towards the surface of the water, or better still by placing it close to the planks of the ves-

sel, the notes appeared long and distinct, and followed each other in constant succession. This fish is about the size of a perch.

SINGING FISH

Anonymous; *Zoologist,* 18:7237-7238, 1859.

That part of the interesting letter from Dr. Buist, in the 'Athenaeum,' relating to the voice of fishes, has induced me to send you this note. The River Plate swarms with fish, and is the habitat of one possessed of a very sonorous voice, like that found in the River Borneo---the account of which is quoted by Dr. Buist from the 'Journal' of the Samarang. During the years 1845-6 H.M.S. Eagle was stationed in the River Plate, and anchored about three miles from Monte Video, with an occasional change a little higher up the stream, or lower down, as far as Maldonado. During my service on board that ship I have often heard a loud piscatory chorus during the night. My cabin was in the after part of the gun-room; and when lying in bed with the "scuttle" open (a small window about six feet from the surface of the river) I have listened with pleasure and curiosity to the loud sounds proceeding from these creatures as they roamed in numbers round the ship. The noise they made was at times very loud and close at hand, and seemed to proceed from many voices --receding now and then, and becoming lost in the distance, and again returning. At other times, as far as I could judge, a solitary voice was heard, and then a few together. There was little variation in the sound, which was of a deep tone, more like the rumbling of a drum or organ than anything else. Nevertheless, whether it was fancy or not, there was something pleasing, and even musical, in the sounds as they became slowly or suddenly distant or near with the retreat or approach of the band. I have no doubt that this drumming sound came from one of the Sciaenoids, probably akin to the Pogonias; but the fish was never seen or caught near the ship where its voice was heard. Amongst the numerous varieties of fish brought on shore on hauling the seine at Maldonado Bay, several were occasionally caught resembling the Pogonias Chromis, varying in weight from 40 to 60 lbs. (From the 'Athenaum.')

Singing Fish.--On the broad expanse, or bay of the Tagus, which extends from Cacilbas-Point to Aldea-Gallega, I have heard proceed from the water (and apparently close under the boat), on several occasions, sounds resembling the vibrations of a deep-toned bell, gong or pedal-pipe of an organ. The boatmen seemed to know them well, and generally exclaimed "The Corvina!" This is a large fish with black fins, which is, I believe, to be met with occasionally in the Lisbon market. (Id.)

• A Curious Climbing Fish

THE CLIMBING FISH

Johnson, R. D. O.; *Natural History,* 19:349-351, 1919.

Up in the Andean heights of the Department of Antioquia, Republic of Colombia, there is a climatic stratum marked by a uniformly cool temperature and great humidity. The rainfall is enormous in quantity. The topography included within the stratum is mountainous in the extreme. The streams are many and torrential in character, and their waters rush roaring down the steep and tortuous channels to the placid rivers of the plains below---they are but a series of falls, cascades, and blustering "riffles." The country rock is schistose in character and comparatively soft and the erosion of the stream beds is very rapid.

Ancient stream beds high up on the canons' sides are pitted with many potholes of unusual interest to the student of dynamic geology. There is not a waterfall in the region today so small or insignificant that it is not busily engaged in boring out a more or less cylindrical hole in the rock beneath. The falling water at the point of impact seems inevitably to set up a rotary motion, carrying stones, sand, and gravel around with it, and the resulting wear bores out the pothole.

Into these potholes falls the drifting, gold-bearing quartz with which the upper Andean regions abound, and within these mills of nature it is ground to an impalpable powder, and the gold freed from its matrix finds lodgment in the gravels and the alluviums of the plains and the river bottoms. It was the lure of the gold that indirectly drew my brother and myself so far into the jungle ---jungle that answers the most rigid definition of the term.

We were employed to install a hydroelectric plant to be used in connection with the operations of a company engaged in placer mining. A permanent camp had been established in niches cut in the steep sides of the canon and was located at an elevation of 115 feet above the roaring Santa Rita Creek.

Since power streams were numerous, we selected the one most convenient to the camp for beginning our work. The bed of this power stream held an average angle of thirty-eight degrees from the horizontal and, for a considerable distance, slipped down over the smooth surface of the worn rocks in an thin broad sheet.

Our first efforts were directed toward ascertaining the volume of flow of the stream. To do this it was necessary to introduce a measuring weir at a point above the take-off of the plant. The weir was soon established and the deflecting dams were built in. When the water was turned, a part of the bed of the stream lay uncovered, exposing a couple of old gravel-filled potholes. Since such potholes not infrequently contained gold, my brother proceeded to dig out one of them while I was engaged in taking the

readings from the weir.

He had been at this task for only a few minutes when he called out to me:

"Say, here's a fish."

I replied saying something about his "seeing things," and proceeded to expatiate upon the impossiblity of his finding a fish in such a place, and upon the utter inability of any fish, even among the best swimmers, to surmount the difficulties of such a stream.

I pointed out the absurdity of imagining a fish swimming with nine-tenths of its body out of the water, as it would have to be, up that part of the stream where the water passed in a thin sheet over the smooth rocks. "He'd have to be an aviator," I said. So I pooh-poohed the idea recklessly.

Harry listened with suspicious patience to my lengthy dissertation, while I, from a theoretical standpoint, utterly demolished his unthinking assertion, then he blurted out:

"Well, are you all through? Here's the fish! This is a fact, not a theory you've butted up against."

He held in his hand a living fish, and a catfish at that, resembling the catfish or horned pout of the North. I took it and looked it over. There it was, a real live fish, nearly a half foot long. There could be no possible doubt about it, in spite of the utter impossibility of the thing.

Harry had his laugh and returned to his digging. I was completely puzzled---but I had pressing work to do. I carefully placed the fish in a small pothole at one side. This hole was about four inches in diameter and twelve inches in depth and held perhaps two or three inches of water. Catfish are hardy, so I figured that there was enough water to last this little fellow until I could give him more attention.

After I had finished my work at the weir, I returned to the little pothole to give that amazing fish a closer scrutiny.

He was not to be found, so I called out, "What did you do with the fish, Harry?"

Harry asserted that he had not taken the fish and that he had paid no attention to it. That certainly was a mystery. I did not think it possible for a five-inch catfish to jump out of a four-inch pothole twelve inches deep. I concluded, however, that that was the only way of escape and contented myself with this rather lame explanation.

Before we returned to the camp that afternoon, Harry had caught two more "cats" in another pothole. These we carried down to the camp in our dinner pail. We arrived at the camp just as the late afternoon meal was being served. I hastily poured the water and the fishes from the dinner pail into a three-gallon galvanized bucket and set it in an inconspicuous place outside the kitchen. After dinner I sought the bucket to get a better look at the fishes which had destroyed a good theory. They were not in the bucket. I inquired of several who might possibly have freed the fishes but no one knew anything about them. This mystery was setting too thick for comfort.

The next day I made a special trip up the power stream and

managed to secure two more of these fishes. I brough them down to camp and placed them in the same pail that had held the others and sat down to watch their maneuvers.

For a time they were content to swim about, butting their blunt noses against the sides of the vessel. Then, to my amazement, one of them thrust its "nose" out of the water and began creeping up the side of the pail. I watched it hitch itself up by short longitudinal movements until it had reached the top edge and fell outside of the bucket. I put it back and watched the performance repeated. Then I transferred one to a tall glass jar and through the glass watched the operation of the creeping mechanism. I caught others and dissected them and studied them until I was in possession of their secret.

This lies in the combined action of two sucking mechanisms. One of these is the ordinary sucker mouth, surrounded by a soft flap, very thin and flexible at the edges. The other is an interesting structure consisting essentially of a bony plate beneath the skin on the under side of the fish where the ventral fins are attached. These fins are broad and flat and their surface is studded with small sharp teeth pointing backward. The bony plate is given a shuttle action by muscles attached fore and aft so that the fins may be moved lengthwise of the fish through a distance equal to about one sixth its length. With this apparatus the fish is able to create a suction pressure, and by means of the alternate action of the two suckers, it is enabled to crawl, inchworm-like, on a smooth vertical surface.

Shortly after this, the mining company undertook the cleaning out of a large pothole which was eight feet in diameter and twenty-two feet in depth. Before the bottom had been reached, the water that remained in the pothole was found to be full of these climbing catfishes. They were naturally greatly agitated by the action of the workmen who were shoveling out the gravel. Several times some of them started to climb out but were frightened by the men and dropped back. I surmised that as soon as the work was stopped for the lunch hour these fish would essay the long climb to the top. I was not mistaken and my watching was rewarded by seeing four climb up a distance of eighteen feet to the pool of water above. They followed a thin film of water that trickled down the rock. This water kept their gills wet and sustained them on a climb that must have been arduous. It required half an hour to make the ascent.

To my own satisfaction I had answered the question of how it was done; there remained the question of why. The fish was evidently a case of extreme modification and adaptation to fit a peculiar environment. Some catfish do not climb, why should these? An analysis of the environment brought the answer.

I found that the Andean torrents were the habitat of myriads of these curious creatures, "capitanes" they are called by the natives. The individuals I had examined were living in a torrential stream almost daily subjected to the sudden fury of sweeping floods. The violence of these floods is unimaginable to one who has not witnessed them. It seems that nothing unanchored in the stream bed can withstand their wild energy. As swimmers,

however, these fishes are clumsy and inept. To witness their awkward, wriggling, swimming movements is to know at once that they could not by that means of propulsion alone make any headway against even moderate currents.

We can understand that to remain at home in time of flood, these denizens of the wild waters anchor themselves by means of their sucker mouths. Yet these catfishes are to be found in all parts of the streams, from the slender spring branches of the high mountains to the sluggish rivers of the plains. Travel they must and by using the climbing mechanism I had seen operate---the alternate action of mouth and ventral suction plate. That they are able to surmount even great falls is evident from their presence in the Santa Rita Creek, for this stream falls into the Santo Domingo River over a precipice more than two hundred feet in height.

• Fish-to-Egg Memory Transfer?

SALMON ADD NEW MYSTERY TO THEIR MIGRATION HABITS

Anonymous; *Science News Letter,* 50:329, 1946.

Do salmon eggs "remember"?

A new angle has been added to the already tangled riddle of salmon migration by the failure of Canadian fisheries' men to restock a depleted river, once rich in fish, by planting its headwaters with eggs taken from other waters in the same system. The story is told by Dr. R. E. Foerster, director of the Biological Board of Canada.

Eagle river in British Columbia, once a fishing ground for the high-grade, red-fleshed---sockeye salmon, long ago lost its profitable silver horde. In an effort to build it up again, millions of eggs were transplanted from two other sources over more than ten years. The eggs hatched all right, and before the young fish left on their migration to the sea many of them were marked by clipping certain fins.

When the time came for the matured fish to come in from the sea and migrate upstream, the marked individuals were very carefully watched for. But of thousands of marked fish only a few scattered individuals were ever caught and identified.

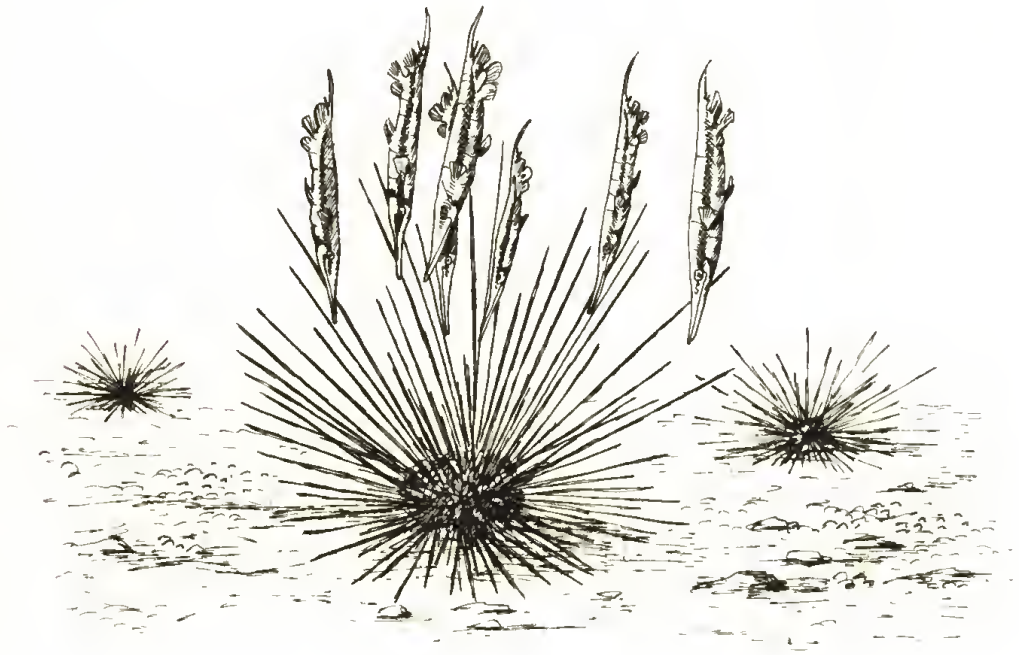
They did not turn up in other rivers, either. Dr. Foerster suggests that the fingerlings may have failed to reach the sea, either through capture by other fish or by remaining permanently on a lake that is part of the Eagle river system. There is, however, no proof for this theory. But if the explanation does not hold, no other satisfactory one seems to be at hand.

• Mimic Behavior

SPECIFICITY AND BEHAVIOR IN SYMBIOSES

Davenport, Demorest; *Quarterly Review of Biology,* 30:29-46, 1955.

A fascinating partnership that appears to be totally uninvestigated is that in the western Pacific of the wonderfully modified striped fishes of the genus Aeoliscus, who stand on their heads over the long spines of their host sea-urchin. What stimuli elicit this curious behavior? The above citations indicate that many of these associations have been studied and described; in some, a few simple experiments on specificity have been conducted. But in every case without exception the behavior concerned is in need of precise experimental analysis by modern techniques. In each case we must identify stimuli eliciting special behavior and determine precisely the manner in which the partners are first brought together. As one may observe in many ecology textbooks, these associations have acquired a reputation of being "unique," "unusual," or "extraordinary." Certainly their adaptive "reason-for-being" is no more extraordinary than that of any other individual



A type of mimicry? Fish stand on heads over sea urchins.

natural adaptation. Actually, this reputation depends largely upon the fact that two organisms are linked together, and the behavior concerned in the linkage is not understood. It is unusual behavior only because, without analysis, it seems to demand reason or, at least, learning. That it is not unusual or inexplicable at all will certainly be disclosed by the critical analysis to which it is so clearly susceptible. (pp. 37-38)

This angler fish swishes its fish-like bait about to improve realism.

• Strange Forms of Symbiosis

A SINGULAR FISH

Anonymous; *English Mechanic,* 33:302-303, 1881.

The sea-cucumbers, or Holothurians, are known to be the most highly-organised of all the Echinodermata. They vary considerably in length, according to species, from about an inch to a

yard. They have a soft, leathery, muscular integument, and in locomotion the body is extended and contracted as in annelids; but the chief organs of locomotion, as in star-fishes and sea-urchins, are suckers, or tube-feet, of which there are usually five double rows. The radiate structure is most apparent in the mouth, which is surrounded with tentacles. There is no proper stomach; the intestine is often very complicated. The respiratory organs are near the anus, and consist of branching tubes. The sea-cucumbers move slowly in the sand, and feed on minute radiolaria, foraminifera, and other organisms.

Now these animals have been long known by fishermen to harbour a curious fish, to which Cuvier gave the name Fierasfer. Several species of it are known. The most common in the Mediterranean, the Fierasfer acus, has been recently made an object of special study by Prof. Emery, at the zoological station at Naples.

To procure Holothurians tenanted by the fierasfer, it is necessary to seek the animals at a certain depth; those living near the shore do not usually contain them. The two Mediterranean species of holothurians, which are most frequently tenanted, are Stichopus regalis and Holothuria tubulosa. When these animals are accumulated in certain quantity in the same tank, the little fishes ere long appear. According to Prof. Emery, who has opened hundreds of holothurians in his search, the fierasfer is generally lodged in the cavity of the body. It penetrates first, as we shall see, by the anus into the intestine. Then it migrates into the pulmonary passages, the thin and delicate walls of which soon rupture in consequence and allow the fish to pass into the peri-intestinal space.

When free, the fierasfer ordinarily swims in an oblique position, the head down and the tail curved towards the back. By undulatory movements of its ventral fin, it moves obliquely forward, keeping about the same level, or obliquely in the direction of the axis of the body. It is but a poor swimmer, and when placed in a tank along with other fishes it is soon devoured, being incapable of flight, of defending itself, or of hiding in a medium uninhabitable by it.

Swimming with its head downwards, the fierasfer explores the bottom of the water and the bodies lying there. If it comes upon a holothurian, it immediately shows some agitation, examines the object on all sides, and having reached one of the extremities, examines it attentively. If it be the head-extremity, the fish returns suddenly, and proceeds to the opposite end, by which the holothurian sucks in and expels the water necessary to its existence. Then commences a curious proceeding. In the time of expiration, when the holothurian is expelling water, the little fish, excited by this mechanical action, applies its snout strenuously to the anal orifice, then curves back its pointed tail over one side of its body, and by a rapid movement of recoil, introduces the tail into the rectum of the holothurian. This accomplished, the fish raises the anterior part of its body, while its tail remains pinched in the holothurian, and pushes itself further and further in with each movement of suction. After a time the anterior part enters in its turn, and the fish is completely inclosed in its host.

Prof. Emery has sometimes seen a small fierasfer get into its

A fierasfer entering a sea cucumber

position at once, while in other cases the progress of the fish is so slow that the patience of the observer is exhausted. While the general mode of introduction is that described, there may be some modifications. Thus the fierasfer may penetrate head-first, or, victim of a mistake, may endeavour, generally without success, to affect an entrance by the mouth of the holothurian.

The fierasfer is not necessarily solitary; on the contrary, it often shares its abode with two or three of its kind. Prof. Emery has seen, in the Naples aquarium, seven fierasfer successively enter the same holothurian, causing their host injuries which proved fatal.

It has already been stated that the fierasfer does not remain in the intestine, which is difficultly habitable because of the quantity of sand in it. We have to note, however, that it always remains near the anus, through which it protrudes its head, from time to time, in search of food. Thus it is not, in any way, either a parasite or a commensal, in the sense attached to these words in natural history - that is to say, it does not live at the expense of the holothurian, either consuming its substance or taking some of the food that animal has amassed for itself. Hence the earlier naturalists who studied the habits of the fish were mistaken in considering it as an example of parasitism by a vertebrate animal. The fierasfer is merely, as Prof. Emery puts it, a lodger, or tenant.

According to Prof. Semper, of Wurtzburg, however, there is on the coast of the Philippine Islands, a small fish of the genus Encheliophis, closely allied to fierasfer, which, also living in holothurians, feeds on their viscera, and is, therefore, a true parasite.

DEATH WHERE IS THY STING?

Lubbock, Roger; *New Scientist,* 86:153-154, 1980.

One of the most notable associations is that between clownfish and anemones. There are about 27 species of clownfish that are found living with some 13 species of anemone. Adult clownfish are never found without an anemone, and spend much of their time swimming idly among the tentacles. The fish lays its eggs beside the anemone and it is only as a larva, floating with the plankton, that the clownfish is not associated with an anemone.

I have made a detailed study of one species of clownfish Amphiprion clarkii and one sea anemone Stichodactyla haddoni, and have discovered that the fish is all but imperceptible to the anemone.

. . . . .

I had to conclude that the clownfish derives its protection from the mucus that the fish itself produces. Not that the mucus contains substances that inhibit the anemone's nematocytes; rather it does not contain the excitatory substances that are present in the mucus of non-symbiotic fish.

If an anemone usually stings a fish, but does not sting a clownfish because its mucus is in some way inert, the symbiosis between the anemone and the clownfish comes down to a problem in cellular recognition. In experiments the anemone can clearly distinguish between a varity of different proteins, glycoproteins, polysaccharides and lipids. The stinging cells discharge only after a rather complex recognition procedure that might involve sensory cilia on the surface of the nematocyte. Mechanical stimulation alone is not enough to trigger the sting; it has to be combined with the correct chemical stimulation as well. The clownfish, although it stimulates the anemone's tentacles mechanically, avoids being stung because, chemically speaking, the inert mucus layer is as good as a cloak of invisibility.

An interesting question is whether clownfish are born with the ability to secrete the "invisible" mucus or whether they acquire this early in life. A partial answer is provided by the observation that anemones seem to fall into two distinct classes. There are those like S. haddoni, which a previously isolated A. clarkii can enter immediately; this suggests that the fish's protection to these species of anemone is inborn. Then there are other anemones, such as Gyrostoma hertwigi which the clownfish cannot enter at once, but rather has to go through a prolonged period of acclimatisation, during which the fish gradually touches the anemone more often, while the anemone's sting response to the fish dies down. Anemones in this second group may be more sensitive, able to detect compounds in the clownfish mucus that do not trigger anemones such as S. haddoni. I am not sure what happens during the process of acclimatisation, but I think it is likely that the clownfish is able to decrease the amount of these excitatory compounds that it secretes, perhaps as a direct result of being stung.

. . . . .

FISHES THAT LIVE IN THE MOUTHS OR GILL CAVITIES OF OTHER FISHES

Gudger, E. W.; *Scientific Monthly,* 24:556-558, 1927.

It is a fact well known to ichthyologists that the hagfishes, Myxine and Bdellostoma, and various lamprey eels, all members of the class Cyclostomata or round-mouthed fishes, have parasitic habits. Indeed, the American Museum has an exhibit, designed and the installation supervised by Dr. Bashford Dean, showing a river lamprey (Lampetra fluviatilis) parasitic on a catfish. Sucking fast to its host, this parasite by means of the teeth set inside the circular rim of its mouth, frets away and eats the flesh of its host and drinks its blood. On the other hand, the hagfishes or slime eels, bore their way into the bodies of their hosts, devour the flesh and internal organs, often leaving nothing but "skin and bones."

Still less well known is the fact that a little catfish in South America, bearing the significant name of Stegophilus insidiosus (the "insidious cover-lover") lives in the gill cavities of his huge kinsman Platystoma. At first it was thought that the little siluroids were the young of the big catfish carried in his mouth until hatched and able to care for themselves (a very extensive practice among these fishes). Then it was presumed that they were messmates of their kinsman, living on small organisms drawn into his mouth in his respiration. But now they are known to be parasites living among the gills of their host and rasping with their sharp teeth and spines through the soft tissues of the gills and drinking his blood. Other fishes of the same group are known to practice the same habits.

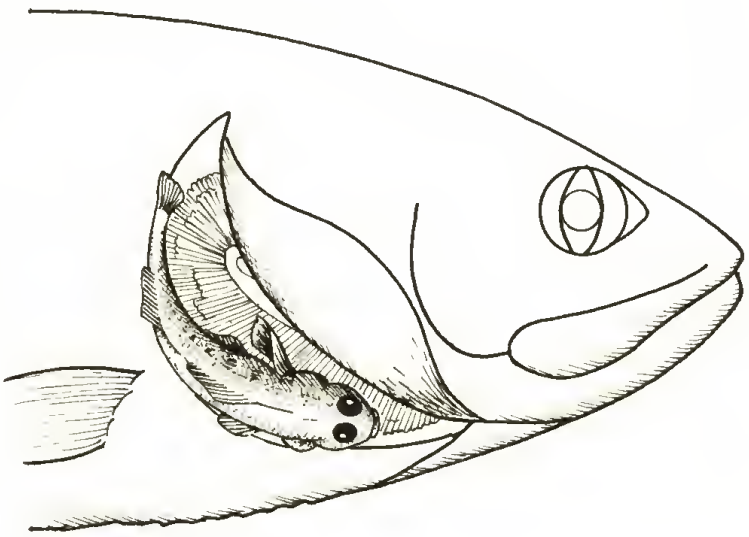
These are cases of true parasitism as the fish lives on the flesh and blood of its host---while the cases now to be considered are cases of commensalism, where the two fishes feed at the same table, or of symbiosis, where the two fishes merely live together ---the weaker for protection by the stronger. The most widespread but relatively little known case of this latter phenomenon, until the publication of my recent article in Natural History is the habit of various members of the family Echeneididae, the sucking fishes of entering the mouths and gill cavities of sharks and rays and of large bony fishes like the swordfish (Histiophorus), the sailfish (Tetrapturus), the sunfish (Mola) and others. This paper aroused considerable interest and various accounts of this habit and several actual specimens of the little fishes have since come in to me. This is a case of commensalism, where the two fishes live in amity---the smaller gaining protection and possibly feeding on the scraps let fall by its protector.

Now, however, we have the still more remarkable case of bony fishes other than Echeneids (sucking fishes) living as commensals or symbionts in the mouth cavities of other Teleostean fishes. The sucking fishes are more or less dependents, but the fishes now to be considered are apparently free-livers. How they gather food is not very clear, but all the known facts follow.

The principal account is from the observations of Dr. Fernando

de Buen, chief of the section of biology in the Instituto Espanol de Oceanografia, Madrid. In checking up recently the bulletin of the Spanish natural history society for titles for our continuation classified card catalogue of ichthyological literature, I chanced upon his interesting illustrated article quoted below. When I wrote Dr. de Buen asking permission to utilize his data and reproduce his figure, he courteously gave permission for the use of the one and sent the original of the other. His account reads as follows:

> While studying the Clupeidae that Professor Lozano possesses in his laboratory at the Madrid Museum; I had the good fortune to find one of them lodging a small Gobius in the gill cavity. The specimen was captured at Melilla (Aug., 1912) during the expedition directed by my father in the north of Africa.
>
> From the position in which it was found and for other reasons which are given later I can not believe that the Gobius caught in the nets and mixed with a profusion of Alosa will introduce itself into the gill chamber of one of these fishes casually. The species identified as Gobius pictus Malm has never been cited off the coasts of Spain nor in Northern Africa, but lives in the depths of the Northern European seas, and one may believe with more foundation that it was transported to the



south during the migrations of the Clupeidae. Moreover the Gobius presents very clear phenomena of adaptation; the first and second dorsals have the first spine growing on the back and this is prevented from being raised by a membrane which fastens it and runs on the same side outside of the median line; this adaptation proves to us that the Gobius has been carried for a long time in the gill chamber of the Alosa. On the other

hand, commensalism between fishes is very rare; but I know of an analogous case (Van Beneden, 1883) where Risso says that he observed at Nice a species of the family Muraenidae lodging in the gill cavity of a Lophius piscatorius L. The Gobius, on account of its small size and the power of using the ventral fins as a sucker (Gobius pictus has this highly developed), possesses qualities very favorable to commensal life.

From the outside of the Alosa nothing abnormal is seen; the gill flap is perfectly adapted and completely hides the fish which it covers. Lifting the flap, the Gobius is seen resting on the shoulder girdle; its head is placed in the broadest part, and its body extends upwards, making place for itself at the end of the gills. The body of the Gobius is compressed and sloping, a transverse section giving approximately a rhomboid figure.

• Fish Behavior and Astronomy

LUNAR INFLUENCE ON THE EAST ANGLIAN HERRING FISHERY

S., G. A.; *Nature,* 135:157-158, 1935.

Fluctuation in the yield from year to year, from month to month, and even from day to day, is one of the outstanding and disconcerting characteristics of all herring fisheries. For thirteen seasons, 1921-1933 inclusive, careful records have been kept of the amount of herrings landed daily at Yarmouth and Lowestoft during the late autumn (October-November) fishery off the East Anglian coast. These figures of daily landings have now been submitted to detailed analysis by the scientific staff of the Ministry of Agriculture and Fisheries, Lowestoft, who find that they reveal a definite monthly rhythm in the catches, the maxima coinciding with the period of full moon.

When the moon is at full in the first week of October its effect is but slight, but the landings gradually increase as the season advances, to culminate in a peak during the week of November full moon. In these circumstances, it is only the middle and later parts of the season which produce good catches. A full moon occurring in the second week of October produces a pronounced peak in the curve of landing, this maximum being followed by one of approximately equal magnitude in the week of November full moon. Full moon in the third week of October produces a still more pronounced peak for this period, but the corresponding November full moon is considerably less effective.

Finally, when the moon is at full during the fourth week of October, it produces the largest peak of all, but the late November moon is accompanied by a rise so slight as to be of little benefit to the fishery.

From these observations it follows that the best prospects for a successful fishery--at any rate from the point of view of production--are found when the October full moon occurs in the second week of the month. When this happens, the combined effects of the October and November moons are greatest, and a period of about five weeks good fishing may be expected. A very late October full moon is the least promising of all, for then only one prominent peak occurs in the fishery. The period of good fishing is therefore very short, and if it should happen that the activities of the fishing fleet be restricted by adverse weather conditions at this time, the chances of a successful fishery in that season are extremely slight.

The occurrence of these peaks in the curves of landings during the weeks of full moon is held to be more than coincidence, and the data certainly appear adequately to support this view. The authors are at a loss, however, to suggest an explanation for this correlation between the yield of the East Anglian herring fishery and a phase of the moon. This task is made all the more difficult by the fact that the same correlation is not exhibited by the herring fisheries in other localities. Either there is no obvious correlation at all between the landings and the moon's phase--as in the Scottish fisheries--or the peaks occur during a different phase of the moon--as at North Shields, where the maxima occur during the 'first quarter'.

It should be emphasised, however, that failure to find an explanation of the influence exerted by the moon on the great East Anglian fishery in no way detracts from the value of the observations which have demonstrated its existence. The authors are therefore to be congratulated upon having fully achieved the two-fold object of their investigations. In a notable advance towards the making of more accurate forecasts of fluctuations in the yield of an important fishery, they have succeeded also in adding materially to the sum of our knowledge concerning lunar periodicity in the behaviour of animals.

EFFECTS OF MOONLIGHT ON FISH

V., G.; *Scientific American,* 9:322, 1854.

In the "Scientific American," page 186, the question is mooted whether the putrefaction of fish and meat exposed to moonlight, is more rapid than at any other time. That moonlight affects fish is not only a traditionary belief, but is a positive fact, which can easily be ascertained by those who have doubts upon the subject. I have known it to be so for more than forty years. I recollect of an instance where a person purchased newly-caught fish at

the fishing station, and threw them floundering into his wagon, without taking the precaution to cover them from the moon's rays of that night. He lived a distance in the country, which required about five hours to reach, and he thought, as the journey was to be made in the cool of the night, all would be well, but he was greatly surprised when he arrived at home at daylight, to find the most of his fish so green and putrid that they had to be thrown away. Why was this? Such an effect would not have been produced upon fish on a moonless night, nor even by exposure to the sun's rays for the same period. All old housekeepers are careful not to expose fresh fish or meat to moonlight. It is also generally believed that it is dangerous to sleep exposed to the moon's rays. These precautions and traditionary opinions had their origin in facts, which I have observed on both land and water. The great thing in mooted questions in natural philosophy, is first to discover and arrange facts; but in accounting for them, there may be a variety of opinions, until some new fact is discovered which settles all the disputed points.

THE MOON AND POISONOUS FISH

Hutchins, D. E.; *Nature,* 90:382, 1912.

Mr. E.G. Bryant, in Nature of November 14, asks a question regarding the effect of moonlight in "turning" fish. I have lived many years in South Africa, and have encountered the same belief, that moonlight will hasten the turning bad of fish; and at one time, when living at Muizenberg, I obtained some experimental proof of the moon's action on fish. It seems curious, at first sight, that moonlight, which has so little effect on meteorological instruments should have this effect on fish. I have thought it probably due to insects or some low form of life which would be abroad, or be stimulated to action, on moonlight nights and not on dark nights.

The action of moonlight in stimulating the rise of sap in trees is widely believed in by practical wood cutters in almost every quarter of the world.

PROBE MYSTERY OF WHY TROPICAL FISH TURN POISONOUS

Anonymous; *Science News Letter,* 60:53, 1951.

Why some tropical fish, usually edible, suddenly become poisonous as food is being probed by scientists.

. . . . .

For people who depend largely on fish for food, the mysteri-

ous sudden poisoning effect is a serious problem. Dr. Bruce Halstead of the School of Tropical and Preventive Medicine and his associates hope that epidemics caused by the unpredictable poison outbreaks can be brought under control, and perhaps forecast, when their studies are completed.

While capable of dealing quick death, fish poisons have been known to produce a disturbance known as "paradoxical sensory disturbance" in which there is apparent reversal of hot and cold sensations. A victim of such poisoning would think a glass of ice water a hot drink.

. . . . .

FISH AND THE AURORA

Anonymous; *Sky and Telescope,* 4:10, August 1945.

Night fishing in a Michigan lake turned from a fizzle into a sportsman's dream a couple of years ago during a brilliant auroral display. The account was carried in Hunting and Fishing, March, 1945, according to the Texas Observers' Bulletin for May. The fishermen could not get a nibble from sunset till after midnight, but when the aurora appeared:

"A magazine could have been read, and the whole lake seemed ablaze from the reflection. At this moment all around us the lake came to life; it actually boiled with fish breaking water and jumping all over, near the boat and as far as we could see.: In 15 minutes one man got his limit, and two others each caught three fine bass. Quietness returned when the aurora weakened, and for another two hours the fishing was as unsuccessful as earlier.

BIOCHEMICAL ANOMALIES

• Coelacanth DNA Content

NUCLEAR DNA CONTENTS OF COELACANTH ERYTHROCYTES

Thomson, K. S., et al; *Nature,* 241:126, 1973.

Among the living fishes, groups of widely different phylogenetic antiquity may be classified by equally diverse sets of characteristics, both morphometric and biochemical. One such character that has been used in the study of the relationships between the major groups is cellular DNA content. It is now well known that, among fishes, modern teleosts have the lowest diploid levels (as low as 0.8 pg) and that elasmobranchs have somewhat intermediate levels (3-15 pg). The Dipnoi have the highest levels among living vertebrates (160-285 pg), and these values are approached most closely by some (but by no means all) Amphibia, such as Necturus and Amphiuma (205 and 192 pg respectively).

In view of this great diversity of nuclear DNA content in fishes, and indeed all vertebrates, it has not always been clear what level might be considered "primitive" and what "advanced". It would therefore be extremely interesting to know the nuclear DNA content of supposedly primitive organisms. We have studied blood samples of a live coelacanth Latimeria chalumnae Smith, captured by a recent American-British-French expedition to the Comore Islands. Cytophotometric measurements of Feulgen-stained erythrocyte nuclei show that the DNA content of Latimeria erythrocyte nuclei is 13.2 pg. (This confirms predictions about histological thin sections of coelacanth tissues.) Coelacanths therefore have diploid DNA levels well above some Amphibia, such as Xenopus (6.3 pg per diploid cell) and in the same range as Rana (153 pg) and elasmobranch fishes such as Squalus acanthias (14.7 pg).

The DNA content in Latimeria was higher than would be expected in the ancestor of all tetrapods and we conclude that in the course of evolution from the original dipnoan-cross-opterygian-amphibian stock, increase in the coelacanth genome has occurred. Thus increased cellular DNA contents have arisen many times, independently, in this whole assemblage.

ANOMALOUS DISTRIBUTION IN TIME AND SPACE

• The Great Tilefish Kill

THE CASE OF THE DISAPPEARING TILEFISH

Sater, Edna N.; *Nature Magazine,* 40:538-539+, 1947.

On March 20, 1882, when the Norwegian bark Sidon docked in Boston with a cargo of sugar from Cardenas, West Indies, the master, Ole Jorgensen, brought this story of a weird encounter with dead fish floating on the surface of the ocean in the North Atlantic.

"The weather was cold and stormy on Tuesday, March 14, when we first sighted them," he told his friends on the pier. "A strong northwest wind was whipping the waves into a frenzy. It was about 1 p.m. when we got close to them. I noted our position. It was latitude 40 degrees and longitude 71 degrees. As far as the eye could reach we were surrounded by dead and dying fish--thousands of them, even millions of them, I guess, floating on the surface of the water. They were big fish. Some of them were three to four feet long. Our speed was six to eight knots an hour. We ploughed through that rough sea for 50 miles, with those big fish coming up on the crest of the waves and slapping against the sides of our vessel."

A reporter from the Boston Daily Advertiser seized upon the strange story and gave it prominence in the columns of that paper's March 21 issue. Down in Washington, D.C. Professor Spencer F. Baird, U.S. Fish Commissioner, received a copy of that story a day or so later. Immediately he began an investigation to determine the extent of the tragedy and to ascertain the species of fish involved. Early reports, while conflicting, had tentatively identified them as codfish.

Because Commissioner Baird was thoroughly familiar with the area in which the dead fish had been seen, he suspected from the first that they were not cod but tilefish--a new species which had been discovered in vast numbers in 1879 in the waters bordering the Gulf Stream, between Hatteras and Nantucket. On March 24 the Commissioner instructed his correspondents in Atlantic seaports to collect all possible information bearing on the subject. Shortly thereafter a specimen of the dead fish was sent to Commissioner Baird, who quickly identified it as a tilefish. Thus the question of identification of the principal species in-

volved in the catastrophe was settled.

The tilefish, rivaling in color the rainbow-hued coral reef fishes of tropical seas, had been described as "the most gorgeously decorated marine fish in northern waters." Seafood connoisseurs of that day said its only rival in flavor was the much-touted pompano.

To Capt. William H. Kirby, of the fishing schooner William V. Hutchins, goes the credit for discovering the tilefish. In May, 1879, while fishing near the hundred-fathom curve south of Nantucket in search of cod and hake, the captain caught several thousand pounds of a "strange and handsomely colored fish." He sent a specimen to the U.S. National Museum, where it was examined and declared to be a new genus and species. The lengthy name of Lopholatilus chamaeleonticeps was tagged to it, meaning a "crested tilus with a head like a chameleon." For popular use, however, by perpetrating a pun on the fourth syllable of the first name, the new species became known as the "tile" fish.

The report on the dead fish floating on the ocean made by the skipper of the Sidon was only the fore-runner of many similar ones to pour into New York and Boston during March and April of 1882. Hard on its heels came stories from at least twelve other vessels relating their encounters with these fish so strangely dead.

. . . . .

Summarizing the material gathered from so many sources, Commissioner Baird and his fishery experts established the extremely conservative estimate of the probable number of dead fish as 1,438,720,000, drifting about in an area 170 miles in length and 25 miles wide. At an average of ten pounds each, which was also conservative because large tilefish may weigh 30 to 40 pounds each, the Fish Commission reported that a total of 14,387,200,000 pounds of edible fish was utterly destroyed--about 288 pounds of fine food fish for each of the 50,000,000 inhabitants of the United States--then!

. . . . .

Wild theories of all sorts were advanced to explain the tilefish catastrophe, with a submarine volcanic eruption, heat, cold, lack of food, and poisonous gases being listed among them. The specimens of dead fish that were caught, however, showed no signs of disease. Neither was there evidence of parasitic infection. Many of them were eaten because they were so fresh and wholesome.

The theory that was most generally advanced by those who studied the subject was that these fish met with a stratum of unusually cold water which paralyzed and rendered them helpless to such an extent that they floated to the surface of the sea dead or in a dying condition.

Other persons were equally firm in their belief that it was not cold but unusually warm water that destroyed the tilefish. Such an idea, however, was soon abandoned if we may judge by this

facetious reference to it in the New York Times of April 22, 1882:

"Theories are still rife as to the reasons for the killing of these fish. The Gulf stream notion, of the fish getting into hot water, not having a leg or a fin to stand on, others are now being ventilated. Said one wise skipper, 'There had been convulsions of nature under the sea. Now, you see, mates, these here loaferlatter lushisses is deep-sea fish. There comes the deuce to pay down below--their bladders gets busted, and up they come like balloons. That's a pint no fish-sharp has studied up yet; don't you see?'"

The "fish-sharps"--aquatic biologists--shook their heads, completely baffled, and there the matter has rested from that day to this. True, the tilefish did reappear ten years later, in 1892, in small numbers. But what caused that terrific mortality and their total disappearance for ten years? Did Mother Nature and Father Neptune conspire to test the effects of an atomic bomb of their own contriving? No one knows. The real cause of that gigantic undersea tragedy is as great a mystery today as it was when it occurred more than six decades ago. The Case of the Disappearing Tilefish may have to be written off as "unsolved." The ocean has succeeded well in cloaking in mystery the comings and goings of its denizens.

• Subterranean Fish

NOTE ON SUBTERRANEAN FISHES IN CALIFORNIA

Chase, A. W.; *American Journal of Science,* 3:7:74, 1874.

In collecting facts about the Artesian wells of the lower Counties, I received the following curious information from Mr. Bard, the agent of the California Petroleum Company at San Buenaventura. He has lately constructed a wharf at Point Hueneme, southeast of San Buenaventura. Wanting water to supply this wharf, he commenced sinking an artesian well on the sea beach, not five feet from high-water mark. At the depth of 143 feet, a strong flow of water was obtained, which spouted forth to a height of thirty feet. It was controlled with a "goose neck" and utilized. One day while the agent was absent, the men around the well noticed fish in the waste water. On his return they called his attention to the fact, and on examination the well was found to be filled with young trout, thousands of them being thrown out at every jet. These trout were all the same size (about two inches long), and perfectly developed. The first examination made was to see whether they had eyes.

These were found perfect. Now there is no stream nearer than the Santa Clara River, several miles distant. Could these fish then have come from its head-waters by some subterranean outlet? There are no trout in the lower portions of the stream. The temperature of the water is the same as that of the wells in this county, viz: 64° Fahr., too warm of course for trout to live long in it.

FISH FROM ARTESIAN WELLS IN THE SAHARA

Duffus, Robert L.; *Scientific American*, 133:110-111, 1925.

A few months ago newspaper correspondents in Algeria thought it worth while to pay cable tolls on reports that "small crabs, fish and shellfish" had been found in water from artesian wells in the Northern Sahara. Some skeptical readers may have passed this by as of a piece with sea serpent fiction. Yet the facts were not only as the dispatches stated, but they have been well known to scientific men for more than sixty years---in short, ever since the French bored their first wells in the desert country south of the Atlas Mountains. They tell the story, not of Algeria alone but of half a continent and of hundreds of thousands of years of geologic change. For those who can understand his language the little Haplochromis desfontainesii (this happens to be the pet name of one of the best known of the fish species found) who pops out of an Algerian well when he is not expected is more eloquent than any political spellbinder.

. . . . .

Underground Fishes. Most of us doubtless think of underground fish as resembling the blind or even eyeless species found in such places as the Mammoth Cave of Kentucky, but the remarkable fact about these fish from artesian wells, whether in the Sahara or elsewhere, is that they are almost always exactly the same as surface fish of the corresponding species. The only reported exception to this rule occurred near San Antonio, Texas, where, according to Professor J.T. Patterson of the University of Texas, a blind catfish was thrown up by an artesian well. But as some surface catfish, like their namesakes, "are nocturnal in habit," living "in crevices, under rocks, stumps and such," this discovery did not prove that Professor Patterson had found a distinct underground species.

The testimony of French scientists who have studied the Sahara fish is overwhelming on this point. In every case the eyes are well formed and the colors are as bright as those of the fish in surface waters. Since every living organism tends to adapt itself to its environment, this can mean but one thing--that the subterranean life in the waters which feed the artesian wells has not been long out of touch with the surface. So, the question arises as to how the fish penetrate to these depths and

how they manage to live there.

To find an answer it is necessary to have in mind the nature and geological history of the Sahara. The hinterland of Algeria extends from the Atlas Mountains (which are geologically a European range cut off by the Mediterranean) over a region larger than France and nearly all desert. According to Dr. Jacques Pellegrin, the leading authority on the fishes of Northern Africa, the Sahara was once much wetter than it is now, and several water courses, now dried up, emptied in the Niger and Lake Tchad. The Arabs have a tradition that the desert, as it now is, was well wooded and watered when they first entered it in the Seventh Century and that it did not dry up until the Thirteenth Century.

A number of fantastic theories have received credence. A Scotchman, Donald McKenzie, proposed in 1875 to cut a twelve-mile channel from the Atlantic Ocean to the coast southeast of the Canary Islands, and so allow the sea, as he believed, to form a vast inland lake which would completely revolutionize the climate of Northern Africa. Another project has been to cut a channel from the Gulf of Gabes, in Eastern Tunis, which would also, it was believed, restore an ancient Saharan sea. Scientists are almost as cautious about these possibilities as they are about the canals on Mars, but they admit that the salt marshes at the base of the Atlas Mountains are survivors of much larger bodies of water.

In certain parts of the desert there are dried-up water courses to which the Arabs give the name of "oued," which means either "river" or " a place where water has been." Two of these, the Oued Mia and the Oued Irgharghar, come together near Tugurt to form the Oued Rir, and it is precisely at this point that the Arab well diggers, and later the French, have been most successful in finding water. The natural conclusion is that the waters of the Sahara, sinking beneath the sand as the climate grew increasingly arid, still follow the old courses. The fish life, once very numerous, must have found its quarters more and more constricted and been compelled to exist, not only in surface pools and the few remaining streams, but in many cases in the underground waters.

An Honorable and Dangerous Profession. It is unlikely that these waters, in the Sahara, are real underground rivers. They probably seep through the loose sand for the most part, forming pools and channels only when they encounter rocky caves and basins. The fish are, therefore, unable to move about freely over great distances. The fact that this must be the case, and the additional fact that they have still remained identical with species as remote as the Nile and the River Jordan is another indication that the drying up of the Sahara and the destruction of the old river systems occurred in comparatively recent times.

Although there are traces of ancient Roman wells in the Sahara, the greater part of the well digging has been done by the Arabs and the French. The French, with their modern machinery, are responsible for such blossoming as the desert has done in the part sixty years. The romance of these wells, both

ancient and modern, is a story by itself. One of the most honorable as well as the most dangerous of Arab professions in days gone by was that of well digger. Some of the wells went down as far as 160 feet, piercing a layer of hard pan, under which water was generally found.

The first French observers were inclined to think that the fish, crabs and mollusks brought up from these wells spent their whole lives underground. More recently they have been of the opinion that in some way this subterranean life maintained communication with the surface. Dr. Edouard Blanc noticed that fish from the wells were generally reduced to skin and bone when found, and believed that the minute algae, small crustaceans and other organic debris found in the water was not enough to keep them alive indefinitely. He, therefore, reached the conclusion that the wells were breeding places, where the young fish grew up, safe from the dangers of the open pools.

In the older wells pools and channels of considerable size are probably formed, so that there is plenty of room for fish of small size to develop unmolested. This would account for the fact that most of the fish taken from the wells are very small. As they grew larger, according to Dr. Blanc's hypothesis, they probably found their way to the surface pools by way of connecting channels.

This theory is satisfactory in most instances, although it leaves some puzzling facts unexplained. For example, it does not clear up the presence of fish in wells which have no surface pools, nor does it account for their existence in water too warm for long survival. The small trout thrown up at Point Hueneme, California, came from water at a temperature of sixty degrees, in which they could not possibly have lived for any length of time.

But the greater mystery is the identity of the species found in the Sahara wells with those not only in Central African rivers and lakes, but even in Palestine. A certain fish belonging to the Chromids--so called because of their vivid coloring--is found in the Sea of Galilee as well as in Algeria. Its distinguishing trait, in environments now completely dissimilar, is its habit of carrying its eggs in its mouth while they are hatching out. Other species found in the Algerian wells are exactly the same, not only as the fish of Palestine, but as those found in the waters of the Congo, the Niger, the Senegal, the Nile, Lake Tchad and Lake Tanganyika.

The Saharan Fish a Traveler. This brings us to some peculiar facts about the distribution of Northern and Central African fish, and perhaps to a solution of the problem. Out of 101 known species in the Nile forty-six are found in the Niger, the Senegal and the Gambia, and most of these latter are also found in Lake Tchad. One-fourth of the species found in Lake Victoria are also found in the nile, and four-fifths of those found in Lake Rudolph are found in the Nile. Putting it another way, of sixty-six species of fish found in Lake Tchad or its tributaries forty-five are found in the Nile, thirty-eight in the Senegal, twenty-four in the Congo and seven in the Zambezi. There is only one

explanation of such facts as these. They indicate that within a comparatively recent time fish were able to pass from Lake Tchad to and from the rivers named.

Where the Saharan fish are also found in the waters of the Lake Tchad basin, of the Nile and of Palestine a similar conclusion naturally follows. Not long ago, as geologists measure time, the Chromids of the Sahara were able to make their way by an all-fresh-water route to Palestine.

The reason for this present isolation is to be found in gigantic changes in the earth's structure occurring along a line running, roughly, from Lake Nyasa, through Lakes Albert and Rudolph to the Red Sea, the Dead Sea, and the Sea of Galilee. This is what is known as "The Great Rift Valley."

The Rift valley was formed by a double line of faults, or cracks in the earth's crust, which allowed a block in the middle to drop. This great change probably began in the Eocene period and continued during the ages that followed. Its effect, as in the plateau region which once occupied the present site of Lake Victoria Nyanza, was to "behead" a number of rivers which had once flowed out. There is good geological and zoological authority for the opinion that before this catastrophe there was a connection between the rivers of Palestine and those of Central Africa.

At that time, very likely, the waters of the Mediterranean did not come as far east as they do at present, and a Palestinian river may easily have flowed into the Nile. Another theory is that a river flowed from Palestine, along the trough of what is now the Red Sea, and formed part of the Central African river system. This supposition is supported by the fact that some species are found both in the River Jordan and in the Central African rivers, but not in the lower Nile, below the cataracts.

Lake Tchad, Dr. Pellegrin thinks, was a sort of clearing house for the fish of the Nile, the Niger, the Senegal, and probably also of the region which is now the Sahara. It is a little difficult to form an accurate picture of Northern Africa in this period. Yet we can certainly think of it as much wetter than the present-day Africa, and with a much more complicated and profuse array of rivers and lakes.

THE BLIND FISHES OF PERSIA

Thines, G.; *Nature,* 271:305, 1978.

The note published in News and Views 264, 113:1976) on the blind cave fishes of Persia mentioned the discovery of the two cavernicolous forms Noemachilus smithi (Cobitidae) and Iranocypris typholops (Cyprinidae) in the same well-like outlet. This is of great interest for the zoogeographical and etho-ecological interpretation of the possible mechanisms which led epigean fishes to colonise subterranean waters and evolve underground into

isolated homogeneous populations composed entirely of eyeless and depigmented individuals. These phenomena of regressive evolution have been well studied on several populations inhabiting caves (Thines L'evolution regressive des Poissons cavernicoles et abyssaux, Masson, Paris, 1969), but little, if any, specific attention has been given to the case of cavernicolous forms found in wells and artificial outlets. It is therefore fortunate that Mr. Smith was able to discover this new Noemachilus form in association with the previously discovered Iranocypris (Brunn & Kaiser Dan. Scient. Invest. Iran, Munksgaard, Copenhagen, 1943). However, the coexistence of two distinct regressed species in the same well has been observed before. In 1962, Mees found two different blind and depigmented Teleosts, Milyeringa veritas (Gobiidae) and Anommatophasma candidum (Synbranchidae) in two wells depending on the same subterranean system and located about 10 miles apart, near Yardee Creek Station, North West Cape, Australia (Mees J.R. Soc. West. Austr. 45, (I), 24; 1962).

The coexistence of distinct cavernicolous forms in a same biotype other than a cave, raises the question of the exact nature and extent of their habitat. Underground waters can be traced with sufficient precision in many karstic systems (the system of sinks, underground caverns and streams found in limestone areas), whereas water sheets feeding wells or pumping sites do not lend themselves to similar descriptions. In addition, such phreatic water tables cannot be explored by speleologists, so that the only topographical data are those furnished by geological surveys. Thus, biospeleologists have had to limit themselves to inference as far as the habitat is concerned. The only sound hypothesis about regressed fishes found in wells and other artificial water outlets is that these forms normally live in the phreatic sheets and are occasionally attracted to zones exposed to light by organic remnants of animal or human origin. The fact that fishes of the same species are sometimes observed in wells separated by a great distance is, in my opinion, the strongest argument in favour of this view. Mees reported the presence of the free-swimming Milyeringa in only two of the three wells he visited (Milyering, Kudmurra and Tandabiddi, the last halfway between and some 10 miles from the other two), whereas the eel-like Anommatophasma was found in all three sites. The cases of Iranocypris typhlops and of Noemachilus smithi seem similar to that of Milyeringa veritas. The ecological conditions in which these fishes live are moreover similar to those reported as early as 1904 by Goeldi for Phreatobius cisternarum (Goeldi Rept. 6th Int. Zool. Congr. Bern, 549; 1904), a very peculiar catfish belonging to the family Trichomycteridae which he found in a cistern of Marajo Island in the mouth of the Amazon and which was rediscovered recently (Delamare, personal communication). I have collected specimens of the blind Clariidae Uegitglanis zammaranoi in the canalisations of the Iscia-Baidoa pumping station (Somaliland) some 60 miles from the well of Uegit, where the first specimens were discovered in 1923 by Gianferrari (Atti. Soc. Ital. Sc. Nat. Mus. Civ. Stor. Nat. Milano, 62; 1923). To the present day, this distance is by far the greatest to have been

reported for the geographical extension of a cavernicolous fish not dwelling in a cave (Thines Rev. Zool. Bot. Afric. 57, 117; 1958). The case of Noemachilus smithi can thus be considered as a typical instance of an already well-known phenomenon, whose meaning for biospeleological research is clear.

• Marine Fish in Fresh Waters

FRESH-WATER SHARKS OF NICARAGUA

U., H.; *Scientific Monthly,* 57:187–188, 1943.

Harvard University ichthyologists are jubilant over the arrival of the only known fresh-water shark in the world. He is Carcharinus nicaraguensis, caught after a series of tribulations, in the tepid waters of Lake Nicaragua in the Latin American republic. Although Harvard has not yet solved the problem of how the shark shook off his salt water habits, perhaps many thousands of years ago, and took to the fresh water of the lake, it has been proved, for the first time, that the creature came from the Atlantic rather the Pacific side of Central America.

The weazened remains of the Nicaragua shark rest in the basement of the University Museum, an unappetizing sight for the layman but a choice morsel for those scientists who devote their lives to the study of rare fish.

Harvard's efforts to obtain a specimen of Carcharinus nicaraguensis started a year ago. Through the cooperation of the Carnegie Institution, which had archaeologists working near the lake, a shark was snagged out of its waters. With due ceremony, it was placed aboard a schooner, in a barrel of salt, for the first leg of shipment to Cambridge. A submarine without a sense of humour spotted the schooner and sank it. Carcharinus nicaraguensis, quite irretrievable, thus returned to the salty habitat of its ancestors and sank to the bottom with the ship.

Dr. Thomas Barbour, director of the University Museum, and Dr. Henry B. Bigelow, professor of zoology, who finally identified "C.N." as of Atlantic origin, renewed their efforts to get a specimen to Cambridge. They had heard that Sweden, many years ago, obtained one, and they knew that a specimen which was on show in Washington in the gay '90's had disappeared. Unless it was being kept under cover Carcharinus nicaraguensis was not represented in this country, dead or alive.

An appeal was addressed to President Anastasio Somoza of Nicaragua. An ardent angler, the President went fishing several months ago and dragged not only one but three sharks out of

the lake. Photographs of the event were taken and sent to Cambridge. Harvard fish experts took new heart. But facilities for shipping the sharks here were lacking, because of the war, and the President was left with nothing but pictures to show for his prowess.

Finally, last February, an American army engineer, Major Charles M. Duke, joined the Harvard hunt and caught the shark now in the University Museum. The creature, when taken out of the water, was about five feet long. The fresh-water sharks of the lake have been rumored to reach the length of ten feet. They have the nasty habits of their salt water relatives and are considered dangerous even by the Nicaraguan natives.

In the '70's, these fresh-water sharks were discovered. Theodore Gill, famous Smithsonian ichthyologist, obtained one and gave it the family name, in honor of Nicaragua.

However, according to Dr. Bigelow of Harvard's museum, this variety of shark was never compared with salt-water sharks to determine whether its original family habitat was the Atlantic or Pacific Ocean.

"It is apparently of Atlantic origin, said Dr. Bigelow. When it got into the lake we can not say. It might have been centuries ago. How it got in is more of a mystery. A river, going down to the sea from the lake, has such steep descents and swift rapids that I don't believe a shark could swim up to the lake. The presence of these fish may be due to convulsions of nature, such as a volcanic eruption, which aeons ago changed the conformation of the land.

"Amazingly enough, the fresh-water shark has changed very little from the salt-water shark of the Atlantic coast of Nicaragua during its residence in the lake. The shape of the mouth and the relative position of the fins show the only differences."

UNRECOGNIZED SPECIES

• Unidentified Giant Fish

BIG FISH

Anonymous; *Zoologist,* 2:6:2812, 1871.

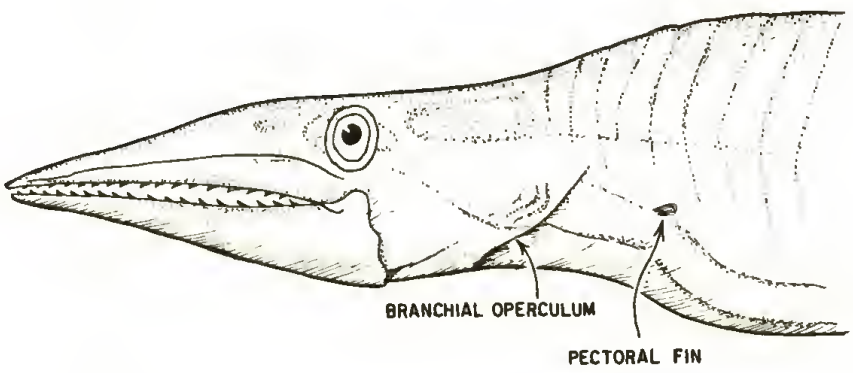
During the last few days fishes of an enormous size have been observed in the lake of Wallenstadt, one of which is described by those who have seen it as being six feet long and proportionately broad. A number of persons have disappeared in the course of the season whilst bathing, and Dr. Simon and others think that they were seized by these enormous fishes by the leg, and dragged under water. No one can at present give any accurate description of these monsters of the deep, but the subject is attracting much attention among scientific men in this country, and we hope soon to place before our readers a detailed account of the result of the investigations which are now being made into this, at present, mysterious subject.

GIANT LEPTOCEPHALUS

Anonymous; *Nature,* 230:278-279, 1971.

Eel larvae are strikingly dissimilar to the adults of the same species, for they are laterally flattened, shaped like an elongate willow leaf, and quite transparent. It is now a matter of history that at one time the larvae were recognized as a distinct group of fishes and given the generic name Leptocephalus, although this name has lingered on as a general label for this type of elongate fish larva. A catch-all name of this kind has been very useful because few of the known larvae are certainly referable to their adult forms, and it is now known that a number of other fish orders have leptocephalus larvae.

In January 1930, the Danish Dana Expedition captured a leptocephalus on the Agulhas Bank, south of Africa, which was 184 cm long. This specimen has earned a place in the mythology of marine biology, for although A. V. Taning, who first reported its capture, merely claimed it to be the largest larval form within the animal kingdom, later authors seized on its length and, comparing



Head of Leptocephalus giganteus

it with the 90 mm maximum of the common eel (Anguilla anguilla) larva, produced a figure of 30 m for the estimated length of the adult!

Recent re-examination of this Dana specimen by Jorgen G. Neilsen and Verner Larsen (Vidensk, Medd. Dansk Naturh. Foren., 133, 149; 1970) shows that this larval putative giant sea serpent is morphologically very similar to a form described in 1959 as Leptocephalus giganteus from off New Zealand. The New Zealand specimen was relatively modest in size (893 mm), but careful comparison shows it to be otherwise identical with the larger specimen (although this is now in poor condition and measures only 1,300 mm having dried out while on loan from Copenhagen). Leptocephalus gigantius have also been found offshore off Florida and D. G. Smith (Copeia, 1970) has recently demonstrated that this form is the larva of a notacanthiform fish.

Neilson and Larsen concur with this view and suggest that all the giant leptocephalus larvae belong, not to the order of eels, but to the notacanthiform fishes. They also describe four other large Dana leptocephalus larvae which range in length from 83 to 450 mm, and were captured off the west coast of North Africa in March 1930.

These show differences from the L. giganteus form, and Nielsen and Larsen suggest that they are probably referable to the same genus or generic group of notacanthiform fish.

The notacanthiforms are bathypelagic and benthic fishes of the deep seas. They include the halosaurs, which are known to develop from leptocephalus larvae, and the spiny eels or notacanths. In general, little is known of their biology and it is not yet possible to suggest which species develops from the giant leptocephalus, if indeed the adult form is yet known to science.

Chapter 6

ARTHROPODS (INSECTS, CRUSTACEANS, ETC.)

Chapter Contents

ANOMALOUS DISTRIBUTION IN TIME AND SPACE

MORPHOLOGICAL PHENOMENA

• Insect Luminescence

LUMINOUS TERMITE HILLS

Knab, Frederick; *Science,* 30:574-575, 1909.

Many years ago, while in the Amazon region, I found that the termite hills, which are there such a conspicuous feature in many localities, are luminous at night. My first acquaintance with this phenomenon was made in the vicinity of Santarem, Brazil, upon a nocturnal walk through the forest. In the company of some natives I was following one of the narrow paths which lead to the scattered habitations. The darkness beneath the canopy of foliage was absolute and progress was only possible by the "feel" of the ground under foot. Suddenly there appeared through the foliage a luminous area composed of innumerable points of phosphorescent light which appeared to shift and fuse into each other, thus forming more brilliant patches which were constantly resolving themselves and again appearing. This light suggested the steady diffused glow of the familiar "fox fire" rather than the more brilliant display of the fire-flies, yet the slow and confused movements which seemed to pervade the whole luminous zone were strongly suggestive of insects. Upon my expression of surprise the natives replied laconically, "cupim," the native name for termite.

The luminous area was indeed one of the large termite hills which are scattered through those parts of the forest not subject to inundation. These termite hills rise from the ground in an irregular conical mass to a height of from five or six feet to perhaps ten or twelve. They are constructed of clay and are exceedingly hard. The mounds are perfectly bare of vegetation and on that account have a characteristic appearance of newness. Afterwards I frequently saw these luminous termite hills and they added in no small degree to the mystery and charm of the tropical nights. I remember one display of particular splendor, seen when visiting at a house which commanded a view over a large clearing. Numbers of termite hills were scattered over the clearing, and at night, when these all glowed and scintillated upon the black forest background, the spectacle was one never to be forgotten.

Unfortunately I took it for granted that such a conspicuous phenomenon must be well known to naturalists and so did not investigate it. Since then I have searched the available literature

on termites and on luminous insects have questioned entomologists and botanists in the vain hope of obtaining information on this subject. The phenomenon appears to have remained unknown to naturalists. The only references to it that I have been able to find are a brief mention in Herbert H. Smith's "Brazil, the Amazons and the Coast," p. 139 (1879), and my own allusion to it in Entomological News, Vol. 6, p.15 (1895). Are the termites themselves luminous or is the phosphorescence due to some fungoid peculiar to the termite hills? Certain it is that the mounds are all phosphorescent. Smith says: "The phosphorescence is in the hills themselves, not, so far as I know, in the insects"; yet, he does not appear to have investigated this question and his statement is merely an opinion. The fact that no luminous neuropteroid insects are known argues against the theory that it is the termites themselves that emit the light, yet observations on nocturnal insects in the tropics, particularly forest insects, are so rare that such a property might easily have escaped notice. Should the light be caused by a fungus it must be one that is peculiar to the termite mounds. In the latter case, however, one would suppose that when, by the clearing of the land, the nests are exposed to the direct rays of the tropical sun the fungus would be killed; but the mounds continue luminous even in the older clearings where they have been exposed to the sun for years.

During my visit to Central America in 1905 I looked for termite nests in the hope of obtaining some data on this subject. However, I saw no termite hills like those so common in the Amazonian forests. The nests of Eutermes, the common form in Central America, which are built on trees and constructed of woody particles, gave entirely negative results. On one occasion I broke open one of these nests at nightfall to see if the termites within were luminous, but they showed no trace of phosphorescence.

THE LUMINOSITY OF TERMITES

Branner, J. C.; *Science,* 31:24-25, 1910.

In Science of October 22, 1909, XXX., 574-575, Mr. Frederick Knab points out that the mounds made by certain Brazilian termites, or possibly the termites themselves are luminous.

Although I have seen many thousands of the mounds made by termites in all parts of Brazil, I do not remember every having observed this luminosity. A specimen of the nest materials was lately sent me by a Brazilian friend from the vicinity of Queluz, in the state of Minas Geraes. This material shows no signs of luminosity at present, though it does not follow, of course, that it never was luminous.

The following note which I translate from "Viagem ao redor do Brazil," 1875-1878, pelo Dr. Joao Severiano da Fonseca, Rio de Janeiro, 1880, page 353, is much more to the point:

> On the head waters of Rio Verde (state of Matto Grosso, Brazil) we saw one night a surprising sight. One of the white ants' nests seemed to be covered with little lights, and these tiny stars made it look like a miniature tower brilliantly illuminated. It was near the tent of Captain Craveiro, the commander of the troops, and that gentleman invited us to share his surprise and pleasure. When the nest was struck with a stick the miniature lights went out as if by enchantment, but only to reappear again little by little, beginning where the blows had been weakest.

I know but one other reference to this phenomenon in the works of Brazilian travelers, and that is the following brief note given in Castelnau's "Expedition dans les parties centrales de l'Amerique du Sud, Histoire du Voyage," Paris, 1850, Vol. II., p. 103. In describing the travels in the neighborhood of the city of Goyaz the author says:

> On the night of the fifteenth in the vicinity of the Agoa Limpa estate we noticed a luminous mass in the middle of the campo that aroused our curiosity greatly. On approaching it we found it to be a termites' mound from which shone a great number of small points of light [petits foyers lumineux]. This phenomenon is produced by the presence of an immense number of small phosphorescent larvae which withdrew into the galleries they had built when one tried to capture them.

The fact that I have lived and traveled in Brazil for ten years without ever having seen this luminosity at all; the surprise of Dr. Severiano da Fonseca at seeing a single instance in Matto Grosso; and the note by Castelnau, who traveled through tropical South America for four years, all lead me to surmise that this luminosity is probably confined to some particular species, or possibly to some special occasions or conditions of termite life.

PHOSPHORESCENT TEXAS EARTHWORMS

Ray, Cyrus N.; *Science,* 101:271-272, 1945.

When going down damp woods earth paths at night the writer occasionally has noticed that bright spots of phosphorescence lighted up in the disturbed soil surface after he passed, glowed for a while and then ceased. He also noticed that by scraping a foot over the area the phosphorescent effect could be repeated an indefinite number of times.

On examination of the path with a flashlight nothing was visible which could have caused the illumination. The tentative conclusion then was that the illumination came from soil bacteria or some other form of invisible microorganism which thus reacted to disturbance. However, the phosphorescence glowed in pea-sized

balls, which when raked about remained as units in new positions and glowed brightly. While these balls of light sometimes were thickly placed there was no uniform distribution through the soil such as one might expect if bacteria were the cause. This phosphorescent effect usually had only been noticed during the cool weather of fall or early winter. On the night of December 6, 1944, the closing of the writer's garage door activated some of this phosphorescence when the door scraped the soil surface. The idea then occurred to remove the spots of light and examine them under bright indoor electric lights to see if the cause could be located. The area of phosphorescence was on the edge of a gravel driveway which was covered with a thin film of leaf mold composed of the small leaves of the mesquite tree.

The phosphorescent substance shone in the thin film of soil above the gravel and some of these units of light were removed and examined under strong illumination and the source of each was found to be a small round, slender, pink earthworm varying from one half inch to one and one fourth inches in length and about as large in diameter as the small end of a hardwood toothpick. These earthworms have the same color, general appearance and movements as those of the much larger worms usually known as angle or earthworms. The worms evidently lie coiled on or very near the soil surface and are covered by a very thin film of leaf mold, and when this is moved the worms glow with white light. The night the first specimens were found the temperature was standing at 36 degrees above (Fahrenheit), and when the writer put the tip of his finger on the largest one, which measured one and one fourth inches in length, he thought that it emitted a slightly perceptible amount of heat, and this apparent effect has since been observed. The writer's sense of touch perhaps is above the average and he does not believe that this observation is subjective and he would like to see the matter checked with some delicate heat-recording apparatus. On the next night at 11:50 many more phosphorescent worms were found and the temperature then stood at 38 degrees above (Fahrenheit). On the following morning at 7:30 the temperature stood at 32 degrees above (Fahrenheit) and a heavy frost covered everything. The soil was not frozen, however, and the worms still remained on or just beneath the surface and their lights shone brighly when disturbed.

On bringing the worms indoors into a temperature of 60 degrees above (Fahrenheit) it was found to be difficult to get any phosphorescent reaction out of them, but on taking them back outside into the nearly freezing temperature and dumping them onto the cold earth they again glowed when touched.

To determine whether the phosphorescence was in the body of the worm or in the slimy secretion around it one of the largest specimens was washed in water and the light then did seem weak, but when the body of the worm was crushed against a stone and rubbed over its surface the whole area glowed brightly for some seconds. Of what use the phosphorescence is to the worm the writer is uncertain, but the observations thus far made might indicate that the light-producing mechanism may generate enough

heat as well as light to enable the animal to remain active during periods of low temperature which inhibit the acitivity of most lower organisms.

BIOLUMINESCENCE

Anonymous; *Nature*, 162:768, 1948.

In connexion with Dr. V.B. Wigglesworth's article on bioluminescence in Nature of September 11, p. 423, Mr. K.B. Williamson, c/o Manson House, 26 Portland Place, London, W. 1, has recalled observations made by him while he was malaria research officer in Malaya some years ago. A dytiseid beetle which fell about three feet off the laboratory bench on to the floor of the old Malaria Bureau, Kuala Lumpur, displayed a unique type of bioluminescence. The beetle appeared to emit bright flashes of white light, three or four at a time, from its eyes. The flourescent type of bioluminescence is invisible even in weak light; but these flashes were conspicuous in competition with the light from a fairly bright electric bulb on the laboratory bench. The groups of flashes were repeated several times within half an hour, when the beetle was again dropped from the same height; but never afterwards, when the dropping was repeated during the subsequent months it was kept alive. In sharp contrast to another Dytiscus, it failed to capture any mosquito larvae or pupae during these months, and it not improbably died of starvation; and may have been blinded by destruction of retinal pigment and damage to the cells producing in Mr. Williamson tentatively suggests that the flashes from the beetle's eyes were caused by reversal of the normal process of vision, energy in the form of light having been emitted instead of being absorbed by retinal pigment, as the result of intermittent outwardly directed nervous impulses, due to shock.

CIRCULAR POLARIZATION OBSERVED IN BIOLUMINESCENCE

Wynberg, Hans, et al; *Nature*, 286:641-642, 1980.

Abstract. While investigating circular polarization in luminescence, and having found it in chemiluminescence, we have studied bioluminescence because it is such a widespread and dramatic natural phenomenon. We report here that left and right lanterns of live larvae of the fireflies, Photuris lucicrescens and Photuris versicolor, emit circularly polarized light of opposite sense.

• Gills in Insects

ON GILLS IN WINGED INSECTS

Anonymous; *English Mechanic*, 20:263, 1874.

Birds are all, without exception, developed in the air; even the aquatic birds lay their eggs and hatch them on land. The young bird comes out of the egg into the air and the bird always breathes only air, although it may occasionally go under water for food or shelter. It is the same with the majority of insects; but not with all. For there are, in various orders of insects, certain groups, in which the flying females deposit their eggs in water; the young animals are developed under water, and only in their last stage of transformation leave the water; gnats, dayflies, and some dragonflies (Agrion) are well known examples. In the case of gnats (Mucken), the young larvae, though living in the water, breathe air, and therefore they come to the surface, just as seals and walruses do their whole life. The larvae of dayflies and dragonflies, on the other hand, breathe water; and so, in the course of their individual life, they pass from water breathing to air breathing, like frogs and salamanders, which have on this account been called "double breathing" animals (Amphibia dipnoa).

Their breathing organs are lateral appendages of the skin, into which the air canals (tracheae) enter from the interior of the body, and ramify. They are thus gills, i.e., organs which are bathed in water, and take up oxygen from it for oxidation of the blood; but they are distinguished from all other gills, e.g., those of fishes, crabs, &c., in that they are not streamed through by a greater quantity of blood than other parts of the body, which might take up the oxygen direct, and thereafter distribute it, but contain numerous air-canals through which the required oxygen is continually renewed--first to the system of air-canals (tracheal system) penetrating the whole body, and thereby to the blood in every part of the body. Hence the name tracheal gills. These organs, in the larvae of dragonflies, are situated at the posterior end of the body; in those of dayflies, at the sides of the posterior part; and in a family nearly related to both, the Perlaria, under the thorax, (in some cases, in the same part as that where the perfect insect has openings of the air canals for breathing--stigmata; in others, in a part removed from this).

. . . .

• Flying Crustaceans

FLYING CRUSTACEA

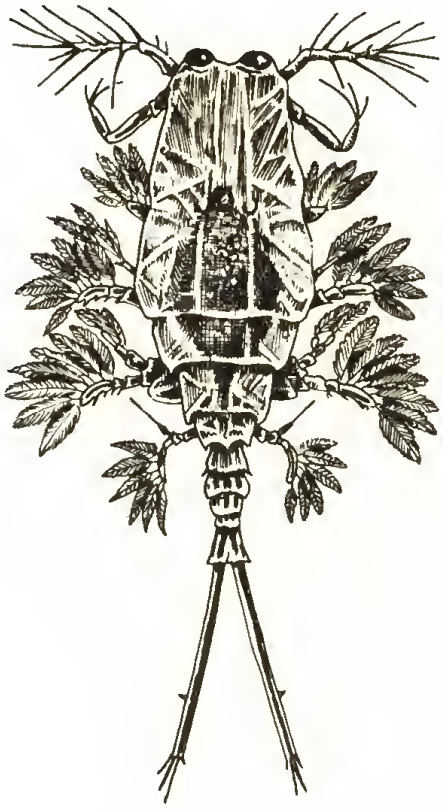
Anonymous; *Scientific American*, 72:347, 1895.

Aside from birds and insects, there have, up to the present, been known several animals that, owing to a special arrangement of such or such a part of the body, are capable of flying, or at least of maintaining themselves in the air for a greater or less length of time. Such are the bats and the flying squirrels, among mammals, and the exocetes among fishes. Such beings belong to the vertebrate branch of the animal kingdom, but analogous facts have been very recently discovered among the invertebrates. The insects are no longer the sole arthropods that have the faculty of cleaving space and transporting themselves by aerial way. The same faculty has been observed in a crustacean--a very small one, which neither in size nor form recalls the crustaceans known to everybody, the lobster or the crab, for example.

The following are the facts in all their simplicity; Dr. Ostrooumoff, a very distinguished scientist, director of the biological station of Sebastopol, made an excursion in a boat last summer along the coast of the Crimea. One morning, while the sea was very calm and the sky of an azure such as one sees only in southern countries, he observed clouds of small beings hovering like flies above the tranquil surface of the water. Approaching slightly, our naturalist was, as were his son and the boy who accompanied him, enabled to observe the phenomenon at leisure, and the following was what was ascertained: Each of these small animals in the first place took a proper position on the surface of the water, as if seeking to gather strength, and then made a leap and described in the air a long and gentle curve in order to fall back again into the liquid element. To collect a certain number of these beings and examine them with the lens was the work of a moment, and what was not the astonishment of the Russian naturalist when he recognized that the captive animals were crustaceans that are quite common in the Black Sea, and that belong to the genus and species Pontellina Mediterranea (order Copepodeae).

If we examine certain small crustacea under the microscope, we shall be much surprised at their odd aspect. We shall mention a few examples. The Calocalanus pavo, which is quite common in the Mediterranean, exhibits a transparent body, and, at the extremity of the abdomen, carries eight golden yellow symmetrically arranged feathers.

Another crustacean of the order Copepodeae has analogous but much more developed appendages; it is the copilia vitrea, an odd little animal, every leg of whose transparent body is provided with



A flying crustacean

a rich fan of microscopic feathers of a brick red.

Finally, a species scarcely distinct from that observed by Mr. Ostrooumoff is the Pontellina plumata. Examined at an amplification of from 40 to 50 diameters, it presents a multitude of silken hairs that ornament its legs as well as the extremity of its abdomen, vulgarly called the "tail" in the crustaceans. These hairs, often arranged as feathers, contrast by their bright orange color with the blue body of the animal and its transparent limbs. There is no doubt that these numerous hairs singularly facilitate the aerial excursions of the crustacean and uphold it after it has once risen into the air by a leap. We might multiply the descriptions of the species, but it seems to us that what we have said will suffice to convince our readers.

. . . .

• Electric Insects?

ELECTRIC INSECTS

Anonymous; *English Mechanic,* 33:425, 1881.

Certain insects are known which give something like an electric shock to anyone touching them. The Redivivus genatus or "wheelbug" of the West Indies, is one so described by Kirby and Spence. Two other examples have been lately recorded in the Entomological Society, by Mr. Yarrell. A letter from Lady de Grey, of Groby, referred to a shock caused by a beetle, one of the Elateridae, and extending from the hand to the elbow. The other case is that of a large hairy lepidopterous caterpillar in South America. Captain Blakeney, on touching, had an electric shock so strong, that he lost the use of his arm for a long time, and his life was even considered in danger.

• Mystery of Blue Blood

"BLUE" BLOOD OF LOBSTERS STILL BIOLOGICAL MYSTERY

Anonymous; *Science News Letter,* 65:254, 1954.

True "Blue Bloods" in the animal kingdom are the lobsters, crayfish, crabs and their relatives. A blue copper pigment in their blood serves the same function as iron pigment in human beings.

The copper pigment, hemocyanin, serves as a vehicle for carrying oxygen in blue-blooded animals just as the iron pigment, hemoglobin, does in red-blooded creatures, the study shows.

Whereas the red pigment is capable of carrying oxygen in the ratio of up to 20 cubic centimeters per 100 cubic centimeters of blood, the blue pigment can carry only up to three cubic centimeters of oxygen per 100 cubic centimeters of blood.

Hemocyanin is thought to have evolved from certain copper enzymes, which all red blooded animals including humans, have in their bodies. Why certain animals developed blue oxygen bearing pigments while others developed red ones is still a mystery.

• Calcium-Based Nerves: Parallel Evolution?

CALCIUM-DRIVEN NERVES: CURIOUSER AND CURIOUSER

Anonymous; *New Scientist,* 77:363, 1978.

The discovery last year of an insect nerve that uses calcium ions to conduct its impulses was an exception which proves the rule, according to an extension of the original work just published by Ian Orchard and his team at Birmingham University.

Nearly all nerves, from insects through to humans, use changes in permeability of sodium ions to pass their messages. Orchard's discovery last year made biologists rethink their complacent acceptance of this dogma, for he found a calcium-dependent nerve in the stick insect (Carausius morosus). These unusual nerves belong to a class that, upon excitation, release certain hormones.

Because of its potassium-rich diet, this insect has a body fluid of such strange composition that no normal nerve could work in it. It was this which prompted Orchard to look at the workings of some stick insects' nerve cells and eventually led to the discovery of the calcium nerve impulse.

Now he and Professor Laurie Finlayson of the Department of Zoology have tested other nerves of the stick insect that lead from a stretch receptor to the central nervous system. These nerves, it turns out, use sodium and potassium like most other nerves known. How do they manage in these body fluids usually associated with calcium?

The answer seems to be by a barrier of cells, a "blood-brain barrier", which regulates the ionic content of the actual fluid bathing the nerves. This barrier, already well documented in other plant-eating insects such as the locust and cockroach, is essential: strip it off, and the nerves simply don't work. Why the neurosecretory nerves Orchard looked at last year have evolved a calcium conduction system instead of utilising this type of barrier remains, for the time being, a mystery (Journal of Comparative Physiology, vol. 122, p.45).

• Crustaceans with More Than Six Legs

A TWELVE-LEGGED SEA SPIDER

Anonymous; *Science,* 78:6, 1933.

A sea spider with six pairs of legs, instead of the customary four pairs, was captured by Sir Douglas Mawson in the Antarctic during the British, Australian and New Zealand Expedition. It was found at a depth of two hundred fathoms.

The discovery was announced at a meeting of the Royal Society by Dr. W.T. Calman, keeper of Zoology at the British Museum, who is in charge of the work of examining Sir Douglas Mawson's specimens.

Dodecolopoda mawsoni, as the new species of animal is named, was exhibited at the Royal Society. The interest aroused among biologists is gauged by the fact that there are some 400 known species of marine arthropidae, or insects, with the standard four pairs of legs, corresponding to the three pairs of legs of land insects. The only previous exceptions to this apparent law of nature had five pairs of legs. The first of the ten-legged "freaks" was found off South Georgia by the American scientist, Eights, approximately a century ago. Though a few other have since been found the existence of twelve-legged pycnogonidae had not been suspected.

Dr. E.W. MacBride, professor of zoology at the Imperial College of Science, suggested that the extra number of legs "may have been added as an afterthought," in the same kind of way as the North American starfish, which originally had five rays and still drops from its larva in five-rayed form, now has further rays that grow in between the primary five.

Both the ten-legged and twelve-legged arthropidae have been found in the Antarctic zone, suggesting that this environment may call for additional legs, possibly to facilitate food capture.

• Remarkable Mimicry in Arthropods

MIMICRY IN NEOTROPICAL BUTTERFLIES

Papageorgis, Christine; *American Scientist,* 63:522–532, 1975.

Since Henry W. Bates suggested in 1862 that the similarity in

the patterns of unrelated butterfly species was produced by the evolution in a palatable species of a pattern mimicking that of an unpalatable species, a great deal of research has been directed toward the phenomenon of mimicry. Jane Brower's studies of predator behavior demonstrated that predators do associate pattern with taste and are fooled by mimetic species. She also studied the relationship between distastefulness of the model and the relative frequency of Batesian mimics it could support Zoogeographic studies showing that models and mimics have parallel distributions were conducted by Carpenter. Sheppard's genetic studies have shown that the evolution of mimetic patterns is a slow process involving the accumulation of many mutations, each producing small phenotypic changes.

Through these and many other studies the validity of Bates's suggestion has been tested and accepted. Fritz Muller (1879) extended the concept of mimicry by suggesting that two distasteful species might also benefit by displaying the same pattern. Studies of Mullerian mimicry, like those of Batesian, have focused on the extraordinarily similar patterns of different species, the evolution of the patterns, and the effectiveness of this protection strategy.

As striking as the pattern convergence of different butterfly species is the polymorphism within a mimetic species. Papilio dardanus has many morphs, each of the Batesian mimic of a different model species. This phenomenon has been attributed to frequency-dependent selection. Heliconius melpomene and H. erato are both distasteful species with many forms. Turner has shown that, except for very small areas of hybridization, these are geographic races and similar races of the two species have parallel geographic distributions. Though these particular instances of divergence within mimetic species have been explained without conflicting with Bates's orginal scheme, there are more general inconsistencies that have not been dealt with.

The logical extension of Muller's theory that unpalatable butterfly species improve their fitness by mimicking other unpalatable species is that there should be, in any one location, one type of wing coloration which serves as the warning signal of all distasteful species. Such a situation does not exist. In tropical Africa, Asia, and South America any one rain-forest locality is characterized by the presence of several mimicry complexes, each bearing a very different warning coloration.

The inconsistency of the actual situation with the predictions of theory is further magnified by pattern divergence of closely related species. Emsley has classified the genus Heliconius into several groups of closely allied species. Species of the same groups often bear different warning colorations, while those of different groups have converged in pattern. There are also examples of distasteful species that are polymorphic, each morph belonging to a different mimicry complex. If it is of advantage for two species to display the same appearance, why should one distasteful species require a predator to learn two lessons? In order to find the explanation for these apparent contradictions, I made a study of the mimicry rings of three rain-forest locations

in Peru in 1972 and 1973. [Details of this extensive study are omitted.]

All species of these mimicry rings feed on nectar, and, conversely, nearly all flower-feeding butterflies of the forest are members of mimicry complexes. (Papageorgis, diss. 1974). Flower-feeding butterflies, because of the nature of the substrate on which they feed, cannot match their background as effectively as species that feed against ground or bark. The strategy of distastefulness, or imitation of the pattern of a distasteful species, seems to have evolved as an alternative.

The patterns these unpalatable butterflies, or their mimics, bear must have only the quality of being easily recognized to be effective warning patterns. They do not have to attract predator attention but, on the contrary, can be cryptic in flight, thus giving a butterfly pursued by an uninformed or forgetful predator the second chance of escaping into the foliage. It is the changing background of lighting conditions on this foliage that requires the change in warning pattern resulting in the divergence of closely related distasteful or mimetic species not predicted by mimicry theory alone.

AGGRESSIVE CHEMICAL MIMICRY BY A BOLAS SPIDER

Eberhard, William G.; *Science*, 198:1173–1174, 1977.

Abstract. Mature female Mastophora sp. spiders attract prey with a volatile substance which apparently mimics the female sex attraction pheromone of the fall armyworm Spodoptera frugiperda (Lepidoptera). The rate of prey capture is similar to that of a conventional orb weaver of comparable body size.

"WOLF-IN-SHEEP'S-CLOTHING" STRATEGY OF A PREDACEOUS INSECT LARVA

Eisner, Thomas, et al; *Science*, 199:790–793, 1978.

Abstract. The larva of the green lacewing Chrysopa slossonae lives in colonies of the wooly alder aphid Prociphilus tesselatus upon which it feeds. It disguises itself as its prey by plucking some of the waxy "wool" from the bodies of the aphids and applying this material to its own back. The investiture protects it from assault by the ants that ordinarily "shepherd" the aphids. Larvae artifically denuded are seized by the ants and removed from the aphid colonies. A larva requires on the average less than 20 minutes to coat itself with wax. A hungry denuded larva gives

the coating procedure about the same behavioral priority as feeding.

PINE CONES, OR COCOONS?

Weir, Jas., Jr.; *Scientific American*, 70:43, 1894.

It is a well established fact that certain animals imitate the forms, colors and even the actions of other animals for the purpose of protection. Some animals carry this imitative instinct into the construction of their nests, burrows or cocoons. In no instance is this faculty carried to such an extent as in the cocoon of the pine tree moth. The larva of this moth spins its cocoon in the pine tree only, and builds it in exact imitation of the young pine cone or bud. This imitation is wonderfully like the genuine cone, and often deceives observers, unless they are exceptionally critical or are looking for the cocoons. I have carefully followed this moth through its metamorphoses from larva to imago. The female resembles the female of the common vaporer moth, and belongs to a kindred family, with habits, however, markedly different.

. . . .

Its favorite food is couch or dog grass (Triticum repens) and kindred grasses, and it does not begin feeding until late in the evening, thus avoiding the attacks of birds. It hides during the day among the closely matted roots of the grass. About the last week in August it abandons its usual feeding grounds, and resorts to pine trees, where it feeds on the pine needles for two or three days. It then begins the construction of its cocoon. When this is about two-thirds completed, it protrudes its head and front pairs of legs and crawls about, dragging the cocoon with it, in search of small bits of twigs and pieces of bark. This it does at night, for this little creature rarely moves for any purpose during daylight. It never entirely leaves the cocoon, but extends its head and body as far as possible in its search for proper materials. The bits of twigs and bark are arranged in rows around the cocoon, and are always parallel with its axis. When this outside decoration is finished it proceeds to a terminal twig and suspends its cocoon at the base of a bunch of needles, exactly where, in the course of nature, a cone would make its appearance.

. . . .

CONVERGENT EVOLUTION BETWEEN PHYLA . . . . .

Berger, Joel, and Kaster, Jerry; *Evolution,* 33:511-513, 1979.

Many cases of potential mimicry are reported simply because the species in question are quite similar in external appearance. However, neither data nor testable hypotheses are advanced to determine if mimicry actually exists. We have observed morphological similarity in the structure of the protective body cases of caddisfly larvae (Helicopsyche borealis: Insecta; Trichoptera) and aquatic snails (Physa integra: Gastropoda).

The resemblance of body coverings between larvae and snails appears analogous to Batesian mimicry whereby the apparent model (i.e., the snail) might be undesirable, but because of a protective body covering rather than a noxious taste. A possible mimic (i.e., the caddisfly larvae) could then enhance its survival probability, if less predation resulted (because of its resemblance to the model) and if a predator could not discriminate between the two species.

The protective cases of caddisfly larvae and aquatic snails are similar in coloration and characterized by a prominent whorl. Originally, H. borealis was described as a snail based on the case only, and, although early taxonomists placed them in the same family (Denning, pers. comm.), fundamental differences exist in shell construction. The cases of caddisfly larvae are formed by cementing sand grains together with excretions from silk glands. In contrast, the shells of aquatic snails are formed by calcium carbonate excretions from the mantle. Since these species occur in sympatry, we tentatively hypothesized that caddisfly larvae mimic the external morphology of aquatic snails. In nature, fishes are a known predator of caddisfly larvae. However, further systematic study of the specific circumstances regarding their predation is necessary.

. . . .

EFFECTIVENESS IN NATURE OF THE SO-CALLED PROTECTIVE ADAPTATIONS . . . . .

McAtee, W. L.; *Smithsonian Miscellaneous Collections,* 85:1-201, no. 7, 1931.

Summary. The hypotheses about protective and warning colors and mimicry are part of the Natural Selection group of theories. These coloration phenomena and other protective adaptations are supposed to have been developed and perpetuated by the selective value they had in shielding their possessors from attack by predators.

Preceding sections of this discussion call attention to the evidence that one group of predators after another is known either to prey habitually upon "specially protected" groups, or to be so largely guided in choice of food by availability as practically to

ignore protective adaptations.

The former is admitted to be true of dragonflies, robber flies, mantids, predacious locustids and Hemiptera, parasitic insects, and of spiders, while the latter is stated to be characteristic of the aquatic immature forms of mayflies, stoneflies, caddisflies, and two-winged flies, and of fishes. Data cited throughout the main body of the present paper show a high degree of indiscriminancy also on the part of amphibians and reptiles.

In fact this general indiscriminancy on the part of predators is so evident that even ardent advocates of the selection theories have been impressed by it and one of them, G.A.K. Marshall, in a paper on the "Bionomics of South African Insects" says:

> If the view advocated by many, that birds cannot be reckoned among the principal enemies of butterflies in the imago state, be true, then I consider that we may practically abandon the whole theory of mimicry as at present applied to the Acraeinae and Danainae of South Africa at all events, for from what I have observed of these insects, I am convinced that their warning coloration cannot have reference to either Mantises, Asilidae, or lizards, which are practically the only other enemies that can be taken into account....That they [birds] have been the chief, if not the only agents in the production of mimicry whether Batesian or Mullerian I have little doubt.

In other words selectionists practically rest their case on the reactions of birds to protective adaptations. The principal object of the present paper has been to show what those reactions are so far as nearctic birds are concerned, and there is no reason to suspect that the results are otherwise than typical for birds of the world.

The most outstanding feature of the records of the animal food of nearctic birds undoubtedly is the marvellous distribution of them through the phyla, orders, and subordinate systematic groups. Within size limits, animals of practically every kind accessible to birds are preyed upon, and as we consider the records for group after group a tendency for the number of captures to be in proportion to the abundance of the animals concerned is unmistakable. Availability undoubtedly is the chief factor involved in the choice of food, and predation therefore tends to be in proportion to population.

Considering bird predation alone this principle leads to a high degree of indiscriminancy in attack upon the whole kingdom of animal life. The combined attack of birds plus all other predators still more closely approaches complete indiscriminancy. In other words there is utilization of animals of practically every kind for food approximately in proportion to their numbers. This means that predation takes place much the same as if there were no such thing as protective adaptations. And this is only another way of saying that the phenomena classed by theorists as protective adaptations have little or no effectiveness.

Natural Selection theories assume discrimination in the choice of prey. The principle of proportional predation so obvious from

the data contained in this paper vitiates those theories for it denotes indiscrimination, the very antithesis of selection.

Finally so far as the types of adaptations discussed in this paper are concerned the influence of such factors as disease and climatic factors, the last the most important of all in reducing animal populations, is completely indiscriminate.

The total mortality of animal groups is known normally to be in strict proportion to their numbers, i.e., a pair of the new generation remains, to replace a pair of the old and it is apparent elimination of all but that pair is very largely due to agencies indiscriminate in their action. There would seem, therefore, to be no discriminative eliminating forces of sufficient strength to bear the very great burden put upon them by natural selection.

• Some Remarkable Adaptations

MARVELS OF ADAPTATION

Macbeth, Norman; *Darwin Retried,* Gambit, Inc., Boston, 1971, pp. 70–72.

Third, many animals behave in incredibly complicated and mysterious ways. The life course of the swallowtail butterfly is a familiar example (though few laymen know the remarkable details, such as the total dissolution and reconstitution of the organs and even of the cells), but other insects are equally striking. These are marvels, beyond any doubt; but there is no compelling reason to regard them as adaptations. Each is a tour de force by a virtuoso, but the virtuoso seems to be exercising his own fantasy rather than adapting himself to mundane conditions in a utilitarian way.

The books are full of examples of this virtuoso work, which is especially common among insects. I will set out one case at length to show how many refinements there can be and how the whole performance shows a master hand.

> In early summer the small wasplike Eumenes amedei of northern Africa and southern Europe emerges from the pupal state as an elegant insect with yellow and black bands. Soon after mating, the female prepares a house in which her young can develop and sufficient food can be stored. She chooses an exposed and sunny situation on a rock or wall, and builds a circular fence of small stones and mortar, the mortar being made from dry flinty dust mixed with her own saliva. The stones are chosen with care, flint being preferred to limestone, and

the fragments selected are all much the same size. Her choice of the most polished quartz fragments suggests (if we are anthropomorphic) that she is not indifferent to the esthetic effect of her handiwork. As the wall grows higher, the builder slopes it toward the center and so makes a dome which, when finished, is about the size of a small cherry. A hole is left at the top, and on this is built a funneled mouthpiece of cement.

The next task is to collect the food supply for the future grub. This consists of small caterpillars about half an inch long, palish green, and covered with white hairs. These caterpillars are partially paralyzed by the sting of the Eumenes and are unable to make any violent effort to escape. They are stored on the floor of the cell. Since they remain alive, they keep fresh until the grub is ready to eat them; if they were killed outright, their flesh would soon dry up or rot. When the cell is stocked, a single egg is laid in each house, and the mouthpiece at the top of the cell is closed with a cement plug, into which a pebble is set.

The egg is not laid upon or among the caterpillars, as in many allied species. These caterpillars are only partially paralyzed, and can still move their claws and champ their jaws. Should one of them feel the nibblings of the tiny grub, it might writhe about and injure the grub. Both the egg and the grub must be protected, and to this end the egg is suspended by a tiny thread of silk fastened to the roof. The caterpillars may wriggle and writhe, but they cannot come near it.

When the grub emerges from the egg, it devours its egg shell, then spins for itself a tiny silken ribbon-sheath in which it is enfolded tail-uppermost and with head hanging down. In this retreat it is suspended above the pile of living food. It can lower itself far enough to nibble at the caterpillars. If they stir too violently, it can withdraw into its silken sheath, wait until the commotion has subsided, then descend again to its meal. As the grub grows in size and strength, it becomes bolder; the silken retreat is no longer required; it can venture down and live at its ease among the remains of its food.

The stone cells are not all stored with the same wealth of caterpillars. Some contain five and some ten. The young females, larger than the males, need twice as much food. But note that the cells are stocked before the eggs are laid, and that biologists generally believe that the sex is already determined when an egg is laid. How does the Eumenes know the future sex of her eggs? How is it that she never makes a mistake?

• Miracles of Metamorphosis

FACTS AT VARIANCE WITH THE THEORY OF ORGANIC EVOLUTION

Watson, E. L. Grant; *Victoria Institute, Journal of the Transactions*, 70:1-21, 1938.

Much as 1 would like to give further examples of these fascinating behaviour-patterns and life-histories, time will not allow me to do so, and 1 will turn to a different aspect of my subject. 1 will ask you to consider the behaviour of the caterpillar at the time when it changes into a pupa, and the events which then occur. The metamorphosis which takes place in the life cycle of insects, and especially that complete series of transformations within the life history of the lepidoptera, has been taken as a significant expression of the transformative processes of life, and it is here that we may find most clearly marked indications, which may lead us to a better understanding of the formative forces which govern the development of living things. The essential differences in form, size and habit which separate the early phases of the larva from the perfect insect cannot fail to capture the attention of any observer, and to evoke the question: How can the transformations from larva to pupa, to imago, be reconciled with the concept of continuous modification by innumerable, slow variations, or with the concept of uninterrupted evolution by gradual functional changes; and further, how can the phenomenon of histolysis in the chrysalis, by which most of the organs are reduced to an amorphous emulsion, preparatory to the coming metamorphosis, be brought about by purely mechanistic, physico-chemical reactions? Is there not here revealed a testimony, which declares that neither the changes in the larva nor the mysterious solution of the tissues in the chrysalis lead up to, or in any obvious way anticipate, the future morphology of the perfect insect; and is not the conclusion unavoidable that this testimony reveals the existence of an ideal, proper to and working within (and perhaps without) the organism in question? This ideal or final cause being the determining factor which governs the transformations.

The process of such transformations can be observed in any butterfly or moth. 1 should like, if time allowed, to give a full description of the life history of Papilio Machaon, the English swallow-tail butterfly, with which I have good opportunity of making myself familiar, but as time is limited 1 will confine my description to the most significant period of that history. 1 will ask you to consider whether this behaviour-pattern is more likely to have come about as the result of chance mutations or variations, or as the result of an innate and directive tendency

governing the life of the species. That you may judge the better, I will describe the process in some detail.

After thirty days from the emergence from the egg, the caterpillar is fully fed, and is ready for the change into the pupa stage. This readiness for change is announced by a restlessness and a desire to walk, which fulfils the purpose of distributing the individual larvae over wide areas, far from the place where the parent insect deposited the eggs.

The first act is to spin on a reed-stem a firm mat, on which to fix the hind claspers. In an upright position, with the hind claspers fixed on the mat, the larva spins the semi-circular band which is to hold the pupa in an upright position. From side to side the head moves, while the fore-feet guide and fasten the thread as far down the supporting stem as they can reach. When this task is completed, the caterpillar is circled round the back by a strong silk cord. It now rests, and during this period the body becomes noticeably smaller, and towards the later part of the time all the claspers are loosed but the last pair; and the creature leans on the band of silk in a shape which is already suggestive of the pupa.

At the appointed time, usually after about fifty hours of quiescence, rhythmical movements are to be observed. These swell from the posterior to the anterior and, becoming at last sufficiently violent to break the thin larval skin, which splits down the back, while a green, tender body seems definitely to push itself through the widening gap, and at the same time the skin, as though pulled back by some invisible instrument, slips farther and farther towards the tail. It passes the silk cord, which one would expect to entangle it, and, by the most extraordinary dexterity of wriggling, the now naked pupa works the skin down to the region of the hind claspers. As a penultimate act, it releases its hold on the silk mat, draws up the tail and lifts clear of the skin, pushing it aside, and finally fastens again on the mat, making, as a seal of its accomplishment, a few quick turns to secure its hold.

The empty skin falls, and the pupa occupies the place of the larva, but it has not yet assumed its pupal form. The posterior end is much rounder than it will soon become, and the part where the eyes and the head are to be is still snub and soft. This condition changes within twenty minutes, and the chrysalis takes its final shape, and the outer integument hardens.

I want to draw particular attention to the following fact: the shape and position of the organs of the butterfly which is to be are at this stage already stamped on the pupa. These marks are on the outside, and there is nothing yet formed inside to correspond with them. This is a significant fact, and one which, when its significance is grasped, will modify the accepted idea that development takes place always and only from a centre outwards. Invisible forces outside the insect have stamped upon it the shape corresponding to that final-cause which is inherent in its being. I want to stress the idea that this final-cause, which I describe as inherent in its being, is not necessarily contained within its material body. At this stage, when the larva turns into the pupa,

the governing ideal declares itself. Although there is within the creature nothing but the old body of the larva which is in process of breaking down, there is on the outside of the pupa the pattern of the perfect insect, with wings, legs, antennae, etc., which are later to be occupied by the as yet unformed organs. This pattern is waiting to be filled by organs not yet made but already determined.

The changes which go on within are not less wonderful than those which have been visible from the outside. A breaking down of tissues is taking place. Cells which are comparable to white blood-corpuscles are generated in large numbers at this time, and these devour most of the organs which have functioned in the caterpillar, reducing these to a kind of non-cellular mush. These changes remain, even in their physical aspect, much of a mystery. but it is maintained that the tissues, which are reduced by the phagocytes, comprise the hypodermic cells of the first four segments, the breathing tubes, the muscles, the fatty bodies and the peripheral nerves. At the time that this change is taking place, the cells of the middle intestine assemble into a central mass, and later a new generation of tissue is formed, partly from this central intestinal magma and partly from the proliferation of special corpuscles called image-bearing discs. Thus it is that the newly formed portions seem to have no direct filiation with the destroyed parts of the larval organism. The creature has in fact died, in so far as it has lost its form, its organs and its habits, and now is experiencing a new orientation towards a quite different form, which is to find expression in a different mode of life.

In this process of metamorphosis we are, I believe, in the presence of the working of a concrete, creative idea upon plastic material. What we have witnessed is the working of a centralising and directive force, which determines the chemical and physical reactions of the organic medium. This principle, which makes itself so clearly manifest in the above instance, is--and there can be no doubt about this--the principle which determines the development of all life. Similar, though less patent metamorphoses occur in all embryological development. When we recognise this fact, the physical forms and the outward behaviour of animals can no longer be considered as constituting their whole being; we become aware of the presence of invisible forces as yet ungauged and unknown, which lie behind the visible phenomena of life, and we realise that Nature expresses invisible values in visible forms; then it follows that many biological and psychological theories, in so far as they try to explain the phenomena of their sciences entirely in terms of physical matter, are trying to do what is impossible; and are in the same position a mathematician would be in if he attempted to make an equation which involved three arbitrary constants passing through five arbitrary points. More terms have got to be put into the ideas before they can fit the facts. This simile has been used by another writer, but it is such a good one that I do not hesitate to repeat it. (pp. 7-10)

MIRACLES OF METAMORPHOSIS

Dewar, Douglas; *The Transformist Illusion,* DeHoff Publications, Murfreesboro, 1957, pp. 213-214.

The metamorphosis of a caterpillar into a butterfly is a process which cannot have developed gradually step by step, generation after generation. Before undergoing metamorphosis the caterpillar has to manufacture a protective covering. This wonderful feat and those that follow are graphically described by Mr. E.L. Grant Watson in his "Enigmas of Natural History." As I have not watched the process I take the liberty of quoting his account of the transformations effected after the caterpillar has entered the pupal stage(p.72): "The shape and position of the organs of the butterfly which is to be, are already stamped on the pupa. It should be particularly noticed that these marks are on the outside and that there is nothing yet formed inside to correspond with them. This is a significant fact and one which, when its significance is grasped, will modify the accepted idea that development takes place always, and only from a centre outwards. The governing idea has at this stage at last declared itself, and although there is within the creature, at this stage, nothing but a green watery pulp, all the places in its organism which are later to be occupied by legs, wings, antennae, etc., are now definitely marked. They are waiting to be filled by the organs, not yet made, but already determined. Later a new generation of tissue is formed, partly from this central intestinal magma, and partly from the proliferation of special corpuscles called image-bearing discs. Thus it is that the newly-formed portions seem to have no direct filiation with the destroyed parts of the larval organism. The creature has in fact died, in so far as it has lost its form, its organs, and its habits, and now, in a manner which cannot be described as anything but mysterious, is experiencing a new orientation towards a quite different form, which is to find its expression in quite a different mode of life."

Does any one really believe that the ancestors of butterflies were as adults just masses of pulp enveloped in cases, having no means of procuring external nourishment? If not, it is for the evolutionist to explain how the process of metamorphosis became intercalated in the life-history of the caterpillar. The caterpillar has biting jaws by means of which it rapidly demolishes the leaves on which it subsists; on the other hand, the mouth-parts of the butterfly are entirely different: the butterfly is incapable of biting anything, its first maxillae are grooved and greatly elongated, often coiled; pressed together they form a tube through which nectar from a flower is drawn into the stomach, which is a suction pump. The transformation of mouth parts of the caterpillar type to those of the butterfly type involves a period during which the possessor of the mouth has either to go without food or subsist on its own tissues. Thus it is impossible that such transformation was effected in the past by a gradual process extending over a period of thousands of

years. Unless the transformation was effected suddenly and miraculously butterflies must from their origin always have had suctorial mouthparts.

• How Cells Determine Their Locations

MOTH CELLS FIND OUT WHERE THEY ARE

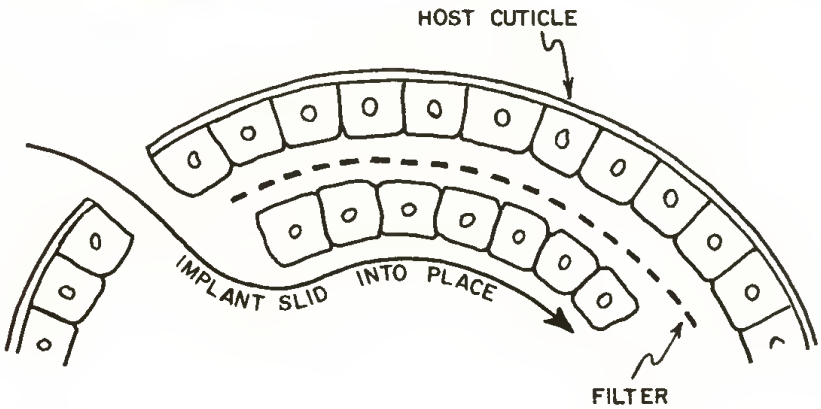
Anonymous; *New Scientist,* 85:317, 1980.

Although we have known for some time that cells can interpret their position in a developing tissue, the way they do it remains a mystery. Scientists have found it hard to design conclusive experiments, and speculation has revolved around two major mechanisms. Either there is some "gradient" of information throughout a developing tissue which cells can interpret, or else cells can "talk" directly to one another. The first mechanisms might work by cells "reading" a gradient of some diffusible substance, while the second would involve cell surface properties of some kind.

New evidence for the "diffusible substance" hypothesis has appeared in the January 1980 issue of Developmental Biology. G. Bhaskaran and H. Roller work at the Institute of Developmental Biology at the Texas A & M University and have been studying the orientation of scales on the outer covering of the abdomen (abdominal cuticle) of the saw moth Galleria.

. . . . .

To find out if this induction involved a diffusible substance or was mediated by contact between cells, Bhaskaran and Roller devised in ingenious experiment. They transplanted a piece of synthetic filter between the implant itself and the host tissue (see Figure). Even when pore sizes were too small for parts of cells



to squeeze through, the implant seemed to influence cell growth. The authors were still to play their trump card, however. They implanted pieces of agar between host and graft for a few days then removed them. They then implanted the agar into a new host tissue without an accompanying graft. The host tissue was induced to form posterior cells!

This evidence suggests that the agar had assimulated some diffusible substance which was responsible for the change in pattern. Whether this type of mechanism will be found in other organisms remains to be seen. Indeed, experiments of this kind have a knack of being re-interpreted when further data are obtained, so we must wait to see if the developmental biology fraternity accept these results at face value.

• Possible Inheritance of Acquired Characters

FURTHER EVIDENCE FOR THE LAMARCKIAN THEORY OF THE CAUSE OF EVOLUTION

MacBride, E. W.; *Nature,* 143:205-206, 1939.

The death of Prof. William McDougall marks the end of an epoch in the history of biology. Prof. McDougall attained world-wide fame as a brilliant experimental psychologist, but his equally important work on the heritability of acquired habit has been, I think, for the most part overlooked. He experimented on rats, and he showed that when rats were forced by unpleasant experience to acquire a habit, they produced offspring which acquired the habit more readily than did their parents until, at the end of nine years, they acquired the habit at the first contact with the unpleasant experience.

The late Miss Dorothy Sladden (whose work has periodically been noted in Nature) also, like McDougall, was engaged in experiments on the heritability of acquired habit, though she knew nothing of McDougall's work. When she died her work, like that of McDougall, was complete, but the final and conclusive part of it was embodied in jottings in note-books and columns of figures. My former colleague, Mr. H.M. Hewer, has edited these notes and published his results, which were noted in Nature.

As Miss Sladden's experiments were of a much simpler type than those of McDougall, a very brief account of them may be given here. She used the stick-insect Carausius morosus, a native of South India, which in Great Britain normally feeds on privet. This insect was subjected to periods of semi-starvation, each lasting two days, when it was offered ivy for food which it

detested. At the end of each period it was revived by a leaf of pivet. Each of these ordeals was called a 'presentaton'. The overwhelming majority of the insects would not accept ivy until the fifth presentation, and some even held out until the tenth presentation. These insects are partheno-genetic and when their eggs were collected and bred from, it was found that they accepted ivy at an earlier presentation than did their parents until at the end of five years they took ivy at the first opportunity. Thus Miss Sladden and Prof. McDougall between them have adduced evidence in support of the hypothesis that the acquired habits of one generation are passed on to the next.

. . . .

• Apparent Vestigial Structures in Insects

VESTIGIAL STRUCTURES

Dewar, Douglas; *The Transformist Illusion,* DeHoff Publications, Murfreesboro, 1957, pp. 172-173.

5. Blind Cave dwellers. Numerous animals, including tailed amphibia, fish, insects and spiders, that inhabit dark caverns are blind. Some of these have traces of eyes; in others the eyes are degenerate. It is doubtful whether imperfect eyes in such cases are the result of evolution, because:

(a) In some species of beetle the males have fairly large eyes, while these organs are much reduced or lacking in the females.

(b) The amount of degeneration of the eye varies greatly in individuals of the same species. Thus, the spider, Troglophyphantes, may be quite blind in one cave and have small eyes in another. McIndoo found in one cavern a species of spider of which the individuals exhibited all degrees of eye degeneration, the number of eyes varying from 8 to 0. Racovitza examined 59 individuals of the Isopod, Trichoniscus gachassini, living in a cave near Algiers and found that 2 near the entrance had small but well-developed eyes, 36 had very degenerate eyes and 21 had no traces of them.

(c) Some animals inhabiting dark caves have perfectly-developed eyes.

(d) The eyes of the blind amphibian, Proteus, normally do not reach the darkened skin, but its larvae, if exposed to red light, develop normal eyes.

(e) Sexton and Winge, when experimenting on the shrimp, Gammarus chevreuxi, which has black eyes, produced suddenly in broad daylight, forms having red eyes, those with white and

those having degenerate eyes. Similar results were obtained in the case of the fruit fly, Drosophila melanogaster.

(f) From abnormal eyeless forms thus produced, individuals having normal eyes may be bred.

(g) Payne raised 69 generations of Drosophila melanogaster in complete darkness without effecting any change in the eyes or in the desire to seek light.

It would thus seem, that, in some circumstances, the gene complex believed to control the development of the eyes does not work normally. Should blind mutants appear in nature these would soon be wiped out, but, if this happened in a dark cave, there is no apparent reason why blind individuals should not survive to produce offspring. (pp. 168-173)

• Rapid Variation in Drosophila

DROSOPHILA'S CONTINUING EVOLUTION

Anonymous; *New Scientist*, 20:791, 1963.

Observations on the relative frequencies of different gene arrangements, involving inversions on the third chromosomes of individual fruit flies from populations of Drosophila pseudobscura and D. persimilis near Mather in the Sierra Nevada of California, have shown that considerable changes have been taking place there from year to year, especially in D. pseudobscura. If it were not that these genetic changes are visible only under the microscope, this would be as spectacular as the industrial melanism which produces a higher proportion of dark-coloured moths (T. Dobzhansky, Evolution, Vol. 17, p.333).

Several types of gene arrangement were found in larvae of flies collected in the wild, and their frequency varied considerably from year to year. That designated PP, for example, was not found at all in 1945, appeared in 1946, rose to 10 per cent in 1947, then fell to 4-5 per cent in 1949, and recovered to 9 per cent in 1962. Other arrangements, CH, ST and AR, showed different patterns of change.

Unfortunately, there has been no success in the search for external factors correlated with the gene changes. After the 1945-1951 surveys, it seemed plausible to suggest that ST increased in frequency, and that AR decreased, during summers following winters with more than average rain or snow; and that drought caused a high AR level with low ST. Subsequent years have not borne this out. Correlations with frequencies in other

species have not been significant, and there is no sign of any dependence on man's activities. The arrangement PP has increased in step with increased use of DDT in near-by agricultural areas, but flies with this genetic configuration have no greater resistance to DDT than do others.

The moral emerging from the observations is that biologists, although they admit that the living world is continuously evolving, tend to assume that any species or population they study will be essentially the same today as it was yesterday. This is now seen to be an erroneous assumption, since between 1945 and 1962--a mere instant of the evolutionary time scale--the Drosophila populations have been genetically reconstituted, and though there are no external differences, there may well have been physiological, and hence ecological, changes in them.

WHY 600 SPECIES OF HAWAIIAN DROSOPHILA? THE SEXUAL SELECTION HYPOTHESIS

Ringo, John M.; *Evolution,* 31:694-696, 1977.

Nearly twenty years ago Zimmerman (1958) pointed out an evolutionary anomaly: there is a startling number of Drosophila species endemic to Hawaii, a number disproportionate to the area, geological age, and seeming ecological diversity of the archipelago. Over 600 species of Drosophila, or roughly 25% of the genus, reside on six islands whose total area is but 16,400 km^2 and whose ages are only 0.7-5.6 million years. Why, then, are there so many Hawaiian Drosophila species? Three major hypotheses have been suggested, which maintain that either natural selection, genetic drift, or sexual selection is the primary cause of speciation. I argue here that the least known of these--Spieth's sexual selection hypothesis--is the most parsimonious and warrants further scientific investigation.

A key issue, which any speciation hypothesis should take into account, is the dramatic morphological and behavioral variety in Hawaiian Drosophila, compared with continental species. Stalk eyes, spoon-shaped tarsi, long and prominent bristles on various body parts, and striking patterns of wing maculation are some of the structural modifications of this group of flies. Territoriality, stereotyped fighting, social dominance, leks (mating arenas), intricate dance-like communal displays, and courtship rituals whose contortions tax the imagination are some of the behaviors that have evolved in Hawaiian Drosophila in the past few million years. Even within species groups there is considerable diversity in behavior and morphology. Many of the morphological differences are directly related to courtship and mating, and contribute to strong sexual dimorphism, so that females of closely related species are similar while the males differ dramatically. By contrast, continental species are not very sexually dimorphic; closely related species are often morphologically indistinguishable and

behaviorally very similar.

The full details of any instance of speciation must surely be complex, yet speciation depends on one common process: genetic differentiation of populations by evolutionary forces to the extent that reproductive isolating barriers (RIBs) develop. Unusually rapid speciation, exemplified by Hawaiian Drosophila, may depend on the existence or increased relative magnitude of an evolutionary force that differentiates populations.

. . . .

• Possible Evidence of Macroevolution

MACROEVOLUTION AND THE FOSSIL RECORD OF ARACHNIDA

Petrunkevitch, Alexander; *American Scientist,* 40:99–122, 1952.

The ideas developed in the present paper do not solve the problems of the origin of the diverse fundamental types of behavior patterns in spiders, nor of the methods of their inheritance. Attempts have been made in the past to explain the development of the more complicated types of web construction as a result of evolution from a simple, uniform type. These attempts seem to me to be futile, in fact impossible in the present state of our knowledge. As yet, there is no evidence of direct inheritance of "mnemic engrams" independent of selection. There is also no evidence of the telescoping of time, comparable to that taking place in the process of learning by successive generations of man. The process leading to the formation of the numerous families of spiders took much longer than the corresponding process in scorpions. This fact should not be interpreted as evidence that the formation of the families was not due to a macroevolutionary production of "hopeful monsters." These may have been produced at considerable intervals. On the basis of experimental evidence derived from the study of other animals, it is assumed that all "hopeful monsters" capable of living are extremely rare and require isolation for their survival as a new subspecies or species.

Wright tried recently to circumvent this difficulty by assuming the possibility of "intergroup selection." But assume for a moment that, under the unknown conditions of the distant geological past, "hopeful monsters" of the same kind were produced in sufficient numbers at the same time, and that this macroevolutionary change was automatically coupled with a change in sexual attraction so that "hopeful monsters" were attracted only by other "hopeful monsters" and did not mate with the individuals which

remained unchanged. The macroevolutionary changes in Arachnida were of a kind which may have affected their viability no more than a situs inversus of viscera in man affects his ability to live. The struggle for survival of such "hopeful monsters" would have been in no way affected by the conditions of intraspecific survival prevailing in their parental stock and would depend only on interspecific factors, survival itself depending only on a balance between mortality and fertility.

Without prejudice toward the studies of such animals as Drosophilia, I believe that Goldschmidt is right when he considers microevolution to be "a blind alley." All morphological eivdence is in favor of the assumption that macroevolutionary changes in Diptera were completed in the comparatively distant past. To study the genetics of macroevolution one should use animals belonging either to an order in which the process is not yet completed or else to an order in which the descendants of both "hopeful monsters" and unchanged ancestors still survive. Spiders seem to comply with the second alternative, but their rate of breeding is slow and their upkeep troublesome. The Acari, or mites, seem to be a better material in this regard, although their evolution has not been elucidated as yet and, from this point of view, the selection of the proper species of mite for experimentation would offer difficulties. Perhaps some fish type would be still better because, under artificial conditions of rearing, fish produce so many monstrosities that the possibility of producing "hopeful monsters" is not so remote. At any rate it seems imperative that the methods for the production of viable "hopeful monsters" should be different from those employed for the production of microevolutionary changes. (pp. 121-122)

• Development of the Social Insects

THE BEE'S KNEES

Stewart, Charles D.; *Atlantic Monthly,* 136:1-11, July 1925.

From what we have observed of the sexual arrangements of the hive it will be seen that a queen is the daughter of a father and a mother neither of whom has had any experience in the work of the bee, either in the hive or out; and these fathers and mothers were descended from others who never had any experience in those things which make up the marvelous mechanism of the swarm. Consequently they can have no acquired traits or habits, or responses to environment, or effects of the use or disuse of organs to transmit to their offspring. And the neuter bees, who

have all the struggles with life, and most of the special fitness which makes the swarm survive, cannot transmit to their offspring any new results of experience, or developments of habit, or gradual conformation to environment--simply because they have no offspring to transmit them to.

Right here is where Darwin's theory of evolution went on the rocks. To any theory of evolution, heredity--the ability to transmit evolved traits to offspring--is absolutely necessary. A 'law' that does not account for all cases that it is supposed to cover is no law at all; consequently the law of evolution has to succeed here or fail to be a law. Knowing this, Darwin was much perturbed when, about the two hundred and fiftieth page of his Origin of Species, he came to this problem of the neuter insect. As he says, it is a difficulty 'which at first appeared to me insuperable and actually fatal to the whole theory.' In fact, he must here submit a theory which seems plausible, or acceptable as a working hypothesis, or it is the end of his theory of evolution--or, for that matter, any other theory of evolution. And so, while his theory went on the rocks at this point, he set to work manfully to pull it off and get it into some sort of working condition.

He lays aside the idea of animals which vary from generation to generation and which transmit these advantageous changes to their offspring, and supposes instead the ability of a queen, somehow acquired, to lay eggs which vary in this advantageous way. Queens which laid such eggs survived because their swarms had advantages which caused them to survive. As to how such queens came to lay eggs with these peculiar potentialities, and different from the eggs of other queens, he puts the whole stress here on 'spontaneous' variation--by which he means variation which we do not understand and cannot account for: Thus the theory went ahead again, but without having been really cleared up even by this supposition. While he had to account for all neuter insects, he was dealing especially with ants; and among ants the neuter insects in a community are not all alike, but have definite classes or castes quite different from one another in structure and instinct. Here the theory would have to be stretched pretty far--almost too far. For how did a queen ant, simply employed in laying eggs, get this ability to lay eggs in which the whole intricate plan of the community, with its various instincts and different physical structure, was all latent and ready to spring forth from this egg and that? How does 'evolution' account for such a thing? Darwin never got quite over the difficulty here, as can be seen in his recapitulation, where he refers to it with an evidently dissatisfied mind.

However, all this is a difficult and recondite subject for the layman to employ himself upon, he would never know his own opinion of evolution if he had to consider and weigh such obscure and questionable facts. If he wishes to bring his mind to the testing-point he must get at it more directly.

Here on this front leg of the bee is that self-threading, combined comb and scraper for the bee's antenna. It is not in the patent-applied-for stage of invention, but is a complete and per-

fect working device. The whole affair would be quite likely to receive papers from the United States Patent Office but a lawyer would have to draw up his basic claims pretty carefully to keep some useful point from being stolen. It might be the invention of some assiduous Yankee-though it is doubtful whether he would get it so perfect that it would not need further improvement and repairing.

Considering this now as a mere 'fortuitous' falling-together of raw material, a mere happening or series of coincidences, a result of pure blind chance the human mind simply refuses to take that position. In anything like this we see preconception. And preconception is mind, intelligent force. It is something quite different from material.

'Darwin considered natural selection, operating by means of small fortuitous individual variations, as the most important factor in organic evolution.'

So says the New International Dictionary, very correctly including the fortuitous. Indeed, blind chance is of the very essence of the theory; for the survival of the fittest, as the result of advantageous variation, implies that other animals had variations which did not happen to be advantageous. The advantageous variation is one which happens to coincide with some feature of the environment; and so natural selection is a theory which gives an animal a 'spontaneous' tendency to vary in all sorts of aimless and undirected ways, and then builds the animal up from some primitive or one-celled form by a long series of coincidences. Essentially it is 'fortuitous.'

The human mind, accepting the theory, and starting out to give a definite application, is immediately brought to a halt. A man simply will ask questions---it is part of his nature and so he wants to know whether the bee's antenna existed first and needed a cleaner for long periods of time, or whether the cleaner happened first and was in need of something to clean. As to the gradual evolution of tools, here is a bothersome thought! Some tools are of such a nature that they are not of use till they are complete. A wheelbarrow without a wheel, a pipe wrench without a jaw, would be an entirely unevolved piece of evolution. The idea might be there, but---! The antenna-cleaner seems to belong to this class of tools. One cannot imagine its progressive stages and see how any one of the imaginary states of incompletion would give its possessor any advantage over other insects in the struggle for existence.

One sees now why evolutionists have more recently taken up the theory of 'mutations,' which is that individuals may acquire new characteristics suddenly, all at a birth. This would be a much better way for such tools to happen---all at once, and overnight as it were, answering the need of an antenna that ought to be cleaned. And so it would appear that the antenna and the cleaner must have been made together---in fact, maybe they were made at the same time the bee was made! But this will not do, for this is not evolution at all. Evolution is opposed to special creation; that is its very definition as set down by the evolutionists. We must not imagine any such thing because it is against

the law of evolution. So there we are again --- the human mind refusing to meet the conditions. For my part I must admit that I am completely frustrated; and so I shall proceed to drop the subject.

Practically, it is like an argument about infinity. Eternity is difficult to admit; a thing without end is beyond the reach of thought. On the other hand it is just as difficult to admit that there could be an end to time or space --- beyond which point there would be neither time nor space. Both are impossible.

Evolution, practically, is something in the same line. At first it is a supreme act of faith, under the guise of a 'working hypothesis'; and very soon it has become a sort of theology, disbelieving which you are a heretic. Right here is the ground of complaint as set forth by Fundamentalists and others. They say that evolution has become a dogma, a creed of science, set forth as if it were truth instead of a mere theory. The 'working hypothesis,' which belongs in the laboratory of the trained investigator, has got itself mixed up in our educational programme. It is a school of thought. Young men and women, instead of being brought face to face with the unknowable, and trained in any sort of contemplation or reflection, are wholly neglected in that important department of their natures.

For my part, I cannot contemplate a wasp, a spider, or a bee without being confronted with absolute Mystery. At the end of every telescope, beneath every microscope, at the bottom of every marl pit, is Mystery pure and simple. Science reveals more for me to wonder at, but solves nothing. This may not be religion exactly, but it is a continual reminder of my own position in the universe. And so I think that a system of education which takes the attitude of accounting for all things, or being just upon the point of accounting for them when the scientists have discovered just a few more facts, is not education at all. It stops just short of the truth; and therefore it is not exactly honest. (pp. 9-11)

UNUSUAL PHYSICAL ABILITIES

• The Magnetic Sense in Insects

MAGNETIC ANTS

Fletcher, H. Bowden; *Nature Magazine,* 40:415+, 1947.

Australia, and only in Australia, there is a species of termite that builds its nest according to the compass. There is nothing rough and ready about the directional instinct of this meridional, or magnetic white ant. The long axis of its nest faces, directly and accurately, north and south, the broad axis east and west. The insect also erects its colony of homes to a preconceived plan, with perfectly oriented rows, surrounding an open space, in which it apparently prohibits any building. Evidently there are special termite building inspectors to police this rule.

The habitat of this one of several species of Australian termites is confined to a narrow, thirty-mile strip of country in the Northern Territory about ten miles south of Darwin. This variety is found nowhere else on this continent, or, in fact, anywhere else in the world, so far as is known. The termites seem to prefer damp, ill-drained soil of dark gray color in country almost devoid of trees. Occasionally they build where there are scattered, dwarfed, native trees, but they never select the higher, red soil country or the open forests not far from their habitat.

The actual nests are usually from ten to twelve feet high, ten feet long, and three and one-half feet through the middle at ground level. The ends are vertical and slightly narrowed towards the apex, which is always serrated, and the actual mounds are just as hard as could be imagined. To the uninitiated they just look and feel like hard, packed mounds of dirt, and anything but a nest. The longer sides are not vertical, and the eastern face is invariably slightly concave, the western side convex below the middle and concave above. The reason for this peculiarity remains hidden in the instinct of the termite, and the scientist has no explanation to offer.

The reason for the accuracy in building due north and south is still interesting scientists, who originally thought it was to face the prevailing winds. The prevailing winds refuted this argument by coming from the other direction. Then it was suggested it might be to enable the winds to give quick drying after the monsoonal rains, but the winds also corrected this fallacy. The now-accepted theory, although minus any accurate proof, is that the

Australian oriented termite mounds

termite seeks to avoid the extreme heat of the summer months in the part in which it lives. This is, to some extent, supported by the location of the actual living galleries, on the outside surfaces only, while the center galleries are filled with cut-up grass and herbage on which the insects live. This grass is collected when the wet season starts, and green shoots are plentiful and are stored for use in the long dry period in the upper half of the nest only. Here it is, of course, safe from flood level.

The Hamitermes meridionalis, as these interesting termites are known to entomology, apparently cannot stand heat. During the middle of the day they seek the galleries at the base of the mound, or go beneath the actual nests. In the winter, when some warmth is required, they use the eastern side in the morning, and move with the sun to the western side in the afternoon.

HOW DOES THE COMPASS TERMITE FIND NORTH?

Anonymous; *New Scientist*, 74:333, 1977.

The constructions of the so-called "magnetic" or "compass" termites are something of an enigma. These weird nests take the form of a block about 1½ metres square but only 10 to 20 cm thick, and the termites that build them get their name because the long axis of the nest is invariably oriented in a north-south direction. Viewed from the east or west a field of such nests looks like a collection of squat buildings, while from the north or south they look more like highrise towers. Considerable speculation has been devoted to why the nests are aligned this way; and what cues the termites use to set their compasses, but with no very clear conclusions.

In 1973 Gordon Griff at the University of Sydney argued, quite convincingly, that termite nests would evolve to have a large surface-area-to-volume ratio because this would promote efficient exchange of respiratory gases across the termitiary walls. A simple way to achieve this would be to have a long slim nest; and then thermal considerations would favour a north-south orientation. The broad face of the nest could absorb energy when the Sun was low in the sky but only its narrow edge would be presented to the fierce zenith Sun. It follows that in semi-shaded areas, where north-south would not necessarily be the best orientation, one might find that the nests were not quite so "magnetic".

Griggs returned to the field to test this prediction and make accurate measurements of termite nest alignment, and he reports his findings in the Australian Journal of Zoology (vol. 25, p.87). Magnetic termites are common in the Northern Territory, so it was no problem for Griggs and A.J. Underwood to find four representative fields some 320 km east of Darwin. One of these fields contained trees and was semi-shaded, but the other three were open. At each site Griggs and Underwood measured accurately the alignment of over 50 nests.

They found that the accuracy of the termite in each field was very similar, but the preferred direction in one field was different from that in the other three; the difference was only a matter of some 2° and unfortunately it was not the semi-shaded field that was the odd but one of the open fields.

This means that the termites probably do not adjust their nest orientation to suit local conditions. Another curious observation was that the preferred direction of the nests lies at around 8° from true north, different from both true north and magnetic north. Griggs cannot explain this, but argues that the good orientation in the semi-shaded field implies a genetic predisposition to build orientated nests. This is probably going beyond his data, and tells us nothing of how the termite, safe inside its nest, knows which way is north.

TERMITES RESPOND TO THE EARTH'S MAGNETISM

Anonymous; *New Scientist*, 20:500, 1963.

Various hints in the scientific literature that termites always face in the same direction when lying in their nests have been systematically checked by G. Becker of Berlin-Dahlem. He kept termites in the dark and was able to verify that several species of termites (though not all species) indeed take up definite orientations when in their nest. Some species align themselves parallel to the Earth's magnetic field, others at right angles.

When the termites were housed in an iron box which destroyed the influence of the magnetic field they settled down in random positions. When they were brought into the field of a powerful magnet they adapted themselves to the new lines of the new magnetic field. The time required for this adaptation varied between a quarter of an hour and several hours.

. . . .

BEES HAVE MAGNETIC REMANENCE

Gould, James L., et al; *Science*, 201:1026-1028, 1978.

Abstract. Honey bees orient to the earth's magnetic field. This ability may be associated with a region of transversely oriented magnetic material in the front of the abdomen. The magnetic moment apparently develops in the pupal state and persists in the adults.

MAGNETIC DOLPHINS . . . AND BEES

Anonymous; *Science News*, 117:376, 1980.

Bees follow a magnetic compass, too. And Joseph Kirschvink of Princeton University suggests that their compass might tell geologists about the strength of the ancient geomagnetic field. Bees show the clearest link between magnetic effects and behavior, Kirschvink says, and therefore make the best candidates for such studies. For example, previous work showed that when bees are exposed to very strong magnetic fields, their sense breaks down and magnetic-related behavior--such as their communicative "dance"---become erratic. The field strength at that break point is about 5 gauss, Kirschvink says. He suggests that this upper limit may have developed through natural selection---those bees able to function when the earth's field reached that strength survived. If so, with some complicated genetics, he proposes that it may be possible to determine when that strength existed.

• Unexplained Senses in Ants

A BIOLOGICAL PUZZLE

Brun, Rudolph; *Nature,* 95:38-40, 1915.

Dr. Brun, of Zurich, has done a fine piece of work in devising an elaborate and ingenious series of experiments which enable us to come to a decision among the rival theories of way-finding among ants. Let us first illustrate the facts. If we pick up one of the higher ants from an ant-road, turn it about in a box, and then empty it out again near the place of its capture, it makes no mistake in hurrying homewards. When an ant goes off alone on an exploring adventure, it often keeps persistently in one general direction, in spite of many divagations to one side or the other and when it turns its face homewards, it does not usually retrace its steps, but pursues a parallel course until it comes near the nest. If a higher ant, such as Formica rufa, be gently but firmly induced to travel on a path chosen for it and not by it, it makes straight for home when freed from coercion. It may run along a line which is the hypotenuse of the triangle the other two sides of which it was compelled to follow, or it may complete a polygonal figure and reach the nest. If members of such species as Formica rufa and F. sanguinea be lifted up and carried some distance and put down in hunting ground which they have not visited for a fortnight, they will return home quickly, confidently, and by the shortest way.

These are four illustrative facts out of many, and the question is how they are to be interpreted. Some authorities still believe that there is no getting past the assumption of a non-analysable sense of direction, such as the Martian of Du Maurier's novel had of the North Pole. Others have swung to the opposite extreme of taking too simple a view, and maintain that is is altogether a question of scent: ants, like dogs, living in a smell-world. Others again lay too heavy a burden on muscle-memory, and others on visual impressions gathered by the way. Dr. Brun shows clearly, we think, that the power of way-finding is usually a composite product, and that there is no mysterious sense of direction.

Of course, there are ants and ants, and there is no doubt that the scent of the nest, of the food, and of the pupae sometimes counts for much. If two adjacent sections of a pre-arranged ant-road be lifted and interchanged, the travellers go on just as they were doing; but if a section of the road---say a zinc plate---be lifted and replaced with its ends reversed, the ants seem to be perplexed at the boundaries, and there may be a temporary block. Facts of this sort have given rise to over-ingenious theories of polarised scent, of positive and negative scent, and so forth. This much seems clear, that the nest-smell

gets fainter in proportion to the distance from home, and that the food-smell increases as the source of supply is approached; and it is very instructive to find that if an ant of one of the olfactory species be transported and placed in the middle of one of the antroads, it does not go home right way, but takes a tentative run first in the one direction and then in the other. In some genera, however, such as Formica, smell counts for little, and the obliteration of the scent by brushing the road or pegging down a spread-out newspaper does not disturb the homing. In connection with smell, it may be noted that the seat of the olfactory sense is in the tips of the mobile antennae, where tactility is also located, so that tactile and olfactory impressions are closely combined.

To many ants the illumination is much more important than scent, as Lord Avebury proved long ago. He got his ants to make a path across a wooden disc concentric segments of which could be rotated, and found that if he turned a ring so that an ant on its journey was made to face the wrong way, it righted itself and proceeded in the old direction. But this was not the case when he made the experiment in uniform shade, or when he shifted the light at the same time as he rotated a segment of disc. One of Brun's experiments with a species of Lasius is very instructive. It was marching with the sun directly in its eyes, when the experimenter put an extinguisher over it and kept it prisoner from 2 to 5 p.m. When it was set free at five o'clock, it turned its back on the position which the sun had reached, moving through 30°, and set off in a straight line homewards, eventually turning sharply to the left to reach its original starting point. Numerous experiments confirm the view that the direction of the light serves as a compass. When Santschi shut off the sun with a large shade and made a false sun by means of a mirror, he got the ants, even on one of their main roads, to march in a direction either at right angles to the original one, or opposite to it, according to the position of the mirror. If, in the absence of sunlight, there be equal bipolar illumination of a given area, there is in many species no orientation.

From waxing and waning scent and from different illumination, ants seem to build up associations, but this is not all. There is evidence in some cases of a memory of muscular movements, especially of the distance traversed, as if the ant kept its eye on a pedometer. There is something very interesting, too, in the phenomenon technically known as Turner's curves. A solitary ant that has travelled successfully from a considerable distance reaches a point quite near the nest; but instead of going on confidently, it stops as if perplexed. In many instances---80 per cent. in Cataglyphis bicolor---it proceeds to describe concentric curves, it may be for 5-15 minutes, and gradually draws near to the door of its home. Is it seeking for a sign, which might be a shining stone among the sand, or a scent, or the faint stridulation of one of its kin? Is it pursuing a trial and error method, very willing to be helped by any hint or combination of hints?

In some cases, e.g., Formica rufa, Brun has proved a baraesthesia, or feeling of gravity. A table was gently tilted, with the

nest at the foot of the slope; a feast of honey was placed in the centre; the ants climbed straight up and straight down. But if, while an ant was supping honey, the table was gently tilted in the opposite direction, so that the way to the nest was up-hill, the ant persisted in going down-hill as before---away from, not towards, home. Among the highest ants Brun finds distinct evidence of definite local memory, based on visual, topographical, and topochemical data, and lasting for two or three weeks at least. And only thus can we understand the confidence with which one of these creatures, transported to a distant part of its range, will make for home. There are ants which trust mostly to scent, and others which are largely guided by the direction of light, but for the higher ants the orientation is a complicated process, the outcome of the registration of manifold imprints received from the outer world--imprints relating to the quantity and quality of scents, the general direction of light, the illumination of particular objects, the slope of the ground, the feel of things, the distance travelled, the turns of the road, the direction of the wind, and even, perhaps, sounds. Individual ants hereditary endowed with great sensitiveness, hereditarily attuned to receive certain kinds of tidings, serve an apprenticeship in the establishment of associations and reach a degree of perfection probably unsurpassed. Such is Dr. Brun's general position, which he defends with strong experimental evidence. There are still to be found old-fashioned fishermen who have attained within a certain range to a wonderful seamanship of an empirical sort; they have built up a body of associations from wind and from wave, from the sky and the "feel" of the sea, and they are seldom far out in finding their way home. And so it is with the higher ants, except that they work even more exclusively from an instinctive basis.

It must be remembered that the orientation power of ants does not stand magically alone. Even brainless animals adjust their body in a position of physiological equilibrium in relation to a stimulus of light or warmth or gravity---a static orientation. When there is direction of locomotion in relation to an external stimulus, we speak of dynamic orientation. This dynamic orientation may be direct or indirect. It is direct when the stimulus or goal is within the range of immediate sense-perception, and it must be noted that for ants this range is only about a yard. Of this locomotor orientation there are various grades---tropistic, reflex, instinctive, and acquired, the first three expressing a hereditary predisposition, the fourth expressing the results of the individual's own learning. On a higher level is indirect orientation, where the goal is beyond the range of direct sensory perception. A complex of imprints or memories, corresponding to the goal, functions in the animal's sensorium, and forms the unifying centre of a whole series of imprints of the environment of the goal. What leads the creature on from step to step---often quickly and, so to speak, unquestioningly, if no contradictory interruption occurs---is the recognition of localised stimuli corresponding to those of the unified reference series. The orientation implies a chain of recognitions, and the recognitions

imply a registration of individual experiences. Without using Brun's somewhat forbidding mnemic terminology, we cannot do justice to his carefully worked-out theory, but we have indicated its general nature. It is essentially what may be called psychobiological, for he thinks of the organism as a historic being that trades with time, that enregisters imprints, and that has its past living in its present, as Bergson has accustomed us to say. These imprints, which the individual ant selectively accumulates, are not like sheets filed in a portfolio of reference; they are interpenetrated with and kept alive by their meaning for the actual everyday life.

ANT'S NINTH SENSE—ONE OF THE MYSTERIES OF ANT LIFE

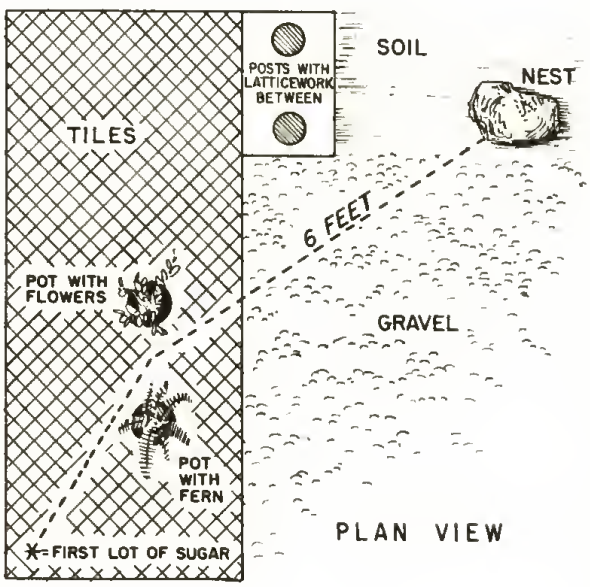
Morley, B. D. W.; *Victoria Institute, Journal of the Transactions,* 71:80-98, 1939.

Before actually discussing the reasons for concluding that the existence of a "ninth sense" in ants is possible, if not probable, and the nature of that sense, it is proper that I should offer a few words of explanation as to why I began my researches. The original aim of my research was an attempt to establish the means by which the sub-family Formicina Mayr, of the ants (Fam. Formicidae Mayr) communicate when at a distance from one another. The family Formicidae is divided into eight sub-families, and have attained their highest development in the Formicinae. (See phylogenetic chart.) It is known that the sub-families Ponerinae Mayr, Cerapachyinae Wheeler, Pseudomyrminae Wheeler, Dorylinae Skuckard, and the most primitive species of the sub-family Myrmicinae Mayr communicate by means of stridulation, sounds being elicited by means of rubbing one smooth, hard surface against another uneven, hard surface. It is possible that some species of the subfamily Dolichoderinae Forel also communicate by this method, but it is certain that the highest developed species of this sub-family and all the species of the sub-family Formicinae do not communicate by this method. It is probable that the species of the eighth sub-family, the Leptanillinae, communicate by stridulation. I entered on this difficult and complicated problem in what seemed to me the most obvious and promising manner. I made a list of all the possible methods of distant communication between ants, and, after having obtained the opinion of various specialists on these, I summed up the possibilities of each. This reduced the possible methods to three:---Smell, Stridulation, and Ninth Sense. (See "On Ants' Methods of Communication," B. D. Wragge Morley, Trans. Suffolk Nats. Soc., 1937. I then proceeded to do a number of experiments after the following manner:---

Data.

12.21 a.m.	An ant was out exploring. Sugar put down at (A), fairly near it.
12.22 a.m.	Ant found sugar.
12.25 a.m.	Ant still at sugar, four other ants came straight from nest to sugar, via (a). They did not visit a or b en route.
12.25½ a.m.	One ant came from nest, missed sugar and went back again.
12.26 a.m.	Ten ants at sugar, five more having come from the nest.

It is worthy of notice that the four ants when they went out to the sugar did not follow the trail of the first ant, but went direct. Thus communication was not effected by following a trail of scent, as suggested by Professor Julian Huxley and von Frisch. Neither



Ant experiment environs. Text reference A = *sugar source;* a *and* b *are pots, top and bottom, respectively.*

was communication affected by means of tapping on the ground, since the vibrations could not possibly carry over that distance. Also the surface, apart from its not being the same in substance over the whole distance, was unsuitable for the latter. More experiments of a similar nature were done, communication being effected in a like manner in approximately 67 per cent. of the experiments; thus establishing that communication was not effected after

the manner put forward by Huxley or by vibration communicated through the ground. This did not mean that scent as a method of communication was disproved by these experiments, since another method of communication by scent can be put forward as follows: --- the ant, on finding the food, produced by some method a scent which was noticeable to the other ants over long distances, causing them when they smelt it to follow it up and thus arrive at the required place. This method of communication would have been possible, of course, under the conditions existing in the experiments just described. It was therefore necessary to determine whether communication could take place under conditions where this method of communication by scent was made impossible. If communication was effected under these circumstances it meant that it was effected by stridulation, or some unknown sense. A good deal of consideration was given to this question---how to evolve an experiment which would supply these conditions---and a great deal of time was wasted in trying out experiments which proved unsatisfactory. It was, after all, quite an ordinary observation which showed me that I was correct in considering this type of communication by scent as an unlikely method of communication from a distance amongst ants. Some ants had escaped (I killed them immediately) from one of my wooden observation nests, resulting in the remaining ants also endeavouring to escape. The nest was, of course, resealed before they were able to do so. The excitement had apparently been conveyed to two ants, taken from the same colony, which were in another observation nest, since they also started to rush about. If communication was effected by scent, it would be impossible, for reasons gone into in an earlier paper, for the ants to convey more than the most elementary message. It is certain that no message expressing such a communication, as would be necessary under the conditions existing at the time of this observation, could be made by means of this method of communication. This was confirmed by later experiments.

Though I was certain that communication amongst ants, far apart, was effected by one of two methods, stridulation or ninth sense, I was unable to progress any farther. It was in endeavouring to do this that I received confirmation of the conclusion drawn from the observation cited earlier. I had some ants, taken from a nest about half a mile away from where I was staying at that time, in a jam jar; an article which is often used as a temporary observation nest. I kept them for about a week, in order to give them time to settle down to a life of set habits, as is usual in an observation nest. I then proceeded with the experiment.

A jam jar was taken to the place where the first lot of ants came from, the other (first) jam jar being left behind. Both jars had tin lids. About 160 ants, together with a certain amount of earth, were scooped up into the jar, the ants being, of course, taken from the same nest as those in the first jar.

The second jar was then taken back to the place where I was staying, but was left outside the room where the other ants were, about twenty-four feet away. The windows were shut and the second jar was fairly air-tight. The ants in jar No. 2 were, of

course, shaken and disturbed by the journey. The ants in jar No. 1 were, however, when I left them, as they usually were at that time, peaceful and below the surface of the earth. Entering the room I found two ants in the original nest (No.1) running round, apparently looking for something; they continued doing so for about two minutes.

It was obvious that communication had been effected between the two jars of ants. This communication could not have been effected by smell, and it seemed probable that it was not effected by stridulation. The results of the experiment were not, however, conclusive on this point, and in any case this type of experiment is rarely conclusive, even if it be repeated many times.

Normally, one would take into account the possibility of the existence of an unknown sense, and would therefore conclude that these ants communicated by means of either that or by stridulation. In this case, however, it was necessary to bear in mind the results obtained by other entomologists, when experimenting in order to try to solve this same problem. These entomologists put forward good reasons, as mentioned earlier in this paper, for the existence of communication by means of stridulation amongst the more primitive ants. Indeed, it has been known for some time that certain ants of the family Dorylinae often make, when out hunting for prey, a sound similar to, though very much weaker than, that produced by the Orthopetera. This aspect of ant communication will be dealt with later in the paper.

[Details of stridulation study omitted.]

Thus it is probable, I consider, that there exists among the Formicinae some ninth sense.

It will be seen that the requirements of this "ninth sense" are as follows:---(1) Communication must be instantaneous, and must not be affected by atmospherics or other mechanical physical conditions. (2) The method of production whereby communication is brought about, and the method by which the communication is received, must not be mechanical. (3) The method must be such that it can be produced under the required conditions, and by the required means, by ants. Communication is instantaneous, as was shown by my experiments and observations. This is supported by the observations of M. Maeterlinck, as is shown by the following quotation from his book, The Life of the Ant: "Whenever two ants meet, they almost invariably tap one another rapidly with their antennae, as though they had something to say. Have they no other means of communication? It is certain that the alarm, when a formicary is attacked, or merely disturbed, is propagated with such lightning-like rapidity that we are almost compelled to explain it by a complex of cellular reactions, instantaneous and unanimous such as occurs in our own bodies when they are seriously threatened or injured." It is obvious that communication would not be instantaneous if it were affected by atmospherics or other mechanical physical conditions. The reason that the method of production must not be mechanical is that if it was, it would necessitate there being organs for the production and reception of

the means whereby communication was established. There seem to be no such organs. The reason for the third condition is obvious.

The conditions set forth above rule out the method of communication that, among entomologists, is wrongly known as "wireless telepathy," or sometimes "telepathy." This "wireless telepathy" is communication by means of waves produced aeroscepsically in the ether (I do not, myself, believe that waves in the ether can be produced aeroscepsically). Thus we are left, I believe, with one possible form that this "ninth sense" can take, that is, the transference of waves produced by the brain of one ant directly to the brains of the rest of the community. This complies with all the conditions; also the fact that this method would explain that phenomenon that develops, curiously enough, with the atrophy of the stridulatory organ, namely, that a colony of ants seem to act as one body, and not as many independent bodies, gives added support.

• "Blind" Drilling of the Ichneumon Wasp

THE LONG-BARRELED BORE

Keithley, Willis E.; *Creation Research Society Quarterly,* 11:183, 1975.

If the caliber of that title sounds a bit queer, consider for a moment the boring auger of the Ichneumon wasp. For here is a drilling tool that defies comparison or comprehension. Of course reference is made to the amazing ovipositor of Megarhyssa lunator and its several similar species.

Can the reader imagine boring into wood a decimeter deep with a mere thread? Or even surmise the purpose for doing so in the first place? Yet here is an insect that has the amazing ability to locate beetle larvae in the cambium layer of a pine tree by tapping the bark with the antennae. (Is this ultrasonics or sonar?)

Then the insect assumes the stance as shown in the cover illustration, and accurately aligns a drill rig," which is composed of two spring-loaded sheaths to guide the threadlike egg tube to its ultimate destination of parasitizing the hapless larvae of the bark beetle.

The intervening layers of bark are penetrated by the ovipositor in a whip-sawing acton, since that grim gimlet is equipped with microscopic saw teeth as seen in Figure 1. Many species can penetrate several centimeters of wood to reach their prey.

One can only wonder at the ingenuity of this adept drillpress.

What a monumental sequence of mutations must have been necessary through trial and error to develop such a specific instrument! But can this precision be attibuted to blind and unpremeditated permutation? Mere evolutionary speculaton cannot suffice to bridge those yawning gaps of instinctive purpose, deliberate design, intelligent integration, and that perennial and plaguing question of survival before the perfection of such a complicated contrivance.

. . . . .

Ichneumon wasp with looped ovipositor drilling into tree

CURIOSITIES OF BEHAVIOR

• Curious Collective Action in Arthropods

SINGULAR PROCESSION OF CATERPILLARS

Davis, A. H.; *Magazine of Natural History,* 3:146-147, 1839.

On the 3rd of May I saw a procession of caterpillars. They were evidently Bombyces, and in form somewhat resembling Arctia caia, very hairy but the hairs white; the body dark brown but marked with paler lines. These caterpillars were crossing the road in single file, each so close to its predecessor as to convey the idea that they were united together, moving like a living cord in a continuous undulating line. At about fifty from the end I ejected one from his station--the caterpillar immediately before him suddenly stood still, then the next, and then the next, and so on to the leader; the same result took place to the other extremity. After a pause of a few moments the first after the break in the line attempted to recover the communication; this was a work of time and difficulty, but the moment it was accomplished by his touching the one before him, this one communicated the fact to the next in advance, and so on till the information reached the leader, when the whole line was again put in motion. On counting the number of caterpillars I found it to be 154, and the length of the line 27 feet. I next took the one which I had abstracted from the line, and which remained coiled up, across the line; he immediately unrolled himself, and made every attempt to get admitted into the procession, after many endeavours he succeeded and crawled in, the one below falling into the rear of the interloper. I subsequently took out two caterpillars about fifty from the head of the procession; by my watch I found the intelligence was conveyed to the leader in thirty seconds, each caterpillar stopping at the signal of the one in his rear; the same effect was observable behind the break, each stopping at a signal from the one in advance; the leader of the second division then attempted to recover the lost connection; that they are unprovided with the senses of sight and smell appeared evident, since the leader turned right and left and often in a wrong direction when within half an inch of the one immediately before him: when he at last touched the object of his search, the fact was communicated again by signal, and in thirty seconds the whole line was in rapid march, leaving the two unfortunates behind, who remained perfectly quiet without

making any attempt to unroll themselves. I learn from a medical gentleman here that these caterpillars feed on the Eucalyptus, and that when they have completely stripped a tree of its leaves they congregate on the trunk and proceed in the order here described to another tree. The caterpillars I saw must be nearly full grown, measuring about 2¾ inches each in length.

. . . . .

AN ARMY OF CATERPILLARS

Anonymous; *Popular Science Monthly*, 2:255, 1872.

A writer in the Gardener's Monthly gives some interesting particulars concerning the habits of the caterpillars, which last spring visited the region about Memphis in such unheard-of numbers. They were so numerous, that several trains of cars coming into the city were stopped on each of the two roads, the masses covering the rails for hundreds of yards in a body, compelling the brakemen to get down and sweep them off before the driving wheels could get sufficient hold to pass over the obstruction. They lived on the young leaves of both forest and fruit trees--the oak, quince, apple, and plum, being their favorite food. Whole orchards were denuded of foliage, and great lanes of bare trees marked their track through the forests. They are characterized by one remarkable peculiarity. Unless prowling through thick grass, or when about half grown, descending by the long web which each spins, from a tall rough forest-tree, they are always arranged in military style; and travel, also, in long, straight lines, several abreast.

A CURIOUS GROUP MIGRATION OF CATERPILLARS

Houston, R.; *Scientific American*, 61:229, 1889.

While out with a surveying party on the Decatur, Chesapeake & New Orleans Railroad, about two and one-half miles north of the Tennessee and Alabama line, I noticed a dark mass lying in an oblique position across the roadbed. It was in the exact form of a black racer, and on closer inspection I perceived that it was a mass of maggots moving just as one of those snakes would. In going over any obstacle, it would glide over with the same stealthy, graceful movement of the snake, and what struck me as the most peculiar part of it was that it, or more properly speaking they, seemed to possess all the instincts of the racer, save that of self-preservation.

INDIANS' HAIR

Ferris, G. F.; *Nature Magazine,* 8:222, 1926.

The traveler who wanders through the wilds of the west coast of Mexico, at least of the State of Michoacan, will not infrequently see upon the trunks of trees or on the under-sides of overhanging rocks curious black patches. Sometimes these patches are quite large, covering perhaps as much as three or four square feet.

If one examines them closely, they appear to be composed of short, coarse, black hairs and because of this the natives call them "pelo de los Indios" or "Indians' hair." If one passes his hand over the patch, it will be seen to move and the hairs begin to wave wildly about, or the patch may even break up. It can then be seen that actually it is made up of hundreds upon hundreds of small animals with extraordinarily long legs.

These animals are what we commonly call "daddy-longlegs" or "harvest men." Scientifically they are known as Phalangida or Opiliones, and they are rather closely related to the spiders for, like the spiders, they have four pairs of legs. There are many hundreds of species, some of them of most peculiar form, and while many are short-legged many others have the legs extremely long and slender. The very small body is carried suspended from the legs as if by a series of derricks. The species that makes patches of "Indians' hair" has legs that are three or four inches long while the body is not much larger than a pill.

The species that makes these patches clings, perhaps by its forward legs, to the surface upon which it rests and the other legs hang loosely down, giving the appearance of a mat of hair in which the bodies are entirely concealed, for the animals crowd very closely together. The particular species that is responsible for this phenomenon is not determined. Neither is it known why the animals congregate in such a fashion.

. . . . .

WHIRLWINDS OF PHANTOM FLIES

Anonymous, *New Scientist,* 31:8, 1966.

Seen from the highlands above Lake Edward in Uganda in the Congo or Rudolf in north Kenya, the sudden appearance of massive smoke-grey clouds, twisting and turning in the sky, sometimes some hundreds of feet in height is, to say the least, intimidating--especially to those who don't know what they are. They resemble waterspouts. A bank of what appears to be localized cloud gradually becomes roughly spindle-shape in appearance. It rises above the completely still, mirage-hazy surface of the water until only a dark thread writhes and twists upwards from the lake, like the neck or twister of a tornado.

The cloud is, in fact, a concerted hatch of lake flies--what

the Belgians in Congo used to call en elosion massive, a cloud so dense that fishermen in open pirogues that strayed into the vortex are said to have been suffocated. On some of the lakes the dead flies are scooped up by the bucketful, pressed into cakes and sold as a delicacy. What interested specialists in the swarming behaviour of insects at a recent meeting of the Royal Entomological Society is the species concerned and the periodicity of the phenomenon in relation to local meteorological conditions. Or, expressed more simply: are the flies picked up by a whirlwind or does the presence of the flies generate a big up-current of air? Some of the answers were given in last week's issue (Series C, Vol. 31, p.14) of the Society's Proceedings.

. . . .

CONVECTION PLUMES AND INSECTS

Wiersma, J. H.; *Science,* 152:387, 1966.

Peterson and Damman [Science 148, 392 (1965) show an interesting photography of plumes rising from elm trees in relatively still evening air. They report that there were no local concentrations of insects in the air, but I am convinced they would have seen such in the plumes had they used binoculars from a short distance. The phenomenon they saw is well known though not often seen. I first saw it on a late-summer evening some years ago, over an oak coppice some 2 meters high. Over every branch ascending at the top I saw stirring columns, like those depicted by Peterson and Damman, maximally about 1½ meters high. I realized that they were composed of mosquitoes only when I came nearer. Then I could see them individually very well. The second time I saw the plumes (my little son drew my attention to them) was on 7 October 1965 over Italian poplars near my house. They were also to be seen over other trees but this time not over small trees, I think, only over trees 10 to 20 meters high. Through binoculars (7 x 50) I could see the mosquitoes very well; they were large mosquitoes, probably Tehobaldia annulata Sch.

The next day, I saw Peterson and Damman's report, and as it was at the end of the afternoon and the same weather as the day before I had a look in the garden of "Hinkeloord," the building of the Institute of Forest Research of the Agricultural University at Wageningen, well known to Damman. And behold--the same phenomenon was to be seen there over different trees, not only broadleaved trees, but also coniferous ones -- Cedrus atlantica, Libocedrus decurrens, and Sequoidaddendron giganteum. On my way home I saw it over beeches and maples, in and around my garden over Chamaecyparis lawsoninana, Cedrus, poplars, and other trees. But the same phenomenon was also to be seen over a television pole on a neighboring roof, although not over a tree that had lost its leaves. We had a warm autumn and many mosquitoes that year. It was somewhat cloudy and there was little wind. Until 26 or 27 October the phenomenon was to be seen

every evening, except on one or two rainy days. I have been told that some years ago, in Zutphen, the fire brigade turned out because the church tower was reported to be burning. The "smoke" proved to be huge masses of mosquitoes dancing over the tower.

. . . .

THE SPINY LOBSTERS' WAGGON TRAIN

Anonymous; *New Scientist,* 43:8, 1969.

In the autumn, an extraordinary change comes over the normally seclusive and nocturnal spiny lobsters of the Caribbean. Forsaking solitude and darkness, they form giant queues, like single-filed waggon trains, and march steadily in parallel lines over the shallow daylit waters near Bimini, the Bahamas and the east coast of Florida. William Herrnkind, of Florida State University, has been looking at the lobsters to discover how--and, more important, why--they get this compulsion to queue (Science, vol. 164, p. 1424).

The behaviour seems to be some sort of mass migration. Queues form when one lobster spots another moving lobster, comes up behind him, and "locks on" to his back with its forward appendages. Herrnkind captured some non-queueing lobsters, and put them in a two-metre diameter circular pool The lobsters queued in a desultory and rather disinterested manner. But when he placed in the pool four lobsters that had been captured while migrating, the creatures queued up and marched purposefully around the perimeter at a steady six metres a minure. Subsequent monitoring of this mini-queue revealed that the four lobsters filed nearly continuously, day and night, for 33 days!

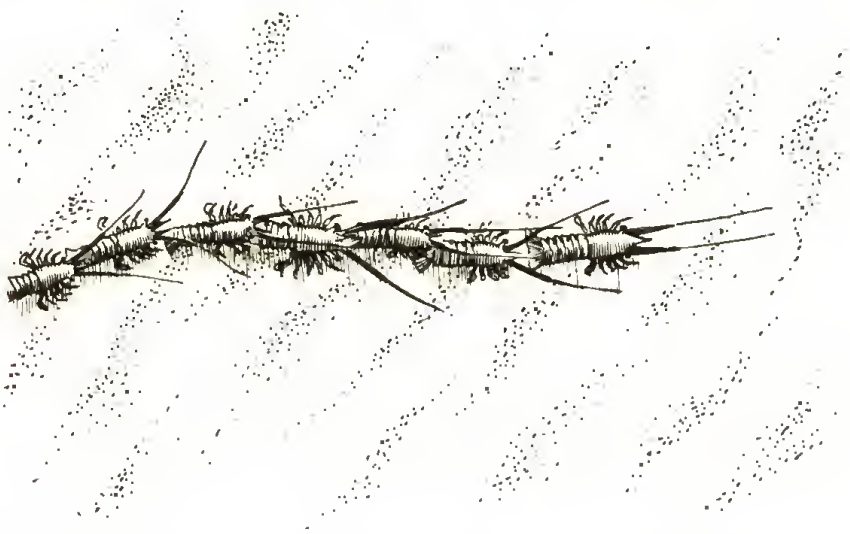
Herrnkind believes this difference between migrating and non-migrating individuals reveals that internal physiological changes are at least in part responsible for the mass movements. The lobsters seem to form the queues when they find themselves moving over open areas. The queue is probably defensive: the vulnerable abdomen of each individual is protected by the spiny foreparts of the lobster close behind (except, of course, for the unfortunate rearguard).

MIGRATION OF THE SPINY LOBSTER

Herrnkind, William F.; *Natural History,* 79:36-43, May 1970.

These mass, single-file marches by spiny lobsters are unique, the only known formation movements by bottom-living crustaceans.

Furthermore, the marches markedly contradict the established view of this species' behavior pattern. Spiny lobsters are nocturnally active; they wander about at night to feed on annelid worms and small mollusks, but return before daybreak to shelter in crevices on the reef, under rock ledges, or among dense fronds of sea whips. Why, then, do all the lobsters in a region become active each fall, moving by day, in formation, over exposed areas where they are never seen at other times?



Perhaps the most striking feature of the mass movements aside from the sheer numbers involved, is the single-file formation, which I call a queue. Some other crustaceans travel in more or less definable groups: fiddler crabs and soldier crabs form great droves, or herds, which scour the beach for food at low tide, while some shrimps aggregate in clusters, or schools. However, none approach a stable, spatial configuration to match the long, straight queues of Panulirus argus. All queuing lobsters maintain the precise course and speed of the leader and move through turns as though they were on rails. More amazing is that all the queues, no matter how far apart, travel in equivalent or parallel headings! Just how do the lobsters organize themselves into queues, establish leadership, and maintain formation? What is the biological significance of the mass migrations? Where do the migrants come from and where are they going? The questions seem endless. (pp. 36,38)

GREGARIOUS KING CRABS

Powell, Guy C.; *Sea Frontiers,* 20:206-211, 1974.

When king crabs attain the age of about one year, they begin

to emerge from rock crevices, kelp patches, and other protective niches where they have been living since settling as planktonic larvae a year before. As these tiny individuals, no larger than a quarter, wander slowly and seemingly aimlessly along the often-jagged rocky substrate, they meet one another and join together. Small aggregates are formed; these meet and join others, forming increasingly larger and larger groups. As the young king crabs of these groups climb one upon another, characteristic ball-shaped pods are formed. These pods join other pods, and eventually they are of such size that they may be 12 feet long and comprised of thousands of individuals.

The aggregations seem to be constantly moving very slowly along the sea floor and changing from one shape to another. Depending upon time of day, substrate relief, tidal action, and other natural disturbances, a single, elongate pile may form and then separate into several pods a short while later. Although the rate of travel of each pod varies, it is not known what determines the speed. Of the various pods for which the distance traveled in a day was measured, the shortest distance was 8 feet and the longest was 61. Movements of 116 feet in 11 days and 183 feet in 17 days have been recorded. The migrations may be vertical (from deep to shallow--and the converse) as well as horizontal (in all directions).

Podding, a term used to describe the general behavior of young crabs forming close-knit aggregations, is believed to provide protection against predators as well as to organize the animals into a coordinated group, so that, acting as a unit rather than as individuals, they can benefit from their coexistence. Thus, pods are analogous to other animal congregations, such as herds, flocks, and schools.

. . . . .

• Collective Synchronous Action

FIREFLIES FLASHING IN UNISON

Morse, Edward S.; *Science,* 43:169-170, 1916.

Fifty years ago in Gorham, Maine, while walking along the road I passed an open field and noticed to my astonishment hundreds of fireflies flashing in perfect unison. I watched this curious sight for some time and the synchronism of the flashing was unbroken. Many times after I have watched these luminous insects, hoping to see a repetition of this phenomenon, but the flashes in every instance were intermittent. Since that time I

have read about these insects in various books without meeting any allusion to this peculiar behavior. At last I have found a confirmation of my early observations. In Nature of December 9, page 414, is the report of an interesting paper read before the South London Entomological and Natural History Society by K.G. Blair entitled "Luminous Insects" in which reference is made to the remarkable synchronism of the flashes in certain European species of fireflies. The explanation offered as to the cause of this behavior seemed to me inadequate. One often notices in the stridulation of the Grillidae the perfect time the insects keep in their concerts and it seems likely that the same impulse must animate these flashing beetles, and the light emitted could be more easily followed than the sound.

The following is an extract from Mr. Blair's paper:

> Apart from its principal function in securing the proper mating of the sexes, the light seems also to be largely used, at any rate by the males, for purposes of display. Where the powers of luminosity are largely developed in this sex the emission of the light is usually of an intermittent flashing type. It has been noticed in various parts of the world that these flashing males tend to congregate in large companies, and that all the individuals of one of these gatherings will flash in concert. All the fireflies around one tree or group of trees, for instance, will flash together, while those around a neighboring tree will be pulsating to a different time. This feature has been observed of a European species of Luciola (though Mr. Main and myself were unable to detect anything of the sort with L. italica at Lugano), of an Indian lampyrid genus not stated, and of the genus Aspidosoma in South America. The American species of Photinus and Photuris do not seem to possess the habit.
>
> The exact reason of this flashing in concert, or the method by which it is brought about, have not been ascertained. It has been suggested that the light is not really intermittent in character, but merely appears so owing to its being alternately masked and exhibited by movements of the creature's body, and that a slight puff of wind might perhaps affect all the members of a company and cause them all to conceal their lights at once. Though this explanation of the intermittent character of the light applies well enough to Pyrophorus, an insect we shall shortly consider, it is certainly not applicable to these Lampyridae. It is true the light is not absolutely extinguished between the flashes, but it is so diminished as to become practically dark; moreover the flashing is unison is too regular to be caused by chance puffs of wind. A more probable explanation of the phenomenon is that each flash exhausts the battery, as it were, and a period of recuperation is required before another flash can be emitted. It is then conceivable that the flash of a leader might act as a stimulus to the discharge of their flashes by the other members of the group, and so bring about the flashing concert by the whole company.

THE SYNCHRONAL FLASHING OF FIREFLIES

Allard, H. A.; *Science,* 44:710, 1916.

In Science for February 4, 1916, E.S. Morse, under the title, "Fireflies Flashing in Unison," mentions having seen fifty years before a striking instance of the synchronal flashing of fireflies. Morse again discusses briefly the same subject in Science for September 15, 1916. He states that he has never since observed this phenomenon in the flashing of these insects. McDermott, in Science for October 27, 1916, also discusses the question of fireflies flashing in unison.

The synchronal flashing of fireflies appears to be a very rare phenomenon in North America. So rarely does it seem to occur that one may consider himself fortunate if he has observed the phenomenon even once in a lifetime. The writer about twelve years ago observed a most remarkable instance of the simultaneous flashing of fireflies in Oxford, Mass. On the night this phenomenon occurred a heavy thunderstorm had recently passed over, followed by a profound calm. From time to time dazzling flashes of lightning illuminated the landscape. The air was very warm and humid, and fireflies became unusually abundant and active, especially in a low field adjoining some woods. Here thousands of these insects were sailing low over the ground, flashing incessantly as far as the eye could see. After a while a most remarkable synchronism in the flashing appeared to take place. From time to time, as if moved by a common impulse, great numbers would flash so closely in unison over the entire field that an extensive sheet of tiny light-points would gleam upon the vision for a moment--and then vanish. This remarkable synchronism in the flashing sometimes continued several times in succession, giving one the impression of alternate waves of illumination and darkness in the distance. At times the rhythmic impulse ceased for a considerable period over the entire field. At other times it appeared to take place only in large groups occupying particular areas of the field. Although the writer has given a great deal of attention to the flashing of fireflies during the last twelve years, synchronism in the flashing of these insects has never since been observed. Depending more or less upon atmospheric conditions, fireflies show considerable variation in the character of their flight and the flashing impulse. At times the insects seem loath to leave the low herbage. On certain evenings they appear to confine their flight over the fields largely to the lowermost stratum of the atmosphere; at other times they rise upward in myriads from the grass early in the evening and drift away in all directions toward the crowns of the trees. At such times the upward flight is frequently accompanied by a weak, prolonged emission of light so that the insects appear to be tiny, glowing sparks propelled upward by gentle air currents.

A HISTORICAL NOTE ON THE SYNCHRONOUS FLASHING OF FIREFLIES

Gudger, E. W.; *Science*, 50:188–190, 1919.

The interesting accounts of this remarkable habit published in Science during the past two years by Professor E.S. Morse and others have led me to make notes of similar accounts found in working up certain books on the East Indies and New Guinea. The excellent summary of our knowledge of this striking phenomenon published in Science for July 26, 1918, by Professor Morse, and the later communication from Mr. George H. Hudson led me to believe that these historical data may be of interest and possible value to those studying this habit in insects.

The first of these accounts was found in Robert W.C. Shelford's book "A Naturalist in Borneo" (London, 1916), a work replete with natural history data of great interest and value. At the time that I made a note of Shelford's observations, I had forgotten that Professor Morse in Science for September 15, 1916, had published Shelford's account from advance proof sheets of his book.

The next account I have chanced upon is from the pen of Nelson Annandale, the well-known zoologist of Calcutta, India. His paper, "Observations on the Habits and Natural Surroundings of Insects," made during the "Skeat Expedition" to the Malay Peninsula, 1899-1900 was published in the Proceedings of the Zoological Society of London, 1900. From Part VI., "Insect Luminosity," the following extract is taken:

> Of all the manifestation of luminescence among animals there is none more curious, or, in the present state of our knowledge, more inexplicable, than the manner in which large numbers of individuals of certain fireflies are able to display their light with absolute apparent simultaneity and unison and with regular intervals of darkness, under circumstances which make it impossible for all the members of the swarm to see one another. Even the power, possessed by some peculiar South-American beetles, of showing lights of different colors on different parts of the body at the same time is not more wonderful, or more conspicuous, than this. The phenomenon is not common on the east coast of the Malay Peninsula, where the soil is sandy; but it is said to be often manifested both in Siam proper and among the mangrove-swamps of Perak and Selangor in the west. I have only been able to see it on one occasion, and that was on the bank of the river Kuala Patani, one fine evening at the end of June.
>
> A large tree was covered with many hundreds of fire-flies, the majority of which seemed, judging from the similarity of their lights, to belong to one species, or perhaps to one sex. There were three individuals seated together, however, whose lights were larger and bluer than those of the others. The lights of all the specimens of the more abundant variety flickered in unison with one another; those of the minority, the three individuals, flickered together also, but in a different

time. At one instant the tree was all lighted up as if by hundreds of little electric lamps; at the next it was in complete darkness, except for three blue points. Then, again it was covered with white points, except for a little patch of darkness where the three blue points had been and would be again immediately. A similar power of displaying luminosity in unison is said to be exhibited by some marine animals, even after they have been removed from the water; but the questions as to how this unison is effected and what is its exact object are obscure. The power by which it is regulated may be somewhat analogous to that which causes all the individuals composing a flock of birds to wheel at the same instant. As Professor Poulton has pointed out to me, the rhythmical display of light among a crowd of individuals appears much more conspicuous to the eye than the simple flickering of a number of independent points.

It will be noted first that Annandale's account is very circumstantial, perhaps more detailed than any account yet at hand. Secondly it should be noted that he writes that "it is said to be often manifested both in Siam proper and among the mangrove swamps of Perak and Selangor in the west." In other words this phenomenon is not so unusual in Malaya as might be surmised from its rare occurrence in our country.

Antedating Annandale by twenty years is an account by Burbidge of an excursion on the Scudai River, Jahore (Johore?), near Singapore. He says (1880) that:

> The silence of the night was unbroken, save by the regular dip of the oars, and as darkness increased, the tiny lamps of the fireflies became visible here and there among the vegetation on the banks. As we glided onward, their numbers increased, until we came upon them by thousands, evidently attracted by some particular kind of low tree, around which they flashed simultaneously, their scintillating brilliancy being far beyond what I could have imagined to be possible.

Still earlier than Burbidge we may find in Sir John Bowring's "The Kingdom and People of Siam: with a Narrative of the Mission to that Country in 1855":

> How can I pass the fireflies in silence? They glance like shooting stars, but brighter and lovelier, through the air, as soon as the sun is set. Their light is intense, and beautiful in color as it is glittering in splendor--now shining, anon extinguished. They have their favorite trees round which they sport in countless multitudes, and produce a magnificent and living illumination; their light blazes and is extinguished by a common sympathy. At one moment every leaf and branch appears decorated with diamond-like fire; and soon there is darkness, to be again succeeded by flashes from innumerable lamps which whirl about in rapid agitation. If stars be the poetry of heaven, earth has nothing more poetic than the tropical firefly.

However, antedating even Bowring, the synchronous flashing of fireflies on the Meinam River had been described by another. In 1690 Engelbert Kaempfer left Batavia as physician to the Dutch Embassy to Japan. For some unexplained reason this embassy went to Nagasaki via Siam, and describing his return down the Meinam River from Bangkok in 1690, Kaempfer wrote:

> The Glowworms (Cicindalae) represent another shew, which settle on some Trees, like a fiery cloud, with this surprising circumstance, that a whole swarm of these Insects, having taken possession of one Tree, and spread themselves over its branches, sometimes hide their Light all at once, and a moment after make it appear again with the utmost regularity and exactness, as if they were in a perpetual Systole and Diastole.

Another account is taken from John Strachan's "Explorations and Adventures in New Guinea," London, 1888. This account is not strictly in line with that preceding, since it seems to be of synchronous movement rather than flashing, but at any rate it seems worth while to quote Strachan briefly. Of the man and his book no information is at hand. On page 38 he writes of fireflies observed near the Fly River:

> We sat gazing enraptured on a pyramid of living light, suspended, as it were, by threads of fairy gold. On a huge black walnut [?] tree there had gathered myriads of fireflies, which, moving through the dark foliage as if to the time of some enchanter's music, presented a scene of exquisite loveliness, which it is impossible to describe. As the fairy mass revolved, now up, now down, then round as to the measured time of a dance, my companion in ecstasy exclaimed "Captain, I would work twelve months for nothing to see such a sight as this."

The last notice that has come to light is distinctly of synchronous movement but it may not be amiss to quote it here. Burbidge on one of his trips to Kina Balu, the great mountain of Borneo, found the natives of Kalawat, a village near its base, raising bees in hives of hollow tree trunks set under the projecting roofs of their huts. Of these bees, Burbidge says:

> The kind of bee kept is very small, much smaller than that common in England, and I was much struck at the peculiar manner in which they wriggle their bodies simultaneously as they congregated in groups on the hive near the entrance.

The above accounts are those that have been found in the course of reading for other ends, but it is more than likely that a systematic search through large numbers of books of travel in the East Indies would bring to light other accounts. At any rate those given indicate that there is a "literature" even though small of this remarkable phenomenon.

SYNCHRONOUS RHYTHMIC FLASHING OF FIREFLIES

Buck, John Bonner; *Quarterly Review of Biology,* 13:301-314, 1938.

Summary. The evidence presented in the preceding pages indicates clearly that rhythmic synchronous flashing of fireflies is a very complex phenomenon, and that it differs greatly in different localities and species.

The four principal theories formulated to explain synchronous flashing are analyzed in detail: (1) The "accident" theory, which postulates that synchronism is initiated by accident, and maintained by the normal rhythm of flashing of the members of the swarm. (2) The "illusion" theory, which holds that the human mind deceives itself into seeing rhythmic synchronism in the few accidental coincidences which would (allegedly) be expected to occur in large number of fireflies flashing at random. (3) The "sympathy" theory, which attempts to relate synchronous flashing to the human "sense of rhythm." (4) The "leader" theory, which maintains that synchronism is produced by the mass of fireflies responding to the flash of a leader.

None of these theories is adequate to explain fully all the features of synchronous flashing observed. In general, a modified form of the "leader" theory appears to be the most satisfactory.

Experimental evidence is presented which indicates that in the American firefly, Photinus pyralis, synchronism is sometimes a by-product of the flashing signals used in mating, and is sometimes due to a follow-the-leader response.

SOUND-PRODUCING ANTS

Forbes, Henry O.; *Nature,* 24:101-102, 1881.

In Nature, vol. xxii, p. 583, which has lately reached me, I read a letter from Mr. Peal on sound-producing ants, and I can corroborate his observations. It is nearly two years since I noted this fact in a species of Polyrachis, which makes its papery nests on the under side of bamboo leaves. The noise, resembling very heavily-falling rain, is caused by the insect striking the leaf by a series of spasmodic taps, both with its head and with the extremity of its abdomen, which it inflexes while so doing.

I came on a second large brown species in September last in Sumatra. The noise which, as in the case of the Polyrachis, resembled heavy rain, could be heard a long distance off. What struck me most about this species was the singular synchronism of the movements. These ants were spread over a space perhaps a couple of yards in diameter on the stem, leaves, and branches of a great tree which had fallen, and not within sight of each other; yet the tapping was set up at the same moment, continued exactly the same space of time, and stopped at the same instant; after the lapse of a few seconds all recommended at the same

instant. The interval was always of about the same duration, though I did not time it; each ant did not, however, beat synchronously with every other in the congeries nearest to me; there were independent tappings, so that a sort of tune was played, each congeries dotting out its own music, yet the beginnings and endings of these muscial parties were strictly synchronous.

RHYTHMIC SYNCHRONISM IN THE CHIRPING OF CERTAIN CRICKETS AND LOCUSTS

Allard, H. A.; *American Naturalist*, 52:548-552, 1918.

The most remarkable instances of rhythmic synchronism I have ever heard have been afforded by the cone-headed grasshopper of the species Neoconocephalus exiliscanorus (Davis). A careful study of the intermittent notes--zeet-zeet-zeet-zeet--of these locusts was made on the edge of a swamp near Vinson Station, Virginia, late in August, 1917.

The characteristic habit of stridulation for individuals of this species is to produce a certain number of consecutive notes, followed by a brief pause. Usually, from fifteen to thirty consecutive notes are delivered before the pause takes place, then stridulaton is again resumed. Rehn and Hebard mention this habit as follows:

> The number of consecutive times without pause that this sound was produced were on one occasion counted, 26-14-20-17; usually on a warm evening an undisturbed singer would average about as above before ceasing a few seconds. The song is rapid, the sounds being emitted on warm evenings about 3 to the second.

When stridulation has become fairly established in a colony of these locusts for the evening, it is likely to be continuous, for if some singers cease their notes briefly, others take it up. Rehn and Hebard, in the publication mentioned above, have also noted this behavior and say:

> When near a colony of this species on favorable evenings after dark the air is vibrant with the sound; as several singers cease others take up the constantly rising and falling song, but at no very great distance the song is inaudible.

In the colony observed by the writer at Vinson Station, Va., three individuals which were somewhat isolated from the rest maintained a perfect rhythmic synchronism for many minutes at a time, including in this period many hundreds of consecutive notes. Now and then all three would be stridulating at the same time, then only two would produce their notes, yet the same per-

fect rhythmic synchronism was always evident. Sometimes all but one would cease to stridulate, then one or both of the others would again take up the rhythm with a precision that was marvelous. It did not matter how often one or another individual joined the chorus following a pause, the notes were always perfectly synchronous from the start and the rhythmic synchronism was maintained.

A representation of this perfect synchronism which was evident as the different "singers" took up the rhythm from time to time may be shown graphically with dashes as follows:

```
                    ----------
          ----------          ----------
   ----------                          ----------
----------                                      ----------
```

On several different nights I observed the same marvelous rhythmic synchronism in this particular group of individuals. Although other groups were "singing" elsewhere, it appeared that their notes were delivered independently of the rhythm of this particular group. From observations of the stridulations of other groups in this same colony, I am of the opinion that it is not unusual for these locusts to develop a rhythmic synchronism in small groups.

It would be interesting to know why some species of locusts and crickets possessing the intermittent habit of stridulation tend to develop a more or less perfect rhythmic synchronism while others do not. Although this is true of the two crickets, (Ecanthusniveus and Cyrtoxiphacolumbiana, I have been unable to note any synchronism in the chirpings of the common arboreal cricket, Orocharis saltator. Although large colonies of these crickets may often be heard in stridulation, each individual appears to stridulate in its own leisurely manner independently of its fellows.

THE KATYDID'S ORCHESTRA

Gould, George M.; *Science,* 2:380–381, 1895.

Possibly the phenomenon I am about to describe is well known to biologists, but to me it is unknown, and it seems so remarkable that it is worth recording. It is the only instance I know of in nature of any continued attempt at concerted harmony and measured time-keeping on the part of many animals. With all the musical or sound-making capacities of animals none seems to have much of an idea of measured time-beating, and in no instance known to me is there any attempt of large numbers to unite the individual notes into a common musical result. The universal fact of preserved individualism, and indifference to unisonal effect, is a noteworthy one when we consider the high degree of musical sense with which some animals are endowed.

Probably every person would express disgust at the idea of the stridulous noise of the Katydid being musical, and surprise at the suggestion that there is any rhythm or unison in many of them, but for weeks the fact has been all too apparent to my family for the purpose of sleep. Our house has been upon a mountain top in North Carolina, surrounded by a grove of trees, and farther away by woods upon all sides. So soon as the sun has set and twilight is advancing, the katydids in the trees begin to 'tune up.' The first notes are scattered, awkward, and without rhythm, but if no wind is blowing, thousands soon join in and from that time until daylight breaks there is no intermission. It is marvellous that the organs can withstand this continual rubbing for eight hours. By choosing out an insect close by and listening to it alone I have convinced myself that the same insect keeps at it at least for hours at a time. These raspings are seldom three at a time, as the popular name would imply, but are the result of usually four or five, sometimes six, distinct but closely joined movements. When united with a thousand others the disjunction of these tones is, of course, not perceptible, and they sound like a single note. In order to make my description clearer, let us suppose one thousand Katydids scattered through the trees to utter their several notes all at once, and call them Company A. Another thousand, Company B., at once answers them, and this swing-swong is kept up, as I say, all night. Company A's note is the emphatic or accented note, and is more definitely and accurately a precise musical note, whilst the note of Company B varies from one to five half tones below, the most conspicuous note being five. In the old-fashioned musical terms I learned as a boy, Company B's note is e.g., clearly and definitely do, while the note of Company B is either la, or more certainly sol, below. Not only is Company A's note more unisonal and definite, but it is firmer, more accented, and it seems to me that more insects join in this note than in the second. Careful observation has convinced me that no insect of Company A or Company B ever joins in the other company's note. The rhythm is usually perfect unless there is a disturbance by a breeze. A sharp gust upsets the whole orchestra and confusion results, but the measured beat is soon refound. In the instants of confusion one can detect the steady see-saw of certain ones, as it were, 'leaders,' or 'first violinists,' who hold the time-measure despite the wind, and who soon draw the lost notes of the others once more into the regular measure or beat. I do not mean to say that by diligent attention one may not at times detect individuals sawing out of time, stray fellows that are indifferent or careless, but the vast majority usually even seemingly without a single exception, if there is no wind or rain, thus swing along hour after hour in perfect time. I have counted the beats several times and find the number is always identical, 34 double beats or 68 single ones in 60 seconds. The effect of the rhythm upon the mind is not unlike that of the woodsman's cross-cut saw handled by two steady, tireless pairs of hands, although the Katydids give a larger volume of sound and the timbre is harsher. The queries arise: Is Company A composed of males and Company B of females? What function does

the orchestration subserve? Is there anything comparable to it among other animals?

PLANT LICE PUMPING IN UNISON

Tanner, W. Lee; *Science,* 72:560, 1930.

The phenomenon, "flashing of fireflies in unison," discussed on page 132 in the January 31 and on page 537 in the May 23 issues, prompts me to report some observations of the curious behavior of aphids. These insects appear to lift their bodies simultaneously in the act of feeding, sucking the sap of a host plant. Following the theory described in the second discussion mentioned above, the insect on seeing his neighbor rise for inspiration or pumping may himself rise to keep in step and thus all in a like way tend to synchronism. But apparently incidence of light is not a motive, as the writer has placed a cardboard screen around the sides of an individual feeding in proximity to many others and this individual kept in step right on with the others, and even when there was a pause all along the line he paused too, and on recommencement of the lifting or inflating of their bodies in the act of sucking again, the screened individual was found to be in step as before, although he could not see any of them. It was noted that a colony of the insects rising in unison on a branch somewhat removed from another collection rising in unison on another branch did not coincide in moment, i.e., each individual colony rose as a man, but did not rise in unison with the other colony.

SYNCHRONOUS RHYTHMIC MOVEMENTS OF FALL WEB-WORM LARVAE

Peairs, L. M.; *Science,* 45:501-502, 1917.

Recent discussion in Science of synchronous actions of certain animals, notably, flashing of fireflies, brings to mind a habit of the larvae of the fall web-worm (Hyphantria cunea), which seems to be of the same nature. Whether it has been noted in the literature or not I am unable to say; probably it has, as it is of common occurrence. In any event a short account of the habit may not be out of place in this connection.

Fall web-worm larvae, scattered over the outside of the web, may be seen, at intervals of from three to five minutes, to start a sharp rhythmic swaying from side to side, accomplished by raising the anterior half of the body to a semi-erect position, then moving it quickly, first to one side then to the other, through an angle of about ninety degrees. The movement is

started by a few of the larvae, but in a few seconds all the individuals in the colony will be moving in the same manner and in perfect unison. I do not have my notes at hand but, as I remember it, the movements were at the rate of about forty per minute and continued each time for from forty-five seconds to more than a minute. Even more suddenly than they start, the movements cease.

What the cause is for this strange habit is a puzzle. It seems to have nothing to do with spinning the web. Artificial stimuli failed to start them before the end of the resting interval although various means were tried. These included sounds, both musical and otherwise, made with various instruments, smoke and strong chemical odors, jarring and several other devices which suggested themselves at the time. Equally futile were attempts to stop the movements.

There seemed to be no leader, the swaying starting one time in one part of the colony or even in several parts at once, and again in some other part. Invariably, however, all would join in.

This habit was first observed by me several years ago, just how long I do not remember. It is associated with my earliest recollections of the insect. I have made more or less careful observation of it and taken notes several times, the first time in 1912. I do not think that I ever saw a colony that did not have the habit and I have had them in the laboratory every summer for several years. Observations of the habit may be made on colonies confined in the breeding cage or on those in the natural conditions. There seems to be no difference.

There is no doubt in my mind that this habit is an excellent example of synchronous rhythmic motion, not occasional or accidental, but habitual with the species. It may be well added to Mr. Craig's single, more or less doubtful, example, that of the chirping of crickets.

A CASE OF SYNCHRONIC BEHAVIOR IN PHALANGIDAE

Newman, H. H.; *Science*, 45:44, 1917.

A recent article in this journal by Wallace Craig on "Synchronism in the Rhythmic Acitivities of Animals" recalls to mind an observation that I made near Austin, Texas, in 1909. At the time of the observation I made some field notes from which the following description is taken.

While engaged in hunting various species of rock lizards I located a vast colony of "harvestmen," which I identified as belonging to the genus Liobunum, resting during the day on the under side of an overhanging shelf of rock on a precipitous hillside. In a somewhat circular area of nearly five feet in diameter the harvestmen were packed closely together in almost unbelievable numbers. I estimated that there were between one and two thousand in the colony. When I first saw them they were all hanging

from the ceiling, as it were, perfectly motionless, but when I came within about six feet of them they began a curious rhythmic dance. Without changing their foot-holds they raised their bodies up and down at the rate of about three times a second, and, curiously enough, the movement of the entire lot was in the most perfect unison. This performance was kept up for over a minute and then stopped gradually as though from exhaustion. I then poked a few of the nearest individuals with a stick and these immediately resumed the rhythmic up-and-down movement, which spread quickly over the whole group, but died down in less than half a minute. When I once more stirred up a few individuals they gave a few rhythmic responses, which stirred the whole colony again, but only slightly. After this a number of individuals began to crawl about and it was no longer possible to stimulate the rhythmic behavior.

When the colony was first seen it was noted that the long legs of neighboring individuals were closely interlocked and this mechanism was sufficent to account for the transmission of stimuli from one part of the colony to another. It should be noted especially that the rhythm was not perfectly synchronous at the beginning, but became so after a few seconds.

Possibly synchronic flashing in fire-flies may be explained as the result of a somewhat similar transmission of stimuli. One flash stimulates others, which at first might lag slightly; but soon a synchronism is built up in a limited region, such as one bush or one tree. Such a synchronism might be transmitted to a whole field.

. . . .

• Puzzles of Circadian Rhythms

DIURNAL RHYTHM IN CAVE CRAYFISH

Brown, Frank A.; *Nature,* 191:929-930, 1961.

Numerous investigations have suggested persistent diurnal physiological rhythmicity to be the rule among organisms, inhabiting the natural day-night environment. However, the only report known to me of results of a study of an animal fully removed for innumerable generations from such daily changes in light and temperature, the case of the cave crayfish. Orconectes pellucidus (Tellkampf), clearly established that these possess no difference in activity between day-time and night-time when kept under controlled conditions in the laboratory.

However, a re-examination of the published data in the light

of recent advances in our knowledge of persistent diurnal rhythmicity in metabolism, has indicated a statistically significant ($P < 0{\cdot}001$), 24-hr. rhythm of activity to be present with minimum activity about 9 a.m. and maximum about 7 p.m. Activity from 8 to 10 a.m. was less than half that from 6 to 8 p.m. This relationship was found on reconsideration of all the published data for complete days in the report already mentioned for animals kept in constant darkness. There was also a statistically significant daily rhythm in standard deviation of hourly activity values.

The crayfishes involved were collected from River Styx, Mammouth Cave, Kentucky, in March 1935 and November 1936. The cave is presumed to have been formed beginning in the late Pliocene or early Pleistocene, in other words, 1-4 million years ago. The crayfishes are highly adapted to cavernicolous life. They are pigmentless and have no eyes. Young are carried on the pleopods, reportedly chiefly during winters, thus implying an annual reproductive rhythm, despite essentially no annual temperature change. Prof. Orlando Park informs me that his temperature measurements of River Styx at various times of the year during the past 25 years have varied within the range $14 \pm 1°$ C. with no suggestion of an annual cycle.

The specific form and phase relations (times of maximum and minimum) of the diurnal fluctuation discovered resemble remarkably that of oxidative metabolism in the incubating chick egg are essentially the mirror image *in toto* of the diurnal metabolic rhythm of the fiddler crab and, treating a.m. and p.m. periods independently, are related directly or as a mirror-image to the daily cycle of potato plants and numerous other organisms investigated so far. These metabolic rhythms have been established to be extrinsic in character, and phase-locked to the Earth's modifications, correlated with aperiodic, weather-associated fluctuations in subtle geophysical factors in what had formerly been deemed conditions constant for the organisms.

This discovery of such a diurnal rhythm, especially with its particular characteristcs, in an ancient cavernicolous species long shielded from any diurnal light and temperature changes gives strong additional support to the concept of the universal presence of an extrinsic 24-hr. rhythmicity timed by the Earth's rotation, available for use in turn, as the timer for specific, adaptive diurnal patterns of surface-dwelling species, phased by light or temperature cycles, or adaptively dissociated from specific, overt physiological patterns in cavernicolous species such as the cave crayfish.

It seems probable that the reported annual reproductive cycle reflects an extrinsic annual metabolic rhythm such as has been reported even in unvarying light and temperature.

UNCLOCKLIKE BEHAVIOR OF BIOLOGICAL CLOCKS

Winfree, Arthur T.; *Nature,* 253:315-319, 1975.

An impressive diversity of organisms seems to have internalised the 24-h periodicity of this rotating planet. Exceptions include the prokaryotes, the bryophytes, most embryos and species evolved in soil, deep caves and the abyss. But in most other creatures, body temperature, enzyme activity, leaf movement, neural firing, mitotic index or other convenient observables typically persist in regular up and down periods of about 24-h, even in ostensibly constant conditions. There are called circadian rhythms to distinguish them from biological activities which persist in 24-h periodicity only so long as they are exposed to environmental cues at the same intervals. Brown and coworkers argue that even these circadian rhythms would stop or grossly change their period should the planet stop rotating, because they depend on the organisms sensing rhythmic fluctuations in subtle geophysical variables. Although some quantities, such as the electrical charge of the atmosphere, fluctuate diurnally in synchrony around the whole planet, many candidates for the role of covert driving rhythm are excluded by the persistence of circadian rhythms on a rotating platform at the south pole. Though it is impossible in principle to exclude all hypothetical drivers experimentally, most workers since the 1930's infer from the following generalisations that one ought instead to look for a mechanism of spontaneous oscillation within the organism.

The circadian period, though often precise to within an hour per month, seldom exactly matches the dominant period of geophysical variables. The circadian period depends on temperature, typically drifts a few percent within one day and is chemically adjustable. Mutants of altered period exist. The phase of a circadian rhythm is labile: with few exceptions (possibly including humans) it is permanently reset by a pulse of light or brief elevation of temperature within the physiological range or by temporarily withholding oxygen.

Moreover, most workers seem to think in terms of an ultimately cellular mechanism, because organ and tissue-level interactions do not seem to be crucially involved.

Protozoa and unicellular algae exhibit typical circadian rhythms (for example of bioluminescence, motility, mitosis and surface antigenicity). The leaves of plants, bits of cut leaf or of cultured leaf callus independently support circadian rhythms of movement or respiration. Isolated mammalian organs and even disaggregated cells persist in circadian variations of electrical activity, enzyme activity, respiration and so on.

Attempts to pinpoint the cellular mechanism of such long-period rhythmicity have led to models emphasising cellular membranes, cytoplasmic enzyme reactions, or sequential transcription of the nuclear message. The diversity of action spectra for rephasing by light suggests a corresponding diversity of molecular mechanisms, at least at the photoreceptor end of 'the clock'. (p. 315)

• Some Marvels of the Social Insects

THE NATURE OF THE INSECT COLONY

Howse, P. E.; *New Scientist,* 21:90-92, 1964.

Consider a colony of social insects -- in an ant-hill or a beehive, for example. It develops from a single pair of insects. A communal nest is built, and different castes arise among the offspring. Some are concerned with feeding the colony, another group with defense against intruders, yet others with reproduction; within these broad categories there is often a further subdivision of labour. This article is concerned with the long-disputed, and often romanticised question of how we should regard this elaborate natural organisation.

Can we consider the whole colony as a single organism in which the individual insects are analogous to the individual cells associated in the body of a plant or animal? To anyone encountering this idea for the first time it may seem very strange, but it is by no means new. A number of philosophers, including Herbert Spencer, Fechner, Adam Smith, and even the Greeks, expressed it long ago. Fechner even went so far as to suggest that the Earth, and then the universe, could be regarded as individual organisms.

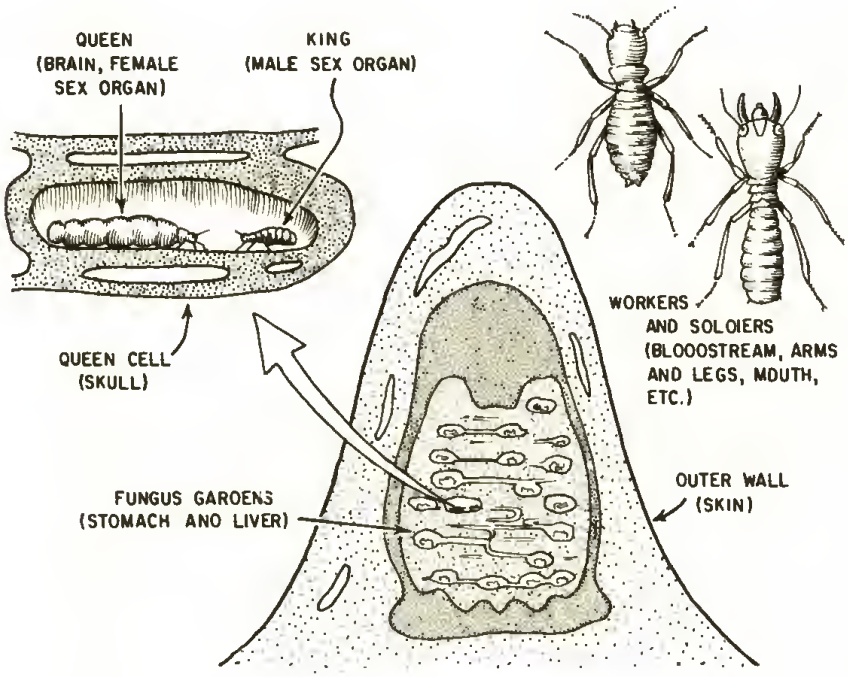
An alternative view to this "superorganism" theory was that the insect society was comparable in every way with human society. A particularly notable illustration of this idea is in the paper that Henry Smeathman gave to the Royal Society in 1781. This was the first report to an English audience of the mound-building termites of Africa. The Fellows were evidently a little incredulous about what they heard, and with reason. It was perhaps remarkable enough to be told that insects a few millimetres long could build towers exceeding the height of a man, but it must have needed an adaptable philosophical palate to swallow the statements that the tower contained nurseries, provision chambers, guard rooms, corridors, bridges, subterranean streets and canals, and a royal palace; and that among the versatile insects were to be found civilians, chemists, water diviners, well borers, architects, engineers and surveyors. Yet Smeathman was anticipated by Aristotle, who assumed that the insect colony was ruled by a king. This error was not corrected until the Dutchman Swammerdamm in 1670 sexed the king of the bees and found it to be a queen. Swammerdamm compared insect societies with the early Christian communities.

The view that the social insects are intelligent was common among the Greeks, and has been supplanted among biologists only in quite recent years. Forel (1848-1931) held that ants were intelligent communists, whilst Sir John Lubbock, a most thorough

researcher, held that ants had "a fair claim to rank next to man in the scale of intelligence."

In the aftermath of the Darwinian revolution, biologists began to realise that the insect colony had no raison d'etre except in reproduction and the survival of the species.

A particularly brilliant essay published in 1911 by Wheeler, the great expert on the social insects, drew the parallel between an ant colony and some kind of gigantic protozoan, in which the queen ant would represent the nucleus of the cell, the mass of ants the plasmodium (main body), and the files of foraging ants the pseudopodial extensions used in feeding (see Figure 1). After the first World War, books were published by two naturalist-philosophers, Maeterlinck and Marais, who put forward the super-organism theory, apparently independently, and philosophised on the "group soul" of the colony.



The termite colony as an organism (Fig. 1)

Whilst Maeterlinck believed that the insect colony was a composite organism, he was preoccupied with comparisons with human society, thus often repeating the views of former generations. In The Life of the Honeybee he proposed that a metaphysical "spirit of the hive" directed the activities of the bees. In The Life of the White Ant he wrote: "In the termitary the gods of communism

have become insatiable molochs . . . the species which appears to be the most highly civilised also appears to be the most enslaved and the most pitiable." Termites, he asserted, also possessed a soul or "occult power which governs". Associated with this "soul" was a "memory" that had accompanied termites throughout their evolution. This memory accounted for the behaviour patterns and "instincts" of the species.

> "Let us imagine", Maeterlinck wrote, "a man who should never have died, so it is with the termites; for these millions of years the same insect has gone on living, with the result that not a single one of its experiences has been lost. There has been no interruption of its existence, or disappearance of its existence, or disappearance of its memories; an individual memory has remained and this has never ceased to function or to centralise every acquisition of the collective soul."

The South African, Eugene Marais, who is also known for his work on the behaviour of apes developed his superorganism theory in his book The Soul of the White Ant in some detail. He firmly believed that the termitary was similar in every respect to the body of an individual organism, in fact was an individual organism. One day, he suggested natural selection might produce a termitary capable of locomotion.

Comparing the human body and the termitary, Marais makes the following main points.

(1) Both are controlled by "some mysterious power", and go through a cycle of birth and death.

(2) Both consist of a cellular structure covered by a thick skin which is undoubtedly "organic" in both cases.

(3) Under the skin there is a "living stream" in both entities. The white blood corpuscles and the soldiers have the same function of collecting around a break in the skin and preventing the intrusion of foreign organisms. The red corpuscles and the workers have the same function of repairing the damaged structures.

(4) In both, food is taken in through certain channels, carried to different centres, and finally distributed as food and building material. One source of food of the termites of the genus that Marais discussed (Macrotermes) is the fungus growing on balls of triturated organic material--the so-called "fungus gardens". Thus the fungus gardens become the stomach and liver of the composite organism.

The superorganism theory has been developed on more critically scientific lines by the American zoologist Professor A. E. Emerson. Emerson finds it unnecessary to believe in any psychic forces controlling the colony. He is not concerned to prove that colony and organism are the same, thus divorcing his concept from that of Marais; like Wheeler, he is concerned with general similarities, especially of a physiological nature.

To appreciate Emerson's superorganism theory (or Supra-organism as he prefers to call it), requires a rather detailed knowledge of animal physiology. Some of his analogies run as

follows: the transmission of excitability among the insects parallels the activity of the nervous system of the organism; the control of the development of the different castes by means of "external" hormones passed among the insects is similar to the control of tissue differentiation by hormones; and the specific odour of an insect colony results in an antagonism between individuals of different colonies, which is analogous to immunological reactions between tissues.

One of the useful outcomes of following Emerson's theory is that the extent of the comparisons that can be made reinforces the supposition that natural selection acts on the colony as a whole--rather than on the individual insects--in the same way that it acts on an individual non-social organism. Therefore, according to Emerson, features of the nest can be considered as equally important in identifying and classifying social insects. This idea can be very important, especially among termites where species are often notoriously difficult to separate. Another useful outcome is that, in considering the colony as an integrated whole, it is possible to appreciate evolutionary trends more clearly. Emerson believes that there is a general trend towards increased "social hemeostasis", by which he means the attainment of stable environmental conditions inside the nest.

Two examples may be given to illustrate this social homeostasis. The temperature of the honey-bees hive is always maintained between 34.5 and 35.5°C. Lindauer has found that if the temperature in the hive rises, the bees first increase the circulation of the air by fanning with their wings. If this is not enough to bring the temperature down they bring water into the hive, which is dispersed on the cells as small droplets and left to evaporate.

Similarly, Luscher has shown that an "air-conditioning" system is present in the mounds of the African termite species Macrotermes natalensis, whereby hot air arises from the centre of the nest and is cooled in large canals near the surface of the mound. The air is in constant circulation and some gas exchange takes place near the surface of the mound; an effective "respiratory system" is therefore present.

When we consider the superorganism theory, it is clear that general similarities and evolutionary trends are thrown into a new perspective. However, when dealing with an analogy it is most dangerous to deduce what is not immediately evident or general similarities can be upheld. For example, if a lung and the circulatory system in a Macrotermes mound were seen to fulfill the same apparent general function, one could not assume that the specific functions were the same, of the same importance to organism and superorganism, or carried out in the same way. Similarly one could not say that the termites building the air canals were influenced by the same factors or were behaving in the same way as the cells that build the lung tissues. From this it follows that Emerson's use of nest structure in identification of species is open to some doubt. According to Dr. W. V. Harris in his recent book Termites, their recognition and control, nest structure is a compromise between the specific plan and

environmental factors such as the climate and the nature of the material available for building.

The superorganism theory therefore in a general way helps us to understand the organisation of the insect society. But we should recognise that the use of the analogy can have its pitfalls when one gets down to details.

• Neuroses and Inhibitions

NEUROTIC BEHAVIOR IN ANTS

Morley, Derek Wragge; *Nature*, 162:74–75, 1948.

While investigating ant learning with maze apparatus, behaviour was observed in one individual which is similar to that described by Cook and Maier as psychopathic or 'neurotic' behaviour in rats. What are thought to have been early symptoms of similar behaviour were later observed in a virgin female of the same colony of Formica fusca Latr.

The particular worker ant concerned was one of several which had already several times successfully completed this maze. On completion of the run, the ants were normally picked up in the experimenter's fingers (an act to which they were habituated and against which they did not struggle) and returned to the observation nest in which they were kept; that is, in effect, to their fellow-workers and a female which was at that time laying eggs.

On this occasion the worker ant had already successfully run the maze almost immediately after being placed in the starting chamber, making few errors and quickly correcting those which it did make. It was then placed back in the nest; but about five minutes later was once more taken from the nest and replaced in the starting chamber. It at once exhibited slight jerking movements of the legs and antennae, but soon commenced to run the maze, moving very quickly and without error or returning, until halfway through, when it missed one of the solution-turns and followed an error-path until it reached its blank end. At first it made no movement back along the error-path, but with rising physical excitement and an increasing jerking of the limbs and antennae constantly touched the three glass sides of the maze in the immediate vicinity of the blank end. After about a minute it returned along the error-path until near its commencement, where for a few seconds it remained standing while exhibiting constant and increasing limb and antennae jerking. It then turned until facing down the error-path, and still jerking moved with a staggering motion backwards (the limb-jerking, which

appeared to be completely out of its control, made ordinary movement a matter of difficulty) and almost at once commenced to circle, still backing, in a small area of the intersection of the tracks. The rapidity of the circling increased and the jerking became even more pronounced, and a few seconds later the ant was rescued, since it was, for other reasons, of experimental value. It was quiescent while held firmly in the experimenter's fingers, but was placed under a tap of cold running water for about ten seconds before being returned to the nest. In the nest it still exhibited slight tremors of the legs and antennae, but soon ran about normally. On the next day the ant ran the maze successfully with some errors, but no sign of its earlier behaviour. On one following occasion, however, it showed pronounced jerking movements of limbs and antennae after making the same error, and commenced trying to clean itself. If was, however, at once rescued and returned to the nest. This particular individual is one of the most active workers of that particular colony, and also proved to be among the three workers of the colony (which consisted of 23 workers and two females) which were most successful at maze-learning.

A virgin female which was originally taken from the same colony but which had been isolated for nearly two months exhibited similar marked jerking movements of antennae and legs on being placed in a strange plaster nest (she had previously been in a glass one containing earth). These movements soon stopped, however, when another virgin female was placed in the nest with her about one minute later.

I know of no previous records or accounts of similar behaviour in ants.

WASP-STINGS

Harris, W. T.; *Science*, 11:62, 1888.

I have read with interest the items recently published in Science on this topic. Forty years ago, when a lad at school in Killingly, Conn. (in that part of the town at present known as Putnam Heights), I learned from schoolmates that any wasps could be handled without danger if one held his breath. I saw the experiment successfully made by many of my fellows, and ventured to make it myself with like results. Since that time scarcely a year has passed without my repeating the trial on wasps that have come in my way. I have never been stung except when I have forgotten myself, and allowed myself to inspire or expire the breath. Sometimes, after throwing the wasp violently away, I have been stung, because it has clung to my finger, and, not observing it, I had breathed. Ordinarily I notice after an experiment a slight feeling of numbness on the part of my hand where the wasp has attempted to sting me. I

am accustomed to judge by this feeling whether the wasp was one of the stinging kind. As to the cause, I do not know of any. But many scientific persons have unscientifically refused to believe my statements, or to test them by experiment, because I could not answer their question, 'How do you account for it?' Whether the forced suspension of breathing paralyzes the nerves near the surface of the skin,--whether it stops the capillary circulation near the surface,--or whether its effect is something altogether different, I do not know. Nor do I see exactly how a paralysis of the superficial nerves, or an influence on the surface circulation, would prevent the poison from giving pain after commencing respiration again, provided that the wasp has succeeded in piercing the outer layer of the skin: for if the poison is exuded from the stinger, as I have sometimes seen it, it would act effectively upon removal of the paralysis when breathing is resumed. But my experience seems to lead to the conclusion that the poison does not penetrate at all during the suspension of the breath, but is left on the surface of the skin, and produces only the effect of a faint numbness after its effects begin to be felt through the outer coating. I do not take up this subject as one who has conducted any careful scientific experiments on it. My account of the matter may, however, help, like former articles in Science, in interesting experts in physiology to make genuine scientific experiments. One may hope that something important will be discovered in regard to the effect of forced suspension of the breath upon the nerves of feeling, the capillary circulation, or the resistance of the skin to penetration.

• Artifacts and Engineering Feats

DEFENSIVE USE BY AN INSECT OF A PLANT RESIN

Eisner, Thomas, et al; *Science,* 184:996-999, 1974.

Abstract. Larvae of the sawfly Neodiprion sertifer (Hymenoptera: Diprionidae) when disturbed, discharge an oily oral effluent essentially identical chemically to the terpenoid resin of its host plant (Pinus sylvestris). The resin is sequestered by the larva upon feeding, and stored in two compressible diverticular pouches of the foregut. The fluid is effectively deterrent to predators. The defensive use by an insect of a plant resin provides an instance of secondary utilization by a herbivore of the protective chemical weaponry of its host.

CALCIUM CARBONATE IN TERMITE MOUNDS

Watson, J. P.; *Nature,* 247:74, 1974.

The presence of appreciable quantities of calcium carbonate in termite mounds on non-calcareous soil has intrigued pedologists for many years. Milne, for example, found a termite mound with 7% calcium carbonate and estimated that it contained about 2 t of calcium carbonate excluding the hard limestone (33% $CaCO_3$) base of the mound. The soil below the base of a termite mount may also be calcareous. The soil underneath one termite mound in an area of non-calcareous soil was found to have a mean of 1.7% calcium carbonate to a depth of 6 m, or about 20 t of calcium carbonate.

The task of finding the primary methods of calcium carbonate accumulation has been hindered by the fact that termite mounds are complex systems involving biological and physical processes. Milne and Pendleton suggested that the most probable biological method was the collection of food by termites and formation of calcium carbonate from the mineralised residues. Other biological methods of calcium carbonate accumulation that have been advanced are as follows: (1) exchangeable calcium in soil collected incidentally by termites with their food; (2) exchangeable calcium in soil collected purposely by termites requiring an alkaline environment; (3) calcareous material collected from below the depth of soil said to be noncalcareous; and (4) calcium-containing groundwater brought up by termites. The physical methods proposed all involve evaporation of water containing calcium bicarbonate from a termite mound as the means of accumulating calcium carbonate, but they differ in the source and mode of entry of water into the mound. Thus Milne suggested that water moves upward into the termite mound by capillary action. Den Doop considered that the water evaporating from the mound was derived from surrounding land in the wet season. Boyer too describes how water from perched water tables drains laterally and collects underneath mounds of Macrotermes subhyalinus. Finally, Hesse found an association between calcareous Macrotermes mounds and poorly drained soil which led him to state that saturation of the base of a termite mound by groundwater was a prerequisite of calcium carbonate accumulation.

The early ideas on the mode of accumulation of calcium carbonate in termite mounds were considered by Pendleton who said of some of them that they "seem too fantastic to repeat". He came to the conclusion that there was no known method by which termites or pedological processes could bring about the observed accumulation of calcium carbonate in termite mounds. There seems therefore to be a need for a different approach to the problem. A weakness of all proposals made hitherto is the long time that it would take to accumulate large amounts of calcium carbonate.

I have constructed a hypothesis that could account for more rapid accumulation of calcium carbonate in a termite mound.

The hypothesis is that calcium carbonate can accumulate in termite mounds by means of a two-stage process.

(1) Elevation of the pH status of the termite mound above that of the pH_c of the groundwater. An increase in the pH of a mound is brought about by termites importing vegetation which is decomposed to release exchangeable bases. Retention of exchangeable bases in the termite mound is facilitated by a low degree of leaching because a mound sheds water.

(2) Saturation of the base of the termite mound by groundwater containing calcium bicarbonate which precipitates as calcium carbonate according to the equation

$$Ca^{2+} + HCO^-_3 \rightarrow CaCO_3 \downarrow + CO_2 \uparrow$$

The tendency for calcium carbonate to precipitate from irrigation water on contact with soil is a well known alkalinity hazard of irrigation schemes, and it has been found by Bower to be related to a modified Langelier saturation index:

$$\text{Modified saturation index} = pH_s - pH_c$$

where pH_s is the pH value of the soil and pH_c is the theoretical pH value that the water would have if in equilibrium with calcium carbonate. Bower states that precipitation of calcium carbonate occurs if the index is positive. Moreover, the percentage of the applied bicarbonate that precipitated was found to be highly correlated with the value of the index.

This pH-dependent precipitation of calcium is related to the presence of bicarbonate and carbonate ions and does not occur with other common anions in groundwater, namely, chloride and sulphate. Formation of calcium carbonate in termite mounds in some regions and not in others may therefore perhaps, be explained by differences in the anion composition of the groundwaters.

The rate of accumulation of calcium carbonate by means of the pH-bicarbonate process outlined here could be relatively fast in situations where groundwater of low pH_c flows past the base of a Macrotermes mound of high pH value. Consequently if this hypothesis is correct, it will be unnecessary to assume that termite mounds have existed for the thousands of years that it would take to accumulate large amounts of calcium carbonate by evaporation of water.

'ANT BUTTER'

Morley, B. D. Wragge; *Nature,* 155:517, 1945.

Recently my father-in-law, Mr. Platonoff, asked me whether I could give him any information concerning a soft pliable yellow substance which he had found in the nests of wood ants in the forests of the U.S.S.R. The substance was in the form of small lumps and was sought by the peasants, who termed it 'ant butter'

(Mouroveenue Maaslo). He did not know for what purpose the peasants used it. It is said that bears seek the 'ant butter', and a disturbed ants' nest used to be taken as an indication of their presence.

This substance was undoubtedly the ant 'incense' of Linnaeus. The occurrence of ant 'incense' ('wirak') was first recorded by Linnaeus, who described ants collecting resin from juniper bushes and stated that the peasants gathered the pieces of resin from the nests of Formica rufa L. and used it as incense, terming it 'wirak.' Wheeler points out that this is probably derived from the German "Weihrauch," incense. De Geer both records and also figures the resin, and states that the wood ants collect it from the pine and fir trees. He says "... the ants collect it [the resin] in little masses of irregular form and varying size, of which the colour is sometimes white, sometimes yellow, and often of a dirty white; the substance is more or less hard except for that which has been amassed fairly recently...."

Reaumur experimented by placing bits of resin in ants' nests, "to see whether they will take on the peculiar quality that makes them resemble myrrh or amber". He also had some correspondence with De Geer on the subject.

Conisthorpe states that "In Northumberland and Scotland these ants (Formica rufa, L.) collect huge quantities of yellow resin---'ant amber'---from the fir trees; I have seen nests full of it."

I have often observed wood ants climbing pine trees in the New Forest (presumably to collect resin) and coming down with their crops full, but have never actually seen the 'ant amber.'

The practice of collecting resin seems to be a widespread habit among the wood ants of the coniferous forests and woods of Europe, but I know of no record of this behaviour from America, nor has anyone yet determined for what purpose, if any, the ants use the resin.

MINERALOGICAL ANTS

Saunders; *Zoologist,* 18:7071, 1859.

Before continuing the account of our journey I must offer a remark connected with an observation I made in the desert. When traversing certain parts of the North-American Steppes and Deserts I have frequently observed ant-hills formed exclusively of small stones of the same mineral species, as, for instance, small grains of quartz. In one part of the Colorado Desert the hills of these mineralogical ants consisted of heaps of small shining fragments of crystallized feldspar, chosen by these little animals from the various components of the coarse sand of these parts. The last time I was at El Paro a North-American driver came to me and inquired the value of a small bag of garnets he possessed. On my asking in what place they had been found I heard that these stones--imperfect crystals of red transparent

garnets--were the material of which the ants build their hills in the country of the Navago Indians, in New Mexico, and that he knew a place where any quantity of them might be collected. These remarks may perhaps not be uninteresting to the question relating to the gold-seeking ants of Herodotus.

USE OF A SELF-MADE SOUND BAFFLE BY A TREE CRICKET

Prozesky-Schulze, L., et al; *Nature,* 255:142-143, 1975.

Field observations and sound recordings of several South African species of Oecanthidae were carried out during the summer and autumn of 1973-74 to augment studies of the acoustic behaviour of the tree cricket performed abroad. We report that Oecanthus burmeisteri uses a leaf as a sound baffle to increase the intensity of its calling song by pressing its tegmina against the edges of a pear-shaped hole gnawed into the leaf. At least two more South African chirping Oecanthus species use a leaf baffle just as efficiently as O. burmeisteri.

. . . . .

A BRIDGE BUILT BY RED ANTS

Anonymous; *Scientific American,* 64:185, 1891.

The following remarkable story, told by an eye witness, is entitled to a place among the instances of intelligence among the lower animals. A cook was much annoyed to find his pastry shelves attacked by ants. By careful watching it was discovered that they came out twice a day in search of food, at about 7 in the morning and 4 in the afternoon. How were the pies to be protected against the invaders?

He did not have long to wait, for at 6:50 o'clock he noticed that off in the left hand corner of the pantry was a line of ants slowly making their way in the direction of the pies. They seemed like a vast army coming forth to attack the enemy. In front was a leader, who was larger than any of the others, and who always kept a little ahead of his troops.

They were of the sort known as the medium sized red ant, which is regarded as the most intelligent of its kind, whose scientific name is Formica rubra.

About 40 ants out of 500 stepped out and jointed the leader. The general and his aids held a council and then proceeded to examine the circle of molasses. Certain portions of it seemed to be assigned to the different ants, and each selected unerringly the point in the section under his charge where the stream of molasses was narrowest. Then the leader made his tour of inspection.

The order to march was given, and the ants all made their way to a hole in the wall in which the plastering was loose.

Here they broke rank and set about carrying pieces of plaster to the place in the molasses which had been agreed upon as the narrowest. To and fro they went from the nail hole to the molasses until, at 11:30 o'clock, they had thrown a bridge across. They then formed themselves in line and marched over, and by 11:45 every ant was eating pie.

FOG CATCHMENT SAND TRENCHES CONSTRUCTED BY TENEBRIONID BEETLES . . . . .

Seely, Mary K., and Hamilton, William J., III; *Science*, 193:484-486, 1976.

Abstract. Three species of coastal Namib Desert Tenebrionid beetles (Lepidochora) build trenches on desert sand dunes. Trenches are constructed perpendicular to fog winds and concentrate moisture during fogs. The beetles return along the ridges of the trenches extracting water from them. The water content of a population of these beetles increased by 13.9 percent during one fog.

• Insect Husbandry and Agriculture

FUNGUS-GROWING ANTS

Weber, Neal A.; *Science,* 153:587-604, 1966.

Summary. Fungus-growing ants (Attini) are in reality unique fungus-culturing insects. There are several hundred species in some dozen genera, of which Aeromyrmex and Atta are the conspicuous leaf-cutters. The center of their activities is the fungus garden, which is also the site of the queen and brood. The garden, in most species, is made from fresh green leaves or other vegetal material. The ants forage for this, forming distinct trails to the vegetation that is being harvested. The cut leaves or other substrate are brought into the nest and prepared for the fungus. Fresh leaves and flowers are cut into pieces a millimeter or two in diameter; the ants form them into a pulpy mass by pinching them with the mandibles and adding saliva. Anal drop-

lets are deposited on the pieces, which are then forced into place in the garden. Planting of the fungus is accomplished by an ant's picking up tufts of the adjacent mycelium and dotting the surface of the new substrate with it. The combination of salivary and anal secretions, together with the constant care given by the ants, facilitates the growth of the ant fungus only, despite constant possibilities for contamination. When the ants are removed, alien fungi and other organisms flourish.

A mature nest of Atta sexdens may consist of 2000 chambers, some temporarily empty, some with refuse, and the remainder with fungus gardens. Thousands of kilograms of fresh leaves will have been used. A young laboratory colony of Atta cephalotes will use 1 kilogram of fresh leaves for one garden. The attines are the chief agents for introducing organic matter into the soil in tropical rain forests; this matter becomes the nucleus for a host of other organisms, including nematodes and arthopods, after it is discarded by the ants.

One ant species cultures a yeast; all others grow a mycelium. In the higher species the mycelium forms clusters of inflated hyphae. Mycologists accept as valid two names for confirmed fruiting stages: Leucocoprinus (or Leucoagaricus) gongylophora and Lepiota n.sp.

• Insects and Magnetic Fields

A MAGNETIZED SPIDER

Anonymous; *Popular Science Monthly,* 13:126, 1878.

In a communication to the Academy of Natural Sciences of Philadelphia, Dr. John Vansant treats of the influence of magnetism on living organisms, and describes at length one experiment with a spider, which was killed by the magnetic emanation. The magnet employed was a small steel one, of the U-shape, the legs of which were about two and one-half inches long by one-half inch wide and one-sixth inch thick, the distance between the poles being about one-quarter inch. Having noticed a small spider actively running along the arm of his chair, he brushed it off upon the carpet, where it began to run, but was somewhat impeded by the roughness of the fabric. He now slid the magnet along the carpet, following after the spider, till it was between the poles. The animal almost instantly stopped, and in a few seconds was motionless; but, at the end of two or three minutes, it began slowly to move its legs and elevate and depress its head. At the end of five minutes the spider was quite still. After the lapse of ten minutes

Dr. Vansant covered both spider and magnet with a tumbler. On the expiration of two hours, he removed the glass and observed the spider with a magnifying-lens. It was apparently dead. The author states that he has killed spiders and other small animals, as worms and insects, as well as some plants, by magnetism, at various times during the past eight years, but never before succeeded in destroying the life of a spider so quickly, and without touching it frequently, though lightly, with the magnet. In the present instance he did not touch the animal at all.

• The Bee Communication Controversy

THE BEE LANGUAGE CONTROVERSY

Krebs, John R.; *Nature,* 258:109, 1975.

Karl von Frisch's Nobel Prize winning discovery of the dance language of honey bees is one of the classics of biology. Over many years von Frisch collected evidence to show that when a successful foraging bee returns to the hive it is able to recruit other workers by communicating the precise distance and direction to the food source, encoding this information in the abstract symbolism of a 'dance' performed on the hive surface. The distance is encoded in the speed of dancing and the direction by the orientation of the figure-of-eight dance in relation to gravity. This remarkable feat places honeybees second only to man and perhaps chimpanzees in the use of abstract symbols in transmission of information.

About eight years ago, Adrian Wenner pointed out that two of von Frisch's best known experiments were not as convincing a demonstration of the dance language as most people had thought. In these two studies, known as the 'step' and 'fan' experiments, von Frisch showed that naive recruit bees would go specifically to experimental food dishes indicated by the dances of experienced foragers, and ignore identical nearby control dishes. In the fan experiment the control and experimental dishes were at the same distance from the hive but in different directions, and in the step experiment the dishes were in the same direction but at different distances. Wenner correctly pointed out that not only is the experimental dish in these experiments unique in being the one indicated by the dance, but also it is the one towards which experienced foragers are flying. The recruits could either be using the dance information or simply watching where the foragers go. Wenner eliminated this confounding variable by training equal numbers of foragers from a separate

(control) hive to go to each of the control dishes, and he now found that recruits from the experimental hive no longer preferred the dish indicated by the dance. Wenner's conclusion from this and other experiments was that recruits may be stimulated to leave the hive by the dance, but that they normally search for food by smell, using the smell of the dancing foragers as a guide.

Naturally enough, Wenner's results aroused enthusiastic criticism, followed by new experiments supporting von Frisch's story, with further counterclaims by Wenner and his colleagues, culminating in a headline in Nature 241, 171; 1793) suggesting that von Frisch's data could be explained without recourse to the dance language theory. One valuable consequence of Wenner's criticisms was that people devised more rigorous tests of bee dance communication, and the latest of these studies, by J.L. Gould (Science, 189, 685; 1975) finally shows beyond doubt that bees use the dance information.

. . . . .

• Insect Behavior and Astronomy

FIFTH AS MANY INSECTS AT TIME OF FULL MOON

Anonymous; *Science News Letter,* 59:377, 1951.

At the time of full moon, nocturnal insects are only about a fifth as plentiful as during the dark of the new moon.

This seeming effect of the moon is reported by two entomologists, C.B. Williams and B.P. Singh, working at Britain's Rothamsted Experimental Station.

For many years those who gather insects for scientific purposes have known that if a bright light is used to attract insects at night, the catches are considerably higher near the period of the new moon than near the full moon. While catches at full moon by other methods were also believed to be poor, there was the chance that the low catches in a light trap might be due to a lowered relative luminosity and a lowered attractiveness of the trap at full moon.

Entomologists Williams and Singh last summer and fall made a test by sucking up insects at night with a strong electric fan, so the light intensity would not affect the catches.

Careful counts of the flies and other kinds of insects captured during five complete lunar cycles showed that the catch in the new moon week was five times that in the full moon week.

. . . . .

LUNAR RHYTHM IN THE EMERGENCE OF AN EPHEMEROPTERAN

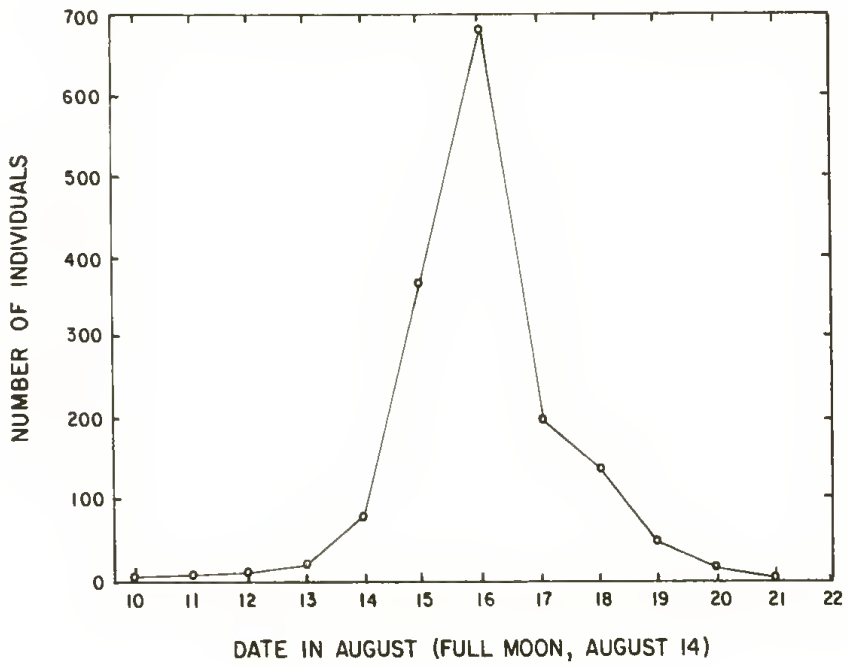
Hartland-Rowe, R.; *Nature,* 176:657, 1955.

In a paper published in 1927, Hora suggests that the swarming of some Ephemeroptera is related to the lunar cycle. Evidence has been obtained which suggests that Povilla adusta Navas, a mayfly widely distributed in Central and Southern Africa, shows such a rhythm of emergence in Uganda. The interest of this lies in the fact that very few examples are known of lunar rhythms in non-marine animals.

It was suspected late in 1953 that adults (sub-imagines and imagines) of Povilla adusta appeared in large numbers only at about the period of full moon, and analysis of the dates of twenty-two swarms observed between March 1953 and April 1955 at Kaazi (twelve miles from Kampala) and Jinja, both on Lake Victoria, and on Lake Albert, shows that such swarms only occurred within five days of full moon, with the greatest number of swarms on the second night after full moon. On three occasions swarms were recorded simultaneously at Jinja and Kaazi, which are fifty miles apart.

The adults come to light between 7:30 and 9:30 p.m. (East African Standard Time) and are not seen at other times; individuals only live for about one hour. A light-trap technique was therefore used when sampling at Kaazi during August 10-21, 1954. Of the 1,521 specimens obtained on these twelve nights, 680 (44 per cent) came on the second night after full moon.

. . . . .



Moon-related emergence of an African Mayfly

LAB-REARED CRABS RECALL THE EBB AND FLOW

Anonymous; *New Scientist,* 37:149, 1968.

Past experiments have shown that a daily or circadian rhythm can be induced in laboratory-reared organisms which have never in fact experienced any 24-hourly rhythm in their normal environmental conditions. Drs. B.G. Williams and E. Naylor, of University College, Swansea, have now shown that the same thing can be done with a tidal rhythm using the shore crab, Carcinus maenas (Journal of Experimental Biology, Vol. 47, p. 229).

They reared crabs from the egg stage under normal laboratory conditions of light and darkness, but with no tidal cycle; they then measured their activity in a steady, but dim, light before and after cooling them to 4°C for 15 hours--a procedure known to elicit a tidal rhythm in crabs removed from the shore.

Analysis of the crabs' activity levels initially revealed a circadian rhythm with a single recurring peak, at intervals of between 23 and 26 hours. After the cold treatment, however, they showed a definite tidal rhythm with two peaks, at 12 to 13 and 24 to 26 hours, respectively.

The ability to respond with a tidal rhythm of activity must thus be a hereditary feature of individuals of Carcinus maenas from tidal waters; it would be interesting to see if any evolutionary traces of this habit persist in populations from non-tidal areas.

ANT-LION LARVA'S LUNATIC RHYTHM

Anonymous; *New Scientist,* 43:369, 1969.

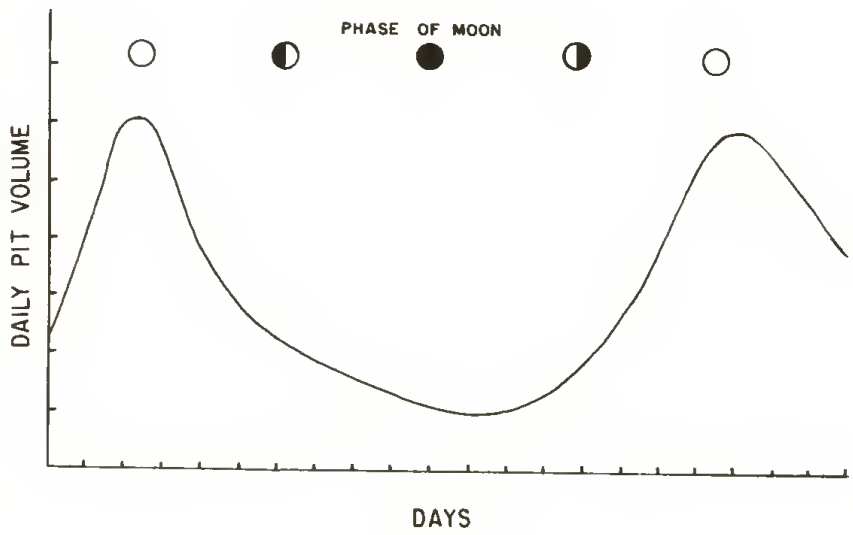
An interesting twist to the currently popular investigations of biological rhythms has come recently from the results of two zoologists from Rhodes University, Grahamstown, South Africa, G.J. Youthed and V.C. Moran have been studying the pit-digging larvae of the ant-lion, Myrmeleonis obscurus. Although they have found a well marked lunar rhythm in these insects, careful analysis has forced them to the conclusion that it is, biologically speaking, completely pointless (Journal of Insect Physiology, vol. 15, p. 1259).

Youthed and Moran used the volume of the pits which these larvae dug to measure their activity level. The activity showed a regular 28 day (or synodic) monthly periodicity, with a major peak at the times of full moon, and with secondary peaks at new moon. The larvae also showed a lunar-day activity rhythm, with a peak four hours after moonrise. It is probably this rhythm, interacting and "beating" with a normal solar-day rhythm, which gives rise to the synodic or lunar-monthly rhythm. The rhythms are clearly driven by an internal clock, since they continued for several days in constant light or constant darkness, and moon-

light could act as a phase-setting factor (or zeitgeber) for both rhythms.

However, whereas solar-day rhythms clearly have some biological value, there is no immediately obvious function for a lunar-daily or lunar-monthly rhythm. Most lunar periodicity so far observed in insects simply arises from a direct response to moonlight (as, for example, in the biting activity of the tsetse fly). One possible reason for the ant-lion's rhythm is that there is more potential prey wandering around on moonlight nights, but the South Africans found no evidence to support this idea--nor for an alternative possibility that the increased activity was involved in spacing the larvae out, since the activity of the larvae was not accentuated by experimental crowding.

Light is known to be the zeitgeber for the ant-lion's solar-day rhythm, so the biological clock involved must be light sensitive. Youthed and Moran's final conclusion, therefore, is that the lunar-day rhythm, and the resultant synodic monthly rhythm, is an accident due to fortuitous phase-setting of the solar day clock by the Moon (since the full moon rises around sunset time), and that it has no functional significance at all.



Ant lion pit volume versus lunar phase. (Adapted from Natural History, *87:64, December 1978)*

MYSTERIOUS MONTHLY RHYTHMS

Palmer, John D., and Goodenough, Judith E.; *Natural History,* 87:64-69, December 1978.

Another rhythm for which there is no apparent adaptive purpose occurs in a freshwater planarian, a small flatworm. Researchers accidentally discovered that when a population of flatworms is maintained in the laboratory, the angle each worm assumes in swimming away from a lighted area varies with the phase of the moon. A very simple apparatus, a great deal of patience, and a high tolerance for repetitive observations are all that are required for the study of this spatial orientation rhythm. The apparatus consists of a grid marked off into 5° sectors, with a small dish of pond water placed over it. A flatworm is positioned in the dish at a starting point, and the angle it assumes in escaping from two beams of light is measured by noting the sector it is in as it crosses a finish line. When many worms are tested in this way, and the average angle assumed by all the worms plotted for each day of the month, the angle at which the animals turn away from the lights is greater at one time of the month than at another. This rhythm also persists in the absence of all common environmental time cues, meaning that planarians also possess a monthly clock. (pp. 67-68)

• Mimicry of Behavior

FLATTID BUG FLOWERS

Wilson, Colin; *The Occult,* Random House, New York, 1971, p. 126.

In African Genesis, Robert Ardrey mentions an example that seems to me a conclusive argument against total, uncompromising Darwinism: the flattid bug. He was standing with the anthropologist L. B. S. Leakey, looking at a coral-coloured blossom like lilac. Leakey touched the twig, and the flower dissolved in a swarm of tiny insects. A few minutes later the insects re-settled on the twig, crawled over one another's backs, and once again became a coral-coloured blossom, a flower which does not exist in nature. Some of the insects were green; some were half green and half pink; others were deep coral; they arranged themselves so as to look like a flower with a green tip.

Now Darwinian selection can explain most examples of 'imitation' in nature; for example, the stick bug, which even has thorns on its back. Random mutation produces a creature that looks rather

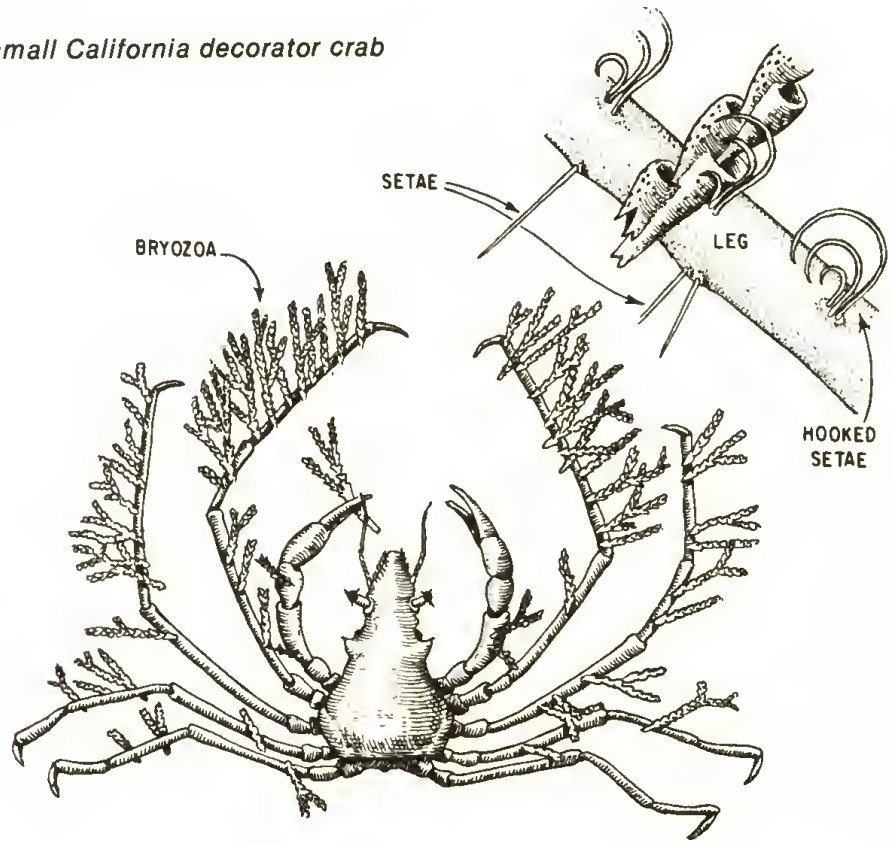
like a twig, and it survives better than its brothers who look more appetising. And as birds continue to eat the non-imitative bugs, nature 'polishes up' the resemblance. But how can that principle be applied to a whole community? 'Natural selection' works in terms of individuals; we cannot imagine a whole community created by some mass accident of the genes, and then learning, accidentally, to imitate a flower. But if we assume that the flattid-bug community is, in a sense, a single individual, a single mind, the problem becomes less complicated.

And if we make this assumption, then we must also drop the idea that the genes cannot be influenced by telepathy. The alternative is to imagine thousands of flattid-bug communities teaching themselves to imitate flowers, but being wiped out in the next generation as their children fail to inherit their colouring, until one day nature kindly takes a hand and allows the trick to become inheritable. (p. 126)

DECORATOR CRABS

Wicksten, Mary K.; *Scientific American,* 242:146–154, February 1980.

A small California decorator crab



Many of the thousands of species of crabs have lines, spots or patterns that serve to break up the outline of their carapace, or upper shell. A few species have a pair of modified back legs with which they pick up objects such as shells or sponges and hold them on or over their carapace. The crabs most adept at camouflage, however, are those that deliberately select bits of material from their environment and attach them to various parts of their shell. Called masking crabs or decorator crabs, these animals can become so heavily encrusted with strands of red or brown algae, pieces of fluffy branched bryozoans, segments of feathery brownish yellow hydroids and brighly colored sponges that an experienced underwater naturalist can sit on a decorator crab before realizing that it is there. (p. 146)

AGGRESSIVE MIMICRY IN PHOTURIS: FIREFLY FEMMES FATALES

Lloyd, James E.; *Science,* 149:653-654, 1965.

Abstract. Firefly females of the genus Photuris, long known to be carnivorous, attract and devour males of the genus Photinus by mimicking the flash-responses of Photinus females. Although suspected, this behavior had not been observed previously.

• Remarkable Adaptations in Insect Behavior

THE HUMAN BOT FLY

Curran, C. H.; *Natural History,* 44:45-48, 1939.

The human bot fly is one of the most remarkable animals in the world. Although it is only a lowly insect and is presumably incapable of reasoning, it does things that would do credit to human ingenuity. As you will see, the insect is well-named the "human" bot fly (Dermatobia hominis), for its larvae develop to maturity in human beings, but the strange thing is that the fly itself has no connection with man. All of the true bot flies lay their eggs directly upon their victims, but the human bot apparently never does this, though by an ingenious method it provides for the transportation of its eggs to the animal body where they will develop. The human bot fly is probably unable to recognize

its victims if it sees them. It has no direct interest in human beings, monkeys or the other animals which might serve as hosts for its offspring. Instead it concentrates upon mosquitoes and other biting flies. Since the bot fly has been observed capturing mosquitoes, we shall use these as an example of how it proceeds.

When the female bot fly is ready to lay her eggs she finds some stagnant pool or pond in which mosquitoes are breeding and settles herself upon some vantage point where she can observe the emerging mosquitoes. When the mosquitoes emerge from their pupal cases their bodies are soft and they are weak and incapable of long flights. As a result they fly laboriously to a blade of grass or to foliage where they can rest while their wings and body wall harden. The bot fly, ever on the alert, espies one of these soft and immature mosquitoes, dashes upon it during its short flight, wraps its legs around it and carries it to a place where it may rest in comfort.

Now begins the business of egg laying. Holding the mosquito carefully in order to prevent its escape or injury, the female bot proceeds to deposit from ten to fifty eggs on the under side of the mosquito's abdomen and on its legs. The bot shows rare judgment in placing its eggs, being always careful not to deposit them where they would hinder the mosquito in flight, and leaving more than the distal half of the legs free so that the carrier may settle without difficulty upon its mammalian victim. When it has deposited its eggs the bot fly liberates its victim and settles down to await the appearance of another emerging mosquito, no longer interested in the fate of the eggs it has already deposited. Altogether thirty or more mosquitoes may be captured and treated to the same indignity.

When released by the bot fly the mosquito loses no time in seeking a place of safety. Perhaps it is relieved to find that its burly captor has seen fit to release it, instead of devouring it as most captors would do. It is undoubtedly inconvenienced by the load of eggs that have been firmly attached to its body, but by the time evening arrives it is ready to join its mates in the search for blood.

If the mosquito should dine upon the sap of plants or upon some cold-blooded animal, the eggs remain inactive. It is only when they come in contact with a warm-blooded animal that sudden activity develops. The very thin shell bursts open immediately, and the tiny maggot crawls hurriedly out and onto the skin of the victim. It was formerly believed that the bot larva was unable to enter healthy skin and that if the mosquito did bite, the young larva was doomed to death unless it could find some injury through which to enter the body of its victim.

But recently Major L. H. Dunn, who reared six larvae in his arms and leg at one time, observed that they were able to enter undamaged human skin. He observed that the young maggots set up an irritation within a day that had the appearance of a mosquito bite. It is, therefore, easy to understand how victims might have assumed that the bite of the mosquito was necessary. Two of the larvae which Major Dunn purposely placed upon his arm were observed under a glass while they laboriously rasped

the skin and bored their way into the tissues. It seems probable that they are also able to enter uninjured skin of other mammals also.

. . . . .

But it is still interesting to speculate how an insect which develops in the body of an animal established the roundabout habit of laying its eggs on precisely the type of insect that will plant them there. If we presume that monkeys, being nearest related to human beings of any animals found in the American tropics, were the original host of the bot fly, we can find a very good reason why an insect carrier of the eggs should have been utilized. We must first realize that the human bot fly is active during the day but that as soon as the shadows of evening begin to deepen the fly seeks a safe place in which to spend the night. The same is true of monkeys. They (or at least the vast majority of them) are diurnal and are quite inactive at night, unless disturbed by some recognized enemy. During the day they are extremely active, and a fly desiring to lay its eggs upon one of them would find it difficult to outwit the alert monkey. Monkeys, as we know, do not hesitate to eat any insects that come within their grasp, and a bot fly, which is fairly large as insects go would at once excite a monkey's attention as a possible morsel of food. As a result of this, the adult fly would have great difficulty in depositing eggs upon its host, whereas a mosquito, being smaller and perhaps more adept might do the job unnoticed.

It is still somewhat difficult to explain why mosquitoes should have been chosen as carriers of the eggs. It almost seems that the bot flies were anxious to lay their eggs upon the monkeys, but being denied this opportunity because of the activity of their intended victims, were able to observe that mosquitoes make a practice of biting monkeys. At the approach of nightfall the monkeys select a place to sleep, and the mosquitoes begin flying in search of food. We know definitely (from their habit of catching them) that the bot flies are able to see the mosquitoes. If one can accredit the bot fly with such an intellectual interest as to be able to reason from egg to mosquito to monkey, then the riddle is solved.

But there are objections to such a theory, the chief being that insects are generally considered unable to reason. They are practically devoid of brains; at best their brain is extremely small and simple in structure.

. . . . .

• Possible Inheritance of Acquired Behavior

THE INHERITANCE OF ACQUIRED CHARACTER

Smyth, Wm. Woods; *Nature*, 80:277, 1909.

I have received the following from my brother Dr. A.W. Smyth, late superintendent of the United States Mint at New Orleans. He has experimented with bees and written papers on them, which have been published in several bee-journals throughout the world.

He says, The commonly accepted view, stated by Dr. Francis Darwin in his presidential address, that the queen bee is entirely isolated, so as to bar the ordinary course of inheritance, is not so. According to Dr. Smyth, some of the workers occasionally lay eggs, and these eggs always produce drones, which, coming to fertilise the queen, opens the path for the ordinary course of inheritance. Upon this principle he bases an explanation of the following facts. In Morocco the honey-bee has foes in the form of certain beetles. To guard their stores the bees have come to build pillars of wax at the entrance to the hive, which prevents the entrance of the beetle. This becomes a habit, and a habit that could only have arisen as an acquired character, and it could only have reached workers through the queen being fertilised by drone-offspring of the workers. When a Morocco queen is brought to this country, where these beetles do not exist, the progeny of the queen continue to build pillars of wax; in the course of time this acquired habit becomes attenuated.

CONFIRMATION OF APPARENT GENETIC ASSIMILATION OF BEHAVIOR IN DROSOPHILA MELANOGASTER

Arnold, P., and Moray, N.; *Nature*, 204:504, 1964.

In a recent report from this laboratory an apparent modification of food preference in Drosophila melanogaster was described, under the combined and opposing effects of natural and artificial selection. A deficiency in the design of that experiment was that the method used to measure food preference behaviour was confounded with a measure of the activity of flies. We have repeated this experiment with a new method of testing.

The general description of our methods can be found in the earlier paper. The new test method was as follows: A Y-tube was used into which the flies were introduced. One arm of the tube ended in a chamber in which peppermint-adulterated oatmeal-and-molasses agar was provided, while normal uncontaminated agar

was placed in the chamber at the other end of the Y-tube. All flies were tested at the same time of day, and after fasting for a standard number of hours. The numbers of flies choosing each food were counted.

The results were identical with those in the earlier experiment. The most important results may be summarized thus. Flies selected for liking peppermint in each generation, and reared on it, produced a population which became strongly inclined to choose the peppermint food in the choice situation. Flies selected for aversion to peppermint, but reared on it, likewise produced a population which was strongly attracted to it. When the latter population was put back on normal food after eight generations they continued to show a preference for peppermint-scented food. Since the graphs of the data are so similar to those of the original experiment, we do not reproduce them here.

It seems plausible to say that since the experiments were begun from a new population of wild-type flies in each case, it is extremely unlikely that the effect is due to the trapping of a rare gene, and that it seems rather to be a genuine case of 'genetic assimilation'.

• Problems in Explaining Cicada Evolution

THE PERIODICAL CICADA PROBLEM

Lloyd, Monte, and Dybas, Henry S.; *Evolution,* 20:133–149 and 20:466–505, 1966.

In the first paper of this series, we outlined the extraordinary facts of natural history that compromise the periodical cicada problem. Periodical cicadas have the longest life cycles known for insects. They are called "periodical" because in any one population all but a trivially small fraction are exactly the same age. The nymphs suck juices from the roots of forest trees and finally emerge from the ground, become adults, mate, lay their eggs, and die, all within the same few weeks of every 17th (or in the South, every 13th) year. Not one species does this, but three, and they always do it together. There are three distinct species of Magicicada that occur together over most of the range , and wherever these species coexist they are invariably synchronized with each other. In different regions, different "broods" of periodical cicadas may be out of synchrony by several years, but the species (in a given region) never are. To top it all the "same" three species--the same as nearly as anyone can tell by looking at them or listening to the songs of the males--exist as 17-and

as 13-year periodical cicadas.

This situation is unique. Also unique is the phenomenonal abundance of Magicicada spp. compared with other kinds of cicadas. The periodical cicada problem is the problem of accounting for these extraordinary circumstances with a coherent theory consistent with ecological principles used to explain the population mechanics and evolution of other species of animals.

In our first paper, we advanced the hypothesis that the unusual abundance of periodical cicadas is an indirect consequence of their unique combination of periodicity, synchrony, and long developmental period. We argued that populations of non-periodical cicadas, some of which appear above ground every year, are governed by their predators, because populations of predators can build up from year to year at the expense of successive year classes of cicadas. This is not possible in the case of periodical cicadas, because the benefit to the predator populations will have become dissipated during the 16 (or 12) years between emergences--years in which virtually no periodical cicadas are to be found above ground. Periodical cicadas, we think, are governed by some other ecological mechanism, at a level far higher than the level at which predators govern populations of non-periodical cicadas. (p. 466)

PERIODICAL CICADAS

May, Robert M.; *Nature*, 277:347-349, 1979.

The difficult questions that are posed by the existence of these remarkable creatures are lucidly set out in the classic papers of Lloyd and Dybas (Evolution 20, 133; 466; 1966). There are two basic problems. First, how did such a cycle, with so long a period, originally evolve? Second, how is the clear-cut periodicity and the synchrony between co-occurring species maintained?

The second question is more easily answered than the first. Given that there already is a long life cycle and a nearly-perfect periodicity of emergence, the cicadas can effectively escape predation. Above-ground predators such as birds, who have no analogous resource in the long intervals, are satiated, and below-ground predators, such as moles, are unexpectedly deprived of food by every periodical emergence. The resulting population densities achieved by periodical cicadas are one or two orders of magnitude greater than those of their more stodgy relatives that appear each year. Lloyd and Dybas estimate that the nymphs may typically attain densities of one ton per acre. The selective pressures are for doing well at high population densities, even at the cost of being conspicuous and sluggish ('predator-foolhardy' is Lloyd and Dybas' term). Any mutant, migrant or developmentally different individual who fails to emerge with the majority will almost certainly be snapped up by predators. Lloyd and Dybas

review observations and experiments bearing this out: typically the cicadas that emerge during the first few days are all eaten by birds; in 1889 Marlatt transported large quantities of eggs to an area where the adults that emerged in 1906 were out of phase, and the entire colony of more than 10,000 individuals was effortlessly consumed by grackles. In brief, the long-period cycle enables the cicadas to saturate their predators, which reciprocally helps to stabilise the cycle as individuals appearing earlier or later are gobbled up.

This conspicuous association between predation and periodical behaviour has led many people to speculate that the magic numbers 13 and 17 arise because they are prime, thus preventing any possible subharmonic resonances by the predators. Robert MacArthur remarked that it may be the only application of number theory, as such, in mathematical biology. I am surprised nobody has yet suggested that the prime number 11 is missing because it permits resonances with the sunspot cycle!

. . . .

To summarise, it is hard to see how periodical behaviour can evolve. In particular, the Hoppensteadt-Keller criterion can only be satisfied by a delicate juggling of the pertinent parameters. But it should be remembered that the periodical cicadas comprise only 3 of some 1,500 cicada species; an easy explanation for the phenomenon would be an embarassment!

ANOMALOUS DISTRIBUTION IN TIME AND SPACE

• Insect Swarms and Population Explosions

REMARKABLE CLOUD OF FLIES

Anonymous; *Nature,* 22:518, 1880.

American papers speak of remarkable clouds of flies that have visited various districts. At East Picton, Nova Scotia (about 44° 50' N., 63° W.), such a cloud was seen on August 21. "They

passed Lismore about six o'clock in the evening close to the shore. They went with the wind, which was blowing lightly from the west, occupying about twenty minutes passing a given point. They made a loud buzzing noise, which was heard by many who missed seeing them. They flew so low that some of them appeared to fall into the water. About two miles below Lismore they slightly changed their flight, heading more to the north. After their passage numbers of strange flies were observed in some of the houses near the shore. They were about half an inch in length, with wings proportionately longer than those of the common housefly, but whether they belonged to the swarm is uncertain." At Halifax, Nova Scotia, immense swarms passed over Guy-boro' (lat. 44° 40' N., long. 61° 30'W.), on September 5. They came from the east and resembled a dark cloud. A communication from Poughkeepsie, New York (lat. 41° 50' N., long. 74° W.) states that a storm of flies was encountered on the Hudson River on the afternoon of September 4. The steamer Martin, bound south, encountered the fly storm between New Hamburgh and Newburgh. It seemed like a great drift of black snow, and it reached southward from shore to shore as far as the eye could reach. There were millions upon millions of the flies, and they hurried northward as thick as snow-flakes driven by a strong wind. They were long and black and had light wings, and the cloud must have been miles in length. Our readers may remember that some weeks ago we recorded a somewhat similar phenomenon as having been seen in various parts of France.

INSECT SWARMS IN SOUTH AMERICA

Nicolls, G. W.; *Knowledge,* 12:170, 1889.

Dear Sir,---I beg to enclose a translation of a newspaper extract, which may be of interest to some of your readers.---Yours truly, (G. W. Nicolls)

> "On March 25, at Buenos Ayres, an enormous quantity of moths was noticed flying about in the evening. At night there fell upon the city, like fine rain, such an unprecedented invasion of small flies that in some places the passers-by were obliged to run. In the theatres, cafes, houses, and streets it was impossible to go a step save through legions of these insects. Some one, out of curiosity, having counted 20,000 in a square vara (about 44 inches), calculated that there could not have fallen less than 500 thousand millions over the whole city. Such a phenomenon is quite unheard of in the locality."

A PUZZLING BUTTERFLY MIGRATION

Ball, E. D., and Stone, W. E.; *Science,* 68:110–111, 1928.

The fall migration of the Monarch butterfly, in great clouds, down the length of the Mississippi valley to the gulf is a well-established phenomenon. Records are accumulating that show a definite fall migration of the long-beaked butterfly from the plains of Texas south or southeast towards the gulf. A spring migration of the Painted Lady butterfly from Mexico northward into California in immense swarms has been recorded several times and seems to be established as a regular movement. These migrations are all of the same general character as the fall and spring migrations of the birds and may well represent the same type of response.

The annual migrations of the Southern Cabbage butterfly (Pieris monuste) are, on the other hand, apparently just the reverse of this, as the immense swarms of this species travel southward down the east coast of Florida at the same time that the bobolink is leaving for the north.

The migration of the present season has been exceptionally heavy and has attracted a great deal of attention. Every automobile passing up or down the coast has emerged from the swarms with its radiator plastered with the butterflies, often to such an extent that heated engines were common and even bearings burned out. Stirling, in the Florida Entomologist, records a similarly heavy flight in 1923 and stated that it was an annual occurrence. Other observers have encountered these swarms every year since that time.

The present swarm was reported as between Jacksonville and St. Augustine on May 10. By May 20 it had moved more than one hundred miles south and at that time extended from New Smyrna to Ft. Pierce, a distance of one hundred and twenty-five miles. At present (June 20) the swarm is reported along the coast south of Miami, a movement of about two hundred miles in the month. On May 20 the writers drove through the swarm for eighty-five miles along the coast and then turned westward at right angles and drove thirty miles to Sanford. On the westward trip butterflies were counted crossing the road at the rate of six per mile or two hundred per hour. They were flying almost due south against a light breeze. In the first few miles from the coast the numbers were greater and they were flying southeast, swinging more to the east as they approached the coast. Along the shores of the Indian River they swung southward again, concentrating into a definite swarm only a few hundred yards in width and in passing obstructions narrowing to fifty yards or less. They were flying low, two to six or rarely eight feet in height and at the rate of six or eight miles per hour against a light wind. At Fort Pierce two hundred per minute were leaving a field one hundred yards wide. They flew about eight hours that day, which would give a total of one hundred thousand per day passing over an area that wide.

The writers observed that, although the swarm as a whole was moving down the shore, a constant procession of butterflies were

striking out across the water to the southeast. Stirling records that from June 9 to 11, 1923, passengers on vessels plying between Nassau and the mainland observed millions of butterflies winging their way southward over the gulf stream. From these observations it would seem that the swarms noted along the coast are not continuously moving bodies but only a temporary piling up of individuals that are later to strike out over the water.

Where these butterflies come from and where they go are still mysteries but a still greater mystery is the force or impulse back of the movement that sends them in a reverse direction from ordinary migratory activity. Before we can hope to interpret this latter force we must have much more information as to the scope of the movement itself, and the present note is submitted in the hope that those who have further information will record it as soon as possible.

• Insects Apparently Imprisoned in Wood

A CURIOSITY

Barlow, John J.; *English Mechanic*, 52:444, 1891.

Perhaps the following note may be of interest:--About ten days ago, whilst some men were sawing into planks a large log of beech, at Messrs. R. Bealey and Co.'s bleach and chemical works, Radcliffe, the said beech-tree having been grown in Shropshire, they cut across a circular hole 5½ in. long and 1 in. in diameter. One end of the hole was 3½ in. distant from the outside of the tree, the other end of the hole being about 4 in. from the centre of the log. Whilst turning over the cut pieces of log, a brown-coloured powder fell out of these holes, and, wonderful to say, two live insects. In size and appearance they were very much like wasps, though, from the shape of their head, very narrow wings, and thick body, I am inclined to think they are altogether different. A most careful and minute examination was made round the hole to discover an inlet or outlet, but to no purpose--not even an air-hole. All round was sound, solid wood, and evidently they must have been in this tree many years. Although alive, they appear to be in a kind of sleep. If brought near to a fire, after a while they begin to crawl about; they cannot fly.

. . . .

• Skewed Populations

WHERE THERE ARE 100 FEMALES FOR EVERY MALE

Owen, Denis; *New Scientist,* 57:668-669, 1973.

On sunny afternoons many orange, black, and white butterflies fly together on the tops of small hills around Kampala in Uganda. Similar aggregations occur in rather open country bordering the shores of Lake Victoria. The butterflies repeatedly fly around quite small areas and late in the afternoon they cluster together in dense masses on the leaves and stems of plants. Here they spend the night and after the morning dew has dried they disperse, gathering together again early next afternoon. If you look closely at these butterflies you find, surprisingly, that they are almost all females. There are a few males usually joined and mating with females, but their overall frequency is no more than between one and five to every 100 females. While aggregating, the females sometimes lay clusters of 50 to 100 infertile eggs on plants which are not the larval food-plant. Occasionally, they lay eggs on the wings of other butterflies. This species of butterfly is Acraea encedon, one of the 170 or so members of the family Acraeidae found in Africa, but the strange behaviour and the existence of highly unusual sex ratios is not known in any other acraeid, or indeed in any other butterfly.

. . . .

• Latent Life or Cryptobiosis

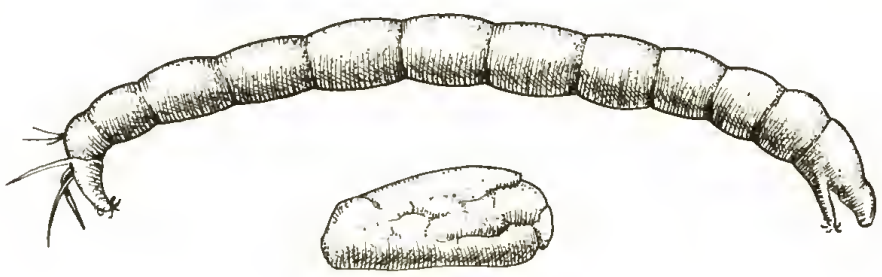
SUSPENDED ANIMATION AND THE ORIGIN OF LIFE

Hinton, H. E., and Blum, M. S.; *New Scientist,* 28:270-271, 1965.

The larva of a chironomid fly (Polypedilum vanderplanki Hint.) is the most advanced organism so far known that can be totally dehydrated. Experiments with it clearly show that complex forms of life can be reduced to a purely morphological state in which certain spatial relations between the parts are all that matter; only when water is added does the organism once more become a physiological entity. A larva can be repeatedly transferred from a physiological state to a purely morphological one --- and back

again --- simply by removing or adding water. Dry Polypedilum larvae are not damaged by immersion in liquid helium. They will survive to undergo metamorphosis and produce apparently normal adults after exposure to 102 --- 104°C for one minute when dry. They make a temporary recovery, sufficient to enable them to crawl about, after being heated in the dry state of 106°C for three hours or to 199-201°C for five minutes. Thus a dry larva can be exposed to a temperature range of 370°C and still, on wetting, produce an apparently normal adult. It will partially recover when exposed to a range of 470°C.

As might be expected, when an organism is in a state of cryptobiosis mechanical injuries are tolerated that would be immediately fatal in the normal state. For instance, a dry larva of Polypedilum can be cut in half or into smaller pieces and kept for several years. If the pieces are then placed in water, they recover for



The larva of a chironomid fly after being dried for three weeks (bottom). Same larva after 20 minutes water submersion (top).

a short time as they take up water, and then they die. Recovery has been judged by seeing the pharynx or dorsal vessel beat or by eliciting responses to tactile stimuli from the pieces. Pieces consisting of the head, thorax, and up to the first five abdominal segments recovered temporarily after being heated when dry to 102-104°C for one minute or to 104°C for five munites. (p. 270)

• The Curious Case of Crosse's Acari

ACCIDENTAL PRODUCTION OF ANIMAL LIFE

Crosse; *American Journal of Science*, 1:32:374-377, 1837.

A communication from Mr. Stutchbury, of the Bristol Institution, has been disseminated through the public journals, of which we give a copy. As there is no doubt of the honor and veracity of Mr. Stutchbury, the extract which this gentleman gives from a letter of Mr. Crosse, may be considered as if communicated by Mr. Crosse himself.

Sir--It may be truly said that facts recorded, faithfully detailed, and made public, are the means by which philosophy is enabled to render her temple more durable on its foundations--every additional fact being the commencement of that which, when understood, forms an outwork of defense, rendering the interior the strong hold and sacred depository of truth.

It was a maxim of the late John Hunter, which he was repeatedly proclaiming, that greater benefit would be conferred upon the community if professional men had the moral courage to publish detailed accounts of all their unsuccessful cases, than could be derived by the publication of those which met with a favorable termination, for then the physician would reject at once such as had failed as modes of cure, without repeating experiment after experiment, thereby saving much valuable time, and certainly sparing much needless pain to the unfortunate patient. So with facts in natural philosophy. Let experiments be recorded, and their results will enable others, either to avoid, or successfully to pursue them, so as to bring them to bear upon principles which may elucidate some great truth, the light of which would probably have never shone upon them, had they not been able to take advantage of the works of pioneers who had proceeded, perhaps, to the very threshhold of the same result.

With regard to a large number of curious and (at the same time, in consequence of the principle not being understood) astounding facts, the investigator would be doing a great injury in the cause in which he labors, should he be deterred from making them known, merely because he cannot at the same time reasonably account for the same. And upon this view of the subject, and to set at rest the vague views which are abroad, I feel it a duty due to the cause in which I am engaged, and to the philosopher with whom they have originated, at once to take the liberty of transmitting to the press, and particularly the local press, from which so many reports have emanated, an abstract of a letter I have received from Mr. Crosse, with an account of his experiments, in the language of a private communication, (not that which he would probably have chosen, had he made the communication himself,) and without further comment.

"The following is an accurate account of the experiments in which, insects made their appearance:--

"Experiment first.--I took a dilute solution of silicate of potash, supersaturated with muriatic acid, and poured it into a quart basin, resting on a piece of mahogany, a Wedgwood funnel was placed in such a manner that a strip of flannel, wetted with the same, and acting as a siphon, conveyed the fluid, drop by drop, through the funnel upon a piece of somewhat porous Vesuvian red oxide of iron, which was thus kept constantly wetted by the solution, and across the surface of which, (by means of two platina wires connected with the opposite poles of a voltaic battery, consisting of nineteen pair of five-inch plates in cells filled with water and 1/500 muriatic acid,) a constant electric current was passed. This was for the purpose of procuring crystals of silex. At the end of fourteen days I observed two or three very minute specks on the surface of the stone, white, and somewhat elevated. On the eighteenth day, fine filaments projected from each of these specks, or nipples, and the whole figure was increased in size. On the twenty-second day, each of these figures assumed a more definite form, still enlarging. On the twenty-sixth day, each assumed the form of a perfect insect, standing upright on four or five bristles which forms its tail. On the twenty-eighth day, each insect moved its legs, and in a day or two afterwards detached itself from the stone and moved at will.

It so happened that the apparatus was placed fronting the south, but the window opposite was covered with a blind, as I found these little animals much disturbed when a ray of light fell on them; for out of about fifty which made their appearance at once, at least forty-five took up their habitation on the shaded side of the stone. I ought to have added, that when all the fluid, or nearly so, was drawn out of the basin, it was caught in a glass bottle, placed under a glass funnel which supported the stone, and was then returned into the basin without moving the stone. The whole was placed on a light frame made for the purpose. These insects have been seen by many of my friends, and appear, when magnified, very much like cheese-mites, but from twice to eight times the size, some with six legs, others with eight. They are covered with long bristles, and those at the tail, when highly magnified, are spiny. After they had been born some time they become amphibious, and I have seen them crawl about on a dry surface.

"Experiment second.--I took a saturated solution of silicate of potash, and filled a small glass jar with it, into which I plunged a stout iron wire, connected with the positive pole of a battery

of twenty pair of cylinders, filled with water alone, and immersed in the same a small coil of silver wire, connected with the negative pole of the same battery. After some weeks' action, gelatinous silex surrounded the iron wire, and, after a longer period, the same substance filled up the coil of silver wire at the other pole, but in much less quantity. In the course of time one of these insects appeared in the silex at the negative pole, and there are at the present time not less than three well-formed precisely similar insects at the negative, and twelve at the positive pole, in all fifteen. Each of them is deeply imbedded in the gelatinous silex, the bristles of its tail alone projecting, and the average of them are from half to three quarters of an inch below the surface of the fluid.

"In this last experiment we have neither acid, nor wood, nor flannel, nor volcanic iron-stone. I will not say whether they would have been called to life without the electric agency or not. I offer no opinion, but have merely stated certain facts."

In addition to this, on Friday, the 10th ult., Mr. Crosse transmitted to Mr. Owen, Hunterian Professor, College of Surgeons, London, a copy (perhaps the original) of the above, in his own hand-writing, with several specimens of the insects themselves, so enclosed in Canada balsam and between plates of glass and talc, as to be easily submitted to examination in the microscope. By the kindness of this gentleman, Mr. Clift, conservator of the museum, in the same establishment, produced them at the Conversazione of the Royal Institution on Friday, the 17th, when they were most satisfactorily visible in the microscope. By an extention of the same courtesy on the part of Mr. Owen, we have been permitted to draw and engrave two of the groups of these mysterious visitors, in order to gratify the prevailing intense desire for accurate information upon the subject.

The insects above delineated are some of those collected and sent to London by Mr. Crosse. We have placed an ordinary cheesemite near them, drawn to the same scale, in order to show the relative size, and the similarity of appearance. Like the mite, the new insects have fine hairs scantily distributed on the body, but these became invisible on the immersion of the insect in the balsam. Their varied positions, the arrangement of their legs, &c., are owing to their accidental suspension in the balsam, and the subsequent pressure of the plate of talc upon them.

We cannot believe that life and organization have been produced by galvanic power, but would sooner suppose that the ova of the insects may have been contained in the materials galvanized or come into them during the process, and that the galvanic power may have quickened them into life, as electricity and animal warmth operate upon eggs.

THE ACARUS CROSSII

Benham, Charles E.; *Nature,* 81:127, 1909.

Some months ago (Nature, February 4) a correspondent directed attention to the account of Crosse's remarkable experiences when experimenting with electric currents, and the appearance of quantities of an acarus in the solutions treated, as fully narrated in Chamber's "Vestiges of the Footsteps of Creation." and the question was asked whether any explanation of such strange phenomena had ever been heard of. No reply seems to have been made, and, presumably, no recent attempts to investigate the mystery have taken place. It may be of interest to note that Chambers's account is fully corroborated in the "National Dictionary of Biography," and it appears that Crosse, though he did not make any suggestions as to "spontaneous generation," but merely related the facts and left explanations to others, found himself the victim of such a shower of abuse that he thenceforth entirely abandoned all research work and retired into obscurity. His experiments would probably have been forgotten but that they were repeated with complete success by another worker. Considering how much more easily prolonged electric action can nowadays be applied, would it not be well if someone would have the patience to repeat once more the exact conditions so amply described by Chambers, and so, if possible, clear up what is undoubtedly a very mysterious occurrence?

Chapter 7
ANIMALS WITHOUT SKELETONS

Chapter Contents

MORPHOLOGICAL PHENOMENA

• Independent Development of Eyes

THE BRIGHT-EYED SCALLOP

Curtis, Helena; *Sea Frontiers,* 12:194–202, 1966.

When undisturbed, the scallop drifts along the sandy bottom of the bay or ocean, lying horizontally (and always with the same valve down), rather than embedding itself vertically, as do many other bivalves. Its shell remains slightly open to admit the water currents that bring food and oxygen, a process aided by the beating of the cilia in the gills. Shining like pinpoints of light through the narrow slit between the fluted shells can be seen what many scientists consider the scallop's most intriguing feature: a double row of brightly shining eyes.

The eyes of Pecten are eyes in the truest sense. Other bivalves, as judged by their ability to orient themselves in relation to light, have photosensitive areas; but the eye of the common scallop is far more complex, possessing cornea, lens, and a double-layered retina, the peculiar properties of which will be discussed in more detail. Behind the retina is the tapetum lucidum, "bright carpet," a mirroring surface such as that found in the eyes of cats and some other night-prowlers. It is the tapetum that gives the eyes their special iridescence.

The scallop's eyes are remarkable not only for their complexity but for their number. A medium-size specimen of Pecten irradians, the blue-eyed scallop, usually has a hundred or more eyes equally distributed between the upper and lower layers. These eyes are located along the margins of the shells among the thick tentacles that form the fringe of the mantle. They are on short stalks and are scattered irregularly among the tentacles, which overhang them slightly like long bangs. The optic nerves of several adjacent eyes run together and then join the circumpallial nerve that traverses the margin of the mantle, connecting eventually with the scallop's "brain," a pair of ganglia lying between the adductor muscle and the hinge.

The eyes of Pecten are literally blue, unlike the eyes of human beings. Blueness in the human eye is actually a developmental anomaly. In "blue-eyed" persons the stromal cells of the iris fail to develop pigment (as they do in brown or black-eyed persons) and the reddish backing of the retina shines through as blue by the same sort of optical trickery that makes the red blood of the

veins seem blue through white skin. In the blue-eyed scallop, on the other hand, the entire area corresponding to the white of the human eye is a soft powder blue, in the center of which is an iridescent twinkle, the reflection from the tapetum.

New Eyes Continually Grow. New eyes form constantly in the growing Pecten, apparently appearing more or less at random as room becomes available for them. Many years ago Dr. Earl Butcher of Hamilton College, working at the Woods Hole Oceanographic Institution, studied the stages in eye formation in a species of scallop. An eye begins as a group of pigmented cells forming at the base of a tentacle and growing inward as a bud or cup which develops into a vesicle. The retinal cells arise from the wall of the vesicle that is turned away from the light, and the tapetum develops from the other wall. As the eye develops, it is pushed outward on the end of a stalk which turns it toward the light. The epithelium overlaying the lens loses its pigmentation to form the cornea, while the cells at the edge of the eye grow darker, eventually becoming the iris. Nerve cells grow out from the retinal layers reaching toward and finally connecting with the arc of the circumpallial nerve.

. . . . .

Thus the eyes of man and Pecten appear to share with each other and with many other creatures a common though far-distant ancestry, perhaps linked through some long-extinct protozoan. Both also, as Dr. Miller points out, have solved the problem of photoreception by presenting a series of membranes---in one case, the coins; in the other, the cabbage leaves---to the incoming light waves.

Of What Use Are the Eyes?. No one ventures to answer the question of why---by what evolutionary byway---the scallop acquired any eye at all, much less one so marvelous. The feeding habits of the scallop, like those of most of its relations, involve only the sifting of whatever riches are brought to it by the tides and currents of its world. Its chief enemy is the starfish, which locks the scallop in a firm embrace, pulls apart the valves by the suction exerted by its tube feet, and extrudes its stomach around the soft parts of the mollusk. If juice extracted from the starfish is released into the waters surrounding Pecten, it reacts immediately with its characteristic leap of alarm, in response to stimulation of the chemosensory receptacles in its tentacles. But a model of a starfish dangled before its 100 shining eyes is greeted only with molluscan sangfroid. In other words, its eyes seem to serve neither in the quest for food nor for escape from predators.

At a loss for a logical interpretation, one can only recall the centuries through which its lovely, fluted shell has been a source of aesthetic delight to the eyes of man; and so one is led to hope, in foolish human fashion, that some of the objects that pass before the eyes of Pecten as it floats along the bottom of the sea bring to it also some scallop-like equivalent of pleasure.

VISION IN ANNELID WORMS

Wald, George, and Rayport, Stephen; *Science,* 196:1434-1439, 1977.

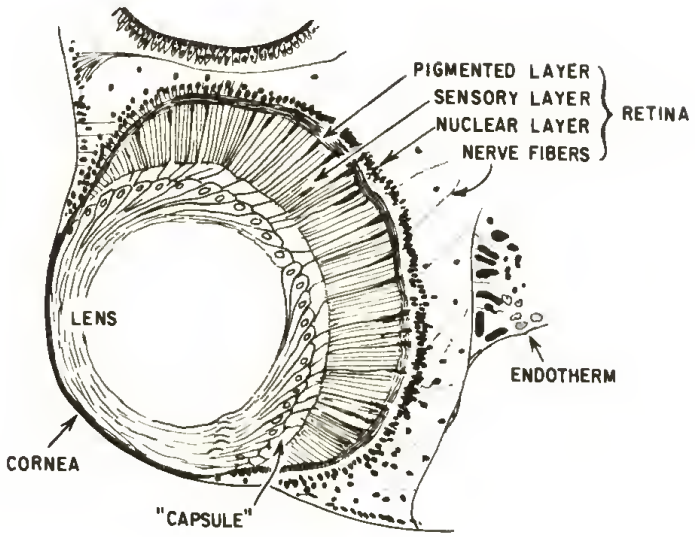
It is often said that of the 12 major animal phyla, only three--vertebrates, molluscs, and arthropods--have evolved well-constructed, image-resolving eyes. That is not so; for a fourth phylum, the annelid worms, has developed excellent eyes. They are confined to a single family of polychaetes, the alciopids, all marine and widely distributed in the warmer waters of the Atlantic and Pacific.

Abstract. In a first electrophysiological study of worm vision, electroretinograms were measured in two alciopid worms: Torrea, taken at the surface, and deep-sea Vanadis. Both forms possess a primary retina in the focal plane of the lens, and accessory retinas lying beside the lens. Such accessory retinas occur also in deepsea fishes and cephalopods. In Torrea the primary retina peaks in sensitivity at 400 nanometers, the secondary retina at 560 nanometers. Both together could serve as a depth gauge, since 560 nanometers attenuates much faster in seawater than 400 nanometers. The Vanadis eyes peaked in sensitivity at 460 to 480 nanometers, a property shared with deep-sea forms of other phyla; and appropriate, since these wavelengths penetrate seawater most deeply, and also are the wavelengths of maximum bioluminescience.

VISION IN CUBOMEDUSAN JELLYFISHES

Pearse, John S., and Pearse, Vicki B.; *Science,* 199:458, 1978.

Cubomedusans have as many as 24 eyes located near the bell margin, and the most complex of these eyes have an epidermal cornea, spheroidal cellular lens, and upright retina. The retina is composed of layers, corresponding to those of alciopids; a presumably sensory layer, a pigmented layer, a nuclear layer, and a region of nerve fibers. The cells described by Berger as sensory contain a zone of pigment granules at the level of the pigmented layer and, in the sensory layer, long "axial fibers" which he tentatively identified as cilia and clearly illustrated with basal bodies. There are roughly 11,000 sensory cells in the eye, a number comparable to that estimated for alciopids. Elongate pigment-filled cells (supportive cells?) extend between the sensory cells in sections from animals fixed in the light, and are partially withdrawn in sections from animals fixed in the dark; Berger interpreted these observations as evidence for a kind of adaptation to light and dark, similar to that in arthropodan and other complex eyes. Between the lens and the retina is a "capsule" through which narrow processes extend from the tips of the long pigment cells to the cells of the lens, features which Berger speculated might be involved in accommodation for near and far vision.



Section of a jellyfish eye

Like alciopids, cubomedusans have been studied little. They are agile and rapid swimmers; specimens only about 3 cm high have been clocked at up to 6 m per minute. They are active both by day and by night, and in the dark exhibit strong positive phototaxis; they will orient accurately to the light of a match as much as 1.5 m away even if the match is extinguished before orientation is completed. Their combination of speed and fine directional sensitivity to light might enable them to orient to luminescent prey at night. Larson observed that cubomedusans attracted to a night-light "neither swam toward prey nor avoided obstacles," but the interesting possibility of image-forming vision under more normal conditions remains. Cubomedusans are unique among cnidarians in engaging in copulation; might the eyes be somehow involved in this behavior?

. . . . .

• Remarkable Forms of Mimicry

THE PROBLEM OF PERFECTION

Gould, Stephen Jay; *Ever Since Darwin*, W. W. Norton & Co., New York, 1977, pp. 103–104.

Modern evolutionists cite the same plays and players; only the rules have changed. We are now told, with equal wonder and admiration, that natural selection is the agent of exquisite design. As an intellectual descendant of Darwin, I do not doubt this attribution. But my confidence in the power of natural selection has other roots: it is not based upon "organs of extreme perfection and complication," as Darwin called them. In fact, Darwin saw truly exquisite design as a problem for his theory. He wrote:

> To suppose that the eye with all its inimitable contrivances for adjusting the focus to different distances, for admitting different amounts of light, and for the correction of spherical and chromatic aberration, could have been formed by natural selection, seems, I confess, absurd in the highest degree.

In essay 10, I invoked gall midges to illustrate the opposite problem of adaptation---structures and behaviors that seem senseless. But "organs of extreme perfection" proclaim their value unambiguously; the difficulty lies in explaining how they developed. In Darwinian theory, complex adaptations do not arise in a single step, for natural selection would then be confined to the purely destructive task of eliminating the unfit whenever a better-adapted creature suddenly appeared. Natural selection has a constructive role in Darwin's system: it builds adaptation gradually, through a sequence of intermediate stages, by bringing together in sequential fashion elements that seem to have meaning only as parts of a final product. But how can a series of reasonable intermediate forms be constructed? Of what value could the first tiny step toward an eye be to its possessor? The dung-mimicking insect is well protected, but can there be any edge in looking only 5 percent like a turd? Darwin's critics referred to this dilemma as the problem of assigning adaptive value to "incipient stages of useful structures." And Darwin rebutted by trying to find the intermediate stages and by specifying their utility.

> Reason tells me, that if numerous gradations from a simple and imperfect eye to one complex and perfect can be shown to exist, each grade being useful to its possessor . . . then the difficulty of believing that a perfect and complex eye could be formed by natural selection, though insuperable by our imagination, should not be considered as subversive of the theory.

The argument still rages, and organs of extreme perfection rank high in the arsenal of modern creationists.

Every naturalist has his favorite example of an awe-inspiring adaptation. Mine is the "fish" found in several species of the freshwater mussel Lampsilis. Like most clams, Lampsilis lives partly buried in bottom sediments, with its posterior end protruding. Riding atop the protruding end is a structure that looks for all the world like a little fish. It has a streamlined body, well-designed side flaps complete with a tail and even an eyespot. And, believe it or not, the flaps undulate with a rhythmic motion that imitates swimming.

. . . . .

A pseudofish displayed by a mussel

In Lampsilis, the inflated marsupium of gravid females forms the "body" of its ersatz fish. Surrounding the fish, symmetrically on both sides, are extensions of the mantle, the "skin" that encloses the soft parts of all clams and usually ends at the shell margin. These extensions are elaborately shaped and colored to resemble a fish, with a definite, often flaring "tail" at one end and an "eyespot" at the other. A special ganglion located inside

the mantle edge innervates these flaps. As the flaps move rhythmically, a pulse, beginning at the tail, moves slowly forward to propel a bulge in the flaps along the entire body. This intricate apparatus, formed by the marsupium and mantle flaps, not only looks like a fish but also moves like one.

Why would a clam mount a fish on its rear end? The unusual reproductive biology of Lampsilis supplies an answer. The larvae of unionids cannot develop without a free ride upon fishes during their early growth. Most unionid larvae possess two little hooks. When released from their mother's marsupium, they fall to the bottom of the stream and await a passing fish. But the larvae of Lampsilis lack these hooks and cannot actively attach themselves. In order to survive, they must enter a fish's mouth and move to favored sites on the gills. The ersatz fish of Lampsilis is an animated decoy, simulating both the form and movement of the animal it must attract. When a fish approaches, Lampsilis discharges larvae from the marsupium; some of them will be swallowed by the fish and find their way to its gills. (pp. 103-107)

WHEN IN ROME .....

Stubbs, Peter; *New Scientist,* 39:66-67, 1968.

Parasitic infections of Man or animals are singularly difficult to treat by drugs for the simple reason that the parasite has always adapted itself so well to its host's environment that the metabolism of both is very close. What might poison the parasite would probably poison the host. However, with many types of parasite, inoculation techniques have also failed. Since most animals are highly sensitive to the presence of foreign tissue within them, and have developed efficient immune mechanisms to reject such alien matter, the intractability of certain parasites in this respect has often baffled biologists. The schistosome parasite is among those which have not responded to inoculation methods.

. . . . .

At MMR Dr. Smithers' team has been working on the schistosome parasite that infects the large intestine, Schistosoma mansoni. One of the difficult things to explain about these parasites is their human hosts for as long as 26---or even 40---years in some cases. Clearly they must be tolerated, in the immunological sense, remarkably well.

Fortunately laboratory rhesus monkeys can be easily infected with S. mansoni. What turned out to be particularly odd about experiments with monkeys, however, was that, once infected, they proved to be strongly resistant to a second challenge infection by cercariae. Moreover, at the same time as they were destroying these infective forms of the parasite they were still harbouring adult worms from the initial infecton. In fact, the NIMR workers showed, it was principally these adult worms which made

the monkeys immune to further infection by a new batch of their immature counterparts. Transferring adult worms piecemeal by surgical means from one monkey to a normal, uninfected monkey rendered this monkey also resistant to further infection.

Thus the adult worms create an environment that is unwholesome to immature forms without becoming affected by this immunity themselves. The researchers have dubbed this new kind of physiological mimicry "concomitant immunity". It seems one very good reason why inoculation techniques have not succeeded in beating bilharzia.

. . . . .

There seems little doubt that this parasite's chameleon-like facility to merge biochemically with its surroundings prevents its rejection by the host and its long persistence in Man. Similar trickery may well characterize other worms of the helminth family (the persistence of malaria and trypanosomes is another kind of sculduddery altogether). But the key question now is: just how does the disguise antigen reach the worm's surface, and can it be prevented from doing so? The answer could have very far-reaching consequence both for the economic recovery of many developing nations---and for human happiness.

• Animals with Chloroplasts

A SLUG THAT FIXES CO_2

Anonymous; *New Scientist,* 43:634, 1969.

The plant and animal kingdoms used to be separated by one unique characteristic: plant cells contain chloroplasts and animal cells do not. Contrary to this classification, a group of animals has now been found that contain fully functional chloroplasts.

The animals concerned are marine slugs which graze on the sea bottom. They have a particularly simple digestive system, consisting of a rudimentary mouth and stomach leading into a series of finer and finer ducts which carry nutrients directly to the tissues. So when the slugs feed on algae by puncturing single cells and sucking out their contents, the algal chloroplasts are distributed throughout the slugs' tissues. It seems that not all the chloroplasts get digested, but some find their way inside the cells of the slugs.

All this was discovered several years ago, but then it was assumed that the chloroplasts were in fact just being slowly digested. Now, Robert Trench, Richard Greene and Barbara Bystrom, of the University of California, Los Angeles, have

shown that the chloroplasts are fully functional and can definitely survive inside their new host for several weeks (Journal of Cell Biology, vol.42,p.404).

Chloroplasts are the characteristically shaped organelles that make plants green: they absorb sunlight and carbon dioxide and make carbohydrate and oxygen. Trench and his colleagues using electron microscopy found quite unmistakeably chloroplasts in the animal cells. When illuminated, the chloroplasts could fix radioactive carbon dioxide into substances which dispersed widely throughout the slug's body. Evidently the chloroplasts could still fulfil their photosynthetic function inside an animal cell. Slug eggs do not obviously contain chloroplasts, so they are probably newly acquired by each generation. The exact source of the chloroplasts is still in doubt, but algae which contain the same photosynthetic pigments as the slugs are likely candidates to provide them.

Presumably the chloroplasts are used to some functional advantage by the slugs. Precedents do exist with other marine organisms that contain symbiotic algae. What is surprising is that chloroplasts can survive outside their original cells for so long. This latest paper dramatically restates the case for their bacteria-like nature at a time when evidence was beginning to accumulate against their complete independence ("Monitor", vol. 43,p.272). It is therefore important to know just how many membranes exist around the chloroplasts. One possibility is that they are merely inside a vesicle of the slug's cell membrane like any other foreign body being digested. An alternative, and further reaching conclusion, suggests that they might be inside the animal cell in just the same relationship with the endoplasmic reticulum as they were in the plant cell. High-resolution electron microscopy will be needed to clear up this point.

• Compound Animals

COMPOSITE ANIMALS

Beard, J. Carter; *Scientific American,* 93:442, 1905.

The circle of individuality, which beginning with that simplest of units of living matter, the free cell, ascends in a spiral, and returning upon itself finds expression in the higher unities of muticellular organisms, of which the original cells are integral parts, thus forming a second and more perfect set of individualities, makes or tends to make another complete circuit, in producing, by the aggregation of this second set, still a third

series of individualities.

Carried through an increasingly intimate association in forms and functions of life, among the so-called compound animals, especially the ascidians, the third circle is still left incomplete, and the missing arc is perhaps more nearly supplied by the Hydrogia than by any other class of animals.

When it first issues from the egg, the hydroids are little flat wormlike creatures, too minute to be examined by the naked eye. Under a lens of sufficient magnifying power they appear fringed with fine hairs or cilia, by means of which they swim about. In a short time this planula, as it is called, attaches itself to some support, and the second stage of its life begins. It loses its cilia, and assumes the shape of a round-bottomed flask. It changes its form several times, and at last is elongated into a stalk, sends out roots, and at the summit blossoms into a flower-like circle of tentacles. The tentacles elongate, clusters of eggs like fruit appear and the life circle seems to be complete. From the roots other individuals arise, some of which, like the parent form, develop hydranths or blossoms, but others, instead of so doing, have only reproductive functions. In these last are developed small zooids, which in some cases become free, and in other cases, as for instance in the species figured in the illustration, Clava leptostyla, they never separate from their parent. The medusa-buds produce eggs and spermatozoa, which in turn give rise to other colonies similar to that described. Here is exhibited a division of function among the individuals forming the colony, which in fact in its entirety is a single animal, which is not to be found among the compound ascidians. The hydranths or individuals carrying tentacles are the nutritive portions of the colony. The tentacles capture, and the stalk digests food for the whole organism, while the gonangia devote themselves to reproducing offspring. Full of suggestion and interest as it is, only the hastiest glance can be given to this branch of the subject. The only remark which the present opportunity allows with regard to it is that, strange as it appears, it nevertheless seems to be shown in the genesis of hydroid communities that an animal can exist in separate parts, and that a continuity of substance is not an indispensable condition to unity of individuality. "From a single egg," says Prof. Clark, speaking of the matter from a different standpoint, "there is developed a number of zooids, from which there escape quantities of medusae (jelly fish) which are frequently capable of feeding and of reproduction. Are each of these jelly fishes reproductive sacs and feeding portions, to be regarded as separate individuals or as parts of one individual? The latter is the true course; an individual embraces all the products of a single egg, and the name zooid is applied to the various more or less independent portions, whatever spaces may intervene between them, both of the medusa buds that float away, and of other parts that may arise by dividing or fission, but never by a new ovarian reproduction." The writer has interpolated and marked the interpolation in this paragraph, in order to carry out the plain inference given by the statement.

But after all, the division of function in the hydroids admits of little diversity of component zooids or subordinate individualities, while in the colonial jelly fish or siphonophora (see illustration) the specialization of offices and of the connected animals which perform them, as well as in the much more definite form and character of the aggregate, bring us very far on our journey toward realizing that a composite animal of the third degree, consisting of the union of complex individuals, may yet have an individuality of its own quite apart from those of the members which compose it. The composite animals belonging to this class consist of communities of individuals, each and all of which are modified and specialized in such a manner as will most effectually serve the communal body to which they are attached. This body, instead of consisting of a fixed and stationary colony of zooids, whose boundaries are shaped and determined by the spot it chances to occupy, has like the higher animals a constant form peculiar to its species, is free, and has the means and power of voluntary motion.

The Physalia arethusa, for instance, consists of a large elongated air sac or float, surmounted by an elegant indented crest, which is nothing else than a zooid enlarged and adapted for the purpose it serves, and a multitude of animals of different degrees of development according to their use, clustered upon its under part toward the broader end. These pendent animals have surrendered the power of separate existence, and so much of individuality as to become little more than the organs of the corporate body. A portion in which all functions have become atrophied except that of progressing through the water, by allowing it to enter their cavities and then forcing it out again, constitute the locomotive organs of the physalia. Other hollow structures open at the end are so many mouths with stomachs attached; they seize upon all such tiny marine organisms as certain other members of the community, modified into long processes armed with stinging and paralyzing organs, shock into insensibility. They devour and digest the food thus provided, and send the nutritive fluid that they elaborate to all parts of the colony. Scattered among these feeding stinging polyps are smaller zooids with egg-shaped bodies, each carrying a long thread. These threads are really nerves, and are very sensitive. Their use is to make the organism to which they belong aware of its surroundings. They are sense organs. Besides these there are two or three other sorts of individuals, which elaborate the particular sort of reproductive cells requried for the perpetuation of the particular species to which the composite animal belongs. These are specialized medusa polyps, of which one sort larger than the other supplies the active sperm cells, while the smaller sort furnishes the inactive egg cells.

This is certainly a remarkable state of affairs, however we may consider it. If the various parts and the different organs of our bodies, the lungs, the stomach, the nerves, and all the viscera and members of our bodies, were individual entities that might be conceived of as originally separate organisms, each part still possessing in some sort traces of a separate consciousness of its

own, it could scarcely be stranger. There are many of these composite animals presenting every degree and variety of complexity, until we reach those in which the individuality of the constituent parts is entirely lost.

And thus a circle in life of which only the roughest sketch can be here suggested (it would require a volume to do it justice) as far as our space allows, is completed.

It is certainly a wonderful and a suggestive circle, one that might possibly have a value and a meaning not only in any given theory of genesis of species, but upon the foundation upon which all such theories must rest, the genesis of the individual.

LONG SAUSAGE-LIKE BODIES IN SEA

Montgomery, K. H., and Long, J. C.; *Marine Observer,* 32:120-121, 1962.

m.v. Benvannoch. Captain K.H. Montgomery. Aden to Penang. Observers, the Master, Mr. J. Mitchell, 1st Officer and Mr. G. Walker, 2nd Officer.

5th July 1961. At 1100 GMP while approximately 30 miles north of Socotra Island we passed many red coloured, jelly-like, sausage-shaped objects of various lengths from about 5-50 ft. and having diameters of from 1-2 ft. These were seen frequently until sunset, usually in threes. During the period the wind was force 7-9 and the sea was very rough, but when the objects tumbled over on the crest of a wave, they maintained their shape. They were floating at a depth of about 3 ft. below the surface and appeared to have no means of propulsion: no openings on their surface were seen. The objects were observed frequently from noon to sunset from north of Socotra to 12°N, 65°E. Sea temp. 81°F.

Position of ship at 1200 GMT: 13° 12'N, 53° 42'E.

9th September. On the homeward run, in the same area, a watch was kept for any of the objects described above, but none were seen.

s.s. Mandasor. Captain J.C. Long. Aden to Calcutta. Observers, the Master and Mr. I.R. Poole, 2nd Officer.

28th July 1961. Five cylindrical-shaped objects consisting of some jelly-like substance were seen floating on the surface of the sea around 1030 GMT. They were red in colour, about 1 ft. in diameter and 6 ft. in length; even though the sea was rough, the objects did not lose their shape. Sea temp. 78°F; wind SSW, force 7.

Position of ship: 12° 40'N., 54° 50'E.

Note. Miss A.M. Clark, of the Natural History Museum comments:

"The size of the objects in both the above observations is remarkable and the details from m.v. Benvannoch are particularly

interesting. The objects themselves must be colonial Tunicates of the genus Pyrosoma. These form hollow cylinders with an opening at one end out of which passes a continuous current of water. The individuals of the colony lie in the gelatinous wall and each one constantly draws in water from outside and ejects it into the central cavity, producing a sort of jet propulsion by their combined efforts. Some kinds of Pyrosoma are said to be soft and pliable but others are more nearly rigid, as these were. The red colour is not common but several species have been described as pink.

"A characteristic of most species of Pyrosoma is that they are strongly luminous. Since the second observation says that the objects were seen until sunset but does not follow up with a subsequent note on luminous bodies in the water, it seems likely that the species concerned was P. spinosum, of which some colonies have been found to lack luminous organs. The John Murray Expedition collected some large colonies of this in the Arabian Sea, but unfortunately these were in fragments by the time they reached the laboratory. The expedition observed a large red Pyrosoma on the surface 'resembling a motor tyre' (or part of one), estimated at about 5 ft. in length. Another expedition has recorded colonies up to 4 m. long (or over 12 ft.)."

GIANT COLONIES OF PELAGIC TUNICATES .....

Griffin, D. J. G., and Yaldwyn, J. C.; *Nature,* 226:464, 1970.

During 1967, reports reached the Australian Museum, Sydney, of very large cylindrical objects drifting in shallow waters off central and southern New South Wales. We shall summarize some of these reports here; a further account will be published elsewhere.

Two of the objects were seen underwater near rocks, one was seen on the surface 5 miles offshore and portions of another were taken from the stomach of a carangid fish (trevally). The largest was estimated to be at least 30 feet long and 3 feet in diameter; the other entire object was about 12 feet long and 3 feet in diameter. Colours ranged from bright pink (underwater observations) to bright red (surface observation). The general shape was elongate, cylindrical, tubular, widest towards one end and gently tapering towards the other. Walls were about 3/4 inch thick and the surface was rough and papillate. The 12 foot object was the shape of a whale, although pliable and readily deformed by surface water movements and the 30 foot object had the same general shape and characters. Both were horizontally free-floating. Similar objects were seen on more than six occasions during June and July 1969, and reports appeared in the Sydney press. Estimates of their length ranged from 14 feet to 45 feet and they were about 2 feet across. All were described as cylindrical and pinkish in colour with an irregular surface.

These descriptions agree with those received in 1967. Most were seen at the surface, but two, one estimated to be more than 30 feet long and one 3 feet long, were seen by skin-divers 50-85 feet down.

These observations strongly suggest giant colonies of the pelagic tunicate Pyrosoma. Berrill has described these colonies as being formed of numerous individuals embedded in the wall of a gelatinous tube, which is usually elongate, more or less cylindrical, closed at one end and open at the other. The individual zooids have their branchial siphons opening to the inside of the tube, which is thus a common cloacal cavity for the colony. Each individual passes a current of water from the exterior into the interior, where all the individual currents form a powerful stream emerging from the open posterior end of the tube. Locomotion is accordingly a continuous jet-propulsion.

The nine species of Pyrosoma are placed in two sub-genera, Pyrosoma ("Pyrosoma ambulata" of early authors and Pyrostremma ("Pyrosoma fixata"). While all the species have been recorded in the Indo-Pacific, only one species, P. atlanticum, is known from the Australian area. This species (with seven named subspecies) has irregularly arranged zooids with relatively long branchial siphons opening at the summit of truncate test processes or spines, thirteen to twenty pharyngeal bars and twenty-five to thirty-six rows of stigmata arranged at right-angles to the longitudinal axis of the zooid. Colonies reach a size of about 60 cm^3.

The objects found in 1967 and 1969 were not P. atlanticum, and have been identified as colonies of Pysosoma (Pyrostremma) spinosum Herdman, 1888, which has an outer surface covered with pyramid-like (quadrangular) spines. In the zooids, the branchial siphon is surrounded by more than twelve tentacles, the ventral ones being longer than any others. The rows of stigmata are obliquely orientated to the longitudinal axis of the zooid; there are about fifty gill slits and thirty transverse bars. This species has been recorded previously from the north and south Atlantic, Indian Ocean and northern Pacific (Philippine area), but not from the southern Pacific.

There have been some reports of large sea objects that could have been examples of Pyrosoma. Berrill's underwater colour photograph of a long cylindrical object, reputedly a large Pyrosoma, shows with remarkable clarity (considering the assumed distance of the object from the camera) a close, continuous, spiralling, lineal structure. No pyrosomas are described as looking like this, and colonies in the underwater photographs we have seen do not show any trace of spiral lineation of zooids. The photograph, however, is similar to one of the egg mass of a cephalopod. In 1966, a 15 foot marine monster, called "Marvin", was photographed by a Shell Oil photographer off Santa Barbara California, at a depth of 180 feet. This free-swimming, cylindrical, striped object variously identified as a etenophore, siphonophore or salp, has all the appearances of a large Pyrosoma colony.

Colonies of Pyrosoma, it seems, can grow much longer than the previously recorded maximum of 4 metres. Two colonies at least

30 feet long and one estimated at 45 feet have been seen off the coast of south-eastern Australia. All these giant pyrosomas including the previously recorded 4 m colony, have been identified as Pyrosoma spinosum Herman.

UMBELLULA IN ITS DEEP-SEA HABITAT

Gilluly, Richard H.; *Science News*, 97:586-587, 1970.

Because of the pressure differentials between great ocean depths and the surface, animals sometimes literally explode when brought from the bottom. At best, they usually die.

This has been the case with members of the genus Umbellula (the only genus in the family Umbellulidae), deep-sea polyps related to coral. Recently, however, Walter H. Jahn, a geologist with the Naval Oceanographic Offic, photographed an Umbellula with remote equipment down 15,900 feet on an abyssal plain in the Atlantic Ocean 350 miles off the coast of West Africa (SN:5/9, p. 459). The Navy believes Jahn's photograph is the first ever taken of the animal in its native habitat and that the photographic technique offers an important new approach to studying deepsea marine life.

In Jahn's photograph, the Umbellula (the name assigned for convenience because of the lack of a species identification) resembles an exotic flower at the end of a straight, rigid, three-foot stem implanted in the bottom sediments. In reality, Umbellula is a colonial animal, and what Jahn photographed were six polyps. Each one of the cylindrical arms at the end of the stem is a single animal.

"This is a rather small specimen, actually," says Dr. F.M. Bayer of the University of Miami, a marine biologist who specializes in coelenterates, the phylum that includes the Umbellula, its close relatives the coral and sea anemones, and more distant relatives including jellyfish. "It is possible for 20 or 30 individual animals to be attached to a single stem."

The first polyp to establish itself in a particular location apparently forms a calcereous stem and the muscular peduncle at its bottom that serves as an anchor. Then other polyps join the first one, and the first stem and peduncle serve them all.

At the outer end of each of the polyps are tentacles, covered with nematocysts, or stinging cells, a feature of most coelenterates. These cells immobilize the prey--"almost any animal that happens to pass by, and in those depths that's not much," says Dr. Bayer--and the tentacles then carry the prey to the mouth. The mouth is simply an opening to the gastrovascular cavity, which is the entire inside of the polyp and which is divided by weight mesenteries that increase the digestive surface. There are stinging cells at the forward edges of the mesenteries that subdue any remaining activity by prey. Dr. Bayer suspects the prey are probably small shrimp and worms, and occasionally fish.

The polyps' gastric tracts are one-way tubes; waste materials are discharged from the mouth

. . . . .

CURIOSITIES OF BEHAVIOR

• Singing Invertebrates

"SINGING" EARTHWORMS

Ruedemann, Rudolf; *Science*, 65:163, 1927.

An article in the Literary Digest of October 9, 1926, has been sent to me by Professor Jesse E. Hyde, of Western Reserve University, because he remembered my mentioning the observation of sound-producing earthworms. The article reports, under the heading "When the Earthworms sing Together" the observation of Professor Mangold, of Freiburg, Germany, that "the earthworms possess voices and that they actually are in the habit of uttering slight sounds, and that they do this not singly but in series marked by definite and varying rhythm."

Seeing that the fact that earthworms make noises had not been known before, as I had assumed, I wish to record the observation that also American earthworms produce sound.

It was first pointed out to me by Mrs. Ruedemann about a decade ago, on a sultry May evening, that the earthworms in our garden back of the house could be distinctly heard. Being incredulous at first, I sat quietly on a chair until I also heard an exceedingly fine rasping noise all around me. It was a chorus of almost unbelievably small voices in the dark. To find out whether the little musicians were really earthworms, I got a flashlight and when the voices, after the quiet resulting from the disturbances of walking over the ground, were again in full chorus, turned the light upon a point close to me, from which I was sure a rasping sound arose. The light revealed a large earthworm, partly stretched out of its burrow. I spotted several more afterwards. We two have since heard the singing every year, always on warm spring evenings about and after dusk. Mrs. Ruedemann also heard it last spring about 4 o'clock in the afternoon on a warm

May day after a rain, and then she could see the "singing" worms all partly stretched out of their burrows.

From the rasping character of the sound and the position of the worms I inferred that the noise was made by the drawing of the setae over some hard object at the edge of the burrow, and the time of the year suggested that the concert is connected with the mating season of the worms. Professor Mangold, on the other hand, concludes that the sound is made through the mouth and is more of the character of clicks, which however may "sometimes become so rapid as to form a buzzing noise." These noises were made only in the burrows in his aquarium.

A member of the museum staff, Mr. Jacob Van Deloo, tells me that he heard the sound frequently, when a boy.

Not being aware that this "musical talent" of the earthworms was unknown to naturalists, I failed to catch some of the musicians for identification. Dr. S. C. Bishop, of the New York State Museum, intends to make a study of this, this spring.

IN RE "SINGING EARTHWORMS"

Walton, W. H.; *Science,* 65:574, 1927.

Since the publication in 1926 of a popular article entitled "When Earthworms sing Together," quoting Dr. Mangold, of Freiburg, Germany, and which has been previously referred to in these columns, considerable publicity has been given the subject by the American press.

For "lo these many days," it has been the writer's custom to keep captive, in numbers as large as one hundred or more, adult specimens of the large cosmopolitan earthworm, Lumbricus terrestris Linn. They are kept in a five-gallon earthenware crock in a cool corner of the cellar for use in a pursuit which in some states of the Union is considered immoral or at least illegal when indulged in on Sunday. In the course of my dealings with these worms I have many times heard the sounds recently referred to as "singing" and, although personally fond of music, have failed to notice anything in the least musical about these faint clicking sounds or stridulations, recently termed "song." The singing of insects, for instance, could be considered as symphonic poems when compared with these insignificant rustlings. Previous to the publication of the recent somewhat sensational statements, little attention was paid to them as it had seemed to me that these sounds were probably produced by the movements of the worms in their burrows, possibly by the escape of air between the viscid lining of the burrow and the mucous surface of the worm's body. It is quite evident, however, that this is not the case, because these stridulations have continued after the worms were transferred from the soil to damp sphagnum moss, which is an ideal medium in preparing the worms for the rites to which I have previously alluded. The determination of the manner in which earthworms pro-

duce these mysterious sounds is fraught with difficulty because the species with which I have dealt at least is very sensitive to the presence of light intense enough to permit one clearly to observe its actions and movements, when on the surface of the soil. That this sense of light resides in the extreme anterior end of the body is abundantly evident from the fact that the worms instantly withdraw to their burrows when the light from an electric torch is flashed upon them even when but a half inch or less of the "head" end of the body protrudes from the soil. In point of fact, the worms seem most sensitive to the light when this is the case. When the entire body is exposed it often requires as much as fifteen or twenty seconds for the worm to become alarmed at the light. The production of the stridulating sounds, however, is not by any means confined to those individuals at the surface, but may be heard plainly, at least under captive conditions, when no worms are visible.

The anonymous author of the original article, previously cited, refers to the earthworm as "dumb both in a legitimate and colloquial sense," but the story of the earthworm as recorded long ago by Charles Darwin abundantly indicates that these lowly creatures are indeed far from stupid, but apparently possess a seemingly disproportionate degree of intelligence.

MUSIC OF SNAILS

Salwey, T.; *Magazine of Natural History,* 1:107–108, 1837.

Three or four years ago, whilst sitting in my room reading late at night, my attention was attracted, for several nights in succession, to a sort of low musical note, which seemed to proceed from one of the windows. It occurred at short intervals, and was sometimes silent for a quarter or half an hour, and then returned again. I thought, at first, it proceeded from something in the room, or arose from some accidental vibration of one of the strings of a pianoforte which stood near the window; but, being satisfied at length that it proceeded from the window itself, I drew up the blind, and discovered one of the common large garden snails crawling upon one of the panes. Immediately upon my drawing up the blind, and the light of the candle being strongly thrown upon the window, the sound ceased, and the snail partly withdrew itself under its shell; nor were its motions resumed so long as the light continued to be thrown upon the window. Being now satisfied, however, that the sound was caused by the motions of the snail on the pane of glass, I carefully noticed the exact position of the animal; and, putting the candle in such a situation that no light should be thrown upon the window, I returned to it, and sat down close to the place where the snail was. In a few minutes the sound returned, and I had now no father doubt of its being caused by the snail; for, bringing the candle back again so as just to throw light enough upon the window to enable me to

perceive his motions, I observed that it was now moving on again across the pane, and the sound evidently accompanied it. Upon stopping its motion again as before, the sound ceased; but always returned when it moved on. I suspected at the time, with the correspondent of the Naturalist, that the sound proceeded from the snail itself, as I was not able to ascertain whether the shell actually was in any part in contact with the glass; but I concluded that this must be the case, and that the sound was merely caused by the slow scraping of the shell on the moistened surface of the glass, producing a phenomenon somewhat similar to that which children amuse themselves in producing by passing a wet finger over the edge of a glass containing water. As far as I recollect, it was always in wet weather when the phenomenon occurred, and when the whole surface of the glass would be overspread with moisture. The above phenomenon has been one of frequent occurrence since the time I first noticed it; and, if it be traceable to the same cause as in the other instance to which I have alluded, perhaps the body of the animal might cause a sufficient vibration in the glass to produce the sound, even if the shell were not in contact with it.

NOTE ON SOUNDS EMITTED BY MOLLUSCA

Portlock; *Report of the British Association, 1848,* part 2, p. 80.

I think it right to draw attention to the Helix aperta, which is very remarkable for its property of emitting, when irritated, a strong and well-marked sound. When I first noticed the sound thus emitted on accidentally touching the animal, I was peculiarly struck by it and immediately referred to Rossmaesler, who I found describes the quality of the animal in a very graphic manner, stating that the sounds were such as indicated irritation. The Helix aperta is very abundant at Corfu, appearing thickly on the squill leaves in the spring, when about the beginning of March the annual increment of growth of the shell is perfectly soft. If the animal be irritated by a touch with a piece of straw or other light material, it emits a distinctly audible sound possessing a singular grumbling or querulous tone. This it frequently repeats if freshly touched, and continues so to do for apparently an unlimited space of time, as I kept one for a considerable time in my house, and heard this sound whenever I touched it.

As Rossmaesler has so fully described this fact, I shall only add that I have, on more occasions than one, heard what I considered a similar, though very feeble sound from the Helix aspersa, and I need not say that the explanation seems very easy from the structure of the animal.

• Swarming and Population Explosions

PELAGIA IN MANX WATERS

Cole, F. J.; *Nature,* 170:587, 1952.

The occurrence of the scyphomedusan Pelagia near Plymouth referred to recently in Nature reminds one that early in October of 1899 the genus invaded Port Erin Bay, Isle of Man, in astonishing numbers. It was a truly remarkable spectacle. The sea looked as if converted into a solid mass of the jelly-fish. The species had not been reported in Manx waters before, nor has it been seen there since. They appeared suddenly, remained for a short time, and as suddenly disappeared. When the record was posted to the late E. T. Browne, he was at first very sceptical as to our identification; but an examination of some actual specimens enabled him to recognize the animal as Pelagia perla. He assumed that its presence in Manx waters was due to "a northerly current bringing down animals through the north channel from the Atlantic shores of Ireland and Scotland". Two of the specimens are preserved in the Zoology Museum, University of Reading. It may be added that on August 10, 1903, a large shoal of Pelagia appeared in Valencia Harbour, along with a huge drift of Salpas on which the pelagias were feeding.

• Unexplained Mass Deaths

WHY DO WORMS DIE?

Friend, Hilderic; *Nature,* 107:172, 1921.

The middle of March saw the slaughter of millions of worms. Morning by morning the pavements, roads, and pathways were strewn with the dead. Great and small, young and old, of every known species and genus, from Lumbricus to Dendrobaena, lay prone. Even if they were able to reach the pasture, lawn, or grass-plot alive, they had not the power to burrow and recuperate. What caused their death? I have asked the question for thirty years, but have never found the answer.

Four main theories have been advanced. They are killed, folks say, by (1) parasites, (2) cold, (3) rain, or (4) poison.

The first theory has long been maintained. It was held by

Darwin ("Vegetable Mould," p. 14), who said that worms were affected by a parasitic fly. The parasites of worms are of very many kinds, but I have collected large numbers of dead and dying worms and examined them with care, yet have found nothing abnormal in this direction. Since worms are cold-blooded creatures they can endure a low temperature without suffering. Moreover, they are often found dead in the spring, when the temperature recorded for the night has not been below 34°.

Darwin (p.125) speaks of Mr. Scott's surprise when told how long they could endure being submerged, "as he did not know how long worms could survive beneath water." It is practically impossible to drown them in a brief time, such as is allowed for their slaughter day by day at this season of the year. And yet in some way showery weather seems to be essential. After March 21 no showers fell at night, and no worms lay dead in the morning.

There remaines the miasma theory. "Nature uses poison gas," says the speculator. This theory would seem good if worms were found dead on tarmac roads, but not on gravel paths, and if they died in a similar way all the year round. But such is not the case. Thus every theory seems to fail.

The worms appear to be paralysed. They crawl at first with vigour, then the rate of progress declines. Eventually they cease to move, die, swell in places or along the whole length of the body, and ultimately become the prey of various scavengers, but are totally ignored by the birds.

It seems clear that the conditions required are warm days and evenings, moisture in the way of showers during the night and early morning, and then a cold snap, but not necessarily a frost. Does the combination of cold and moisture paralyse them? Are the dorsal pores choked? Or are they exhausted in their efforts to regain their closed burrows? At present I am unable to carry out the research and experiments upon which alone a satisfactory judgment can be based. Has anyone ever found the answer?

• Power of Flight in Molluscs

FLYING MOLLUSCS

Gladden, R. J.; *New Scientist*, 33:556, 1967.

Your contributor who wrote, in the note on "killer snails" (9 February), "Molluscs are marvellous things. They can do almost everything except fly" has made a scientific blob. They

can fly, at any rate some of them can. He even lists the flyers, the Squids-Phylum Mollusca.

It is a not very well known zoological fact that these animals--they propel themselves in water by pumping it with great force through a tube along the side of the body--can get up sufficient speed to leave the water. They then unfold two flaps of skin, which are ordinarily used for steering and quiet swimming, and glide like flying fish for distances of fifty yards or so.

Let your contributor refer to Thor Heyerdahl's Kon Tiki Expedition, Chapter 5 entitled "Halfway", where he will read incontrovertible evidence of this squidoddity, and will recognize the wording of my second paragraph.

THE FLYING SQUID

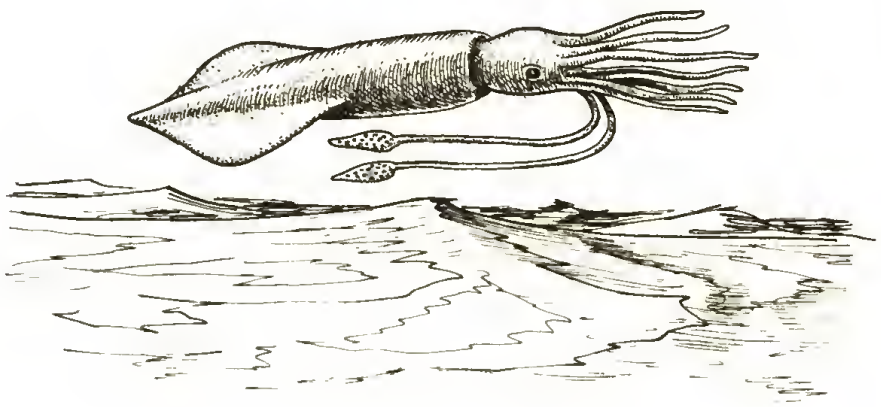
Williams, W. Matthieu; *English Mechanic,* 20:505, 1875.

I have read with much interest Mr. Proctor's expositions of the philosophy of aerial navigation, and some of the correspondence on the same in the English Mechanic, but have not met with any reference to a very striking illustration of the great sustaining power of atmospheric reaction for which Mr. Proctor contends.

The illustration to which I refer is the flight of the Loligo, or "flying squid." This animal is one of the Sepiadae, or cuttlefishes, and quite different from either of the flying fishes properly so-called (the Exocetus or the Dactylopterus. The flying squid has no wing-like fins, and yet it rises from the sea and skims through the air in a remarkably bird-like manner. When I first saw a shoal of these at a distance I supposed them, as a matter of course, to be small sea birds such as "Mother Carey's chicken's" but was puzzled by their great numbers and very small size. This was in the Mediterranean, between Cape Matapan and the north coast of Africa. I saw a thousand or more in the course of a few days, flying in shoals or flocks. Some of them skimmed clear across both bulwarks of our vessel, a schooner, and into the sea on the other side, and a few fell on the deck. These were about 8 in. long, with a head, goggle-eyes, beak, and tentacles, like the common cuttlefish of our coasts, the sepia officinalis from which the pounce bones are obtained. The head was inserted in a sheath or body more slender than that of our common cuttlefish, and supported internally by a horny paddle-shaped rod in the place of the ordinary sepia, or pounce bone. The tail end of the sheath had a fin-like extension on each side of about the shape and proportion of an anchor fluke. These flaps or flukes were about half as long as the sheath, or one-third of the length of the whole animal, and their extension on each side did not exceed that of the thickness of the middle part of the body. They were smooth and fleshy, not ribbed or bony, like the fins of a fish, but were rather stiff and somewhat cartilaginous. They are evidently the whole apparatus of flight. The appended rough

sketch from memory may afford some idea of the shape of the animal.

It was many years ago when I saw these creatures. Being then unacquainted with the habits of the cuttlefish, I made no observation enabling me to answer a curious question which now suggests itself, viz., Does the squid fly head foremost or tail



foremost? The general mode of cuttlefish locomotion gives probable precedence to the tail, and dynamical considerations appear to strengthen this probability, but Mr. Proctor can deal with this question better than I can. Given a fish-shaped body truncated at one end and tapering, thinning, and outspread, with flaps at the other; which end should be projected foremost in order to secure the maximum sustentation from atmospheric reaction?

I can only make a rough estimate of the length and height of flight. The height reached about 15 ft., and the length exceeded 100 yards. The range of flight varied very little with the individual animal. This was shown very plainly when a shoal of 30 or 40 were all skimming through the air together. The curve described by their paths was remarkably uniform.

There were many bonettas and porpoises about, and the squids were evidently hunted by them, and their plunges into the air were made for the purpose of evading pursuit. These squids are highly prized by sailors as bait for bonettas and other large fish.

Assuming that the squid, like some other cuttlefishes, has the power of darting rocket-like and tail foremost through the water by first distending its bag-like body and then driving out its watery contents in powerful jets through the openings by the side of its head; I suppose the rationale of its flight to be as follows:--- The squid is pursued by a large fish, it dashes forwards and slightly upwards with a force capable of driving it rapidly through the water in spite of the great resistance of that medium. Presently the foremost or tail end plunges from water into air, and there meets with a much diminished resistance, but the head and spouting or locomotive apparatus is still immersed in water; thus the impelling force is maintained for some little time after the re-

sistance has diminshed, and hence an important acceleration must occur just during the critical time while the animal is leaving the water. Assuming that the squid has the power of maintaining the horizontal position of the flukes during flight as well as while swimming, the gravitation of the heavier head would determine the required axial inclination upward in the direction of progress. Thus the squid nearly resembles Mr. Proctor's ideal machine that is to be started by an accelerating run down an inclined plane, excepting that Mr. Proctor puts his tail behind, while if I am right, the squid flies tail foremost. Query, would Mr. Proctor's machine be improved by adoption of the paradoxical proceeding of the squid?

• Strange Examples of Parasitism

PARASITISM

Dewar, Douglas; *The Transformist Illusion,* DeHoff Publications, Murfreesboro, 1957, pp. 216–216.

The existence of hundreds of different kinds of endoparasites presents a formidable, if not insuperable objection to the evolution theory. An endoparasite is one which must live during all or part of its life inside another organism, and which perishes if prevented from so doing. A.C. Chandler certainly does not overstate the difficulty when he writes ("Introduction to Parasitology" (1944) p.14): "It would be difficult, if not impossible, to explain, step by step, the details of the process of evolution by which some of the highly specialized parasites reached their present condition."

Consider the liver-fluke, Distomum hepaticum. This spends its adult life in the intestine of a sheep. The eggs after being laid pass out of the sheep into the open where they hatch out as little ciliated embryos that swim about in water. In order to survive, these larvae must find a pond snail (limnaea truncatula.) Having reached the snail the larva finds its way into the pulmonary chamber. Here it loses its cilia, increases in size and buds off germinal cells into its own body-cavity. While in this condition it is known as a sporocyst. The buds it casts off develop into secondary larvae known as rediae; these are elongated oval in shape, have a mouth and stomach, and are provided on the under side, with a pair of knob-like processes by the aid of which they move. They force their way out of the sporocyst and enter the tissues of the snail where they give birth to other rediae that develop into larvae known as cercariae, which in outward appearance resemble broad-bodied tadpoles, and even

exhibit a "notochord." Using the long tail as an organ of locomotion, the cercariae work their way out of the snail, then attach themselves to blades of grass where each sheds its tail and encases itself in a tough sheath. They remain in this condition until they happen to be eaten by a sheep when grazing. Inside the sheep they find their way into its liver where they develop into the adult state and acquire sexual organs. They then migrate into the intestine of the sheep and thus complete the cycle of their life history.

Consider the improbability of a free-swimming worm evolving into a liver-fluke. Such a creature would be killed by being swallowed by a sheep unless it were endowed with the ability to resist the dissolving activities of the saliva and other digestive juices. Moreover its eggs would have to similarly equipped and to have the power of hatching out into larvae having the instinct of finding their way into the pulmonary chamber of a certain species of pond snail.

Notice that in the earliest part of its life the liver-fluke has to be adapted to life in the open, next to existence in the pulmonary chamber of a snail, then to living inside its tissues, after that once more to life in the open, then in the stomach of the sheep and next in its liver, and finally in its intestine. Failure of the animal to accommodate itself to any of this succession of conditions would lead to the rapid extermination of the species. Moreover the liver fluke is not the only species to enjoy such a variety of environments. Hundreds of other species lead a similar existence; in some the variety of environments is even greater. Thus the tape-worm, Diphyllobothrium latum, spends part of its life in a copepod, part in a fish and part inside a human being.

THE ALIEN STRATEGY

Love, Milton; *Natural History*, 89:30–32, May 1980.

Some parasites have opted for a different strategy: they alter the normal behavior of their hosts so as to increase their chances of transmission to other organisms. An example is the sheep liver trematode, Dicrocoelium dendriticum. Although a sheep is the definitive host, intermediate ones through which the worm must pass are a snail and an ant. Since ants do not form the bulk of a sheep's diet, the problem would be to increase the opportunity for sheep to accidentally swallow infected ant hosts. We might expect there to be any number of frustrated sheep liver trematodes frittering sway their golden years inside ants.

In fact, the worm overcomes this dilemma by burrowing into the ant's brain and forcing it to behave so as to optimize the chances of its being ingested by a sheep. Normal behavior for an uninfected ant is to be active on the soil surface during the cool mornings and afternoons, retreating to burrows during midday, when higher temperatures might be detrimental. A para-

sitized ant, however, climbs up to the top of a grass blade during morning hours, clamps its jaws into the blade, and becomes immobile. As midday temperatures rise, the ant retreats down the blade, only to return in the cooler afternoon.

This is an elegant relationship indeed. The ant's (and the parasite's) chances of being eaten by sheep are increased, but the host's midday retreat is still allowed, insuring that the host will survive the higher temperatures and repeat the behavior if necessary.

. . . . .

Sphaerularia bombi is a nematode that infects queen bumblebees. In late summer and fall the queens burrow into soil banks and hibernate. There they are infected with adult female nematodes. Once inside the queens, the females produce larvae, which eventually take control of the bees. Instead of making burrows when the next hibernation season arrives, infected bees fly about continually and dig small soil depressions into which they deposit nematode larvae. The infected queens die soon afterward, while the larval worms mature, males and females mate, and the fertilized female worms lie in readiness for unparasitized queen bees.

. . . . .

• Where Resides Flatworm Memory?

FLAT WORMS FOR STUDY OF MEMORY STORAGE

Anonymous; *New Scientist*, 9:318, 1961.

An investigation into the learning ability and memory of flatworms by Dr. J.V. McConnell and his collaborators in the University of Michigan has recently attracted some attention, though not always for the right reasons. In some early experiments, Dr. McConnell and Dr. Thompson found that it is possible to establish a conditioned reflex in the planarian Dugesia. The worm was trained to contract when a light was switched on (although it does not normally do so) by coupling illumination with a slight electric shock. Initial training takes some 150-200 experiences of this treatment, but if the worms are left undisturbed for 4 weeks they can be retrained after only 40 trials, showing that a considerable amount of the learned behaviour is retained.

More recently, Dr. McConnell has studied the retention of learned behaviour in regenerated fragments of the worm. The

worms are first trained, then cut in half. After 4 weeks each half has grown into a whole worm, and if these are retrained it is found that both have retained as much of the original training as if the worm had never been bisected. It is clear from these experiments that memory is not stored in the cerebral ganglion, for the brain of the worm that was originally trained is carried over into only one of the regenerated worms; the other has grown a new cerebral ganglion.

The cerebral ganglion of worms is not quite the dominating part of the nervous system that the brain is in higher vertebrates, and the possibility that memory is stored diffusely throughout the nervous systems in not particularly disturbing to zoologists and psychologists. But experiments carried out by Dr. D.M. Maynard and Miss R. Jacobson, also of the University of Michigan, dispose even of this possiblity.

They bisected an untrained worm and then trained the head-end to contract when illuminated. They left the head to regenerate a new tail and then, without further training, bisected this individual and allowed it to regenerate. They tested each of the resulting two individuals to find how much of the original training had persisted, and both had retained a great deal.

Now the individual that had grown from the front half of the bisected worm contained the cerebral ganglion of the piece that was originally trained, but the other individual was composed entirely of regenerated tissue and contained none of the original nervous system. Memory storage, therefore, cannot depend upon the nervous system remaining intact, and these experiments give strong support to the theory that when flatworms learn there is a diffuse biochemical change in the animal rather than a change only in the nervous system.

. . . . .

• Possible Chemical Transfer of Learning

CANNIBALISM AND MEMORY IN FLATWORMS

McConnell, James V.; *New Scientist,* 21:465-468, 1964.

When I came to the University of Michigan in 1956, I interested Allan L. Jacobson and Daniel P. Kimble in working with me in an extension of the Texas experiment. Most scientists probably first encountered planarians when taking an introductory course in zoology, for these animals are famous for their powers of regeneration. If a member of the species Thompson and I used is cut in half across the middle, each half will regenerate into a complete

organism in a matter of a few weeks. Indeed, the tail section will regrow a new brain and new eyes within five or six days. It occurred to Thompson and me, during that first study, that it would be interesting to condition an animal and then cut it in half. We would then retrain both head and tail regenerates to see if either half showed any evidence of retaining any of the prior conditioning. That is precisely the study which Jacobson, Kimble and I performed.

Rather than using the massed series of light-shock trials that Thompson and I had employed, we gave our animals only 25 to 50 trials a day, with a minute or more between trials. Under these conditions, the planarians showed much more rapid learning than before and it took but 134 trails on the average, to bring our experimental group to a criterion of 23 conditioned responses out of any block of 25 contiguous trials. Once the animals reached this criterion, they were immediately cut in half and given some 4 weeks to regenerate, after which they were retrained to the original criterion.

We expected the heads to show retention of the earlier learning for, after all, they retained the brain of the original animal. As Table 2 indicates, the head regenerates showed no more "forgetting" over the month's rest than did our control animals which were conditioned but not cut in half. We were not prepared for the equally great amount of savings shown by the tail regenerates, however. Even though the tail sections had to grow an entirely new brain, many of them displayed almost perfect retention of the original training. Indeed, many later studies both in my own and in other laboratories have shown that the tails typically show <u>better</u> retention than do the heads, probably because the tails, lacking brains, are protected from the interfering effects of new learning during regeneration, while the heads are not.

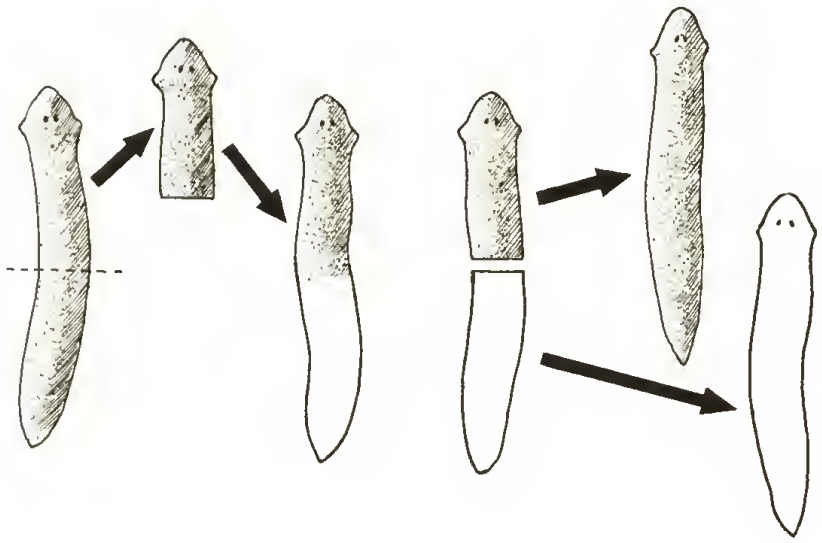
The animals in our regeneration control group were given training trials only <u>after</u> they had first been cut in half and allowed to regenerate. The data from this control group suggest that cutting and regeneration in and of themselves do not sensitise the planarians to light; indeed, the opposite is the case.

The behaviour of our tail regenerates was, to say the least, rather unsettling. We were forced to conclude that memories were not necessarily stored just in the planarian's brain. In fact, our data suggested that learning resulted from some biochemical change which was widespread throughout the planarian's body, for subsequent informal studies showed that we got essentially the same retention whether we cut the worm in halves, thirds or fourths.

This "chemical" hypothesis ws tested by Reeva Jacobson (no relation of Allan Jacobson, but recently married to Daniel Kimble). Working under the direction of zoologist Donald Maynard and myself, she cut animals in half, discarded the tails and trained <u>only</u> the heads. Once the animals had reached the criterion, they were given several weeks to grow new tails. The tails were then cut off and both heads and tails again allowed to regenerate. As shown in Figure 2, the tail regenerates were completely reformed

animals. Yet, upon testing, these animals showed significant retention of the original learning. Our chemical theory, naive as it was, seemed a trifle more tenable. But what chemical, or chemicals, were involved?

. . . . .



Test of the "chemical" hypothesis of learning in planarians. First, a worm was cut in half and the tail discarded. The head was immediately trained before regeneration could take place. After training, the head was allowed to regenerate, then cut in half again, and both halves allowed to regenerate. Then both halves were trained to the original criterion. (Fig. 2)

At about this time (1961), it occurred to us that, if memories were stored chemically in the planarian body, we might be able to transfer some part of the "learning" from one animal to another if somehow we could get the chemicals out of one worm and into the second. We tried many unsuccessful techniques for making the transfer before we hit upon "nature's way"---letting the planarians do the transferring for us. Jay Boyd Best, one of the first experimenters to train planarians in a maze, wrote to us about an observation he had just made---the hungrier the worms were, the more readily did they turn cannibal.

Barbara Humphries, Reeva Jacobson and I at once starved some untrained planarians and, when they were quite hungry, fed to them pieces of other planarians which had just been classically conditioned. Once the cannibals had had several meals of "educated" tissue, they were given their first conditioning sessions. To our great surprise (and pleasure), from their very first trials onward the cannibals showed significant evidence that they had somehow "ingested" part of the training along with the trained tissue. We repeated this study four times, using control groups

which were fed untrained tissue and performing the experiments in "blind" fashion (i.e., the person doing the training of the cannibals did not know which animal belonged in which group). Each time we obtained the same results. Shortly thereafter, Karpick and John repeated the study at the University of Rochester and, since then, other investigators from California to England have replicated our original observations. Somehow, in a fashion still not clear to us, some part of the learning process seems to be transferable from one flatworm to another via ingestion.

. . . . .

In my view, the cannibalism data suggest that a major overhaul of our theories of learning is long overdue, for these data imply that, while contiguity is often a sufficient condition for the establishment of an engram, it is by no means a necessary condition. It may well be profitable to consider "learning" as being nothing more nor less than a chemical change which brings about a relatively permanent alteration in the organism's behaviour. No matter how the chemical change was induced, whether by "psychological contiguity" or by "cannibalism", learning would have taken place. In the past, since we knew little or nothing about the nucleic-acid chemistry of the neuron, the only way in which an engram could readily be created in an organism was by putting the animal through a training regimen. Once we know specifically which biochemical alterations cause which types of behavioural changes, however, we should theoretically be able to induce the necessary chemical alterations in a wide variety of ways. Indeed, we might even be able to synthesise "memories" in a test tube and inject them directly into the animal's neurons, thus causing the animal to "learn" in a fashion undreamed of by the early association theorists. One of the most far-reaching effects of the cannibalism and chemical research, then, may be a re-examination of what was once considered the most basic postulate of all learning theories, the law of psychological contiguity.

PLANARIANS AND MEMORY

Jacobson, Allan L., et al; *Nature*, 209:599-601, 1966.

1. Transfer of Learning by Injection of Ribonucleic Acid. Several recent investigations of planarians have contributed suggestive evidence regarding the biochemical basis of memory. McConnell and John found that cannibal worms which had ingested classically conditioned worms later performed better than did cannibals which had ingested untrained worms. Corning and John obtained evidence that ribonuclease may 'wipe out' conditioning in regenerating planarians. Two preliminary reports have indicated that injection of ribonucleic acid (RNA) extracted from trained planarians may enhance responding in untrained planarians.

[Details of study omitted.]

Several conclusions can be drawn from this experiment: First, classical conditioning in planarians is a distinct and valid phenomenon, and pseudo-conditioning effects, at least under the present conditions, are negligible. This finding corroborates that of Baxter and Kimmel, but in the present experiment the differences between groups were much more pronounced. Secondly, injection of the RNA preparation from group CC was sufficient to induce responsiveness to the light which could not be accounted for on the basis of nutrition, handling, or stimulation received by the donor group. Rather, the critical feature for both the conditioning effect and the RNA transfer effect was apparently the contiguity between the stimuli (light and shock). Since such contiguity is generally considered to be the defining characteristic of 'true' conditioning, the present data would seem to suggest that a specific learned response was transferred by way of the injection of the RNA preparation. This conclusion is strengthened by the fact that no shock was used during testing, and thus no new learning could have occurred.

. . . . .

• Use of Tools and Artifacts

WORM EATS SMALL ENEMY MERELY TO GET ITS WEAPONS

Anonymous; *Science News Letter*, 28:342-343, 1935.

The hero of this small zoological saga is a worm, known scientifically as Microstomum. Its enemy is a creature called Hydra, belonging to a still lower order on the evolutionary scale. They live in freshwater ponds and quiet streams.

Hydra possesses hundreds of little daggers, in the form of explosive stinging cells. With these it paralyzes and captures its prey, and also defends itself against enemies.

Microstomum has no such armament. It attacks, kills and eats Hydra for the sole purpose, apparently, of getting possession of the latter's stinging cells. Hydra cannot defend itself against Microstomum's attack, because the worm secretes a substance that protects it from the tiny daggers. Only when it unwittingly approaches Hydra is it vulnerable.

Once Microstomum has captured and eaten its enemy-victim, the stinging cells go through a most astonishing course. First, the white blood corpuscles of the worm, which move about in its body as freely as though they were tiny independent one-celled animals, take charge of the cells. There are three types

of these; and one of them, which is of no use to the worm, is digested and disappears.

The other two types of stinging cells, which are well adapted to Microstomum's defensive uses, are carried to the surface layers of the body, and there arranged into a formidable defensive armament by cooperation between the white blood cells and the fixed cells of the body.

Thus Microstomum is given a complete borrowed armament, which aids in repelling its enemies.

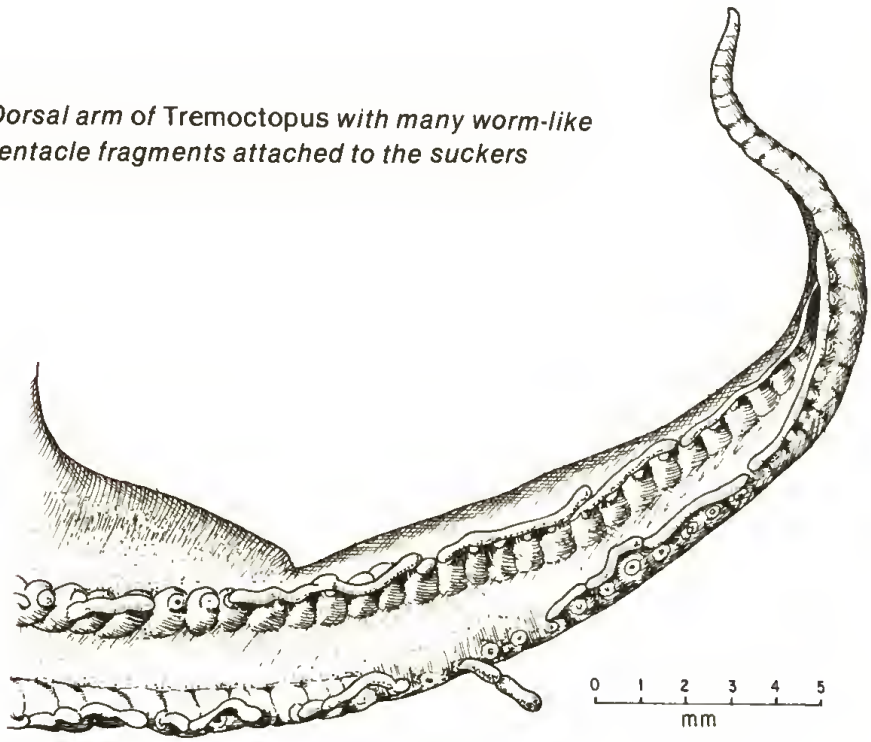
This armament, moreover, is useful not only to the worm that first captures it, but can be passed on to its offspring even to the third and fourth generations.

TREMOCTOPUS VIOLACEUS USES PHYSALIA TENTACLES AS WEAPONS

Jones, Everett C.; *Science,* 139:764-766, 1963.

Abstract. Immature octopods (Tremoctopus violaceus) have been found with numerous fragments of tentacles of the colelenterate Physalia attached to the suckers of their dorsal arms. The probable method of acquisition, the evidence of adaptation for holding the tentacles, and the possibility that the octopod uses these coelenterate tentacles as offensive and defensive weapons are discussed.

Dorsal arm of Tremoctopus *with many worm-like tentacle fragments attached to the suckers*



• Effects of Electric and Magnetic Fields upon Behavior

MAGNETISM HEALS MUTILATED FLATWORMS

Anonymous; *Science News Letter,* 19:28, 1931.

Magnetism as a healing agency was suggested many years ago, but was seized upon and exploited by quacks to such an extent that it fell into complete disrepute. Now, however, comes a reputable scientist who has apparently been able to do something of the kind with a strong magnetic field, at least with lower organisms.

Prof. R.A. Muttkowski, of the University of Detroit, told the American Asociation for the Advancement of Science how he has exposed mutilated flatworms to the influence of a powerful electromagnet, and found that they grew their lost body-parts more rapidly than did untreated "control" specimens. Too much of a good thing, however, proved harmful.

RESPONSES OF THE PLANARIAN, DUGESIA, AND THE PROTOZOAN, PARAMECIUM, TO VERY WEAK HORIZONTAL MAGNETIC FIELDS

Brown, Frank A., Jr.; *Biological Bulletin,* 123:264–281, 1962.

The question of whether living things are sensitive to terrestrial magnetism has undoubtedly fleeted through the minds of innumerable persons since this geophysical factor first became known. But neither the naturalist, observing the behavior of organisms in the field during their continuing responses to the myriads of more obvious physical factors, nor the experimental biologist, casually testing the response of living things to artificial magnetic fields, even very strong ones, found any consistent evidence that living creatures perceived this weak terrestrial force. It is a common observation that animals in nature may come to bear at any given moment apparently all possible compass relations in their bodily orientation; orientation of the normal resting or foraging animal to the horizontal component of magnetic field would be expected generally to be of no adaptive consequence.

In recent years, however, two kinds of phenomena have come to the forefront in biological research which are exceedingly difficult, in some instances impossible, to account for in any orthodox physiological terms. These two phenomena are the apparent

persistence, in rigorously controlled constancy of all the more obvious factors of the physical environment, of biological senses of time and space in their terrestrial relationships. The first includes the mechanism for timing the well-known persistent daily, tidal, monthly, and annual periodisms. The second involves the still completely mysterious "map sense" or capacity to localize position in space which is so evident in a wide spectrum of kinds of animals capable of "homing."

Also demanding a rational explanation is the means by which organisms, even when screened from fluctuations of every obvious weather-related factor, still display variations in metabolic rate, even of quite substantial magnitudes, correlated with the essentially aperiodic weather disturbances. There remains no reasonable doubt that organisms are sensitive to some subtle geophysical factors which pervade the ordinary "controlled" conditions of the physiology laboratory. Fluctuations in these unidentified factors must bear information related to weather changes, as well as to terrestrial time and space.

In a series of recent experiments, it has been demonstrated that the marine mudsnail, Nassarius obsoletus, is able to perceive small changes in strength of the horizontal component, H, of a magnetic field close in strength to that of the earth's natural field. The snail can, furthermore, distinguish between directions of the fields, both the earth's and weak ones produced by bar magnets. The character of the response to magnetic field exhibits rhythmic changes that are regulated by the solar-day and lunar-day "clocks" of the snails, and by a synodic monthly one. The following study was conducted to learn whether such responsiveness was of wider biological distribution.

. . . . .

Summary

1. The orientational response of the planarian, Dugesia at a given time of solar day undergoes what appears to be a semimonthly or monthly fluctuation, probably a consequence of the possession of a lunar-day rhythm in response to some compass-directional factor.

2. The monthly rhythm in Dugesia is modifiable by a weak magnetic field.

3. The monthly rhythm appears to undergo an annual modulation.

4. Dugesia differentiates between a horizontal field parallel to the long axis of the body and a field at right angles, and between N and S poles, and, furthermore, is able to resolve intermediate angular orientations of field with remarkable precision.

6. The response of Dugesia alters its character in passing from a field close to the earth's strength to one as little as 10 gauss, suggesting the perceptive mechanism to be specifically adapted to such a weak field as the geomagnetic one.

7. There is suggestive evidence that the protozoan Paramecium also responds to very weak magnetic fields.

8. Some possible roles for organisms of such astounding responsiveness to very weak magnetic fields are discussed briefly.

RESPONSE OF THE PLANARIAN, DUGESIA, TO VERY WEAK HORIZONTAL ELECTROSTATIC FIELDS

Brown, Frank A., Jr.; *Biological Bulletin*, 123:282-294, 1962.

A deep-seated, persistent, rhythmic nature, with periods identical with or close to the major natural geophysical ones, appears increasingly to be a universal biological property. Striking published correlations of activity of hermetically sealed organisms with unpredictable weather-associated atmospheric temperature and pressure changes, and with day to day irregularities in the variations in primary cosmic and general background radiations, compel the conclusion that some, normally uncontrolled, subtle pervasive forces must be effective for living systems. The earth's natural electrostatic field may be one contributing factor.

A number of reports have been published over the years advancing evidence that organisms are sensitive to electrostatic fields and their fluctuations. More recently Edwards has found that activity of flies was reduced by sudden exposures to experimental atmospheric gradients of 10 to 62 volts/cm, and that prolonged activity reduction resulted from gradient alteration with a five-minute period. In 1961, Edwards reported a small delay in moth development in a constant vertical field of 180 volts/cm., but less delay when the field was alternated. The moths tended to deposit eggs outside the experimental field, whether constant or alternating, in contrast to egg distribution of controls. Maw, studying rate of oviposition in hymenopterans, found significantly higher rates in the insects shielded from the natural field fluctuations, whether or not provided instead with a constant 1.2 volts/cm. gradient, than were found in either the natural fluctuating field, or in a field shielded from the natural one and subjected to simulated weather-system passages in the form of a fluctuating field of 0.8 volts/cm.

A study in our laboratory early in 1959 by the late Kenneth R. Penhale on the rate of locomotion in Dugesia suggested strongly that the rate was influenced by the difference in charge of expansive copper plates placed horizontally in the air about six inches above and closely below a long horizontal glass tube of water containing the worms. Locomotory rates in fields of 15 volts cm. (+ beneath the worms) were compared with those in fields between equipotential plates. The fields were obtained with a Kepco Laboratories, voltage-regulated power supply. A comparable study with the marine snail, Nassarius, by Webb, Brown and Brett, employing a Packard Instrument Co., high-voltage power supply, confirmed the occurrence of such responsiveness to vertical fields of 15 to 45 volts/cm., and advanced evidence that the response of the snails displayed a daily rhythm.

More recently, it was demonstrated that mud-snails, even while submerged in sea water, were able to resolve a horizontal field difference of 2 volts/cm. in the air at right angles to their bodies, and to exhibit a characteristic orientational response. The fields were obtained with B batteries. The snails appeared able to distinguish the direction of the very weak gradient across

their bodies. The character of the electrostatic response was altered simply by changing from South to East the compass direction in the earth's field in which the response was assayed. There seemed to be an influence upon the electrostatic response, by some natural force the effectiveness of which altered with geographical orientation of the organisms.

The following study was made in order to determine whether a comparable sensitivity to weak electrostatic fields obtains for a common fresh-water planarian, and if so, to learn more concerning its properties.

. . . . .

Discussion

In the study which is reported here, the exact values of the fields to which the animals were subjected were never known. The natural field was unquestionably reduced substantially and maximally in the horizontal axis connecting the two equipotential plates, and minimally in all axes at right angles to this, including both horizontal and vertical ones. The important thing for this study was that whatever horizontal potential gradient remained at right angles to the initial path of the worms, the experimental gradient in one direction added 2 volts per cm. to that field, and in the other direction subtracted this amount. By such means it was possible, therefore, to determine whether the animal could resolve such small changes.

The orientation of the worms in the experiment was observed while they were submerged in tap-water whose source was Lake Michigan. Such water is, relative to the surrounding air, a good conductor. Therefore, the overall electrostatic gradient to which the worms were directly subjected was far smaller than the 2 volts/cm. gradient in the air. The value can be estimated to be 6 to 8 orders of magnitude below that in the air as a consequence of the "Faraday-cage effect" of the worm's ambient aqueous medium. To exhibit such responses as the worms did in these experiments would require a sensitivity to essentially static electric gradients of the order of fractions of a microvolt per centimeter.

The significance of this demonstrated sensitivity for animals is apparent. In speculations on the mechanism involved in the reported responses of insects to atmospheric gradients, surface charge has been importantly considered. In the light of the "Faraday-cage action" of every organism's body as it behaves as a volume conductor, it has been difficult to believe that the minute residual gradients within the organism, correlated with the larger atmospheric gradients, could result in any response of individual cells or organs located protectively inside the external boundaries of the organism. The studies with the worms, and earlier studies with the marine snails submerged in sea water (even a slightly better conductor), have proven there exists cellular sensitivity adequate to require a reconsideration of the mechanism of response in such terrestrial organisms as the insects, hamsters and even man.

Sensitivities of the order of those established by this study provide one means for an influence of weather-system changes on organisms. Such meteorological changes are not uncommonly accompanied by electrostatic fluctuations more than one hundred times as great as the experimental ones employed in this study. An innate ability of living things to interpret specific parameters of electrical change in their environment may prove to be a partial explanation of apparent forewarnings some organisms have appeared to receive relative to meteorological disturbances.

Ability to resolve small differences in strength of horizontal vectors of atmospheric electrostatics, and their direction as well, can contribute as a navigational aid. This would comprise an electrostatic "compass." Such a compass may be used along with other aids, such as response to magnetic field and visual responses, including use of celestial references.

The earth's atmosphere displays periodic variations in diverse electrical parameters. These relate importantly to movements to the earth with respect to sun and moon. Ability to resolve strength, direction, and frequency and amplitude of oscillations in electrostatic field, can theoretically provide an organism with a means of deriving valuable information as to the period lengths of the natural geophysical rhythms. Both local-time and universal-time components are present in these fluctuations. Responsiveness to electrostatic fields may possibly be one of the normally contributing factors to the timing system of the extraordinary clocks of animals and plants.

Such sensitivity of a protoplasmic system to an electric field as appears to be present renders it probable that protoplasm is far more sensitive to electromagnetic fields of radio-frequency than has generally been conceded, or even reported, up to the present. This possibility is further supported by the correspondingly great sensitivity to extremely weak magnetostatic fields reported elsewhere. It is conceivable that failure to disclose such perceptivity may commonly be a consequence of an inability, to date, to discover an invariable kind of response by the organism to such a stimulus.

The complexity of the response mechanism of the planarians to electrostatic fields as revealed by these studies, and the relationships of the response to both temporal and spatial orientation, certainly suggest the hypothesis that responsiveness to this factor plays still undisclosed and important roles in the lives of terrestrial creatures.

Summary

1. The planarian Dugesia is able, even while in water, to perceive a change of 2 volts/cm. in electrostatic gradient in the surrounding air.

2. There is reason to presume that in order to show this response the organism is responding to differences in ambient static gradient of the order of fractions of a microvolt per cm.

3. The strength and character of worm response to a right-angle potential change are related to the direction the worm is

oriented in the earth's geographic field, and to time of day.

4. A field-change in South-bound worms in the morning effects clockwise turning. A similar field-change for North-bound worms effects counterclockwise turning. In the afternoon the relationship of electrostatic response to geographic direction is essentially the mirror-image of that of the morning.

5. Dugesia is able to distinguish the direction of a gradient across its body.

6. A few of the possible significances of these findings are discussed briefly.

• Astronomy and Behavior

LUNAR PERIODICITY IN LIVING ORGANISMS

Fox, H. Munro; *Science Progress*, 17:273-282, 1922.

A supposed influence of the moon on plants, animals, and on the affairs of men is found mixed with the religious ideas of nearly all primitive peoples. It persists to-day in folklore and superstition. Most of us have turned a coin in our pockets for luck on first seeing the new crescent, wishing that with the growing moon the money and our general prosperity might too be increased. This in reality is the key to the origin of most moon beliefs. The moon is one of the most striking of natural objects seen by primitive man, and one whch varies most in aspect. By association of ideas the moon's increase and decrease is then supposed to influence the increase and decrease of any changing process.

This assumption has left its trace in the superstitions of many present-day peasants in Europe and elsewhere, who believe that hair, nails, corns, etc., cut under a waxing moon grow again more quickly than when cut under a waning moon, and that sickness disappears most rapidly when treated under a waning moon. The same ideas were current in ancient Rome, for Pliny says that Tiberius "for hair-cutting observed the increasing phases." Hence sheep are shorn under a waxing moon, and to-day in Greece the peasants arrange that lambing and calving shall take place in the growing phases. The same belief can be traced into nearly all departments of human activity. Thus in parts of Africa to-day wars, journeys, or business must be commenced with a waxing moon if they are to be successful, and the same is related by Caesar (De Bello Gallico) and Tacitus (Germania) of the ancient Germans. Marriages, too, must take place during the first half of the lunar month.

From this idea it is but a short step to associate the waxing and waning moon with the growth and decay of vegetation. The belief is found already in the Zend-Avesta and again in a number of Greek and Roman authors. Connected with the same notion is the conception of the moon as a source of moisture and thus an aid to plant growth. Plutarch says that "dew falls most at full moon," and in another place, when speaking of Osiris as a moon-god, refers to "his humid and generative light favourable to the growth of plants." The same function was ascribed to the Greek Artemis.

The supposed role of the moon as a source of moisture seems to derive from the fact that there is most radiation from the ground and hence the greatest deposit of dew on cloudless nights. The moon has no effect on the cloudiness or otherwise of the sky--meteorologists are decided on this point, in spite of the saying of sailors that "the moon eats up the clouds"--but it is on cloudless nights only that the moon is visible, and it is just on these nights that dew is formed. Thus the dew is credited to the moon. So gardeners fear the April moon, for at this season excessive radiation injures young shoots by reducing their temperature unduly. This occurs on cloudless nights, which are most noticeable when there is a moon to light the sky.

There is an ancient belief and one that is widespread to-day that sowing and planting must be done under a waxing, reaping and cutting under a waning moon. Presumably the crops are supposed to be increased like many other things by the growing moon. The idea lying behind harvesting or felling timber beneath a waning moon is more difficult to trace, but may be connected with the drying influence which is ascribed to the declining phases in the same way as dew attributed to the waxing and full moon. At all events the belief is most widespread that wood for building purposes is not durable unless cut after the full moon, and indeed until the Revolution French law ordained that trees should be felled only under a waning moon. In Trinidad, where the belief is strongly held, experiments were recently made which demonstrated the absence of any basis of truth to the superstition.

Passing now to animals, it is principally on marine invertebrates that the moon is said to exert an influence. There are a number of references to this in the classical authors. Aristotle says that the ovaries of sea-urchins acquire a greater size than usual at the time of full moon. Cicero notes that "oysters and all shell-fish increase and decrease with the moon," and Pliny states that "careful observers attribute to lunar power the increase and decrease of the bodies of oysters and all shell-fish." The same belief is common to-day in many of the fish-markets around the Mediterranean and in other parts of the world. The amount of edible matter in sea-urchins, molluscs, and crabs is stated to vary with the phases of the moon: the animals are said to be "full" when the moon is full, and empty at new moon. Some gourmets maintain too that the taste varies with the moon.

With the object of testing the truth of the popular statement I made systematic examinations of the testes and ovaries of a sea-

urchin (Diadema setosum) at Suez during the summers of 1920 and 1921. For this species of Echinoid the reports of the fishermen turned out to be founded on fact to a surprising degree. There is a periodic reproductive cycle correlated with the lunar period, the genital products being spawned into the sea round about each full moon during the breeding season. After spawning the testes and ovaries become reduced in bulk, the declining phases of the moon being occupied by recuperation and the commencement of the preparation of fresh genital products. After the new moon these increase in ripeness and in bulk, and with this development the visible size of the genital glands grows once more.

Microscopic examination of the gonads of a number of individuals between the first quarter and full moon shows the majority to be swollen and packed with fully formed spermatozoa or eggs. A small proportion are "spent," that is they show evidence of having lately extruded their genital products. A week later the relative proportions are reversed; some individuals have gonads still full of spermatozoa or eggs, but most are "spent." Between the third quarter and the new moon all gonads are shrunken in size and contain nothing but developing spermatocytes or oocytes. From now onwards until the first quarter of the next moon these cells show progressive stages in development into spermatozoa and eggs, destined to be spawned round about the time of full moon. This lunar cycle is repeated throughout the breeding season.

In seeking a causal connection between the reproductive rhythm and the lunar month an influence of the tides first suggests itself. But whereas Diadema has a single reproductive cycle in each lunation there are two spring and two neap tidal periods, i.e. a double cycle. However, during the summer months at Suez the new moon spring tides have a greater range than the full moon springs, so that the maximum tidal range is attained once only during each lunar month. The higher and lower water at the new moon springs might conceivably react on the Echinoids by the different hydrostatic pressure (affecting, e.g., the tension of dissolved gases) or by causing the animals to be at a greater or lesser distance than usual from the source of oxygen or of light. But the average excess tidal range at new moon springs over that at full moon springs during the period studied was only 58 cm. This small difference could scarcely affect the urchins, for they are not sessile animals, but move actively and their vertical range of migration during the course of an hour is often far in excess of this figure.

The possibility of tidal influence could be tested by keeping urchins in a floating cage. If the lunar reproductive cycle were thereby abolished the tidal connection would be demonstrated; but a contrary result from the experiment would not dispose of a possible influence of the tides, for an established rhythm in a physiological process is often persistent after the original cause has been removed. One has only to recall the case of Convoluta, a marine turbellarian worm which lays its eggs only at neap tides. It has been found that animals hatched and reared in the labora-

tory keep the same egg-laying habit (Gamble and Keeble, Quart. Journ. Micr. Soc., 1906). In any case the experiment of eliminating the tides was unfortunately impracticable with Diadema owing to its size. Fully grown specimens measure over one foot from tip to tip of the spines, and it was impossible to obtain large enough floating boxes to contain a hundred or more individuals. But I intend to seek further evidence regarding the possible effect of tides by studying Echinoids in localities with greater and lesser tidal ranges than at Suez. I am convinced, however, that if a similar lunar reproductive cycle exists in the sea-urchins at Naples or Plymouth it is very little pronounced. For I have made use of the Echinoids at these places to obtain spermatozoa and ova for other experimental purposes for months continuously without ever noticing a rhythmic variation in condition or quantity of genital products. At Suez the period in each lunar month when spermatozoa and eggs are unobtainable would necessarily force itself upon the notice of the investigator.

The possiblity of a direct effect of the light of the moon on the Echinoids could be tested by keeping specimens in the dark. Although the large size of Diadema again precluded this experiment at Suez, I intend to carry it out in another place with a smaller Echinoid. If the light has an effect it must necessarily be more constant in the cloudless summer nights of Egypt than in Europe.

Another possiblity was that the light of the moon might act by causing the urchins to feed either more or less on moonlit nights. A systematic examination and comparison of the gut-contents of specimens taken at dawn (1) after a moonlit night, and (2) after a night without moon, showed, however, no difference either in quantity of nature of food.

The sea-urchins fished at Alexandria are locally believed to change with the moon in the same manner as the Suez ones. A parallel investigation of the Alexandrian urchins (Strongylocentrotus lividus) has shown, however, that in this case there is no lunar reproductive cycle. The Alexandrian species is identical with one of those fished and eaten at Naples, which, as mentioned above, has apparently no lunar cycle. How, then, are we to account for the ancient Greek and Roman belief in such a periodicity, and for the present-day statements in the Naples and Alexandria fish markets? I can only suppose that what is a fact at Suez has come uncritically to be believed all over Egypt, and that in very early times the Egyptian belief was brought to Greece, whence it spread, and has remained all around the Mediterranean coasts.

In addition to sea-urchins, it is believed in the Egyptian fish-markets that the flesh of mussels and crabs varies in quantity with the moon. In mussels, just as in sea-urchins, the genital glands form the bulk of the edible material, in crabs it is the muscular tissue. It is these tissues, then, that are supposed to vary in bulk with the changing moon, but I have found that this is not true. Presumably what really occurs in the sea-urchins is imagined by the people to take place in all "shell-fish."

The eggs of the sea-urchin when spawned into the sea are

normally fertilised immediately by spermatozoa shed by a neighbouring male. In the course of a few hours the segmenting eggs develop into free-swimming ciliated larvae which rise to the surface of the sea, where they become members of the great floating population known as the plankton. Now it is obvious that the periodic spawning of Diadema must be reflected in the plankton of the Gulf of Suez. The larvae of this sea-urchin swimming at the surface of the sea must vary in numbers and in stage of development with the phases of the moon. In the same way there must be a lunar periodicity in the frequency of the pelagic larvae of all other animals which have a lunar reproductive cycle. By studying plankton from different parts of the world I hope to discover which these animals are.

Up to the present the best known case of lunar reproductive periodicity scientifically investigated has been that of the Palolo Worm (Friedlander, Biol. Centralbl., 1898-1901). This Polychaete (Eunice viridis) lives in the coral reefs in Samoa and other Pacific Islands. At the last quarter of the moon in October and November the posterior parts of the worms laden with genital products become detached from the anterior portions. While the latter remain among the coral the genital segments swim up to the surface of the sea, where they shed their spermatozoa and eggs. This swarming takes place at low tide on several successive days, the swarms being composed of enormous numbers of individuals which die after having spawned. The swarming worms are fished by the natives who relish them as a delicacy, and each year the fishermen judge the right time to prepare nets and boats for the catch by the state of the moon. It is not known what gives the stimulus to the worms to spawn, whether moonlight, tide, or some other physical change depending on the moon.

In Japan there is another Palolo (Ceratocephale Osawai) which swarms both at full and new moons (Izuka, Journ. Coll. Sci. Tokyo, 36, 1903). Here the bilunar periodicity is presumably due to the spring tides, but we are ignorant of the manner in which the greater tidal range may react on the animals. In the Atlantic yet another kind of Palolo (Staurocephalus gregaricus) swarms usually at the last quarter, but occasionally also at the first quarter of the moon (Mayer, Carnegie Inst. Pub., 102, 1909). Mayer was the first to abandon the observational method which had proved so unfruitful in seeking for a causal explanation of lunar periodicity in reproduction. He carried out direct experiments. Eleven mature worms were placed in a floating (i.e. tideless) box thirty days before the swarming time was due. Only four of these worms swarmed, whereas in nature all mature individuals swarm. Thus the tide appears not to be the sole cause of swarming, unless, of course, those worms which swarmed in the floating box did so owing to a tidal rhythm previously acquired, as in the case of Convoluta mentioned above. Mayer also put twenty-two worms in floating boxes protected from the moonlight. None of these animals swarmed. The light seems therefore to be a necessary contributory cause for swarming. These valuable experiments should be repeated with a greater number of individuals. It is obvious, however, that whether

light, tide, or some other factor gives the worms the stimulus to leave their holes in the rocks and swim up to the surface, some influence which varies with the lunar period must affect the worms at a date long anterior to the swarming time. For in order that all the worms should be sexually mature and ready to swarm together at, say, the last quarter of a certain moon, the development of the genital organs must have been initiated in all of them together at some definite previous lunar phase.

There are certain other marine worms which show a lunar reproductive periodicity. Potts found that sexually mature individuals of Odontosyllis in British Columbia swarm once a year from the sea-bottom to the surface in order to spawn. This occurs in August, during the first or third quarter of the moon (Proc. Camb. Phil. Soc., 17, 1913). At Bermuda this worm also exhibits a lunar periodicity in swarming. Another Polychaete, Nereis, at Woods Hole, Massachusetts, swarms at sunset throughout the summer months, but during the waning phases of each moon only (Lillie and Just, Biol. Bull., 24, 1913; and Just, Biol. Bull., 27, 1914). At Naples Nereis swarms round about both the first and third quarters of the moon from October to May (Hempelmann, Zoologica, 25, 1911). The latter bilunar, i.e. apparently tidal, periodicity is remarkable since the tidal range at Naples is much less than at Woods Hole, yet swarming at the last-mentioned place is not correlated with the tides. The behaviour of Nereis is the more anomalous since twenty years' observation in the Thames estuary by Sorby (Journ. Linn. Soc., 29, 1906) failed to detect any lunar rhythm in the swarming of this worm. Amphitrite, also a Polychaete, lays its eggs at new and full moon spring tides at Woods Hole (Scott, Biol. Bull., 17, 1909). Scott supposes that the higher temperature of the sand flats on which the animals live, due to the abnormally low water at spring tides, together with a more abundant food supply at these times, are the immediate causes of the tidal reproductive cycle.

Among Molluscs the only case I know of was communicated to me in a letter by Dr. W.J. Crozier, who states that at Woods Hole a chiton spawns at the time of full moon.

To my knowledge the only other case of reproductive rhythm in animals correlated with the lunar period is in the human race. Arrhenius (Skand. Arch. f. Physiol., 8, 1898) showed by a statistical investigation of 25,000 cases that there exists a low correlation between the frequency of births and the tropical lunar period of 27·32 days. Rather more births take place in a certain part of the tropical lunar month than in the remainder. Since this nativity rhythm might depend upon a periodicity in menstruation, Arrhenius then set to work to find out whether the latter exists. His material was furnished by 12,000 cases from Stockholm maternity hospitals, where the date of the last menstruation before the onset of pregnancy is recorded for each patient admitted. The same kind of periodicity was found as in the case of the births, but to a more pronounced degree. Taking the day on which the moon passes through the ecliptic from North to South as the first day of the of the tropical lunar month, the curve giving the frequencies of menstruation for each

day in the month gradually descends to a minimum on the eighteenth day, when there are 6·5 per cent. fewer cases than the average for all the days of the month, and then gradually rises again to reach a maximum at another part of the month. In each individual the onset of menstruation depends, of course, upon a number of physiological and psychical causes, which occur without any rhythm. In addition there is a rhythmic causative factor having the period of the revolution of the moon. This factor is weak and will only make itself felt when acting in conjunction with the other causes. Menstruation may occur, then, on any day according to the resultant effect of all the causative forces acting upon the person, but in more instances than not it will fall in that part of the tropical lunar period in which the periodic factor is acting.

Arrhenius suggests that the factor in question is the amount of electricity in the atmosphere which he has shown to vary rhythmically with a period of 27·32 days. But in addition to this period the atmospheric electricity has a shorter (non-lunar) one of 25·93 days. If now the same shorter period could be demonstrated also in the menstruation figures, the possibility of a causal connection between the two phenomena would be heightened. Further investigation showed that the shorter period was present in the case of menstruation. In addition, the effect of the two periodicities should be to cause the average length of the menstrual period to be 26·605 days. The average of 1834 individuals in Denmark was found to be 26·68 days.

Arrhenius demonstrated the same double periodicity for epilepsy, but found it to be absent for bronchitis and mortality.

Among plants the sole authentic cases of lunar shythm in reproduction seem to be among the Algae. Dictyota dichotoma is a common marine alga found in various parts of the world. At Beaufort, North Carolina, this plant produces one crop of sexual cells in each lunar month (Hoyt, Bot. Gaz., 43, 1907). The same species at Bangor, Plymouth (Williams, Ann. Bot., 19, 1905), and Naples (Lewis, Bot. Gaz., 50, 1910) has a tidal reproductive rhythm, i.e. two cycles per lunation. This difference in behaviour in different localities has not been explained. It is the more problematical since the tidal range at Beaufort is small, resembling the Mediterranean conditions and differing widely from the considerable tidal range on the English coasts. The figures for the average tidal ranges are as follows: Bangor 17·9 feet, Beaufort, 2·8 feet, Naples 1·0 feet. Sargassum too, shows a bilunar reproductive cycle (Tahara, Bot. Mag. Tokyo, 23, 1909).

Popular beliefs in a favourable influence of the moon on plant growth are both world-wide and ancient, although most of them are probably on a par with the superstition that a waxing moon increases, a waning moon decreases, any process such, for instance, as the acquisition of wealth. Seneca, Galen, and other classical authors speak of the moon as hastening the maturation of fruits. Athenio is more precise, writing that "cucumbers in gardens grow at full moon, showing a visible development, and so do sea-urchins." This quotation is of special interest in relation

to a common belief of the modern Egyptians that melons, marrows, and other fruits of the Natural Order Cucurbitaceae grow most rapidly on moonlit nights. An investigation of the rate of growth of these fruits, extended over three months, has shown me, however, that there is no basis at all for this belief. As the lunar influence is supposed to show itself especially in these cucurbitaceous fruits, it may safely be assumed to be absent in all fruits.

Nevertheless, since the popular belief in the existence of a lunar cycle in sea-urchins has turned out to be so unexpectedly true, one is bound to ask whether any other of the superstitions about plants too are based on fact. It is conceivable, for instance, that moonlight may have a photosynthetic effect. Kofoid (Bull. Illinois State Lab. Nat. Hist., 8, 1908) found a lunar periodicity in the frequencies of plankton organisms in the Illinois River. Allen (Univ. Cal. Pubs. Zool., 22, 1920) discovered the same thing in the San Joaquin River, California. The maximum frequency of Algae occurred at full moon, that of Crustacea a little later. While the crustacean presumably follows the algal maximum because the animals feed on the plants, Kofoid attributes the algal maximum occurring about full moon to a photosynthetic effect of moonlight. He supports this hypothesis by reference to experiments of Knaute (Biol. Centralbl., 18, 1898), who states that he found the oxygen content of water containing Euglena to be higher in moonlight than in darkness, which means that the moonlight causes photosynthesis. Knaute says that the photosynthetic effect of moonlight is to that of sunlight as 2:9. This is a surprisingly high ratio, since the light of the sun is about 600,000 times that of the full moon. In view of the great theoretical importance of lunar photosynthesis, if it really exists, I am at present repeating Knaute's work. Preliminary experiments, using, not an Alga, but the aquatic flowering plant Elodea, have shown that here the lower limit of illumination necessary for photosynthesis is of a totally different order of intensity from even full moonlight. The method used was based on the change in hydrogen ion concentration in the water in which the plant was placed, due to the change in its carbon dioxide content.

There are, however, two demonstrated effects of moonlight upon plants. Firstly, Musset (C.R. Acad. Sci., 1883 and 1890) showed that a number of flowering plants are positively phototropic to moonlight. The stems bend towards the moon and during the course of the night move round to follow his path. Secondly, Loftfield (Carnegie Inst. Publ., 314, 1921) states that at night stomata open as a result of illumination by moonlight. The consequent periodic opportunity for increased transpiration and respiration might conceivably cause other rhythmic changes in the plant, with a lunar period.

In conclusion, two cases of lunar effects on the migration of fish must be mentioned. The first concerns herring, and is of peculiar interest since the rather intricate causal connection has been elucidated. The work was done by Pettersson (Quart. Journ. Roy. Meteorol. Soc., 38, 1912). Herring are not found in the Baltic because the water is not sufficiently salt. They migrate into the Kattegat however, with an undercurrent of

salter water which exists beneath the less dense surface layers. This undercurrent is oscillatory and the movement shows itself by changes in the level of the deep salter water. The crests of the submarine waves occur at intervals of about thirteen and a half days, that is, the undulatory movement has the tropical lunar period of twenty-seven days and depends on the moon's declination. That the herring move with the salt water is shown by the fact that the dates of the greatest herring catches are those on which the crests of the waves of salter water occur. But this is not all. During the last century and a half the years of maximum and minimum herring fisheries are found to coincide with the years of maximum and minimum declination of the moon, which are 9·3 years apart. It may be presumed that the fishery has this period of 18·6 years because of a variation of the salter undercurrent with the same period. The herring fishery has also a longer period which can be traced back 1,000 years. The maxima are 111 years apart. This is the greater sunspot period, but since $6 \times 18{\cdot}6 = 111{\cdot}6$, the moon's declination may again be the real cause.

The second case of a lunar effect on the migration of fish is the following. Dr. Johann Schmidt informs me that Danish eel fishermen, who catch silver eels wth nets in the shallow water as they migrate out of the Baltic in the autumn, expect to obtain better catches in the interlunar periods than when there is moonlight. In Egypt, too, the eel fishermen look forward to the best catches when there is no moon. Mr. G.W. Paget, Egyptian Government Director of Fisheries, suggests in explanation of this a direct effect of the light of the moon in stopping the migration of the eels, basing his opinion on successful experiments carried out in Denmark on artificially stopping eel migrations with acetylene lamps. The Italians, too, light fires when they want to interrupt a migration in order to clear their nets.

PERIODIC SPAWNING OF 'PALOLO' WORMS IN PACIFIC WATERS

Burrows, William; *Nature,* 155:47-48, 1945.

The Mbalolo, Eunice viridia, is a marine annelid found in several places in islands of the Western Pacific. In Fiji I know of two and have heard of a third place in which the periodic 'rising' takes place. One such spot is between the shore and the fringing reef at the village of Tokou, in the island of Ovalau.

Another is in the Yasawa islands off the north-west coast of Viti Levu, and I have heard that there are one or more spots in the Lau group, which forms the eastern limits of the Fiji Islands. It also occurs in the Samoan group, where it is known as Palolo; and probably elsewhere.

During my period as commissioner, stationed at Levuka, I have seen the 'rising' at Tokou on several occasions. Nearly every year there are two risings, the first known as Mbalolo

lailai (small Mbalolo) and the second as Mbalolo levu (large Mbalolo); 'large' and 'small' do not refer to the size of the worm, but to the quantity of the worms. Occasionally there is no Mbalolo lailai.

In a normal year the Mbalolo lailai appears about the end of October and is followed by the second, and main, rising about two or three weeks later. The main rising always occurs at dawn, and, literally, the worm comes up with the sun. It is, also, always at the time of high water.

The worm, when it comes to the surface, is headless, and it is known that the head remains alive in the reef. The parts shed are from ten to fifteen inches long when they reach the top of the water and continue to wriggle.

There was an old Fijian living in the village of Tokou who was a foreteller of the day of rising, and I used to apply to him for information as to when I should take part in the event. To do so required some preparations: a boat and crew with dip-nets ready at the village; somewhere to sleep until about 04.00, when one had to prepare to move out; lanterns, torches and so forth. The information obtained from the prophet was not always reliable. On the other hand, my experience was that when he said he was certain, it was so.

The uncertainty occurred when the usual date--connected with the phases of the moon, no doubt--had gradually got further and further from the normal. Whatever the actual explanation of the date of spawning, the fact remains that the Mbalolo makes its annual rising at an approximate date by the calendar year but at an actual date by the moon and tide. When the difference becomes more than a few weeks, the date combines solar influence and the appropriate lunar and tide conditions. Records have been kept in Levuka, Fiji, for some seventy years, but I have not yet been able to have access to them.

To attend a 'rising' is an unforgettable event. With the necessary preparations made, I have boarded my boat at 04.30 after a sleep in the village and paddled out to a position about half-way out to the main reef, which skirts the shore at this spot at a distance of about a mile. Then torches are shone into the water vertically from the boat's side to see if there are any indications. If it is the right day, small stray bits of the worm make their appearance, and nets are got ready. Then, when the first light of dawn appears, great funnels of worms burst to the surface and spread out until the whole area is a wriggling mass of them, brown and green in colour.

When the tropical sun rises perpendicularly from the sea the catch is in full swing, and hundreds of boats, canoes and punts are filling up kerosene tins and jars by the simple process of dipping them out with nets.

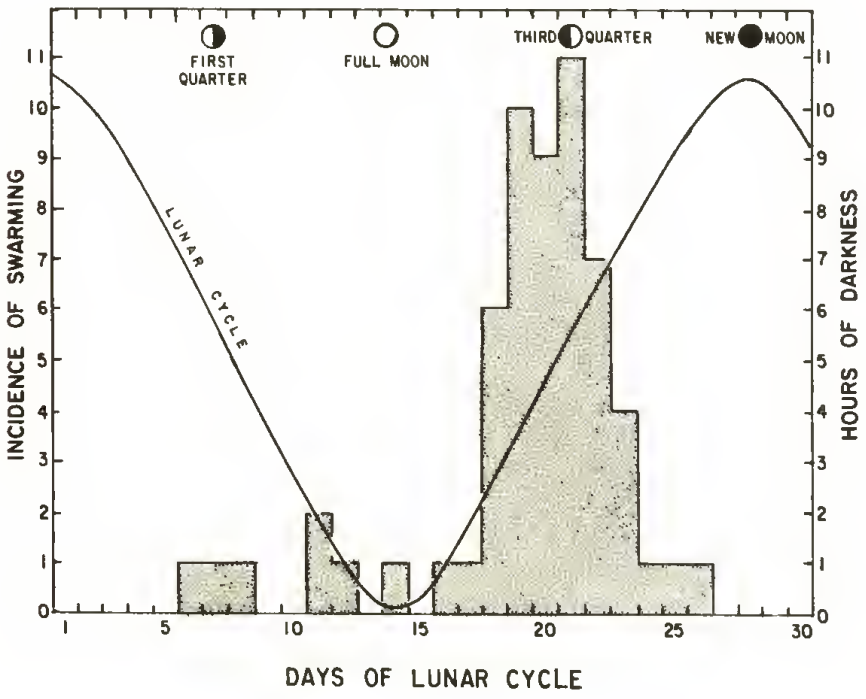
The worms also provide an annual feast for the fish; for all round and between the boats big fish and sharks cruise quietly along, gulping them in, and take no notice whatever of the boats or their occupants.

As the sun makes itself felt, a change begins to occur in the length of the worms. They begin to break up into shorter and

shorter bits, until some three hours after sunrise the entire surface of the sea shows nothing more than patches of scum.

Mbalolo is rightly prized as very good eating and, if one can forget what it looks like before being cooked, is delicious. Fijians--and I have known Europeans to do likewise--eat some raw, when perhaps it may resemble oyster; I could never bring myself to try. In this raw state it is said to have a stimulating effect on fecundity. The Fijians also say that a dish of it eaten in any form will protect a person from all sickness until the Christmas Day following.

A curious fact is that all fish caught in the neighbourhood of the rising are poisonous to human beings for about ten days or a fortnight after the event.



Swarming incidence of the Atlantic palolo compared with the lunar cycle. (Adapted from Science News Letter, *39:219, 1941)*

MAGNETIC RESPONSE OF AN ORGANISM AND ITS LUNAR RELATIONSHIPS

Brown, F. A., et al; *Biological Bulletin,* 118:382–392, 1960.

It has been demonstrated that mud-snails, Nassarius obsoleta, initially directed magnetic southward in a symmetrical field constant for all factors normally considered able to influence their orientation, exhibit a daily rhythm in the direction of their mean paths. The amount of dispersion of paths of a population of snails about their mean path also displays a daily rhythm. Both of these characteristics of spatial orientation were shown to be quantitatively alterable, in a manner highly significant statistically, by experimentally changing the strength of the ambient magnetic field. Although much of the observed variation, both in the orientation of control animals in the earth's field and in the modified orientation in response to experimentally increased strengths of the magnetic field, was accounted for in terms of the daily rhythm of response, much variation still remained unaccounted for. The following study was made to determine whether lunar periodisms in responsiveness to the magnetic field were also present.

. . . . .

Discussion

There appears little reason to doubt that spatial orientation of snails, expressed as an amount of turning, or a klinokinesis, possesses a lunar rhythmicality. This is displayed first as lunar-daily, or approximately 24.8-hour, periodicities in both mean orientational paths with signs taken into account and total turning, both clockwise and counterclockwise. The latter is indicated here in terms of standard deviation of pathways. It is interesting that the form of the lunar-day cycle, in the range of hours corresponding roughly with those available for the solar-day study, namely 5 AM through 9 PM, is strikingly similar to that of the solar-day one in both gross features and phase relationships. At both the times of sun-rise and moon-rise the left turning is minimal and it generally increases towards the early solar and lunar "evenings,"--i.e., times of setting of sun or moon. Also, in both the solar and lunar days standard deviation of pathways is minimal about the time of sun-rise and moon-rise, and increases systematically while the sun and moon are above the horizon, decreasing again as these heavenly bodies set. Furthermore, the amplitudes of the solar and lunar cycles are of the same general magnitude. The presence of these two similarly conspicuous periodisms would be expected to give rise to a synodic monthly cycle, and such a cycle appears to be present. The occurrence of a semi-monthly cycle of influence of the experimental magnetic fields suggests that either the lunar-day or solar-day cycle of mean path is bimodal. Though the former is suggestive of bimodality, the complete form of the solar-daily cycle has not been

determined. A semi-monthly rhythm would conform to that of the recurrence of the tides at a particular hour of the day in the natural habitat.

It is evident that since the snails were always subjected, whether in the field or laboratory, to daily illumination changes, a solar-daily cycle could be imparted to the snails by light cycles. The lunar-day period, on the other hand, could be gained by the snails in the field from the tides, but during the periods of as long as 8 to 10 days when the snails were retained in the laboratory any exogenous lunar period in terms of obvious environmental factors was not available. In view of the persistence of the lunar-day rhythm with conspicuousness equal to that of the solar-daily period, it would appear probable that both the lunar-day cycle and the solar-day cycles are being derived continuously in response to some unidentified subtle environmental factors in a manner comparable to that demonstrated for the solar and lunar periodisms in Uca pugnax and of the metabolism of Nassarius itself.

The similarities between barometric pressure changes and certain parameters of spatial orientation of snails suggest strongly that orientation in the snails is in some manner dependent upon subtle environmental factors other than magnetic fields and that these other factors may also induce considerable alteration in snail behavior. This is indicated since even the experimentally augmented magnetic fields may be overriden by the factor correlated with the barometric pressure. One possible factor which has been suggested is electrostatic field. The correlation with barometric pressure recalls the correlation observed for metabolic fluctuations in the snails in constant conditions, including pressure, during the summer of 1958, except for its being a mirror-image. Since there is a reason to believe that the spatial orientation described in this study is at least in some measure a klinokinetic response to the natural and experimental magnetic fields, it is possible that fluctuations in other subtle geophysical factors can influence the sensitivity of the magnetic responder-system. However, the possibility can not be excluded that a responder system for another factor is being altered by the magnetic field changes. The apparent effectiveness of certain barometric pressure correlated in modifying the orientational responses, and the well-established role of these correlated in influencing the rate of cellular oxidations, suggest that the sensitivity to the magnetic field is in some manner related to rate of oxidative metabolic changes.

These striking relationships between strength and character of magnetic orientation and phases of the lunar-day and synodic monthly periods contribute, together with the earlier demonstrated solar-day relationships, a substantial degree of predictability to the responses of the snail to magnetic forces. Collectively they increase still more not only the probability that this magnetic response is real, but that it plays a significant role in both the lunar-dominated tidal and the solar-day periodic behavior of these animals.

It is interesting, further, to note that in the lunar-day fluctuation in orientation, with sign taken into account, those times of lunar day when the mean path of the snails is essentially straight or even slightly to the right, the experimentally augmented magnetic field effects turning to the right, and as the snails normally orient during the lunar day more and more to the left as one approaches lunar nadir, the effect of the increased magnetic field is to produce progressively stronger left-turning. This relationship is quite comparable to that found for the solar day and provides further support for the view that response to the earth's natural field normally occurs.

There has been noted a remarkable similarity in both the gross form and the detailed trimodal character of progressively decreasing maxima of the lunar-day. N-S magnetic field responses of snails and the mirror-image of a highly significant mean lunar-day cycle of spontaneous motor activity found in white mice over the 5-month, partially overlapping period, March through July, 1959 similarly treated as a three-hour moving mean. The maxima and minima were essentially synchronous when the two cycles, studied in two places, Massachusetts and Illinois, were adjusted to simultaneity (universal time). The coefficient of correlation for the two trimodal cycles was -0.85. Similarly high simultaneous correlations have been reported earlier between fluctuations of the nucleonic component of cosmic radiation in Illinois and fiddler crab and sea-weed metabolism in constant conditions in Massachusetts. This similarity between the cycles of mice activity and response of snails to the magnetic field supports the hypothesis that in this magnetic response we are dealing with a widely, probably universally, occurring biological phenomenon.

The proof of a remarkable sensitivity of an organism to one subtle environmental factor (magnetic field) operating at an intensity only slightly above the earth's natural one, and the evidence for its modification in these constant conditions by fluctuations in a second natural factor, poses even the very fundamental problem of the dispensability, or indispensability, of the earth's milieu of subtle factors and its natural fluctuations, as a normal ecological consideration in organismic survival and species propagation.

Summary

1. The direction, and mean amount, of turning in snails initially directed southward into a constant symmetrical, illuminated field displays a lunar-day rhythm with minimum turning about the time of moon-rise, and maximum turning at lunar nadir. There is also a lunar-day cycle of standard deviation of snail pathways, with a minimum about moon-rise and a maximum near moon-set.

2. The response of snails to an experimentally augmented magnetic field also exhibits a lunar-day rhythm with maximum turning to the left at lunar nadir.

3. The specific character of the lunar-day rhythm of the response to the experimental magnetic fields gives further support for the view that magnetic field is normally involved in snail

orientation.

4. The mean daily response of snails to experimental magnetic fields, expressed as differences from the response of controls in the earth's natural field, displays a semi-monthly rhythm. Maximum right-turning in response to a magnetic increase of 10-fold over that of the earth occurs one to two days before new and full moon, and maximum left turning just before the times of the first and third quarters of the moon.

5. There is a synodic monthly fluctuation in mean daily standard deviations of snail paths with maximum deviation about two days before new moon and minimum deviation about the time of full moon.

6. Some suggestive correlations are demonstrated between barometric pressure and the spatial orientation of snails in an environment constant with respect to all generally accepted orienting factors.

7. It is pointed out that similarities in influence of some unidentified barometric pressure correlates on (a) magnetic orientation of snails, (b) general cellular oxidations, and (c) spontaneous activity cycles present reasons for postulating that the latter two phenomena are in some manner related to the magnetic field response and suggest that response to magnetic field is a widely distributed biological phenomenon.

8. Evidence is presented that suggests there is a biological influence of a universal-time-related, rhythmic, environmental factor.

ANOMALOUS DISTRIBUTION IN TIME AND SPACE

• Worms Far Underground

WORMS 300 FEET UNDERGROUND

Anonymous; *Scientific American,* 45:8, 1881.

The Gold Hill (Nevada) News reports the discovery of a queer species of worms in the face of the Lord Lorne mine, near Lower

Gold Hill. The worms occur in a solid stratum of stiff clay, 700 feet from the mouth of the tunnel, and 300 feet below the surface of the earth, amid the vein matter of that portion of the Comstock. Superintendent McDougal found quite a number of them by soaking and washing the clay, and they are no defunct relics of antediluvian times, but are all alive and kicking, incredible as it may appear. These queer little subterranean worms are about three-quarters of an inch long by about an eighth of an inch in diameter, short and thick, resembling some species of grub. Each is incased in a very neat little shell of silicious material, corrugated and firm, of a bluish cast, like silver ore, with small round spots, having a metallic luster. At his forward end appears a vicious-looking little head, and six legs or feelers capable of being easily folded when he draws back into his shell. On top of his head is a small helmet or cover, of the same material as the shell, so that when he hauls in for a snooze or self-protection his top-piece or helmet just closes the hole nicely. Why this hard shell covering or protective armor, or how it is that these very peculiar worms are found alive at such a depth in virgin ground, is not easy of explanation. Their presence can be accounted for on the score of some deep crack or disturbance of the earth at some time, yet what they are doing there and what supports them is a mystery, for the clay is no way rich, though it is wormy. They certainly are a great natural curiosity.

• Biological Explosions in the Fossil Record

EARLY CAMBRIAN MARINE FAUNA

Axelrod, Daniel I.; *Science,* 128:7-9, 1958.

One of the major unsolved problems of geology and evolution is the occurrence of diversified, multicellular marine invertebrates in Lower Cambrian rocks on all the continents and their absence in rocks of greater age. These Early Cambrian fossils included porifera, coelenterates, brachiopods, mollusca, echinoids, and arthropods. In the Arthropoda are included the well-known trilobites, which were complexly organized, with well-differentiated head and tail, numerous thoraic parts, jointed legs, and--like the later crustaceans--a complex respiratory system. From a phylogenetic standpoint the Early Cambrian faunal assemblage is generally interpreted to represent rather simple ancestral types in their respective phyla, which rapidly diversified into numerous types (species, genera, families, orders) during and

following the Early Cambrian. Their high degree of organization clearly indicates that a long period of evolution preceeded their appearance in the record. However, when we turn to examine the Precambrian rocks for the forerunners of these Early Cambrian fossils, they are nowhere to be found. Many thick (over 5000 feet) sections of sedimentary rock are now known to lie in unbroken succession below strata containing the earliest Cambrian fossils. These sediments apparently were suitable for the preservation of fossils because they often are identical with overlying rocks which are fossiliferous, yet no fossils are found in them. Clearly, a significant but unrecorded chapter in the history of life is missing from the rocks of Precambrian time. (p 7.)

• Latent Life or Cryptobiosis

CRYPTOBIOSIS

Crowe, John H., and Cooper, Alan F., Jr.; *Scientific American,* 225:30-36, December 1971.

This curious phenomenon, the ability of certain lower invertebrates to survive in a state of suspended animation, has been of much interest to biologists ever since. It was once called anabiosis, or "return to life," because for a long time experimenters believed that the dried animals had actually died and that when they were moistened they returned to life. This apparent resurrection was even cited during the 19th century as evidence in support of the theory of spontaneous generation. Man has been preoccupied with death ever since his first recognition of his own mortality. The prospect that any organism was able to return to life after death thus held an immeasurable fascination.

. . . . .

It is not known just how long a cryptobiotic organism can persist in a state of suspended animation. Some nematodes have been revived after being kept in the dried condition for 39 years. A museum specimen of moss that had been kept dry for 120 years yielded a number of rotifers and tardigrades. When the animals were moistened, a few of them revived, but all died within a few minutes. They had of course been in the dry state for an exceptionally long period of time. Even under natural conditions, however, cryptobiosis greatly extends the normal life-span. Ernst Marcus of the University of Sao Paulo has estimated that a tardigrade would have a life-span of less than a year if it never entered the cryptobiotic state, whereas one that alternated active

An active tardigrade, a cryptobiotic animal, magnified about 100 times. Water usually makes up about 85% of its weight.

and cryptobiotic periods might survive for as long as 60 years. (pp. 30-31)

. . . . .

A central issue in the study of cryptobiosis that also has considerable theoretical importance for biology in general is whether or not there is actually a total cessation of metabolism in cryptobiotes. It is normal to think of an organism in which metabolism has ceased as being dead. If we also think of death as an irreversible state, however, it seems to be necessary where cryptobiotes are concerned to define life and death in terms other than the presence of absence of metabolism. We believe that among cryptobiotes life should be defined in terms of the continuity of organized structure. As long as the structural integrity of a cryptobiote remains intact the organism is capable of resuming the active state that is generally deemed to be characteristic of life. If the structural integrity is destroyed, however, the organism is no longer cryptobiotic; it is dead. (p. 30)

UNRECOGNIZED SPECIES

• Globsters: Unidentified Carcasses Washed Ashore

THE FLORIDA SEA-MONSTER

Verrill, A. E.; *American Naturalist,* 31:304–307, 1897.

On the 5th of December, 1896, a portion of a very large marine animal was cast ashore on the beach twelve miles south of St. Augustine, Florida. When it first came ashore it was much mutilated at one end, and had evidently been dead some time, and was apparently, in an advanced state of decomposition. Contrary to expectation, it has resisted further decay, and still remains, after more than three months, nearly in the same state as at first. It was first brought to my notice by Dr. De Witt Webb, who has devoted a great amount of time and labor to its investigation and preservation. Through him I have received a dozen different photographic views of it, taken at different times, and showing it both in its original state and when it had been moved and partly turned over. Quite recently he has sent me several large masses of the thick and firm integument, of which the mass is mainly composed. By his efforts it has recently (with much labor) been moved several miles nearer to St. Augustine, to the terminus of a railroad, and protected from the drifting sand. It is likely to keep some months longer without much change, and to be visited by large numbers of people. The figures now given are copied from photographs made two days after it came ashore. At that time the sand had collected around it to the depth of about eighteen inches.

As shown by the figures, it has an elongated, pear-shaped form, broadly rounded at the larger, closed end, and considerably flattened toward the smaller and much mutilated end. At this end, as shown in both views, there are large, divergent ridges covered by the frayed-out fibrous tissues. These ridges are folds of the integument but were at first mistaken for the stumps of arms, like those of an Octopus, and were so described in letters received by me. Moreover, Mr. Wilson who visited it, when first found, claimed to have found a portion of an attached arm, 36 feet long, buried in the sand. This last statement, in the light of later investigations, must have been erroneous and was entirely misleading.[1] At that time, however, it seemed quite consistent with the form and appearance of the mass, which was des-

cribed by Dr. Webb as closely similar to the body of the common small octopus. The photographs show this resemblance very clearly; and the ridges at the mutilated end, then supposed to be the stumps of mutilated arms, seemed to confirm the view that the mass was the mutilated body of a huge octopus,[2] and as such it was described by me in the American Journal of Science and elsewhere.

As soon as specimens of the tissues were sent to me, even a hasty examination was sufficient to show that this view was not correct, for instead of being composed of hardened muscular fibers,[3] as had been supposed, the thick masses of tissue were found to consist almost wholly of a hard, elastic complex of connective tissue fibers of large size. The masses sent vary from four to ten inches in thickness. They are white, and so tough that it is hard to cut them, even with a razor, and yet they are somewhat flexible and elastic. The fibers are much interlaced in all directions, and are of all sizes up to the size of coarse twine and small cords. The larger fibers unite to form bundles extending from the inner surface radially. According to Dr. Webb, who opened the mass, these cords were attached in large numbers to a central saccular organ, which occupied a large part of the interior of the thicker part of the specimen. This might, perhaps, represent the spermaceti case. Naturally most of the interior parts had decomposed long before it was opened,[4] so that we lack details of the interior structure. Externally there is but little trace of cuticle. The surface is close-grained and somewhat rough, with occasional gray patches of what may be remnants of the outer skin, much altered by decay. The thick masses contain a slight amount of oil, and smell like rancid whale oil, but they sink quickly in water, owing to their great density. No muscular tissue was present in any of the masses sent, nor were there any spaces from which such tissues might have disappeared by decay.

It is evident that such a dense and thick covering of fibrous connective tissue could not have come from any mobile part of any animal, but must have served for passive resistance to great pressure or concussion.

The structure of this integument is more like that of the upper part of the head of a sperm whale than any other known to me, and as the obvious use is the same, it is most probable that the whole mass represents the upper part of the head of such a whale, detached from the skull and jaw. It is evident, however, from the figures, that the shape is decidedly unlike that of the head of an ordinary sperm whale,[5] for the latter is oblong, truncated and rather narrow in front, "like the prow of a vessel," with an angle at the upper front end, near which the single blow-hole is situated. No blow-hole has been discovered in the mass cast ashore. There is a depression, shown in the side-view, near the large end, that I at one time thought might be a blow-hole; but Dr. Webb states, that it is a "sulcus" or pit about two feet long and six inches deep, apparently not connected with the interior cavity and probably due to mutilation. The specimen was doubtless floated ashore by the gases of decomposition ac-

cumulated in the interior cavity, indicating the absence of any free external opening to it, from which the gases could escape.

Photographs made of the under side of the thicker part, when it was turned up by powerful tackle, show an irregular roughness on that side, extending well forward, but not to the end. This roughness may be due to abrasion, or it may show where the skull was attached. If the mass really came from the head of a sperm whale, it would seem that it must have projected farther forward beyond the upper jaw than does the nose of an ordinary sperm whale, and it would, apparently, have been much broader and blunter, or "bottle-nosed." It is possible, of course, that its form has changed considerably since death; but in view of it wonderful toughness and firmness, no great change of the larger end, supposed to be the anterior or nose-end is probable. All the pulling and hauling and turning of it partly over, by the aid of six horses and strong tackle, have not served to change its shape materially, or rather its elasticity serves to restore it to its former shape. Its toughness and elasticity remind one of the properties of thick vulcanized rubber.

It is possible to imagine a sperm whale with an abnormally enlarged nose, due to disease or extreme old age, which, if detached, might resemble this mass externally at least. It seems hardly probable that another allied whale, with a big nose, remains to be discovered. Notwithstanding these difficulties my present opinion, that it came from the head of a creature like a sperm whale in structure, is the only one that seems plausible from the facts now ascertained.

---

[1]The memorandum written by Mr. Wilson and forwarded to me by Dr. Webb is as follows: "One arm lying west of body, 23 feet long; one stump of arm about 4 feet long; three arms lying south of body and from appearance attached to same (although I did not dig quite to body, as it laid well down in the sand and I was very tired), longest one measured over 23 feet, the other arms were three to five feet shorter."

[2]This was also the opinion of a large number of naturalists who saw the photographs sent to me.

[3]A highly contractile muscular integument is an essential feature of all cephalopods.

Statements that the creature cannot be an Octopus, but is of cetacean nature, were published by me in several local daily papers within a day or two after the specimens were first examined by me, and shortly afterwards in the New York Herald and in Science.

[4]It should be stated that after visiting the specimen, two days after it came ashore, Dr. Webb did not again see it for several weeks, owing to very stormy weather and its distance from St. Augustine. Nor did anyone suppose, at that time, that its tissues could be preserved or utilized for study, owing to its apparently advanced decomposition. The outer skin rapidly decayed, but the fibrous mass seems very durable.

[5]The dimensions of the head of a large sperm whale, 84 feet long, are given as follows: Length, about 25 feet; depth, 8 to

9 feet; breadth, 5 to 6 feet. The blow-hole is like a slit, about a foot long, and has a sigmoid curve. It is on the left side, close to the tip of the nose. The spermaceti case occupies a large space within the right side of the head. It is supported by strong fibrous tendons.

AN OCTOPUS TRILOGY

Wood, F. G., and Gennaro, Joseph F., Jr.; *Natural History*, 80:15–24, March 1971.

The pieces corresponded closely to Webb's description.* There was very little fat or oil in them; they were almost as white as soap. I cut the samples I wanted with a pathologist's knife, which uses replaceable blades; the connective tissue was so tough that it dulled four blades simply to cut one or two finger-sized pieces. I wrapped these in cheesecloth and put them in a tightly covered jar together with some of the fluid. (The Smithsonian's jar was lost during a move; the samples I had taken are all that is left of Octopus giganteus Verrill.)

Unfortunately, there were no distinguishing structures in the pieces I dissected. No suckers, identifiable skin structures, or even muscular masses were discernible. All the pieces had the same homogenous, tough, white, fibrous texture. Only from one piece was I able to carve a small specimen from what appeared to be the periphery of the animal. I judged this by what looked like "natural" smoothness on one margin of the piece, perhaps indicating an original surface, although I had no way of knowing whether it was an outer or inner surface. Certainly, there was none of the typical covering layer one could expect from either a mollusk or mammal.

In my laboratory, I prepared the specimens for histological analysis together with "control" specimens of contemporary squid and octopus. When the slides were ready, I eagerly turned to the microscope to observe for myself the structure of this peculiar sea beast. Would the cells be the highly differentiated cells typical of a mammal, indicating that despite the lack of oil or blubbery smell the piece had really come from some type of whale? Would the architecture be similar to the squid sample or to that of the octopus that I had for comparison?

To my great dismay, no cellular material at all was discernible. Perhaps because the tissue mass had lain for so many days on the beach of St. Augustine, or perhaps because the formaldehyde or alcohol had had sufficient time to penetrate for adequate preservation, nothing of the original cellular architecture remained. I found, however, that my control samples, which had been prop-

---

*This excerpt describes Gennaro's analysis of the Smithsonian's sample from the "Florida Sea-Monster" investigated by Verrill in 1897.

erly prepared for histological analysis, also failed to show much cellular arrangement. But even more striking than the absence of cellular structure was the presence of distinctive patterns of connective tissue. Differences between contemporary octopus and squid tissue struck the eye immediately, and each was obviously different from the typical pattern of mammalian tissue.

It occurred to me that I still might learn something by observing and comparing the connective tissue patterns of the specimens under polarized light. The highly ordered fiber protein molecules oriented in the plane of the section doubly refracted the light and showed up brightly, while those that were perpendicular appeared black.

Now differences between the contemporary squid and octopus samples became very clear. In the octopus, broad bands of fibers passed across the plane of the tissue and were separated by equally broad bands arranged in a perpendicular direction. In the squid there were narrower but also relatively broad bundles arranged in the plane of the section, separated by thin partitions of perpendicular fibers.

It seemed I had found a means to identify the mystery sample after all. I could distinguish between octopus, and squid, and between them and mammals, which display a lacy network of connective tissue fibers.

After 75 years, the moment of truth was at hand. Viewing section after section of the St. Augustine samples, we decided at once, and beyond any doubt, that the sample was not whale blubber. Further, the connective tissue pattern was that of broad bands in the plane of the section with equally broad bands arranged perpendicularly, a structure similar to, if not identical with, that in my octopus sample.

The evidence appears unmistakable that the St. Augustine sea monster was in fact an octopus, but the implications are fantastic. Even though the sea presents us from time to time with strange and astonishing phenomena, the idea of a gigantic octopus, with arms 75 to 100 feet in length and about 18 inches in diameter at the base--a total spread of some 200 feet--is difficult to comprehend. Yet still stranger things have been seen and reported in the sea. Melville, himself a nautical man and an extremely careful observer of the natural history of the sea, speaks in Moby Dick of "a vast pulpy mass, furlongs in length and breadth of a glancing, cream color." Remember, a furlong is an eighth of a mile, or 660 feet. There's something to think about. (pp. 24 and 84)

Chapter 8

PLANTS

Chapter Contents

MORPHOLOGICAL PHENOMENA

• The Twisted Tree Enigma

MORE ABOUT TWISTED GRAIN IN TREES

Koehler, Arthur; *Science,* 73:477, 1931.

Science for February 13, 1931, contains an article by C.K. Wentworth noting the predominance of right-handed twist in spirally grained trees. Similar observations have been recorded by others. A Forest Service official on the Pike National Forest, Colorado, reports that out of 396 alpine fir trees, 85 per cent. had right-handed twist and 14 per cent. left-handed twist, leaving only 1 per cent. with straight grain. Similarly, 26 pines showed 14 individuals with right-handed twist and 4 with left-handed twist. The author also was struck with the predominance of right-handed twist when trying to find trees with left-handed twist suitable to photograph. On the other hand in an examination of 463 Douglas fir timbers at a mill in Tacoma, Washington, he was surprised to find 94 with left-handed twist and only 8 with right-handed twist (very slight twists not being considered). The other timbers were straight grained.

No satisfactory explanation of the cause of spiral grain has yet been made. There even remains the question as to whether it is due to heredity or environment. H.G. Champion, of the Forest Service of India, reports that seed from straight-grained trees give fewer spirally grained seedlings than seed from twisted trees. The resulting grain, however, was examined only in the young stems of seedlings, and it is not certain whether the same condition would be maintained as the trees grow older.

On the other hand, Paul van Oye reports from France that trees with tap roots have no torsion, those with lateral roots have slight torsion, and those with running roots have it to a marked degree. This corresponds to the general observation that in the higher altitudes where the soil is scant and tap roots can not develop, spiral grain is much more common than in the deeper soil at lower elevations.

The frequent deduction, as made by Wentworth, that twisted grain may be due to prevailing winds acting on asymmetrical crowns is not tenable since there is no evidence within the tree trunk that actual twisting of the trunk took place after the wood was formed. Such twisting would show distinct mechanical injury to the fibers which is not found to be the case. Furthermore, the

twist would be greatest near the center and least at the periphery of the trunk, assuming that it developed gradually over a period of years. Usually the reverse is the case.

Any satisfactory explanation of the cause of spiral grain must also explain why trees should be straight grained, since whatever factors are operative in keeping the fibers of most trees parallel with the axis of the trunk are modified in producing spiral grain. To say straight grain is the normal condition is not adequate, since in some hardwood species, especially in the tropics, the normal condition is for the fibers to be inclined right-handed for a number of years, then left-handed for about the same period, and then back to right-handed, and so on.

TWISTED TREES

Cahn, A. R.; *Science*, 73:561, 1931.

I have read with interest the notes in Science for February 13 and March 27 dealing with trees with twisted bark. My observations covering a large part of the province of Ontario, Canada, may be of interest in this connection. In this region I have often noted the twist of evergreens, especially of the cedar (Thuja occi-dentalis, white pine (Pinus strobus), Norway pine (Pinus resinosa) and Jack pine (Pinus divaricata). I have never noted it on a "hardwood" in the region. In the cedar the twist is very common, straight-grained trees being far less abundant than twisted-grained. Last summer I camped on an island in Lake Kahnipiminanikok, and my party amused itself one rainy day noting this twist on cedars, some one having discovered the predominance of right-handed twists. We counted (from my notes) 312 cedars on the island; of these 219 were twisted; of these 187 were right-handed twists. Later an Indian emphasized the need of straight-grained cedars in the hewing of paddles, and the difficulty of obtaining such grains in that vicinity. He also pointed out the fact that the twist is more common in large trees than in young ones, indicating that this character is acquired by some environmental factor. This twist is not alone in the bark, but in the wood as well. It is frequently so extreme as to be a spiral. Among the white and Norway pines the twist is far more common in trees exposed to severe weather conditions, especially to strong winds. Thus I noted that twisted trees occurred more commonly on exposed rocky cliffs and small, open islands, where they receive the full blunt of winter gales. A twisted tree in the heart of the forest is quite rare. But why do they twist so predominantly to the right?

• Fasciation: Plants Running Amuck

THAT STRANGE THING CALLED FASCIATION

Haskin, Leslie L.; *Nature Magazine,* 25:166-168, 1935.

Were you ever astonished while looking over your garden on some fine spring morning to find among your tulips one plant with two, or three, or even four blossoms growing upon the top of a broad, flat stem; or two pansy faces growing together, joined back to back; or, perhaps, a lily that has grown up on a stem, broad and flat as a lath, and with a great packed head of numberless blossoms, twice as many as any normal lily should bear? Have you ever in your walks through woods or fields found some weed, or bush, or vine that towered to twice its normal height, or spread to twice its normal girth, and that was twisted, and branched, and covered with bloom having an abnormal number of parts, or perhaps some of them thickened and fleshy, looking more like a grotesque head of cauliflower than like a normal bloom? If you have noticed any of these things you have undoubtedly seen that strange freak of Nature that the botanists call fasciation. This word comes from the Latin fascis, meaning a bundle, for in these plants it appears as though a number of independent twigs had somehow become bound together in an inseparable union. The whole might be compared to the fasces---that bundle of rods that the Roman lictors bore before the magistrates as a mark of authority.

Fasciation in its more simple form is a not uncommon deformity, and attracts little attention. In its more complex manifestations it becomes a grotesque wonder, and a marvel to the botanist as well as to the layman. Its commonest form is perhaps that seen in the familiar cockscomb. Here is a garden in which fasciation has been encouraged by the growers until it has become hereditary, so that we no longer wonder at seeing the top of the plant spread out in the form of a broad, crimson crest, but take it as a matter of course. What would you think, however, should some great tree near your home suddenly begin to take on such a crested form? One such tree grows in a park at Beecroft, Australia, an Australian pine whose joined and broadened branches form a great woody "cockscomb" nearly thirty feet in height. If this tree continues to grow and thrive as it has in the past, what may it not become? Some Australian pines reach a height of over two hundred feet!

In some cases fasciation is confined to the fruit alone. This is especially common in apples, and in the fruit of the Italian prune, grown so largely on the Pacific Coast. Here, upon one stem, you will find a double fruit; a fruit joined solidly from base to blossom, or joined only at the base, and having two distinct apexes. In picking strawberries, too, you may often encounter a double berry and wonder whether it is the product of one blossom or two.

When next you find such a fruit, look at the stem and see if it is not broad and flattened. If it is you may know that here is a true case of fasciation. Fasciation in nuts has been given a place in the folk-lore of lovers in the age-old game of philopena, "much-loved". The game is usually played by a youth and a maiden upon the finding of a nut with two meats within an apparently normal shell. Each lover then eats one of the meats, and when they meet again some penalty---usually a kiss---is exacted of the one who forgets to utter the endearing word. Such double meats are frequently the result of partial fasciation.

When stems are fasciated, there is often a typical twisting or curving habit of growth that is most interesting. How this twisting comes about is easily explained. What happens when two horses firmly hitched together start to run away, and one of them goes with twice the speed of the other? Almost at once you will see them begin to travel in circles, and the greater the disparity of speed the smaller will those circles be. So it is when two stems of diverse growth are unalterably fasciated together. The

Fasciation in a plantain

one that grows most rapidly will soon be running in circles about the other, and thus a twined, or curving growth ensues. If the variation on growth is extreme, the resulting curves may become so short and abrupt that the very fibre of the plant becomes ruptured, and the stem actually breaks itself from the violent wrenching of its tissue. Just what peculiar form the curves in a fasciated plant may assume depends on how many potentially sep-

arate stems are thus fasciated, and the varying rates of each one's growth. Some become tapering spirals, some run in regular loops, like a coiled spring, and some make such close turnings that an actual tube is the result.

Still more curious are those forms of fasciation in which it seems that instead of a binding of parallel stems, there is an actual blending, one within another, of many stems. In the first form the flowers and leaves, while greatly increased in numbers, nevertheless appear normal in all their parts. In the second form the parts themselves are merged, and then we have the curious sight of a normally five-parted bloom having, instead, ten, twenty, forty, or, finally, an almost innumerable number of petals, stamens, or pistils. In some cases so many parts may be blended that the whole bloom becomes a mere fleshy tubercle, without any recognizable organs. Only one rule can be laid down as to the form that extreme fasciation may take. This is that it will be quite different from anything that you have previously seen.

What causes fasciation? That no one fully knows. One thing is plain. It bears a somewhat close analogy to the bearing of twins, and "Siamese twins", among animals. A bulb or root may be stimulated by an excess of fertility in the soil, or by some other cause, to start a number of new plants by natural division. In some way, however, the division is never fully completed, and from the heart of that plant a monster is born. This might be called healthy fasciation. In other cases disease has some part in the process, and gall mites are supposed to be a contributing cause. As is well known, the sting of gall mites causes an abnormal growth in the surrounding tissue. The stem begins to swell, as in the case of a normal insect gall. Then, perhaps, the plant partially overcomes the poison, and instead of a restricted, normal gall there appears a number of partially joined, monstrous branches that cause the passer-by to marvel and to exclaim.

• Gas Cavities in Trees

GAS IN THE CAVITIES OF TREES

Anonymous; *Scientific American,* 109:207, 1913.

Prof. J. A. Ferguson, of the Pennsylvania State College, reports a curious phenomenon connected with the cutting of hardwood trees in the Ozark Mountains. Cavities near the base of the trees are often found to contain gas. When these cavities are cut into by the oak tie cutters of the region the gas escapes with a whistling sound, showing it to be under pressure, and if lighted

it will burn with a faint yellow flame. The sides of the cavities containing gas are in all cases darkened and look as though seared with a subterranean supply of natural gas, and the land on which they grow is valued accordingly. An examination of the gas collected from a cottonwood tree was made by Prof. Bushong, of the University of Kansas, and it was found to be substantially the same as natural gas with the addition of some free hydrogen. Prof. Ferguson believes, however, that this gas is the product of decomposition of the heartwood of the trees.

• Drinking Orchids

ORCHIDS THAT DRINK

Anonymous; *English Mechanic*, 74:152, 1901.

What is probably the most extraordinary plant ever discovered has now been found by E. A. Suverkrop, of Philadelphia, says a correspondent of the Chicago Inter Ocean, who, during trips to South America, has for some years been contributing to the collection of his friend, Prof. N. E. Brown, of the Herbarium, Kew Gardens, London. The amazing plant which Mr. Suverkrop has now found is an orchid that takes a drink whenever it feels thirsty by letting down a tube into the water, the tube, when not in use, being coiled up on top of the plant. "One hot afternoon," says Mr. Suverkrop, "I sat down under some brushwood at the side of a large lagoon on the Rio de la Plata. Near at hand was a forest of dead, shorn trees, which had actually been choked to death by orchids and climbing cacti. In front of me, and stretching over the water of the lagoon and about a foot above it, was a branch of one of these dead trees. Here and there clusters of common 'planta del ayre' grew on it, and a network of green cacti twined round it. Among the orchids I noted one different from the rest, the leaves, sharp, lance-head shaped, growing all around the root and radiating from it. From the centre or axis of the plant hung a long, slender stem about 1/8 in. thick by 1/4 in. wide, the lower end of which was in the water to a depth of about 4 in. I at once went over to examine my discovery. Imagine my surprise, when I touched the plant, to see this centre stem gradually contract and convulsively roll itself up in a spiral-like roll of tape. But more surprising yet was the object and construction of this stem. I found on close examination and dissection that it was a long, slender, flat tube, the walls about 1/32 in. thick, cellular in construction, open at the outer end, and connected at the inner end to the roots of a series of hair-

like tubes. By subsequent observation I found that when the plant was in want of water this tube would gradually unwind till it dipped into the water. Then it would slowly coil round and wind up, carrying with it the amount of water that that part of the tube which had been immersed contained, until when the final coil was taken the water was dumped, as it were, direct into the roots of the plant. The coil remained in this position until the plant required more water. Should the plant, however, be touched while the tube is extended, the orchid acts like the sensitive plant (mimosa) and the coiling is more rapid. I found many of these plants, all directly over the water or over where the water had been. In the latter case it was almost pitiful to see how this tube would work its way over the ground in search of the water that was not."

• Luminous Plants

ACCOUNT OF A LUMINOUS APPEARANCE OF THE COMMON MARIGOLD

Dowden, Richard; *Report of the British Association, 1843,* part 2, p. 79.

This circumstance was noticed on the 4th of August, 1842, at eight p.m., after a week of very dry warm weather; four persons observed the phaenomenon; by shading off the declining daylight, a gold-coloured lambent light appeared to play from petal to petal of the flower, so as to make a more or less interrupted corona round its disk. It seemed as if this emanation grew less vivid as the light declined; it was not examined in darkness, which omission will be supplied on a future occasion. It may be here added, in the view to facilitate any other observer who may give attention to this phaenomenon, that the double marigold is the best flower to experiment on, as the single flower "goeth to sleep with the sun," and has not the disk exposed for investigation.

SINGULAR PHENOMENON

Ingham, S.; *Knowledge,* 4:60-61, 1883.

A short time ago, I was pricking out some annuals on a flowerbed, on which some geraniums were already planted, when I was

surprised to see flashes of light coming from a truss of geranium flowers. At first I thought it was imagination, but my wife and a friend who were present also saw them. Time was about 9 p.m., and the atmosphere clear. There were other geraniums of a different colour on the same bed, but there was no effect on them. The particular geranium was a Tom-Thumb.

Is this at all common? I have never seen or read of it before.

PHOSPHORESCENCE IN PLANTS

Crie, Louis; *Scientific American*, 46:282, 1882.

In living vegetables emissions of light have been observed in a dozen phaenogamous plants and in some fifteen cryptogamous ones. The phosphorescence of the flowers of Pyrethrum inodorum, Polyanthes (tuberose), and the Pandani has been known for a long time. Haggren and Crome were the first to discover such luminous emanations from the Indian cross and marigold, and a few years ago I myself was permitted to observe, during a summer storm, a phosphorescent light emitted from the flowers of a nasturtium (Tropoeolum majus) cultivated in a garden at Sarthe.

Several botanists have also spoken of the greenish light from the Schistostegia osmundacea, a small plant of the moss family, inhabiting caverns, more particularly in the north of Europe. In this case, however, the phenomenon, which is somewhat complex, is produced by the persistent protonema of the plant, which reflects a beautiful emerald-green color. Meyen, also, has called attention to a small alga of the group of the oscillatoriae, which inhabiting the waters of the Atlantic at the equator, is both colorless and luminous. But such emissions of light are especially peculiar to the fungi. The agaric of the olive-tree (Agaricus olearius), which is remarkable for its beautiful golden yellow color, grows in Provence in the months of October and November, at the base of olive trees and on the trunks of the hornbeam and oak. Mr. Tulasne has remarked that this toadstool, when still young, gives out a bright light and remains endowed with this remarkable property as long as it continues fresh. The seat of the phosphorescence is most usually the surface of the hymenium, although the stipe or stem is also sometimes phosphorescent in some species. The agaric of the olive tree gives out its light only while living; with its death the phenomenon at once ceases. The light emitted is white, steady, and uniform, and resembles that from phosphorus dissolved in oil. This light contains the radiations belonging to the different regions of the spectrum; and when it is produced there is always observed an active absorption of oxygen. The light of a phosphorescent toadstool is extinguished in hydrogen, carbonic acid, or nitrogen. The brilliancy of the white light emitted, far from increasing in pure oxygen, is diminished. As well known, it is the same with regard to phosphorus, which does not shine in pure oxygen.

Below 3° to 4° the phosphorescence disappears, to reappear when the temperature rises; attaining its maximum at 8° to 10°.

We know of still several other luminous toadstools: Agaricus igenus, which grows in the Island of Amboin; A. noctilucens, observed at Manila (Philippine Islands); A. Gardneri which grows in the Brazilian province of Goyaz, on the dead leaves of a dwarf palm; and A. lampas and some other Australian forms. The Agaricus gardneri was discovered in Brazil by Mr. Gardner. This learned botanist met with this species during a dark night in December in walking through the streets of Villa de Natividate. Some boys were amusing themselves with what he at first supposed to be a kind of large firefly, but on inquiry he found it to be a beautiful phosphorescent toadstool which grew abundantly in the neighborhood on the dead leaves of a dwarf palm. The whole plant gives out a bright light similar to that emitted by the larger fire-flies, and having a pale greenish hue. This circumstance, and its growth on a palm, had given it the name among the inhabitants of "flor de coco." The same fungus grows also in Borneo.

"The night being dark," says Dr. Cuthbert Collingwood, in his account of the Bornean plant, "the fungi could be very distinctly seen, though not at any great distance, shining with a soft pale greenish light. Here and there spots of much more intense light were visible, and these proved to be very young and minute specimens. The older specimens may more properly be described as possessing a greenish luminous glow, like the glow of the electric discharge, which, however, was quite sufficient to define its shape, and, when closely examined, the chief details of its form and appearance. The luminosity did not impart itself to the hand, and did not appear to be affected by the separation from the root on which it grew, at least not for some hours."

The same writer also adds: "Mr. Hugh Low has assured me that he saw the jungle all in a blaze of light (by which he could see to read, as, some years ago, he was riding across the island by the jungle road; and that this luminosity was produced by an agaric."

Mr. James Drummond discovered in Australia two toadstools which at night gave out an extremely curious light. One species was growing on the stump of a Banksia in Western Australia. When the plant was laid upon a newspaper it emitted by night a phosphorescent light which enabled persons to read the words around it, and it continued to do so for several nights with gradually increasing intensity as the fungus dried up. The other species was detected some years afterward. This specimen measured sixteen inches in diameter, and weighed about five pounds. This plant was hung up to dry in the sitting room, and on passing through the apartment in the dark it was observed to give out the same remarkable light.

The luminous radiations of these cryptogams are very varied in their character. We have already seen that the light emitted by the olive tree agaric is white, steady, and uniform; Agaricus igneus shines with a bluish light which recalls that which the

leaves of the poke (Phytolacca decandra) give out at times; while A. gardneri emits a greenish light.

But this phosphorescence is in no wise confined to the genus Agaricus, for recently in our own country (France) I have observed Auricularia phosphorea and Polyporus citrinus emitting luminous radiations. The first of these grows on partially rotted trees, and the other on the trunks of willows, oaks, and apple trees. Some time ago, a remarkable case of luminosity was recorded as occurring in England: "A quantity of wood had been purchased, and afterward dragged up a hill to its destination. Among this was a log of larch or spruce. Some young people going to pass the night on the hill, were surprised to find the road strewn with luminous patches, which, when more closely examined, proved to be portions of bark or little fragments of wood. Following the track, they came to a blaze of white light which was perfectly surprising. On examination, it appeared that the whole inside of the bark of the log was covered with a white byssoid mycelium, of a peculiarly strong smell, but unfortunately in such a state that the perfect form could not be ascertained."

Mr. Tulasne was the first to make known the spontaneous phosphorescence of dead oak leaves. "These leaves," says the learned mycologist, "were all of the preceding year, and had fallen naturally at the approach of spring. Their tissue still possessed elasticity and great cohesive strength. None of them was luminous on its whole surface. In general, the most brilliant points were those where the brown or gray color of the leaf was lightest--those especially that a peculiar alteration of the parenchyma had rendered very thin and almost whitish. I also saw shining in the same way buds that were dried up and partially destroyed, as also a small twig that had certainly perished on the oak that had produced it. The disarticulated surface of this twig alone gave out a bright light. The brilliant surfaces of these different objects were all more or less moist with water. Wiping them off with the finger diminished their brilliancy, yet it was necessary to rub them briskly for some instants to render them dark, and no phosphorescent matter adhered to my hand."

The rhizomorphs, or vegetative apparatus, of a large number of fungi are likewise phosphorescent. These productions extend beneath the soil in long branching cords and threads in the vicinity of old stumps, especially of the oak, which are in a state of decomposition, and to which they are affixed by some of their threads.

All these plants that we have just mentioned emit light during their life and when they are in a state of decomposition. We might also cite the phosphorescence of the milky juice of certain Euphorbiaceoe, and of the pulp of certain fruits (such as that of the peach and apricot) that are beginning to decay, but we think that we have said enough to demonstrate the frequency of the phenomenon of phosphorescence in the vegetable kingdom.

LUMINOUS MOSS

Webster, Ellen Emeline; *Natural History,* 58:309-311, 1949.

It may flash like emerald gold one minute, then as quickly dim into invisibility. How could anything so entrancingly beautiful be scarcely mentioned in books on mosses? It is so little known that my first view of it brought the thrills of original discovery.

My path in search of this remarkable plant has bristled with interrogation and exclamation points. It was late in September when my lifelong friend, Miss Mary Lamprey, sent us, her callers, in search of this rare botanical treasure. For many years she had been harboring its growth in her barn cellar on a New Hampshire hillside. The sight fairly made us catch our breath. We saw on the hard-packed, cellar-bottom soil--between barrels and piles of boards, between a wagon body and a chopping block--aisles of dazzling, sequin-flecked, green-gold carpeting! We gazed in fascinated amazement. What magic brush had surpassed all previous achievements to produce these emeral-gold surfaces!

We stooped to scrape up a spoonful of the soil to look at it more closely. Instantly all its goblin-like luminosity faded out, and there appeared just plain black dirt! Absolutely nothing to clutch but well-trodden earth! Not a sprig of moss did we see! Not a particle of lichen-like growth met our eyes!

Straightening up, we looked about. There, again, on the granite underpinning, on and under a straddling sawhorse, were splashes of glittering green. These spots were not only a bright green; they glowed as if electric lights were behind them. We could scarcely believe our eyes, still less explain the wonderful spectacle.

Days passed. Several libraries were searched without finding mention of Luminous Moss in textbooks or encyclopedias. At the New Hampshire State Library, the late Dr. A.J. Grout's Mosses With a Hand Lens did give the most intriguing description of Schistostega that has yet been written by an American. He had been impressed as we had with the peculiarities of its luminosity and with its fleeting "temperament."

• A Plant with Fast Reflexes

THE BARBARIC BLADDERWORT

Keithley, Willis E.; *Creation Research Society Quarterly,* 9:95, 1972.

If one were asked to tell the difference between a plant and an animal, the obvious response would be that the animal moves,

while the plant does not and that the animal captures its food, while the green plant produces its foods from sugar which it manufactures in photosynthesis. Both of those assumptions would be invalid for some plants which exhibit a surprising "agility" in a tropism called turgor movement.

One of the most spectacular of these reactions is seen in the common bladderwort, Utricularia vulgaris. This plant is completely water-borne and reveals a carnivorous nature by snaring small aquatic insects and crustacea. The traps consist of many small bladders or sacs attached to the stem by a very fragile petiole (See cover illustration). At the mouth is a hinged trapdoor with a very sensitive trigger. To set the trap, the pod or sac is collapsed, just as you would suck the air out of a toy balloon.

When a swimming insect or other small animal touches the trigger hair, the bladder immediately expands in 1/50 of a second! This creates a vacuum in the sac and the inrush of water engulfs the hapless insect, which is now ingested while the door is closed and the trap reset.

But the operation is even more spectacular than this description. The petiole by which the trap is attached is quite weak and fragile. Thus, since the sac is not securely anchored, the hydraulic resistance of the inrushing water actually draws the entire trap forward, and in that twinkling of an eye, strikes out like a snake the full length of its petiole. A jaded biologist may be revived with a strand of Utricularia, a binocular microscope, and a common straight pin. As the trigger hair is touched, this astounding gnat-trap will snatch the pin so rapidly that the action cannot be seen, but so violently that the tug can actually be felt.

Of course, the obvious questions are, how is such a tropism accomplished without nerve or muscle tissue; what caused it to develop an appetite for plankton; how did it satisfy that yen before the trap was invented; and how did the plant then develop such an extravagant mechanism? We search in vain for any process of adaptive progression, or any primeval ancestry which had profited from some fortuitous experiment of "Mother Nature."

Would it be too tenuous to suggest a Divine act of creation? No other evidence provides an adequate answer.

• Extremely Rapid Growth

A MORE PHENOMENAL SHOOT

Prouty, W. F.; *Science*, 54:170, 1921.

The July 1, number of Science records a "phenomenal shoot"

which grew near Raleigh, N.C. This shoot grew from the stump of a beheaded tree of Paulownia tomentosa in one season to the length of 19 feet 5 inches; had twenty internodes, and was 7.75 inches in circumference at the base. This shoot is thought by Mr. Wells to be "a record for the tree type of woody plant in the temperate zone."

During the past season the writer kept track of a shoot which grew from stump of a beheaded tree of Paulownia tomentosa. This shoot grew during the season of 1920 to a length of 21 feet 6 inches, it has twenty-four internodes and is ten inches in circumference at the base. One of the leaves, measured in the latter part of July, was 38 inches in largest dimension. This shoot grew in clay loam soil residual from granite on property adjoining the campus of the University of North Carolina, Chapel Hill, N.C. The shoot is on exhibition in the Geological Museum of the University.

• The So-Called Graft Hybrids

MYSTERY OF THE "GRAFT HYBRIDS"

Anonymous; *New Scientist,* 2:7–8, September 12, 1957.

A first-class scientific mystery is brewing. In Moscow, Pekin and Belgrade biological laboratories are producing what are alleged to be "graft hybrids." British and American biologists object that the existence of such creatures is inconsistent with accepted genetical principles.

Ever since Darwin's day claims have from time to time been made that hereditary qualities can be transferred from one variety of plant or animal to another by grafting or other non-sexual union. In the period 1926-1936 the French botanist Lucien Daniel made a number of seemingly convincing demonstrations of "hybrides de greffe" in fruit trees and certain species of Helianthus. Since then a flourishing school of research on graft-hybridisation in tomatoes and related plants has been built up in the Moscow Institute of Genetics by I. E. Gluschenko.

In Hungary a small but apparently clear-cut tomato experiment was reported a little time ago by L. Felfoldy. Yet when A. J. Bateman repeated the same experiment on the same material in England, he found no sign of an effect of the stock upon the graft. This led Felfoldy to suspect his own results on the grounds of possible uncontrolled cross-pollinations by insects.

But during the same period independent reports arrived from China and from Japan describing dramatic and heritable transfor-

mations in different egg-plant varieties. And now R. Glavinic of Belgrade, who in 1955 reported her tomato grafting work to the 15th International Horticultural Congress in Holland, has published extensive positive results in such detail as to raise hopes that from it new genetical laws may be deduced and a new branch of the subject opened.

But at the same time in Russia A. R. Zhebrak, a confirmed sceptic of graft-hybridisation, has performed similar experiments and drawn an uncompromising blank.

. . . . .

• Possible Inheritance of Acquired Characters

THE HERIDITARY TRANSMISSION OF ACQUIRED CHARACTERS

Haldane, J. B. S.; *Nature,* 129:817–819, 1932.

Last year Prof. E. W. MacBride restated the case for Lamarckism in a discourse under the title of "Habit, the Driving Factor of Evolution". Prof. MacBride's contributions to the science of embryology have been most distinguished, and his apparently heretical views on the historical process of evolution, for example, concerning the descent of the vertebrates from a common stock with the echinoderms, have been confirmed by subsequent work. But just as the best of historians may not be the best judge of the politics and economics of his own day, so an eminent student of the history of evolution may conceivably be mistaken as to its causes. These must be decided on by observing the changes which actually occur in living organisms under controlled conditions.

I shall begin by giving an example of one of the rather rare cases where an acquired character is unquestionably inherited. If we cut off the growing point of a young tomato plant it produces new shoots. A few of these regenerated shoots are somewhat abnormal. The leaves are larger, and if these shoots are cut off and rooted, a new race of tomato is produced. It differs from the original stock in the following respects. The leaves are thicker, less dissected, and have more anthocyanin. The fruits are smaller but more numerous. The whole tempo of the plant is slower. It does not start growth so quickly, but continues for a longer time, and finally reaches a larger size. What is more, not only do the abnormal plants transmit the character by vegetative propagation, but if their flowers are self-fertilised the seedlings resemble the abnormal parents. They can only be crossed with the original type with great difficulty, and the hybrids are

extremely sterile.

Now, if we examine a dividing nucleus of the ordinary tomato, we find 24 chromosomes. The abnormal type has 48, or double this number. Such plants are called tetraploids, because they have four similar sets of chromosomes, instead of two as in normal tomatoes. We know that the chromosomes are responsible for most, if not all, of the heritable differences between different tomatoes. By our injury we have provoked a stable and self-perpetuating rearrangement of the physical basis of heredity. Where this is possible, acquired characters are inherited.

Induced tetraploidy has doubtless been of some importance in plant evolution. But it does not produce very striking changes. Moreover, there is no suggestion that they are adaptive, that is to say, that the tetraploid plants are better off than the diploid in any way; in particular, they do not appear to resist mechanical injury any better. The importance of the Lamarckian hypothesis for evolution lies in the possibility that adaptive changes, such as undoubtedly occur in the life of an individual, are handed on to its offspring. (p. 817)

I pass over several other cases where apparently Lamarckian effects have been due to infection, and Kammerer's experiments, which terminated in his suicide, to the very remarkable results obtained by McDougall on rats. Unlike the other workers here cited, he began with an approximately pure line of rats which had been inbred for many generations. He also took considerable precautions to avoid selection. Unfortunately, the procedure of training was twice changed during the experiment, so I shall only describe the latter half.

The rats at an age between three and four weeks were dropped into a tank of water in which two turnings led to platforms covered with wire gauze by which they could emerge. One of these (alternately on the right and left) was illuminated, and a rat stepping out of the water on to it received an electric shock. Each rat was dropped in six times daily until it learned to avoid the illuminated platform. Learning was regarded as complete when this occurred on twelve successive trials. After twelve generations, all but one of which had been trained under slightly different conditions, but had shown a decided increase in educability, the procedure was finally standardised. In nine generations the average number of errors fell from 80 to 25, and the number made by the best rat from 42 to 3. In an experiment on a related group, the number of errors fell from about 170 in the first generation to 114 in the fifth. Finally, the worst performers were selected during two generations, but in spite of this, the time needed for training fell.

To obviate the possibility of tradition, mothers of a slightly trained stock were mated first with slow-learning and then with quick-learning males. The first mating gave an average number of 166 errors, the second of 62. The character acquired appears to be caution. It does not seem to be general intelligence, general timidity, or a specific fear of the bright gangway.

One very important point which emerged was that the progeny of the trained rats at first showed much greater variation in

learning power than those of the untrained. If capacity for learning is inherited within the group (which could be ascertained or disproved from McDougall's records were they published in full), the process of change would have been greatly accelerated if selection had also taken place, if in fact the spread of acquired variations through the population had been aided by selection, as Darwin thought was the case in ordinary evolution.

Now, while it is clear that McDougall's experiment is in a class by itself, it has been criticised by Sonneborn, among others. Sonneborn notes that the intensity of the shock varied considerably and was not measured, and that McDougall found that rats sujected to light shocks took nearly three times as long to learn as when the shocks were heavy. So a progressive increase in the shock intensity could account for McDougall's results. He further points out that, if the method of choosing two rats 'at random' from a litter was to take the first two available in a cage, this would tend to select rats of a particular psychological disposition. Various other criticisms can be made. Hence it would seem that, while one must admit that McDougall has made out a prima facie case, a suspension of judgment is not unreasonable before we regard him as having demonstrated a principle in evolution which is unsupported by other evidence of the same calibre. It is, however, worth noting that in another experiment, broken off after four generations, McDougall found that the time needed for training increased in successive generations. He put this down to an increase in timidity, thus contriving to eat his cake and have it. (pp. 856-857)

• Some Remarkable Adaptations of Plants

EVOLUTIONARY MECHANISMS IN POLLINATION BIOLOGY

Baker, H. G.; *Science,* 139:877-885, 1963.

Tropical epiphytic orchids provide some of the most intricate pollination systems, and these can only be operated successfully by particular insects. They represent the ultimate in reliance upon floral mechanisms for the promotion of outbreeding and might be expected, on a basis of Darwinism, to show to a high degree correlation of floral evolution with differentiation in the insect visitors. Unfortunately, in some cases, at least, this is not so.

In the tropical American orchid Gongora maculata, the pollinator is a small bee, Euglossa cordata. The male bee is attracted to a flower by its fragrance, the source of which is a deep cleft at the base of the lip. If, in attempting to reach this, the bee

clambers onto the keel-like, slippery plates of the lip, it may fall, slide on its back over the smooth concave surface of the column, strike the tip of the anther, and remove the pair of pollinia. Insertion of these pollinia from the back of the insect into the very narrow stigmatic cleft is possible only after a rather protracted period and, in this way, cross-pollination is more likely to occur than selfing.

Another orchid, Coryanthes speciosa, operates an equally complex but entirely different mechanism. Here again, male bees are attracted by the fragrance. As it alights on the mesochile, the bee maintains its position by the vigorous use of its wings. Ultimately, these hit against a drop of fluid (secreted by the glands at the base of the column) which had been hanging over the head of the bee. This dislodged drop then carries the bee with it into the liquid-filled bucket formed by the epichile. The struggle of the bee to free itself from this prison is likely to be prolonged and is successful only when it pushes its way up past the anther and emerges through the lateral opening of the lip, with two pollinia attached to its back. By this time the fragrance of the flower has vanished and the bee flies away. However, the next day, fragrance and the secretion of the droplets of fluid return, and a bee, presumably already carrying pollinia, may go through the process again and leave a pollinium on the stigma.

The punch line to this story of two elaborate but entirely dissimilar mechanisms is that the bee involved in each case is of the same species, Euglossa cordata. Apparently the multiplicity of characteristics in each kind of flower (including the appropriate timings of glandular secretions and fragrance) have evolved without any obvious evolutionary change in the bee. This suggests that one or both of these systems for the pollination of orchids is the product of recent, sudden evolution. In any case, it is very difficult to imagine how they could have been built up gradually, and one is tempted to see in them the result of fortuitous but apparently successful variation of a number of characters at the same time. This is not to deny the probability that innumerable unsuccessful "mutations," or "fortuitous recombinations," occurred before the efficient one was produced, or that each system has been brought to its present degree of perfection by subsequent minor modifications and elaborations. Although flowers and insects may both be evolving, there is no need to suppose that the steps taken by each will necessarily be contemporaneous. (pp. 879-880)

In the case of Aquilegia and Penstemon, the assumption has been made that suitable pollen vectors were available when the new types of flower evolved suddenly. The converse is suggested in other cases. Thus, the fossil record of the bats (Chiroptera) does not reach back beyond the Paleocene epoch, and the flower- and fruit-visiting Megachiroptera appear to have evolved too late to be able to enter the New World from the Old World tropics. In the New World tropics, nectar-lapping bats appear to have evolved relatively recently from an insectivorous microchiropteran stock. On the other hand, the tree genus Parkia is pantropical; its South American species have been separated from those of the Old World since the Eocene epoch at the latest. Nevertheless, Parkia trees

have been shown to be pollinated by bats in Southeast Asia, West Africa, and South America, and the same specialized adaptations exist in each area. Thus we have a real problem; the plants appear to have been ready for the bats before the bats were available. What could have been the nature of their visitors before the bats took over?

A similar problem is presented by the nocturnally flowering kapok tree, Ceiba pentandra (Bombacaceae), which is pollinated by bats in West Africa and South America. (p. 881)

FLOWERS COLLABORATE TO FOSTER WINNING WAYS

Ridley, Mark; *New Scientist,* 86:139, 1980.

Different plant species that live together and are pollinated by hummingbirds have independently evolved flowers that resemble each other, according to James Brown and Astrid Kodric-Brown, of the University of Arizona, who have made a study of hummingbird-pollinated flowers in central Arizona (Ecology, vol 60, p 1022). Evidently the advantage to the plants of being more attractive to the pollinating hummingbirds more than outweighs the risk of receiving pollen from another species.

Plants exchange nectar for pollination, in a trade that benefits plants and pollinators. Birds, for example, learn what kinds of flowers give nectar, and by moving between the plants, transfer pollen. Previous studies usually found that coexisting plants have different flowers, a phenomenon which ecologists have explained as a mechanism by which the plants ensure that they receive pollen only from their own species. The differences between flowers make it easy for the pollinators to discriminate between species.

Brown and Kodric-Brown, however, found that nine species living together in a single area and pollinated by hummingbirds all have similar red, tubularshaped flowers. The tube is shaped so that it will admit only a hummingbird's beak and tongue. Apart from their flower shape, the plants differ in many ways, indicating that they must have converged to the same kind of flower during evolution, rather than that they simply shared a pattern inherited from a common ancestor.

. . . . .

INTRAFLORAL POLLEN DIFFERENTIATION IN THE NEW WORLD . . . . .

Mori, Scott Alan, et al; *Science,* 209:400-403, 1980.

Abstract. Floral biology, pollen germination, and scanning electron microscopy studies have indicated different types of pol-

len within the same flower in at least two species of New World Lecythidaceae, subfamily Lecythidoideae. Two types of pollen are produced in different parts of the androecium and serve different functions. One type attracts pollinators by providing a reward, and the other functions in fertilization. The former type has lost its ability to germinate, at least under in vitro conditions.

A carpenter bee collects infertile pollen from a species of Lecythidacea. *Fertile pollen at the top of the flower clings to the bee.*

UNIQUE ADAPTATION FOR SUBMARINE POLLINATION IN SEAGRASSES

Pettitt, J. M., et al; *Nature,* 286:487, 1980.

Abstract. Seagrasses are flowering plants that are adapted for pollination submerged in the sea. Many species, including the

Australian sea nymph, Amphibolis antarctica, have filiform pollen grains. These grains have no exine---the resistant pollen wall layer characteristic of terrestrial plants---and each may be up to 5 mm in length. The pollen has a similar density to seawater so that it remains suspended or floats after release from the anther. The stigma of the sea nymph and related seagrasses is a simple branched structure which protrudes between the leaves when receptive and has a surface secretion with cytochemically detectable enzymatic activity. This stigma coating is not dispersed in seawater. However, submarine pollen-stigma interactions have never been observed in any species of seagrass. We demonstrate here that pollination in the sea nymph has several unusual features when compared with the events in terrestrial flowering plants. These features include the waterproof nature of the adhesive binding the pollen to the stigma surface, the unique mechanism for pollen germination in the inaperturate filiform grains, and the mode of pollen germination in the inaperturate filiform grains, and the mode of pollen tube penetration into the stigma. Taken together these characteristics suggest a considerably more refined level of adaptation to the marine environment than hitherto supposed.

• Possible Convergent Evolution

CONVERGENT EVOLUTION OF MEDITERRANEAN-CLIMATE EVERGREEN SCHLEROPHYLL SHRUBS

Mooney, Harold A., and Dunn, E. Lloyd; *Evolution,* 24:292–303, 1970.

It has been long noted that in areas of climatic similarity throughout the world the aspect, or physiognomy, of the vegetation is also similar. This relationship of climate to vegetation forms the very foundation of ecological plant geography. There has been a long history of attempts to utilize various nonfloristic, physiognomic characterizations of vegetation and to relate these to regional, local, and microclimate. Although the details of the different research approaches have been subject to a certain amount of controversy the broad generalities are above dispute. Environmental similarity can produce growth form similarity no matter what the evolutionary history of the flora in question.

In California, for example, within the chaparral vegetation, the dominant plants belong to such diverse families as the Ericaceae, Rhamaceae, and Rosaceae, yet all possess a large degree of growth-form similarity, i.e., deep-rooted, evergreen, sclerophyllous shrubs. Even more striking than this within-vegetation type

homology is the homology found between vegetation growing in similar but disjunct climates.

The mediterranean climatic type (summer drought, winter rain) occurs in California, South Africa, central Chile, southern Australia, as well as in the Mediterranean region. In all five of these areas the native vegetation has a similar appearance: a dense scrub dominated by woody evergreen sclerophyllous species. The isolation of these geographic areas and the almost complete taxonomic dissimilarity between their resident floras indicates that they have had distinct evolutionary histories (p. 292).

• Mimicry in Plants

HOST-PARASITE RESEMBLANCE IN AUSTRALIAN MISTLETOES: THE CASE FOR CRYPTIC MIMICRY

Barlow, Bryan A., and Wiens, Delbert; *Evolution,* 31:69–84, 1977.

The Adaptive Roles of Mimicry in Higher Plants. Considerable confusion surrounds the concept of mimicry. Wickler restricted the term to situations where a potential prey species deceives a predator by providing false signals of unpalatability or warning. Cryptic, or camouflaged species, thus avoid predation by giving no signals whatever; thus Wickler places such organisms in a category totally distinct from mimicry. We use the term "mimicry" in a very broad sense, meaning "any situation where resemblance to another object (animate or inanimate) confers upon that organism increased Darwinian fitness". The nature of the mechanism appears to us to be secondary to the effect.

Mimicry in animals, particularly insects, is a long-known phenomenon with an extensive literature. In flowering plants the subject has been largely ignored, although Wickler included examples of plant mimicry in his recent review.

The best known examples of mimicry in plants involve pollination phenomena such as pseudocopulation, pseudoantagonism and other forms of floral mimicry, particularly in orchids. Proctor and Yeo considered these phenomena to be examples of Batesian mimicry. Chemical mimicry occurs in carrion and dung flowers, and behavior mimicry is also associated with this pollination mechanism in Ceropegia (Asclepiadaceae) where shimmering black hairs, when activated by mild wind currents, appear to resemble masses of crawling flies. Proctor and Yeo suggested further that the development of similar floral patterns in a number of phyletically diverse angiosperms represents Mullerian mimicry. Definitive experimental data on this point, however, are lacking.

Mimicry involving seeds or fruits is also known. In the leguminous genus Adenanthera the seeds mimic berries so effectively that they are rejected by seed-eating birds, whereas frugivorous birds readily accept them. Crop "mimicry" is known in a number of weedy genera, e.g., Camelina, Silene, and Spergula, where growth habits and fruits or seed characteristics have come to resemble those of one particular crop they infest.

Mimicry involving vegetative structures appears to have a variety of adaptive roles. In some insectivorous plants the trapping devices apparently mimic flowers, or at least present a number of floral cues, e.g., colors and nectar, which ostensibly lure pollinating insects into the traps. In some Passiflorae the stipules mimic the eggs of Heliconius butterflies. These butterflies will not oviposit on plants which already have eggs deposited on them; thus through egg mimicry Passiflorae presumably achieve a degree of protection from predation by Heliconius larvae. Gilbert also suggested that many species of Passiflorae apparently mimic the leaves of a wide variety of plants which are unsuitable hosts for Heliconius. The apparent rarity of this kind of mimicry is probably related to the near absence of visual orientation in butterflies; Heliconius is one of the few Lepidoptera known to respond to visual as well as chemical stimuli.

The kind of mimicry which may be established in mistletoes is that in which vegetative structure is such that the whole plant blends cryptically with its background. Most of the examples of this type involve mimicking of abiotic objects such as stones or soil. This is well documented in the case of the so-called "stone plants" of the South African deserts. These succulents, e.g., Lithops and many other genera, show a remarkable resemblance to the rocky substrates on which they occur. They do not simply resemble small stones, but actually duplicate the specific color and texture of the particular rock types to which their distribution is restricted. These genera are scattered through a number of unrelated families including the Asclepiadaceae (milk weeds), Aizoaceae (carpet weeds), Crassulaceae (stone crops), Euphorbiaceae (spurges) and also Liliaceae. Obviously the phenomenon has evolved independently many times. Similar examples (but as yet totally undescribed) may occur in some cacti in the southwestern United States, viz., Pediocactus and Sclerocactus where some species also appear to mimic their substrates. For example, P. peeblesiana resembles rounded stones and is found only along ancient shorelines where such rocks predominate. Other species, e.g. P. sileri and S. spinosior, appear to duplicate the color of their substrates.

There are few known cases of plants which mimic other plants with which they are associated. The spines of Pediocactus toumeyi and Sclerocactus wrightii apparently mimic the leaves of certain grasses with which they normally occur, i.e., Bouteloua (gramma grass) and Hilaria (galleta), respectively. A possibly similar situation may exist in Opuntia glomerata, where the spines are highly modified and form dry, ribbon-like structures which could serve no defensive function. According to Britton and Rose, the species occurs on "dry hills" of western Argentina and the dry,

ribbon-like spines could easily mimic the leaves of dried grasses which they much resemble.

The crypsis in both stone plants and cacti is presumably a response to grazing pressure by mammals, but no definitive studies have been made. Some cacti, however, are known to be utilized for food by rodents. In South Africa extreme grazing pressure must have been exerted on the stone plants by the immense herds of herbivores which occurred in these arid regions until about 150 years ago.

Mistletoes, in their almost unique situation as arboreal, perennial plant parasites, are therefore unusual in their ecological opportunity to benefit from crypsis of other plants, namely their hosts. The adaptations documented below may therefore represent one of the most significant cases of whole plant/plant mimicry available.

. . . .

Summary. The close resemblance in leaf form and presentation between many Australian species of loranthaceous mistletoes and their usual hosts is interpreted as protective mimicry. It is argued that this mimicry evolved primarily as a response to predation pressure by herbivorous, arboreal marsupials. The phenomenon is strongly developed in the Australian loranthaceous flora, but rare or absent on other continental areas. This is attributed to a relatively high palatability of mistletoes compared with that of dominant Australian forest trees, which has increased the attractiveness of the mistletoes as food plants. Many mistletoe species may currently exhibit lower host specificity than in the recent amelioration of the climate, the geologically recent extinction of large, mainly terrestrial herbivores, and perhaps also to extermination of the herbivore predator.

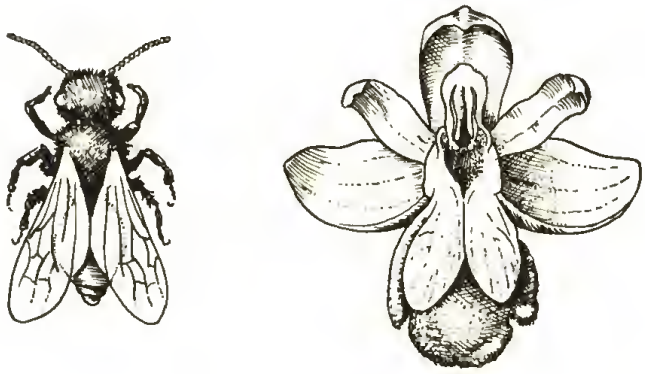
SPECIFICITY AND BEHAVIOR IN SYMBIOSES

Davenport, Demorest; *Quarterly Review of Biology*, 30:29-46, 1955.

However, as Baerends says, there are numerous symbioses, such as those between flowering plants and their hymenopterous pollinators, in which releasers are most definitely present and may be precisely identified. Among these he selects the highly interesting phenomenon of pseudocopulation:

"... the flowers of some orchid genera are parasitic on the releasing mechanism of some Hymenoptera which they attract without presenting them a food source ... every orchid species is here bound to only one or two insect species. In Europe and North Africa several students ... have found combinations of Ophrys muscifera with the Sphegid wasp Gorytes mystaceus, of Ophrys speculum with the Scoliid wasp, Dielisciliata, and of Ophrys fusca, Ophrys lutea and Ophrys arachnitiformis to solitary bees of the genera Andrena, Colletes and Eucera. In Australia, Coleman ...

has found a similar relation of the orchid Cryptostylis leptochila to the Ichneumonid wasp, Lissopimpla semipunctata. It is always the males that visit the flowers and fertilize them by carrying out the copulation act on the libellum. The flowers and especially the libellum resemble an insect very much, and Pouyanne could experimentally show that the sign stimuli releasing the attack of the insects are sent out by the libellum. As far as I know there is not much known of the stimuli releasing copulation attempts in Hymenoptera, but I happen to have had the opportunity to do some observations and experiments with males of the Sphegid Ammophila adriaansei. In these males motionless objects of roughly similar



This orchid mimics a female wasp and induces pseudocopulation by male wasps to transfer pollen.

coloration and similar dimensions released copulation attempts, features therefore equivalent to those we find in the orchid libellum. Chemical factors do not seem to play a role....

"The orchids are especially fertilized by the insects as long as the female Hymenoptera have not left the pupae (the males usually hatch a week to a fortnight earlier). During that time the males are likely to suffer from threshold lowering with regards to the sexual reactions; they will therefore more easily respond to non-adequate stimuli."

On the basis of this last statement one may wonder whether Baerends is not being delicate when he states that the orchids are parasitic on the wasps and that they only benefit from the association.

He continues: "The orchids need the insect to get fertilized; still they do not produce nectar or other food stuffs, but they make use of a releasing mechanism in the hymenopteron, a mechanism that has so much biological importance to the insect that it will be retained. Their action toward the flower is of no biological value to Gorytes, but it does not harm it either. As the

copulation act of Gorytes is of great importance to Ophrys, its releasing organ will be maintained in the flower, and we may even expect it to develop further." (pp. 40-42)

LIVING STONES

Anonymous; *English Mechanic*, 77:406-407, 1903.

The most curious specimens of vegetable or plant life in existence are the so-called "living stones" of the Falkland Islands. Those islands are among the most cheerless spots in the world, being constantly subjected to a strong polar wind.

In such a climate it is impossible for trees to grow erect, as they do in other countries, but Nature has made amends by furnishing a supply of wood in the most curious shape imaginable. The visitor to the Falklands sees scattered here and there singular shaped blocks of what appear to be weather-beaten and moss-covered boulders, in various sizes.

Attempt to turn one of these "boulders" over and you will meet with a surprise, because the stone is actually anchored by roots of great strength; in fact, you will find that you have hold of one of the native trees.

No other country in the world has such a peculiar "forest" growth, and it is said to be next to impossible to work the odd-shaped blocks into fuel, because the wood is perfectly devoid of "grain," and appears to be a twisted mass of woody fibres.

UNUSUAL PHYSICAL ABILITIES

- **The Rain Tree**

RAINING TREES

Anonymous; *American Journal of Science*, 1:15:170, 1829.

In the ancient histories of travellers in America, and also by

Thevet in his Cosmographia, mention is made of a tree which attracted the clouds from the heavens, and converted them into rain in the dry deserts. These relations have been considered as fables. There has lately been found in Brazil a tree, the young branches of which drop water, which falls almost like a shower. This tree to which Leander has given the name of Cubea pluviosa, is transferred by M. Decandolle to the genus caesalpinia (pluviosa), in his Prodromus, vol. II, p. 483. Also, many vegetables, as the calamus rotang, and tender climbing plants, the vine, and other twigs, at the season of sap, weep abundantly, particularly when they are cut.

THE LEGEND OF THE RAIN TREE

Anonymous; *Scientific American,* 105:244, 1911.

During the last few months American newspapers have revived the well-worn tale of the "Peruvian rain-tree," which affords protection against drought. The leaves of this tree are said to have the property of condensing atmospheric moisture in large quantities and precipitating it in the form of rain. According to one writer, "the water falls from the leaves and oozes from the trunk and forms veritable rivers, which can be led as irrigating canals to any point desired. Making liberal allowance for evaporation and infiltration, a square mile grove of the trees would supply for the tribution about 100,000 gallons of water daily.

On the strength of similar stories one of the trees to which the name "rain-tree" has been most often applied, viz, Albizzia (or Pithecolobium) Saman, has lately been exploited and sold extensively in Australia. The virtues claimed for it have proved to be altogether illusory, although it is useful as a shade-tree, and is widely planted for this purpose in semi-tropical and tropical countries.

The legend of the "rain-tree" or "raining-tree" dates back to the stories of the Fortunate Isles, where no rain fell from the skies and the soil was refreshed by the moisture shed by a tree of the sort described. The early navigators brought home stories of similar trees in the East Indies, in Guinea, Brazil, etc. The Peruvian rain-tree appears to have been brought to the notice of the world by the reports of a United States consul in Peru, about 1877. These reports were widely quoted at the time, and led the government of India to seek information on the subject from the authorities of Kew Gardens. The investigations of W.T. Thistleton Dyer brought to light a plausible explanation of at least a part of the rain-tree stories.

The traveler Spruce reported his own experiences with the rain-tree as follows:

"The Tamia-caspi, or rain-tree of the eastern Peruvian Andes, is not a myth, but a fact, although not exactly in the way popular rumor has lately presented it. I first witnessed the phenomenon

in September, 1855, when residing at Tarapoto. I had gone one morning at daybreak, with two assistants, into the adjacent wooded hills to botanize. A little after seven o'clock we came under a lowish spreading tree, from which with a perfectly clear sky overhead a smart rain was falling. A glance upward showed a multitude of cicadas sucking the juices of the tender young branches and leaves, and squirting forth slender streams of limpid fluid."

This is not the only explanation. That many plants spontaneously exude moisture under suitable conditions is well known. The phenomenon is called "guttation," and has perhaps been most fully described by A. Burgerstein in his work "Die Transpiration der Pflanzen" (Jena, 1904). The moisture drawn up from the roots of plants most frequently passes off into the air in a gaseous form; i.e., by transpiration. If the air is saturated with moisture, and if the supply of moisture to the roots is copious, then liquid drops will be exuded sometimes in large quantities. Molisch records a case in which a single leaf of a species of Colocasia gave off 190 drops per minute. Burgerstein gives a list of 241 plants, belonging to 101 families, in which guttation has been observed.

This process goes on chiefly at night, and in cloudy and foggy weather; i.e., when the relative humidity of the air is highest. It is altogether probable that in the moisture parts of the tropics there are trees which exhibit this phenomenon in such a degree that the name "rain-tree" may be fittingly applied to them. It is, however, certain that no such process can occur in a dry climate, and that the proposal to plant the rain-tree as a panacea against drought is entirely chimerical.

RAIN FROM CLOUDLESS SKY

Anonymous; *Monthly Weather Review,* 14:287, 1886.

The following is from the "Charlotte Chronicle" of October 21, 1886, published in Charlotte, North Carolina:

> Citizens in the southeastern portion of the city have witnessed for three weeks or more a very strange phenomenon, and it is to the effect that every afternoon at 3 o'clock there is a rainfall in one particular spot, which lasts for half an hour. Between two trees at the hour named there falls a gentle rain while the sun is shining, and this has been witnessed every day during the past three weeks by the people in the neighborhood.

The Signal Service observer at Charlotte makes the following statement in regard to the phenomenon:

> An unusual phenomenon was witnessed on the 21st; having

been informed that for some weeks prior to date rain had been falling daily after 3 p.m. on a particular spot near two trees, corner of 9th and D streets, I visited the place and saw precipitation in the form of rain drops at 4.47 and 4.55 p.m., while the sun was shining brightly. On the 22d I again visited the place and from 4.05 to 4.25 p.m. a light shower of rain fell from a cloudless sky. The two trees referred to are red oaks, with limbs trimmed to within a few feet of the trunks, and are surrounded by many more of the same kind; they stand four or five feet apart and one hundred yards from the summit of a knoll at the base of which is a small ravine. Sometimes the precipitation falls over an area of half an acre but always appears to centre at these two trees and, when lightest, occurs there only.

• Electrical Plants?

AN ELECTRIC PLANT

Anonymous; *Scientific American,* 37:35, 1877.

In a recent number of the Hamburger Garten-und Blumenzeitung, Levy describes a plant, which, if the statements of this traveler are true, must be a most remarkable wonder. It is one of the phytolacca which seems to be new, and has received the name of phytolacca electrica. The curious fact about this plant is its strongly marked electro-magnetic properties. On breaking off a twig a sensation is produced in the hand like that given by a Ruhmkorff induction coil. This sensation was so marked that he began to experiment with a small compass. The compass began to be affected by it at a distance of seven or eight paces. The needle vibrated on approaching nearer to it, and finally began to revolve rapidly. On receding, the phenomena were repeated in reversed order. In the soil where this plant grew, there was not a trace of iron or other magnetic metal, like nickel or cobalt, and there is no doubt that the plant itself possesses these peculiar properties. The strength of the phenomena varied with the time of day. During the night it is almost nothing, and reaches its maximum about two o'clock in the afternoon. When the weather is stormy the energy increases still more, and when it rains the plant appears withered. Levy also states that he never saw any insects or birds on or about this electrical plant.

• Long-Lived Seeds

LONGEVITY OF NELUMBO FRUITS

Anonymous; *Nature,* 168:905, 1951.

Considerable interest was aroused in Great Britain in 1933 by the flowering at the Royal Botanic Gardens, Kew, of plants of the sacred lotus (Nelumbo nucifera Gaertner, syn. Nelumbium speciosum Willd.) raised from fruits taken there by a Japanese botanist, Ichiro Ohga, who had found them in the peat of a long-dried-up Manchurian lake (Bot. Mag. Tokyo, 37, 87: 1923).

A like interest has been aroused this year in the United States by the germination at Washington in March 1951 of Nelumbo fruits having the same origin. These fruits came from a peat layer overlaid by several feet of loess on which trees grew; hence Ohga concluded that they must be at least 300-400 years old. Nelumbo fruits are nut-like and commonly regarded as seeds, the apparent fruit being a large obconical receptacle; they have a very hard coat impervious to air and water, and will not germinate until this coat has been broken or softened, for example, by filing or by the use of concentrated sulphuric acid. Fruits known to be 150 years old were germinated by Robert Brown; others known to be fifty-six years old were germinated by Becquerel (see Kew Bulletin, 263; 1933). An interesting paper on "How old are the Manchurian Lotus Seeds?" by Prof. Ralph W. Chaney, Department of Paleontology, University of California, indicates that these Manchurian fruits may have retained their vitality over a period much exceeding four hundred years, possibly for thousands of years.

The sequence of events, as outlined by Chaney in The Garden Journal of the New York Botanical Garden, 1, No. 5, 137 (September 1951) and by Ohga, seems to have been as follows. On the floor of a lake, about a mile across, in the Pulentien Basin of southern Manchuria, there accumulated a thick layer of plant fragments, among them the durable fruits of the lotus, a species which grown in shallow water. This process may have taken several centuries. Later the lake drained, and dust from the Gobi desert covered the peat with several feet of loess. The Japanese geologist Endo believes this loess deposit to be 50,000 years old; if so, these fruits may have begun their long sleep at a time when Peking man (Pithecanthropus pekinensis) roamed eastern Asia.

To botanists accustomed to the short period during which most seeds and fruits retain their vitality, Ohga's estimate of a longevity of four hundred years was startling; Endo's estimate of 50,000 years accordingly seems fantastic, although a form of packing which protected the embryo and its food-reserves against molecular change so effectively for four hundred years might well

protect them indefinitely. There can be no doubt that these Manchurian lotus fruits are very old. The species nowadays does not grow wild within a thousand miles of the Pulantien lake, and these Pulantien fruits differ in size and shape from those of the modern Indian form of the species with which they have been compared. A preliminary investigation by Dr. W.F. Libby, University of Chicago, of the residual carbon-14 isotope of a few fruits available in the United States puts their age at 1,040 years (±210) according to a note in Science, 114, 296 (September 1951). Further investigation is planned.

The retention of viability by Nelumbo fruits for a thousand years or so is in its way as great a botanical sensation as the discovery of Metasequoia glyptostroboides a few years ago.

PERMANENCE OF VITAL POWER

Anonymous; *Scientific American,* 32:72, 1875.

In clearing away the refuse from the ancient silver mines of Laurium, in Greece, a large number of seeds of a papaveracea of the glaucium genus were found, which must have been buried there for at least fifteen hundred years. Exposed to the beneficient influence of the sun's rays, they rapidly took root, flourished, budded, and blossomed, their yellow corollas being beautiful in the extreme. This interesting flower, unknown to modern science, is particularly and frequently described in the writings of Pliny and Dioscorides, and is thus again resuscitated, after having disappeared from the surface of the globe for more than fifteen centuries.

THE MUMMY WHEAT CONTROVERSY

Anonymous; *Nature,* 127:675, 1931.

For many years now, a popular belief has existed that seeds which have been removed from ancient tombs retain their ability to germinate. Wheat grain, the so-called 'mummy wheat', has been a case in point for several decades. This question was brought forward again during the discovery and examination of the tomb of Tutankhamen in 1923, by Mr. Howard Carter and Lord Carnarvon, and has received attention from various quarters since that date. Now another claim has been made by an American farmer, that wheat taken from the tomb of Tutankhamen has been made to grow, and this fact has received much publicity in the press. It is all a question of viability. The viability of a seed depends on several factors, both internal and external.

Some seeds will not germinate immediately, and are said to be dormant, such dormancy again being conditioned by after-ripening processes, etc. The result is that seed viability varies considerably within the plant kingdom. For example, the acorn is viable for one season only, whereas charlock will last for twenty or thirty years. Hawthorn, even given germinating conditions, remains dormant for the first season; but immature wheat will germinate, given the necessary conditions, as seen in the case of wheat germinating when still in the ear, during a wet season. On the other hand, mature wheat is viable for some considerable time. Not only that, the grain can withstand extreme conditions to an exceptional degree.

Other plants show a similar tendency. The subject was discussed in an article by the late W. Botting Hemsley in Nature of May 2, 1895, p. 7, exactly thirty-six years ago. He states there that kidney bean seeds, which had remained in the herbarium at Tournefort for a hundred years, germinated; and Mimosa pudica will remain viable for sixty years. But the viability of wheat thousands of years old is a different matter. Sir E.A. Wallis Budge states in the Times of April 23 that grain from a tomb of date 1200 B.C. was tested for him by the late Sir William Thiselton-Dyer at Kew, and gave negative results. Many others, too, have tried since, with similar results. Yet, such positive results as claimed by some, need explaining. The question is: Were such claimants sure of their wheat? For hundreds of years, the halls of tombs have been used as granaries by the natives. The grain can conceivably be ascribed to that, and therefore possibly be only a few years old. Also, 'mummy wheat' has become so popular that guides have resorted to tricks whereby they dig up 'mummy wheat' (in the presence of the tourist) which the guides themselves had placed there some time before. So far, there has not been one authentic case of 'mummy wheat' being viable, and it is extremely unlikely that there ever will be. A viable seed is still living and therefore respiring, however slowly. Decay is therefore taking place, since there is no anabolism. Such decay varies in rate; but it is not likely that it is so slow as to last over thousands of years.

MUMMY WHEAT

Anonymous; *Nature*, 134:730, 1934.

Popular belief in the viability of wheat grains which have been interred in ancient tombs, sometimes thousands of years old, has during the past few years been severely shaken by morphological and physiological tests on genuine mummy wheat, and also by bringing into question the authenticity of other so-called specimens. But in many people's minds, the possiblity of mummy wheat being viable seems still to exist. A survey of this subject was given in Nature of May 2, 1931, p. 675, where genuine

mummy wheat and the more questionable cases were discussed. In Nature of August 19, 1933, p. 271, an example of some so-called mummy wheat from an Indian tomb was shown to be actually a recent one, the whole idea having been based, at the best, on a misunderstanding. The possiblity of the inordinate longevity of some seeds clearly never fails to appeal to the imagination. An article reviewing work on this subject appeared in Nature of September 23, 1933, p. 469. On September 6 last, Sir E.A. Wallis Budge offered, through the medium of the Times, to supply samples of wheat obtained from a nineteenth dynasty tomb in Western Thebes, to responsible institutions in order that the germinating capacity of these seeds could be tested. Although the results of all such tests have not been announced so far, attention should be directed to a report by Mr. W.H. Parker, director of the National Institute of Agricultural Botany, Cambridge, which appeared in the Times of October 29. After subjecting the seeds to strictly controlled germination tests, every grain had completely decayed within sixteen days, and had become attacked by a growth of mould. Morphological examination of the embryos before the tests had also indicated that the sample was incapable of germination.

MUMMY PEAS

P., J.; *English Mechanic,* 82:43, 1905.

Some months since references appeared in "Ours" to the subject of the vitality of mummy peas, doubt being expressed as to the successful raising of the resultant plant. I thought probably for the edification of readers you would like to hear some evidence on the point.

I have in my possession a pod containing three peas as yet unopened, which is part of the crop resulting from the successful germination of peas taken from a mummy which was unwrapped at the British Museum. The Rev. F. Pigou, Dean of Bristol, grew them. He says the bloom is somewhat different to the Engligh pea, and that there cannot be two opinions on the subject. The mummy was probably embalmed 4,000 years ago.

ARCTIC LUPINS BLOOM AFTER 10 000 YEARS

Black, Michael; *New Scientist,* 36:148–149, 1967.

In plants, seeds of many species exhibit the ability to remain alive (viability), often under adverse conditions, for varied lengths of time. The botanical literature is studded with examples, but now it seems that a new record has been established.

In last week's issue of the journal Science (Vol. 158, p. 113), three Canadians--A.E. Porsild and C.R. Harington of the National Museum of Canada and G.A. Mulligan of the Canadian Department of Agriculture, Ottawa--relate the discovery and examination of a sample of seeds of the arctic lupin, Lupinus arcticus, found in the Yukon Territory. Although at least 10 000 years old, some of the seeds have been successfully germinated and grown in the laboratory.

During mining operations at Miller Creek, during July 1954, a mining engineer, Mr. Harold Schmidt, discovered a system of burrows, between 3 and 6 metres below the surface of the silt, which itself was 8 to 12 metres thick, were excellently preserved. They contained skulls and skeletons of the rodents, later identified as the collared lemming, and a number of seeds, probably originally set down as a food store.

The seeds attracted little attention but were kept for 12 years, fortunately under dry conditions, before being handed over to a staff member of the National Museum of Canada when he visited the area. On reaching the authors of the Science article, the two dozen seeds in the sample were readily identified as being those of the arctic lupin. When tested, 6 of the seeds germinated after 48 hours on wet filter paper and have produced normal plants. One of the plants has even developed flowers.

. . . . . .

CURIOSITIES OF BEHAVIOR

• Compass Plants

THE POLARITY OF THE COMPASS PLANT

Whitney, W. F.; *American Naturalist,* 5:1-3, 1871.

The first mention of the so-called "polarity" of the Compass Plant, Silphium laciniatum, was made in communications addressed to the National Institute, by General Benj. Alvord, then Brevet Major, U.S.A., in August, 1842, and January, 1843; although the fact was well known to many hunters and others, as sus-

sequent letters have shown. The truth of his statement having been doubted, General Alvord presented another communication at the second meeting of the American Association for the Advancement of Science, held at Cambridge, August, 1849, in which he confirms his own observations by those of other officers, all agreeing in the conclusion that the radical leaves of the plant really present their edges north and south, while their faces are turned east and west, the leaves on the developed stems of the flowering plant, however, taking rather an intermediate position between their normal or symmetrical arrangement on the stem and their peculiar meridional position.

General Alvord's first conjecture, that the leaves might have taken up so much iron as to become magnetic, having been negatived by analysis, he suggested that the resinous matter, of which the plant was full, and from which it was sometimes called "Rosin Weed," might have some agency in producing electrical currents.

As to its geographical distribution, he stated that it extended from Texas on the south to Iowa on the north, and from Southern Michigan on the east to three or four hundred miles west of Missouri and Arkansas; its chief habitat being rich prairie land.

At the same meeting, Dr. Gray stated that "there were plants then growing in the Botanic Gardens here, and these did not present the edges of their leaves north and south, or in one plane more than another." He thought "that the hypothesis of electrical currents was hardly probable, as rosin was a non-conductor of electricity; but that it was due to the fact that the leaves were inclined to be vertical, and the direction of their edges north and south was the one in which their faces would obtain an equal amount of sunlight."

The statement of General Alvord was confirmed by the Rev. Mr. Morris, "who had observed the fact while running lines for surveys on the prairies."

At the nineteenth meeting of the American Association for the Advancement of Science, Rev. Dr. Hill presented a paper on "The Compass Plant," in which he gives additional evidence for the truth of General Alvord's statement.

In November, 1870, Dr. Gray received a letter from Mr. Charles E. Bessey, of the Iowa State Agricultural School, in which he says: "we have the curious 'Compass Plant,' S. laciniatum, growing in great abundance throughout all this region. The polarity of its leaves is very marked. Use is made of it by the settlers when lost on the prairies in dark nights. By feeling the direction of the leaves they easily get their bearings."

From the record of these observers there can be little doubt that the leaves on the prairies do assume a meridional bearing; and the cause assigned for this by Dr. Gray is undoubtedly the correct one, viz.: that both sides of the leaf are equally sensitive to light. It only remains to be shown what renders its two sides thus equally sensitive. It is well known that the two sides of a leaf usually differ in structure, that the number of stomata, or breathing-holes, is much greater on the under than the upper surface; and that the tissue of the upper is denser

than that of the lower stratum. As the two surfaces of the leaf of S. laciniatum appeared somewhat alike, Dr. Gray suggested that it would be well to examine the leaf microscopically in order to see if it corresponded with ordinary leaves in the above respects, or with truly vertical leaves, the two surfaces of which are usually similar or nearly so; also to compare with it the leaves of other species of Silphium, in which no tendency to assume a north and south position is shown. The species observed were the S. laciniatum, or Compass Plant in question, S. perfoliatum, S. compositum and S. terebinthinaceum; the magnifying power used was about four hundred diameters; and the results obtained may be tabulated as follows:

NAME OF SPECIES.	Average number of stomata in the field of the microscope at one time, on the	
	UPPER SURFACE.	UNDER SURFACE.
S. laciniatum, ...	20	20
S. perfoliatum, ...	10	30
S. compositum, ...	3	9
S. terebinthinaceum, . . .	10	20

The cellular structure of the leaf of S. laciniatum, on making a traverse section, appeared to be homogeneous throughout; but, in the herbarium-specimens this could not be determined with certainty. For this, and for more extensive comparison of the stomata of the two surfaces, further examinations should be made in summer upon the fresh plant.

But the observations here recorded appear to show that the meridional position of the edges of the leaf is to be explained by the structure of the two surfaces, which being identical, at least in the important respect of the number of the stomata, seek an equal exposure to the light;--the mean position of equal exposure, in northern latitudes, being that in which the edges are presented north and south, the latter to the maximum, the former to the minimum of illumination.

Note.--In Longfellow's reference to this plant, in "Evangeline":--

"Look at this delicate plant, that lifts its head from the meadow,
See how its leaves all point to the north, as true as the magnet.
It is the compass-plant that the finger of God has suspended

Here on its fragile stalk, to direct the traveller's
journey
Over the sea-like pathless limitless waste of the
desert,"--

it is curious to see how he has misapprehended the character and aspect of this coarse and stout plant.

• Do Plants Possess Unrecognized Senses?

DOES WHEAT HAVE EARS?

Anonymous; *New Scientist*, 46:321, 1970.

From India a few years ago came reports on the effect of playing a daily half hour of music to young growing rice plants. Although increased yields were mentioned, few people took the experiments seriously. Meanwhile, however, work on the effect of ultrasonic frequency stimulation of the germination and growth of such plants as barley and pine has progressed, and has been correlated in all cases with the increased activity of various enzymes such as alpha-amylase and peroxidase. And now the possibility raised by the Indian work--that ordinary sound, of an audible and not ultrasonic frequency, can stimulate the growth of plants--has received some support from a recent paper in the Canadian Journal of Botany (vol. 48, p.659).

M. Measures and P. Weinberger, working in the Department of Biology at Ottawa, have tested the effect of four different sound frequencies on the growth of two strains of wheat, a Rideau winter wheat and a Marquis spring wheat. An important technique in wheat farming, employed to accelerate the onset of flowering, is to subject the damp seeds to a period of time at about 2°C, a process called vernalization. While their seeds were undergoing this treatment in refrigerated growth cabinets, Measures and Weinberger exposed them to continuous sound of various frequencies (300 Hz, 1250 Hz, 5 kHz and 12 kHz) under carefully controlled conditions.

At the end of four weeks the seeds were germinated and the seedlings planted out. After eight weeks' growth, the Canadian workers measured various aspects of the plant. These included its height, the number of leaves, and the fresh and dry weight of the root and shoot systems. The results were spectacular. Those plants subjected to the 5 kHz sound were considerably larger in all the parameters measured, often being over twice the size of untreated controls. The Marquis wheat root system,

for example, was 250 per cent heavier than the control. Similar results were obtained with the Rideau wheat. The 300 Hz sound gave less spectacular results, while the other two frequencies tried had little or no effect.

Measures and Weinberger are unable to come up with a convincing explanation for their results. Whether they prove to be of practical importance in the near future remains to be seen.

PLANT "PRIMARY PERCEPTION": ELECTROPHYSIOLOGICAL UNRESPONSIVENESS TO BRINE SHRIMP KILLING

Horowitz, Kenneth A.; *Science,* 189:478–480, 1975.

Abstract. A test of the "primary perception" hypothesis proposed by Backster in 1968 was made by recording electrical activity from the leaves of Philodendra scandentia while randomly ejecting the contents of micropipettes filled with brine shrimp or distilled water into boiling water. Test conditions conformed to those published by Backster or communicated in personal exchanges. Data were analysed from five experiments, in each of which recordings were made from four plants in the presence of three brine shrimp killings and two control water ejections. Inspection of the data and analysis by two statistical methods revealed no relationship between brine shrimp killing and electrical "responsiveness" of philodendron.

• Plants Sensitive to Touch

THE FACULTIES OF PLANTS

MacDougall, T. D.; *English Mechanic,* 88:206–207, 1908.

A large number of species of plants has become sensitive to the touch or blow of a solid object in a manner broadly analogous to the touch reactions of animals. One form of this reaction is exhibited by plants which climb by the aid of tendrils. Tendrils are generally long slender organs sensitive on one surface only, although in some species the percipient cells cover the entire surface. When one of these organs comes into contact with a solid object, the outer sensitive cells are stimulated, and communicate an impulse to cells not far distant, and curvature en-

sues within a second, or a few seconds at most, which generally results in curling the organ around the object. Singularly enough, these organs distinguish between a touch and a blow. The rudest shock or jar does not set the tendril in action, so long as the sensitive cells do not receive a pressure of some continuity, but the most delicate contact of the smallest object will cause stimulation. Thus, a bit of spider's thread, or the finest silk-fibre, weighing no more than the fiftieth of a grain, will serve to excite curvature. Water, or even as heavy a liquid as mercury, will not cause curvature when poured over a tendril; but if minute particles of chalk are suspended in the water the repeated contact of these bodies will set up a reaction. This is, in fact, an appreciation of the difference between pure and muddy water, which is probably beyond the capacity of any organ of touch of the human body. After a tendril has grasped a support by means of the above mechanism, the free portion of the organ is thrown into a corkscrew which has the effect of pulling up the stem and anchoring it by an elastic spring.

The "sensitive plant" (Mimosa), a small decumbent shrub native to the Tropics, offers a striking illustration of another form of sensitiveness to mechanical stimuli, by which shocks and blows, but not contact, are appreciated. The base of the leaf-stalk is attached to the stem by a highly-developed pulvinus, or motor-organ to act, and the leaf is quickly dropped through an arc of 90 deg. If a stronger stimulus is given, an impression is conveyed up and down the stem to other leaves, and the effect of a single snip of the scissors on a leaflet may be transmitted through a stem a yard in length at a rate of third of an inch per second, in a manner highly reminiscent of the action of nerves. (p. 207)

• The Coughing Plant

THE COUGHING PLANT

Anonymous; *English Mechanic*, 72:296, 1900.

The following story about a "plant that coughs" has just that little amount of truth which makes it interesting. "By accidental transportation of its seeds the vine called the coughing bean (Eutada tussiens), a native of moist Tropical regions, has gradually spread to such less congenial spots, especially railway embankments, where it endures drought very well, though its growth is stunted. But there is one thing which it cannot stand, and that is dust. When the breathing pores become choked by

dust, the gases accumulate within the leaf for a time, and then are forcibly expelled in an audible paroxysm of coughing and sneezing which makes the leaf tremble violently. At the same time, the whole plant becomes red in the face, so to speak, through the sinking in of the green chlorophyll grains and the appearance of particles of red colouring matter on the surface. The Eutada is sometimes cultivated as a house plant. Sweeping the room is very aprt to set the poor plant a-coughing, to the intense astonishment of persons who are unfamiliar with its peculiarities." There is a genus known as Eutassa, and there is also a genus known as Entada, to which the above remarks no doubt refer. Entada is really a "bean," and the pods of E. scandens often reach the length of 6 ft. to 8 ft., and are used for various purposes by the natives of the regions in which they grow.

• Synchronous Flowering of Bamboos

THE VIRTUES OF THE BAMBOOS

Simmonds, N. W.; *New Scientist*, 19:334-338, 1963.

Among the most interesting features of the bamboos are their flowering habits. Starting from a young seedling, the culms are at first small and short-lived and then, as the rootstock system builds up, the culms become progressively larger and more numerous until the mature stool is attained: this may take 10-12 years for the big clumped species. Thereafter the stool remains more or less constant in size, new culms being produced as fast as old ones die or are cut out. The stool may be maintained thus for another 20 or 30 years without flowering; then, one year it will flower, fruit and die. Furthermore, sister seedlings tend to flower and die at the same time and, even more remarkably, vegetatively propagated plants also show synchronous flowering. Clearly, there must be some sort of built-in clock (perhaps calendar would be a better word) transmitted both to sexual and vegetative progeny. The same sort of pattern, common in the bamboos, is also known in a palm, the Tailpot (Corypha) of South India, but in relatively few other plants.

Not all the bamboos show this synchronous habit, though most of the big clumped species do. Some flower only on single culms which then die; others flower and fruit sporadically over many years. And even the markedly synchronous flowerers sometimes bear a few flowers out of season, while, contrariwise, the sporadic flowerers sometimes perform in great bursts. (p.337)

SYNCHRONICITY OF FLOWERING IN BAMBOOS

Gould, Stephen Jay; *Ever Since Darwin*, W. W. Norton & Co., New York, 1977, p. 98.

The flowering of any species must be set by an internal, genetic clock, not imposed from without by some environmental clue. The unerring regularity of repetition supplies our best evidence for this assertion, for we do not know any environmental factor that cycles so predictably to yield the variety of clocks followed by more than a hundred species. Secondly, as mentioned above, plants of the same species flower simultaneously, even when transplanted half a world away from their native habitat. Finally, plants of the same species flower together, even if they have grown in very different environments. Janzen recounts the tale of a Burmese bamboo only half a foot high that had been burned down repeatedly by jungle fires, but flowered at the same time as its unhurt companions standing 40 feet tall.

How can a bamboo count the passing years? Janzen argues that it cannot be measuring stored food reserves because starved dwarfs flower at the same time as healthy giants. He speculates that the calendar "must be the annual or daily accumulation or degradation of a temperature-insensitive photosensitive chemical." He finds no basis for guessing whether the cycles of light are diurnal (day-night) or yearly (seasonal). As circumstantial evidence for implicating light as a clock, Janzen points out that no accurately cycling bamboo grows within 5 degrees of latitude from the equator--for variations in both days and seasons are minimized within this zone. (p.98)

Unusual Plant Rhythms

THE RHYTHMIC NATURE OF ANIMALS AND PLANTS

Brown, Frank A., Jr.; *American Scientist,* 47:147-168, 1959.

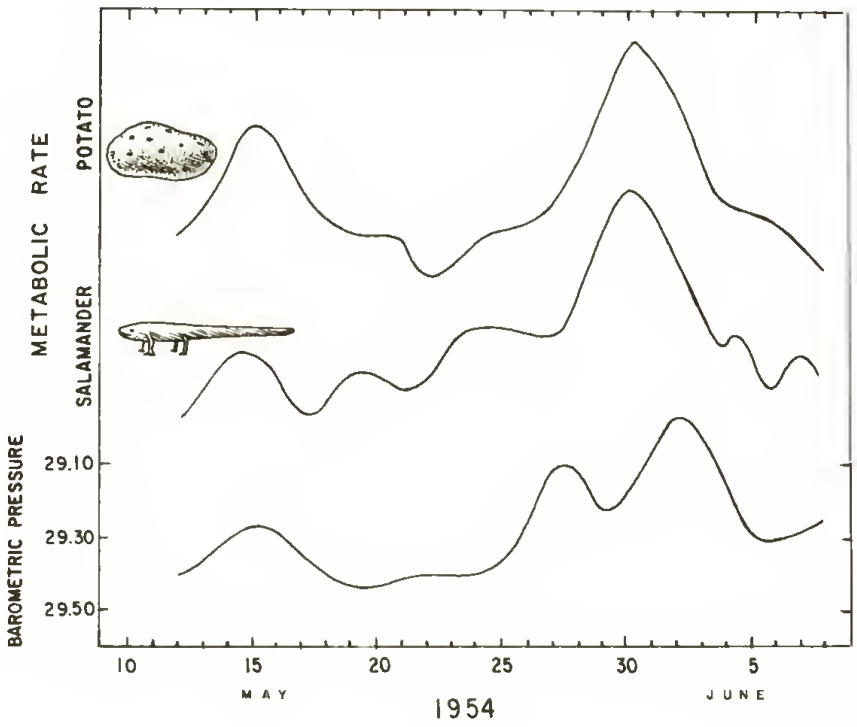
It is well known that there are solar and lunar tides of the atmosphere. These are reflected in rhythms of barometric-pressure, on the average, always rises during the early morning hours to a high about 10 o'clock and then falls to the low point of the day, during the middle to late afternoon, the time of the low point depending on the time of year. This is a precise average 24-hour rhythm. The potato has similarly a precise average 24-hour cycle of metabolism with a minimum rate at midnight and a

maximum rate at 6 in the afternoon. This average daily cycle can be shown to include the average of two kinds of daily oscillations. Both the barometric pressure cycles and the potato cycles exhibit irregularities from day to day. The barometric pressure cycles are distorted by large changes due to still unknown forces, which are associated with weather changes. The important fact for us here is that the potato is informed of how fast the pressure is rising in the morning and falling the the afternoon. This we know first because how high, or how low, the potato metabolism is at 6 in the morning of any given day is simply related to how fast the barometric pressure was changing from 2 to 6 the preceding morning. And, secondly, how high the rate of metabolism in the potato is at 6 in the evening of any given day is related to how fast the barometric pressure was changing from 2 to 6 the preceeding afternoon.

Hence, the potato, in the form of its daily metabolic fluctuation, even in constant conditions of pressure, through being hermetically sealed in rigid containers, is, in effect, informing us through its metabolic changes what the weather distortions were in the regular pressure cycle of yesterday. And, whatever the means by which this information reaches the potato, the same information is providing the potato, figuratively speaking, with information as to what the weather-associated, barometric pressure will be the day after tomorrow, by the height of the afternoon peak of metabolic rate. This is tending to trace out the form of the barometric pressure changes but doing so two days in advance. Notice how both kinds of living things generally vary inversely with the barometric pressure change (bottom curve), but both tend to anticipate the pressure changes by an average of two days. In fact, every living thing studied in our laboratory during the past 2 years---from carrots to sea-weed and from crabs and oysters to rats---has shown this capacity to predict very safely beyond chance the barometric pressure changes usually two days in advance. It is interesting to contemplate the problem of a meteorologist sealed incommunicado, for weeks or months in constant conditions and asked to give 2-day weather predictions---or, for that matter even to tell you the weather today.

The potatoes also indicate to us that they are, while sealed in constant conditions, obtaining information about another well-known environmental daily rhythm, namely that of high-energy background radiation. This is so penetrating that it pervades all ordinary buildings and containers. This radiation is highest about 6 in the morning and lowest between noon and 6 P.M. The daily range, or cycle amplitude, in the radiation, though averaging about 2 per cent, varies greatly and unpredictably from day to day. The potato cycles also show variable total range, or amplitudes, from day to day. But, the amplitude of the potato cycles on a given day is very clearly related to the amplitude of the background radiation cycle the day before. The greater the fluctuation in radiation yesterday, the greater the fluctuation in metabolism today.

The potato sealed in constant conditions also obtains informa-



Day-to-day changes in potato and salamander metabolism compared with barometric changes.

tion as to the outdoor air temperature. As clearly seen in Figure 6 (omitted), the higher the outdoor temperature up to about 57°F., the higher the amplitude of the daily metabolic fluctuations. This relationship is reversed for higher temperatures. As everyone knows, there are clear daily and annual rhythms in air temperature. (p. 162-164)

A QUEER MAPLE TREE

Manting, M. G.; *Science*, 14:257, 1889.

A hard-maple tree in the yard of S.G. Scott at Plainwell, Mich., is an object of great curiosity. It has been shedding its foliage through September, but new leaves are again appearing, and after the fall frosts the tree again drops its leaves. This it has done regularly for several seasons. It differs only in respect of shedding its foliage twice a year, from other maples standing within a few feet of it.

WINTERING IN MALAYA

Soper, J. R. P.; *New Scientist*, 41:146, 1969.

Referring to Dr. Michael Black's article on phytochrome ("How light controls plants", 26 December, p.716), there are two tropical phenomena which, so far as I know, have not been adequately explained as yet. I wonder if Dr. Black or any other of your readers could offer any suggestions?

The first is the "wintering" of rubber trees in Malaya. Towards the middle of December, all the trees in the extreme north of the peninsula shed their leaves, and a new flush of growth accompanied by flowering sets in about 10 days later. This process starts somewhat before the shortest day, but it does not take place simultaneously over the whole country: it moves from north to south, and takes roughly six to eight weeks to reach the state of Selangor. Further south there is no regular wintering, and Holtum and Corner have shown that in Singapore individual rubber trees exhibit a 13 month rhythm, quite independent of season. At any given time some trees may be wintering, some flowering and some fruiting. At this latitude (1° N) other perennial species have different rhythms varying from five to 13 months; but further north, the north to south progression can be seen in most species of common fruit trees.

The second phenomenon is even more baffling, and relates to the behaviour of clove trees in the islands of Zanzibar (6°S) and Pemba (5°S). The trees are evergreen, and I cannot now remember when the old leaves are shed. The crop consists of the flower buds harvested just prior to opening, and it matures from south to north in each island, though a small proportion of the trees do not follow this pattern. The first buds are ready to pick in June in Zanzibar, a month later in Pemba. Harvesting continues over a period of some six months, moving slowly from south to north. An experienced eye can detect differences between leaf and flower buds from January onwards, so the inital formation of flowers seems to start at the time of the longest days.

Both these phenomena appear to be connected with the movement of the sun, but the manner in which they are caused remains a mystery.

• Plant Behavior and Astronomy

INFLUENCE OF THE MOON ON TREES

Eos; *English Mechanic,* 13:234, 1871.

As far as I have been able to make observations in the matter, I can only state that I lived for some years near forests of Giganta Bambusa, attaining such dimensions in that humid climate of the Assamese frontier as to furnish milk buckets entire (between the knots) and all manner of domestic utensils requiring hollow cylinders of size. I was always requiring large supplies of bamboos for building, fencing, mat-making, &c., and the people of the country used to insist on my cutting them on the dark nights only, as the rise and fall of the sap keeps pace with the moon's age, and that, as a consequence, such bamboos, when charged with sweet sap, ferment, decay, attract weevils and borers, or go off into "dry rot." This I know, that I have seen many roofs, and enclosures rapidly dropping into yellow dust, while my own premises escaped; and the bamboos all came from the same forest. Many other kinds of vegetation are said by the natives to be similarly affected, as the plantain, date palm, &c.; and some of the tribes believe in lunar influence on garden crops, and are particular about the moon's age in sowing peas, &c. &c.

MOON MADNESS

Burr, H. S.; *Yale Journal of Biology and Medicine,* 19:249-256, 1944.

It has been said that man does not live by bread alone nor unto himself alone. This, being interpreted, means of course that man is not a separate unit untouched by his immediate environment of other people and things, nor unaffected by the tides of the Universe. Rather it emphasizes the fact that man is completely dependent on universal law and order. He, like all other inhabitants of the Universe, can never defy universal law but must always be directed by it. This is the reason why it is so important to know the order of the Universe. We know something about it, but our knowledge is tragically small. The field is so great that it is hard to know where to begin. This being true, the astute procedure is to begin with that which lies immediately in front of one, attacking the simple and more or less obvious problems with, however, what perhaps might be called a universal viewpoint. The report which follows is a first attempt to examine

the impact of many environmental forces, both local and universal, on the continued existence of a single living form---a maple tree. The impact of these environments has been recorded, using electrometric technic continuously, day and night. The results of the first three months are presented and others will appear as the experiment continues. The method employed, briefly, is as follows:

Two non-polarizable and reversible silver-silver chloride electrodes were introduced between the bark and cambium on the trunk of a young maple tree. The electrodes were approximately 5 feet apart; the lower electrode being 6 inches from the ground. Leads from these two electrodes ran to a microvoltmeter, the output of which was fed into a General Electric photoelectric recorder. This apparatus made it possible to record continuously the changing potential difference between the two electrodes. Temperature, barometric pressure, relative humidity, the weather and, finally, the phases of the moon were recorded. A variety of electrode placements were used without, however, changing in any significant fashion the recorded potential difference. Every two or three days the records were removed from the recorder and every hour, on the hour, the height of the standing potential determined. This was possible because the rate of change within the hour was slow. Normally the upper electrode---5 feet, 6 inches from the ground---was markedly positive to the lower electrode although under certain conditions this relationship might be reversed. Since the paper rate was 6 inches per hour, graphs were constructed from the hourly potentials. Two 3-day samples of this graph are shown in Figures 1 and 2. In the first graph, three days were selected from the August recordings. In the second graph a similar three days from the month of November are shown. The August recordings were taken from a tree in the town of Lyme, Connecticut, during the summer of 1943. The November records were taken from a tree in New Haven, Connecticut, in the fall of the same year. The similarity between these two graphs is striking; the more so since the entire setup in New Haven was different from that in Lyme. Not only were the trees different but the microvoltmeter and the photoelectric recorder were different. The type of electrode employed was fundamentally the same but had certain technical modifications. Attention is called to this because it indicates quite clearly that the over-all results were not due to instrumental or technical artifacts. The records show a very characteristic diurnal rhythm to be found, however, only in selected cases, since other factors frequently obscured the pattern. In the early morning from two to four hours before sunrise, the potential difference was at a maximum. From that time the potential dropped without interruption until the afternoon. In the August records the hour of the lowest point reached varied rather widely from noon until eight in the evening. In the November records the low point for three successive days was at 4 o'clock in the afternoon. This is an hour and twenty minutes before sunset. During August (19, 20, and 21) the sun at approximately 7:35. It is clear that in the November records there might be some relationship between the time of

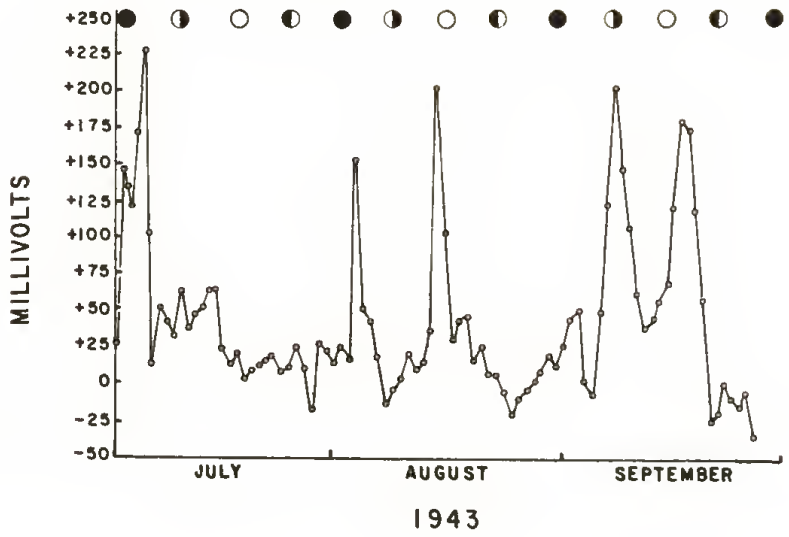
sunset and the low point in the potential. This is not borne out in the August records which show that shortly after sunset the potential was rising, having reached the low point at some variable hour just preceding. Diurnal rhythms in plants are quite well known and have been extensively studied. The electrical records show nothing unusual except that the same kind of rhythm can be determined by electrical methods as is found by other observational technics. In two of the August days the peak of maximum potential difference occurred at 4 o'clock in the morning. The sunrise of these two days was at 5:55 and 5:56 respectively. In other words, the peak was approximately two hours before sunrise. In two of the November records (19 and 20) the peaks occurred around 6 o'clock in the morning, on which days the sun rose at 7:39 and 7:40, a little more than an hour and a half after the peak. Without going into the causes of this rhythm, it looks as though the tree were storing electrical energy, beginning with sunset, with the maximum amount stored appearing in the early morning. These results suggest that electrical recording of biological activity is an advantageous technic, since it involves no disturbance of normal processes.

During the day the normal physiological activity of the tree---photosynthesis, transpiration, etc.---steadily withdraws energy from this reservoir until late in the afternoon when the reservoir is empty. It should be pointed out, of course, that the size and magnitude of the potential difference is in part determined by the geometry of the electrode placement and that the records here given apply only to the given situation.

Further study of the continuous recordings displayed another astonishing fact shown in Figure 3. This is a graph of the average potential difference between the two electrodes for each 24-hour period from July 1st to September 30th. It will be noted at once that there is a curious rhythm, inasmuch as there occurs approximately every thirty days a tremendous and very sharp rise in the potential difference.

Ten days later a second rise occurs of the same order of magnitude. In between these peaks of potential difference there are short periods during which the potential difference is very low. Plotting the results in terms of daily averages obscures the fact that during the maximum potential difference there is an extraordinary and pronounced change in the character of the recorded potential difference. Between the peaks of potential difference the recorded standing potential is either very constant or very slowly changing, giving rise in most instances to an almost straight line. When, however, the potential rises to the maximum the steady state character of the potential disappears and is replaced by slow oscillations of potential, rising and falling, 30 or 40 millivolts, every minute or two. These bursts of activity persist for approximately an hour and then disappear, not to reappear until another sharp rise is developing days later.

When the temperature, humidity, barometer, and weather are plotted against these changing potential differences, it becomes clear at once that none of these environmental phenomena are related in any significant way to the electrical manifestations.



Average potential difference between electrodes inserted in trees. (Fig. 3)

Since this is a pilot experiment, continuous records of environmental change were not taken. Reliance was placed on spot determinations. It may well be that a more critical examination of the factors involved might yield significant relationships. It is clear, however, from the data collected that ordinary environmental forces have little or no impact upon the electrical activity of the tree, except the possible correlation between diurnal rhythms and sunlight.

The periodicity of the peaks of the potential difference described above is an astonishing finding. From a number of previous studies, it is known that sharp increases in potential difference often accompany increased growth rates. While the tree was probably growing during the summer months, it certainly would be an unexpected phenomenon if the growth turned out to be intermittent. However, there is ample evidence also to show that marked changes in potential differences accompany normal biological activity. So far as is known, activity in a growing tree is not of this intermittent variety. In any event, whatever these electrical manifestations are the sign of, the significant finding is the wide range in potential differences and their periodic occurrence. To be sure, in the second peak of July the voltage gradient is not high. However, at this time many experiments were being performed in order to rule out artifacts and to establish the validity of the first peak and it is quite possible that the maximum was missed.

The phases of the moon were included in the graph in Figure 3 for two reasons. In the first place, it is an old wives' tale that

the phases of the moon have profound effect on living organisms. This belief, of course, suffers from lack of scientific validation in most instances; nevertheless, it is widely current. If lunar activity does affect living systems in significant ways, it should be possible to find an electrical correlate. In the second place, there are a few well-authenticated examples of the corelation between lunar activity and living things. The most striking of these is the marine worm, Palolo, which is known to spawn only during the third phase of the moon in October and November in the South Pacific. It is true, of course, that even if a correlation between phases of the moon and biological systems were found, it is more probable that there are inherent protoplasmic rhythms which have the same periodicity as the lunar cycle, rather than a direct cause and effect. It cannot be denied, however, that both lunar cycles and these periodicities in living things may be the consequence of some still more fundamental cosmic factor acting upon both responding systems. It is clear that when the potential differences in the maple tree are plotted against the lunar phases that the correlations are not exact. The first peak occurs at or shortly after the first phase of the new moon. Ten or more days later the second peak occurs in the immediate vicinity of the full moon. On the basis of the evidence so far collected, one could predict by means of electrical measurements the changing lunar phases within forty-eight hours.

The findings here offered in a preliminary report present fairly adequate evidence of the existence of an electrical correlate of a diurnal rhythm in a growing tree. Furthermore, there seems to be no significant relationship between meteorological data and the electrical changes. There is, however, strong evidence for the existence of periodicity of some as yet unexplained character. The peaks of electrical activity seem to be more closely associated with the phases of the moon than with any other discernible factor. It is possible, of course, that cosmic rays, radio storms, and many other unknown factors may be implicated. Further study of this will be necessary, but it seems evident that some environmental factor other than meteorological changes or that some inherent protoplasmic property of the system must be considered. In other words, these studies in their present preliminary state offer another evidence of order in the Universe upon which a relatively simple living system is in some degree dependent. In any event, the experiment is being continued and at the end of one full yearly cycle the complete data will be analyzed and reported.

THE MOON AND PLANT GROWTH

Beeson, C. F. C.; *Nature*, 158:572-573, 1946.

Beliefs that the phases of the moon have a differential effect on the rate of development of plants are both ancient and world-

wide. Proof by rational experiment seems to have been sought more than two hundred and fifty years ago by La Quintinyer, the horticulturist, and some years later by Duhamel du Monceau, the forester. Neither obtained any positive evidence of lunar influence. Since then, scientific interest in the subject has been received intermittently, either by the 'rediscovery' of lunar rites in the agriculture of civilized countries, or by the impact on Europeans of the impressive faith of primitive peoples, particularly in the tropics and sub-tropics.

The literature on the moon and plants can be assigned to two groups: one comprising reiterations of peasant beliefs, myths and rules, both ancient and modern, and similar unsubstantiated statements; the other comprising experiments supported by numerical data capable of statistical analysis. This second group consists of (a) experiments mainly of the anthroposophical school, which demonstrate the existence of lunar effects on the growth of plants; and (b) experiments of professional horticulturists and foresters, which prove that there are no such effects, or that, if they do exist, they have no value in agricultural practice.

The beliefs which dominate primitive rural economy and the emphatic reports of credulous observers are very numerous, but they provide no significant evidence. Only experimental data need be considered; they may be briefly summarized as follows:

(a) Kolisko's work. According to the investigations of L. Kolisko in Stuttgart during 1926-1935, the particular phase of the moon at the time of sowing does influences the period and the percentage of germination, as also the subsequent growth of the plant. The most favourable date to sow is two days before the full moon for leaf- and fruit-bearing garden crops (such as cabbages, peas, tomatoes), for root-crops (such as radishes, beetroots, carrots), for flowering garden annuals, and for wheat, maize, etc. In general, these plants show better germination, more vigorous growth, and greater yields than those sown just before the new moon. Kolisko affirms that the lunar influence is not fully effective unless there is rain or artificial watering during the germination period, but the stimulus once acquired remains decisive throughout the periods of growth, flowering and fruiting. As regards growth during a lunar phase, and found that on the whole the response of wheat is greater during the waxing than the waning phase.

Later experiments to determine the depth to which the action of the moon penetrates the soil, Kolisko found that at a depth of 1 metre the effect on wheat is nearly identical with that at the surface; at 2-3 metres the maximum growth is generally reached in a full-moon period; between 5 metres and 16 metres the influence is weaker but is still shown by greater growth at the time of the Easter full moon. She considers that each year has a certain dominating lunar period, and that the Easter full moon has a special significance for the whole year. E. and L. Kolisko's recent book, "Agriculture of Tomorrow", reviews cosmic influences on plant growth.

(b) Other investigators' work. Germination. Experiments on the germination of garden crops have been done by Becker

(1937-38), Bergdolt and Spanner (1937-39) at Munich; by Mather and Newall (1940-41), at the John Innes Horticultural Institution; similar experiments with spruce seed were done by Rohmeder (1935-37) at Munich. All these investigators agree that no consistent effect of the moon is observable, and that all chance variations possibly assignable to any one of the moon's quarters are evened out with an adequate number of repetitions. At meetings of the Societe Nationale d'horticulture de France in 1924, several horticulturists testified to the absence of lunar effects on sowings and seedling growth; earlier work of Arago, Flammarion and others in France between 1859 and 1909 was cited in confirmation.

Reproduction. Periodicity in the production of sexual cells of the marine alga, Dictyota dichotoma, has been demonstrated by Williams (1905), Hoyt (1907) and Lewis (1910), but the period is fort-nightly on the coast of Wales and Naples, and four-weekly in New Carolina, and the phase dates differ.

Polarized light. Semmens (1923) showed that moonlight is plane-polarized and increases hydrolysis of starch with diastase. Esenbeck and Suessinguth (1930) and Macht (1926) showed that polarized light of low intensity may produce a very slight increase in growth in length of plants. Wright (1927) found the highest degree of polarization of moonlight at the ends of the first and third quarters. The anthroposophists consider that the moon's influence works in darkness and below ground.

Felling dates and seasoning of timber. Moisture-content is the most important physical condition influencing the rate of decay of wood. The amount of water in the wood of a living tree is known from many careful determinations to differ with the species of tree, and for some species to vary seasonally, for others to be fairly constant throughout the year. No variation related to lunar phases is known for any tree, but Beeson and Bhatia (1936) found a regular lunar rhythm of sap increasing from the full to the new moon and decreasing from the new to the full moon in Dendrocalamus strictus in India. Knuchel and Gaumann's (1930) work with spruce and silver fir in Switzerland based on a sequence of fellings in the same phase of the moon is typical of the exact knowledge now available. The season of felling is proved to have no substantial influence on specific gravity, moisture-content, shrinkage, resin-content of working qualities, but it strongly influences the rate of seasoning. The effect of the season of felling on the rate of drying, and the effect of weather on the activity of decay organisms dominates any effect that may be due to the phase of the moon.

Borer damage. Numerous entomological records show that the liability of felled trees to attack (that is, oviposition) by borers depends on one hand on the dates of the emergence period and longevity of the adult insect, and on the other hand on the progress of drying out of bark and sapwood, or the amount of depletion of starch. The two latter factors can be controlled in many species of trees by logging procedure which entirely ignores lunar dates. Beeson and Bhatia (1936) and Gardner (1945) have proved that the intensity of Dinoderus damage to

bamboos in India depends on the amount of starch present in the felled culm; the starch-content of the living culm varies seasonally, not according to the lunar phase, and no advantage is obtained by felling in relation to the phase date.

Lunar periodicity exists in some animals, but they are marine or aquatic species.

Yield of resin, latex, etc. Variations in the yield of resin, latex, maple syrup, gums and tannin are explicable in terms of tapping systems, genetic factors, weather and environmental conditions; experience is very considerable, but in no case has any advantage attributable to the moon been discovered. For example, Ferrand's work (1941) on Hevea brasiliensis in the Belgian Congo revealed that the daily concentration of latex varies with weather conditions, and the local concentration in the same tree depends on the exposure to sunlight of the crown directly above the tapping point. Changes in the gutta-content of the root of Euonymus verrucosus in the U.S.S.R. follow the seasonal development of the plant, and the resin-content varies in inverse proportion (Yurkevich, 1944). The maximum tannin-content of seeds of Terminalia chebula is found in seeds collected in January anywhere in India; this period is also that of optional germinative capacity (Prasad, 1946).

Summary. The only experimental evidence for the existence of lunar influence on the growth of land plants is that published by L. Kolisko. All other investigators in many parts of the world have been unable to discover any consistent correlation between the moon and the vital processes of land plants; some admit that if a lunar effect does exist it is so obscure as to have no value in agricultural practice.

FLOWER BLOOMS BY MOON

Anonymous; *Science News Letter*, 43:141, 1943.

A flower that appears to follow the phases of the moon in its blossom periods, bearing abundant blooms during first and last quarters but standing bare-stemmed or nearly so during new and full moon, is described in the National Horticultural Magazine (January).

The plant is a South African member of the iris family known horticulturally as Morea iridoides; its iris-like flowers are white, marked with yellow and blue. Its peculiar behavior was not noticed by a botanist but by a psychologist, Prof. Knight Dunlap of the University of California at Los Angeles, one of whose hobbies is gardening. He kept several clumps of Morea, growing in rather diverse habitats, under close observation for over a year.

In southern California, the Morea blooms almost all year round, though it produces fewer flowers during the winter months.

"The Moreas bloom normally within two periods in each lunar month," Prof. Dunlap states. "One period commences on the date of the first quarter, and ends the day before the full moon. The other period runs from the date of the last quarter up to the new moon. In the other phases, (new moon and full moon), there are normally no blossoms. In a blooming quarter, the first blossoms may appear on the first day, or on the second or third day. By the last day of the quarter, sometimes a day or two earlier, the last blossom has withered; the petals either dropped off or curled up. In the winter season, even the best plants may pass one or more bloom quarters entirely."

Apparent exceptions, when flowers appeared during the normal non-flowering periods of full and new moon, appeared to be connected in some way with spells of summer "high fog" which considerably reduced the illumination received by the plants.

What it all may signify, Prof. Dunlap is not prepared to argue on the basis of data now in hand. He hopes that other flower growers, both professional and amateur, will take the trouble to make careful observations as opportunity may offer.

"Meanwhile," he adds, "I am satisfied to present evidence confirming the principle familiar to students of mythology, namely: Ancient superstitions often have foundation in fact."

MONTHLY CYCLES IN AN ORGANISM .....

Brown, Frank A., Jr., et al; *National Academy of Science, Proceedings*, 44:290-296, 1958.

Discussion.--The foregoing results demonstrate clearly that during the period of this investigation the potatoes possessed a statistically significant average synodic monthly cycle while in conditions which were constant with respect to all external factors usually considered to influence them. Since for the potato tuber such a monthly cyclicity has no currently conceivable adaptive significance, it seems probable that there are cycles of this frequency in some external physical factor, to which a living thing such as the potato may respond, even while maintained in so-called "constant conditions." In support of this view are the recent reports of lunar monthly cycles of O_2-consumption and spontaneous activity of earthworms and of spontaneous activity of the salamander, Plethodon cinereus, in constant illumination and temperature. In these instances, too, an adaptive significance of cycles of this frequency is unknown. On the other hand, in the shrimp, Anchistioides, in which a synodic monthly cycle of molting occurred even while the animals were maintained inside the laboratory in aquaria, an adaptive character of the cyclicity is evident in its relationship to reproductive synchrony within the species and is associated with the normal lunar monthly swarming rhythm in this shrimp.

There was clear proof presented through discovery of correlations significantly different from zero between the potato metabolism and concurrent aperiodic fluctuations in forms or amplitudes of the daily cycles of both barometric pressure and background radiation that these organisms in the conditions of constant illumination, temperature, humidity, oxygen tension, pressure, etc., were still responding to some pressure- and radiation-correlated external physical variable during those large fractions of the 13-month period of this investigation for which possible correlations were analyzed.

Just as synodic monthly cycles may, of course, be simple derivatives, by periodic reinforcement, of precise persistent organismic solar-day and lunar-day cycles, so does this study provide further support for the reported simultaneous occurrence of persistent, metabolic, solar- and lunar-day periodisms in organisms as diverse as crabs, salamanders, potatoes, seaweed, and carrots.

Inasmuch as one of the ways in which the synodic monthly cycles become evident is through a modulation in form of the daily cycles, it would seem reasonable to postulate on this basis that there is some common denominator in the fundamental mechanisms of maintenance of these two cycles of widely different frequency, which persist in the so-called constant conditions.

The character of the lunar modulations of the daily cycles are quite consistent with the view that the influences of the sun and moon are qualitatively and temporally similar, but with the sun exerting the greater influence. If in the mean solar-day cycle, we interpret the form as a consequence of solar influence on the daily cycle to be one of maximal O_2-consumption about 6 hours later in the day than solar zenith, then the forms of the lunar-modified daily cycles can be interpreted on the basis of a comparable temporal relationship of time of lunar zenith to maximal lunar influence. At new moon, with zenith at solar noon, the maximal lunar influence would be expected near 6 P.M. At first quarter, with zenith near 6 P.M., the maximum influence would be near midnight. At full moon, the maximal lunar influence would be expected to center over 6 A.M. and, at third quarter, to center over the noon hour. These expectations are essentially fulfilled. This qualitative similarity relative to bodies intrinsically as different in most respects ar our sun and moon might suggest that gravitational forces in some manner enter into the chain of forces coupling them with biological activities, possibly through certain geophysical fluctuations associated with the well-known solar and lunar tides of the atmosphere.

In summary, it has been shown that during the 14-month period when this study was being made, there were statistically significant synodic monthly cycles of O_2 consumption, with a skewed form. This was demonstrated for four more or less independent parameters of metabolic change which were found to exhibit among themselves only low correlations in day-to-day changes. In each instance, in the average skewed monthly cycle for this period, the minimum occurred at the time of new moon, and maximum at the time of third quarter. (pp. 295-296)

LEAVES THAT DUCK FROM THE LIGHT OF THE MOON

Anonymous; *New Scientist,* 43:222, 1969.

At night the leaves of many plants take part in a stately dance, bending, dipping, twisting and even--in a few species--folding up. Darwin, writing in 1880, claimed that this leaf movement must have some adaptive significance, and concluded that "the benefit thus derived is the protection of their upper surfaces from radiation into the open sky"--in other words, from chilling. The explanation is scarcely satisfactory, since the plants that are most extreme in their nightly leaf movements are tropical. Now two biologists from Tubingen University, Erwin Bunning and Ilse Moser, have come up with the neat suggestion that the plants are actually protecting themselves against radiation from the sky--to be exact, the light of the Moon (Proceedings of the National Academy of Sciences, vol.62, p.1018).

Bunning and Moser have shown that the intensity of a tropical moon vertically above some plants is more than enough to upset the synchronization of their internal biological clock. Normally, this clock is kept accurate by the cycle of day and night; a full moon, simulating the twilight of dawn, might then reset the clock to the wrong time. The Germans have no direct evidence for their theory, but they have shown--using an "artifical moon"--that the night movement of leaves reduces the illumination falling on them to a level well below that at which photoperiodic effects can occur.

CORRELATIONS BETWEEN LUNAR PHASES AND RHYTHMICITIES IN PLANT GROWTH .....

Abrami, Giovanni; *Canadian Journal of Botany,* 50:2157-2165, 1972.

Abstract. A previous investigation of fluctuations in temperature and changes in growth in seven herbaceous species under field conditions revealed a periodic lack of correlation between the two variants. An original step-by-step periodogram analysis of the curves of correlation obtained by plotting growth rate against temperature has been used in the present study. Temperature has been expressed considering the maximum and minimum daily air temperature and the upper and lower threshold values at which the temperature effect on growth is considered to be null. The growth curves, which express observed stem elongation, have been transformed to daily values of growth rate. Using a modified equation of the "logistic curve," which best fits the observed growth pattern, a theoretical represenation of growth has been applied to the data. The correlation coefficient has been calculated plotting the values of temperature summation against those of growth rate for periods of 7 days. The stem growth of the seven species has revealed rhythmic components

of 29.5-, 14.7-, 9.9-and 7.3-day periods. The first two components of 29.5 and 14.7 days, which are the most evident, appear to be the best correlated with the phases of the new and full moon. This fact suggests that some unknown periodic environmental change is directly or indirectly connected with the position of the moon. The other two shorter components in growth, of 9.7 and 7.3 days, may be endogenous rhythms that are expressive of the overall physiology of stem development of each species.

LUNAR-CORRELATED VARIATIONS IN WATER UPTAKE BY BEAN SEEDS

Brown, Frank A., Jr., and Chow, Carol S.; *Biological Bulletin*, 145:265-277, 1973.

Summary

1. Rate of water uptake by bean seeds during the initial four hours displays a significant quarterly lunar variation.
2. Under what appear to be minimally disturbed environmental conditions relative to environmental electromagnetic fields, maximum rates tend to occur close to new and full moon and the moon's quarters.
3. One or more of the quarterly cycles may undergo periods of inversion either apparently "spontaneously" or in response to such experimentally altered environmental conditions as those found within a walk in constant temperature chamber, or effected by very slow uniform rotation.
4. The character of an interaction between vessels of beans located close to one another displays a synodic monthly variation. A maximum in interaction-induced negative correlation between two samples occurs 4 to 5 days after full moon, and in positive, 4 to 5 days after new moon.
5. These results give further support for the hypothesis that living systems can exist in either of two states, + and - with respect to their correlation with fluctuating biologically effective and normally uncontrolled, pervasive geophysical parameters, and that this sign is experimentally alterable.

PUT IT DOWN TO SUNSPOTS

Anonymous; *Nature*, 215:1021, 1967.

In the July issue of the Canadian Journal of Botany, Dr. Bruce Cumming, of the University of Western Ontario, claims

that there is a correlation between periodicities in the germination of seeds and variations in the solar radio flux, which is an indicator of sunspot activity. Seeds of Chenoporium botrys were stored in darkness under controlled conditions of heat, humidity and pressure. Each week a certain number were then removed for germination tests, and the percentage germinating was recorded. There was no correlation, apparently, between germination and humidity, pressure, magnetic intensity or the phases of the Moon. There was, however, a close relation between germination frequency and the level of sunspot activity one week before the wetting of the seeds, although linear trends in solar flux and germination were opposite. At the same time, correlation between germination frequency and sunspot activity is expressed in the form of somewhat compressed diagrams--more than a hundred pairs of values have had to be represented on a horizontal scale only 12·5 cm across. Statistical analysis of periodic functions is necessarily complex, and many workers have emphasized the difficulties of comparing periodicities. It is therefore with caution as well as great interest that further details of Dr. Cumming's work will be awaited.

• Plant Behavior and Electricity

EFFECTS OF ELECTRICITY ON VEGETATION

Anonymous; *American Journal of Science,* 1:31:160, 1837.

M. Baric states, that "last year, in the month of July, the lightning struck one of the poplars in my avenue---the fluid breaking off at the time a few branches at the summit of the tree, followed down the tree without breaking the bark, and at last passed into the earth, throwing up two cubic feet of earth. The poplar at the time was about a foot in circumference: at the present time it is double that size, whilst those near by have made no perceptible increase in size.

ATMOSPHERIC ELECTRIC CURRENTS, NORMAL AND ABNORMAL, AND THEIR RELATION TO THE GROWTH OF PLANTS

Blackman, V. H.; *Royal Meteorological Society, Quarterly Journal,* 50:197-201, 1924.

In one of the works of Beccaria published in 1775, we find the declaration of a view which was to dominate many of the latter workers and is of the greatest interest from our present point of view---that of the importance of atmospheric electricity in plant growth. A few quotations will best show Beccaria's beliefs.

"With regard to atmospheric electricity, it appears manifest that nature makes an extensive use of it for promoting vegetation."

"The common saying of countrymen that no kind of watering gives the country so smiling a look as rain, may be explained on the same principle." i.e., that the rain clouds are electrically charged.

He goes on to say "the . . . drops penetrate into them (i.e., the plants) the better as everyone carries along with it a portion of the penetrating dilating element" (i.e., electricity). This reads almost like a prevision of an observation on the charge on rain drops.

Even before this time the idea that electricity might forward the growth of plants had occurred to some workers. A Scotsman, Manbray, in 1746 in Edinburgh seems to have been the first of these. He electrified two myrtles which flowered and produced new branches, while the unelectrified control plants gave no flowers. Jallabert in Geneva in 1748 electrified pinks and hyacinths and claimed that more rapid flowering was so produced. The Abbe Nollet carried out experiments at about the same time. He tried the effect of static electricity on the rate of evaporation, using a series of shelves of iron suspended by silk cords and electrically, connected, by means of an iron chain, with a globe of glass. The globe of glass was rotated by two strong men who were relieved at intervals, while a third person applied his hands to the globe! He claimed that the evaporation of water was thus increased. Nollet, encouraged by the observations of Mambray and Jallabert, undertook experiments with animals and plants using the same apparatus. He claimed that electrified grains germinated earlier and grew faster than unelectrified ones.

These experiments of Mambray, Jallabert and Nollet were carried out before those of Dalibard and Franklin. As already stated Beccaria was probably the first to suggest that atmospheric electricity was of importance to vegetation. Duhamel du Monceau (1700-1788) was the first naturalist to make observations on the relation between the electric state of the atmosphere and the development of vegetation (1758). He insisted on the rapid growth of plants in thundery weather, and the advantages of rain over ordinary watering. The view that plants grow especially rapidly after a thunderstorm is widely held by farmers of the present day, and it is quite possible that there is some favourable electrical action, but one cannot overlook the fact that thunderstorms often follow a period of drought.

Bertholon, who published a book, De l'Electricite des Vegetaux (Paris, 1883), also held strongly the view that atmosphere electricity was an important factor in the growth of plants. He proposed to remedy any default in the quantity of electricity available to the plant by artificial means. He devised his "Electro-vegetometre." A tall post bore an insulated metallic point above which was connected below with a distributor borne on a horizontal arm which could be lengthened or shortened and could be swung round the central post. Thus the plants growing in a circular area round it could be supplied with additional electricity. He also suggested other ways by which plants, including even trees, could be sprayed with water artificially electrified by the means of an influence machine.

It must have occurred to some of my audience that an experiment to determine whether the minute atmospheric current normally passing to earth is of value to the plant, would be to observe the effect of screening plants in such a way that the atmospheric current no longer passed through them. This does not seem to have occurred to any worker before a Frenchman, Grandeau, carried out in 1877 an experiment with tobacco plants in soil in two metal boxes holding 19 kilograms of earth. One of them had a cage of iron wire placed over it, the meshes being 15 x 10 cms. and the wire 0.5mm. thick. In a few weeks he observed differences; the uncaged plant had flowered and set seed, while the caged one had no flowers and the weight was only 50 per cent. of that of the uncaged. Similar experiments were also carried out with maize. These results are, of course, quite unconvincing. The differences between individual plants kept under identical conditions are far too great to allow one to draw deductions from one or two examples. A result of this kind would seem also to prove too much, for it must be remembered also that plants grown indoors under glass and in the close neighbourhood of trees and buildings are screened from the air-earth current. If this current were such an important factor in plant-growth as the 50 per cent. reduction described by Grandeau indicates, it should hardly be possible to grow plants satisfactorily in greenhouses. Lesage, in 1913, carried out similar experiments with thorn apple (Datura) using a conducting iron cage, and a non-conducting silk cage for the control plant. Marked differences in favour of the controls grown in the open were again observed, but such experiments are very difficult to perform in a satisfactory manner. In protecting the plant from the electrical current one is liable to alter the light intensity, the amount of rain, and the air movement to which plants are exposed. Lesage, in fact, found that the rate of evaporation from a water surface under the cage was less than that in the open air.

Mr. Legg and I, during the last 3 years, have carried out similar experiments, using plants of barley grown in pot-culture. Such a method of experimentation is more suitable than that of culture in the garden or field, since one can be sure that soil conditions to which the plants are exposed are similar, and artificial watering protects them in part from the vagaries of weather conditions. The screening consisted of very fine galvanised steel

wires (0.35 mm. diam.) placed parallel with one another about 1¾ ins. apart and connected with earth. Each year has shown regularly a slight reduction in yield of the "caged" plants as compared with the controls. The results are given below in terms of dry weight, that of the control plants being in each case taken as 100.

1921.	1922.	1923.
95.3 ± 2.3	98.4 ± 4.1	93.3 ± 4.1

A single result of this kind may be the effect of pure accident, but an accumulation of such small differences tends to show that the conditions under the screen are definitely less favourable than those of the open air. There is still the possibility that the result is due to differences of light and air movement under the screen, so an experiment has been planned for this year in which the wires at the top of the cage are to be kept at the same voltage as that normally existing at that height. The plants will be compared with the plants grown under an ordinary earthed screen. We see that the favourable effect of the normal atmospheric current on plant growth, the claim for which was first made about 170 years ago, is not yet supported by incontestable evidence, though some slight effect is probable.

Considering now the question of the attempt to increase the growth of plants by abnormally high electric currents artifically produced, we have seen that a few experiments were made by early workers, but they are of little scientific importance. The modern stage of this work begins with Lemstrom. Lemstrom, was a professor of physics at Helsingfors and also a meteorologist, who made a number of journeys to the northern and arctic regions and studied there the aurora borealis. While on these journeys, he was impressed by the rapid growth of the vegetation during the short arctic summer. Recognising the special electrical conditions of the atmosphere in these regions, he was led to the belief that the rapid growth was to be ascribed to a large air-earth current passing to the plants. The occurrence of an abnormally large current in these latitudes is still unproven, but imbued with this belief Lemstrom, in 1884, started experiments in Finland, in which he subjected various crops to abnormally high atmospheric currents artificially produced. He conducted his experiments by erecting on his experimental plots, posts bearing insulators supporting a thick conducting wire; each plot had three series of posts, one row along each side and one down the middle. The thicker wires bore thinner discharge wires, about 1/40 in. in diameter, and running parallel about 4 ft. apart. These wires are kept about 1 ft. above the plots and bore a number of fine pointed barbs to act as discharge points. By means of an influence machine driven by a gas engine, the wires were charged to such a high voltage that the current leaking from the fine points passed through the air to the plants below. Lemstrom carried out experiments during a number of years and with a large number of different crops both in Finland and elsewhere. The results of his experiments he collected together in a book which was publishd in Germany in 1902, and appeared in English

in 1904. In this book, he described increases of yield in the case of many crops produced by this means, the increases being often of 45 per cent. or more.

. . . .

As to the action of the discharge, it has been suggested that it is similar to the application of electric light and the effect is due to the additional energy supplied. Such a view, however, cannot, be substantiated. The energy obtained from sunlight by a crop of barley is of the order of 16×10^9 calories per acre while the additional energy supplied in the field experiments (50 watts per acre for 6 hours a day for 5 months) is of the order of 4×10^7 calories. Thus the energy of the discharge--not all of which is received by the plant--is much less than 0.5 per cent. of the light energy absorbed by the crop, while the additional yield is of the order of 20 per cent. Again, in the experiment with maize shown on p. 203 the calorific value of the additional dry material (2.3 grms.) produced, is of the order of 9,000 calories, while the additional energy supplied was only 15 calories. The effect produced by electrification is clearly out of all proportion to the energy supplied; the physiological action must thus be classed as of the nature of a stimulation.

The laboratory studies have shown that one of the physiological effects of a minute electric discharge is to accelerate the rate of growth, at least in the case of the growing shoot of a barley seedling. That there are other effects will doubtless be established, but at all events a beginning has been made in the analysis of the physiological effects of the discharge.

In brief, one may say that although no indubitable evidence has been obtained as to the favourable effect of the normal atmospheric current, yet the converging evidence now available from the laboratory, from pot-cultures, and from the field, can leave no doubt as to the favourable effect on the growth of plants which larger electrical discharges are able to produce.

ELECTRICITY IN HORTICULTURE

Anonymous; *Nature*, 138:70-71, 1936.

Electro-culture has to take into account the effects of electric heating, electric lighting and the voltage stress on the life of plants. The first application of electricity took the form of high-voltage discharges produced in close proximity to various plants in an endeavour to obtain artificial stimulation. In the Engineering Supplement for May of Siemens Magazine, a survey is given of recent developments, and the photographs shown of the effects produced are convincing. It has often been noticed that an increased growth of crops sometimes occurs during thundery weather. In the Arctic, where the average atmospheric potential gradient is high, it has been observed that the growth of veg-

etation during the short summer is more vigorous than in southern climates. Experiments have been carried out on cereals, potatoes, beets, tomatoes, strawberries and raspberries, and increases up to forty per cent have been obtained. Electricity in the form of light was the next application in the aid of horticulture. For vigorous plant growth a minimum of about 4 hours of sunshine and altogether about 10 hours of daylight per day are required. As an example of the effect obtained by providing lighting for 10 hours each night with an illumination of about 4 foot candles, pansies after 83 days gave an average of approximately 16 flowers per plant as against 2 flowers for unlighted plants; and asters after 157 days' treatment bloomed 33 days earlier than usual. The red-yellow rays accelerate growth by stimulating the chlorophyll and the absorption of carbonic acid. Electric cables are also described which are used for heating the soil, and excellent economical results have been obtained.

APPLICATION OF HIGH-FREQUENCY ELECTROSTATIC FIELDS IN AGRICULTURE

Ark, P. A., and Parry, Willet; *Quarterly Review of Biology,* 15:172-191, 1940.

Discussion. High-frequency electrostatic fields cause some definite changes in biological material. Although the real mechanism by which changes occur in various materials exposed to the action of oscillating electrostatic fields has not been established with certainty, the application of the phenomenon to various problems in biology and in particular to some branches of agricultural science should not be overstated. Numerous investigations show that insect pests, fungi, and bacteria may be destroyed by heat induced by the passage of electric forces. It may be that only certain vital parts of the living object are heated momentarily to such a degree that death ensues. This is in accord with Malov's theory of "point heat".

Inasmuch as it is both interesting and important to know the underlying causes of the effects of high-frequency electrostatic fields, the value of it is readily grasped when the destructive work of various cereal weevils can be controlled permanently by a simple, inexpensive and non-chemical procedure or when wood-destroying fungi may be destroyed by passing lumber between the plates of oscillating generator thus saving considerably in terms of labor and expense.

These experiments indicate that seed-borne pathogens (bacteria and fungi) may be destroyed on or in the seeds without injuring the latter. It may also be possible to increase the power of germination of seeds whose germination has been lowered appreciably or which are naturally more difficult to germinate. (p.183)

EFFECTS OF MAGNETIC FIELDS ON TOMATO RIPENING

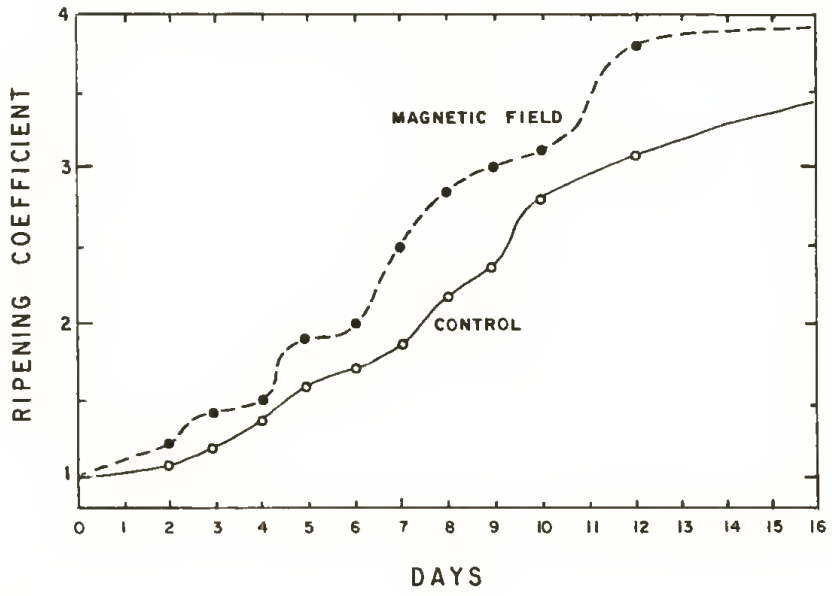
Boe, A. A., and Salunkhe, D. K.; *Nature,* 199:91-92, 1963.

According to Duclaux, Pasteur explored the possibility of the effect of a magnetic field on the production of optical isomers of tartaric acid. He theorized that the Earth's magnetic field caused tartaric acid to be of one optical isomer in nature, whereas laboratory synthesis produced a mixture of both isomers. Recently, Krylov and Tarakonova proposed an auxin-like effect of a magnetic field on germinating seeds. They called this effect magnetotropism. Auxins or synthetic plant growth regulators have been used to stimulate ripening of immature fruits.

Investigations were conducted here to determine the effect of a magnetic field on the ripening of green tomatoes (Lycopersicon esculentum Mill. var. V. R. Moscow). Four permanent magnets of considerable strength were utilized. Fruits of uniform maturity were placed between the magnetic poles. The experiment was conducted in a ripening chamber at 21° C and 85 per cent relative humidity under dark conditions. The ripening rates of treated fruits were compared with those of untreated controls in the same room under similar conditions. A numerical value for colour (1= green, 2-breakers---just starting to colour, 3=pink, and 4=red) was assigned for each fruit in a given sample at each observation period. The coefficient of ripening was calculated as follows:

$$\text{Ripening coefficient} = \frac{\text{total score for a sample}}{\text{No. of fruits in the sample}}$$

In all cases the treated fruits ripened faster than the controls. Furthermore, the fruits nearest the magnetic south ripened faster



than those nearest the magnetic north.

To expand this experiment further, cherry type (Lycopersicon esculentum Mill. Breeding lot No. 1064) tomato fruits of uniform mature-green maturity were used. The small size of these fruits made it possible to increase the number of fruit in each replication. In all cases the treated tomatoes ripened faster than the controls. The polarity effect resembled that noted in the exploratory experiment.

Speculations as to mechanism associated with this phenomenon could be as follows: (1) an auxin-like character of the magnetic field, as proposed by Krylov and Tarakonova, or perhaps an auxin-activating mechanism speeds the ripening process; (2) activation and/or acceleration of the enzyme systems could enhance respiration; (3) radical formation could speed up respiration and thus accelerate ripening.

The possibility that fruit ripening is initiated by a hormone is well known. The experiments carried out by Krylov and Tarakonova demonstrate the similarity of the magnetic effect and effects of plant growth regulators. Pauling and Coryell have shown the magnetic characteristics of the iron porphyrin compounds. The cytochrome oxidase system, which belongs to this group of compounds, could be functionally influenced by a magnetic field. Smith and Cook have reported on the effect of magnetic field on trypsin activity. The formation of radicals is a common step in organic reactions. It is possible that a magnetic field could promote the loss or gain of protons and/or other reactive groups from compounds in the respiratory cycle.

. . . . .

ULF TREE POTENTIALS AND GEOMAGNETIC PULSATIONS

Fraser-Smith, A. C.; *Nature,* 271:641-642, 1978.

In conclusion, measurements with tree electrodes show that trees may be used as 'antennas' to detect ULF geomagnetic pulsations. The measurements also show that ULF tree potentials are largely produced by ULF fluctuations of the geomagnetic field (the remaining component of the potentials is probably thermal noise). Presman noted that electromagnetic fields usually have an adverse effect on living processes. If the ULF geomagnetic pulsations have any adverse effect on the growth of trees (and, as we have seen, they must induce electric currents in the living material) these effects could possibly be observed in tree ring data. Pc1 geomagnetic pulsation occurrences vary markedly over a solar cycle and thus, if these particular pulsations effect tree growth, a solar cycle in tree ring data could occur. LaMarche and Fritts searched unsuccessfully for a relation between the tree ring data and sunspot numbers. The phase of the Pc1 pulsation solar cycle, however, differs by several years

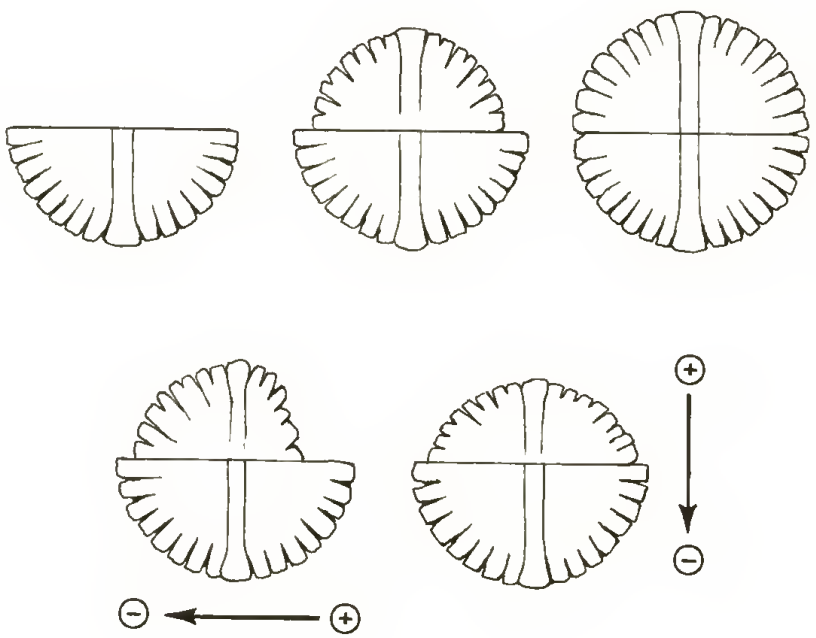
from the sunspot cycle and, assuming the two cycles affect tree ring data, they may tend to obscure each other's effects. Furthermore, other geomagnetic pulsations and higher-frequency electromagnetic signals have their own cycles of occurrence, and their effects on tree ring formation, if any, could add further to the complexity of the tree ring data. Studies of these possible effects are desirable, because the tree ring data could provide a unique record of past ULF and higher-frequency geomagnetic activity. (p. 642)

ELECTRIC CHARGES MAY SHAPE LIVING TISSUE

Anonymous; *New Scientist,* 86:245, 1980.

Scientists testing the effect of external electric fields on growing cells have come up with evidence that the cell generates its own electric field in control shape and form in living organisms (Journal of Cell Science, vol. 42, p. 261.).

Danny Brower and Richard McIntosh of the University of Colorado at Boulder experimented on a disc-shaped alga about 1 millimeter across called Micrasterias. The alga consists of just one unusually shaped cell, with a regular pattern of lobes around the edge (see Figure). When the disc matures, it splits into two halves, each of which has a central "polar" lobe. The two half-discs each regrow the missing halves to reattain maturity. (top)



Brower and McIntosh placed the algae on glass slides in a dish of nutrient medium. They applied an electric field of 14V/cm through a pair of electrodes attached at either side of the dish. When the Micrasterias were placed with their polar lobes at right angles to the field, the new half did not grow as big as the parent half, and the ornamentations on the lobes were also less pronounced. The new half grew larger at the side nearest the cathode. (bottom)

How can electric currents affect patterns in this way? The main skeletal constituent of Micrasterias is cellulose in its cell wall. Brower and McIntosh suggest that the electric current somehow interferes with the cell membrane, which makes the cellulose. Perhaps cellulose formed in an asymmetrical way results in an abnormally shaped alga. If this is so, the authors speculate, then it could be electric currents generated by the organism itself that produce the normal pattern.

• Parasitic Trees

PARASITIC CONIFER FOUND IN NEW CALEDONIA

deLaubenfels, David J.; *Science,* 130:97, 1959.

Abstract. A rare and unusual species of Podocarpus from New Caledonia was collected and discovered to be parasitic on another conifer. The attachment is by modified roots imbedded between the cork cambium and the vascular cambium. A parasitic gymnosperm is something new to science.

ANOMALOUS DISTRIBUTION IN TIME AND SPACE

• The Puzzle of the Grass Balds

ORIGIN OF THE SOUTHERN APPALACHIAN GRASS BALDS

Wells, B. W.; *Science,* 83:283, 1936.

With the opening of the Smoky Mountain National Park the attention of an increasing number of scientists will be directed to the southern mountain bald problem. At the higher altitudes local grass areas varying from 1 to 100 acres in size are to be found sharply delimited in a setting of either balsam-spruce forest or the high altitude deciduous trees, such as southern red-oak or chestnut. Andrews Bald (alt. 5,820 feet) on the end of a south spur of Clingman's Dome Mountain, near the center of the park, is an excellent example of a grass bald. Roughly approximating a square in outline it presents 75 acres of nearly pure mountain oatgrass (Danthonia compressa), changing to a sedge (Carex flexuosa) at its upper wet margin. This luxuriant, deep-sodded, local grassland contrasts most amazingly with the dark margin balsam-spruce forest which arises sharply on every side of it. From the depth of the humus layer and from the asymmetric growth of the trees bordering it, the area is known to antedate the settling of the region by white men.

To plant ecologists of the eastern United States, the origin of the grass balds has been an unsolved riddle. If natural forces (including fire) have been involved, why haven't much larger areas of the high mountains in the last ten or twenty centuries gone over into the "bald"? But the balds are mere dots on the mountain landscape.

On the basis of intensive studies made during the past summer, the hypothesis is presented here that many of the grass balds are in reality ecological artifacts of Indian origin; they represent old Indian, high mountain, summer camp sites, which when abandoned went through a ruderal stage into the oatgrass subclimax, a community which under the climatic conditions of the high ridge and mountain tops of the southern mountains is able to resist the invasion of the original forest almost entirely and to a marked degree the shrubs of the environment as well.

The Indian encampment hypothesis is supported by the following facts: Indians preferred the ridge trails for travel and

hunting. The grass balds are generally located on broadly rounded ridge or knob tops (never on sharp ridges useless for camp purposes) and on the warm southern exposures of these. Unusually good springs may be found at or near the lower margins of the grass areas. Disturbed areas in certain balds were found in which the plant succession leading to the grass was taking place, a succession never noted following the destruction of the forest by logging or fire.

This grass bald succession into a subclimax may be observed frequently on the high altitude trails. In fact, many long trail sections in rather open forest are nothing but linear grass balds, showing in every respect the true bald character. And the initiation of these trail balds is wholly due to human interference operating at the soil level.

Fire in balsam-spruce is universally followed by the "fire-cherry" community, which under repeated fire succeeds itself by basal shoot regeneration. Fire alone can not explain the origin of the grass bald nor, it is believed, can any other natural factor or combination of natural factors.

A fuller presentation of this hypothesis is in preparation which, it is believed, will go far in solving the riddle of the origin of the Southern Appalachian grass balds.

• Remarkable Plant Distribution Patterns

VEGETATION PATTERNS IN SOMALILAND

Anonymous; *Nature,* 167:261, 1951.

In a paper entitled "Vegetation Patterns in the Semi-desert Plains of British Somaliland" in the Geographical Review (116, 199; 1950), Dr. W.A. Macfadyen develops in some detail a thesis put forward in a letter in Nature (165, 121; 1950) that the arc-shaped ripple-like marks seen in air photographs of this semi-desert country are caused not, as sometimes believed, by wind erosion, but by the rhythmical arrangement of concentrations of vegetation. Accompanying photographs afford evidence of the existence of vegetation arcs, and the origin of these arcs is attributed by Dr. Macfadyen to the effect of storm water carrying along seeds, vegetation debris and animal excreta which are deposited in the form of strand lines giving rise to vegetation arcs. That the operative agent is water rather than wind is suggested by the fact that the arcs are always oriented so that their chords are at right angles to the direction of drainage, with their convex edges upslope. Sometimes the arcs are cut through,

at right angles to their chords, by "water lanes" which are clearly visible on air photographs, but have not been identified on the ground. They appear to be drainage channels produced by flows of water which are more localized and intense than those producing the vegetation arcs. The detailed processes by which the vegetation arcs and water lanes are formed are obscure; but the phenomena are of sufficient interest to warrant further study. In particular, it is important to ascertain beyond question whether overgrazing, which is reputed to have caused much damage through wind erosion, is in fact occurring at all.

ARCTIC "VEGETATION ARCS"

Williams, P. J.; *Geographical Journal,* 125:144–145, 1959.

Vegetation arcs to which Professor Ross Mackay refers in his letter in the June 1957 issue of the Journal, are widespread not only in Arctic and Sub-Arctic Canada but also in northern Europe and Asia. Such patterns cover much larger areas (often many square miles in extent) than the now well known and curious stone polygons and allied patterned ground features, and their significance both in engineering and geomorphological studies is likely to become far greater. They have been little referred to in English but much more so in German and Russian literature.

A particularly well defined form are the string bogs, or strangmoore, which occur notably in eastern Canada, where they provided one of the problems of the terrain traversed by the iron ore railway to Schefferville. They are boggy areas which are characterized by long dykes or banks of somewhat silty material covered with peaty vegetation, which dam up shallow pools. The banks which may stand a metre or more high, are aligned parallel to the contours at intervals of some tens of metres on slopes of probably a degree or less. Their mode of formation is in doubt, although it is probable that thrusting of the ice in the pools during the winter is important. Considerable hydrostatic pressures are reported for the water below the ice in winter. Another possibility is that there is a downslope movement of material by slow flowage, possibly in the frozen state, and which might be similar to the movement of certain forms of solifluction terraces. Considerable disturbance of a peat profile is to be expected where such features occur or have occurred in the past.

They are particularly interesting in that, although covering much of the surface over wide areas, there is a considerable element of climatic control in their distribution. Troll has pointed out in his classic paper that they are restricted to areas with a continental climate of only moderate cold, lying outside the regions of continuous permafrost. Professor Mackay must be referring to somewhat different features, or at least to very localized occurrences, when he refers to those on Cornwallis

Island, which is outside the climatic regions associated with string bog formation. There are various structures producing vegetation arcs, as a result of the regular sequence of plant habitats provided by regular terraces or steps on a slope surface. Such features are themselves the result of a variety of processes, but their resemblance to those in Somaliland, or other arid or tropical areas, is surely only superficial.

. . . . .

• Curious Population Explosions

PLANT ECOLOGY OF BOMBED SITES

Hartog, Mabel; *Nature*, 150:320, 1942.

Many of the unsightly scars left by the air-raids on London are now being hidden by a carpet of vegetation. Some notes on its growth on a small bombed site in a terrace in the heart of Kensington may be of interest. A three-storied corner house with a basement area and no front garden of any kind was completely destroyed by a direct hit in September 1940, but the site and that of the adjacent damaged house was not cleared until the autumn of 1941. It was then fenced off by a wooden paling which enclosed the former stretch of pavement in front of the two houses.

At that time there was scarcely a trace of plant growth. Now the site is gay with blossom. For the past six weeks there has been quite a mass of the handsome pink rosebay (Epilobium angustifolium) growing 3 ft. tall, which is becoming one of the most abundant weeds of waste ground and neglected gardens in London, no doubt owing to the wide distribution of its light tufted air-borne seeds; but what is more remarkable, there are no less than twenty strong young shrubs of Buddleia variabilis coming up, the largest of which, growing on the site of the former pavement, are now coming into flower. One of them is already six feet high and has, in flower and bud, thirty-six heads of bloom, some of them nine inches long. The nearest possible parent plant which I have been able to trace is fully a hundred yards away.

Altogether there are twenty-two different species at present in flower on the site, belonging to ten natural order (9 composites, 3 grasses, 3 species of Epilobium, 1 Crucifer (shepherd's purse), 1 Caryophyllaceae (chickweed), 1 Leguminosae (clover), 1 Plantaginaceae, 1 Chenopodium 1 Polygonum, 1 Loganiaceae (Buddleia), besides thick patches of moss with fruiting capsules.

Chapter 9

MICROORGANISMS AND CELLS

Chapter Contents

MORPHOLOGICAL PHENOMENA

• Square Bacteria

A SQUARE BACTERIUM

Walsby, A. E.; *Nature,* 283:69–71, 1980.

Abstract. I have come across a bacterium which has the form of a thin square sheet. In most bacteria such a shape would be precluded by the osmotically-generated internal hydrostatic pressure but this organism, found in a saturated brine pool, has little or no cell turgor pressure. Its shape is probably determined by the pattern in which the cell envelope particles assemble. These square bacteria are so thin and transparent and are so unlike any bacteria previously described that I would have overlooked them if they had not possessed gas vacuoles, and I had not been looking for different forms of gas-vacuolate organisms. The squares derive buoyancy from their gas vacuoles and float at the brine surface.

• Crystalline Life Forms

SIMIAN VIRUS 40 CRYSTALS

Lattman, Eaton E.; *Science,* 208:1048–1050, 1980.

Abstract. Small (10 to 150 micrometers) cubic crystals of simian virus 40 have been grown by ammonium sulfate precipitation. Electron micrographs of thin sections from these crystals reveal ordered arrays of virus particles.

• Unexpected Radiations from Cells

INVISIBLE RADIATIONS FROM CELLS ARE DETECTED BY YEAST

Anonymous; *Science News Letter,* 19:63, 1931.

Invisible radiations from living cells that stimulate other cells to divide and grow are being detected by a new method by Dr. Dmitry N. Borodin, working at the Boyce Thompson Institute at Yonkers, N.Y.

These radiations, whose very existence is still doubted by many biologists and physicists, seem to be something in the nature of ultraviolet light, with a wavelength about half that of the shortest-wave visible and not monochromatic light. They are so feeble that only the most delicate methods can be used to detect them, and these have required so much skill in manipulation that possible errors in experiment or observation have rendered many results dubious in the eyes of skeptical scientists. "M-Rays," as they are often called, have been recorded thus far from 56 living things. Among these are bacteria, yeasts, plant tissues (mostly roots), fertilized eggs and embryonic tissue of animals blood, nerve cells, cancer growth and muscles.

Dr. Borodin's method is an effort to simplify the detection of the radiations without losing sensitiveness. In two drops of malt sugar solution he plants a few yeast cells. The drops hang down in a miniature moist chamber formed by a cover glass at the top, a glass ring, and a filter at the bottom consisting of a crystalline quartz plate or a sheet of cellophane, which are both transparent to short-wave radiations. Under one of these drop cultures in a hollow microscope slide, placed below the filter, he puts living bacteria, or yeast colonies, plant or animal cells or tissues suspected of being capable of sending out the "M-Rays." The other he leaves as it is, as a check or control. Then he puts both in an incubator at a definite constant temperature, to promote rapid growth of the yeast in both drops.

In a definite time, visible colonies of yeast appear in both drops, showing difference in size. They are photographed through a microscope, and the areas of the colonies as they appear on the plates are measured by means of an instrument called the planimeter. Dr. Borodin states that in a long series of these planimetric measurements of hanging-drop yeast colonies the ones subjected to the influence emanating from living cells have practically always shown greater growth than their untreated twins kept as controls. The new method of "M-Ray" detection is called planimetric drop culture method.

MITOGENETIC RAYS

Hollaender, Alexander, and Schoeffel, Eugene; *Quarterly Review of Biology,* 6:215–222, 1931.

The problem of light emission in vital processes and in chemical reactions has often been discussed, but because of contradictory experimental results and a tendency toward an over-speculative interpretation of them much of the published work on the subject has been received with skepticism and for the most part soon forgotten.

In recent years, however, new interest has been aroused in the problem by the discovery of biological radiation and its influence on cell division. The pioneer in this work was Alexander Gurwitsch of the Histological Institute of the First Soviet University in Moscow. Since the first publication in 1923 more than forty papers on this subject have come from the pen of Gurwitsch and his co-workers. Of these, two deserve special mention. One, entitled "Das Problem der Zellteilung physiologisch betrachtet" summarizes the work in this field up to 1926. A later survey article in Protoplasma reviews the subject up to the year 1928. Inasmuch as no adequate summary of the researches carried on in this interesting field has been published in English it has seemed to the present authors worth while and desirable to prepare such a review.

Gurwitsch defines the mitogenetic effect as the action at a distance of one object upon another living object, producing an increase in the intensity of cell division in the second object. The former agent is designated the Sender, the latter is called the Detector.

The number of investigators working on this problem is steadily increasing. The experiments of Gurwitsch were first repeated by J. and M. Magrou, later by Reiter and Gabor, and also by Seibert and others. Thse investigators have verified the findings of Gurwitsch, in essence at least. Others, however, Rossman for instance, obtained negative results. These experiments will be discussed in some detail.

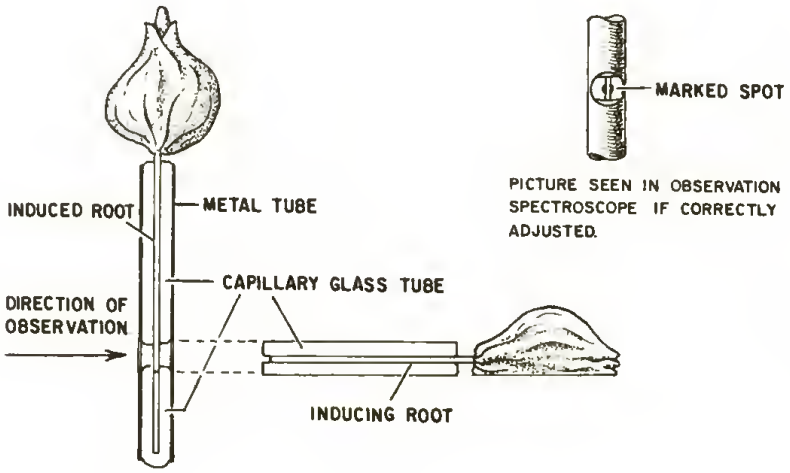
Gurwitsch's Experimental Technique. The original experiment of Gurwitsch will serve to illustrate what is meant by the mitogenetic effect. Bulbs of the common onion (Allium lepum) were allowed to stand in water for some time. After the roots had grown to a length of twelve to thirteen centimeters they were examined and the one that was most perfectly formed and had the most symmetrical tip was chosen for experimentation, and the rest cut away. A close fitting glass tube was then slipped over the single remaining root, which was supported in a horizontal position and kept moist during the experiment. This root served as the sender. A second root, to serve as detector, was prepared in the same way and placed in a glass tube; this time, however, the root extended beyond the length of the glass tube for a distance of about five millimeters. Over the protruding end of this root another glass tube was placed, leaving, however, a-

bout two millimeters of the root exposed. This preparation was, of course, also kept moist.

The two roots were next brought close together, the detector in a vertical position, the sender in the horizontal position with the axis of the sender pointing directly at the exposed section of the detector root. This adjustment was very carefully made by means of a telescope so that "the projected axis of the sender root struck the detector exactly in the median line." The whole set up was held firmly in position by a rigidly built frame. It was most important that the adjustments should be carried out with extreme care since on them the success of the experiment rested. After remaining in this position from two to three hours the sender root was removed, the detector allowed to stand a certain length of time, then the direction of induction was marked and the root was prepared for sectioning. The last five millimeters of the root were cut in longitudinal sections, parallel with the direction of induction, and the number of mitotic figures to the right and left of the median line counted. The experiment revealed "a regular sharply circumscribed preponderance of mitoses in the center of the induced side of the root." The excess of mitotic cells on the side toward the sender over those on the opposite side was about fifty per cent. The results of a determination are given in Table I.

The outcome was not materially different when a thin crystalline quartz plate was interposed between sender and detector. Gurwitsch (3) considered that these experiments proved the existence of a radiation, which he called mitogenetic rays.

. . . . .



Gurwitsch's experimental arrangement for demonstrating the controversial mitogenetic rays.

MITOGENETIC RADIATION

Metcalf, W. S., and Quickenden, T. I.; *Nature,* 216:169-170, 1967.

Between 1920 and 1935 a great deal was published on the subject of mitogenetic radiation--a radiation usually considered to be ultraviolet, which is emitted by dividing cells and which stimulates other cells to divide. These studies were originated by Gurwitsch and are still carried on in the Soviet Union, but almost ceased in Britain and the United States in the 1930s after much careful but negative work. Nobody there was able to stimulate cell division with weak ultraviolet light or to detect radiation from rapidly dividing cells with photo electric or biological detectors.

The sensitivity of photon counters has increased more than 200 times since the 1930s and modern equipment has been claimed to detect radiation from yeast and tissue cultures. This communication reports an attempt to check these measurements.

According to a communication from Gurwitsch's laboratory quoted by Troitskii, two drops of 6 per cent oxalic acid added to 3 ml. of 0·2 per cent potassium permanganate emit radiation of intensity similar to mitogenetic radiation. We have constructed a photon counter which readily measures light in the range 2000-6000 Å from this reaction.

. . . . .

Suspensions of yeast or bacteria in their rapidly dividing logarithmic phase of growth (as shown by opacity measurements) were introduced into the cell by a syringe through a long, black plastic tube. The yeast and bacteria were suspended in a nutrient medium of soy trypticase broth which emitted detectable phosphorescence for up to 5 min after it entered the cell. After this no light could be detected from any of the yeast or bacterial suspensions listed in Table 1 [omitted], although the light from the reference solution of Troitskii was easily measured with the same equipment.

We conclude that any radiation from these growing cells must be less than one-fifth of that from the reference solution quoted as a source of light comparable with mitogenetic radiation.

THE GUIDING LIGHT OF CELL DIVISION?

Anonymous; *New Scientist,* 36:176, 1967.

Does mitogenetic radiation really exist or not? This radiation, generally considered to be ultra-violet, is supposed to be emitted by dividing cells and also to stimulate other cells to divide. Studies of this phenomenon originated in the Soviet Union in the mid-1920's, and have been carried on there ever since. In Britain and the United States, however, after preliminary interest between

1920 and 1935, the subject was dropped as nobody was able either to stimulate cell division with weak ultra-violet light or to detect radiation from rapidly dividing cells. Now the subject has been raised again by a report (Nature, Vol. 216, p.169) coming from scientists in New Zealand and Australia who claim to have detected radiation from rapidly dividing yeast and bacteria cells using extremely sensitive photon counters. The detected radiation, however, is so weak--a fifth of the amount claimed by the Russians to be normal for mitogenetic radiation--that further work is necessary before the existence or non-existence of mitogenetic radiation can be finally settled.

SOVIETS CLAIM THAT CELLS FLASH LIGHT TO OTHER CELLS

Anonymous; *New Scientist*, 57:172, 1973.

An incredible report, alleging the possibility of communication between living cells by means of ultraviolet waves, has recently come out of the high-powered Siberian scientific centre at Akademgorodok. It is the result of work done by Vlail Kazhacheyev, Simon Shurin and Lyudmila Mikhailova of the Institutes of Clinical and Experimental Medicine, and of Automation and Electrometry. They claim that their research reveals a new way in which biological information may be transmitted.

Normal living cells, they found, could be affected by neighbouring poisoned or infected cells if the two groups were separated by a quartz glass barrier. When they infected one set of cells with a virus symptoms of disease broke out almost immediately in the neighbouring, separated cells. The same thing happened with mercuric chloride poisoning, an over-dose of ultraviolet rays, and other lethal agents. The three researchers say that they could never find any trace of the viruses or chemical agents in the second cell container, though the cells somehow became "infected". The effect did not happen with an ordinary glass barrier which would filter out any ultraviolet rays. Quartz transmits ultraviolet.

According to Shurin, the team had to assume that living cells possess an unknown radiation channel for the transmission of information. They found that a normal cell emits an even output of photons, but when it is infected by a virus the radiation pattern changes sharply, though there is no explanation yet as to how the disease symptoms are transmitted. Shurin forecasts optimistically that knowledge of the emission from a cell could provide valuable information about disease.

The work of these Siberian biologists has been registered by the USSR Committee for Inventions and Discoveries as a new discovery, but their results have still to be confirmed in the West.

• Reproducing Cell Clusters

MAMMAL CELL CLUSTERS THAT "REPRODUCE" THEMSELVES

Anonymous; *New Scientist,* 24:45, 1964.

How an organism's cells become differentiated for specific parts and functions is a question that provides an active field of research but one that as yet has many key questions to answer. Some recent observations on the behaviour of cultured cells by M. Moskowitz, of Purdue University, may have a bearing both on the question of cell differentiation and the closely related topic of how tumours grow (Nature, Vol. 203, p. 1233).

Professor Moskowitz last year described how mammalian cells grown in certain types of artificial media formed aggregates. Since then he has refined the culture technique and now finds that the cell aggregates, which he has termed "aggregens", behave very much like independent organisms or, alternatively, like free-living tumours.

Cells in an individual aggregen multiplied until the unit reached a certain size when it divided into smaller aggregens each of which continued to grow and multiply like single-celled animals. Professor Moskowitz has studied the continuing process over a period of eight months and there appear to be no signs of degeneracy or loss of vigour in his cultures.

Depending upon the kind of cell which he grew, the aggregens showed different surface features and internal structures--two species developed an outer layer of cells surrounding an apparently loosely associated collection of cells in the centre. When they were forced through a fine pipette the aggregens broke open, releasing large numbers of cells, many of them alive, and leaving behind a thin, sac-like structure.

It was evidently necessary to replace the growth medium every few days, and to agitate the culture, for the aggregens to continue growing. One kind grew in diameter from a fifth of a mm to five mm in a month; it then developed grooves in its surface and, a week later, split up into a dozen fragments. These grew and repeated the cycle.

The concentric structure of these strange cell clusters may be explained, Professor Moskowitz thinks, in terms of nutrition; while the aggregens are small all cells receive enough food to multiply but when the "body" gets too big only the outer cells continue to get their full complement, while the inner ones degenerate in some manner and appear changed. Possible differences between normal and tumour cells cultured in this way may well highlight features of cancer cell behaviour better than cultures of single tumour cells and may lead to improved ways of studying how drugs react with tumours.

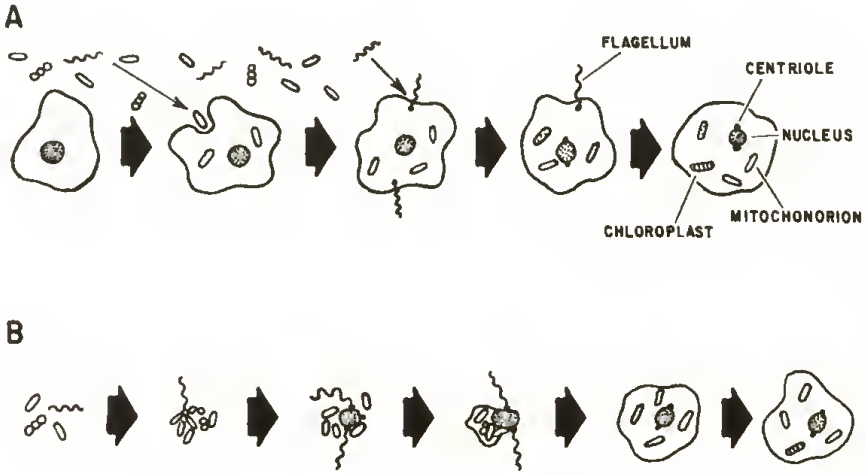
• The Mystery of Mitochondria and Chloroplasts

WHERE DID MITOCHONDRIA COME FROM?

Williamson, Donald; *New Scientist,* 47:624-626, 1970.

Mitochondria are tiny structures found in the cytoplasm of all the more highly developed living cells. Classed as organelles, their main function is to carry out the sequence of biochemical reactions involved in respiration, thus providing the cell with its energy. In their microscopic appearance, staining reactions and---in some cases---their apparent ability to reproduce themselves by dividing in two, they superficially resemble rod-shaped bacteria. It is therefore not surprising that as long ago as 1890 the microscopist R. Altmann should have had the idea that mitochondria (and for that matter other organelles such as the chloroplasts of green plants) might in fact be derived from bacteria.

Altmann's suggestion, and variants of it, have repeatedly been discussed by later authors. As illustrated in Figure 1 there are two main possibilities. In the first, a hypothetical non-bacterial ancestral cell ingested, or was invaded by, free-living bacteria which survived and entered into a symbiotic relationship with their host, eventually evolving into present day organelles. In



Present-day cell organelles may have evolved from bacteria in two ways: (A) A primordial cell ingests bacteria which then evolve into organelles; (B) Different bacteria specialize separately and then fuse into organelle-containing cells.

the second, different bacteria came to co-exist in some form of colony, underwent specialization in different directions and eventually, by fusing together, gave rise to the early ancestors of today's non-bacterial organelle-containing cells.

Both hypotheses have their difficulties; but in any case any notion of a bacterial ancestry for the mitochondrial organelles was, until recently, "too fantastic for present mention in polite biological society", as one writer put it. The trouble was that a superficial similarity to bacteria was hardly adequate evidence of kinship. Reasonable proof required the detection in mitochondria of features which were otherwise exclusively bacterial and which would therefore pin-point the bacterial origin of the organelles. Obviously this could not be achieved until such unique markers of "bacteria-ness" had first been recognized in free-living bacteria. And this sort of knowledge just did not exist.

Now, however, all this has changed, and the basic concept that mitochondria and other organelles have evolved from ancestors of an orginally bacterial nature is so widely accepted that it has almost acquired (in my view, a shade prematurely) the aura of a fundamental truth. (p. 624)

JONAH AND THE WHALE IN MICROCOSM

Anonymous; *New Scientist*, 42:625, 1969.

One of the most attractive ideas in biology at the moment is that sub-cellular organelles such as mitochondria and chloroplasts were once free-living unicellular organisms which, long ago, came to live in symbiosis inside the cells of higher organisms. Evidence regarding the degree of autonomy that these organelles have within the cell continues to accumulate in the literature of cell biology. And now a recent investigation by Dr. J.B. Hirshon, of Long Island University, throws some light on how the partnership may have originated (Biological Bulletin, vol. 136, p.33).

Hirshon has been studying the unusual phenomenon of symbiosis between two unicellular organisms. Basically, he has been looking at the feeding behaviour of the familiar protozoan Paramecium bursaria in the presence of a number of species of single-celled algae--including a Chlorella species that was isolated from Paramecium in the first place--and some inert particles in the same size range as the algae. The Paramecia were clearly rather finicky about what they ingested. Although they all took in the inert organic particles, only a proportion ingested the algae, and this proportion varied according to physiological conditions of the algae: if the algae had been grown in the light, for instance, nearly all the paramecia ingested some; if the algae had been kept in the dark, on the other hand, fewer Paramecia chose to take them in. Of the seven species of algae present, one was never ingested.

But the most important result was that five out of the six

species of algae that were ingested by the Paramecia were not digested, but were able to maintain themselves--at least to some extent--in the protozoa's cytoplasm. Of these five species, two--the Chlorella species originally from Paramecia, and the usually free-living C. vulgarts--managed to establish themselves as genuine symbionts, reproducing inside the Paramecia at a fast enough rate to ensure that when the Paramecia themselves divided, the progeny always received some Chlorella cells. The other species were less successful in this respect.

The development of a symbiotic union between the two unicellular organisms therefore involves a series of steps, each of which filters out unsuitable potential partners. The first step is the selective ingestion of algae by Paramecium, then comes the test of resistance to digestion, and finally the hurdle of rapid enough reproduction. In contrast to other more highly evolved cases of symbiosis, this example--which may well have been one of the earliest to develop--is not necessarily specific and permanent, but is instead a dynamic interaction between two species, whose equilibrium can easily be upset. It may, however, parallel the way in which primitive bacteria and blue-green algae were first incorporated into primitive cells, to become, in time, those permanent and vital organelles, mitochondria and chloroplasts.

WERE CHLOROPLASTS ONCE ALGAE?

Anonymous; *New Scientist,* 34:36, 1967.

Although DNA is mainly found in the chromosomes, it also occurs in some other structures inside living cells. One such place is in the chloroplasts of green plants--the small bodies responsible for photosynthesis. Evidence for its existence here is, however, largely experimental, and only in the case of the single-celled green alga Chlamydomonas is there direct evidence from electron micrographs. Now an electron microscope study of the chloroplasts of a red alga. Laurencia spectabilis, by T. and A-A. Bisalputra, of the University of British Columbia, has located DNA here too, and has given support to a recent speculation about the origin of chloroplasts (Journal of Ultrastructure Research, Vol. 17, p.14).

When the two botanists examined the chloroplasts of this alga under the electron microscope, they discovered fibrils with a diameter of approximately 25 angstroms in the low density areas of the matrix. As these fibrils are digested by the specific enzyme deoxyribonuclease, they concluded that they consisted of DNA. The areas containing DNA seemed to be scattered at random and, in some cases, several were found in one chloroplast.

The chloroplasts themselves, small lens-shaped structures from 1 to 1.5 micrometres wide and 2 to 3 micrometres long, are bounded by a double membrane. The photosynthetic lamellae inside them are flattened discs with granules on their surface. In

many ways they resemble the relatively simple blue-green algae, and these workers suggest that their observations support the idea that the chloroplasts of higher plants ultimately originated from blue-green algae which were symbiotic.

• The Complex Nature of Cells and Microorganisms

WHAT IS A MICRO-ORGANISM?

Pollock, M. R.; *New Scientist,* 27:637-639, 1965.

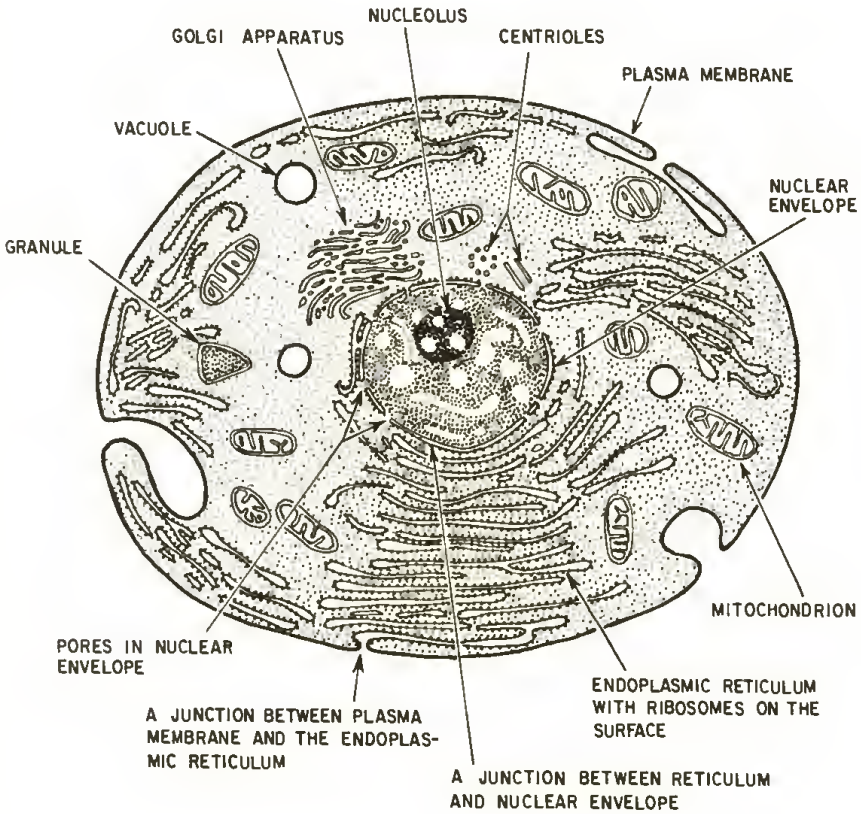
Most biologists have been brought up to regard a living organism as a highly integrated, more-or-less "self-sufficient" internally coordinated unity, with an evolutionary history traceable--in theory at any rate--through a single line of ancestry back into the pre-Pre-Cambrian past.

Yet we know very well that these systems we refer to, and recognize, as organisms are mutually interdependent. Relationships, varying from the loose ecological associations of plant, animal and bacterial life in general, to the intimate and mutually obligatory type of life shown for instance, by alga (fixing atmospheric carbon dioxide by photosynthesis) and fungus (scavenging sources of nitrogen) in the lichens, are the rule rather than the exception. Yet even in the lichens, it is possible to separate the two components--at least in the controlled conditions of the laboratory--and still recognize each as some sort of a living unity in its own right. There are, however, protozoan and coelenterate (sea-anemone and the like) systems where this symbiotic process has become even more intimate and the photo-synthesizing plant components have degenerated into mere particles within the cell closely resembling the chloroplasts which give green plants their pigmentation. It therefore seems not unreasonable to explore the possibilities of pushing the concept of symbiosis beyond its original limits; let us consider the extent to which individual cells can be regarded as balanced systems of symbiotically related groups of molecules possessing some (albeit limited) degree of independent variation within the cellular environment.

For this purpose it is easier to confine ourselves in the first place to micro-organisms where a single cell is an organism in the original sense. And it is in microbiology that recent discoveries lend some support to the idea of the cell as a sort of molecular mosaic, containing differentiated entities of distinct chemical composition, some degree of self-reproductive autonomy and perhaps even independent evolutionary origin. At the moment it would appear that such entities depend for their

properties upon the presence of nucleic acid--in most cases probably deoxyribonucleic acid (DNA). DNA of course is well known to function, in a suitable environment of the "right" enzymes energy sources and building-blocks (bases, amino-acids, etc.), as a specific primer for its own synthesis. Each type of DNA is a long molecular thread, with its own particular sequential thread, with its own particular sequential pattern of the four main bases; adenine, guanine, thymine and cytosine. It functions as a specific information tape not only for copying itself, but for determining the amino-acid sequences--and therefore the specific characters--of the particular enzymes which may be uniquely concerned in the metabolism and general functioning of the entity with which the DNA is associated.

It now seems almost certain that many of the most important



The many organelles within a cell may be regarded as "endosymbionts" or internal symbiotic organisms with the potential of evolving separately.

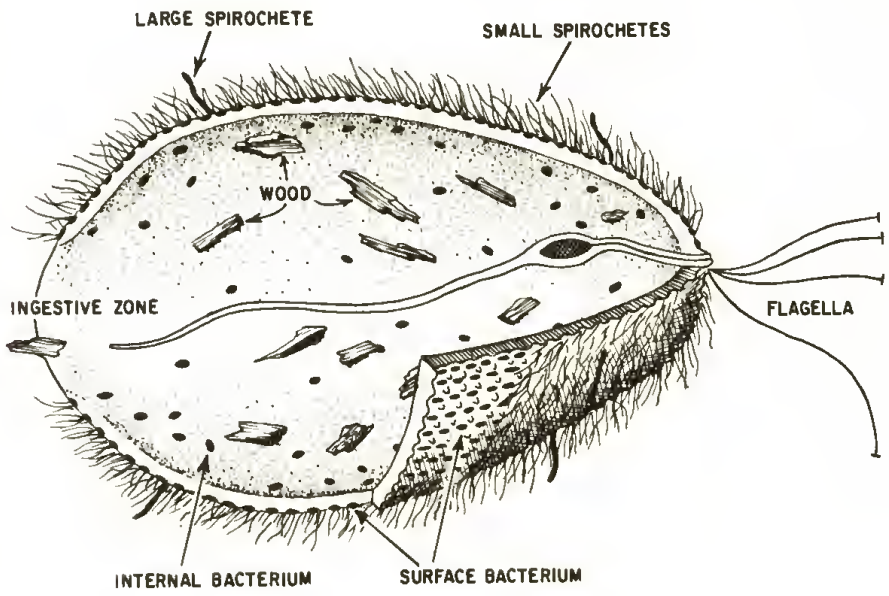
of the minute structural features, or "organelles" within the cell such as mitochondria, chloroplasts, cinetosomes, etc. possess a particular DNA of their own. They are produced, not simply as a conglomeration of molecules derived directly from information provided by nuclear DNA, but to a large extent--if not entirely--by a distinct self-copying and specific coding process within their own systems. these autonomous properties are even more marked in the various types of extrachromosomal entity (for example, the "episomes" which include transmissible sex factors or the "plasmids" like drug-resistance-transfer factors) which carry a number of heritable characters in many species of bacteria. In a formal sense, these are still types of symbiosis since organelles such as chloroplasts depend for their persistence upon the specific environment within the cell they "inhabit", and the cell itself, if deprived of all its chloroplasts will die or remain chloroplast-less forever.

. . . .

These exceptions to the general rule of the non-intrusion of DNA's from different species, rarely though they occur, could be of great evolutionary significance. Moreover, there may exist other cases, yet unsuspected, and it does not seem unreasonable to speculate on the possibility that some cells (micro-organisms in particular) may have evolved, on occasions, by fortuitous and often cataclysmic associations of "bits and pieces" of nucleic acid (derived by separate evolutionary pathways) as well as by the classical mechanism of a more gradual endogenous expansion and modification along a continuous line of development. The pattern of evolution in its earlier stages may thus be arranged, in some areas, as networks of diverging and re-associating lines of development, with a mesh extending more openly than the generally accepted one of recombinations restricted to those occurring within a single species. Perhaps we, ourselves--or our children, through ourselves--may be more of a prey to casual encounters with a dangerous (or beneficial) piece of stray nucleic acid than we would care to admit! Indeed certain types of infectious vertebrate cancers do appear to have their origin in just such an event.

However that may be, such notions and such phenomena as those already described may be expected to stimulate man to try his hand in practical genetics by attempts to modify the genetic structure directly. Already there have been reports of breaking through a nucleic acid intrusion barrier in a spectacular fashion by growing the animal virus of vaccinia (smallpox) in a bacterium. (Bacillus subtilis.) This was apparently achieved by a subtle and delicate de-coating of the virus nucleic acid by means of specific protein-hydrolysing enzymes. Another animal virus (encephalomyelitis), it is now claimed, has been grown in Escherichia coli. Analogously, certain mouse cells and human tissue cells have recently been jointed--through artificial culture together in the laboratory (by means of a purely local destruction of the barrier membranes between them)--their nuclei fused during cell division, and the two sets of chromosomes found to be growing

and multiplying quite happily within the same nuclear membrane (see "Hybrid cells from mouse and man", by Professor Henry Harris, New Scientist, Vol. 25, p.420). Such phenomena will surely affect our concept of organism in some degree.



The protozoan Myxotricha paradoxa *lives in the guts of Australian termites. It hosts three symbionts of its own that help it digest the wood eaten by the termites. (Adapted from* Scientific American, *225:49, August 1971)*

• The Subtleties of Disease Systems

HOW HAVE INSECT-BORNE DISEASE SYSTEMS EVOLVED?

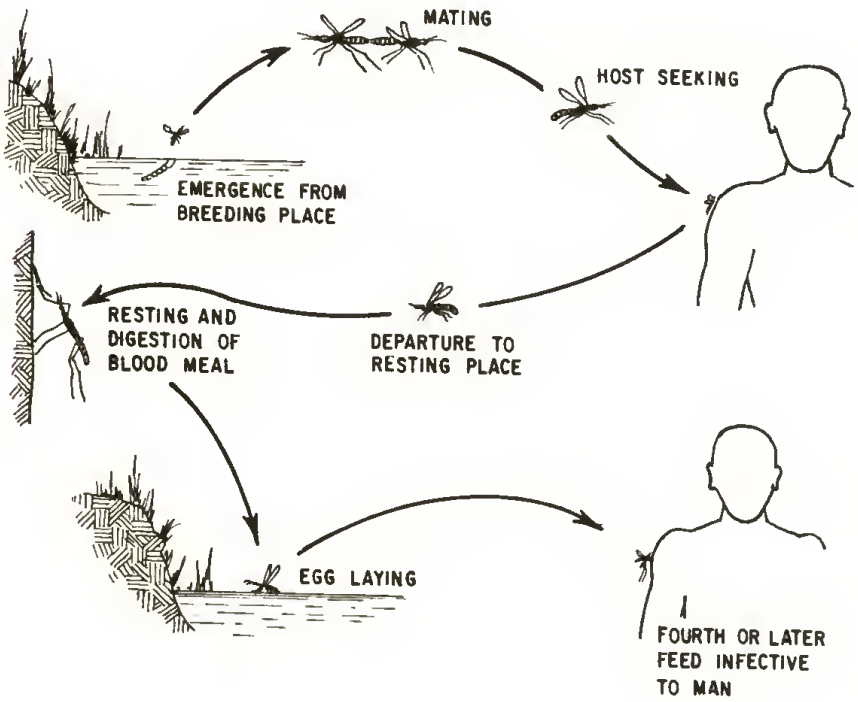
Mattingly, P. F.; *New Scientist,* 25:22–25, 1965.

Malaria, sleeping sickness, various worm infestations and the great variety of other human arthropod-borne diseases are responsible for some hundreds of millions of cases every year. They

are representative of a wide variety of ecological systems involving birds, amphibians and reptiles as well as mammals. Their resilience in the face of the powerful weapons arrayed against them by man reflects an organic integrity acquired over many millions of years, during which the evolution of each organism invoved has both conditioned and been conditioned by the evolution of the others.

The special feature of these systems is the part played by an anthropod "vector", usually an insect but sometimes a mite or tick, which actively conveys the parasite from a particular vertebrate host to one or more others. They thus differ in an important respect from those systems in which the invertebrate host merely liberates the parasite into the surrounding medium, as is the case with many mollusc-borne worms, and those, such as the guinea worm, carried by small aquatic crustaceans (copepods) which are simply swallowed by the vertebrate host.

The best-known arthropod-borne parasite system is that of malaria, illustrated in Figure 1. Looked at from the viewpoint of the parasite, it appears an odd and precarious way of life, involving as it does the participation of at least two other species in suitable phase. It is typical of many such systems and, while we can see all too clearly that these are viable today, to say how they evolved is not easy. We cannot even be sure whether a given parasite originated as a parasite of the arthropod vector or



The malaria disease system

of the vertebrate host, though in the great majority of cases opinion favours the latter.

Because they cause less damage to their arthropod vectors than to their vertebrate hosts, the idea that the arthropod-borne parasites must have originated as parasites of arthropods has been frequently advanced. The weakness of this argument lies in the fact that a healthy vector is absolutely essential for the survival of the parasite whatever its origins. Between becoming infected and becoming infective a vector such as a mosquito must find a shelter in which to digest its blood meal, emerge from it, find a site in which to lay its eggs, and then find another host. It must often do this several times in succession before the parasite becomes mature. A reduction of the efficiency of the vector at any stage in these hazardous proceedings is liable to be fatal to the parasite. In these circumstances a reduced ability of the parasite to cause disease in its arthropod vector, as compared with its effect on the passive vertebrate host, seems only to be expected. One must admit that it is difficult to see how a parasite survives during intermediate stages of adaptation to a new vector.

. . . . .

• Possible Inheritance of Acquired Characters

HERITANCE OF ACQUIRED CHARACTERS

Horsfall, Frank L., Jr.; *Science*, 136:472-476, 1962.

A beautifully documented example of the heritance of acquired characters is provided by the association of bacterial cells and certain bacterial viruses. Some of these infective agents may induce an alteration in their bacterial host cells which is characterized by the dormant potentiality to produce more bacterial viruses, identical to that which led to the new character. Of most importance, the character so acquired is handed on to daughter cells in enduring continuity and has all the features of a genetically determined heritable change. It is now thought that part of the genome of the bacterial virus is incorporated into the genetic apparatus of the host cell and goes along with it in close register during cell division.

Even more startling is the biosynthetic alteration that is induced in non-toxigenic diphtheria bacilli by certain bacterial viruses. Under the influence of appropriate viral genetic material, such bacteria acquire the attribute of synthesizing a new and highly damaging protein--diphtheria toxin. This new character is clearly heritable and is handed on from cell to cell without the

intervention of any additional viral infection.

The states of lysogeny and diphtheria toxin production are both examples of infective heredity. Each is induced by a bacterial virus which contributes the imprint of new gene structure on a chromosome of the infected host cell. Daughter cells are marked by the new character, which has become transmissible and heritable. In this manner, certain changes in the genetic apparatus which formally correspond to mutations can be produced at will and, like those first demonstrated with the transformation of pneumococcal types by deoxyribonucleic acid, are predictable as to their nature and the products that result. (pp. 473-474)

UNUSUAL PHYSICAL ABILITIES

• Latent Life or Cryptobiosis

THE PROBLEM OF ANABIOSIS OR LATENT LIFE .....

Keilin, D.; *Royal Society, Proceedings*, B150:149-191, 1959.

It is now well established that some micro-organisms can, under certain conditions be deprived of all visible signs of life and yet these organisms are not dead, for, when their original conditions are restored, they can return to normal life and activity.

This peculiar state of an organism is referred to in the literature as viable lifelessness, suspended animation, viability, latent life or the not very suitable, but widely used term anabiosis to which I shall return to later on. In the meantime, the term anabiosis, or latent life, will be used in this lecture for the state of an organism when its metabolic activity is at its lowest ebb, sometimes reaching a hardly measurable value, and in some cases actually coming to a standstill, the physiological and biochemical processes being reversibly arrested for different periods of time. (p.150)

IX. Anabiosis and Its Bearing on Problems of the Origin and the Continuity of Life on the Earth

I have already mentioned that some of the experiments on anabiosis were stimulated by some early speculations on the problem of the origin of life (cf.p.165). Several generations of workers, failing to find a fruitful method of approach to this problem, obtained a certain feeling of relief in relegating the origin of life to some remote part of the universe. In a paper dealing with the resistance of dried non-sporulating bacteria, Shattock & Dudgeon (1912) wrote: 'The most fascinating problem in connection with the vitality of bacteria in vacuo is the possiblity of their interplanetary life.' However, they found that dried bacteria are rapidly killed by ultra-violet radiation; they also mentioned that, according to Sir James Dewar (who presented their paper to the Royal Society), ultra-violet rays kill undried bacteria frozen at the temperature of liquid air (-190°C). They concluded that their results militate against the view 'that free particulate life has entered the earth's atmosphere as a result of light propulsion, from extramondane space.' This conclusion was in complete agreement with the view expressed by Becquerel in 1910 and further developed in his recent work.

On the other hand, the ability of some organisms to withstand desiccation and very low temperatures enables them to survive in polar regions or at very high altitudes. Such organisms, according to Becquerel, may even assure the preservation of life under the most adverse hypothetical conditions. It is interesting in this respect to recall a remark made by Darwin in his autobiography: 'With respect to immortality, nothing shows me [so clearly] how strong and almost instinctive a belief it is, as the consideration of the view now held by most physicists, namely that the sun with all the planets will in time grow too cold for life, unless indeed some great body dashes into the sun and thus gives it fresh life. Believing as I do that man in the distant future will be a far more perfect creature than he now is, it is an intolerable thought that he and all other sentient beings are doomed to complete annihilation after such long-continued slow progess. To those who fully admit the immortality of the human soul, the destruction of our world will not appear so dreadful' (see Life and letters, vol.1, p.312).

Although no answer to the speculations of physicists can be given which would have satisfied Darwin in so far as it concerns the fate of man and some other higher organisms, 'the latent life of germs [wrote Becquerel], at very low temperatures is still the most efficient way for the preservation of life on earth as long as possible. In fact were cold to overcome our planet, either by its moving farther away from the sun, or by absorption of solar radiation or their extinction caused by some astronomical event, our atmosphere would be liquefied as this has already taken place in the case of Jupiter, Uranus, Neptune and Pluto. The animals and plants would disappear. Only some spores, some eggs and resuscitating animalcules would subsist ready for a new evolution if favourable cosmic conditions return.' How-

ever, according to the more recent speculations of astronomers, the extinction of life on earth will be brought about not by a decrease, but by an increase in the intensity of solar radiation, which will gradually raise the temperature of the earth and ultimately vaporize it. All forms of life, including spores, eggs and the resuscitating, or anabiotic organisms will already be destroyed when the temperature of the earth approaches 100°C. On the other hand, astronomers give us many million years of respite before the earth will reach even a moderately high temperature: one which will already be lethal to man and to some other higher forms of life. Man thus has ample time to develop into what Darwin earnestly believed to be 'a far more perfect creature than he now is' and probably to find some way of escaping the annihilation now predicted by astronomers. Unfortunately, a far greater and more imminent danger to man may come not from any hypothetical cosmic event, but from man himself, if some of his great scientific achievements, which provide him with unimaginable power of destruction, should happen to serve the end of his still greater folly. (pp. 181-182)

THE DISCOVERY OF LIVING MICROORGANISMS IN ANCIENT ROCKS

Lipman, Chas. B.; *Science*, 68:272-273, 1928.

About one and one half years ago the writer began some experiments which he has carried on since, as time permitted, to determine whether or not living spores of bacteria or fungi or resting bodies of other micro-organisms might still exist inside of ancient rocks. The basis of my decision to start upon such an apparently hopeless quest will be given in a future detailed report on the results obtained. It suffices for the purposes of this preliminary note to state in general terms the startling fact that I have discovered living organisms in a Pre-Cambrian rock from the Algonkian in Canada and in one from the Grand Canyon of the Colorado. I have also discovered other types of micro-organisms in a Pliocene rock which derives from a depth of several hundred feet from which it has recently been uncovered. It is impossible in this note to furnish details of the technique employed, but it may be said that drastic sterilization measures for the outside of the rocks studied were employed. While some of the organisms which appeared in the cultures are doubtless derived from the free air which had momentary access to the rock in the process of the technique employed, certain organisms were found which occur in every plate culture made with the rocks examined and which are of a strikingly different type from any which are usually found in plates made with soils or rocks. These organisms make a very sparse growth on media which support excellent growth of other organisms and seem to belong to the interior of the rocks studied. At least one and perhaps two

such singular types of organisms were found which possess many characters of the Actinomyces group. They are spore-bearing rods occurring in chains, and I have become convinced that they are indigenous to the rocks in the spore form. The question of whether they have relatively recently gained access to the interior of the rock or have always been there remains to be determined by further investigation. Detailed studies with improved technique are now in progress to answer the numerous questions which have arisen as a result of my discovery. Many types of rock will be studied and especially specimens derived from great depths where surface contacts could have played no part in furnishing the results noted. The Pre-Cambrian specimens thus far used were surface samples, but the Pliocene specimen was a deep sample as explained above. The organisms in the Pre-Cambrian rocks and those in the Pliocene rocks are quite different from one another. No algae of any kind, and no nitrifying bacteria have ever been found even in cultures maintained for several months or more.

It need hardly be said that the significance of the facts stated above is extremely great from the physiological standpoint and also from the evolutionary standpoint.

. . . . .

BACTERIA IN PENNSYLVANIA ANTHRACITE

Turner, Homer G.; *Science,* 76:121-122, 1932.

Several times during the last few years notes have appeared in scientific journals and newspapers calling attention to the discovery by Dr. C.B. Lipman of living bacteria in Pennsylvania anthracite. According to Lipman these bacteria are "descendants directly from cells which have lain dormant there from the time of the coal's formation, which, according to one method of the geologist's reckoning, would be fifteen million years, and according to another method, from one to two hundred million years."

In accordance with commonly accepted theories, anthracite was formed from coals of lower rank through the action of dynamic metamorphism or by contact with igneous rocks. A microscopical examination of Pennsylvania anthracite shows that the most resistant plant substances, such as waxes, gums, and resins, have been changed to anthracite. Furthermore, the original argillaceous sediments associated with the vegetation have been changed to shale and slate-like rocks containing micas and other metamorphic minerals. It seems unreasonable, therefore, to expect a relatively perishable substance like the protoplasm of bacteria or the spores thereof to be exempt from the changes which have taken place in their vastly more resistant associates.

Mr. M.A. Farrell and the writer made a careful study of Pennsylvania anthracite taken from the same bed and same mine from which Dr. Lipman secured his samples and concluded that

this anthracite contains no bacteria other than common living forms which have found ingress through fracture cracks and coal laminae communicating with surface water and air.

. . . . .

It is difficult to see how reliable conclusions regarding the presence of ancient bacteria in anthracite can be reached without a thorough study of the history, structure and texture of the coal both in the field and in the laboratory. Long before the shaft is sunk the coal has been subjected to possible contamination through circulating ground-waters. As the shaft is sunk, impure water and air advance with it. Practically every mine is equipped with pumps to keep the water low enough to permit working, and even then flooding is not uncommon. The possibility of securing an uncontaminated piece of coal or rock from a mine is so remote that the whole problem resolves itself into a study of permeability. The student of coal petrography realizes that one piece of coal may be impervious, while another piece from the same bed may contain fractures or laminae which could easily be penetrated by large or small bacteria. A permeability study, then, would be of little value without a knowledge of the texture and structure of the sample used.

The reported finding of bacteria millions of years old is news and as such can do no harm. But, when it begins to appear as a fact, and is used to overthrow well-founded theories on the origin of coal, it is time to ask ourselves whether or not it is true. My object in writing this discussion is to check wild theories and speculations which are being advanced by other writers on the assumption that Lipman really found bacteria of great antiquity in anthracite.

REVIVING BACTERIA FROM ANCIENT SALTS

Anonymous; *New Scientist*, 29:89, 1966.

When H.J. Dombrowski of Freiburg, West Germany, claimed several years ago that it was possible to cultivate bacteria which had been enclosed in salt crystals for hundreds of millions of years, he started experiments to find out how such preservation of life could have been achieved. His tests have shown that bacteria which cannot produce spores will be killed when the nutrient media on which they live slowly dry out. A later addition of nutrient "soup" does not revive them.

If, on the other hand, more and more salt is added to the nutrient until the solution is supersaturated, and this liquid is then slowly dried until all the salt in it is completely crystallized, bacteria are trapped within the crystals. Such bacteria can be revived after seven years, recent tests have shown.

Bacteria could be revived from German salts 200 million years old, 260- and 360-million-year-old salts from Canada, 400-million-

year-old siluric salts from New York, and salts from Irkutsk dating from the lower Cambrian (500-600 million years ago). The revival of these half-billion-year-old bacteria has been filmed (Umschau, 1965, No.23, p. 734).

CURIOSITIES OF BEHAVIOR

• The Magnetic Sense

BACTERIA TURN INTO MAGNETS BY GROWING CRYSTALS

Anonymous; *New Scientist,* 81:187, 1979.

Can an individual cell be sensitive to the Earth's magnetic field, and if so how? Richard Blakemore, Richard Frankel and Ralph Wolfe, of the Department of Microbiology at the University of New Hampshire, now report in Science (vol. 203, p.1355) their first steps towards answers. They have isolated a bacterium from the wild, which will grow crystals of magnetite when cultured in an iron-rich medium. These crystals turn the cells into magnets!

The team found its bacterium in the mud below the water of Cedar Swamp, Woods Hole, Massachusetts. The bacterium--called spirillum and looking like a tiny helical balloon--consistently rotated to line up north-south. Characteristics other than its magnetic properties suggested it was a previously undescribed magnetotatic spirillium, which led to its liberated name of MS-1.

. . . .

SOUTH-SEEKING MAGNETOTACTIC BACTERIA IN THE SOUTHERN HEMISPHERE

Blakemore, R. P., et al; *Nature,* 286:384-385, 1980.

Abstract. Several species of aquatic bacteria which orient in the Earth's magnetic field and swim along magnetic field lines in

a preferred direction (magnetotaxis) have been observed in marine and freshwater sediments of the Northern Hemisphere. Their orientation is due to one or more intracytoplasmic chains of single-domain magnetite particles. These linearly arranged particles impart a net magnetic dipole moment to the bacterium, parallel to the axis motility. Northern Hemisphere magnetotactic bacteria with unidirectional motility swim consistently in the direction of the magnetic field, that is, to the geomagnetic North. This implies that their magnetic dipole is systematically orientated with the North-seeking pole forward. The magnetic polarity can be reversed by single, magnetic pulses of high field strength (1-2 μs, 300-600 G), and these bacteria then swim along magnetic field lines to the South. Due to the inclination of the Earth's magnetic field, magnetotactic bacteria which swim to the North in the Northern Hemisphere are directed downward at an angle increasing with latitude. It has been suggested that this downward-directed motion confers a biological advantage by guiding the bacteria, when dislodged, back to the sediments. On the basis of this hypothesis, magnetotactic bacteria of the Southern Hemisphere would be expected to swim to the South to reach the bottom. We report here several morphological types of magnetotactic bacteria present in sediments of the Southern Hemisphere. These bacteria indeed swim consistently to the South, hence downward along the Earth's inclined magnetic field lines, as hypothesized. As revealed by electron microscopy, they contain internal chains of electron-opaque particles similar to those observed in magnetotactic bacteria from the Northern Hemisphere. Like their Northern Hemisphere counterparts, their magnetic polarity can be permanently reversed and they cannot be demagnetized. We also report on Northern Hemisphere magnetotactic bacteria incubated in Southern Hemisphere magnetic conditions, confirming the biological relevance of downward directed motility.

• Methane-Utilizing Bacteria

PHYLOGENY: ARE METHANOGENS A THIRD CLASS OF LIFE?

Maugh, Thomas H., II; *Science,* 198:812, 1977.

Modern biology's concept that living organisms split into two major lines of descent more than 3.4 billion years ago may need some revision. Recent evidence from a team of investigators at the University of Illinois headed by Carl R. Woese and George Fox suggests that there is at least a third line of descent, and perhaps more.

The two lines of descent are prokaryotes, organisms such as bacteria and blue-green algae whose cells do not have a well-defined nucleus, and eukaryotes, more complex organisms whose cells do have a nucleus. Organisms can also be placed in these categories on the basis of similarities in metabolism and genetics. Members of the proposed third class, the Illinois group says, superficially resemble bacteria, but have genetic and metabolic characteristics that make them unique. These organisms are methanogens, anaerobic, methane-producing microorganisms that occur in places as disparate as the gastrointestinal tract of man and animals, sediments of natural waters, sewage treatment plants, and hot springs. They participate in the terminal stages of the degradation of organic matter, living on the carbon dioxide and hydrogen produced by anaerobic bacteria and converting them to methane.

Three main lines of evidence indicate that the methanogens are no more related to the bacteria than bacteria are related to higher organisms:

(1) The cell walls of methanogens do not contain muramic acid, the characteristic constituent of the peptidoglycans that form bacterial cell walls.

(2) The metabolism of methanogens is substantially different from that of bacteria.

(3) The methanogens have RNA sequences that are markedly different from those in other organisms.

. . . .

• Thermosynthetic Life

SMOKERS, RED WORMS AND DEEP SEA PLUMBING

West, Susan; *Science News*, 117:28-30, 1980.

In February and March 1977, a team of geologists and chemists led by John B. Corliss of Oregon State University of Corvallis made 24 dives to the rift in Woods Hole Oceanographic Institution's 23-foot-long research submarine Alvin. Their path was guided by about 70,000 photographs taken by a camera sled called ANGUS (Acoustically Navigated Geophysical Underwater Survey System) that is towed along the ocean bottom. To their delight, the researchers found five warm water vents. The plumes shimmered with heat--a maximum of 20°C--and a milky white precipitate formed in the rising fluid (SN: 3/19/77, p. 182). Unexpectedly, they also found a dazzling assortment of animals--red-tipped tube worms, giant clams, mussels, crabs,

seaworms, limpets and others--at depths that are usually sparsely populated (SN: 4/30/77, p. 279).

. . . . .

Perhaps the most unexpected discovery of Alvin's recent dives is the community of creatures associated with the vents. Two miles below the surface, the animals thrive where no light can reach. An analysis of the sulfurous vent water and the stomachs of some of the animals reveals the secret--sulfur-oxidizing bacteria. Apparently, say the researchers, this is the first community of animals not based on photosynthesis. And, though related species live elsewhere, "this is not the usual list of deep sea animals," says Robert Hessler of Scripps.

Several of the recovered specimens, such as the tube worms, the clams and the crabs, represent additions to their respective groups. Especially interesting are an apparently new family of polychaete worms called Pompeii worms because they live on the chimneys directly beneath the shower of precipitates. Several of the animals contain high amounts of hemoglobin, a possible adaptation, suggests one scientist, to the toxicity of the abundant hydrogen sulfide in the vents.

. . . . .

• Learning in Microorganisms

YOU DON'T NEED A BRAIN TO BE ABLE TO LEARN

Anonymous; *New Scientist,* 45:193, 1970.

Can microscopic single-celled animals learn? To put it more scientifically, do creatures normally classified as Protozoa show adaptive changes in their behaviour as the result of individual experience? This question has been puzzling zoologists for many years now, and though some investigators have claimed to have shown learning ability in certain Protozoa, their results have not been universally accepted; there were often alternative explanations for the behavioural changes. The latest claim to have observed learning in a Protozoan, however--this time involving a ciliate similar to, but smaller than, Paramecium, and called Tetrahymena pyriformis--seems to rest on a firmer basis. It comes from Dr. Sten R. Bergstrom, of the University of Uppsala (Scandinavian Journal of Psychology, vol. 9, p.215 and p.220; vol. 10, p.16 and p.18.

In his first series of experiments, Dr. Bergstrom tried to find out if the free-living ciliate, which normally pays no particular

attention to light, could be induced to avoid it. He exposed samples containing large numbers of Tetrahymena to light and, at the same time, gave them electric shocks. Tetrahymena usually swim fairly straight, using their cilia, but do show a typical "avoidance reaction" when they come up against some unfavourable stimulus. The reaction involves reversing the beat of the cilia, moving backwards a short way, then swinging around through an angle, and finally proceeding forwards again, in a different direction. Thus, if an avoidance reaction to light did develop, and the animals were placed in a partially illuminated chamber, a few of them should be found in the illuminated areas.

For the initial "training", Bergstrom put just over 1000 individuals of Tetrahymena in a small glass chamber, and gave them one of four treatments: a mild electric shock while simultaneously shining a light on them from below; light by itself; shock alone; and neither light nor shock. Then he placed a perforated screen under the chamber, between it and the light so that there were 15 illuminated areas, together occupying 30 per cent of the chamber's volume. Bergstrom next photographed the partly illuminated chamber every 30 seconds, to see what proportion of Tetrahymena failed to avoid the light areas.

Those animals which had experienced any of the last three treatments--light or shock alone, or neither--swam happily throughout the entire chamber, showing no tendency to avoid the illuminated areas. But those creatures who had experienced light and shock together clearly avoided the light, the proportion in the illuminated areas gradually decreasing over the first few minutes of testing, and then remaining fairly steady at a low level.

Bergstrom next tried to find out how many times an individual Tetrahymena needed to experience light and shock together before developing an avoidance response to light. Starting with a large number of individuals, he withdrew samples for examination after they had experienced 1, 6, 11, 16, 21, 26, or 31 pairings of light and shock, comparing them as before in the partly illuminated chamber. A real tendency to avoid light developed after 16 or more light-shock pairings. Checking on how long the "memory" lasted, Bergstrom found that the initial marked avoidance of light gradually waned as time went on, although it was still noticeable two hours later. Interestingly, the "memory" was not affected by division of the cells which occurred during this time.

Bergstrom claims to have established that, under the conditions of his experiments, Tetrahymena clearly manifest the ability to establish at least a temporary connection between an initially neutral stimulus (light) and their normal avoidance reaction to electric shock, when they experience the two stimuli together. Although this can presumably be classed as associative learning--which is, of course, shown by a wide variety of animals--Tetrahymena, being unicellular, has no nervous system. The mechanism behind the learning thus becomes a definite and tantalizing problem. It may well be of a chemical type; and the main significance of Bergstrom's work is that investigation of learning in such a small and relatively simple animal could lead to real progress in our understanding of learning and memory in larger and more complex animals--ourselves included.

• Communications between Cells

DISTANCE AS A FACTOR IN THE DEVELOPMENT OF ATTRACTION FIELDS BETWEEN GROWING TISSUES IN CULTURE

Katzberg, Allan A.; *Science,* 114:431-432, 1951.

A single explant of a tissue when cultured in vitro in either a fluid medium or a coagulum will usually produce a pattern of growth that is distributed radially about its circumference. It has been demonstrated, however, that when two explants are placed in the same culture, the resultant growth is often organized so as to form a pattern about the axis between the two explants. The term "attraction field" has been used to describe this phenomenon.

Examination of the plasma clot in the zone between the explants by means of polarized light revealed a birefringent pattern that resembled the lines of force of a magnetic field. Exploration of this area with the aid of a micromanipulator revealed that syneresis of the plasma clot had placed the fibrin strands under tension. There was also evidence of an adhesive force between the growing fibroblasts and the fibrin, as the cells could not be separated from the fibrin strands without breaking the latter. Mircoscopic examination of this zone at various periods during the development of the attraction fields showed that the fibrin strands underwent a gradual reorientation from a brush-pile pattern in the freshly prepared culture to an oriented pattern after the attraction field had formed.

. . . . .

From these data the conclusion is drawn that the intensity of the forces that initiate the development of the attraction fields for the directional orientation of the proliferating cells varies inversely as the square of the distance between the growing tissues.

"ATTRACTION FIELDS" BETWEEN GROWING TISSUE CULTURES

Weiss, Paul, and Katzberg, Allan A.; *Science,* 115:293-296, 1952.

Ghosts have a way of refusing to be laid. One such ghost is the alleged "attraction" for each other's cells supposedly exerted by two growing parts--for instance, two tissue fragments cultured in a common medium. More than 20 years ago I gave the first description of the striking phenomenon of an oriented

cell bridge forming, under certain conditions, between two growing centers; for convenience this may be referred to as the "two-center effect." At the same time, and repeatedly since, I have pointed out that the superficial impression of "attractions" (in the customary sense of the word) being at play is a sheer illusion. The correct interpretation, gained by stepwise analysis of the factors involved, has been amply documented and published. It has found even wider currency through the publications of other authors.

It is somewhat perplexing, therefore, to find in a recent article in Science (114, 431 [1951]) the whole phenomenon rediscovered, redescribed and, by the implications of the terms used, again misinterpreted. In the article in question entitled "Distance as a Factor in the Development of Attraction Fields between Growing Tissues in Culture," the author, Allan A. Katzberg, states that "the term 'attraction field' has been used to describe this phenomenon." This term is absolutely inappropriate and misleading. Its reaffirmation by the author mars what is otherwise a correct, if not altogether novel, presentation. It matters little that the author is only dimly aware of the systematic work that has been done in this field before. After all, his observations fully confirm the known facts. But the way in which he treats them is apt to lead back to the same old confusion from which our analysis had shown the way out. Therefore, and because of the rather broad implications of the two-center effect for morphogenesis in general, it seems desirable to restate the case briefly. (Paul Weiss)

. . . . .

A short article of this nature does not permit lengthy historical reviews of previous work or of the precise techniques employed, and so the description of the development of this complex mechanism was compressed into one or two short sentences. For instance, the sequence of events entailed in the statement "Each tissue explant may be considered to set up a mechanism that acts as a stimulus for the oriented pattern of growth for its own cells, as well as the cells of the other explant that shared in the development of the field" can be superimposed on the more lengthy outline of events tabulated by Dr. Weiss. Certainly the growing explants induce syneresis of the fibrin matrix which, in turn, produces tension that reaches its maximum in the common axis between the explants, thus establishing a preferred and oriented pathway for the proliferating cells of both explants. No other explanation was considered, and no inferences were made to a possible "mutual attraction" or that the cells were "pulled over by traction or lured over by mysterious attractions." These two phrases not only are entirely foreign to the article but definitely distort the purpose of it.

If, as Dr. Weiss claims in his letter, "By no means are they pulled over by traction or lured over by mysterious 'attractions'," why do we read in his fascinating chapter on "Differential Growth", under the topic of "Orientation of Growth," in which the orientation of growth between two centers of contraction is

discussed, the following statements (p.176):

> By its radial orientations such a contraction pattern can evidently guide peripheral cells toward the center. This is one of the major mechanisms by which cells are drawn toward distant destinations, as if "attracted." [and in the next paragraph], tension is not the only agent capable of orienting tissue, structure. Any other physical force that is capable of affecting the orientation and aggregation of popular molecules (electrostatic fields, electrophoresis, streaming, etc.) may have comparable effects.

It was specifically this section of the chapter that made this writer especially cautious and on guard when he drafted the recently published article, so as not to include words or phrases that could be misinterpreted and that would present impressions for which he had no data. And nowhere in the article was it intimated that this was a new discovery of the phenomenon. That was clarified in the first paragraph. (Allan A. Katzberg).

. . . .

PROTEINS HAVE EXPORT PROBLEMS TOO

Freedman, Robert; *New Scientist,* 81:376-378, 1979.

To microscopists, a living cell, with its amazing internal complexity and its multiplicity of distinct components, is a potential image of beauty and wonder. But to a biochemist, dedicated to the analysis of such a cell in terms of its molecular composition and chemical functions, it is even more awe-inspiring and puzzling. For each sub-cellular compartment, each internal structure carries out a defined set of functions and these functions are more or less determined by the proteins present. This makes it clear that the manufacture and transport of proteins is one of the cell's most important tasks. And recent work is beginning to hint at the means by which proteins are directed to their "proper" places in the cell.

At first sight it might seem convenient for every sub-cellular entity to manufacture its own proteins. But this would require that these entities all contain the machinery for making proteins in addition to the equipment needed for their own specialised functions. In fact, although a very small number of proteins are made "on site", most of the proteins required for the functions of all sub-cellular structures are made in the cytoplasm, the area between the nucleus and the cell's outer wall. Proteins required in the mitochondria to catalyse oxidation of foodstuffs, proteins contained inside lysosomes and responsible for "intra-cellular digestion"---all are manufactured by ribosomes located in the cytoplasmic compartment.

Of course this raises a major problem of logistics. How is it ensured that the different proteins move reliably to their proper cellular destinations? And what of the numerous types of proteins

which cells manufacture and then secrete? We have only to think of the digestive enzymes of the gut, the antibodies, hormones and bloodclotting proteins in the blood-stream, and the extracellular structural proteins such as collagen, to realise what an enormous problem this is. And it is not confined to the obviously complicated multicompartment cells of higher organisms. Even bacteria face it to some extent.

Many bacteria are surrounded by an inner membrane beyond which is a complex outer membrane. Between the two is a region called the periplasmic space. These localities each contain different and characteristic sets of proteins. Even some viruses, usually thought of as the simplest form of life, have to ensure that newly synthesised viral proteins go to the correct destinations in the cells they infect (their host cells), so that new virus particles can be constructed.

Any number of speculative schemes could be designed to explain how a cell carries out the major task of protein distribution. But it is now beginning to appear as if a single strategy is used in all these various situations. The strategy involves sections of proteins known as signals.

The evidence for this notion came first from studies on animal cells that manufacture proteins and then export them. Cells which secrete a lot of protein contain an extensive intracellular membrane known as the endoplasmic reticulum. In these cells, many of the ribosomes (the structures on which proteins are synthesised) are attached to one surface of the endoplasmic reticulum, rather than being simply free in the cytoplasm. Studies with

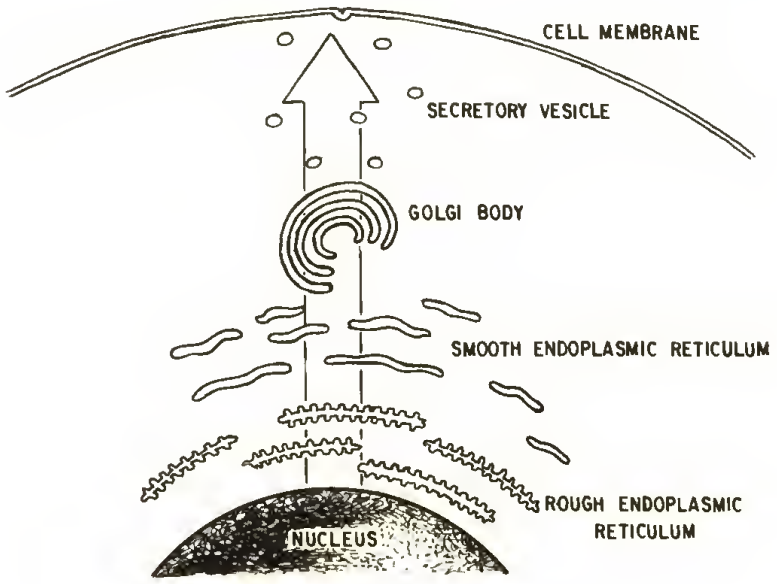


Diagram showing the route taken by proteins on their way from point of manufacture to point of export from the cell

radioactive tracers show that proteins to be secreted first appear inside the space created by the endoplasmic reticulum and then follow a defined route via the Golgi complex before being secreted.

For secreted proteins the mechanism of distribution is clear, but biologists realised recently that there was a second informational aspect of the problem which remained mysterious: What sorts out the external from the internal products? What is the cellular analogue of the internal and external mail trays? Does some cellular post-person make an individual judgement on each package? Or are the external products already stamped?

[Details of specific experiments omitted.]

So the general significance of the "signal" mechanism for the distribution of newly synthesised proteins is established beyond doubt. But three important problems remain. First, what are the differences between the signals operating in a single cell and ensuring despatch of different proteins to their characteristic destinations? To answer that will require structural analysis of precursor forms of a large number of varied proteins. Secondly, what is the actual mechanism which takes a protein out of one cellular compartment, across a membrane and into another compartment? For secreted proteins we have the clue that the signal sequence is highly hydrophobic (soluble in fat), so it is easy to envisage how this sequence buries the leading end of the protein into the endoplasmic reticulum membrane. But beyond that any mechanisms describing the translocation of the whole protein are purely speculative.

Finally, there is the intriguing question of the nature and location of the processing reactions in which the various signal sequences are excised. For the secretory proteins there is good evidence that the endoplasmic reticulum must be present while the protein is being synthesised in order that correct processing can occur: for instance, in a laboratory experiment it is not sufficient to translate the message RNA in a test-tube system and then add fragments of the membrane. Furthermore, if vesicles made from the endoplasmic reticulum are present during translation, the newly synthesised protein ends up inside them. In other words, in such cases, the synthesis of the protein is accompanied by its movement across the endoplasmic reticulum and its processing to remove the signal sequence.

. . . . .

• The Peculiar Phenomenon of Cell Fusion

CELL FUSION: DOES IT REPRESENT A UNIVERSAL URGE TO 'JOIN UP'?

Thomas, Lewis; *Science Digest*, 86:52–54, December 1979.

My favorite is cell fusion. I had always taken it for granted that the cell was the most private and exclusive of all biological entities, and it was a flabbergasting surprise to be told that there are circumstances under which two different cells, touching each other, will dissolve the structure of their membranes at the place of touching, their contents will flow into each other, and then their separate nuclei, which contain all the private DNA coding for the specific individuality of each cell, will fuse. And that the end result is now a single cell, bounded by a single membrane, containing a single nucleus and all the genetic information needed for being both kinds of animal. It is one of the strangest events in all of nature. Sounds like he got laid.

You can do this with pairs of cells from totally unrelated species: a mouse cell will fuse with a human cell to form that sort of single-cell hybrid. What is more astounding, the hybrid cells will then divide, producing colonies of new hybrid cells. It is a fallible process, as you might expect; as the new, unnatural cells divide, they tend to cast off one chromosome or another and you may end up with a population of cells restored to the genetic endowment of one line or the other. But sometimes the hybrids are stable, replicating endlessly as a combined chimeric form.

There is, of course, a practical and quickly useful side to the phenomenon. Antibody-forming lymphocytes, for instance, can be fused to cancer cells. These hybrid cells, called "hybridomas," will endlessly produce whatever antibody you like. For this reason, they are becoming commercially more attractive than rabbits.

But there is also a more puzzling aspect of the phenomenon, about which nothing can be done except to wonder. Why on earth should it exist at all? What conceivable purpose is served by a mechanism so tidy and reproducible, but at the same time seeming to be such an outright violation of nature? The different cells seem to want to fuse together, and they do this in the face of all the laws whose existence we assume must have always been on the books to produce a planet with millions of discrete, separate, and wildly different species.

. . . . .

• Cooperative Action among "Low" Forms of Life

THE AMOEBA—A SLIME MONSTER AND ITS VICTIM

Beard, J. Carter; *English Mechanic*, 77:543, 1903.

The other day, when amusing myself with my microscope, a catastrophe occurred in the field of vision, which is quite equal in its tragic elements to, and much more wonderful than, anything I have ever seen without the aid of my instrument.

I had under inspection something that looked like a bit of slime. It had neither head, body, nor limbs, nor any division of parts. It had no more apparent organisation than the drop of slime or jelly it so much resembled, yet it, without doubt, possessed animal life and power of movement.

It is called the amoeba, and is probably one of the strangest objects in existence. As I watched it through the glass it began to progress slowly in one directon. Instead of crawling like a worm or a snail or creeping like an insect, it simply flows, being a sort of liquid animal. No other word can express its motion. First it throws out projections, or false limbs or feet, as they are called. This it can do in any given number and from any part of its substance. Then it ran its entire substance into these projections. Having collected itself, with a more literal reference to the meaning of the term than that in which it is commonly used, the amoeba is ready to repeat the process and advance another step. Perhaps the nearest and best illustration of how this is managed is furnished by a bit of water making its way down the inclined lid of a desk; the small currents or splashes it sends ahead of the main body answer to the pseudopodia, or false feet, of the amoeba, and its alternate filling up of these small channels and its bursting forth, and sending out new ones almost exactly parallel the progress of the animal. There is this difference, however--liquid can move only down inclined surfaces, while the amoeba is enabled by some incomprehensible agency to move along the level or even ascend inclined planes.

The creature has no heart, brains, blood, nervous system, or muscles, and yet it seeks, pursues, and captures and devours its prey, and seems to have a mind and will of its own, and to enjoy life fully.

As I was watching the ever-changing shapes assumed by the amoeba in its progress, my attention was called to an object close at hand which I had not before noticed. This was the most delicate, fairy-like little sylph it is possible to imagine--a sort of a living top or iridescent crystal, flashing prismatic rays as if it inclosed a tiny rainbow, as it stood spinning in the water backward and forward on its stem. I knew it for a urocentrum, a more highly organised animal, if that term is applicable in contrasting it with an animal like the amoeba with no organism at all.

I watched the slime monster with redoubled interest. It was certainly gliding along in the direction of the fairy top. How it was conscious, without ears to hear, eyes to see, or nostrils to smell, of the presence of the urocentrum, I must leave to far more learned and capable physiologists to make intelligible. I cannot even suggest an explanation. It was sufficiently evident that the urocentrum was its point of attack. There could no longer be any doubt. The pseudopodia at last touched the living top; then they encircled it. The urocentrum seemed to be aware of this, and moved restlessly in its prison. After a while, however, as if it received some paralysing shock, the fairy top ceased to spin; the ends of the pseudopodia are fused together, and the slime monster flowed over and engulfed the little creature, which is its manner of swallowing its victim. Thus having pursued its prey without the aid of limbs, and devoured it without a mouth, the amoeba proceeded to digest it, although stomachless.

If anything related to the animal can be stranger than the peculiarities embodied in the foregoing narration, it is to be found in the complete breaking up of one animal and the production thereby of a number of baby amoebae, which when united formed the parent. When this remarkable change is about to happen, the amoeba ceases to move or take food and forms about itself a thick shell or covering. As an egg hatches the shell bursts, and the amoeba is found to have resolved itself into a number of little balls, each ball a perfect amoeba, and quite able to set up in business for itself. Stranger still, they sometimes conclude to unite forces, and by coalescing or flowing together, again become an animal.

A THEORY OF THE CONTROL OF THE DIFFERENTIATION IN THE CELLULAR SLIME MOLDS

Bonner, John Tyler; *Quarterly Review of Biology,* 32:232–246, 1957.

In the life history of cellular slime molds separate independent amoebae aggregate in masses, and each of these masses proceeds to develop as a unified individual. Some of the amoebae within the mass differentiate into stalk cells, and the remainder differentiate into spores. Depending upon how many amoebae enter the aggregate, there is a wide variation in the size of the fruiting bodies. Yet there is a constant relationship between the number of stalk cells and the number of spore cells. This problem of proportional development is of basic importance not only in the slime molds but in all organisms exhibiting regulative development.

My colleagues and I have ascertained many pertinent facts on this proportionality of development in the slime molds in earlier work, and some new orginal information will also be included here. From this whole array it is possible to construct a theory which ties these facts together. There is no assurance that the theory represents the actual mechanism of development, and certainly

alternative theories could be proposed that would fit the facts. This particular theory, however, has the merit of simplicity, and does serve to illustrate what kind of mechanism could conceivably be operating in the development of these organisms.

Before describing the theory it will be well to review some of the known facts, and to present some original material that will serve as a basis for the theory. But since there may be some readers who are not completely familiar with the cellular slime molds, I shall first discuss briefly the life history of Dictyostelium discoideum and then compare it with some related species.

The spores of D. discoideum are covered with a hardwalled capsule. Upon germination each capsule liberates one amoeba. The amoebae divide mitotically and remain entirely separate from one another, feeding independently upon bacteria. When they multiply to a population of sufficient numbers, they stream together to form large collections of many cells or pseudoplasmodia, and this aggregation process appears to be largely due to a chemical substance, acrasin, to which the amoebae are chemotactically sensitive. Certain of the amoebae, which form the center of the aggregate, apparently produce it sooner than others, and in this way an acrasin gradient is set up which is effective in orienting the amoebae.

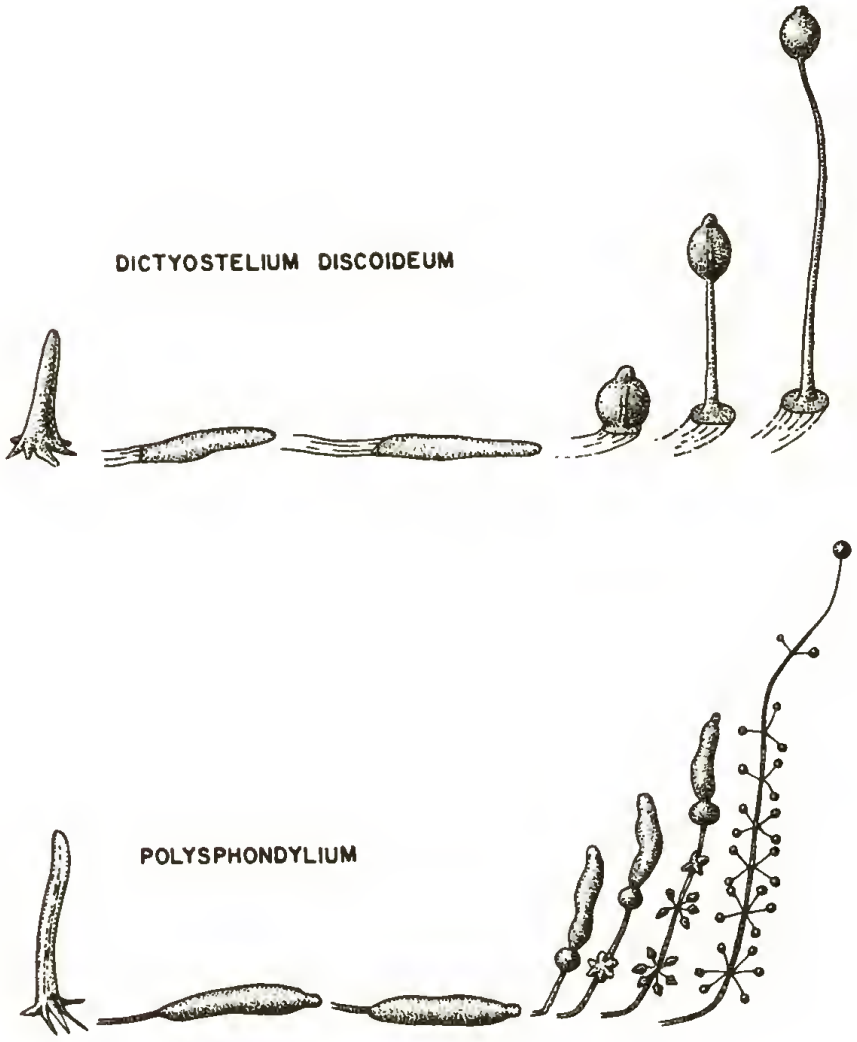
The aggregated cell mass assumes a sausage shape and crawls about the substratum for variable periods of time. During this migration phase it is sensitive to light and heat gradients, orienting itself toward light and toward warmer regions. Differentiation begins at this stage. The anterior cells of the sausage are destined to become part of the supporting stalk and the posterior cells will turn into spores. The final fruiting involves a series of morphogenetic movements in which the anterior presumptive stalk cells are pushed down through the spore mass, and in so doing, these stalk cells become large and vacuolate and are permanently trapped in a delicately tapering cellulose cylinder. During this culmination stage, the spore mass is lifted up into the air and each amoeba in the mass becomes encapsulated into a spore.

. . . .

Summary. A basic problem in the study of the development of slime molds is an explanation of their proportional development. It has been possible to work out a theory which satisfactorily accounts for the many experimental observations that have been gathered on this subject.

Differentiation begins in the sausage-shaped cell mass, and from previous work it is known that there are certain specific chemical differences between the anterior, pre-stalk end and the posterior, pre-spore end. Furthermore, from new observations presented here, it is now known that these presumptive areas are proportionate in cell masses of different size and even in parts of a cell mass that has been cut into fractions.

Clearly, then, a cell in the anterior end "knows" how many cells lie posteriorly, for if they are removed experimentally the anterior cells accommodate proportionately to the new loss. The



The migration and final fruiting stages of two types of slime molds

theory assumes that this information is carried by a polar movement; in fact, it specifically suggests that certain fast cells are constantly arriving at the tip, and if there is a reduction of the posterior portion then there is a corresponding reduction of these messenger cells.

The "messenger" carried may be simply a supply of food necessary for a reaction taking place at the anterior end. This anterior reaction is the one which is responsible for the formation of the pre-stalk and stalk condition. In any one species, then, there is a balance between (1) the rate of delivery of the polar factor to the anterior reaction and the rate at which this reaction produces (2) the stalk and (3) pre-stalk portions. Different

species and strains have different proportionality characteristics, and these differences could be accounted for by differences in the rates of the three above-mentioned processes.

In considering the evolution of such a mechanism of differentiation, the first step might have been an increase of variability of the cells within the cell mass with respect to their stalk-forming and spore-forming abilities. Since the cell movement in such masses is polar (presumably because of the acrasin mechanism and pull tensions among the amoebae), the fast and slow cells sort out, and such concentration of one cell type has led to a facilitation or an amplification that is the anterior reaction. Therefore, random variability in a cell mass has ultimately, according to this hypothesis, produced a discontinuous, proportionate cell difference that has all the qualities of a controlled pattern of differentiation.

WHY DOES LIGHT MAKE SLIME MOULDS MOVE?

Anonymous; *New Scientist*, 24:801, 1964.

The migrating "slugs" of the slime mould Dictyostelium--aggregations of initially separate, amoeba-like individuals--will respond by moving towards light. A botanist at the University of Wisconsin has recently tried to find out just how light causes them to change direction. (D.W. Francis, Journal of Cellular and Comparative Physiology, Vol. 64, p.131.)

He discovered that the intensity of light needed to produce a response was far too low for temperature differences on the two sides of the "slugs" to play any part in the effect. When he carried out experiments with light of different wavelengths, both D. discoideum and D. purpureum showed response peaks at about 375 and 550 millicrons, respectively. Neither of the possibilities that the amoebae contained a photosensitive pigment, or that some characteristic of the outer slime sheath of these colonies of animals brought about the response, was borne out after spectrographic studies, however.

One factor definitely involved is the "lens effect"--the light intensity is stronger on the side of the "slug" away from the light because of focusing by the cylindrical surface of the protoplasm. A light shining from vertically above on one side of the tip of a "slug" made it turn away from this side. It is not known how this increased light intensity causes turning, though it might be by inducing individual amoebae to move faster on one side.

CELLULAR SAMARITANS

Chedd, Graham; *New Scientist*, 40:256-257, 1968.

Living cells are rather selfish individuals, even those--members of a multicellular organism--which have to subjugate themselves to the organism as a whole. But in the last few weeks, two papers have been published which report very different examples of an unexpected, and remarkable, "help-your neighbour" cooperation among cells. In both instances, a cell which is metabolically incompetent to deal with a given situation is helped out by contact with a neighbouring, competent, cell. One report concerns like cells, cells of the same "race". The other is apparently a straight case of "Good Samaritanism": cells of one species aiding cells belonging to a different race altogether.

The like-helping-like report involves an experimental system in which human cells lacking the ability to carry out an essential synthetic step in metabolism are aided by normal cells in contact with them. This, although a previously unknown process, does not strain credulity too far. But the second report is astonishing. Its author claims, on impressive evidence, to have found an interaction between cells of a bacterium and a barley plant. In crude terms, once a bacterium, lodged, say, against the root of the barley plant, has learnt to cope with a new factor in its environment, it can pass the fruits of its experience on to the plant so that it too can benefit. Although both reports concern radically different systems, and the mechanism concerned in the two processes may turn out to be very different, both apparently involve the transfer of information from one cell to another.

. . . . .

The author of the bacteria-to-plant report is a peripatetic Norwegian biochemist, Per Nissen, who can presently be found at the Agricultural College of Norway, Vollebekk. The enzyme concerned in this case is choline sulphate permease, which enables those cells containing it to take up choline sulphate from their environment. The bacterium P. tolaasi, will synthesize choline sulphate permease, provided it is placed in an environment containing choline sulphate; in other words, the enzyme is inducible, only being made when it will be of some use to the bacterium. Barley contains very low levels of choline sulphate permease, but, in contrast to P. tolaasi, the presence of choline sulphate does not normally induce the barley cells to synthesize the enzyme: if barley roots are placed in a solution of choline sulphate, they do not take it up from solution at a very fast rate. Nissen's unlikely discovery is that barley roots placed in a choline sulphate solution in the presence of P. tolaasi will take up the chemical very rapidly indeed (Biochemical and Biophysical Research Communications, vol. 34, p.696). Moreover, he has shown that the bacteria themselves must be induced to produce choline sulphate permease, and that the bacteria must be

in contact with the roots. Like Subak-Sharpe, Nissen is not yet sure what is the material that is transferred from bacteria to plant. He too suggests a nucleic acid or a regulator substance.

Apart from its intrinsic interest, Nissen suggests that this previously unknown transfer of information may play an important role in nature: bacteria in the soil surrounding plants may confer some of their metabolic versatility and adaptability to the plant roots. Of even more potential importance, on a practical level, Nissen concludes that it may be possible to extend the metabolic capability of plants--perhaps persuade them to grow in all kinds of unlikely substrate--by employing appropriate bacteria as helpmates.

• Disease and Astronomy

CHOLERA AND SUN SPOTS

Anonymous; *Scientific American,* 26:376, 1872.

Mr. B.G. Jenkins recently read, before the Historical Society of London, a remarkable paper on cholera, in which he maintained that the disease is intimately connected with auroral displays and with solar disturbances. "I believe that I am able to show that a remarkable connection exists between the maxima and the minima of cholera epidemics and of solar spots. You are all probably aware that the great astronomer Schwabe discovered that the sun spots have what is called a ten year period; that is, there is a minimum of spots every ten years. It was also discovered that the diurnal variation in the amount of declination of the magnetic needle has a ten year period. The same was proved in regard to earth currents, and also aurorae. The maxima and minima of the four were found to be contemporaneous. This was a great result; but Professor Wolf, on tabulating all the sun spots from the year 1611, discovered that the period was not ten years, but 11.11 years. This period is now the accepted one for the sun spots; and it has been established for the magnetic declination, and by Wolf for the aurorae. Now, it is a curious fact that the last year of every century, as 1800, has a minimum of sun spots, so that the minima are 1800, 1811.11, 1822.22, 1833.33, etc. The maxima do not lie midway between the minima, but anticipate it by falling on the year 4.77 after a minimum; for example, 1800 was a minimum year, then 1804.77 was a maximum year. Now, cholera epidemics have, I believe, a period equal to a period and a half of sun spots. Reckoning then from 1800, we get as a period and a half the date 1816.66, which was shortly before the great Indian outbreak;

another period and a half gives 1833.33, a year in which there was a maximum of cholera; another, 1866.66, a year having a maximum of cholera; another, 1883.33, as the year in which there will be a cholera maximum. It follows from what has been already said that 1783.33 would be a year in which cholera was at a maximum. Now it is a fact that in April 1783 there was a great outbreak of the disease at Hurdwar.

I am not, however, prepared to say that sun spots originate cholera; for they may both be the effects of some other cause, which may indeed be the action of the other planets upon the earth and upon the sun.

My own opinion, derived from an investigation of the subject, is that each planet, in coming to and in going from perihelion--more especially about the time of the equinoxes--produces a violent action upon the sun, and has a violent sympathetic action produced within itself--internally manifested by earthquakes, and externally by auroral displays and volcanic eruptions, such as that of Vesuvius at the present moment; in fact, just such an action as develops the tail of a comet when it is coming and going from perihelion; and when two or more planets happen to be coming to or going from perihelion at the same time, and are in, or nearly in, the same line with the sun--being, of course, nearly in the same plane--the combined violent action produces a maximum of sun spots, and in connection with it a maximum of cholera on the earth. The number of deaths from cholera in any year--for example, the deaths in Calcutta during the six years 1865-70--increased as the earth passed from perihelion, especially after March 21, came to a minimum when it was in aphelion, and increased again when it passed to perihelion, and notably after equinoctial day; thus affording a fair test of my theory."

SUNSPOTS AND EPIDEMICS

Anonymous; *Meteorological Magazine,* 63:266, 1928.

On p. 47 of the present volume of the Meteorological Magazine we reviewed a paper by A. Tehijevsky in which it was claimed that historical events tend to occur more frequently near sunspot maximum than near sunspot minimum. He has now contributed a further study in which the occurrence of epidemics of cholera and influenza is related to the sunspot cycle. The relationship is found most clearly with the cholera epidemics, for of fifteen which occurred between 1766 and 1900, no fewer than twelve coincided with a sunspot maximum. The relationship of influenza to sunspots is less clear, for though this malady shows indications of a periodicity of 11.3 years, its outbreaks come not exactly at sunspot maximum but at an average interval of 2.3 years before or after.

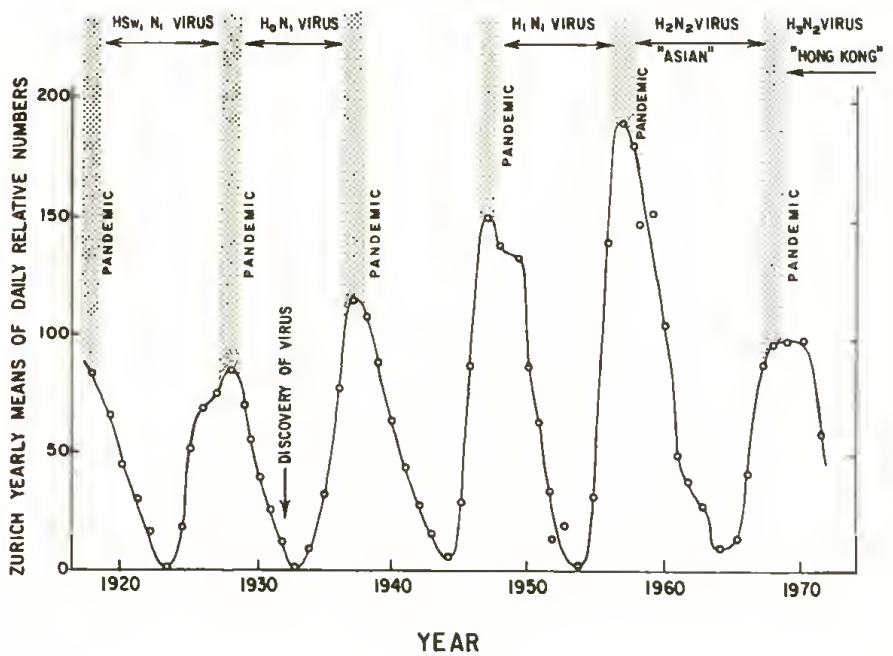
The way in which the variations of solar activity take effect is not yet clear, but the author considers that it is probably

through their influence on atmospheric electricity and especially potential gradient, which may affect both the bacteria themselves and also the power of humanity to resist disease.

SUNSPOTS AND FLU: A CORRELATION

Hope-Simpson, R. E.; *Nature,* 275:86, 1978.

Seasonality exerts a profound but ill-understood influence on epidemic influenza. North of the tropic of Cancer and south of Capricorn epidemics usually break out in the winter when the relevant protion of the globe of the Earth is most distant from the Sun. The purpose of this note is to call attention to a longer-term association of type A influenza with solar activity that has become apparent since human influenza virus was discovered in the epidemic of 1932-33, namely that the periods of world-dominance of successive major subtypes of influenza A virus have synchronised closely with the periodicity of sunspots (see Figure). The two sets of cycles are probably synchronised at least as far back as 1917.



Sunspot cycle peaks often seem synchronized with flu outbreaks.

Influenza pandemics have occurred in the twelve months after type A influenza virus has undergone an antigenic shift leading to a new major subtype. The new major variant with altered haemagglutinin (H) and neuraminidase (N) finds the population inadequately protected by influenza caused by its predecessor. The change has enabled it to bypass their immunity. The predecessor promptly disappears and the new major variant with its minor varieties dominates the world causing all the type A influenza for the next decade or so until itself displaced by the next antigenic shift. The major subtypes since 1933 have been identified serially according to H & N composition as H_0N_1 1947-1957, H_1N_1 1957-1968, H_2N_2 1968-1978. Each major subtype completely disappeared at the next antigenic shift except that H_2N_2 viruses reappeared in many parts of the world in 1977 and are still (28 July 1978) causing influenza concurrently with H_1N_1 viruses. The pandemic of 1918 is usually attributed to antigenic shift producing the major subtype HSw_1N_1. A small outbreak caused by virus of this major subtype recurred in 1976.

Sunspots undergo cycles of activity varying from seven to seventeen years, average eleven years. Activity increases to maximum more rapidly than it declines to minimum, and maxima sometimes stand upon a plateau of several years of approximately equal activity. The true solar sunspot cycle consists of two consecutive cycles of minimum to minimum activity (see Gibson, Edward G. Rev. Geophys. Space Phys. 10, 395-462; 1972). Antigenic shifts of influenza A virus to H_1N_1, H_2N_2 and H_3N_2 coincided with sunspot maxima of 1947, 1957 and 1968 respectively. The sunspot maximum of 1937 coincided with severe and widespread influenza, but no antigenic shift was recorded. The sunspot maximum of 1928 conincided with a pandemic which may have signalised the antigenic shift from HSw_1N_1 to H_0N_1 virus. The previous sunspot maximum of 1917 anticipated the pandemic of 1918-19. Fascinating as it is to attempt to investigate the association throughout the centuries of sunspot records, influenza records are probably too dubious to make the attempt worthwhile. The next sunspot maximum is however to be expected shortly, perhaps in 1979. The behaviour of type A influenza virus during the next few seasons will be watched with keen interest tinged with apprehension.

STYLONICHIA MYTILUS AND THE LUNAR PERIODS

Gray, Ernest; *Nature*, 167:38, 1951.

During a recent investigation at the University Botany School, Cambridge, of the association of ciliate protozoa and bacteria in a chalk stream, Stylonichia mytilus in common with other ciliate protozoa exhibited a series of 'pulses', or maximum waves when it spread down the stream, and minimum phases when it could be found only in the upper reaches. Forms in binary fission or

conjugation were sometimes noted at the end of the maximum phase, whereas the minimum phase was generally marked by the appearance of small forms. The pulse of Stylonichia could not be related to any constant climatic or physical factor (rainfall, pH, temperature, etc.) or to total bacterial numbers at 22°C. or any particular group of bacteria. However, the maximum phase often occurred about a week to ten days after one of the lunar periods. From May to December 1947, the maximum phases occurred on twelve out of thirteen occasions a week to ten days after a new moon or a full moon; the greatest maximum peak was attained nine days after a new moon in July and four days after a full moon in December. By contrast with the dry weather and clear nights of 1947, the year 1948 was wet and dull. Stylonichia was only recorded between May and September, yet on five occasions it reached its maximum phase on an average eight days after a new or a full moon.

. . . .

ANOMALOUS DISTRIBUTION IN TIME AND SPACE

• Earliest Life Forms Seem "Too Advanced"

COMBINED STRUCTURAL AND CHEMICAL ANAYSIS OF 3,800-MYR-OLD MICROFOSSILS

Pflug, H. D., and Jaeschke-Boyer, H.; *Nature,* 280:483-486, 1979.

Cell-like inclusions detected in the cherty layers of a quartzite, which is part of the Isua series in South-west Greenland, consist of biological materials, according to analyses by Raman laser molecular microprobe. The available radiometric data place the age of the sequence at around 3,800 Myr. Thin sections of our specimens were taken in their primary positions within the rock matrix. The material used was compact and unweathered. No maceration, etching, impregnation or other methods were applied which might have produced artefacts.

About 100 well preserved specimens noted in the material were

sealed within the quartz grains or in the silica cement in a three-dimensional condition. This sealing apparently results from a synsedimentary permineralisation caused by colloidal silica, which subsequently crystallised to form chalcedony. Our observations suggest that these specimens are primary constituents of the sediment, and are not contaminants from a later time.

The fossils occur as individual unicells, filaments or cell colonies. Cells and cell families are usually surrounded by multilaminate sheaths which show a characteristic laminar structure. All specimens observed apparently belong to the same kind of organism, named Isuasphaera.

The individual cells are more or less ellipsoid in shape. The mature cells range between 20 and 40 µm in diameter. The cell encloses a more or less globular hollow which is partly filled with organic matter. Apparently, this filling is a remnant of the former protoplasm which has been degraded during fossilisation. A vacuole is often contained in the cell lumen. (p. 483)

There is little doubt that Isuasphaera is an organism. This is indicated by the morphological and chemical details. A problem results from the similarities between Isuasphaera and recent yeasts. If we conclude that yeasts existed during Isua times, life must be much older than 3,800 Myr. According to our present knowledge, the Earth's sphere formed about 4,450 Myr ago. Consequently biophile conditions, implying a solid crust, cool temperatures and liquid water cannot have been realised on Earth earlier than about 4,400 Myr ago. The time span of roughly half a billion years, however, appears too short for the evolution from a simple organic compound to a eukaryotic organism to have occurred. (p. 485)

AUSTRALIAN FOSSIL: THE DAWN OF LIFE?

Anonymous; *Science News*, 117:406, 1980.

In the beginning there were bacteria; the major question has been when was the beginning? Until recently, the date established for the first life forms was 2.3 billion years ago. Then, the discovery of fossil imprints in sedimentary rocks pushed the date back to 3.5 billion years (SN:4/12/80, p.229). Now UCLA paleobiologist J. William Schopf reports the first direct identification of the actual bacterial cells that produced the ancient imprints. The chainlike fossils, magnified 2,100 times, represent at least five types of microbial forms of life, according to Schopf. They were contained in fragments unearthed from a desolate region of northwest Australia by UCLA and Australian scientists. The fossils "demonstrate that the surface of the earth was habitable some three and a half billion years ago, and that it was populated by numerous types of relatively advanced bacterium-like forms of life," Schopf says. Less conclusive evidence of life in 3.8-billion-year-old rocks from Greenland was previously

reported (SN:9/15/79,p.229), but those findings have been challenged as representing remnants of "fluid inclusions" or bubbles that were not of biological origins.

• Supposed Creation of Life from Lifeless Matter

LIFE FROM ROCKS

Gardner, Martin; *Fads & Fallacies,* Dover Publications, New York, 1957, pp. 116-122.

The long abandoned doctrine of abiogenesis, the spontaneous generation of living forms from non-living matter, has always been a promising field for the modern pseudo-biologist. In 1836, an English amateur scientist named Andrew Crosse, puttering around with electrical experiments on his estate in the Quantock Hills, was startled to find microscopic insects appearing as a by-product of his research. He had been trying to produce artificial minerals by sending an electrical current through certain chemical mixtures. Here is Crosse's description of what he observed through his microscope: *

> On the fourteenth day from the commencement of the experiment I observed through a lens a few small whitish excrescences, or nipples, projecting from the middle of the electrified stone, nearly under the dropping of the fluid above. On the eighteenth day these projections enlarged, and seven or eight filaments . . . made their appearance on each of the nipples. On the twenty-second day these appearances were more elevated and distinct, and on the twenty-sixth day each figure assumed the form of a perfect insect, standing erect on a few bristles which formed its tail. Till this period I had no notion that these appearances were any other than an incipient mineral formation; but it was not until the twenty-eighth day, when I plainly perceived these little creatures move their legs, that I felt any surprise, and I must own that when this took place I was not a little astonished. . . .

About a hundred insects were born in this manner. Crosse reported that the smaller ones seemed to have six legs and the larger ones eight. He guessed them to be of the genus Arcurus (mites), "but of a species not hitherto observed." Eventually they left the

---

* See p. 714 for more on Crosse's experiments.

liquid entirely, and flew about the laboratory, hiding in dark spots as though avoiding light. Later experiments produced the mites in great numbers. Another amateur by the name of Weeks, who lived in Sandwich, repeated Crosse's experiments, taking great precautions to keep air-borne eggs from entering the liquid. He not only found the same mites, but also noted that their numbers varied directly with the percentage of carbon in his solutions.

The details of Crosse's research may be found in the Memorials of Andrew Crosse, by Mrs. C. H. A. Crosse, 1857; A History of the Thirty Years Peace, by Harriet Martineau, 1849; and Lieutenant Commander Rupert T. Gould's Oddities, a Book of Unexplained Facts, 1928.

In 1872, another English scientist, Henry Charlton Bastian, created an academic tempest with his two-volume work, The Beginning of Life, Bastian used the term " heterogenesis" for the process of spontaneous creation he believed was taking place constantly in nature, and which he claimed to have witnessed in his microscope. When colleagues repeated his experiments, however, taking better precautions against air bacteria, the results were disappointing.

In 1906 John Butler Burke, also British, published a similar work titled The Origin of Life. Using radium, Burke was able to produce primitive artificial forms---half living and half crystalline ---which he called "radiobes." This work also created a stir at the time, but eventually was forgotten after other scientists failed to confirm Burke's findings. *

Much more astonishing than the work of any of the previously mentioned men were the discoveries of Morley Martin, an amateur biochemist who began his strange adventures with the microscope in 1927 at his privately financed laboratory in Andover, a village in Hampshire, England. Briefly, what he did was this. He took a piece of Azoic rock (the Azoic era is the earliest in geological history---so early that no fossil traces have yet been discovered in it), and heated it until it formed a light ash. From this ash, by a complex chemical process, he obtained what he called "primordial protoplasm." When this was subjected to X-rays (exercising great care to prevent air contamination), its crystals gradually developed into microscopic forms of living plants and animals---chiefly fishes. In an area one inch across, for example, he once found 15,000 of these miniature fishes. It was his belief that they had been preserved in a state of suspended animation during the millions of years since the Azoic Age.

When Morley Martin died in 1937, he had published only a pamphlet, now exceedingly rare, titled The Reincarnation of Animal and Plant Life from Protoplasm Isolated from the Mineral Kingdom, 1934. Unfortunately, his laboratory notes explaining the details of his research were kept in a code that has not been deciphered. The best reference to his work is a chapter on him in Maurice Maeterlinck's La Grande Porte, 1939. The following paragraphs are quoted from a translation of this chapter in a mimeographed booklet, The Morley Martin Experiments, issued in 1948 by the

---

* See Chapter 11 for more on Burke's radiobes.

Borderland Science Research Associates of San Diego:

> Under the enlargement of the microscope globules were seen taking shape within the protoplasm and forming vertebrae which elongated into a spinal column in which the ribs were inserted; then came the outlines of the limbs or claws, the head, and the eyes. These transformations are normally slow and require several days, but at times they took place under the eyes of the observer. One crustacean, for example, having developed its legs, walked off the field of the microscope.
>
> These emergents therefore live, sometimes move, and develop as long as they find sufficient nourishment in the protoplasm in which they were born: after that, their growth is checked or else they devour one another. Morley Martin has, however, succeeded in feeding them by the help of a serum whose secret he kept. . . . (pp. 116-118)

EXPERIMENTAL DATA IN EVIDENCE OF THE PRESENT-DAY OCCURRENCE OF SPONTANEOUS GENERATION

Bastian, H. Charlton; *Nature,* 92:579-583, 1914.

Experimental Data in Evidence of the Present-day Occurrence of Spontantous Generation

In the autumn of 1905, I found that microbes would grow and slowly multiply when inoculated into a weak solution of neutral ammonic tartrate in distilled water, and that though the organisms would grow in this solution without the aid of light, that light distinctly favoured the process, since when an inoculated solution was equally divided, the half which was left exposed to ordinary diffuse daylight became turbid much more quickly than the other half which had been placed within a dark incubator, even though the temperature of this latter was as much as 20° F. higher than that of the portion exposed to daylight.

This was an experience in opposition with previous bacteriological doctrine, and it has been found to be of much importance in connection with experiments which I soon after commenced, and have ever since been continuing, bearing upon the question of the origin of life.

Nature of the Experimental Solutions.

My first experiments were made with ordinary commercial sodium silicate (water-glass) diluted with an equal quantity of distilled water: a few drops of this fluid, varying from 1-8, being added to an ounce of distilled water containing six drops of dilute phosphoric acid and six grains of ammonium phosphate, or else to an ounce of distilled water containing simply eight drops of

liq. ferri pernitratis of the British Pharmacopoeia.

These solutions at first, and up to the summer of 1910, were the two experimental fluids always made use of, varying only in the number of drops of the dilute sodium silicate employed, in accordance with varying strengths of different samples of this product.

These solutions of water-glass have been found to deteriorate and undergo some slow changes (a rather copious white deposit gradually forms in the bottle in which they are kept), and after about eighteen months my solutions would no longer yield the same kind of experimental results as at first. Moreover, during the last twelve months I have been unable to obtain any satisfactory sample of water-glass.

Strangely enough, Kahlbaum's 10 per cent. solution of sodium silicate, which is a comparatively uniform product, has never yielded any satisfactory results when it has been used in the preparation of my solutions.

On mentioning these troubles in the summer of 1910 to Dr. Otto Rosenheim, of King's College, he kindly gave me some of a very dilute solution of colloidal silica, prepared with great care after Graham's method, and made with the aid of the 10 per cent. solution of sodium silicate above referred to, the use of which had hitherto always proved unproductive.

This solution of colloidal silica gave more uniformly good results than I had ever obtained before, when I used ten to twelve drops of it to the ounce with the usual quantities of dilute phosphoric acid and of ammonic phosphate--though I have never been able to obtain a single successful result when using it with pernitrate of iron in the preparation of the yellow solution. Unfortunately, however, the weak solution of silicic acid, like the common water-glass solution, has seemed gradually to deteriorate, and that, too, much more rapidly, though in appearance the solution shows no change. Up the present I have never been able to repeat successful results with a second solution when it was more than four months old.

Thus, it seems clear that the specimens of water-glass with which I first experimented successfully must have contained other favouring ingredients not present in the 10 per cent. solution of sodium silicate; further, that though this solution yielded only barren results, yet the colloidal silica prepared from it and from strong hydrochloric acid yielded the best results of all when used as an ingredient of one of the colourless solutions, but uniformly poor results when mixed with iron for a yellow solution.

From the point of view of the capability or the reverse of the different fluids for engendering living matter (owing to differences in chemical composition) these seeming contradictions may have no real significance; though the opposite point of view that my positive results may be due to the pre-existence of organisms in these particular solutions and not in the others which yielded negative results is a position that would seem quite impossible of reconcilement with the variations in composition above cited--even if the process of sterilisation had not intervened.

Sterilisation and After-Treatment of the Experimental Vessels.

The experimental tubes were prepared in this manner. A little more than half an ounce of either of the solutions was put into each of a number of sterilised glass tubes. These were then hermetically sealed, and subsequently heated for five to twenty minutes to temperatures ranging from 125° to 145° C., or else to 100° c. for twenty minutes on three successive days.

As in all previous experiments concerning the possiblity of spontaneous generation by Pasteur, Pouchet, Tyndall, and many others, the destructive influence of heat was relied upon for ridding the fluids and vessels of any pre-existing living things that might be contained therein these being very much less numerous in my saline solutions than in hay infusions and other organic media of which it was the custom formerly to make use.

Saline solutions were used by me because they could be submitted, within limits, to higher temperatures than organic infusions without destroying any possible productivity; and because in using them there would be a closer approximation to the conditions that must have existed when the surface of our earth first cooled down below the temperature of boiling water, so that a natural origin of living matter might thereafter become possible.

After sterilisation the sealed experimental vessels are exposed to diffuse light and a varying amount of actual sunshine for periods of from four to ten months or more before the contents of the tubes are examined microscopically, though the terminal month may, with advantage, be passed in an incubator at some temperature between 27° and 37° C.

When "controls" are opened, say any time within one or two weeks of sterilisation, no organisms, except it may be one or two embryonic forms, are to be found, especially if the solution of ammonic tartrate has been filtered through No. 0 Swedish paper, though when other tubes of the same series come to be examined after the several months of exosure to light and heat above mentioned many well-developed organisms are often found, which can be proved to be living.

These organisms are for the most part Torulae, and minute simple moulds of different kinds. Specimens of such organisms are shown in Fig. 1, as they were taken direct from the tube, the Torulae in this case being unusually abundant.

Bacteria are much less frequently met with, mostly motionless, though occasionally motile. Plasmogenic products simulating cocci and bacilli in appearance are also by no means uncommon in these colloidal silicate solutions.

According to De Barry and other authorities, no germs of moulds can survive a single immersion for a few minutes in water at 100° C.; while Torulae are uniformly admitted to be killed by immersion for a minute or two in water at 60° C.

I have ascertained that the mixed Torulae and fungus germs to be found in the bloom on the surface of grapes have been killed by immersing the grapes for only thirty seconds in boiling water.

Further, I have found that the Torulae and minute moulds that

tend to appear after a short time in unheated weak solutions of silicic acid are, like other fungus-germs, unable to survive a single boiling for five minutes. Yet the least severe sterilising heat employed in my experiments has been a boiling for twenty minutes on three successive days.

. . . . .

In my work entitled "The Nature and Origin of Living Matter," an abridgment of which has been published by the Rationalist Press Association, I have considered the question of "spontaneous generation" from a broader point of view (pp. 128-141), and have endeavoured to show how multitudes of facts can be explained in accordance with my views, that from the time when living matter first appeared upon the earth it has probably ever been constantly reappearing, as at present, and giving birth to the simplest living units, such as now swarm upon its surface. These processes are such that they must always take place beyond our ken, seeing that they necessarily begin with mere molecular collocations, gradually going on to the formation of particles of an ultra-microscopic order. Such infinitesimal particles gradually emerge into the region of visible things as revealed by high powers of the microscope, and take on this or that simple organic form in a manner that (though by processes much more complicated) is somewhat akin to the mode by which crystals emerge from different mother liquors, and take on this or that particular crystalline form.

Thus, while the fact of the present occurrence of the de novo origin of living matter is, in my opinion, beyond the region of doubt, I fully recognise that the actual steps of the process remain to be discovered. I have elsewhere referred to some of the probable steps of the process, and the prominent part that may be taken by inorganic catalysers under the influence of sunlight, and in some quite recent experiments by Prof. Benjamin Moore and J.A. Webster, in a paper entitled "Synthesis by Sunlight in Relationship to the Origin of Life," they have been able to demonstrate the probable actual first step of such a process--one that is known to occur as a first step in the nutrition of plants. We are, however, as yet only on the threshold of anything like an explanation of the various stages of this supreme mystery, for the unravelling of which philosophers and chemists have hitherto striven in vain. As with many other natural phenomena, the fact of the occurrence of which cannot be questioned, so here only conjectures are available as to the precise mode in which it may have been brought about. We must, however, repose our faith in the uniformity of natural phenomena, as one of the cardinal postulates of science, and if living matter had a natural origin in the far-distant past, there is, from that point of view, good ground for believing what our experiments seem to testify, that it occurs at the present day.

• Extraterrestrial Influences on Life and Evolution

ORIGIN OF INFLUENZA

Gravitans; *English Mechanic,* 53:456, 1891.

There cannot be much doubt that whatever the influenza (or dengue) may be, it is a rather serious complaint in a climate such as England possesses; but it is not quite so novel as is thought, for it was certainly known a hundred years ago. I see that a Mr. G.H. Willis, writing to the Times, says that "Perhaps the Italian scientists of the last century were not so wrong in attributing the disease to some occult influence of the heavens," and, criticising some remarks that had previously appeared, he propounds the following curious hypothesis, which is rather refreshing, especially to those who have had the disease:--Dr. Parsons, he says, appears to reject the aerial theory, because the descending currents of air do not admit of the infective cause being carried from the earth's surface into the upper atmosphere for transmission to places to windward of the supposed places of origin. But has the disease a terrestrial origin? May it not originate in space? I believe I am right in supposing that volumes of gas float in space as well as solid atoms, and that when our planet comes in contact with any such volume, the atmosphere absorbs and dilutes the gas, rendering it imperceptible, just as the atmosphere burns up solid atoms when they come in contact with it, and so preserving the earth from constant bombardment. But, abnormally, just as an aerolite reaches the earth before it is consumed, so the earth may come in contact with a volume of poisonous gas of great density, some of which may find its way to the earth's surface in sufficient force to generate disease of a malarious nature affecting the nervous system and respiratory organs. The spread of the disease from east to west can be easily accounted for, if it originates from above, by the "slip" in the outer atmospheric envelope of the globe, which would carry the malarious gas gradually westward, relatively to the surface motion of the earth, to descent towards it and to be capriciously distributed by the lower air-currents.

. . . .

THE CARBONACEOUS CHONDRITES

Mason, Brian; *Space Science Reviews,* 1:621-646, 1963.

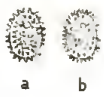
"Organized Elements". In 1961 Claus and Nagy announced the

identification of "microscopic-sized particles, resembling fossil algae, in relatively large quantities in the Orgueil and Ivuna carbonaceous meteorites". The controversy around this discovery has been a hot one, and is not yet clearly resolved. A conference of interested scientists was held in New York in May 1962 and the identification and interpretation of these "organized elements" or "life forms" were thoroughly discussed, without any general agreement as to their nature and origin. It was agreed that at high magnifications certain regular morphological forms could be recognized in samples of carbonaeous chondrites, and that these could be concentrated by chemical and physical methods--acid extraction, density separation, and so on. The competing interpretations can be broadly grouped under three headings: (1) These "organized elements" are terrestrial contaminants; (2) they are crystals of organic or inorganic compounds (3) they are the remains of extraterrestrial organisms.

The possibility that some of these "organized elements" are terrestrial contaminants is not unlikely. Many of these carbonaceous chondrites (the Orgueil meterorite especially) are very porous and friable; they enter the atmosphere from the near vacuum of outer space, and must immediately "breathe in" air and floating organisms; they lie on the ground for some time before they are collected; they usually undergo a lengthy odyssey and pass through many hands before reaching the apparent security of a museum collection; and even in museum collections they have not been housed in sealed containers, and have been subject to contamination not only with local bacteria, spores, and pollen but possibly also with exotic organisms brought in with other collections from different parts of the world. Under these circumstances it would be truly remarkable if a variety of terrestrial microorganisms were not found in them. The investigators of the "organized elements" have been conscious of this problem, and have tried to avoid it as far as possible by examining material taken from freshly broken surfaces of the meteorites. Nevertheless terrestrial contamination must be seriously considered as a source of at least some of the organized elements". We may hope that future falls of carbonaceous chondrites will give an opportunity to obtain superior material for investigation to that now available.

The second possibility, that some at least of the "organized elements" are crystals, has been vigorously pursued, especially by Dr. Anders and his co-workers at the University of Chicago. They showed that the Ivuna and Orgueil meteorites contain microscopic hexagonal crystals of troilite (FeS) closely resembling the hexagonal "organized elements" described by Claus and Nagy. By centrifuging these meteorites in chloroform (a procedure used by Claus and Nagy to concentrate the "organized elements") Fitch and his co-workers found numerous spherical particles in the residue of the light fraction; these spherical particles, similar in appearance to some of the "organized elements", proved to be supercooled liquid droplets of sulfur and hydrocarbons. In this connection it is desirable to emphasize the diversity and complexity of form in microscopic crystals, and the very real possibility

CLASSIFICATION OF THE "ORGANIZED ELEMENTS" *

10 μ	*Type Organized Element*	*Taxonomical Nomenclature*	*Descriptive Charateristics*
	1.	Apolinarisphaera meteoricola Clausisphaera fissa	Small spherical or ellipsoidal. Solitary or many celled colonies, doubled wall.
	2.	Stemmatopila uniporata	Slightly ellopsoidal, single pore at apex. One or two small spines at base. Doubled wall.
	2.	Disacerra sulcata Subreticulate pollen	Doubled wall. Flattened, spherical, solitary. Small papilla at apex, with subpapillary furrow.
	2.	Apophoreta aethrodescensa Incertae sedis type B	Doubled wall thick. Small papilla at apex, with irregular outlined crack in center.
	3.	Ancilicula vetusta	Doubled wall. Shield-shaped Flattened, solitary. Straited, with a free depressed, long oval area in center.
	4.	Dactyliotheca daedala Incertae sedis type A	Thick doubled wall. Cylindrical solitary. Striated, that radiate in center of the circular surface.
	5.	Daidaphore brezelii Caelestites sexangulatus	Polyhedral body with 3 tubular protrusions bases are markedly thickened and parallelly striated. Walls double thick, except at bases of collars, 3 vacuole-like structures. Fillamentous material protrudes from collars.
 a b	6.	Siderolappa lapillata	a – Coarse spines. Doubled thick wall many spines covering entire surface with a single subapical pore. b – fine spines. Same as (a).
	7.	Oscenoscaeva proavita	a – side view. Quadrangular shape with apices bearing a spine and the sides having 3 spicules. Small square in the center of the body. b – surface view.
	8.	Tissue type B	Ribbon like pliable tissue, with longitudinal striation.
	9.	Protoleiosphaeridum A	Vesicle subspherical, folded, smooth, containing pyritoorganic?

*Adapted from: Briggs, Michael H., and Mamikunian, Gregg; "Organic Constituents of the Carbonaceous Chondrites," Space Science Reviews, 1:647-682, 1963.

of their simulating the morphological forms of organisms. One need only call to mind the remarkably intricate ice crystals in snowflakes, an analogy nicely expressed by Morrison (1962), when he suggested the "organized elements" were "carbonaceous snowflakes".

The possibility that the "organized elements" are the remains of extraterrestrial organisms, has been argued most strongly from their morphology and their difference from any known terrestrial organisms. Staplin has dignified two of the "organized elements" which he separated from the Orgueil meterorite as new genera and species of uncertain classification, possibly algae. Drs. Claus and Nagy noted that some of the "organized elements" reacted to standard biological staining techniques; however, these stains are indicative, not of biogenic material as such, but of certain types of chemical linkages found in but by no means confined to such material. Fitch and co-workers have also found that terrestrial rocks such as kimberlite react to biological stains in much the same way as the Orgueil meteorite.

Attempts have been made to culture microorganisms from carbonaceous chondrites in a germ-free laboratory. Some growth appeared in the culture media after several months. However, in view of what is known about the near-certainty of terrestrial contamination of these meteorites, this result can hardly be taken as significant evidence for viable extraterrestrial organisms in these meteorites. (pp. 639 - 640)

. . . .

DOES EPIDEMIC DISEASE COME FROM SPACE?

Hoyle, Fred, and Wickramasinghe, Chandra; *New Scientist,* 76:402-404, 1977.

Recent studies by R.S. Rajan, D.E. Brownlee, D. Tomandle, P.W. Hodge, I.H. Farrar and R.A. Britten, of cometary debris in the form of micrometeorites in the stratosphere have shown chemical similarities to Type I carbonaeous chondrites. If a cometary impact led to the start of life, the question arises: would subsequent arrivals of cometary material carry biological or prebiological material which might affect terrestrial biology? The boldest answer must be yes; that is to say, extraterrestrial biological invasions never stopped and continue today. These invasions could take the form of new viral and bacterial infections that strike our planet at irregular intervals, drifting down onto the surface in the form of clumps of meteoritic material probably similar to those studied by Dr. Rajan and his colleagues. Recurrent waves of new disease could represent renewed attempts at the evolution of life on comets, infection reaching the Earth when its orbit crosses the trails of debris from these comets.

Reports of the sudden spread of plagues and pestilences punctuate the history of many countries. The most recent such disas-

ter was the 1918-19 influenza pandemic in which 30 million people died. Different epidemics, scattered throughout history, bear little or no resemblance one to another. But they all share a common property of afflicting entire cities, countries or even widely separated parts of the Earth in a matter of days or weeks. Thucydides describes the plague of Athens in 429BC thus:

"It is said to have begun in that part of Ethiopia above Egypt...On the city of Athens it fell suddenly, and first attacked the men in Piraeus; so that it was even reported by them that the Peloponnesians had thrown poison into the cisterns."

A similar description of sudden onset and rapid global spread is relevant to almost all earlier as well as later epidemics. Such swiftness of transmission is hard to understand if, as is usually supposed, infection can pass only from person to person or be carried by vectors such as lice and ticks. And this explanation is particularly untenable for widespread epidemics which occurred before the advent of air travel, when movement of people across the Earth was a slow and tedious process.

. . . .

The abrupt appearance in the literature of references to particular diseases is also significant in that they probably indicate times of specific invasions. Thus, the first clear description of a disease resembling influenza was in the 17th century AD, while the earliest reference to the common cold was in about the 15th century AD. Also it is significant that earlier plagues, such as that in Athens, do not have easily recognisable modern counterparts.

Further major epidemics of disease could be caused when the Earth crosses the debris of new long-period comets. Relatively minor variants of the "same" disease--e.g. the common cold--could be due to more frequent, regular passages of the Earth through debris of shorter period comets.

The factors governing the actual pattern of global incidence for any particular extraterrestrial invasion could be complex. If bacteria or viruses are dispersed in a diffuse cloud of small particles, the incidence of disease may well be global. On the other hand, a smaller disintegrating aggregate of infective grain clumps falling over a limited area of the Earth's surface could provide a geographically more localised invasion. Systematic effects such as air currents over the Earth's surface could also be relevant in controlling the transport and dispersal of particles. In particular, certain latitude belts might well be more favoured than others for either the accumulation and settling of these particles, or their avoidance. Furthermore, spatial variations in settling times, corresponding to variations in atmospheric conditions at different locations, could mimic a situation where an epidemic apparently spreads from a localised focus--the spread having no casual connection whatever with the terrestrial "focus".

Our suggestion, if correct, would have profound biological, medical and sociological implications. A continual microbiological vigil of the stratosphere may well be necessary to eliminate the havoc which will ensue from extraterrestrial invasions in the future.

IS EVOLUTION A TRAVELLER FROM OUTER SPACE?

Anonymous; *New Scientist,* 77:139, 1978

The new genes which enable animals and plants to evolve on Earth may come from space. So speculated Sir Fred Hoyle last Monday evening while delivering the first Milne Lecture at Oxford. Sir Fred was enlarging upon the theory which he expounded with Professor Chandra Wickramasinghe in New Scientist (vol 76, p 402) last November. This theory advances the idea that life did not originate on Earth itself but, rather, on comets which can provide a more suitable environment. Periodic approaches of life-bearing comets to the Sun would have released ice-bound and freeze-dried cells "like the scattering of seeds" to drift down on to the Earth's surface.

Today, the two astronomers suggested, we see the diminshed remnants of this process as outbreaks of epidemic diseases caused by the showering down on our planet of viruses and bacteria which have evolved away out in space.

. . . . .

One strength of the theory is that it could, in principle, explain many of the baffling epidemiological conundrums. Sir Fred expanded on this theme, pointing out many anomalies in the distribution and spread of the 1918 and 1968/69 flu epidemics, and concluding that simple person-to-person spreading was not, whereas the atmospheric dispersion of a space-borne agent was, an adequate explanation.

In conclusion Sir Fred produced a biological argument which many will undoubtedly find bizarre. The established biologists ask why extraterrestrial bacteria or viruses should attack terrestrial life forms. Sir Fred counters: "Isn't it equally remarkable that, with all the time since Creation, terrestrial life hasn't evolved some totally effective blanket immunity to all pathogens?" Viruses appear to him to be particularly vulnerable, and their host cells often "deliberately suicidal" in allowing themselves to be infected. Why is this?

His answer is ingenious, if perhaps fallible. Complex life forms, he says, during evolution have gained an average of about one new gene every 1000 years. It is by now well known that the so-called transforming viruses can insert bits of their DNA--new genes--into their hosts' DNA. Some biologists believe this to be an important mechanism in evolution. If there were total immunity to all pathogens it would not be possible--and, speculates Hoyle, there could be no evolution. On Sir Fred and Wickramasinghe's theory these new genes come from cometary viruses.

THERMAL HISTORY, CHEMICAL COMPOSITION AND RELATIONSHIP OF COMETS TO THE ORIGIN OF LIFE

Irvine, W. M., et al; *Nature,* 283:748-749, 1980.

Could comets be relevant to the origin of life? A principal problem in the synthesis of biologically significant molecules in the aqueous terrestrial environment is the polymerisation of amino acids and nucleic acid monomers. Dehydration reactions involving volcanic heating and/or evaporation and catalysis in tidal pools have been invoked. Suitable environments might more naturally occur in a comet, where we may suppose temperature regimes from considerably above the boiling point of H_2O to a perhaps substantially depressed (by dissolved salts) freezing point, solid clay and metallic surfaces for catalysis, possible circulation of an aqueous solution with dissolved organics, and much less dilution than would be expected in a terrestrial ocean. Moreover, the time span determined above during which liquid water might be available in some comets is consistent with geochemical evidence for the epoch of the first O_2-releasing photosynthesis on Earth. The biological requirement for free energy in a dark environment has been considered by Anders in a discussion, similar to the above concerning the possibility that asteroids might have supported life (we note also the existence of apparently photosynthesis-independent biocommunities at mid-ocean thermal vents). Present models postulate that the Sun is surrounded by a vast cloud containing perhaps 10^{11} -10^{12} comets and thus providing environments for a vast number of 'biological experiments'. When a comet enters the inner Solar System, it leaves behind a trail of debris which is encountered by the Earth as meteor showers and micrometeoroids. The Earth accretes about 10^7 kg yr^{-1} of such material. Micrometeoroids smaller than 10^{-6} g will not be heated above 100°C on entry, and the interiors of meteorite-sized objects also remain at moderate temperatures. It follows that comets may have played a part in seeding the primitive Earth with biological polymers possibly capable of self-replication, or at least of evolving towards that capability.

Is it conceivable, as suggested by Hoyle and Wickramasinghe, that in subsequent times the Earth has been infected by organic material which arose independently in cometary environments and which was viable in terrestrial organisms? The resemblance between the genetic systems of viruses and cells implies that evolution of viruses depends on the evolution of cellular genetic systems and therefore makes the independent evolution of viruses on many different comets very unlikely. On the other hand, it seems difficult to exclude the possibility that small nucleic acids of cometary origin might occasionally be viable in terrestrial organisms if (in the apparent manner of viroids) they could reproduce without the expression of self-specified proteins, so that they need not have evolved a genetic code translatable by the host cell. (p.749)

UNRECOGNIZED SPECIES

• Curious Marine Protoplasm

REALITY OF THE "BATHYBIUS"

Anonymous; *English Mechanic,* 29:590, 1879.

A quantity of peculiar slimy matter was dredged in the North Atlantic by the naturalists of the exploring ship Porcupine from a depth of from 5,000 to 25,000 feet. It is described as exhibiting, when examined on the spot, spontaneous movements, and as being obviously endowed with life. Specimens of this, preserved in spirits, were examined by Prof. Huxley, and declared by him to consist of protoplams, vast masses of which must thus in a living state extend over wide areas of sea bottom. To this wonderful slime Huxley gave the name of Bathybius Haeckelii. Bathybius has since been subjected to an exhaustive examination by Professor Haeckel, who believes that he is able to confirm in all points the conclusions of Huxley, and arrives at the conviction that the bottom of the open ocean, at depths below 5,000 ft., is covered with an enormous mass of living protoplasm, which lingers there in the simplest and most primitive condition, having as yet acquired no definite form. He suggests that it may have originated by spontaneous generation, but leaves this question for future investigators to decide. The reality of Bathybius, however, has not been universally accepted. In the more recent investigations of the Challenger the explorers have failed in their attempts to bring further evidence of the existence of masses of amorphous protoplasm spreading over the bed of the ocean. They have met with no trace of Bathybius in any of the regions explored by them, and they believe that they are justified in the conclusion that the matter found in the dredgings of the Porcupine and preserved in spirits for further examination was only an inorganic precipitate due to the action of the alcohol. It is not easy to believe, however, that the very elaborate investigations of Huxley and Haeckel can be thus disposed of. These, moreover have received strong confirmation from the still more recent observations of the Arctic voyager, Bessels, who was one of the explorers of the ill-fated Polaris, and who states that he dredged from the Greenland seas masses of living undifferentiated protoplasm. Bessels assigns to these the name of Protobathybius, but they are apparently indistinguisable from the Bathybius of the Porcupine. Further argu-

ments against the reality of Bathybius will therefore be needed before a doctrine founded on observations so carefully conducted shall be relegated to the region of confuted hypotheses. Assuming, then, that Bathybius, however much its supposed wide distribution may have been limited by more recent researches, has a real existence, it presents us with a condition of living matter the most rudimental it is possible to conceive. No law of morphology has as yet exerted itself in this formless slime. Even the simplest individualisation is absent. We have a living mass, but we know not where to draw its boundary lines; it is living matter, but we can scarcely call it a living being.

. . . . .

Chapter 10

GENETICS AND HEREDITY

Chapter Contents

MORPHOLOGICAL PHENOMENA

• Conservative Nature of Natural Selection

ANCIENT LINKAGE GROUPS AND FROZEN ACCIDENTS

Ohno, Susumu; *Nature,* 244:259–262, 1973.

No one would argue that the central concept of biology which congeals divergent disciplines into one cohesive whole is the Darwinian concept of evolution. Evolution, in turn, is contingent on natural selection. It is then of the utmost importance that we do not misunderstand the nature of natural selection. At first, natural selection was thought of as an omnipotent advocator of progressively adaptive changes. Those who still hold such a view see evolution as a relatively fast process, all inclusive in nature and affecting the entire genome. More recent amino acid sequence analyses of a large number of direct gene products, on the other hand, indicate that the true nature of natural selection is most often that of an avid conservative extolling the virtus of status quo. Could it be that extensive adaptive radiation of mammals actually involved only a very small fraction of the functional gene loci, while the rest remained relatively unmolested? If so, the genetic difference that separates man from other beasts might be surprisingly small.

Several years ago my colleagues and I made two postulates on the genetic content of human chromosomes: (1) As an evolutionary freeze must have been placed on the X chromosome of a common mammalian ancestor, any gene locus which is found to be X linked in man must automatically be X linked in all other mammalian species. (2) At the stage of fish or amphibian, a mammalian ancestor must have undergone at least one round of tetraploid evolution. Consequently, a homologous relationship may still be found among pairs of apparently nonhomologous human chromosomes.

Since then, cytological as well as genetic evidence in favour of those two postulates has steadily accumulated. Here I shall discuss the extremely conservative nature of natural selection and the concept of frozen accidents within the context of human evolution and human linkage groups.

Man and Apes. Differences between man and apes are obvious to the naked eye, yet such differences need not have involved a large number of gene loci in adaptive changes. Although man is said to be a naked ape, a full set of fur can apparently be re-

stored to the human body by a single dominant mutation. At each polymorphic gene locus, natural selection can favour a certain genotype only by eliminating other alternative genotypes as unfit. Accordingly, a population which is undergoing rapid adaptive changes has to pay a considerable price for each gene locus which is subjected to selection. This is what Haldane apparently meant by the "cost of natural selection". If follows, then, that successful adaptive changes cannot involve too large a number of gene loci, otherwise a population would be exterminated by natural selection while undergoing these changes. Although polymorphism has been documented for about 33% of the human gene loci tested, the very fact that human populations are not in the slightest danger of extinction suggests that natural selection has been oblivious to these polymorphisms; polymorphic alleles most often representing functionally neutral mutations. It would not be surprising if most of the gene loci in the genome were not involved in adaptive radiation which separated man from apes. Indeed, haemoglobin α as well as β chain amino acid sequence is identical between man and the chimpanzee, while the gorilla differs by one amino acid substitution at each of these two chains.

Evolutionary Rates. Inasmuch as a greater number of the sequenced peptide chains have undergone evolutionary changes at rates slower than that for haemoglobin chains, one would expect that at many gene loci there would be no detectable difference between man and apes. Although samesense mutations cannot easily be detected by sequence analysis of peptide chains, the expected ratio between missense and samesense mutations is only 17:6. Expressing the rate of evolutionary changes in terms of accepted point mutations (PAM)/100 amino acid residues/100 million years (m.y.) (which is about the time for which mammals have existed on Earth) the value obtained for haemoglobin chains is 14, whereas lower values of 0.06 for histone IV, 3 for cytochrome c, 4 for insulin and 5 for trypsinogen have been obtained. Evolutionary conservatism is indeed evident at many of the gene loci.

One might argue that natural selection in the past has elevated most of the gene loci of higher vertebrates to states of near functional perfection, and that this is the very reason we no longer see much evidence of adaptive changes in many gene loci of man and apes. Indeed, so far as haemoglobin chains are concerned, the rate of evolutionary changes appears to have slowed down noticeably in higher primates. Yet such an argument is revealing only a part of the truth and merely restating the point which I made earlier that natural selection can play the role of advocator of adaptive changes only on a very small fraction of the functional gene loci in the genome. One can easily argue that Escherichia coli or any other contemporary organism in its own way represents a state of near functional perfection.

The rate at which an organism evolves is greatly influenced by its generation time; the longer the generation time, the slower the rate of evolution. Man, with a generation time of 15 yr, has gone through only 330 generations in 5,000 yr, whereas the mouse with a generation of 2 months would have gone through 30,000 generations in a comparable time. The observed evolutionary slow-

down in higher primates may simply be the consequence of progressive prolongation of their generation time.

. . . .

• The Improbable Complexity of Genes

NATURAL SELECTION AND THE COMPLEXITY OF THE GENE

Salisbury, Frank B.; *Nature,* 224:342–343, 1969.

Modern biology is faced with two ideas which seem to me to be quite incompatible with each other. One is the concept of evolution by natural selection of adaptive genes that are originally produced by random mutations. The other is the concept of the gene as part of a molecule of DNA, each gene being unique in the order of arrangement of its nucleotides. If life really depends on each gene being as unique as it appears to be, then it is too unique to come into being by chance mutations. There will be nothing for natural selection to act on.

The problem was discussed at a symposium of mathematicians and biologists in 1966, but they failed to solve the difficulty. I feel that virtually no one present except Eden and Schutzenberger, who outlined the problem, really understood what the commotion was all about. Some years ago I also outlined the problem. My outline begins by overstating the case somewhat, but it provides a background for discussion of modern discoveries which may be pointing toward a solution. I believe that the solution remains to be found.

Natural Selection and Metabolic Pathways. In 1945 Horowitz suggested a mechanism of molecular natural selection which seemed to account for the origin of metabolic pathways. Reproduction of a prelife molecule (we now would think of DNA) required the presence of suitable precursors (presumably phosphorylated nucleotides). Current theory holds that all of these were present in rich abundance at the beginning, but ultimately at least one of them became limiting. Any enzyme material which might then be produced by random changes in the DNA and its controlled protein, and which was capable of producing the precursor, would be selectively preserved in the struggle for molecular existence. One gene might be added to another by the selection pressures of disappearing precursors until the final autotrophic organisms came into existence. In such a case as this, where a specific enzyme had to appear to provide a limiting precursor, it is possible to ask the following questions. In reasonable time intervals, is mutation by random rearrangement of nucleotides likely to produce an enzyme which will meet the need brought about by the require-

ment for some specific precursor? Will there be an enzyme (gene) for selection to act on?

A typical small protein might contain about 300 amino-acids, and its controlling gene about 1,000 nucleotides (three for each amino-acid). Because each nucleotide in a chain represents one of four possibilities, the number of different kinds of chains is equal to the number 4 to the power of the number of links in the chain; that is, 4^{1000}, or about 10^{600}.

Imagine that the primaeval ocean was uniformly 2 km deep, covering the entire Earth, containing DNA at a concentration of 0·001 M (about 700 g of DNA/l. of solution), each double stranded molecule with 1,000 nucleotide pairs. Also imagine that each DNA molecule reproduces itself one million times per second, a single nucleotide substitution (a mutation) occurs each time a molecule reproduces, and no two DNA molecules are ever alike. In four billion years, 7.74×10^{64} different kinds of DNA molecules will be produced. On 10^{20} similar planets in the Universe, this would be 7.74×10^{84} (say 10^{85}) different nolecules. If only one DNA molecule were suitable for our act of natural selection, the chances of producing it in these conditions are $10^{85}/10^{600}$ or only 10^{-515}. If 10^{100} different kinds of molecules could each carry out the necessary precursor synthesis, this is equivalent to saying that 166 of the nucleotides might be changed without loss in ultimate activity of the enzyme. Still, only one molecule out of every 10^{500} would be acceptable, and after four billion years on 10^{20} planets, 10^{415} of the first 10^{500} possibilities remain to be synthesized. The chances are, then, still unimaginably small (10^{-415}) that a proper DNA molecule would be produced in this time. And if the proper molecule did appear by that fantastic accident, the problem comes up again the next time a precursor becomes limiting.

In the 1 km deep oceans on the 10^{20} planets during the 4×10^{12} years, the DNA chain can have only 141 nucleotides if all 10^{85} possible kinds are to be produced. This would code a protein chain only forty-seven amino-acids long.

The point of these numbers is that one DNA chain 1,000 nucleotides long can be a unique individual in a population of 10^{600} other unique indivudals. Numbers of this size have no precedent in anything but the concepts of information theory. Assume, for example, a cubic universe with dimensions of 20 billion light years on each side. In Angstroms, this would be about 10^{39} Å^3 on a side, with a volume of "only" 10^{117} Å^3. Imagine the number of universes required to contain 10^{600} tightly-packed DNA molecules!

In spite of the wild assumptions, the problem should be apparent. In the evolution of life on Earth, we are dealing with millions of different life forms, each based on many genes. Yet the mutational mechanism as presently imagined could fall short by hundreds of orders of magnitude or producing, in a mere four billion years, even a single required gene.

. . . . .

• The Problem of Excess DNA

DOES DNA CONTENT LIMIT EVOLUTION?

Anonymous; *New Scientist,* 41:644, 1969.

The DNA content of cell nuclei from different animal species shows considerable, and rather puzzling, differences. One species of salamander, for example, has 25 times more DNA per nucleus than Man; and all salamanders so far investigated have a higher DNA-to-nucleus ratio than any known mammal. DNA content cannot, then, be taken to relate the level of evolution reached by any particular species. In fact, it seems that during the evolution, at least of the vertebrates, the amount of nuclear DNA has risen and fallen at various times. Drs. O.B. and C.J. Goin, of the University of Florida, now suggest that the amounts of DNA in various types of vertebrate can be correlated with the patterns of evolutionary change they have undergone (American Midland Naturalist, vol. 80, p.28).

Underlying their idea is the argument that if an animal's complement of DNA is, as it were, "full-up" then only relatively small evolutionary changes can take place, in the form of minor amendments or mutations. The Goins suggest that really large evolutionary changes will only be possible when there is some extra DNA available, which can be brought into use.

. . . .

SPLIT GENES: MORE QUESTIONS THAN ANSWERS

Anonymous; *Mosaic,* 10:38-44, October 1979.

Many molecular biologists have taken to using the naked word "revolution" to describe recent events in genetic research. Many --but not all. Some, inclined to conservatism, shade their enthusiasm. Francis Crick, for example, who with collaborator James Watson elucidated the structure of DNA, qualifies; "In the last two years there has been a mini-revolution in molecular genetics."

Mini or otherwise, events of the last two years have sent shock waves reverberating through the research community. Until now, it has been presumed that genetically what is true of E. coli is true of elephants--that what is learned of the gene structure of one could be readily and linearly extrapolated to the other. That this is not the case has come as a stunning surprise. This is despite the admonition, in the words of Philip Leder of the National Institutes of Health, that scientists" ought never to be surprised by surprises."

Bacteria such as E. coli are the workhorse of molecular biology. From them have come many of the fundamental precepts of molecular genetics. What has been discovered in the last two years, however, is that there is a difference--perhaps even more fundamental than the presence of a cell nucleus in one and not in the other--between the genetics of a higher organism and that of a bacterium.

In the un-nucleated bacterium, the gene coding for a protein exists as one continuous length on a strand of DNA--the master molecule of heredity. In higher organisms, the gene may be split into several discontinuous segments, separated by intervening sequences or gene inserts. These inserts are also DNA; their function has yet to be unscrambled. But that they do not participate directly with "active" segments in the same protein manufacturing processes seems clear.

The fact that genes come in segments can be simply stated. But its ramifications are profound. It means that the machinery of heredity in higher organisms, from yeast to man, engages in formidably complex bio-chemical choreography. It is a process involving cutting and slicing of genetic information in a way that requires the instructions encoded in widely separated parts of a DNA molecule to be snipped out and reassembled as a cohesive genetic message.

It is as if a weaver, working from a tangled skein of multicolored yarn, were able to snip the bits of wanted color from along the length of the yarn, skipping the unwanted lengths, and splice the chosen bits and pieces into a pattern out of which a finished design--for protein synthesis--could emerge according to some program or plan. The one-to-one, co-linear relationship between a segment of DNA and the protein for which it codes has gone down the drain--at least for higher organisms. And many fundamental certainties of molecular genetics have gone with it.

Bits of DNA coding for protein--"sense" segments--separated by "nonsense" segments, says Crick, are "almost as surprising as if you were reading, say, a novel by Jane Austen and in the middle of a paragraph you found a rather garbled version of an advertisement for deodorant." To complicate the picture further, in some viral systems the "nonsense" segments that are dropped out turn out to be "sense" segments for some other gene function.

Aside from posing riddles relating to editing and programming apparatus in the translation of genetic information, the discovery of segmented genes is forcing scientists to rethink some very fundamental precepts of evolution. There's new speculation, for instance, that contrary to all previous assumptions, bacterial and animal cells do not represent differences in evolutionary degree; they represent differences in evolutionary kind. They have apparently followed largely divergent paths of evolution. That not much is certain in so arcane a world, however, is demonstrated by an anomaly's anomaly. The mitochondria that inhabit the cells of yeast, it has been speculated, once in evolutionary time might have been independent bacteria that set up symbiotic housekeeping inside yeast cells where they perform a photosynthetic function.

Nevertheless, they, like their nonphotosynthetic hosts, show segmented genes. Mitochondria are "almost bacteria," comments Thomas Broker of Cold Spring Harbor Laboratory. "They could represent the most primitive split gene known."

Further, and of much more immediate and clinical relevance, the new gene configuration is prompting a major reappraisal of the nature of the disruptions or errors that lie at the root of various inherited disorders; certain inborn diseases may well be a product of mutations in the "nonsense" sequences linking the gene components rather than in the segmented "minigenes" themselves. (p. 39)

Questions and implications. As the list of discontinuous genes grows so do the questions multiply. First, there are the practical issues of fundamental concern to molecular biologists seeking to unravel the details of the editing machinery. How are the intervening sequences removed from the primary transcript, the hnRNA, to yield mRNA? What are the signals? Which enzymes are called into play to perform the various editing tasks? Are there just two--a snipper and a splicer? Or are there hundreds, each specific to a particular gene fragment? How is the process orchestrated?

And then there are the subsequent, inevitable questions: What role do genes-in-pieces serve? What are evolutionary implications? Why so much "dead space" in the eukaryotic genome? What is the function of these "nonsense" stretches of DNA? Why genes in segments, even if the leftovers do perform some other genetic function?

At first glance, says Harvard University's Walter Gilbert, the genetic machinery in eukaryotic cells seems terribly inefficient--what with 80 to 90 per cent of the DNA (and its concomitant hnRNA) remaining silent. But measured over the long run, he suggests, the arrangement undoubtedly offers significant advantage in terms of evolutionary progress. Gilbert argues that the split genes help speed evolution along. He sees the existence of gene inserts as a major driving force of evolution. (p. 43)

. . . .

The key, in Gilbert's view, is that the exons code for "domains," specific functional subunits of the protein, and that the shuffling helps to "mix and match" domains. One small piece of evidence at hand to support Gilbert's contention is that exons, the building blocks of full genes, indeed code for protein domains. The evidence comes from an analysis of immunoglobulins, which have four readily distinguishable functional domains. Gilbert's proposition would require that the genes coding for the immunoglobulins have a like number of exons. When looked at in mice, the immunoglobulin gene did indeed have four separate exons, separated by three introns.

Gilbert's speculations have not won unanimous endorsement. One disputant is NIH's Philip Leder. While he agrees that intervening sequences (a term which he prefers to introns) play a critical role in evolution, he proposes that their role may have a totally different impact. The thrust of his argument is that

intervening sequences could just as readily serve as evolutionary stabilizers, as biological brakes that retard genetic combinations and protect against runaway mutations. In Leder's view, intervening sequences may well represent a strategy for freezing genes in place--a way to preserve stability by cutting down on the amount of recombination which, by a process of "unequal crossover," could lead to a loss of those genes.

The debate--new as it is--is far from resolved. "Chances are it may never be," says Leder, "since evolutionary arguments can't be proven conclusively one way or the other. Intervening sequences may have arisen for reasons that will forever remain obscure."

Of more immediate concern to Leder and others are the medical implications inherent in the discovery of intervening sequences. "It has opened us up to an entirely new way of thinking about genetic diseases," says Leder, whose background is in both molecular biology and medicine. "It now seems likely that some individuals have genetic diseases of a kind we would never have recognized before, namely diseases brought on by disruptions in intervening sequences. We might expect, for example, that specific base arrangements at the flank ends of the intervening sequences help to flag splicing sites, and that such bases can suffer mutations so that the proper mRNA and its protein either don't get made at all or are produced in reduced, deficient amounts." That these sequences at the splice junctions turn out to be universally used in all the eukaryotic systems studied to date makes it "an incredibly basic signal," Cold Spring Harbor's Broker notes. (p. 44)

. . . .

• Ancient Split in Line of Evolution?

IMPLICATIONS OF RNA•DNA SPLICING IN EVOLUTION OF EUKARYOTIC CELLS

Darnell, James E., Jr.; *Science*, 202:1257-1260, 1978.

Summary. The differences in the biochemistry of messenger RNA formation in eukaryotes compared to prokaryotes are so profound as to suggest that sequential prokaryotic to eukaryotic cell evolution seems unlikely. The recently discovered noncontiguous sequences in eukaryotic DNA that encode messenger RNA may reflect an ancient, rather than a new, distribution of information in DNA and that eukaryotes evolved independently of prokaryotes.

• The Genetic Code May Not Be Universal

MITOCHONDRIA SPRING SURPRISES

Hall, B. D.; *Nature,* 282:129–130, 1979.

The concept of a totally universal genetic code was based initially on studies of E. coli, tobacco mosaic virus, and human haemoglobin. The extrapolation of this conclusion from three genetic systems to all organisms was justified by the theoretical premise that any mutation which changed the ancestral code would cause so many different deleterious effects that it would have been selected against. Impressive though this argument may be, eukaryotic mitochondria have not heeded it. DNA-protein sequence comparisons for mitochondrial genes have revealed three variant codon assignments, at least one of which (UGA coding for trypotophan rather than acting as a termination codon) is used both in human and in yeast mitochondria.

The mitochondrial genome codes for a small number of proteins of the respiratory chain (eight in yeast) and contains genes for the tRNA and rRNA molecules of the mitochondrial translation system (Borst & Grivell Cell, 15, 705;1978). Thus, as regards specification of the set of codons and anticodons used, mitochondrial decoding of genetic information is autonomous and independent from that performed by the cytoplasmic system.

. . . .

CURIOSITIES OF BEHAVIOR

• Relocating Genes

GENE RELOCATES DURING DIFFERENTIATION

Anonymous; *Science News,* 110:372, 1976.

The polypeptide chains that make up antibody molecules seem to disobey a basic rule of modern biology. Rather than a single gene coding one polypeptide, different genes code for the two regions of each chain (SN: 10/19/74, p.253). Now there is evidence that, during differentiation, one gene is moved adjacent to another antibody chain gene.

A central problem in immunology is how the immune system produces thousands of different antibody molecules to respond to the multitude of possible foreign toxins and pathogens. It would be impractical to have a separate gene for each molecule.

A partial explanation has been found in the combination of a constant portion of an antibody chain with one of a number of variable portions. Further, in ways yet unexplained, changes in the variable region give antibodies their characteristic property of combining specifically with whatever substance elicited their formulation.

. . . .

OVERLAPPING GENES: MORE THAN ANOMALIES?

Kolata, Gina Bari; *Science,* 196:1187-1188, 1977.

Beginning in the 1950's and continuing into the early 1970's was the golden age of molecular biology. Investigators worked out the genetic code and discovered how certain bacterial and viral genes are controlled. The field advanced so quickly that many molecular biologists turned from studies of bacteria to studies of higher organisms, saying that the genetic systems of bacteria held no more surprises for them.

This dismissal of bacteria and their viruses may have been premature. Studies of these simple organisms have recently

yielded results that shake the foundation of the theories of molecular biology. First, researchers found that bacterial genes do not necessarily remain stationary, but may jump from place to place on DNA molecules (Science, 30 July 1976, p.392). Some researchers are now saying that genes also move about on chromosomes of higher organisms. Another major shake-up may be imminent as investigators find that genes of some bacterial viruses, and possibly genes of bacteria as well, are not the discrete entities they were always thought to be.

Since the early days of molecular biology, genes have been pictured as non-overlapping sequences of DNA. That is, it was thought that no gene sequence begins or ends within the sequence of another gene. Detailed studies of a few bacterial and viral genes confirmed this view, and most investigators did not question it.

In the past few years, molecular biologists discovered that two bacterial viruses have overlapping genes, but they often accompanied their reports with the cautionary note that these viral genes may be exceptions to the general rule of nonoverlap. Very recently, however, two groups of researchers have found evidence that bacteria also may have overlapping genes. Some molecular biologists now speculate that overlapping genes may be common in cells. The presence of overlapping genes in bacteria has not been proved, but if they do exist, many theories of molecular biology may have to be reevaluated. For example, current views of gene organization and the control of gene expression, as well as views of the information content of DNA molecules and the effects of mutations on DNA, may have to be substantially revised.

The hypothesis of nonoverlapping genes is a keystone for many genetic theories. It is taken for granted, for example, when investigators calculate what proportion of an organism's DNA consists of genes and what proportion consists of control regions and "spacer" sequences whose functions are unknown. This hypothesis is also crucial to estimates of how mutations affect cells. Some researchers have stated that evolution proceeds too rapidly to be based on randomly occurring mutations, each of which presumably affects only one gene. Since the assumption that genes do not overlap was firmly entrenched in the minds of investigators, this difficulty was explained by invoking other mechanisms of evolutionary change, such as gene rearrangements.

. . . .

• The Genetic Code Is Not Fool-Proof

CONFUSION FOR THE GENETIC CODE-BREAKERS

Anonymous; *New Scientist,* 24:493-494, 1964.

Evidence is accumulating that the genetic code may not be as fool-proof as geneticists would prefer. In fact, the business of decoding the chemical information contained in an organism's genetic material may be rendered considerably harder by the discovery that the physical conditions under which the relevant experiments are carried out can affect the result. A recent finding made by S.M. Friedman and I.B. Weinstein, of Columbia University and the Francis Delafield Hospital, New York, is that both temperature and the concentration of positively charged ions can upset the translation of the code. (Proceedings of the National Academy of Sciences of the USA, Vol. 52, p.988).

. . . .

They found not only that a messenger-RNA made of a chain of a single base would code for two distinct amino acids (as other workers had recently discovered), but also that which of these two amino acids predominated depended on temperature and the concentration of positive ions. For example, at a given magnesium concentration, the ratio between them changed by a factor of seven as the temperature varied from 65° to 37°C. Other kinds of positive ions induced the same artificial messenger-RNA to code for a variety of yet different amino acids and the same sort of conditions introduced similar ambiguities into the coding patterns of artificial messenger-RNA made up of alternating pairs of bases.

What the recent experiments show, in short, is that the protein-forming machinery seems to read a code word differently according to the prevailing temperature and other external conditions. It is most probable that the effect is due to something that happens to the chemical bonds on the messenger-RNA. Whether or not it occurs to any extent in living organisms will be hard to test but it seems likely that some of the genetic effects that are already known may be explained by related processes. The most worrying thing must surely be that it makes suspect nearly all the results obtained in unravelling the genetic code.

VIRUS VIOLATES AND IMPORTANT BIOLOGICAL LAW

Lewin, Roger; *New Scientist*, 72:148, 1976.

A tiny virus, known technically as ΦX174, which infects certain bacteria and looks rather like a spherical sea-mine complete with spikes, is about to make history: it is the first living organism in which has been discovered a gene that can be read in two different ways, thus producing two entirely different proteins. Ever since molecular biology became sophisticated enough to illuminate the relationship between the structure of genes and the structure of proteins for which they code, there has grown a steadily stronger conviction that a code written into the chemical sequence of a gene may be read in one way only. Common sense, and experience, seemed to bear this out. But now that a research team in Cambridge, England, has shown that ΦX174 contains not one, but two examples of overlapping genes the traditional view will be abandoned and the search for more will begin.

. . . .

BIOLOGY'S 'CENTRAL DOGMA' TURNED TOPSY TURVY

Anonymous; *New Scientist*, 46:614, 1970.

The central dogma, first formulated in the early 1950s and given that name by Francis Crick, states that information in a living cell flows in one direction only: from DNA to RNA to protein. As the receptacle of the cell's information, DNA ensures its continuance by replicating itself at every cell division. In order to express the information it holds, the DNA acts as a template for the synthesis of messenger RNA, which in turn directs the synthesis of the appropriate protein. The central dogma can thus be expressed:

It is no exaggeration to say that the central dogma has been the backbone of molecular biology. And although a few people--notably that arch-sceptic Barry Commoner--have challenged its contention that protein cannot affect the information content of RNA, and RNA that of DNA, very few people have taken the challenges seriously.

The discovery that viruses exist whose genetic information is encoded in RNA provided something of a problem for the central dogma. But Spiegelman himself played the crucial role in solving it, by discovering within two RNA bacteriophages (virus that infect bacteria) an enzyme which enables RNA to replicate itself. In these viruses, then, RNA acts both as the self-replicating genetic material, and as the "messenger" coding for template synthesis. The central dogma, in these phages, was truncated but not violated.

Shortly after this work however, in about 1965, a biochemist by the name of Dr. H.M. Temin (now at Wisconsin University)

claimed to have discovered, within a cell invaded by an RNA virus, a stretch of DNA that matched with the viral RNA. The implication was that the RNA acts as a template for DNA synthesis --in direct contravention to the central dogma. His results were dismissed as cranky.

. . . .

Before Spiegelman's work it was only too easy to ignore "Teminism". But now it must be taken very seriously indeed. It appears that when RNA tumour viruses invade a cell, they first direct the synthesis of DNA; this DNA then replicates and orders the synthesis of new viruses via messenger RNA in the normal way. In reply to a prompting by M.G.P. Stokes, the meeting's chairman, Spiegelman suggests that perhaps Teminism might lead to a way of preventing the replication of RNA tumour viruses by finding a block to this extraordinary reaction.

But perhaps Teminism is more widespread. Perhaps the central dogma is violated in other systems than the RNA tumour viruses? This would really set the cat among the pigeons. Spiegelman was clear on one point; the central dogma was not shattered to the extent that proteins might "code" for RNA. "If it were discovered that you could reverse the last step from RNA to protein", he said in reply to a question, "I would become a theologian". But what with a potential cure for cancer, and at least half of the central dogma turned on its head, the large audience at the meeting had other things than the election to talk about as they emerged into the June sunshine.

• Trying to Account for Antibody Diversity

THE GENES THAT MAKE ANTIBODIES

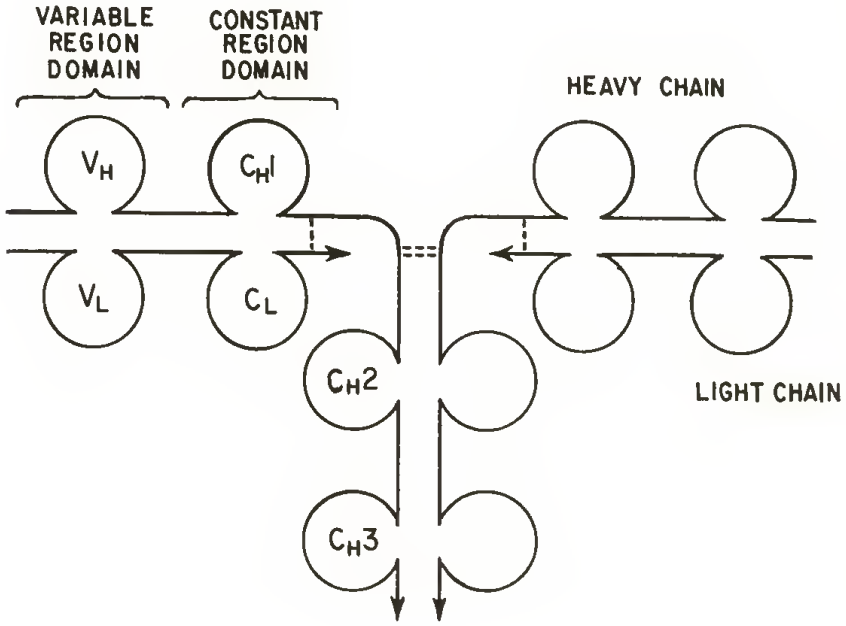
Rogers, John; *New Scientist,* 86:155-157, 1980.

When you have an infection, such as flu, your best defence lies in the fact that your body can recognize the molecules of the flu virus as different from your own molecules. Antibodies are the large proteins which recognise these differences. They can bind to the foreign molecules on the surface of the flu virus and trigger the body's defences against it. Thus antibodies are the essence of immunity against infection.

Antibodies are made by special cells of the immune system, in the blood and throughout the body. Each cell makes a single type of antibody. The antibody molecule consists of two identical

light chains and two identical heavy chains. Light and heavy chains are encoded by different clusters of genes.

Both types of chain are folded up into successive "domains", which are regions with similar structures and similar protein sequences. the first domain in each chain is called the variable, or V region, and is the part which recognises and binds foreign molecules. An animal may have a repertoire of a thousand different V_L (light) and a thousand different V_H (heavy) regions, which in combination account for up to a million potential antibodies.



Schematic of an average antibody molecule. The variable and constant regions are each on light and heavy chains. The heavy chains are longer than the light chains. By shuffling the variable and constant regions of both types of chains, thousands of different antibody molecules can be manufactured.

The remaining domains--one in light chains, three or four in heavy chains--constitute the constant, or C, region. Most animals make only two alternative C_L regions (κ and λ) and 5 to 10 alternative C_H regions ($\mu, \delta, \gamma, \alpha$, and ϵ).

The fascination of antibodies for geneticists lies in their diversity. A typical animal, which to an immunologist means a mouse, can product perhaps a million different kinds of antibody. They are enough to combat not only a vast range of natural hazards, but also a plethora of synthetic chemicals which are never en-

countered outside the laboratory. According to the central principle of molecular biology, all these protein molecules must be made by translation of genetic messages encoded in RNA molecules, and the RNA in turn must be transcribed from a DNA template. So the problem is this: how does the inherited DNA contrive to produce such a diversity of antibodies?

During most of the 1970s, a long scholastic debate dragged on between proponents of three different answers to this question. Now, these unproductive arguments have been superceded by actual data, as the new techniques of DNA cloning and sequencing have uncovered the genes themselves. And it turns out that all three schools were right! First, we each inherit hundreds of genes encoding different antibodies. Secondly, a single gene can undergo further alterations or "somatic mutations" within an individual. Thirdly, the inherited genes can be put together in combinations to increase the diversity still further.

. . . .

• Uneven Chromosome Splits

UNEVEN CHROMOSOME SPLIT

Anonymous; *Science News Letter,* 60:118, 1951.

Science now has its mystery of the missing chromosomes. When a germ cell divides in the process of building up the human body it has always been thought that the 48 chromosomes or particles within the cells, divide exactly evenly. Now Dr. R.A. Beatty, senior scientific officer of the Edinburgh University Genetics Laboratory, has told the British Association for the Advancement of Science that the neat picture of distribution of hereditary characters, that science has followed ever since the discovery of heredity by Gregor Mendel, is not true.

With the discovery of the chromosomes a classical picture was developed whereby the dividing fertilized germ cell equally distributed its chromosomes between its daughter body cells, which in turn again made an equal distribution of chromosomes between their daughter cells, and so on.

This makes a nice tidy picture and explains observed genetic phenomena beautifully, but, Dr. Beatty pointed out, now that scientists have begun to count the chromosomes in the body cells they are learning it just isn't true. Some recent counts made of chromosomes in human cells from different tissues show a random deviation of the chromosome numbers from the expected 48. In

most cases there were less than 48, though what happened to the missing chromosomes is still anybody's guess.

Further investigation of this unequal chromosome distribution situation is of vital importance, as the Soviet school of Lysenkoist geneticists are claiming that it bears out their contentions on somatic--body cell--inheritance.

• Possible Inheritance of Acquired Characters

LAMARCKIAN PROTEINS

Lindegren, Carl, C.; *Nature,* 198:1224, 1963.

Crick once stated explicity that the stability of the nucleic acid molecule ruled out the possibility that acquired characteristics might be hereditary--molecular biologists, in spite of a curiously detached attitude toward the philosophy of biology, have generally tended to support Weismann and to reject the views of Lamarck and Darwin concerning the effects of the environment on heredity.

But it has become increasingly apparent that the inheritance of acquired characters must be considered seriously, and Koshland, at the Atlantic City meeting of the Federated Societies, presented evidence acceptable to physical chemists proving that enzyme action depends on the alteration of the conformation of a flexible polypeptide chain of the enzyme by the substrate--a revelation which makes it essential to reintroduce Lamarckian concepts into molecular biology along the lines originally suggested by Pauling and recently exploited by Lindegren in speculations on the origin of life and the mechanism of gene action. If the conformation of an enzyme-protein can be controlled by a substrate, then the conformation of one protein can be controlled by the structure of an associated protein. Therefore, a change produced by an environmental effect on the structure of a proteinaceous organelle in one generation (by heat denaturation or by the oxidation of sulphydril groups) would be transmitted to the daughter organelles in the succeeding generation, if (as Koshland's theory suggests) the structure of the proteins already assembled into the organelle controlled the conformation of the protein molecules subsequently added to the organelle.

The only protein-containing organelles which could be exempted from transmissible environmental hereditary effects would be those which were completely disassembled at each generation. The abolition of most Dauermodifikationen by sexual reproduction,

but not by vegetative reproduction, suggests that the components of most organelles responsible for the transmission of acquired hereditary characters are disassembled when gametes are formed, but also implies that some sex cells transmit some altered daughter organelles in an altered condition to the next generation.

Yeast genetics has supplied many examples of genes which respond mutagenically and specifically to the effects of a substrate, but the facts have generally been discounted in the absence of the theory to justify their acceptance. Now that Koshland has demonstrated an acceptable mechanism, it is possible to give credence to those reports which stipulate that a carbohydrate substrate may direct the mutation of a gene controlling the production of an enzyme capable of hydrolysing that specific substrate. My receptor hypothesis assumes that the receptor homologous to the activator of McClintock and the operator of Jacob and Monod) is a heritable and mutable protein resembling the enzyme controlled by the gene. Koshland's communication at Atlantic City was followed by another in which Monod proposed that one component of the gene is a plastic protein, thus bringing the receptor-ad the operon-hypothesis into closer agreement.

Pirie has suggested that biopoiesis might have involved an early polyphyletic phase of long duration in which organic substances played only a minor part while inorganic catalysts were of primary importance. His view has been supported by Abelson's evidence indicating that the original prebiotic milieu was probably poor in amino-acids. It seems possible that primitive forms based on inorganic catalysts might have incidentally synthesized a pool of amino-acids.

Fox's experiments indicate that proteins might have arisen spontaneously in such a pool simply by exposing the mixture to heat. Subsequent to the accumulation of a protein-pool, a protein-based biopoiesis might have developed in which the catalysts were primarily polypeptide chains, and in which adaptability, hereditary stability and mutability depended on a folding process like that suggested by Pauling and demonstrated by Koshland. But one might suppose that the protein-based bionts were exceptionally sensitive to environmental effects and, hence, exceptionally sensitive to environmental effects and, hence, exceptionally unstable.

The development of a more stable living state based on nucleic acid must have followed rather than preceded the protein-based system, because of the fantastic requirements for proteinaceous enzymes which are imposed on an organism for the implementation of nucleic acid syntheses. The most primitive nucleic acid-bionts would have possessed two important advantages over (and would readily have replaced) even the most advanced protein-based competitors, first, because the reproductive systems of nucleic acid-based bionts are so much more stable and, secondly because nucleic acid-based systems are able to store capacities to synthesize adaptive enzymes in the form of 'self-reproducing' nucleotide sequences over long periods during which these capacities are not used.

. . . . .

• Selfish DNA: The Ultimate Parasite

SELFISH GENES, THE PHENOTYPE PARADIGM AND GENOME EVOLUTION

Doolittle, W. Ford, and Sapienza, Carmen; *Nature,* 284:601-603, 1980.

The assertion that organisms are simply DNA's way of producing more DNA has been made so often that it is hard to remember who made it first. Certainly, Dawkins has provided the most forceful and uncompromising recent statement of this position, as well as of the position that it is the gene, and not the individual or the population, upon which natural selection acts. Although we may thus view genes and DNA as essentially 'selfish', most of us are, nevertheless, wedded to what we will call here the 'phenotype paradigm'---the notion that the major and perhaps only way in which a gene can ensure its own perpetuation is by ensuring the perpetuation of the organism it inhabits. Even genes such as the segregation distorter locus of Drosophila, 'hitch-hiking' mutator genes in Escherichia coli and genes for parthenogenetic reproduction in many species---which are so 'selfish' as to promote their own spread through a population at the ultimate expense of the evolutionary fitness of that population---are seen to operate through phenotype.

The phenotype paradigm underlies attempts to explain genome structure. There is a hierarchy of types of explanations we use in efforts to rationalize, in neo-darwinian terms, DNA sequences which do not code for protein. Untranslated messenger RNA sequences which precede, follow or interrupt protein-coding sequences are often assigned a phenotypic role in regulating messenger RNA maturation, transport or translation. Portions of transcripts discarded in processing are considered to be required for processing. Non-transcribed DNA, and in particular repetitive sequences, are thought of as regulatory or somehow essential to chromosome structure or pairing. When all attempts to assign to a given sequence or class of DNA functions of immediate phenotypic benefit to the organism fail, we resort to evolutionary explanations. The DNA is there because it facilitates genetic rearrangements which increase evolutionary versatility (and hence long-term phenotypic benefit), or because it is a repository from which new functional sequences can be recruited or, at worst, because it is the yet-to-be eliminated by-product of past chromosomal rearrangements of evolutionary significance.

Such interpretations of DNA structure are very often demonstrably correct; molecular biology would not otherwise be so fruitful. However, the phenotype paradigm is almost tautological; natural selection operates on DNA through organismal phenotype, so DNA structure must be of immediate or long-term (evolutionary)

phenotypic benefit, even when we cannot show how. As Gould and Lewontin note, 'the rejection of one adaptive story usually leads to its replacement by another rather than to a suspicion that a different kind of explanation might be required. Since the range of adaptive stories is as wide as our minds are fertile, new stories can always be postulated.

Non-phenotypic selection. What we propose here is that there are classes of DNA for which a 'different kind of explanation' may well be required. Natural selection does not operate on DNA only through organismal phenotype. Cells themselves are environments in which DNA sequences can replicate, mutate and so evolve. Although DNA sequences which contribute to organismal phenotypic fitness or evolutionary adaptability indirectly increase their own chances of preservation, and may be maintained by classical phenotypic selection, the only selection pressure which DNAs experience directly is the pressure to survive within cells. If there are ways in which mutation can increase the probability of survival within cells without effect on organismal phenotype, then sequences whose only 'function' is self-preservation will inevitably arise and be maintained by what we call 'non-phenotypic selection'. Furthermore, if it can be shown that a given gene (region of DNA) or class of genes (regions) has evolved a strategy which increases its probability of survival within cells, then no additional (phenotypic) explanation for its origin or continued existence is required. (p. 601)

SELFISH DNA: THE ULTIMATE PARASITE

Orgel, L. E., and Crick, F. H. C.; *Nature,* 284:604-607, 1980.

The object of this short review is to make widely known the idea of selfish DNA. A piece of selfish DNA, in its purest form, has two distinct properties:

(1) It arises when a DNA sequence spreads by forming additional copies of itself within the genome.

(2) It makes no specific contribution to the phenotype.

This idea is not new. We have not attempted to trace it back to its roots. It is sketched briefly but clearly by Dawkins in his book The Selfish Gene (page 47). The extended discussion (pages 39-45) after P.M.B. Walker's article in the CIBA volume based on a Symposium on Human Genetics held in June 1978 shows that it was at that time already familiar to Bodmer, Fincham and one of us. That discussion referred specifically to repetitive DNA because that was the topic of Walker's article, but we shall use the term selfish DNA in a wider sense, so that it can refer not only to obviously repetitive DNA but also to certain other DNA sequences which appear to have little or no function, such as much of the DNA in the introns of genes and parts of the DNA sequences between genes. The catch-phrase 'selfish DNA' has

already been mentioned briefly on two occasions. Doolittle and Sapienza (see the previous article) have independently arrived at similar ideas.

The large amounts of DNA in the cells of most higher organisms and, in particular, the exceptionally large amounts in certain animal and plant species--the so-called C value paradox--has been an unsolved puzzle for a considerable period. As is well known, this DNA consists in part of 'simple' sequences, an extreme example of which is the very large amounts of fairly pure poly d(AT) in certain crabs. Simple sequences, which are situated in chromosomes largely but not entirely in the heterochromatin, are usually not transcribed. Another class of repetitive sequences, the so-called "intermediate repetitive', have much longer and less regular repeats. Such sequences are interspread with 'unique' DNA at many places in the chromosome, the precise pattern of interspersion being to some extent different in different species. Leaving aside genes which code for structural RNA of one sort or another (such as transfer RNA and ribosomal RNA), which would be expected to occur in multiple copies (since, unlike protein, their final products are the result of only one stage of magnification, not two), the majority of genes coding for proteins appear to exist in 'single' copies, meaning here one or a few. A typical example would be the genes for α-globin, which occur in one to three copies and the human β-like globins, of which there are four main types, all related to each other but used for slightly different purposes. Notable exceptions are the proteins of the immune system, and probably those of the histocompatibility and related systems. Another exception is the genes for the five major types of histone which also occur in multiple copies. Even allowing for all such special case, the estimated number of genes in the human genome appears too few to account for the 3×10^9 base pairs found per haploid set of DNA, although it must be admitted that all such arguments are very far from conclusive.

. . . . .

In summary, then, there is a large amount of evidence which suggests, but does not prove, that much DNA in higher organisms is little better than junk. We shall assume, for the rest of this article, that this hypothesis is true. We therefore need to explain how such DNA arose in the first place and why it is not speedily eliminated, since, by definition, it contributes little or nothing to the fitness of the organism. (pp. 604-605)

Chapter 11
LIFE CHEMISTRY

Chapter Contents

MORPHOLOGICAL PHENOMENA

• Life-Like Chemical Forms

THE CREATION OF "ARTIFICIAL LIFE"

Gruenberg, Benjamin C.; *Scientific American*, 105:231+, 1911.

From time to time we are informed that the Riddle of the Universe has been solved by the artificial production of "life" from non-living materials. But each time we wondered how it was done, for a few days, and then find out that it wasn't done at all. The riddle is still there. The expression "artificial life" has been used in recent years for two entirely different sets of ideas.

On the one hand is the attempt to make artificial combinations of matter behave like living protoplasm--that is, to make live matter out of the non-living materials lying all about us. On the other hand is the attempt to make the eggs of various animals develop without the co-operation of the sperm--or to produce "artificial parthenogenesis," as it has been called. Both kinds of experiments are calculated to throw much light on the fundamental nature of "life;" but they differ considerably in their methods as well as in the point of view that prompts them. We shall discuss first what has been done toward the making of "artificial life" in the cruder sense.

The problem of artificial life is connected intimately with that of the origin of life, and also with that of the characteristics or distinctive properties of living matter.

From earliest times there has persisted the idea that plants or animals may arise "spontaneously" from non-living matter, not withstanding the common experience of mankind that wherever the origin of a living thing could actually be observed, it was seen to arise from another living thing. This widespread belief in "spontaneous generation" no doubt owed its tenacity to the fact that in many cases the continuity between generations could not be directly observed. For example: Eggs of eels had never been seen until within our own times; maggots were known to develop in putrifying meat, but no one had seen the flies deposit their eggs in the meat; the presence of tape-worms and other parasites within the bodies of human beings and other animals; the presence of insects within the "galls" of plants, without any apparent means of entrance--all these facts served to perpetuate

the idea that life can, and regularly does, arise spontaneously. The idea that all organisms arise from pre-existing organisms is a comparatively modern one; and it was accepted on philosophical grounds long before there was any actual demonstration of the principle. From the nature of the case, however, the principle omne vivum e viro cannot be proven, since it involves a negative. It is equivalent to the statement that living matter does not arise from non-living matter. In the seventeenth century Francisco Redi, a physician of Florence, showed that by preventing the access of insects to meat, no grubs or maggots could be formed in it, no matter how much it putrefied. In opposition to these experiments, a Scotch clergyman named Needham attempted in the middle of the following century to show by means of experiments that "wheat-eels" and other animals do arise spontaneously. He showed incidentally that tiny animalcules appeared in extracts of vegetable matter, on long standing, even after these had been boiled. But his contemporary, the Italian monk Spallanzani, showed that if the decoctions were thoroughly boiled and closed against the entrance of dust, there would be no appearance of life. Although Spallanzani's experiments were not beyond criticism, his conclusions were in harmony with those accepted by practically all biologists today--namely, that all existing plants and animals have originated from others of essentially the same kind.

But although the notion of "spontaneous generation" has been thus abandoned by all scientific thinkers in modern times, the question of the origin of life took on a new interest with the spread of evolutionary thought during the latter part of the nineteenth century. Darwin did not attempt to answer this question. He took for granted the origin of life, in some simple form, as the beginning of that evolution to higher forms which he attempted to explain with his theory of natural selection.

Apart from the theory of special creation for each species, there is only one opinion possible as to the origin of life, and that is that at some time in the past non-living material passed into a living condition--in other words, that life did originate "spontaneously!" This notion of spontaneous origin of life must not be confused with the older notion of the spontaneous generation of plants and animals of all degrees of complexity.

Alfred Russell Wallace and others think it necessary to assume that the origin of life from non-living matter was brought about by the interposition of some spiritual or divine influence, of the nature of which we know nothing. Charlton Bastian and another school of biologists think that the transition of non-living of biologists think that the transition of non-living matter into the living state has taken place continually from the first, and is taking place now. But most biologists accept the view that living matter originated from non-living in the very remote past, and that all living things now existing are derived by a succession of generations from the primitive origins.

The Swedish physicist Arrhenius and the Englishman Lord Kelvin are strong advocates of the view that life upon this earth was developed from primitive "germs" that were brought here in

the remote past "upon the wings of light" or upon some meteorites, through lifeless space. It has been pointed out a number of times that this account of the origin of life simply shifts to some other planet or system the theater of the origin, without giving us any suggestion as to the manner of the origin.

The requirements of any evolution theory are met by the assumption that life originated "spontaneously" from non-living matter at some time in the history of this planet, or of the universe. But to know whether such an assumption is warranted, we should know more about the characteristics of living matter. Until comparatively recent times the processes going on in living plants and animals had been so little studied that the idea of life being dependent upon external conditions was entertained by very few men. With the development of modern physics and chemistry, and with the application of the methods as well as of the results of these sciences to the study of living things, this idea rapidly spread, until it is now pretty generally accepted. Moreover, the strong tendency during the past two generations to unify all knowledge, and to extend the principles of one science to the problems of all others, have stimulated the search for the physical and chemical foundations for "vital" processes.

Of all the lines of research thus developed, only three need here be discussed.

The word "protoplasm" had a varied career for about twenty years; but about fifty years ago it began to be definitely applied to "the physical basis of life," or the substance found in the cells of all plants and all animals. We know a great deal about this peculiar substance--or rather combination of substances; but there is still a great deal more that we do not know. There have been many opinions as to the structure of this living matter--that it is fibrilla, that it is like a net-work, that it is a mass of foam. The microscope does not tell us all that we should like to know--and it tells us one thing at one time, and other things at other times. It is quite possible that the "structure" of protoplasm is different in different organisms; or that it is different in different parts of the same organisms; or that it is different within the same cell under different conditions. At any rate, Prof. Butschli, to demonstrate his idea of the "alveolar" or foam-like structure of protoplasm, mixed some finely powdered potassium carbonate with olive oil that had been heated from 125 to 140 deg. F. The oil becomes partly saponified, producing an acid. This acid reacts on some of the carbonate, producing minute bubbles of carbon dioxide. Under the microscope a drop of this frothing mixture looks like a speck of protoplasm, and the movements in it even resemble the streaming in a living cell.

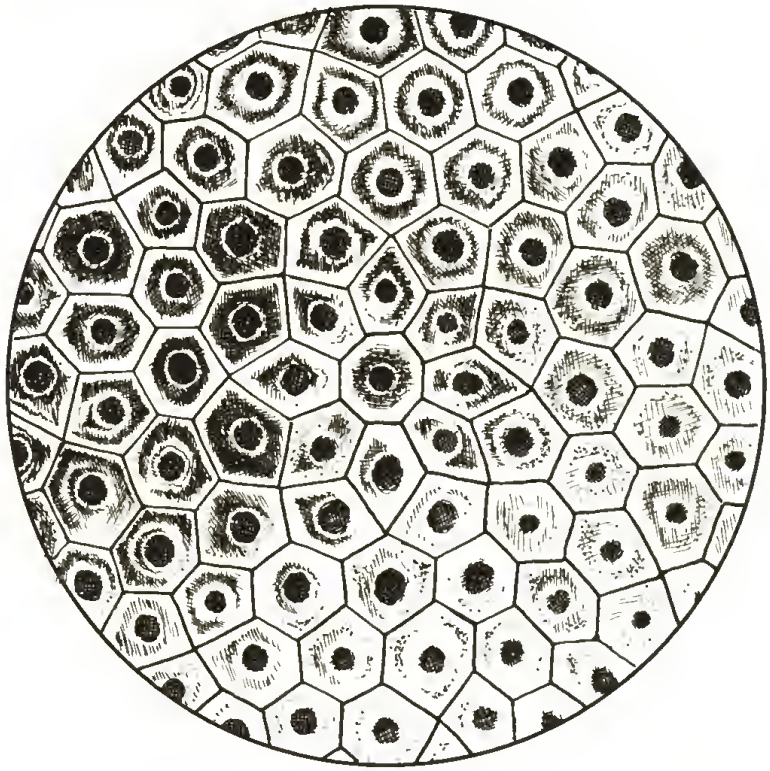
Some fifteen years ago these experiments were hailed by certain journalists as successful attempts to produce artificial life. But no one who knows anything about the problem, least of all Prof. Butschli, thought that these experiments had anything to do with the creation of life. The experiments were successful in the sense that they gave us a working model made of familiar materials, to help us understand the minute structures and movements within the cell. How hard error does die in suggested by

the fact that these experiments have been described over and over again in all grades of publications as a method for making "artificial protoplasm" until this very year.

After the fact of cell-structure in organisms had become well established, one of the problems that interested the experimentalists was that of the growth of the cell wall, which to all appearances is not itself alive. To test the theory that growth is produced by a stretching of the membrane with subsequent deposition of new material in the interspaces of the old wall, Moritz Traube carried on some very suggestive experiments. He allowed a solution of sugar and gelatine to come in contact with a solution of tannin through the narrow opening of a small tube. Now as soon as the tannin touches the gelatine it forms a membrane through which the water may easily pass, but through which the sugar cannot pass at all. The globule within the membrane continues to absorb water from the tannin solution by osmosis, stretching the membrane. The membrane grows, and under certain circumstances puts forth branches in various directions, the whole taking in the appearance of some weird plants or "sea-weeds."

Traube's further experiments, from 1865 on, and those of others, extended our knowledge of osmosis, threw some side lights on the mechanism of the cell-growth, and furnished amusement to many persons, for these osmotic gardens are curious and somewhat mystifying. But no biologist has in all these years suspected that Traube's phenomena were in any way related to "artificial life" until a few years ago Dr. Stephane Leduc, professor of physics in the medical faculty of the University of Nantes, announced that he had discovered the physico-chemical foundations of life. He presented before the French Academy of Sciences a large number of demonstrations, in which he made "plants grow from artificial seeds." These seeds consisted of sugar plus some salt that would form a precipitation membrane with the substances of his growing medium. For example, copper sulphate in a "seed" would cause growths in a solution of potassium ferrocyanide. These growths were in no way different from those obtained by Traube and others, except that Leduc used a larger variety of substances in his experiments than had been used by other experimenters. Anyone can make for himself a garden of such "artificial plants" with very little trouble. Perhaps the easiest way is to place at the bottom of a jar a number of crystals of salts of heavy metals (e.g., sulphates of copper, iron, zinc, cobalt) and carefully pouring over them a 10 per cent solution of sodium silicate or "water glass." The insoluble silicates of the metals will form the membranes, osmosis will cause them to stretch, variations in the density of the medium will cause irregular branching, and the colors of the salts within the membranes will give a further resemblance to "plants."

Leduc went farther,however, and produced imitations of living structures down to the smallest details. By placing a drop of colored salt solution into another solution of lower concentration, he produced "artificial cells." More definite cell membranes are produced by placing drops of 10 per cent solution of potassium



"Artificial cells" formed by the diffusion of 10% potassium ferrocyanide in a 10% solution of gelatine

ferrocyanide in a 10 per cent gelatine solution. The appearance of nuclear division within a cell he produced by placing two drops of some solution near each other within another solution. That all these phenomena are due to osmosis or diffusion is well known to all physicists; that they are not in any true sense identical with "growth" and "nuclear division" may be known only to the student of biology. Among other demonstrations made by Leduc in this connection was the formation of a "field of force" by the diffusion of liquids of different densities.

That the "artificial life" of Leduc had no real bearing on the fundamental physiological problems has been shown repeatedly, and Prof. Leduc has in later statements denied that he confused his phenomena with life processes, although his original confusion is on record in the proceedings of the Academy. Perhaps the best analysis of Leduc's results is that of Prof. Maurice D'Halluin, director of the physiological laboratory at the University of Lille. This experimenter not only repeated all of Leduc's "demonstrations" but even showed that the various weird forms assumed by the "artificial plants" could be controlled by changing the concen-

tration of the solutions, or by changing the proportions of the different materials used. Nevertheless we still find fairly frequent references to Leduc's overstepping the threshold between the non-living and the living by means of "artificial seeds."

In the spring of 1905 we had another "artificial life" sensation in the announcement that Mr. John Butler Burke, "of the Cavendish Laboratory of Cambridge University," had obtained some curious, life-like structures from the action of radium upon sterilized bouillon. Mr. Burke was following up a suggestion made many years before by Prof. E. Pfluger of the University of Bonn, that the essential difference between living protein and non-living protein lay in the fact that in the former oxidation or transformation of energy is internal, and that this internal oxidation could perhaps have been induced by cyanogen, a compound of carbon and nitrogen which was probably produced during the incandescent stage of the earth's history. Having failed to induce phosphorescence or other indications of the internal transformation of energy in organic substances by means of cyanogen, Mr. Burke resorted to radium.

The test-tubes containing gelatine and beef-extract in the proportions used for bacteriological cultures were properly sterilized, and a minute quantity of radium chloride or bromide was added. The appearance of the gelatine changed, suggesting bacterial growths. Under the microscope there were seen tiny roundish bodies that increased in size, divided and moved about slightly! This was certainly life-like. In order to make sure that they were not bacteria due to contamination, the tubes were examined by Dr. Sims Woodhead, professor of pathology at Cambridge. This authority declared they were not bacteria at all. They were not even living things, for they dissolved in water; they disappeared entirely after standing in diffuse daylight for a little while, to reappear after a few days of darkness. Moreover, on being transferred to fresh bouillon, they did not increase in numbers.

But the world was clamoring for artificial life; Burke could not lose so good a lead. Instead of calling his new discovery bacteria, which they were not, he called them "radiobes," implying that they were life-like things produced by radium. They were unlike any other living thing, so Burke set to work and elaborated a new definition of life, so that the radiobes could fit in. Otherwise he should not have created "artificial life."

That the radiobes were not living things, in the accepted sense of the word living, must be obvious to nearly everyone. But what are they then? Prof. Sir William Ramsay explains these structures are due to bubbles of hydrogen and oxygen gas (arising from the decomposition of water by the radium salt) and "radium emanation." The "growth" is due to the continuous production of more gas by further decomposition of water by the emanation within the bubbles; the expansion of the gas might also lead to the breaking up of the bubbles into two or more.

At any rate, the water-soluble radiobes are not alive, and it was later found that they could be produced in the bouillon by other substances besides radium, such as salts of barium, stron-

tium, and lead. "Radiobes" produced by these substances also have "nuclei" consisting of insoluble sulphates of the metals used.

The failure of scientists so far to produce "artificial life" is not to be charged against the science of biology. Very few of the attempts to produce "artificial life" have been made by biologists, who realize too well the complexity of the problems involved. The biologists will be satisfied for a number of years to come if they succeed merely in analyzing what goes on in a living cell, in terms of physical and chemical processes. From time to time they will attempt to imitate a structure of a process by means of a working model; but they will not speak of artificial life until they are quite sure of all the conditions that play a part in this most intricate of phenomena.

THE LIVING CRYSTAL

Anonymous; *English Mechanic*, 69:573, 1899.

From time to time statements have been made at the various International Medical Congresses as to the investigations of Dr. Otto von Schron, professor of pathological anatomy since 1864 in the University of Naples. In a communication signed "H.J.W.D.," the Daily Chronicle publishes some remarkable statements, the substance of which will be found in the following:-- Dr. Von Schron has discovered something whose name alone suggests its great importance to chemistry, pathology, and philosophy. It is the Living Crystal. He has discovered that living matter, largely albuminous in character, takes the crystalline form, and, while still living and crystalline, obeys so many of the laws and manifests so many of the properties of inorganic crystallisation, as to have no doubt whatever of its crystalline character. Prior to this discovery, crystallisation, as we know it in the diamond, the rock crystal, rock sugar, and similar familiar forms, was one of the profoundest concrete mysteries of science. That inanimate and non-conscious particles of matter should, of their own accord, arrange themselves in perfectly symmetrical forms, and always assume the same form under the same conditions, has been a phenomenon so extraordinary that many theories have been conceived to account for it, while none of them have been entirely satisfactory. It is as if one threw a handful of pebbles out of the window, and saw them form themselves, on the ground, into a hollow square, and always form precisely the same hollow square when the experiment was repeated. Amidst all the theories, no one seems to have hitherto found what is probably the true one-- viz., that crystallisation in its terrestrial origin was a manifestation of the force called life force.

. . . .

The first living crystals were seen by the Doctor as long ago

as 1886. It was so incredible a sight that he could not at first believe it. They were the crystals of the bacilli of Asiatic cholera, long needle-shaped prisms. All the bacilli thus far examined produce distinct crystals of different forms. The bacillus subulis, for instance, forms bayonet rhombs, the bacillus tenise forms hexagonal prisms. The crystals of tuberculosis are square rhombs: of anthrax, elongated rhombs: any given bacillus being immediately identifed by its crystal, which never varies in the shape assumed in its original formation.

These objects, as anyone may see, are perfect crystals in form. Yet, as anyone may also see, they are alive, and their life, their motion; their reproduction are as visible and undoubted as their death, when it ensues, is undoubted. Their death occurs when all the living matter which originally formed part of the crystal has eliminated itself. Upon death they become the crystals that we know, ordinary mineral crystals. The duration of life is indefinite. All the changes in condition from "granular" to "molecular" and "atomic" may take place in a term of hours, according to conditions of temperature and development, or the life of the crystal may be maintained by other conditions through any extent of time. The Doctor has many living crystals, notably of tuberculosis, which have been under observation for years.

THE ARTIFICIAL CREATION OF LIFE

Tingley, Charles Edward; *English Mechanic,* 82:247-248, 1905.

Succeeding the experiments of Loeb, and prior to those of Burke, were those announced by Dr. Charles Littlefield; but since the claims of the latter were so exceedingly broad, and the methods employed so very loose, the scientific world has paid very little attention to them. Nevertheless, a widespread interest has been created in the man and his work by the popular press, for the subject is one which appeals no less strongly to the lay than to the technically-trained mind. For this reason a critical review of his experiments may not be ill-timed.

What lends a glamour to the researches of this biologist is the fact that he cherishes the illusion of having actually produced not only the simple organic cell, but also a much higher and more complex form of life. The method by which he has generated supposed life in a sterile soil he does not seek to conceal; but, instead gives a clear and connected account of it as well as of the theory upon which it rests, and though one may well find fault with the first, certainly no objection can be raised to the second.

The following instructions and description of the operation have been given by Dr. Littlefield, by which the micro-organisms are supposed to be produced. The supplies are of the simplest kind, and can be obtained in any drug store. These comprise a large, but shallow, glass vessel, having a capacity of one quart, several

smaller glass dishes, a bell jar sufficiently large to inclose these receptacles, and, finally, a good high-power microscope. The chemicals used are sodium chloride, or common table-salt, alcohol, ammonia, and distilled water. In the larger vessel 2 oz. of the salt is dissolved in 6 oz. of the water, and when this is done 6 oz. of 90 per cent. pure alcohol is added.

A portion of the solution thus formed is poured out of the larger into the smaller dishes, when 2 oz. of officinal aqua ammoniae is stirred in with a clean glass rod, and the bell jar is then placed over them. A chemical reaction is set up, and in the course of a few minutes bubbles of hydrogen will begin to form on the surface of the fluid, and a closer observation will show these little spheres to be gyrating with high velocity. In the course of half an hour the bubbles will cease to form; the liquid is then ready for the crucial test. With the microscope at hand, and previously focussed so that a globule of the unstable solution may be quickly observed, a very small portion is transferred from the dish to the glass slide, where the latter is adjusted on the stage and a magnified view is had. On examination detached particles of matter are seen moving through the medium from the centre to the circumference with extreme rapidity, and continued investigation indicates other changes the liquid is undergoing. Crystals begin to appear, and those first formed are the characteristic transparent cubes of sodium chloride, and hence these are incapable of further development. After these other crystals follow, and some assume a hexagonal form on the surface of the saturated solution, and it is from these latter minute six-sided bodies that the growth of the elementary organisms is said to take place.

The point is now reached, according to Dr. Littlefield, where the intangible force we know as life joins the lifeless matter, as current electricity energises a coil of wire, and a microscopic organism possessing what Herbert Spencer defines as the "co-ordination of actions" begins its existence, which consists of a series of definite and successive changes, both in structure and composition, which take place within itself and without destroying its identity.

The growth of this supposed rudimentary vital element next follows in sequence, as it is metamorphosed from the hexagonal crystal into a free, smooth, disc-shaped cell, we are informed that it bears a close resemblance to a red-blood corpuscle. The cellular disc now gradually expands in a direction at right angles to its surfaces, and an ovoid form results, from which pseudopodia or temporary extensions protrude, similar to the amoeba, and which in the latter are designed to take in food, for locomotion, &c.

In commenting on his achievements, the doctor says: "I have carefully watched the development of a large number of these cells or germs, and they do not vary in the least detail as to their growth from the above description showing unmistakable design and the actuality of life's processes. Moreover, mineral substances do not change, except by accretions from without, and then not always in regular form and order. From the result of my

experiments, I am forced to conclude that there are two factors responsible for the manifestation known as life; one is a force or influence due to certain vibrations of the ether, and the other is a certain combination of atoms so arranged as to be capable of responding to these inpressed vibrations. As an illustration, they act somewhat as the rods and cones of the optic nerve in the retina of the eye, which are so constituted that they may receive and focus certain vibrations of the luminiferous ether, giving us the phenomena of light and the sensation of sight. So there are combinations in nature so constituted and arranged in their atomic structure as to arrest the vibrations which act as electromagnetic manifestations of a higher order than those of light; and these give us the phenomena of physical life, and the physical basis of this compound is salt, ammonia, and water in the presence of hydrogen, easily obtainable from alcohol, which is made up largely of this gas." Dr. Littlefield goes much further, and carries his huge claims to the startling extent of affirming that he has produced a full-fledged insect which, though invisible to the naked eye, under the microscope became an entomological object the like of which has never been seen before. "It resembled an elongated house-fly" (to quote the doctor again) "having two antennae protruding from its head, while from its body grew six attenuated legs, the two nearest its head being of the comparative form and length of a grasshopper, while its transparent wings were covered with light-coloured hair. This new insect is the outcome of thousands of experiments, and it has no counterpart in the textbooks dealing with that branch of zoology."

It is a far cry from a simple protoplasmic cell to that of a highly organised insect such as that just described--in fact almost as far as it is from lifeless crystals to living matter. Oppositely, the higher critics will have none of it, basing their conclusions on practically the same grounds that Prof. Tyndall took in relation to Dr. C. Henry Bastian's experiments nearly thirty-five years ago. This scientist, it would seem, was eminently qualified to investigate the origin of life, for he was recognized as an authority on biology and the pathology of the nervous system, and he was a strong advocate of the doctrine of spontaneous generation of life. In one of his many papers he pointed out the results he had obtained in creating life artificially, and he declared that "observation and experiment unmistakably testified that living matter is constantly being formed de novo and in accordance with the same laws and tendencies which determine all the more simple chemical combinations." Prof. Tyndall took up the matter and carefully tested Dr. Bastian's experiments, but took precautions, which the latter had neglected, to prevent the ingress of life during the processes of sealing the vessels, and though he varied the experiment in many ways no germs of life manifested themselves, so that Tyndall felt impelled to thus testify: "I affirm that no shred of trustworthy evidence exists to prove that life in our day has ever appeared independent of antecedent life."

The moral of Tyndall's statement is obvious; the value of Dr. Littlefield's or anyone else's experiments in the artificial genera-

tion of life lies absolutely and solely in excluding every trace of pre-existing life and thus preventing contamination which must otherwise surely follow during the progress of the tests. Carelessness in this respect has led biologists, even those who believe in the hypothesis of abiogenesis, to cry down every attempt made looking toward the artificial production of life. At various times Spencer, Huxley, Darwin, and Pasteur were firmly convinced that they had found the secret of life, but repeated experiments wherein antecedent life was more rigorously excluded than before proved their efforts futile.

Evidently error of a similar nature has crept into the tests of Dr. Littlefield, and this is not said without due consideration, for the present writer has performed the experiment as above written, not one but many times, and in every instance the result was not successful beyond the mere crystallisation of the chlorides.

It is true that more recent reports state that the development took place under sealed glasses, thoroughly sterilised before beginning and sealed from the air when placed on the shelf; but it is obvious that there was every chance for pre-existing life to slip in, and so what would otherwise have been regarded as a wonderful achievement in science has not been taken very seriously by men skilled in either chemistry or biology.

RADIOBES

Anonymous; *English Mechanic*, 82:292, 1905.

Mr. J. Butler Burke, of the Cavendish Laboratory, Cambridge, expounded his theory of radium as a possible source of vital energy at the Manchester Athenaeum last week. There was a large audience.

At the outset Mr. Burke said it was seven years since he left Manchester for the South, and it was a great pleasure to him to return and to lecture in connection with the Manchester Athenaeum. He would leave it to others to say what bearing his experiments had upon the problem of the origin of life or whether they had anything to do with that question at all. But he could not be called heterodox if he suggested that living matter might have sprung from lifeless matter. The ancients Hippocrates and Aristotle had held that doctrine, and in the Middle Ages Augustine and a host of writers. It was first considered seriously by an Italian philosopher named Rede, who came to the conclusion that whever life appeared it sprang from pre-existing life. Investigation was continued by Needham, Buffon, Pasteur, Bastian, Tyndall, and Huxley. Pasteur sterilised some meat, inclosed it so that no bacteria could come in contact with it, and, finding that no form of vital energy arose, came to the conclusion that life could not arise from dead matter. Upon this Mr. Burke commented that spontaneous generation might take aeons to arise. Later experiments with a gelatine medium had shown that by the action

of sodium chloride artificial cells were produced in the gelatine, and these cells went through some of the processes which living bodies appeared to go through. Examined by means of the polariscope, these artificial cells, which the discoverer named liquid crystals, were found to have the optical characteristics of crystals.

Mr. Burke proceeded to describe his own experiments with sterilised bouillon (to which gelatine had been added to give it solidity) in a test tube, which was not sealed, but stopped up with cotton wool. The object of using wool was to enable the experimenter to raise the bouillon to a high temperature by compressed air. A much smaller tube containing radium salts was placed within the larger tube, and when the process of sterilisation had taken place a wire connected with this smaller tube was pulled out, so that the radium tube was broken, and the radium came in contact with the gelatine and the bouillon. After twenty-four hours a remarkable culture appeared on the surface of the gelatine, which made its way downwards into the medium. This culture consisted of a number of minute globules, which had the appearance of crystals. When the tube was opened and the medium examined under the microscope, a number of dumbbell-shaped bodies were found, which, after a week, became biscuit-shaped, and in a fortnight began to disintegrate and reproduce. The reproduced bodies did not show the vigour of the original bodies, and the sub-cultures were not formed so rapidly as in the case of bacteria. These forms dissolved in hot water and in daylight, so they were not mere contaminations. Sir William Ramsay had suggested that these radials might be of the nature of bubbles formed by the action of the radium on the gelatine. But that theory did not fit in with the facts which Mr. Burke had observed. His own view was that the radials resembled in some degree the more elementary types of life which arose in the remote past. In face of the evidence it could not be said that they were not living things. It was possible and probable that here they had the dawn of the beginning of life. It was conceivable that in the vast aeons of the past the radio-activity of the earth itself acting upon protoplasm may have given rise to life. It was not enough for us to think Imperially--we must think in aeons if we would become familiar with the nature of this problem. It was eminently desirable that we should attribute to matter not merely physical properties; we must suppose matter to contain potentially at least, the properties of life and mind. That would remove a great barrier between physical and mental effects.

SOFT CRYSTALS SHOWING APPARENT LIFE

Gradenwitz, Alfred; *Scientific American*, 95:387, 1906.

The present tendency of physical science is rather toward evidencing a continuity between provinces that were formerly entirely separated from each other than to establishing new

boundaries corresponding to new categories. The impossibility of establishing a definite boundary between solid and liquid states has been evidenced in the course of the last few years by the work of Prof. O. Lehmann, of Karlsruhe, Germany, whose researches on liquid crystals deserve the highest interest the more so as the soft crystallinic forms produced and investigated by this physicist show some striking analogies with the world of living beings, thus constituting another link in the chain of recent researches on the boundary between living and apparently dead matter, to which attention has been drawn especially by Butler Burke's recent investigations. As a complement to these researches on one hand and to Prof. Leduc's work on the other (of which the writer has published an account in a recent issue of the Scientific American) the following observations may be of interest.

An organic substance called para-azoxy-cinnamic-acid-ethyl-ether, obtained in the "fleeting" crystallinic state between 139 deg. and 248 deg. c., is one of the most remarkable substances susceptible of assuming this condition. In fact, the phenomena observed under the microscope seemingly show a perfect analogy with the phenomena of living beings, so that partisans of the ancient theory of spontaneous generation avail themselves thereof in establishing the basis of their theory. While modern science abhors premature conclusions, so far from discarding such phenomena as are incompatible with present laws, it should most thoroughly and without any prejudice examine any facts brought to its notice. In the present case its task will be to investigate how far the forces working in the living organisms agree with the forces of lifeless nature. A striking difference in the behavior of these two classes, as so far observed, has been that while the growth of living beings takes place in virtue of internal absorption ("intussusception"), and while by the copulation of two individuals into a single one, or the subdivision of one individual into two or more, an increase or decrease in their size is produced, the crystals so far known would grow only in virtue of the gradual adding together of molecules. Now the substance referred to in the beginning just shows the same phenomena as were so far attributed solely to living matter, and in addition exhibits some most striking motional phenomena, that are quite analogous to those of micro-organisms. So far from considering these soft crystals as living beings, Prof. Lehmann suggests that they fill up a gap in our knowledge of molecular effects, the forces acting in the case of both classes being possibly identical.

When heating a small amount of the substance above mentioned, after moistening it with some monobromine-naphthaline as solvent until only a few particles of the jelly are left, and cooling to about 200 deg. c., some short, square columns with rounded edges and angles will, under the microscope, be found to be separated, showing in some cases the shape of pyramids. The lower the temperature, the less will be their tenacity, possibly owing to their absorbing some of the solvent in a way analogous to other crystals, dyeing stuffs, etc. While being colorless when inspected

in a longitudinal direction, they show a yellow and sometimes a reddish-yellow tint on being viewed transversely.

Whenever two individuals come in contact with each other they are seen immediately to combine like two oil drops, especially in case their position is a corresponding one. In the event, however, of their being placed in oppostion, the summit and base of the pyramids coinciding, twin crystals will be produced, showing at the juncture, owing to the refraction of light, a cross on a gray rhomb.

While crystals of an oily consistency are produced as the temperature continues dropping, they are no more able to withstand the pressure of surface tension (increased owing to the decrease in temperature, and which acts like an elastic membrane encompassing the whole) so as to be compressed to spheres, the crystalline structure of which is only manifested by their special refraction. If any one of these spheres is turned over, it is found not to be absolutely round, but to show a flattening or funnel-shaped depression at some point, from the middle of which a dark straight line leads to the centrum of the sphere. If the latter be so placed that the depression is situated either on the top or underneath, everything being symmetrical round the center, it will show a set of concentric circles; this is what is called the "first main position." If on the other hand the depression be situated sideways, the dark stroke will be seen leading from the center to the periphery; this is what Prof. Lehmann terms the "second main position." If now two drops combine in the first position, one drop with only one core or center and one depression will be obtained; and the same in the second main position if the positions be corresponding ones. If, however, the depressions be placed in opposition, both will be maintained in the resulting drop. If finally two individuals strike each other at their depressions, a twin form will be produced, the spheres being combined without flowing together into a single one. Such twin structures may also be produced spontaneously, an extension growing out of the depression of a drop gradually taking the size of an equivalent sphere. This shows a perfect analogy with the springing up of a bud, as observed in the case of microorganisms, this bud being thrown off after some time, and continuing its existence as an independent individual of the same species as the original. A perfect analogue to the ordinary phenomenon of subdivision as observed in the case of the smallest organisms is likewise noted. The drop in the first main position being frequently drawn out to a bacterium-shaped small rod, this being suddenly divided into two pieces. Before this separation a sort of partition wall is observed at the dividing point, as ascertained by a strange light refraction due to the twin position of the molecules.

So far from being at rest, the drops are susceptible of rotating around their axis. The miniature rods frequently show phenomena of motion analogous to those of the diatoms, being a slow creeping both forward and backward, while passing through what seem to be obstacles to the motion. The small rods in some cases suddenly take the shape of long serpents, bending incessantly with great

energy, and even moving from their ordinary position and performing what seems to be a peristaltic motion. The growth of these serpents, which is evidently intensified by cooling, actually forms an analogy to growth by internal absorption (intussusception) in the case of living organisms. Though the separation of substance occurs at the surface of the serpent, its thickness remains perfectly constant, there being only an alternation of its length. Any molecules joining the structure are evidently drawn immediately into the interior dispersing the existing molecules. Sometimes such a serpent will expand with extraordinary speed, covering the whole field of vision of the microscope, while sometimes disappearing before the eyes of the observer. If the latter succeed in studying the various phases of the phenomenon, he will see the serpent to be instantly contracted to a sphere, thrown away by the force of contraction.

Similar phenomena are observed in the case of the ordinary rods. These will in fact bend into a ring, while a contraction to a sphere occurs as soon as the ends meet. A similar effect is observed in the case of the contact of two rods, while a combination of rods to twins and threefold structures is observed in some cases. Serpents will sometimes spring up from the depression of drops in the second main position, or else a given serpent will give rise to the production of a thinner one, or else its thickness will gradually decrease during growth, resulting in a structure analogous to a germ filament, showing a similar oscillatory motion of the tail.

Rods and serpentine structures are frequently observed in the case of the separation of a bud connecting the latter to the mother individual as well as in the case of a subdivision. The addition of foreign substances may result in some kind of poisoning, the phenomena of motion being slackened or the morphogenetic force vanishing, or else some disfiguration being produced. Even the absorbing force of the glass may result in disturbances, the drops being attracted by it. A multitude of remarkable structures is formed even in normal conditions, a serpent being, e.g., suddenly separated into a chain of droplets, or else into a miniature rod showing expansions which are gradually converted into a drop.

LEDUC'S ARTIFICIAL PLANTS AND CELLS

Gradenwitz, Alfred; *Scientific American,* 96:234-236, 1907.

A strong reaction against the somewhat childish endeavors of the alchemists to convert one element into another and to generate living beings from inert matter, pervades the history of nineteenth century science. Perhaps we have been prone rather too eagerly to discard the doctrines of former times, banishing many theories which in the course of the last few years have again been found worthy of serious discussion.

We are no doubt at present on the eve of great revolutions

in our scientific views; the phenomena of radioactivity have shaken the belief in the immutability of the atom and even the principle of the preservation of matter, at least in its familiar form. Nor does the distinction of three strictly separated states of aggregation stand the test of the recent investigation; transitions are found to exist between the different states, and we are warranted in presuming that between the material and the immaterial (the luminous ether) there are likewise numberless intermediary states. Finally there have been discovered transitional stages between inert matter and living beings, from which many interesting conclusions in regard to the nature of life can be drawn.

While Prof. Lehmann's recent researches on apparently living crystals have shown that certain bodies, mineral in outward appearance, behave like living organisms of the lowest type (bacteria), Prof. Leduc, of Nanges, has found the vital functions in animal and vegetable cells to be controlled exclusively by the physical laws of diffusion (osmosis) and cohesion (molecular attraction). On the basis of these phenomena he has even succeeded in artificially producing objects which, not only in appearance but in behavior, closely resemble natural cells, growing, absorbing food, and propagating themselves in exactly the same way.

The botanist might be somewhat embarrassed when asked to incorporate in his familiar system of classes, orders, and families the forms illustrated in Figs. 1 to 4 [not reproduced]. Still he would hardly have any doubt of their genuineness, their whole aspect being typical of representatives of the vegetable kingdom, especially of certain water plants.

Nevertheless, they are not living beings of any sort, but artificial bodies formed in the laboratory of the chemist. While their very aspect is certain to inspire interest, it is obviously far more interesting to observe them in the making, to watch how from an artificial seed a shoot springs and develops (at a rate readily controlled by the experimenter) into stems, leaves, buds, twigs, ears, and blossoms, and after some time dies like a real plant. The birth and death of a plant can thus be artificially reproduced within the space of a few hours.

Below are given some details concerning the artificial seed and the medium in which it is immersed for germination. A seed one to two millimeters in diameter, consisting of two parts of saccharose (cane sugar) and one part of copper sulphate, is immersed in an aqueous solution containing two to four per cent of potassium ferrocyanide, one to ten per cent of sodium chloride or some other salt, and one to four per cent of gelatine. In this solution, the seed germinates in a few days or a few hours according to temperature; under favorable conditions the germinating process can even be shown as a lecture experiment in a few minutes.

The seed surrounds itself with a membrane of copper ferrocyanide which is permeable to water and to certain ions, but is impermeable to sugar. This semi-permeability produces a high osmotic pressure in the interior of the artificial seed, resulting in the absorption of matter from the surrounding medium and thus

in the growth of the whole structure. If the liquid be spread on a glass plate, the growth takes place in a horizontal plane. In a deep vessel, on the other hand, the plant form grows simultaneously in a horizontal and a vertical direction, forming stems which on arriving on the upper surface of the liquid, spread out in flat leaves resembling those of a water plant.

A single artificial seed one millimeter in diameter can thus produce 15 to 20 vertical stems which sometimes reach a height of 25 to 30 centimeters, being either simple or branched, frequently carrying lateral leaves or twigs and terminals shaped like spheres, mushrooms, ears, spires, etc., according to the composition of the culture liquid.

These experiments thus prove that the functions formerly considered as being characteristic of the process of life are due to and controlled by purely physical forces. In fact, the forms in question obviously receive their food by intussusception or internal absorption like living beings, whereas crystals, as is well known, increase by external accretion. Furthermore, the plant forms are really organized, possessing all those organs (stems, leaves, and terminal parts) which are characteristic of plants. As finally the substance used in building up these artificial plants, viz., copper sulphate, rises in stems up to 30 centimeters in height (with a diameter of one millimeter) they are necessarily provided with an apparatus of circulation. Their growth is thus no doubt a real one like that of a plant, a small (artificial) seed developing into a complex form several hundred times larger than itself.

It is further interesting that the products of growth arising from the artificial seed are, like real plants, susceptible to numerous chemical and physical reactions. In fact, their development is arrested by many poisons, while their direction and growth are determined by differences in the internal diffusion pressure and in temperature. However, there are still further analogies between these artificial organisms and real ones. The former, like the latter, are endowed with the power of healing any injury, as whenever a stem is broken before the completion of its growth, the fragments cling to and combine with one another, after which the process of growth again commences. There is only a single function of living plants which has not so far been artificially reproduced, viz., propagation in successive generations. Except for this defect the whole of the vital process of vegetable organism would have been imitated artificially, at least in its outward appearance; this problem, however, seems to be susceptible of realization like those already solved.

. . . .

From Lehmann's researches on apparently living crystals it is inferred that certain crystallized structures show a behavior quite analogous to inferior organisms, moving, growing, feeding, and propagating themselves like the latter. The investigations by Prof. Leduc which have been described above, on the other hand, prove that the fundamental element of animal and vegetable organisms, viz., the cell, is exclusively controlled in its vital func-

tions by the same physical laws that govern the forms of the mineral kingdom. From both sides there is thus being constructed a bridge between the province of inert matter and that of living matter, and in the place of the strict barriers previously supposed, we are warranted in presuming the existence of a multitude of gradual transitions and intermediary stages.

It should be observed that the Leduc phenomena were first observed by Traube in 1867 (Archiv. f. Anatomi u. wissenschaftliche Medizin, 1867, p.67), who produced them. Such artificial cells have long been known as Traube cells. Traube also produced them by means of tannin and lead acetate, water glass and lead acetate, gelatine and tannin, and the like. In repeating Leduc's experiments Prof. Hans Holisch found that the acetate and the chloride of copper produce better results than the sulphate. The sugar, salt, and gelatine serve to increase the growth and ramification, but it should be pointed out that Reinke described branched and tree-like artificial growths more than twenty years ago. If crystals of copper sulphate are thrown into a solution of water glass they become enveloped in light blue pellicles of copper silicate and these silicate cells develop into tree-like forms if sufficient water glass is present.

Even Leduc's discovery that artificial cells, like natural cells, are affected by various influences was anticipated by Traube, who described the effects of light and gravitation and the variations in form and rapidity of growth produced by adding grape sugar, salt, etc. In Molisch's opinion Leduc's experiments mark no advance beyond the results obtained by Traube in 1867. His artificial cells teach nothing new and they are no more like living organisms than a paper flower is like a real flower or a wax doll is like a living child.

. . . .

DR. CRILE'S "AUTOSYNTHETIC CELLS"

MacDougal, D. T.; *Science,* 73:sup x, January 16, 1931.

To the biologist concerned with the form and architecture of the living cell, the announcement of the results of Dr. Crile's researches on masses of stuff which show some of the properties of living matter will come as something of a shock.

The physiologists, however, especially the group who are engaged in studying the properties and the ultimate arrangement of particles in protoplasm, find in Dr. Crile's results many things of absorbing interest. Furthermore, there is a growing belief among workers that we may within the near future be able to set up small masses of material in the condition of a jelly in which many of the activities characteristic of living matter may take place.

Thus, for example, I have definite recollection that Jacques Loeb, whose researches are well known to all biologists, expressed

high hopes that something like living matter would be compounded within the laboratory within a very few years.

Many of the experimental attempts in this direction have gone no further than the making of minute blobs of colloids which on the glass slide and under the microscope gave resemblances to the indefinite and constantly changing forms of the amoeba. The physiologist is primarily concerned with the energetics, performances or processes which go on unceasingly in living matter. In my own experiments in this direction, begun in 1922 at the Desert Laboratory of the Carnegie Institution, in Arizona, I went no further than making capsules of cellulose, lining them with mixtures of jellies made up of the materials which enter into the composition of the plant cell.

Although the intimate arrangement of these materials could not be said to have been identical with that in living material, except in a general way, yet these experimental devices displayed two forms of activity quite similar to that of the abosrbing hairs of roots. In any often-repeated series of experiments the permeability of these jelly layers was found to be similar to that of the tissues of living plants. The common mineral nutrient elements sodium, potassium, magnesium and calcium entered these "artificial cells" at the same relative rates as in a piece of living tissue.

The second performance in which the activity of living stuff was imitated was one in which these "artificial cells" maintained their acidity for days at a time when immersed in an alkaline solution, after the manner of the protoplasm of a plant growing in an alkaline soil.

Some of these experiments were shown to Dr. Crile at the Desert Laboratory. I have therefore viewed these exhibits of Dr. Crile's results at the Cleveland meeting of the American Association for the Advancement of Science with considerable interest.

Dr. Crile brings together proteins, lipoidal brain extracts and mineral salts in small cavities on glass slides. Masses of material resembling unicellular organisms of various types appear in a few seconds.

The chief interest, however, lies in the fact that when quantities of this material sufficient for chemical and physical tests are accumulated, with characteristic electric potentials, stainability and other physical properties are readily measurable, and respiration data similar to those of masses of living tissue are secured. The transformation of energy indicated is at a rate which changes and runs through a cycle after a manner shown only by living organisms.

Only the worker who has engaged in experiments of this kind is in position to appreciate the enormous amount of wearisome labor necessary to secure the most meager results. It may be regretfully said that the difficulties attending a repetition of Dr. Crile's experiments will delay a checking-up of his results by other workers, which is so highly desirable in all scientific research.

Neither Dr. Crile nor any one else makes the claim that he has

actually "created life" in the laboratory. But the way is indicated along which we must travel in the endeavor to gain a fuller understanding of the nature of living matter.

A NEW THEORY ON THE ORIGIN AND NATURE OF LIFE

Herrera, A. L.; *Science,* 96:14, 1942.

Of the numerous theories which have been proposed to account for the origin of life nearly all have been hypothetical reconstructions based on biochemical considerations, but unsupported by laboratory demonstrations linking the implied syntheses with the genesis of specific structural entities which simulate, both in appearance and in behavior, forms already known to the biologist. After devoting forty-three years to the experimental investigation of this problem, I have been able to produce two principal groups of phenomena which appear to me to be significant:

1. Colpoids. Mix thoroughly 200 cc of very fresh, pure olive oil with 800 cc of clear, pure gasoline. Place the mixture in a flat-bottomed container (a tray of the sort used to develop photographic negatives is advantageous) and add, drop by drop, a solution of pure sodium hydroxide (12 g to 100 cc) to which has been added a gram of hematoxiline to serve as a stain. The resulting macroscopic cells exhibit lively ameboid movement, intracellular streaming and fission. Their activity is inhibited by chloroform. We are dealing here with saponification activated by osmotic currents and electric disturbances set up by the chemical reaction, but the parallel with overt phenomena of life on the protozoan level is so striking that one is compelled to seek identical explanations. This experiment was presented a number of years ago at a session of the New York Microscopical Society by Dr. C. W. Weiant and more recently was exhibited in motion pictures before the Congress of Biophysics in New York.

2. Sulphobes. Taking it for granted that formaldehyde is an essential stage in the synthetic activity of green plants, I made a methodical study of the action of reagents on formaline. The fumes of ammonium sulfide acting upon thin layers of formaline produce many of the structural aspects of protoplasm. Since, according to Pflugger, life is due to cyanogen and its derivatives on up to the proteins, I decided to dissolve ammonium thiocyanate in formaline, spread the material in very thin layers, and then waited several hours before making a microscopic examination. I have repeated this experiment, under varying conditions, for a period of ten years, thereby obtaining thousands upon thousands of microscopic structures with activities analogous to those of living organisms. Chemical products include vestiges of starch, at least two amino-acids, a condensation product of protein character and globules of green, yellow and red pigments. The latter substances I am now investigating. They do not manifest

absorption rays in the spectrum, perhaps because present in such minute quantities. Structures noted comprise cell, ameba and tissue forms of infinite variety, imitating virtually the whole microscopic world. More than 6,000 varieties, among them the counterparts of diatoms, spermatogonia, spores, chromosomes and astrospheres, direct and mitotic divisions, plasmodia, etc., have been recorded and published during the past ten years. I have sent to foreign scientists and scientific institutions more than 900 specimens preserved in Canada balsam and will gladly send additional samples without cost to any interested inquirer.

Now let it be remembered that it is possible to synthesize the thiocyanate used in these experiments by subliming sulfur in a matrass with ammonium nitrate and carbon. Sulfur alone sublimed on cold glass yields no end of cellular patterns, by virtue of its molecular polymorphism and resulting allotropic states. In view of these facts, may it not be that the emanations from volcanoes--sulfurous, cyanic and ammoniacal--have produced and continue to produce microorganisms by chemical synthesis? I intend to study the sublimates of the solfataras of Popocatepetl from this standpoint. Sulfur is present in nearly all proteins and in all living organisms and thus merits special attention in any theory of the origin of life. The particular theory offered here of course lacks confirmation. Much further research is required for that, but it is a theory which, up to a certain point, finds laboratory corroboration. It is itself the outgrowth of experimental method.

CARBONACEOUS "SNOWFLAKES" AND THE ORIGIN OF LIFE

Morrison, Philip; *Science,* 135:663, 1952.

Abstract. The possibility that the intricate "organized elements" found in some carbonaceous chondrites are to be interpreted, not as microfossils of once-living cells, but as organic-chemical analogues of similarly intricate snow crystals is raised; tests and implications are discussed.

IN THE BEGINNING . . . LIFE ASSEMBLED ITSELF

Fox, Sidney; *New Scientist,* 41:450–452, 1969.

In recent years, workers in our group have developed a dynamic laboratory model of evolvable primordial life based on protein-like polymers which I have termed proteinoids. These molecules, which possess almost all of the properties of contemporary proteins, are formed by the condensation of dry amino acids at

temperatures above the boiling point of water. Their most important property, however, is that they are internally ordered; the sequences of amino acids of which they are constructed are not random, and the proteinoids can be separated into a handful of relatively uniform and closely similar fractions. Results of this kind provide us with new data and perspectives on the "order-disorder" question. Moreover, a number of laboratories have demonstrated that proteinoids have primitive kinds of enzymic activity. These arrays of catalytic activity for the conversion of a variety of molecules indicate how elaborate metabolic pathways could have started. The reproducibility of the enzyme-like activities are more easily understood in association with ordered molecular structures.

In the presence of water or solutions of salt, polymers of the proteinoid type immediately assemble themselves into microspherical units, typically 0.5 to 3 micrometres in diameter. In common with contemporary cells, these have uniform size, relative stability of total structure, osmotic properties, catalytic properties, and the ability to allow through their boundaries small molecules, while retaining large molecules. The ultrastructure of these units, as revealed in the electron microscope, is virtually indistinguishable from that of a simple bacterium such as Bacillus cereus or PPLO. When polymer from the interior diffuses outwardly the boundary is seen to be a double layer. Through their propensity to form buds resembling those observed in yeast and bacteria, the proteinoid microspheres participate in the propagation of their own likeness; they can "reproduce".

These units thus possess an array of structures and catalytic and other activities such as would be necessary for evolution. The conditions of formation of the polymers and of the organized units are even now so widespread geologically as to strengthen the plausibility of a geological origin of life in earlier times, and to raise the question of whether life could be arising now. The entire sequence of processes is fast, rugged, and operationally simple.

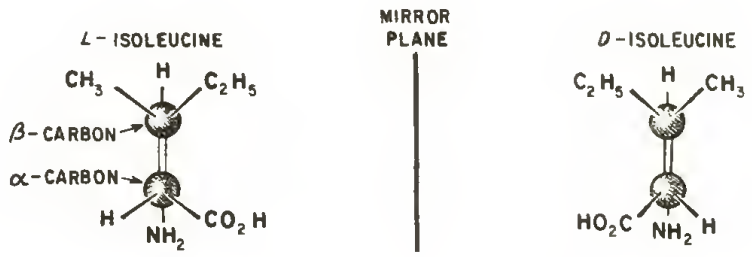
• The Enigma of Biological Optical Activity

WHERE DO WRONG-HANDED AMINO ACIDS COME FROM?

Anonymous; *New Scientist,* 44:448, 1969.

Nature almost always uses one hand, with only an occasional show of ambidexterity. Thus, all proteins are composed of one

particular configurational form of amino acid, denoted L. The basic reason for this asymmetry is that molecules have three-dimensional structures. Some of them are made up in such a way that it is possible to construct a different molecule from exactly the same component atoms which is the mirror image of the original molecule. Just as the reflection of a left hand is a right hand, so a "left-handed" molecule has a right-handed" mirror image. And, just as a left hand cannot be placed face down onto a right hand and cover it completely, so a left-handed molecule cannot be superimposed on a right-handed one.



Mirror-image molecules

For some unknown reason, one particular handedness predominated during the evolution of life and, since many living processes depend upon the accurate fitting together of molecules like keys into locks, a wrong-handed molecule will not work. So, when wrong-handed or D-amino acids occur in nature, as they do quite extensively in a class of compounds called peptide antibiotics, it is obviously of interest to find out how they got there.

Earlier this year, M. Bodanszky of Case Western Reserve University and D. Perlman of the University of Wisconsin (Science, vol. 163, p.352) pointed out that D-amino acids are not incorporated ready-made into peptide antibiotics during biosynthesis. They proposed that L-amino acids epimerize at the alpha-carbon, that is, convert to the D form, at some stage in the building of the peptide molecule.

. . . .

CHIRALITY AND THE ORIGIN OF LIFE

Ulbricht, Tilo L. V.; *Nature,* 258:383-384, 1975.

One of the unsolved problems (there are many) concerning the

origin of life is the choice by living organisms of one set of optical isomers: as is well known, only L-amino acids are found in proteins, whereas laboratory syntheses from optically inactive starting materials yield 1/1 (racemic) mixtures of L- and D-isomers. One can understand how the optical purity of living organisms is maintained, once it exists, because enzymes, composed exclusively of L-amino acids, catalyse the formation of only the desired isomer. Moreover, many organisms even have a special enzyme, D-amino acid oxidase, to remove the unwanted isomer. But how did it all begin?

Could there have been some asymmetric factor in the environment which influenced the choice? It has been suggested that life began in the vicinity of optically active quartz crystals, but then the choice of L- or D- was merely a matter of chance. Another suggestion is that circularly polarised light might have been the agent. Many years ago, Kuhn showed that circularly polarised light could induce optical activity by asymmetric decomposition, and much more recently true asymmetric synthesis has been accomplished using circularly polarised light. But consideration of the interaction of the Sun's radiation with its own or with the Earth's magnetic field, and the fact of the frequent reversals of the Earth's magnetic field, indicates that there was no predominant handedness in the circular polarisation of incident sunlight over any significant time spans.

A further possibility is an asymmetry in the environment from natural radioactivity. There must have been quite a few scientists who were struck by the analogy between the asymmetry at the level of elementary particles (left-handed electrons and right-handed positrons) and asymmetry at the molecular level, when the discovery was announced in January 1957 that parity was not conserved in β decay. But the existence of the analogy does not mean that any causal relationship exists. Vester and I examined this possibility, suggested some mechanisms and early in 1957 carried out experiments to try and induce optical activity in chemical systems under the influence of polarised (left-handed) β rays. (For a review of the earlier literature, see Ulbricht, Origins of Life, 6, 303; 1975.) One of the mechanisms proposed was the following: longitudinally polarised β rays→ circularly polarised bremsstrahlung → optically active molecules.

. . . . .

• Dynamic Molecules

NMR AND X-RAY STUDIES SHOW LARGE MOVEMENTS WITHIN PROTEINS

L., G. B.; *Physics Today,* 32:17–19, November 1979.

X-ray crystallography provides information on the average structure of protein molecules. The images of atoms have always been broadened, but until the last few years this broadening was generally attributed to inaccuracies in the structure determination, lattice disorder and purely thermal vibration. Most crystallographers were skeptical about interpreting the temperature factors as structural motion. A typical small globular protein has a molecular weight of 20 000 a.m.u. and a diameter of 30-40 Å.

Recent theoretical and experimental studies of muscle protein myoglobin suggest that portions of the molecule can experience large mean square displacements. A similar effect has been found in lysozyme. Extremely precise nmr studies of a small protein--bovine pancreatic trypsin inhibitor--have explored the dynamics on a nanosecond time scale. Measurements done at Stanford University on fluorescent depolarization in bovine serum albumen and azurin indicate significant motion on a subnanosecond time scale.

Intramolecular motions have also been suggested by the trapping of a solvent (such as water) inside a molecule, earlier nmr studies that show portions of the molecule rotating or vibrating with respect to each other, fluorescence quenching and relaxation, and isotope exchange.

One example of the biological importance of such motions is the binding of oxygen to the muscle protein, myoglobin, which stores oxygen until it is needed for oxidation. The binding site is buried inside the molecule and is only accessible to oxygen if the protein is flexible.

The motions cover an enormous time range--from seconds to nanoseconds--depending on the process.

. . . .

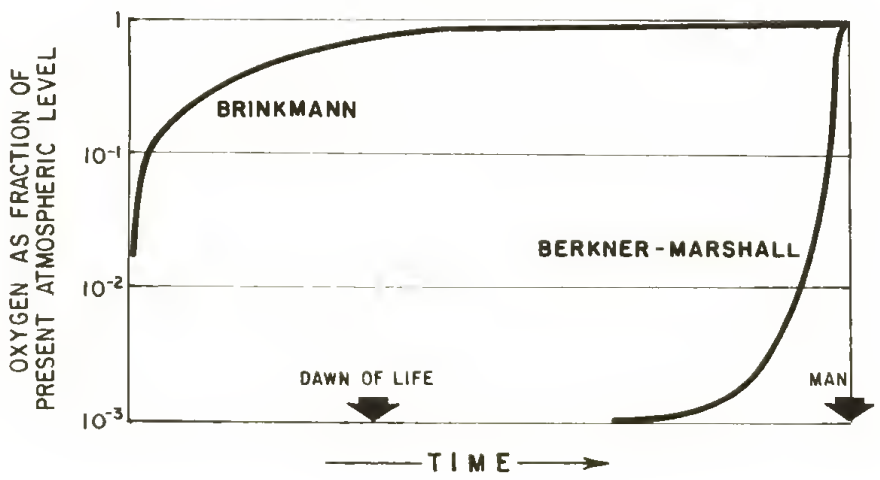
• Life's Origin and Atmospheric Oxygen

THEORETICAL BLOW TO THE ORIGIN OF LIFE

Anonymous; *New Scientist*, 45:344, 1970.

One of the basic tenets--perhaps the basic tenet--of theories concerning the origin of life, is that the first large organic molecules from which living things eventually evolved were formed in an atmosphere almost devoid of oxygen. A reducing atmosphere is deemed essential for the formation of these molecules--as it is, indeed for the survival of the first postulated life forms. Five years ago, L. V. Berkner and L. C. Marshall developed a much-admired theory for the evolution of the Earth's atmosphere, which accommodated these requirements, oxygen not appearing on the scene in any quantity until the first photosynthetic organisms evolved (see "Oxygen and evolution" by L. V. Berkner and L. C. Marshall, New Scientist, vol. 28, p.415). R.T. Brinkmann of the California Institute of Technology has now re-examined Berkner's and Marshall's calculations, claiming that they had made several mistakes. He found that our atmosphere could have been highly oxidizing over a large fraction of geologic time (Journal of Geophysical Research, vol. 74, p.5355).

Within our solar system the Earth's atmosphere is unique in having oxygen as a major constituent. A good deal of evidence



Atmospheric oxygen in the early history of the earth may upset the current theory of life's origin.

accumulated by Earth scientists has indicated that our atmosphere was formed from volcanic gases which include water vapour, but no oxygen. Oxygen therefore was generated through secondary processes. The major sources of atmospheric oxygen are photodissociation of water vapour by ultraviolet light from the Sun, and photosynthesis in plant life. Berkner and Marshall claimed that photosynthesis is primarily responsible for our present oxygen content since the "Urey effect" limits the amount of oxygen produced by water dissociation. According to the Urey effect, oxygen absorbs the ultraviolet light necessary for its own production, providing a form of self-regulation.

According to Brinkmann, this self-regulating mechanism just does not work. His arguments are rather specialized, the main objection to the Berkner-Marshall calculation being that they incorrectly interpreted laboratory data on ultraviolet absorption by oxygen. Using his interpretation of the data, Brinkmann found that the oxygen level could have reached an appreciable fraction of its present amount in the absence of biological activity. The figure shows the disparity of the two calculations.

Brinkmann's result precludes biological evolution as presently understood. The existence of a local reducing atmosphere in some parts of the primaeval Earth provides one way out of this quagmire. But is clear that primitive life theories need the support of some very sophisticated calculations to overcome oxygen fixation.

CURIOUS BIOCHEMICAL REACTIONS

• Biochemical Clocks

BEHAVIOR AND METABOLISM

Anonymous; *Nature,* 222:519, 1969.

Plants and animals seem to be able to estimate time with uncanny accuracy, which is demonstrated by daily metabolic rhythms, and the migration of birds and the flowering of plants at specific times of the year. Living things must have a reference for such accurate timing. The biological clock is thought

to be a form of oscillator, perhaps analogous to a pendulum. In constant conditions, the oscillator will have a period characteristic only of itself, but in nature, light or some such stimulus would force the oscillator to have a period of about twenty-four hours. Most biological clockwatchers reason that living organisms have their own internal time standard. Professor F.A. Brown of Northwestern University, Illinois, is almost alone in believing that the living organism can sense subtle stimuli from the environment, which act as pacemakers for the internal clock. In a recent article (Canad. J. Botany, 47, 287; 1969) he has marshalled the evidence from some twenty years of research into a most plausible argument.

. . . .

If biological rhythms are controlled by an internal timer, it would be reasonable to suppose that this might be an oscillating biochemical reaction. Such a reaction has been sought for some time, and now E. Kendall Pye at the University of Pennsylvania has provided what seems to be the first evidence that prolonged oscillations in a biochemical reaction can exist in vivo and in vitro (Canad. J. Botany, 47, 271; 1969). In some anaerobic conditions, glycolysis in yeast cells pulses in a rhythmic manner, which can be observed as a rise and fall in the concentration of reduced diphosphophyridine nucleotide. The mechanism which Pye has experimentally elucidated for this reaction fits Higgins's general theory of oscillating reactions. This requires that the substrate be fed into the system at a steady rate, and that the product of one of the reactions in the sequence activates the enzyme which catalyses that reaction. The product of this reaction must then be removed by a further enzyme step. Pye has found that glucose is supplied to the glycolytic system at a steady rate. As is well known, it is phosphorylated enzymatically to glucose-6-phosphate, and then converted to its isomer fructose-6-phosphate (F6P). Phosphofructokinase (PFK) catalyses the phosphorylation of F6P to fructose-1-6-diphosphate (FDP) with the concomitant production of ADP. If this reaction is initially slower than the rate of supply of glucose, F6P and ADP will gradually increase. Now Pye can demonstrate that ADP is the activator for PFK, so the rate of phosphorylation will increase until it is proceeding faster than the supply of glucose. The concentration of ADP will decrease and with it the rate of phosphorylation. The cycle recommences when the rate of phosphorylation has fallen below the rate of supply of glucose. Overall control of the rhythm is probably maintained by changes in ADP concentration affecting the reaction catalysed by 3-phosphoglycerate kinase later in the glycolysis sequence.

Pye makes it quite clear that no known rhythm in yeast can be correlated with this metabolic oscillation, but the system does possess many characteristics which make it suitable as a model for the type of timing reaction thought to underlie biological rhythms.

• Biochemistry and Astronomy

BLOOD FLOCULATION INDEX AND EXTRATERRESTRIAL INFLUENCES

Gauguelin, Michel; *The Cosmic Clocks,* Henry Regnery Company, Chicago, 1967, pp. 154-157.

For twenty years the Japanese biologist continued collecting observations, establishing the existence of strange ties between the floculation of blood serum and several cosmic events. His experiments show, in fact, that the changes in the serum occur in particular when a group of sunspots passes across the central meridian of the sun, that is, when the sun directs a concentrated beam of waves and particles toward the earth.

Takata also noted an interesting effect of the sun that had previously gone unnoticed: the floculation index, very low during the end of the night, showed a sudden rise of the coming of day. The amazing fact is that the increase of the curve begins a few minutes before sunrise, as if the blood somehow "foresees" the appearance of the sun. Our preceding chapter may have accustomed us to such incredible "foresight" on the part of living things, but Takata was at the time unaware of the various experimental results that would have helped to explain his finding. To assure himself that the effects he observed were due to solar radiation, he decided to see what happened when an experiment was performed above the protective atmospheric screen that isolates us from much of the sun's activity. In an airplane he flew to an altitude of above thirty thousand feet with a volunteer whose blood was taken every fifteen minutes to check the effect of varying altitude. As predicted, the floculation index rose spectacularly as the plane rose and the atmospheric shield became thinner, confirming the role of solar radiation in this matter.

Then the Japanese biologist asked himself another question: Wouldn't the moon eliminate the effect during eclipses, by placing itself between the sun and the earth? Three times, in 1941, 1943, and 1948, Takata was able to bring subjects and equipment to areas in Japan where there was a total eclipse---and each time he verified his hypothesis. As the moon began to cover the face of the sun the floculation index started to decline, reaching its lowest point when the eclipse was complete. The solar radiation that accounts for the Takata effect is apparently deflected by the moon; yet neither houses nor thick concrete battlements had succeeded in doing so before. The only previous experiment in which the Takata effect had not been observed took place in a mine shaft at Mieken, six hundred feet underground.

A tremendously strong solar radiation is involved here, one that is almost impossible to stop. This immediately reminds us of

Brown's observation that spatial influences penetrate the most carefully sheltered experimental conditions. Some elements of the human body, sheltered as they are within the blood vessels, are still exposed to the vagaries of that great cosmic clock, the sun. It was Takata himself who formulated the concise definition: "Man is a living sun-dial." (pp. 154-157)

• Biochemical Evolution

RNA EVOLVES BY ITSELF IN A TEST TUBE

Anonymous; *New Scientist,* 44:542, 1969.

Though it is nearly a century since Darwin wrote his treatise On the Origin of Species, there are still a few weak points in the theory of evolution. Often evolution seems to have made huge jumps, leaving no traces of any intervening steps and no hint that anything but the complete system could ever have functioned at all. Some animal behaviour patterns fall into this class and so does the system for storing and using genetic information in nucleic acids. Thanks to a paper in the Proceedings of the National Academy of Sciences (vol. 63, p.805) this latter problem no longer looks so daunting.

Reuben Levisohn and S. Spiegelman have shown that nucleic acids could have been subject to natural selection even in the absence of living cells and the full enzymic machinery that supports present-day DNA and RNA replication. In their experiments at the University of Illinois they took the RNA from bacteriophage QB and allowed it to reproduce in a test tube with the aid of the virus's RNA replicase. By a series of transfers from tube to tube, under conditions where one of the four necessary nucleotides was in short supply, they selected for a distinct new mutant that reproduced faster than the wild type. A similar result occurred in the presence of an inhibitor.

The new RNAs appeared no different from the parent strain in length or base composition, so any changes must have been quite subtle. Nevertheless it was quite undeniable that evolution had occurred in the test tube upon a single molecule that did nothing except reproduce itself. The significance of this is that the same process could have gone on millions of years ago with crude non-enzymic catalysts and simple nucleic acids. So that when the first cell came along there would have been quite complicated self-reproducing systems already in existence to be incorporated as the genetic machinery.

Chapter 12
GENERAL LIFE PROCESSES

Chapter Contents

GENERAL CHARACTERISTICS OF LIFE

• The Variety and Omnipresence of Life

BIOLOGICAL PECULIARITIES OF OCEANIC ISLANDS

Gulick, Addison; *Quarterly Review of Biology*, 7:405-427, 1932.

One of the most challenging of all questions in animal and plant distribution is the problem of the peopling of remote oceanic islands by appropriate forms of terrestrial life; and the peculiarities which we find in such island populations probably furnish valuable clues toward the elucidation of more than one of the laws of organic evolution.

How is it possible at all for creatures that would die almost at the touch of sea water to precede man by a million years on islands standing solitary in mid-ocean? Are their remote homes really the left-over fragments of ancient inter-continental land bridges, or are these creatures prima facie evidence that their ancestors possessed an almost inconceivable capacity for passing uninjured over vast stretches of open ocean? The extremes of hypothesis that have been proposed in response to this dilemma show us how difficult it has been to find a reasonably credible solution.

At the present time scientific writers are fully agreed only as to a very limited number of unmistakable oceanic islands--- islands, that is, whose terrestrial fauna acknowledgedly cannot have arrived by the land-bridge method. Unfortunately, the majority of these localities are but very imperfectly studied, because of their inaccessibility, and several of them suffered heavily at the hands of human inhabitants before scientific collectors could arrive.

The Flora and Fauna of Easter Island. Aside from mere sand-spits and bird rocks, the most solitary of all high islands is undoubtedly Rapanui, or Easter Island, the easternmost outrider of Polynesian colonization. This lonely triangle of volcanic cones and dead craters, with some 45 square miles of land surface, is the wave-worn remnant of an island that could once have claimed about twice that area. It looms above the horizon at a distance of 2,280 miles west of the South American coast, and 1,590 miles east of the tip of the Paumotu Archipelago, where begins the mid-Pacific zone of multitudinous islets known as Polynesia. The only near neighbor of Rapanui on this vast expanse is a trivial bird

rock, 250 miles to the east. Other neighbors, more distant, are Ducie, an uninhabited atoll lying 1,000 miles westward, Pitcairn 1,300 miles, more or less, in the same direction, and the South American islands of Juan Fernandez and Mas-afuera, which are respectively 390 and 450 miles from the Chilean coast.

Being inhabited for many centuries by a Polynesian tribe, the native flora and fauna of Rapanui have doubtless suffered both losses and additions through artefact. Domestic fowl quite certainly, and rats most probably, came to it as fellow passengers with the Polynesians in their ocean craft, furnishing its only known examples of land birds and quadruped mammals. Any other land birds, if they ever existed, have been so long extinct that the natives have no memory of them. Thus we may perhaps have here one of the ultra-rare cases of a high island that has never been colonized by any passerine birds.

The pre-European flora was judged by Knoche to consist of 55 species, representing 50 genera. Skottsberg reviews the list more critically and is only inclined to credit 30 of the flowering plants, representing 26 genera and 15 families as genuine ancient inhabitants, and 6 species from 6 genera as Polynesian introductions. To these may be added 12 species of ferns, two of them endemic and ten more widespread, which are not supposed to have been brought by man. He marks down all other vascular plants thus far reported from Rapanui as late introductions.

None of the genera are peculiar, and no two endemic species are of the same genus. In short the flora is, as Baur would express it, emphatically "disharmonic," meaning by this that its species lack the local coherence of inter-relationship, and that the assortment of forms fails to constitute a normally balanced and diversified total flora. The affinities are largely with the Polynesian region, although three of the pre-human species are counted as American. Only two ferns, three grasses and one tree are credited as endemic species, all the others being known from other parts of the world. As compared to other islands the plants introduced by Polynesian navigators are unusually few, although the legends of the natives claim that the entire flora of Rapanui was brought here by their heroic ancestor.

The trade winds and ocean currents, as we now observe them, come to Rapanui consistently from the east, but the carrying power of hurricanes in this mid-Pacific zone is believed to be predominantly from the west. Hence the impossibility of drifting mangrove seeds reaching here from western Micronesia, where they are native, and the apparent failure of cocoanut palms to arrive here before their planting by man. Yet wind-borne seeds, quite incapable of traveling 2,000 miles upon a trade wind must have been brought repeatedly by storm winds from Polynesia, as shown by the general affinities of the flora.

The supposedly native insects are limited to a weevil, apparently endemic, and two aquatic Neuroptera, one of them a South American form, and the other not reported outside of this island. Unfortunately the insects of other parts of the Pacific are not yet sufficiently known to make it certain that either of the two seemingly endemic species are genuinely peculiar to Rapanui.

Among land snails the outstanding occurrence is an endemic species and possibly genus known as Pacificella variabilis, measuring when adult about 3.8 mm. long by 1.5 mm. in diameter. It is related to the Tornatellinidae of Polynesia and Juan Fernandez. One other species, Melampus paschus, is included as an endemic form, belonging to a Micronesian and Indonesian genus that is very tolerant of exposure to salt water. The list of land snails is completed by two commercially widespread slugs and an Indonesian species of Melampus, which also falls under suspicion as a possible introduction.

Aside from the usual pests that come on ships, the remaining items in the land fauna are two Polynesian lizards, a South American earthworm, one or two butterflies, a Polynesian cricket, a scanty list of beetles (more especially an earwig and a water beetle), and certain well-known "tramp" species of ants, spiders, myriapods, etc. It is doubtful whether any of these were here before man, but impossible to prove that some among them may not have been.

The significant points in these not very complete data are:---

(1) The flora is emphatically "disharmonic," revealing that life here is definitely insular, and not ascribable to landbridges.

(2) The fauna is not only "disharmonic" but excessively meager and scrappy, and does not show a very high degree of species differentiation. In only one instance is there a suspicion that differentiation has reached a generic level, and for that genus only one species has been reported. As this means a relatively young fauna, and the geology favors a fair antiquity for the island, there is more or less reason to suspect that the ancestors of most of the pre-human species now found there arrived since the date of some cataclysm, such as a devastating volcanic eruption.

(3) Despite its extreme remoteness, this island was successfully colonized by a number of plants and a few animal forms, more especially one or two snails and apparently a scattering of insects. Thus it is evident that some forms of life can find their way across vast stretches of ocean without human intervention. (pp. 405-407)

Insular Evolution. Far distant islands serve as an especial test for the efficiency of the evolutionary processes in creating new adaptations. They are seeded with plants, and then supplied with a few insects, birds, and bats, and a few wind-waifs and driftwood waifs of small, hardy creatures. Their shores are littered with sea animals of all discriptions, various of which might theoretically crawl out and take possession of a terrestrial realm. There is thus presented a great opportunity for some radically new evolutionary departure. Do we find any? Only in a most limited measure. Really radical new departures seem to require a longer time allowance than these geologically not very permanent islands can furnish. A rail may forget its wings and become definitely terrestrial; a micro-snail may blossom out and appear like a big helix; a bird may take up new food habits and undergo great alterations of bill character; but nowhere has any sea creature snatched the opportunity to evolve into a new land animal. Even marine turtles, which visit the beaches to lay their eggs, have

not reverted to life on land. But the older the isolated region, the more nearly it escapes from this criticism. On Hawaii, where apparently the time factor has been least limited, and where insular life has worked out its own destiny perhaps more completely than in any other location, we find the nearest approach to the creation of genuine novelties,---birds that have a truly original equipment for extracting wood-boring grubs, and brook fishes possessed of sucker-like fins.

The question whether creatures that have been transformed through insular isolation have ever come back as triumphant new biological successes, able to modify the major stream of continental evolution, has not yet found any clear answer. Present day evidence has to be inferential, and the fossil records are rarely, if ever, detailed enough to give evidence on this question. Students of the Galapagos birds judge that the Geospizidae are today a vigorous stock (17), flexible as well as novel in its adaptations, and presumptively as fit as other bird-groups to face competition and climatic changes. From this we might argue that existence on an island, if not too prolonged, need not unfit a stock for the contest for survival in a heavily populated continent.

It is worth noting that both horticulturalists and animal fanciers have been pleased with various of the world's island products, such as the Bermuda fern, St. Helena violet, Canary finch, and the like. Yet, after all, the contribution of domesticated plants and animals made by oceanic islands is relatively slight by comparison with the great yield from continental islands like Japan, Madagascar, or the East Indian groups. Even in the oceanic zone, those native plants that are ecologically predominant or outstanding from the human utilitarian view-point are largely the ones whose powers of dispersal---like the cocoanut---render them least drastically isolated in a remote insular location. Although this picture is too complex to prove the point, it harmonizes with the suspicion that geographic isolation, if it becomes too drastic and too prolonged, will cease to aid the main current of evolutionary progress.

The topic of insular evolution should not be left without a mention of the circumstance that successful breeds of domestic animals have been developed in a large number of instances in such locations as the Isle of Man, Malta, Jersey, Guernsey, and the Friesian islets. The cattle on these North Sea and Channel islands may fairly be claimed as a positive evolutionary advance in adaptation to the human element of the environment.

In the broad, the general picture of evolution that we have observed under rigorous oceanic isolation is filled with an extraordinary wealth of varietal and species characters devoid of serious adaptive significance, together with a fair display of not too radical novelties of adaptation. Indeed, if we can apply what we see here to an appraisal of evolution elsewhere, we must conclude that the developmental scene is filled chiefly with a rich by-play of all manner of non-utilitarian divergences, while only here and there some truly progressive, permanently significant innovation may come into existence. (p. 423-424)

PATTERNS OF FAUNAL EVOLUTION

Anderson, Sydney; *Quarterly Review of Biology*, 49:311-332, 1974.

Summary

1. A major pattern of diversity in the results of biotic evolution, of ecological dynamics in many situations, and of complex non-biological situations of various types is a conspicuous discrepancy in the sizes of groups. The relative numbers of groups (x) containing different numbers of items (y) tend to be related as an inverse power function, $y = c/x^m$, which is also expressible as

$$\log y = \log c - m \cdot \log x,$$

where log c and - m are constants expressing the intercept and slope, respectively. An xy plot of these two values falls in a straight line on a log-log graph, and a cumulative frequency diagram on a log-log graph is a line that bulges upward in the middle.

2. Three principal approaches to the elucidation of this pattern have been mathematical description, models based on deterministic assumptions, and models involving stochastic assumptions. Some models mix assumptions. This is reasonable. However, authors have often been quite unclear as to what they are assuming, and that is unfortunate.

3. Both ecological and evolutionary theorists have exhibited deterministic biases. Sometimes these have been deliberate, at other times not. Sometimes the bias proceeds from a coherent philosphical position, but more often the philosophical premises are unstated and unexamined.

4. Several new sets of taxonomic data are seen to fit the pattern described. The pattern is evident in results of revisions based on the different premises of numerical phenetics and phylogenetic systematics as well as in conventional eclectic classifications.

5. A minimally distorted model of natural diversity and an optimum information retrieval system for the human mind probably are not compatible. This makes taxonomy a frustrating and intellectually stimulating field, because taxonomists would like to produce classifications that are both undistorted and useful.

6. Three simple models tending to equilibria are given. Two of these models are alternative statements of assumptions that yield the broken stick model; the third model yields the relationship xy = c. Monte Carlo models are mentioned, to suggest an approach to the problems of faunal evolution and ecological dynamics that has not been much used.

7. Stochastic models can be employed as null hypotheses. When a significant departure appears, then we can reject the random and look more closely for some other order of things.

8. A major pattern of diversity described here cannot be derived from any reasonable set of deterministic assumptions known to me, nor from premises established by study of lower

levels of ecological and evolutionary organization. The pattern seems easily derivable from stochastic assumptions, although the modeling needs to be developed further than has been done here.

9. Philosophically, this approach does not refute a deterministic position, for an appearance of randomness can result from great ignorance as well as from real disorder. Practically speaking, it means that we are a long way from a "causal" explanation for any specific large-scale example of faunal evolution or of ecological dynamics and that our predictions about these things are likely to be poor in most cases.

10. The theoretical question of causality becomes the practical question--What do you do when you can't be sure of the results? The response to this question should not, in my opinion, be despair. It should be caution, with continuous reassessment, and humility.

• The Discontinuity of Life

CATASTROPHISM VERSUS EVOLUTION

Dobzhansky, Th.; *Science,* 92:356-358, 1940.

The Material Basis of Evolution. By Richard Goldschmidt. 460 pp. New Haven: Yale University Press. 1940. Price $5.00.

This book contains the only basically new theory of organic transformation propounded during the current century. For notwithstanding the colossal literature concerning organic evolution which has accumulated since the publication of Darwin's "Origin of Species," only three main types of theories can be distinguished. One type is based on Lamarck's assumption of direct adaptation by inheritance of results of use or disuse of parts. Other theories are built on Darwin's principle of natural selection of the fittest variants produced by interactions of the organism's inherited structure with the external as well as internal environments. Theories of the third group assume autogenesis, that is, unfolding of the potentialities hidden in the organism, impelled by an urge toward development in a certain direction. According to all these theories organic transformation takes place by evolution, that is, gradually by accretion of more or less small changes. According to Goldschmidt, however, it occurs by cataclysmic upheaval rather than by summation of individually small steps.

Lamarckianism has become obsolete owing to its basic assump-

tion having fallen short of experimental verification. Autogenesis has always been in conflict with the principle of causality in vogue in the materialistically-minded modern science. Darwinism underwent great changes because of the forward strides of genetics, but the unbroken continuity of ideas between the "neo-Darwinism" and Darwin's original theory is evident. The appearance of Goldschmidt's book connotes an at least temporary end of the undivided reign of neo-Darwinian theories. For Goldschmidt not only relegates natural selection to a place of relative unimportance, but in effect rejects evolution beyond the narrow confines in which it has been admitted to exist by Linnaeus and many creationists. His theory belongs to the realm of catastrophism, not to that of evolutionism; his break with the Darwinian tradition is almost complete, and his ideas are related to those of G. St. Hilaire rather than to those of any modern evolutionist. Only deVries has occasionally expressed himself in a language similar to Goldschmidt's, but this similarity proves to be a spurious one. Genetics has been considered the foundation of neo-Darwinism. Throughout his book Goldschmidt stresses the fact that his views are a result of his life-long work in genetics, but it must not be forgotten that he has a splendid command of other biological disciplines as well. Goldschmidt's interpretation of genetic data is different from the rather generally accepted one.

Only the salient points of Goldschmidt's brilliantly developed and masterfully presented theory can be summarized here. Goldschmidt asserts a fundamental dualism of the processes of organic transformation by distinguishing microevolutionary and macroevolutionary changes. "Microevolution by accumulation of micromutations--we may also say neo-Darwinian evolution--is a process which leads to diversification strictly within a species, usually, if not exclusively, for the sake of adaptation of the species to specific conditions within the area which it is able to occupy." This process has nothing to do with the origin of new species: "the subspecies are diversifications within the species, but there is no reason to regard them as incipient species." On the other hand, "the change from species to species is not a change involving more and more atomistic changes, but a complete change of the primary pattern or reaction system into a new one . . . One might call this different type of genetic change a systemic mutation. . ." The effectiveness of systemic mutations is tremendous: "Macroevolution may proceed by large and sudden steps which accomplish at once what small accumulations can not perfect in eons, and this on the specific as well as on any higher level." "Species and the higher categories originate in single macro-evolutionary steps as completely new genetic systems." The origin of mimetic resemblances, of mutual adaptations of flowers and insects, the transformation of Simanthropus into Homo sapiens might have taken place thus suddenly. The bon mot of Schindewolf that "the first bird hatched from a reptilian egg" is quoted approvingly, although it is not clear to the reviewer whether Goldschmidt intends this to be taken literally. The role of natural selection in macroevolution "becomes reduced to the

simple alternative: immediate acceptance or rejection."

Although, in the reviewer's opinion, Goldschmidt's theory is invalid, it must be admitted that Goldschmidt has marshalled an impressive array of evidence in its favor. If races are not incipient species, if species arise by changes akin to explosions, the living world must be divisible into discrete species compartments. The lack of universality of such rigid subdivisions is what has ever since Darwin been one of the main arguments in favor of evolution. The first 183 pages of Goldschmidt's book are devoted to disproving this argument. The alleged transitional cases between races and species are scorchingly criticized and shown, according to the author, to be spurious. He admits that there are transitions between some of the "species" of the taxonomists, but these are not deemed to be real species. Goldschmidt has no crusading zeal to induce taxonomists to abandon their errors, but he insists that to an evolutionist "Where species can be distinguished with certainty . . . they are different and are separated by a gap, if not by an abyss." However, no definition of what a real species should be is given, so that one is left wondering how to detect them in practice.

Pages 184-250 contain the climax of Goldschmidt's theory: the statement of the case for systemic mutations. "A systemic mutation (or a series of such), then, consists of a change of intrachromosomal pattern," and involves "only the arrangement of the serial chemical constituents of the chromosomes into a new, spatially different order. . ." An excellent review of the data showing that species differ in the linear sequence of materials (gene arrangement) in the chromosomes is given, and the conclusion is reached that the source of macroevolutionary changes is herein detected. A thorny question obtrudes, however, in the reviewer's mind. According to Goldschmidt, genes do not exist, and what has been known as gene mutation (the microevolutionary one!) is based on very small pattern changes in the chromosomes. What, then, is the fundamental difference between the ordinary mutations and systemic mutations?

Next (pp. 250-395), follows the chapter on "Evolution and the potentialities of development," in which Goldschmidt seeks to prove that the normal embryogeny contains potentialities which, when evoked by a genetic change, may lead to a complete reorganization of the end product. Here Goldschmidt is really at his best: pertinent facts from experimental embryology, comparative anatomy and physiology, unknown to or ignored by many geneticists, are organized into a logical and convincing system. It is difficult not to agree with Goldschmidt's conclusion that "The physiological balanced system of development is such that in many cases a single upset leads automatically to a whole series of consecutive changes of development. . . If the result is not, as it frequently is, a monstrosity incapable of completing development and surviving, a completely new anatomical construction may emerge in one step from such a change." Whether or not the production of such "hopeful monsters" may reasonably be considered the main channel of organic transformation is, however, an entirely different matter, as Goldschmidt himself admits (p. 251).

It is impossible to attempt here a critique of Goldschmidt's theory, for this would require a book approximately of the same size as his own. Certain lines which such a criticism might take are adumbrated above. In his concluding chapter (pp. 396-399), Goldschmidt indicates that one of the advantages of his views over neo-Darwinism is the simplicity of the former. It is indeed simpler to assume that major groups of organisms arose ready-made by catastrophic systemic mutations than to visualize the complex interplay of the numerous agents which could bring similar results on the neo-Darwinian scheme. But in the reviewer's opinion the simplicity of Goldschmidt's theory is that of a belief in miracles. It must, nevertheless, be recognized that Goldschmidt's keenly critical analysis has emphasized the weaknesses and deficiences of the neo-Darwinian conception of evolution, which are numerous, as even partisans ought to have the courage to admit. It would seem that this fact alone obliges any one interested in the modern evolutionary thought to read Goldschmidt's book.

DOCUMENTATION OF THE ABSENCE OF TRANSITIONAL FORMS

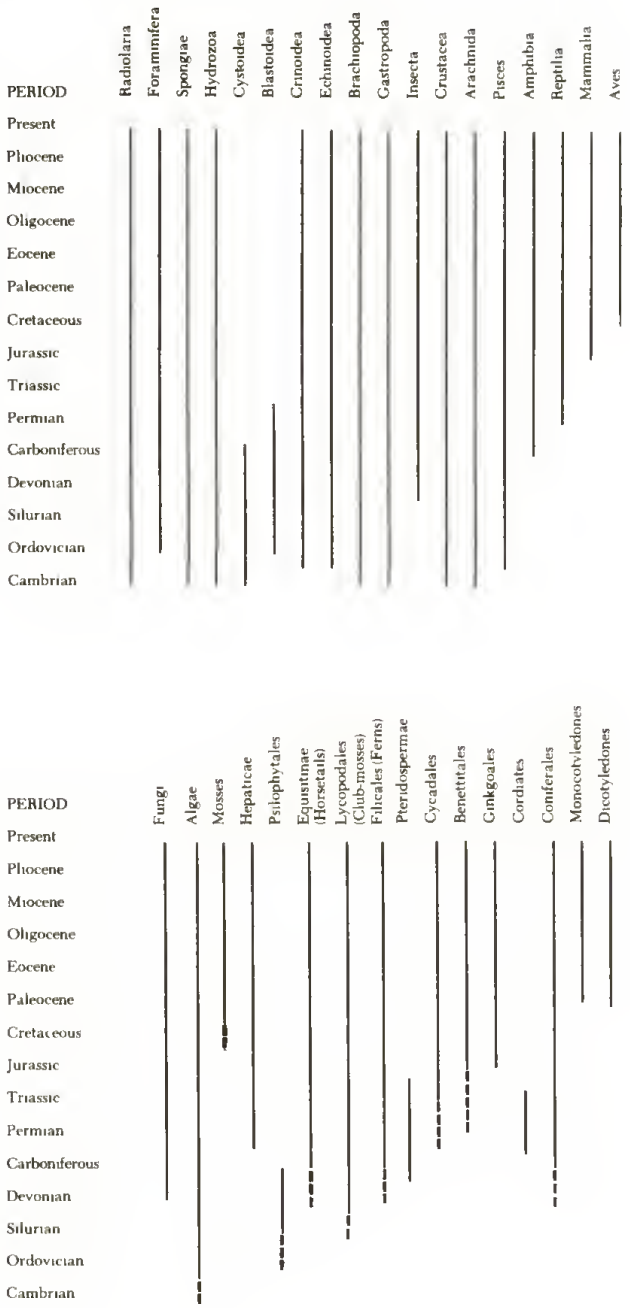
Moore, John N.; *Creation Research Society Quarterly,* 13:110–111, 1976.

Clear documentation of the position that there are no transitional forms, and no diversification and branching from general to special, is available in the 1967 publication, The Fossil Record (A Symposium with Documentation), jointly sponsored by the Geological Society of London and the Palaeontological Association of England. Attention to this thoroughly scientific work resulted from the suggestion of Father Vincent J. O'Brien, former science master at Castlenock College, County Dublin, Ireland, and past Chairman of the Association of Irish Teachers of Science.

In this research volume, some 120 scientists, all specialists, prepared 30 chapters in a monumental work of over 800 pages to present the fossil record for plants and animals divided into about 2,500 groups. Also these specialists prepared 71 highly instructive and authoritative charts that are included throughout the chapters of the book. (See Charts 1 and 2.) Acknowledgement is made in the Introduction of Part II (p. 158) of the fact that some zoological specialists attempted to indicate possible limited "connections", but such tenuous relationships always involved possible "connections" within major divisions of animals, viz. Porifera, Brachiopoda, Mollusca, Agnatha, Amphibia, Aves, Mammalia. No such limited "connections" were recorded by any botanical specialist.

However, a conclusive generalization drawn from these charts is as follows: Each major form or kind of plant and animal is shown to have a separate and distinct history from all the other forms or kinds!!!

Groups of both plants and animals appear suddenly in the fos-



(Top) Chart 1. Generalized geological record of animals. Vertical lines represent durations of each animal group. No common ancestors are known. (Bottom) Chart 2. Generalized geological record of plants. Broken lines indicate doubts as to the earliest appearance of some groups. Again, no common ancestors are known. (Based on: Harland, W. B., et al, eds.; The Fossil Record, *Geological Society, London, 1967)*

sil record. For example, most mammals appear suddenly in the so-called Eocene division, and are as diverse then as researchers find them to be today. Whales, bats, horses, primates, elephants, hares, squirrels, etc., are all as distinct at their first appearance as they are now. There is not a trace of a common ancestor, much less a link with any reptile, the supposed progenitor; and the same is true of the sudden appearance of about 50 families of flowering plants in the so-called Cretaceous division of the accepted geological time scale.

Many summary paragraphs could be included here on the outstanding scientifically documented information about plants and animals in the fossil record. But the important point to make is that knowledge of the content of the above cited book is not recent. Specialists in the proper fields have possessed most of these facts for decades. And proponents of the General Evolution Model, who are familiar with the facts of paleontology, admit existence of gaps between all higher categories. They admit that this is an undeniable fact of the fossil record.

THE RETURN OF HOPEFUL MONSTERS

Gould, Stephen Jay; *Natural History,* 86:22+, June-July 1977.

Geneticists can study the gradual increase of favored genes within populations of fruit flies in laboratory bottles. Naturalists can record the steady replacement of light moths by dark moths as industrial soot blackens the trees of Britain. Orthodox neo-Darwinians extrapolate these even and continuous changes to the most profound structural transitions in the history of life; by a long series of insensibly graded intermediate steps, birds are linked to reptiles, fish with jaws to their jawless ancestors. Macroevolution (major structural transition) is nothing more than microevolution (flies in bottles) extended. If black moths can displace white ones in a century, then reptiles can become birds in a few million years by the smooth and sequential summation of countless changes. Change of gene frequencies in local populations is an adequate model for all evolutionary processes--or so the current orthodoxy states.

. . . . .

Goldschmidt raised no objection to the standard accounts of microevolution; he devoted the first half of his major work, The Material Basis of Evolution (Yale University Press, 1940), to gradual and continuous change within species. He broke sharply with the synthetic theory, however, in arguing that new species arise abruptly by discontinuous variation or macromutation. He admitted that the vast majority of macromutations (for example, two-headed turtles and two-legged sheep) could only be viewed as disastrous--these he called "monsters." But, Goldschmidt went on, every once in a while a discontinuous macromutation

might, by sheer good fortune, adapt an organism to a new mode of life, a "hopeful monster" in his terminology. Macroevolution proceeds by the rare success of these hopeful monsters, not by continuous small changes within populations.

. . . . .

All paleontologists know that the fossil record contains precious little in the way of intermediate forms; transitions between major groups are characteristically abrupt. Gradualists usually extract themselves from this dilemma by invoking the extreme imperfection of the fossil record--if only one step in a thousand survives as a fossil, geology will not record continuous change. Although I reject this argument (for reasons discussed in last month's column), let us grant the traditional escape and ask a different question. Even though we have no direct evidence for smooth transitions, can we invent a reasonable sequence of intermediate forms, that is, viable, functioning organisms, between ancestors and descendants? Of what possible use are the imperfect incipient stages of useful structures? What good is half a jaw or half a wing?

. . . . .

• Which Animals Are Selected?

SURVIVAL OF THE ORDINARY

McAtee, W. L.; *Quarterly Review of Biology,* 12:47-64, 1937.

Introduction. Natural selection theory has been so frequently criticized that it seems well nigh impossible to bring forward an entirely new argument. Perhaps by presenting matters in a fresh light, however, and even by iteration alone, the attention of open minds may be focussed on some of the fatal weaknesses of the theory.

The thesis of the present paper is not original but it was independently reached and has been the subject of reflection for years. I find it fairly well stated in a letter written in 1917 from which the following quotation is taken.

> "I am unable to put much faith in natural selection however, and none in that phase of it expressed by the phrase 'survival of the fittest.' It seems Quixotic to assert, that among the very large numbers of offspring, produced by most animals, only the fittest survive. Chance enters into the

equation so largely that the fittest stands a proportional chance of being the first eliminated. It seems to me that the survivors will almost invariably come from the great median group of ordinary specimens, and not from either the small proportion of subnormal or of supernormal individuals. In other words, natural selection will usually leave typical specimens to reproduce a species, and is a conservative rather than a progressive process."

About the only change I would make in that statement today would be to quote "natural selection" as well as "survival of the fittest," as both of these terms are mere slogans used almost invariably without the slightest analytical perception. A fair interpretation of typical assertions might read: "Mortality occurs; there are survivors; natural selection therefore has been effective, and the survivors are the fittest." As has been pointed out on the various occasions this is quite irrational, and means nothing but that survivors survive.

[The detailed arguments are omitted.]

Conclusion. Among large numbers of organisms, the "fittest" (two out of millions for instance) are an exceedingly small proportion. It is wholly incredible that any process could be devised even by man, much less brought about by the forces of nature, that could insure "survival of the fittest."

It is evident, moreover, from the instances discussed in this paper that elimination, which bears most heavily on the immature stages, tends to be highly indiscriminate. For this reason also it is very probable that, as a rule, not "fittest," but commonplace specimens survive to propagate the race.

Elimination among the immature being practically indiscriminate, the survivors will be a random or ordinary lot. Since a high proportion of the total elimination occurs among the young, there is opportunity for comparitively little "selection" later among the adults, and most of them (necessarily of average make-up) will breed.

Many species have a tendency toward an "all or none" type of survival. It is very strongly developed in those exhibiting age-class phenomena, and is only slightly less evident in cyclic species and others subject to considerable fluctuation in numbers. The mass elimination thses species experience (sometimes total for certain periods or areas) certainly takes the "fittest" along with the rest, while their characteristic mass survival obviously is not limited to the "fittest" (a group which according to selection theories, as we have already noted) is very small. There is no conclusion possible, therefore, but that the continuity of these forms (which include probably a great majority of all existing species) is through ordinary representatives. On all these counts (elaborated in previous pages), therefore, the unavoidable deduction is that average representatives of species, not the "fittest," survive.

What do these considerations mean in relation to the theory

of "natural selection?" The preservation of the commonplace or fit is something very different from the "survival of the fittest." The latter, though incredible, if it existed, could produce evolution; the former, and it is certain, what actually occurs, by itself cannot.

"The survival of the fit," "the elimination of the unfit," favorite succedanea of neo-Darwinians (of themselves) will not do the business, will provide no motor for the evolutionary machine. Even "the better equipped survive, the worse equipped die," idea is scarcely an improvement, because of the reversion that must always occur to account for the observed maintenance of fluctuating variations. From better or "fitter" to fit or ordinary is a gradation so small that it is easily within the scope of the usual range of variation. It is certain that descendants of organisms so characterized would, on account of merely normal regression and variation, be indistinguishable from the average of their kind.

The evidence marshalled in this paper is to the effect that reproduction of species, on the whole, is carried on by ordinary individuals. They come from the great median mass of the population, and maintain the normal range of fluctuating variations. In the absence of other effective forces, propagation in this manner cannot change the character of the race nor produce evolution.

• Symmetry and Asymmetry in Life

SYMMETRY AND ASYMMETRY PROBLEMS IN ANIMALS

Neville, Anthony Charles; in *The Encyclopedia of Ignorance,* Ronald Duncan and Miranda Weston-Smith, eds., Pergamon Press, New York, 1977, pp. 332-337.

What Is Animal Asymmetry? When symmetrical body form first appeared in the evolution of the animal phyla, the early examples were radially symmetrical, like a sea anemone. This was associated with a sessile way of life. It is useful for an animal which sits mostly in one place to be able to sense the approach of danger from all (radial) directions. Then muscle began to be exploited as the main means of mobility, replacing rows of beating cilia, and locomotion consequently became faster. It was then an advantage to become streamlined and so bilateral symmetry of external form became a dominant evolutionary theme. A bilaterally symmetrical animal can be mirror-imaged about its midline. During sub-

sequent evolution, this bilateral symmetry has often been lost, so that what superficially appears to be a bilaterally symmetrical animal is seen on closer inspection to be asymmetrical; such an animal cannot be divided exactly into two halves. Asymmetry may be subtle (e.g. the zig-zag pattern down the back of an adder) or obvious (e.g. the spiral coiling of a snail). The examples may be structural, or behavioural (e.g. earthworms can distinguish right from left in maze experiments). Often asymmetry has evolved as a special functional adaptation (e.g. insect jaws which work like left-handed scissors in some species, right-handed in others).

Biological structures are formed by a hierarchy of assembled sub-units, each of which forms the construction unit for the next size level of organisation. Examples, in ascending order of size and complexity, are small molecules, macromolecules, microfibrils, fibrils, fibres, organelles, cells, tissues, organs, whole organism. This means that symmetry and asymmetry can coexist independently at different levels in the structural hierarchy of an individual animal. Thus all the amino acids from which proteins are made are L-amino acids. My left ear contains proteins with L-amino acids and so does my right ear. Taken at this level of structure, therefore, all animals which appear from the outside to be bilaterally symmetrical, are really asymmetrical. Again, whereas evolutionary pressures led to streamlining of exterior form in the mackerel, its internal viscera are not bilaterally symmetrical and neither are the stripes on its back. In adult beetles, the orientation of the first layer of fibrous chitin laid down after hatching from the pupa follows the bilateral body symmetry. But subsequent layers are laid down asymmetrically. In flatfish, reversal of external asymmetry usually occurs without reversal of internal viscera. It is concluded that separate control mechanisms must exist for the various levels of asymmetry.

Some examples of asymmetrical adaptations in animals will be presented, as a prelude to the discussion of major unsolved problems in developmental biology which they reveal.

Some Telling Examples

Hermit Crabs. Hermit crabs live inside empty shells of molluscs. Primitive ones are bilaterally symmetrical as they live inside homes which are not asymmetrical (e.g. scaphopod shells; pieces of bamboo tube). More advanced forms show striking corkscrew twists, evolved in adaptation to the spiral shells of gastropod molluscs. They twist the same way as a right-handed corkscrew (dextral), correlated with the dextral twist in the majority of gastropods. The robber crab Birgus shows a striking sequence of symmetry changes. When it leaves the sea it is bilaterally symmetrical; it then emerges into the land and becomes dextrally asymmetrical, inhabiting mollusc shells. Finally, air-breathing lungs are developed and the crab leaves its mollusc shell to become free-living on land, redeveloping bilateral symmetry.

Fiddler Crabs. Crabs and lobsters show a marked size and shape difference between the large claws on the two sides of the

body. The larger one is adapted for crushing, the smaller one for picking up pieces of food. In the extreme example of the fiddler crabs (Uca spp.) the large claw may weigh 20 times more than that of its partner, amounting to 70 per cent of the weight of the whole body. Only males develop this large claw, which is used in courtship, both for display and stridulation. Chance injury determines whether the large claw develops on the right- or left-hand side. If a claw is lost in a young male, another regenerates. In this case the surviving original claw grows on (it has a start) to become the large one.

. . . . .

Flatfish. The flatfishes are the most asymmetrical vertebrates. In the plaice, for example, the bilaterally symmetrical larva settles on its left side on the sea bed when about 30 days old. A spectacular metamorphosis then occurs, at the end of which both eyes lie on the top (right) side. Cryptic coloration is found only on the right side and the fish now swims with the right side uppermost. There is a difference in the form of the scales of the two sides as well as in the sense organs. An experiment reported last century needs repeating: when young flatfish were prevented from settling on their left side before metamorphosis, and made to keep swimming, they retained their bilateral symmetry.

. . . . .

The Evolution of Bias

We have seen that in fiddler-crabs chance injury determines whether the large claw develops on the right or the left side of the body. Also, in the primitive flatfish Psettodes there is an equal number of left and right asymmetrical individuals. There are, however, many examples where there is not an equal chance of left or right asymmetry. It is clear that some species are able to select left from right during development, and in some cases to make a correct choice nearly every time. Figure 3 [not reproduced] shows dextral (very common) and sinistral (very rare) examples of a snail (Cepaea nemoralis), in which bias approaches 100 per cent. In flatfish there is an evolutionary trend towards greater bias.

Problems Posed

Consideration of animal asymmetry reveals some challenging unsolved problems in developmental biology. How can an animal build a bilaterally symmetrical body using asymmetrical components (e.g. L-amino acids, D-sugars, right-handed a-helix)? In the inorganic world, bilaterally symmetrical crystals form spontaneously with the expenditure of the minimum amount of energy. Since the building units of organisms are asymmetrical (molecules, organelles, cells, etc.), it has been argued that the achievement of bilateral symmetry in a whole organism must involve higher energy expenditure. For instance, in the replication of two-stranded DNA (giving right-left symmetry of base pairing), two enzymes seem to be required (ligase and replicase) instead of one. Hence at this molecular level, bilateral symmetry is expensive in energy terms.

The problem recurs in reverse. Given a bilaterally symmetrical animal, how can it evolve to give rise to an asymmetrical form? DNA codes for protein which control an organism's shape and metabolism. Since the DNA code occurs identically in every somatic cell of the body (i.e. not counting sex cells), how can it be interpreted differently on the two sides of the midline, in a non-erratic manner?

This problem becomes even more challenging when the question of bias is considered. Not only must we postulate differential interpretation of the genetic code on the two sides of the body, but we have to suggest that the code "knows", often with great accuracy, what to build on a specific side of the body. (A trivial explanation of bias could be that there is a difference in mortality rate between left and right individuals of a species. However, extensive experiments with huge numbers of the snail Limnaea peregra did not support this explanation.)

Furthermore, asymmetrical bias can vary geographically. If bias were to be affected environmentally we might have expected it to be reduced (i.e. to approach 50 per cent). However, this is not so. For example, in the Pacific flounder (Platichthys stellatus), the bias to sinistrality at metamorphosis is 49-60 per cent off North West America; 68 per cent off Alaska; and 100 per cent off Japan. There is also evidence of geographical variation in asymmetrical bias in the twisting direction of male abdomens of the fly Clunio marinus. Clockwise twisting dominates in Norway and is about equal with anti-clockwise twisting in Brittany, but anti-clockwise dominates in north Spain. Again, bilateral gynandromorphs occur in common blue butterflies (Polommatus icarus), with half the body male (blue wings) and the other half female (brown wings). Such examples are more common in Ireland than in England. Similarly, in the case of the snail Cepaea nemoralis, sinistral examples are frequently found on sand dunes at Bundoran and Donegal in the Irish Republic; they are extremely rare in most other places.

• Superorganisms

SOCIAL COORDINATION AND THE SUPERORGANISM

Emerson, Alfred E.; *American Midland Naturalist,* 21:182-206, 1939.

Discussion. I think that the foregoing discussion amply demonstrates that from both ontogenetic and phylogenetic considerations, the social organism, at least as evidenced in insect societies, parallels the integrative dynamics of the organism at

lower levels of individuality and at the same time, through a more complex coordination, tends to bring the environment of the organism under control, much as the multicellular organism brings the environment of the cell under control. It may be that more exact knowledge of the mechanisms and functions will show that some of the forces producing the analogies are not similar and catagories applicable in a given realm may not be stretched beyond that realm. It should also be emphasized that, valid as the superorganismic concept may be as applied to insect societies, other principles may also be applied to societal organizations which will help us better to understand these phenomena.

The application of the concept to human society is beyond the competence of the speaker. In certain respects, the comparison seems valid, but in others which rest upon "social heredity" as compared to germinal heredity, striking differences occur which may be too great to make their analogy significant. I feel, however, that biologists and sociologists need to study comparable facts critically in the light of new discoveries, and in numerous cases, what may seem superficial analogy may be shown to be due to fundamentally similar causal factors. "Social heredity" bears certain similarities to germinal heredity and may in part be under the influence of a sort of natural selection. Social integration in human society shows certain similarities to social integration in insect societies. These similarities, as well as differences, should be studied as scientifically as any of the more physiological integrating mechanisms of the organism.

It might be of interest to see what level of organismic evolution the highest insect colony has reached. The lack of clearly defined social organs, controlling social brain, and permanent symmetry patterns for large taxonomic categories, would indicate a relatively low level of organization. Our best parallels seem to be with organismic characters not far above the colonial protozoa, sponges, or coelenterates, and many of our analogies are hardly above the level of a unicellular organism. Much of the criticism of the superorganismic concept rests upon the difficulties of finding convincing analogies with the highest organisms.

In some ways the human social groups seem above the level of the social insects and in other ways the social insects seem to have attained a higher integration. Human social institutions may almost be thought of as social organs, but the germinal basis of society among the insects implies a longer evolutionary history and greater stability, efficiency, and coordination. Chen concludes, "The social increment in man and other animals is never so great as that of ants. The very large social increment in ants is probably due to the fact that ants are much more highly socialized than men, hens, rats, and goldfishes; hence they are much more affected by being isolated or grouped." The failure of human society to evolve a more complete specialization of reproductive and somatic functions makes the organismic analogy less convincing than in the case of the insect society.

Human social groups are able to adjust to a complex environment with greater facility and versatility, particularly through

cultural or "social hereditary" mechanisms combined with intricate learned behavior. Insect society works more smoothly, with less social friction and with less drag from poorly integrated elements.

The concept of the insect superorganismic individual not only rests upon the significant correlation of much factual evidence, but can definitely contribute to the formulation of a number of important biological principles. Of course, much of our evidence rests upon the use of analogy. Some biologists have tended to assume that analogy and superficiality are intimately wedded. Bowne has referred to Spencer's confusion of logical classification with genetic order as the "fallacy of the universal". I hardly think it necessary to argue the importance of analogy before a group of ecologists. Much of our technique in discovering the nature of the influence of fundamental environmental factors upon biological systems rests upon the discovery of analogies. What homology is to the geneticist, analogy is to the ecologist. One must understand analogy to detect homology and vice versa. Convergent adaptive evolution is only one great concept resting upon the use of analogy. It is my own opinion that an understanding of the forces which produce analogy are as scientifically profitable as an understanding of the forces producing homology. Natural Selection often results in the evolution of similar functions determined in part by quite different and therefore analogous mechanisms. We thus have evidence to indicate that function may dominate the mechanism, even without recourse to mystical teleology. Wright, referring to Karl Pearson's view, says, "science is alway essentially statistical, being always based on classifications which of necessity throw more or less diverse things into common categories." However, one must have detailed objective evidence, when using analogical method, that the environmental forces have identity, much as the concept of homology demands an identity of genetic and developmental forces. The existence of significant analogy should stimulate us to an increased study of causative factors.

I shall do no more than mention the remarkable parallelism between the intraspecific or homotypic superorganism and the interspecific or heterotypic ecological community. Others in this series of discussions have dealt with what might be called the interspecific superorganism.

Cleveland points out that colony life in the more primitive termites is essential since the cellulose digesting protozoa are lost at each molt and reinfection can only take place through association with non-molting individuals. In the case of the roach, Cryptocercus, however, transmission of protozoa only occurs at the time of molting so that the recently hatched roach must live with molting nymphs. New colonies could not be started by adult pairs of roaches, therefore, while new colonies of termites can be founded by a pair carrying protozoa. We thus see how interspecific symbiotic relations may determine major patterns of family and social life among these insects.

The symbiotic protozoa and termitophilous and myrmecophilous insects, together with the scavengers, predators, parasites, nest eaters, and inquilines, show varying degress of integration

with the high social insects. Many of these interspecific associations depend upon the evolution of societies, particularly the slave makers and social parasites, as well as the symphiles among ant and termite guests. The same forces which bring about the integration of the organismic units within the species can also be shown to be active in the ecological community which, of course, came into existence not long after the origin of life itself. The organism, in a sense, projects itself into the community because so many species in the community are adjusted to special organismic structures and integrative mechanisms. The ecological community also has an ontogeny (succession) and a phylogeny. One might question the phylogeny of the ecological community on the basis of the lack of germinal continuity of the unit, but we must remember that individual genes and chromosomes show independent assortment in the history and dynamics of the germ plasm, and thus parallel the independent assortment of germ plasms among the species composing the integrated ecological community. The ecological community may exhibit loosely integrated components, but such closely integrated relationships as the cellulose-digesting symbiotic flagellates with their roach and termite hosts have a long history indicating greater stability than the "ageless" mountains.

Possibly we should classify ecological associations as ascending levels of super-superorganismic integration which show partial physiological isolation between the community types, and complete isolation only between the biota of this planet and that of some other planet. The integration of all life on the earth forms a "Leviathan," to use Patten's term, and the analysis and synthesis of the mechanisms of this "Leviathan" are the major duty of the ecologist. Let us not, however, raise the superorganismic concept to an all or none principle. Let us rather use the perspective it gives us to stiumlate further study and understanding. (pp. 198-201)

• Doubts about the Recapitulation Theory

RECAPITULATION AND ITS SUPPOSED CAUSES

Holmes, S. J.; *Quarterly Review of Biology*, 19:319-331, 1944.

The very existence of a tendency for ontogeny to recapitulate phylogeny has long been a highly controversial question, and one finds distinguished biologists aligned on both sides of it. No one

can deny that there is a general resemblance between the ontogeny and phylogeny of higher organisms simply because they start with a simple cell, an ovum or a one celled ancestor, and end in the same adult individual. Oscar Hertwig (1906) has argued at quite needless length that we cannot compare the ovum of a mammal with a unicellular animal because the former differs enormously from the latter in its complex of developmental factors which are the product of a long process of evolution. This obvious difference was recognized by Haeckel and other proponents of the biogenetic law, and I cannot see that it disposes of the recapitulation theory, which must stand or fall on the basis of observations in comparative embryology and morphology. The arguments most relied upon by the recapitulationists are based on similarities between ontogeny and phylogeny which are too specific and peculiar in character to be accounted for as simply a consequence of the increasing complexity of the two series, even though they reach a common goal. This is shown by the embryonic history of the gill arches, renal system, the development of the skull and other skeletal structures, the behavior of rudimentary organs, and numerous other facts adduced by the recapitulationists. Such resemblances are quite generally conceded to be explicable only as a consequence of origin through evolution. But among the biologists who accept this conclusion we find some who contend that facts such as the history of the gill arches in the embryos of birds and mammals are due to a tendency to recapitulate the adult characters of ancestral forms, while others contend that they are merely developmental similarities resulting from a common ancestry. This is the basic issue which has separated controversialists over the biogenetic law from von Baer to the present. There is resemblance, according to von Baer (1828-1837), not between an early mammalian embryo and an adult fish but between a mammalian embryo and a fish embryo up to a certain stage. Von Baer strongly criticized the doctrine of recapitulation as set forth by Kielmeyer (1793), Meckel (1821), Oken (1847), and Serres (1824, 1842, 1859), and substituted for it his own well-known law to the effect that embryos belonging to any of the great groups of animals are at first very much alike and develop along closely similar paths to a degree dependent upon their degrees of affinity. Thus fish, reptiles, birds, and mammals are much alike in their earlier stages, but the fish and mammals diverge first; birds and mammals keep pace longer, and in the mammals man and ape show significant differences only at a relatively later period. Here there is parallelism but not recapitulation. (p. 319)

The retention of gill pouches and gill arches in the embryos of higher vertebrates has long been cited as evidence of recapitulation. The opponents of this doctrine could say that this fact simply shows a parallel development of fish and the higher vertebrates in early stages. The long persistence of these structures, when so many other fish-like characters have disappeared, is commonly explained on the theory that they form necessary stages in the development of permanent organs of the higher vertebrates. The Eustachian tube in man, for instance, arises from an anterior gill slit which is phylogenetically the older

organ. It is claimed that if the tube continues to be formed the gill slit must continue to be formed or else some other de novo method must be substituted. This is no easy task, although similar feats have at times been accomplished. By far the easier method is to lug along the old precursory procedures. In many ways an appearance of recapitulation results from conservatism in the methodology of organ formation.

Although an embryo as a whole would not closely resemble any ancient progenitor, one cannot assert that the same statement necessarily applies to a particular organ. There might be recapitulation of certain ancestral structures or even of a limited succession of them, but even where long lost parts are retained on account of their physiological importance in embryogeny, they usually become greatly modified in course of time. This may be partly owing to the influence of other parts, to changes in the hereditary factors that cooperate in their production, or again to taking on new developmental functions as in the case of several of the endocrine glands. Doubtless the assumption of endocrine activity has saved many an organ from extinction. One might expect, therefore, that the extent to which embryonic organs are palingenetic would be exceedingly varied. By the proper selection of cases one might build up an impressive array of evidences for the theory of recapitulation. This would not prove that ontogeny is largely determined by phylogeny, in the way supposed by Haeckel. Old adult structures may be quickly lost or retained for an apparently unreasonable length of time depending on circumstances affecting their functional importance in development. There can be no doubt that the critics of the biogenetic law as formulated by Haeckel are right in many of their contentions. Embryo of higher forms do not closely resemble the adult stages of the series of progenitors. But the critics often attempt to dispose of the doctrine of recapitulation too simply, by saying that all the similarities in question are due to the fact that one ontogeny resembles another owing to a common heredity. Garstang (1922), for instance, in an attempt at a critical restatement of the biogenetic law states that "The phyletic succession of adults is the product of successive ontogenies. Ontogeny does not recapitulate phylogeny; it creates it. There is a general correspondence between the successive grades of differentiation in ontogeny and the successive types of organization which characterize the steps of the phyletic progress. This general correspondence exists because each series---the ontogenic and the phylogenic---was preceded by the same phyletic series of ontogenies. The outcome of each successive series was an adult representative of one of the successive types of organization. The last ontogeny of the whole series is the one under consideration. Inevitably there is recapitulation of successive grades of differentiation, but repetition of adult ancestral stages is necessarily and entirely lacking." (pp. 325-326)

Our brief survey has shown, I think, that the doctrine of recapitulation has had a rather curious history. This is largely because recapitulation has turned out to be something quite different from what it seemed to most of its adherents. We cannot regard

it as a uniform expression of the mysterious creative forces of nature, as held by Kielmeyer, nor as a direct consequence of heredity as maintained by Haeckel. We cannot consider it a manifestation of memory like the repetition of a tune, nor as a mechanically determined pushing back of embryonic characters, as postulated by Weismann. We may be justified in construing it as in part a function of the way in which germinal mutations fit into the physiology of development, but the appearance of recapitulation may aso be merely an expression of the uniformities of relative growth. In the light of present knowledge, recapitulation represents no simple principle deducible from some more general law. It is rather a conglomerate effect of heterogeneous causes. We shall doubtless know more of at least some of these causes when the causal factor of embryonic development are better understood than they are today. (p. 330)

FLAWS IN THE RECAPITULATION THEORY

Dewar, Douglas; *The Transformist Illusion,* DeHoff Publications, Murfreesboro, 1957, pp. 188–191 and 194–196.

Facts Fatal to the Theory.

1. Admittedly it does not apply to the embryonic development of plants. This is inexplicable if recapitulation be a law of nature, and, if, as transformists believe, plants and animals are descended from a common ancestor.

2. Between the one-celled stage and the blastula stage every embryo passes through a two-celled, four-celled, eight-celled, sixteen-celled, thirty-two-celled stage. These, while mechanically essential, cannot represent ancestral stages, because no animal or plant, living or extinct, is known composed of two, or four, or eight, or sixteen cells, etc. In the animal and plant kingdoms there is a great gulf between the one-celled Protozoa and the many-celled Metazoa. Moreover, according to D'Arcy Thompson the facts of surface tension prohibit the existence of a two-celled creature. Thus the recapitulation theory comes to grief almost from its outset!

3. It sometimes happens that the embryos of closely-allied species pursue different developmental courses. Thus the Crustacean Peneus and the common crayfish resemble one another so closely that, if there be anything in the evolution theory, they are near akin. Nevertheless Peneus hatches out in the form of a larva known as the Nauplius and passes through the stages known as Metanauplius, Protozoea and Zoea before it assumes the adult form, whereas the crayfish emerges from the egg in a form like that of the adult, after having undergone development as direct as possible.

Most crabs leave the egg as a Zoea larva, then pass through two more stages before they assume the adult form. Some, however, leave the egg in adult form. Many of the latter are fresh-

water species which would be carried out to sea by the current of the river in which they are hatched, if they emerged from the egg as free-swimming larvae as most sea crabs do. Obviously both the crabs that undergo metamorphosis and those which do not cannot recapitulate the supposed history of their race.

4. Transformists believe that birds are derived from ancestors which possessed teeth, but no traces of teeth are found in any of their embryos.

5. The head of the human foetus progressively lessens in relative size as it develops, instead of becoming progressively bigger as the evolution theory requires.

6. While the growing embryo shows all the supposed ancestral stages of the urinary system it shows none of the presumed stages in the transition of the respiratory system from gills to lungs.

7. Some organs develop in such a way in the embryo that they cannot possibly represent ancestral stages, e.g. the eye and the heart.

8. Another fact fatal to the recapitulation theory is the early stage at which every embryo assumes all the features of the genus to which it belongs. In their zeal to prove the truth of the evolution theory many zoologists have overlooked this; indeed some have actually misled the public on this subject. T. H. Huxley gave, in "Man's Place in Nature" figures of the embryos of a dog and a human being to show how closely they resemble one another, but he does not say that the latter is an embryo only 23 days old. He describes in some detail the development of each embryo up to this stage, emphasizing the similarity of the process in the two cases. He does not continue beyond this stage, alleging that to do so would be tedious and unnecessary for his purpose. He did not add that it would have destroyed his argument! He is guilty of more than mere omission---for he writes: "Indeed it is a very long time before the body of a young human being can be readily discriminated from that of a young puppy." The truth is that by the time the human embryo is forty days old it is impossible to mistake it for that of a dog, and by the forty-fifth day it is unmistakably that of a human being. Sir Arthur Keith writes of the human embryo: "Human characteristics begin to peer through its higher primate qualities before development is a month old." Thus an expert can identify an embryo as human by the thirtieth day.

It is to be regretted that a few transformists in their anxiety to demonstrate that man has descended from a lower animal have published pictures greatly exaggerating the similarity between human and animal embryos. Haeckel was a bad offender. He was charged by Brass, Hiss, Rutimeyer, Koelliker and Keibel with "faking" his illustrations. The worst of Haeckel's misdemeanors was to cause the same plate to be printed thrice over and to label one a human embryo, the second a dog embryo and the third a rabbit embryo, in order to show how similar the embryos of these are.

The human embryonic period is about nine months. Haeckel gives in "The Last Link" (1898) thirty-two stages through which

man is supposed to have passed during his evolution from a single-celled ancestor. If this supposed history were recapitulated at an even rate, the passage through each of these stages would occupy about eight days, and it would be impossible by inspection to discover whether or not any embryo were human until about the 270th day of its existence, whereas it is possible to do so about the 30th day. Man is not peculiar in this respect . . The foetus of a cat is distinguishable from that of a dog before the teeth or claws have shown themselves: in other words an embryo bears the stamp of the family to which it belongs before the appearance of the features by means of which systematists distinguish its family from other families. Clearly then, the development of the embryo does not follow a supposed ancestral history: the final form of every organism is determined at a very early stage of its development. If in the past any type has been transformed into another type, the transformation must have been determined very early in embryonic life.

9. Every transformist believes that the horse of today is descended from an ancestor having five toes on each foot. They all cite as an ancestor of the horse Eohippus of the Eocene period which has four toes on each fore-foot and three on each hind. But at no period of the embryonic development of the horse is there any trace of five toes. The embryonic foot exhibits three rays, the middle one of which is the largest and develops into its toe, while the smaller lateral ones become the splint bones. There is no recapitulation of a five-toed ancestor. This does not prevent transformists from asserting that the presence of a tail in the human embryo from the 5th to the 8th week of its existence is the recapitulation of the stage of a long-tailed ancestor. This is supposed to be recapitulated, but not the 5-toed stage of the horse ancestry.

Most evolutionists put an evolutionary interpretation on all the phenomena of embryonic development. All these however are capable of another interpretation which fits the facts comfortably, and satisfactorily accounts for the contrivances by means of which a highly complicated organism, that not only inherits the characteristics of each parent, but is distinguished by small differences from every other individual of the species, is developed in a few weeks from a fertilized ovum of microscopic size. From start to finish intelligence is at the back of this process, the difficulties of which are overcome by a series of beautiful devices. (pp. 188-191)

Transformists unfailingly cite the so-called fish-stage through which the embryo of every vertebrate passes as one of the best "proofs" of evolution. Prof. H. Munro Fox writes ("Biology" (1936) p. 303):

> "The embryos of both birds and mammals, including man himself, have gill-slits and a two-chambered heart. In these embryos, unlike the tadpole, the gill-slits are never of any use. Later on in development they disappear. This means that the land vertebrates once had fish-like ancestors and

still, while they are embryos, birds and mammals continue to pass through a stage which their aquatic ancestors passed through when they were embryos."

The truth is that the so-called fish stage of every embryo must be passed through for the same reason that during construction a four-storied building must pass through a two-storied stage.

The so-called fish heart and gill-arches have to be formed because the head region of the embryo from a very early stage onwards, requires a copious blood supply. This necessitates the early formation of a heart or pumping organ and a simple system of blood vessels. These have to be formed before there is time to develop the four-chambered heart necessary to the higher animal. To accomplish this, one or other of two devices must be adopted. Either a simple heart must be developed to function while another complicated heart is developing, or the simple heart must be so constructed that it can become transformed into a four-chambered heart while it is operating as a heart. In this case the latter course is adopted, and by a most ingenious arrangement this simple heart while it is continuously working is converted into a four-chambered heart. In some other organs, such as the kidney, the former course is adopted (see p. 198).

The heart develops as follows: Two tiny tubes are formed which run parallel. Those coalesce to form a single tube; the wall of the front part of this thickens, and the thickened part becomes separated from the thinner hind part by valves. The heart is now an effective pumping machine composed of two communicating chambers; a posterior one, the auricle, and an anterior one, the ventricle, which, by contraction of its thick muscular wall, expels blood into the arteries, the backward flow being prevented by the valves between the two chambers. In fishes this type of heart persists throughout life, being suitable for a gill-breathing animal of comparatively simple structure. Animals higher in the scale need a more complicated heart and in them the embryonic heart becomes three- or four-chambered, as the case may be, by the growth of a septum in one or both of the chambers.

Clearly then the reason why the mammalian embryonic heart is at first a simple tube is, not that mammals evolved from fishes, but that, as the mammalian embryo must have a functioning heart at a very early stage, the simplest possible type of heart is formed. As development proceeds the form of the heart changes to meet the increasing demands made upon it.

The visceral arches are as necessary to the embryo of the higher vertebrates as to that of a fish; in both they form part of the lateral wall of the cephalic extremity of the embryo, serve as paths for the aortic arches and later contribute to the formation of various organs. As regards the blood vessels which pass along them. At the moment when the heart is ready to function the head region is in urgent need of a supply of blood, in consequence, each branch of the ventral aorta pushes forward and passes along the foremost visceral arch and so reaches the back without piercing the alimentary canal. Having reached the top of the visceral arch each vessel bends back and conveys blood to the

middle and hind regions of the embryo. Then to meet the increasing demand for blood, three more aortic vessels arise in the next three arches. Then there are four vessels on each side conveying blood from the ventral to the dorsal aorta. Then, to meet the increasing demands of the head region, the first three aortic arches become converted into the internal carotid and other arteries which supply the head. Meanwhile another aortic vessel is formed in the last visceral arch. This eventually forms part of the pulmonary artery. The aortic vessel ends up as part of the aorta of the adult. Sometimes a fifth vessel develops in the fifth visceral arch, this later disappears without giving rise to any blood vessel in the adult.

Having served their purpose in embryonic life the visceral arches give rise to several organs required for use by the adult. Up to the time of the appearance of these visceral arches the development of all vertebrates follows the same lines, because all are constructed on the same plan. From this stage onwards development differs according as the animal breathes in water or in air. (pp. 194-6)

• Possible Inheritance of Acquired Characters

SYMPOSIUM ON THE INHERITANCE OF ACQUIRED CHARACTERS

Scott, William B.; *American Philosophical Society, Proceedings,* 62:270-273, 1923.

My function this afternoon is to act as a prologue; and, in the first place, to explain to those of you who are not biologists precisely what the problem is, and, in the second place, to give a brief historical outline as to the course and fluctuation of opinion with regard to it.

The problem may be briefly stated thus: Whether the characters which are acquired in the lifetime of a parent are transmissible to the offspring. Acquired characters are those with which the individual is not born, or hatched, or otherwise brought into the world, nor such as would normally develop at certain stages of life. A man, for example, is born without a beard, but a beard can not be regarded as an acquired character, it is merely an illustration of what Darwin called "inheritance at corresponding stages of life." Acquired characters are modifications peculiar to the individual. There is no question at all that congenital characteristics, those with which the individual is born, are largely transmissible. The doubt comes as to whether such

modifications as arise in the organism, due to injury, accident, disease, or the habits of life, are ever passed on, in however slight degree, to the offspring.

Many savage tribes practice mutilation of one kind or another, or deformation of children. The Flathead Indians, for example, distort the heads of their babies, and for centuries the women of the upper class in China have had their feet distorted by binding. Then again aptitudes and habits are, especially in the oriental countries, practiced by the same family for many centuries. Is there any evidence that the children of those families, aside from early training, more easily acquire skill and attain a higher degree of it than the children of families in other handicrafts? It must not be supposed that all acquired characteristics are of the nature of those previously mentioned. They may be structural, as the result of use or disuse on the part of the individual. Parts which are not used at all tend to shrink and atrophy, while those which are regularly and vigorously exercised increase. Are such effects ever heritable?

Now, without taking sides, pro or con, it remains for me to sketch briefly the course of scientific opinion on this topic.

For a long time the transmissibility of acquired characters was taken for granted, and the great difference between the views of Lamarck and those of Darwin was as to the importance in the causation of evolutionary changes which was to be attributed to such transmission. Lamarck gave much higher importance than did Darwin to these individual changes and acquisitions. Nevertheless, Darwin explicitly accepted the heredity of acquired characteristics, and repeatedly calls in the facts of use and disuse to explain the course of development. In one famous chapter he compares the skeletons of wild and domestic ducks, and shows that in the latter the bones of the wings are relatively smaller than in those of their wild progenitors; and he explains this difference by the fact that the domestic duck uses its wings but little, or not at all. In order to explain the transmission of such characters from generation to generation, Darwin devises an elaborate theory of heredity, the theory of Pangenesis, though this never found any considerable degree of acceptance. It must also be recognized that Darwin attributed a distinctly secondary importance to the acquisition of individual characteristics and their inheritance by subsequent generations. To him the all-important changes were the congenital variations with which the individual was born. Nevertheless, it is distinctly unfair and inaccurate to insist that the theory of the transmission of acquired characters is purely Lamarckian and not Darwinian. The difference between these two illustrious men in this regard was as to the degree of importance which should be attributed to this process.

It has long been known that cave animals of various classes, fishes, amphibians, insects, and spiders, which live in total darkness throughout their entire lives, are nearly always blind and without pigmented color. This general phenomenon (which is also largely, but not exclusively, shared by the creatures of the deep sea) was formerly attributed to the direct action of the darkness itself upon successive generations of these cave animals; and this,

acting cumulatively, produced the effect of blindness and lack of pigmentation; and a more typical instance of the supposed transmission of acquired characters could hardly be selected than this.

An entirely different aspect was given to the whole question by the publication, from 1889 to 1892, of the various essays of the late Professor Weismann, of Freiburg, in Germany. Weismann's career was quite a remarkable one. Beginning with the microscopic work, then universally expected of every German professor of zoology, he was forced by failing eyesight to take to other lines of work; and he published the results of a series of experimental investigations upon butterflies, amphibians, and other creatures, which were gathered up and preserved in book form, and published in England with a preface by Darwin. This work was followed by a remarkable series of speculations upon the constitution of the germ plasm---i.e., the wonderful material which is the carrier of heredity. To a certain extent, Weismann's view as to the continuity of the germ plasm was anticipated by Sir Francis Galton in England, who compared the germ plasm to the chain of a necklace, and the individual to the pendant hanging from each link.

The work of Gregory Mendel, the famous Austrian monk, which was published in 1866, was so far ahead of its time that it fell dead-born from the press, and was altogether forgotten until Mendel's results were rediscovered by the German and Dutch botanists. Weismann thus knew nothing of Mendel's work; and it is very surprising to see how, by a process of pure speculation, he reached very similar results. Time would fail to give any, even the briefest, account of Weismann's views as to the constitution of the germ plasm, nor is it necessary in this connection. It will suffice to say that Weismann was led to deny, in toto, the possibility of the transmission of acquired characters, not from experiments or observations upon that particular problem, but because his theory of heredity could provide no machinery for such transmission. He thus became an extreme Darwinian, in the narrowest sense of the word; and one of his papers bears the title "The Omnipotence of Natural Selection," which he regarded as the sole active factor in organic development. Weismann's views speedily found a very wide acceptance throughout the world; and it is probable that the overwhelming majority of botanists and zoologists would today deny the possibility of the transmission of acquired characters. Even the medical men speedily accepted the same conception of the heredity of disease. Aside from cases where the unborn child is directly infected, it is now largely maintained that what is transmitted from parent to child is the liability to a particular disease; and not the disease itself---the constitution which lays the individual open to infection.

For years past in many quarters the question has been regarded as definitely closed and settled, but it will not down; and in some of the testimony which you will hear in the course of this symposium you will see the reason why this problem remains one of sempiternal interest.

INHERITANCE OF ACQUIRED CHARACTERISTICS

Koestler, Arthur; *The Roots of Coincidence,* Random House, New York, 1972, pp. 133-135.

The preceding section may have evoked in the reader a feeling of deja vu, because earlier on (p.82) I mentioned another type of "filter theory" related to Evolution. I am referring to the neo-Darwinian theory, according to which the hereditary substance in the germ cells is protected by an almost inviolable barrier against influences originating in the outside world. The "almost" refers to cosmic rays, noxious heat, and chemicals which might penetrate the barrier and cause mutations in the genes. Most of these are harmful, but from time to time there are lucky hits, and these, with the aid of natural selection, keep the wheels of evolution turning. Apart from that, any possibility of some acquired characteristisc becoming hereditary is prevented by the barrier. Lamarckism, which postulated that beneficial improvements in physique or skills acquired by the parents could be transmitted to the offspring, must be discarded as an unscientific superstition.

This is the neo-Darwinian doctrine. And yet certain evolutionary phenomena, quoted over and again in the literature, seem to point stubbornly at some Lamarckian factor in evolution. A simple example is the skin on the soles of our feet, which is much thicker than elsewhere. If the thickening occurred while the baby learned to walk, there would be no problem. But the thickening is inherited, the baby is born with it. Equally puzzling are the inborn callosities on the camel's knee, and the bulbous thickenings on the ostrich's undercarriage, one fore, one aft, on which the ostrich squats. These too are, like the skin on our soles, already present in the embryo; they are undeniably inherited characteristics. Yet in conformity with the prevailing dogma, we are asked to believe that the advent of these callosities at the exact spots where the animal needed them was due to pure chance ---like the scarab appearing at Jung's window.

One could almost substitute ESP for IAC (Inheritance of Acquired Characters) to see the same pattern of argument emerging, and the same quasi-theological passions accompanying it. The Lamarckians found themselves in a predicament similar to the parapsychologists': they were unable to produce a repeatable experiment. Cases of apparent IAC in the animal kingdom were rare, the phenomena were capricious; each apparently clear-cut case was open to different interpretations---and as a last resort, to accusations of fraud. Moreover, though the Lamarckians were convinced that IAC did occur, they were unable to provide a physiological explanation for it---as parapsychologists are unable to provide a physical explanation for ESP. (pp. 133-135)

• Life's Supposed Urge

URGE AND MOLECULAR BIOLOGY

Mora, Peter T.; *Nature,* 199:212-219, 1963.

Definition of Biological Urge and Delineation of the Problem. Living entities have several characteristics which make them distinct from non-living. They are autonomous; the individuals are similar but not exactly alike; they group in hierarchical systems; their mature state is more or less invariant, but still they are in continuous flux. They are self-maintaining; they build a complex and highly improbable structure necessary for the fulfilment of their function; they restore this structure when worn out, and repair it when damaged. They continuously adjust themselves to their enrivonment, and they may adjust to even completely novel conditions, which they or their predecessors never could have faced before. Most important of all, a certain urge or drive towards self-fulfilment characterizes the living organism in all its activity and at all levels, from the simplest to the most complex.

Several words have been used for describing at least certain aspects of the last-mentioned attribute. Unfortunately, almost all the words available are anthropomorphic and therefore have some unwanted associated meaning; moreover, they often include properties which can be explained by evolutionary selection. Some of the words, such as 'wholeness', 'directiveness', 'orientedness', are not very useful for my purpose, and others, such as 'purposiveness', 'consciousness', 'entelechy', or 'vis vitalis', are clearly unsuitable, because they imply more than the materialistic approach I intend. I choose 'conatus', which, used in this context (Spinoza), means drive, effort or urge. I hope the word urge is unpretentious enough (actually it is almost vulgar) to prevent my being labelled a metaphysician. At the same time it best indicates to me the most important and peculiar attribute of the living, the relentless striving to survive, to absorb, to expand, to dominate.

The assimilation of metabolites, the continuous use of energy for the sake of the living entity, is an example of the urge. A living entity (for example, a cell in tissue culture) does try to expand, organize the elements around it (the random mixture of molecules in the medium) and so to speak, subjugate or dominate its vicinity, regardless of whether it encounters other living units or not. Now consider the formation of a clone from a single cell. Replication, and generally reproduction and progeny formation, may be thought of as an extension or even a consequence of the urge of the individual living unit towards self-continuance and a spreading over or overwhelming of the available suitable space.

So far as I know, no good reason has been advanced on a molecular level for the occurrence and perpetuation of both these processes. The fact that we now are learning about these key molecules (intermediary metabolites, DNA, enzymes, ATP, etc.), their structure, and some of the details of the mechanism of metabolism and of replication on molecular level (the complicated schemes of intermediary metabolism, DNA strand separation, complementariness of nucleotide bases and the pertinence of this to replication, etc.) does not explain why metabolism and multiplication occur at all, or why living systems build their improbable but functional structure, or what keeps them living.

The problem I raise of how to approach such obvious phenomena by scientific research is not helped by saying that this is just in the nature of living matter, an inherent attribute, the way mass or gravitational attraction, etc., is an attribute of matter. But mass, gravitational attraction, and other physical properties are inseparable attributes of all physical matter, and they are not restricted by time to a certain period. The urge is a peculiar attribute, restricted only to the matter of the living while the living is alive. Fundamentally, I am indeed asking what the difference is between the living and the non-living matter, and whether molecular science as we know it is capable of dealing with this question.

This question then asks why is living matter alive, and actually involves really not the 'how' but the 'why'. One may say that the purpose of science is not to answer the 'why' but the 'how', and the question of 'why' should be left to philosophy or to belief. However, I do not question why living matter exists at all, as this would be a part of a philosophical problem of existence in general; but how to approach scientifically the problem of to what extent and why living matter is different while alive from the non-living matter. (pp. 212-213)

PHYSICAL AND LOGICAL CONSTRAINTS

• The Laws of Thermodynamics and Life

THERMODYNAMICS AND THE ORIGIN OF LIFE

Morris, Henry M.; *Institute for Creation Research, Impact Series, No. 57,* March 1978, pp. 1-4.

Evolutionists are embarrassed by the Second Law of Thermodynamics. Dr. V. F. Weisskoff, President of the American Academy of Arts and Sciences, has recently pointed up the problem in the following words:

> The evolutionary history of the world from the 'big bang' to the present universe is a series of gradual steps from the simple to the complicated, from the unordered to the organized, from the formless gas of elementary particles to the morphic atoms and molecules and further to the still more structured liquids and solids, and finally to the sophisticated living organisms. There is an obvious tendency of nature from disorder to order and organization. Is this tendency in contradiction to the famous second law of thermodynamics, which says that disorder must increase in nature? The law says that entropy, the measure of disorder, must grow in any natural system.[1]

The "obvious tendency of nature from disorder to order and organization" is, of course, only an assumption of evolutionists. The real tendency in the natural world, as expressed by the Second Law of Thermodynamics, is from order and organization to disorder. This very obvious problem is commonly bypassed by evolutionists with the naive statement that the earth is a system open to the energy of the sun and that this fact resolves the problem! Creationists in turn have reminded them that while an open system and available energy constitute necessary conditions before a growth in order (or information) can take place, they are not sufficient conditions. In addition, there must be a precoded program containing the necessary information to direct the growth of the system and one or more conversion mechanisms to convert the external energy into the highly specific work of internal growth. Since the vast system of the hypothetically evolving biosphere as a space-time continuum seems to lack both a

program and mechanism, it is clearly precluded by the Second Law.[2]

It has been especially difficult to imagine ways to get life started in the first place. How can unordered non-living chemical elements be combined naturalistically into the extremely sophisticated ordered information in a replicating system? The common belief that this problem has been practically solved by modern biochemists is premature, to say the least. Freeman Dyson says:

> We are still at the very beginning of the quest for understanding of the origin of life. We do not yet have even a rough picture of the nature of the obstacles that prebiotic evolution has had to overcome. We do not have a well-defined set of criteria by which to judge whether any given theory of the origin of life is adequate.[3]

The nature of the problem in trying to account for the origin of a replicating system has been well expressed by Angrist and Hepler:

> Life, the temporary reversal of a universal trend toward maximum disorder, was brought about by the production of information mechanisms. In order for such mechanisms to first arise it was necessary to have matter capable of forming itself into a self-reproducing structure that could extract energy from the environment for its first self-assembly. Directions for the reproduction of plans, for the extraction of energy and chemicals from the environment, for the growth of sequence and the mechanism for translating instructions into growth all had to be simultaneously present at that moment. This combination of events has seemed an incredibly unlikely happenstance and often divine intervention is prescribed as the only way it could have come about.[4]

Small wonder! In the real world, every effect must have an adequate cause, but the usual laws of science do not seem to intimidate evolutionists. In the strange land of evolutionary credullity, wonderful things may happen --- plans draw themselves, mechanisms design themselves, order generates itself from chaos, and life creates itself! Yet evolutionists call creationists unscientific because they postulate an adequate Cause (divine intervention) to account for the marvelous Effect called life.

In creation/evolution debates, creationists commonly place great emphasis on the Second Law of Thermodynamics as an overwhelming evidence against evolution. Although there have been approximately a hundred such debates held within the past four years, with leading evolutionist professors on major college and university campuses, the latter have never yet been able to come up with an answer of any consequence to this problem. Even more amazingly, most of them do not even seem to understand the problem, either dismissing it as irrelevant or else making some vacuous reference to ice crystals or open systems!

There are apparently only a few evolutionists who realize

the magnitude of the problem and have been trying to find a solution. Some of these attempts have been discussed in previous Impact articles.[5] [6]

By far the most important of these efforts, however, has been the suggestion of a Belgian scientist named Ilya Prigogine. Dr. Prigogine is a widely-known chemist and thermodynamicist, with faculty appointments both at the University Libre de Bruxelles and at the University of Texas at Austin. An indication of the strategic significance of Prigogine's ideas, is that they have recently won for him the Nobel Prize in Chemistry. Judging from the popular announcements, the main reason for this award was the ray of hope Prigogine has given evolutionists in their battle with entropy!

According to Newsweek, for example, the significance of Prigogine's work is as follows:

> Scientists who have sought to explain the origin of life as the result of chemical interactions have been confounded by the second law of thermodynamics: energy tends to dissipate and organized systems drift inevitably toward entropy, or chaos.... Prigogine's insights will give biologists new grounds for learning how the first random molecules organized themselves into life forms.... Progogine thinks the Nobel committee recognized that his work is building a bridge between the physical and human sciences.[7]

According to an interview in a professional chemical journal, Prigogine himself was "really surprised" at the decision of the Nobel committee. He also said: "The fact that the Nobel committee has chosen this one subject is a great encouragement."[8]

If, indeed, Prigogine had shown that the tremendous amount of information necessary for molecular self-replication can be produced naturalistically despite the entropy law, his achievement would be well worth the Nobel Prize. It would be all the more remarkable in view of the fact that Prigogine himself has "not actually worked in a chemistry lab for decades."[9] At best, however, he has only offered a theoretical speculation, not an experimental demonstration. It is hard to avoid the suspicion that the Nobel award in this case was due less to the scientific value of Prigogine's achievement than to the urgent need of the evolutionary establishment for some kind of answer, no matter how superficial, to the entropy problem.

Just how has Dr. Prigogine proposed to harmonize molecular evolution with the Second Law? Here it is, in his own words:

> In all these phenomena, a new ordering mechanism... appears. For reasons to be explained later, we shall refer to this principle as order through fluctuations. The structures are created by the continuous flow of energy and matter from the outside world; their maintenance requires a critical distance from equilibrium, that is, a minimum level of dissipation. For all these reasons we have called them dissipative structures.[10]

These "dissipative structures" are supposed to exhibit a higher degree of structure, or order, than they possessed before being subjected to a large influx of outside energy, while at the same time their generation is accompanied by a large dissipation of energy in the form of heat. The main example cited by Prigogine is the formation of convection currents and vortices in a fluid subjected to a temperature gradient.

Under such conditions, vortices (or other fluctuations or instabilities) may be generated and maintained. These, supposedly, mainfest higher "order" than the system possessed previously, even though such order has been produced at the cost of excessive over-all energy dissipation. This phenomenon has long been familiar to hydrodynamicists but Prigogine suggested that it may also apply in certain chemical and biological reactions which are proceeding under non-equilibrium conditions.

That such vortices or any other analagous "dissipative structures" could actually be called a device for naturalistic generation of higher order, and then that such a description could be awarded a Nobel Prize is almost unbelievable! This writer's own Ph.D. dissertation over a quarter of a century ago described in quantitative and analytical form the generation of turbulent vortices in fluid flow over rough surfaces.[11] These, indeed, are dissipative structures, requiring the dissipation of much flow energy in the form of heat for their generation. Their own rotational energies in turn are soon dissipated by breaking up into smaller vortices, so that no permanent increase in order is produced, even if such vortices are assumed (very questionably) to possess a higher degree of order than the energy gradient which generated them. "Big whirls make little whirls that feed on their velocity; little whirls make tiny whirls, and so on to viscosity!"

In any case dissipative structures could hardly serve as a substrate for still higher order, since they themselves require an abnormally large input of energy just to maintain their own structures. Prigogine himself says that, as far as chemical or biological reactions are concerned, the generation of dissipative structures is apparently limited to "auto-catalytic" processes. But catalytic processes, like fluid vortices, do not generate higher order--they merely speed up reactions which themselves are already going downhill thermodynamically in the first place. And any imaginary "auto-catalytic" processes would certainly require already-living systems for their own generation, so they can hardly explain the generation of living systems!

Although Prigogine wistfully expresses the hope that his speculations may someday lead to an understanding of how life may have evolved from non-life, he is at least more cautious than those of his fellow evolutionists who are currently exhuberating over it. He warns:

> It would be too simple to say that the concepts of life and dissipative structures are intermingled... But it is not just one instability that makes it possible to cross the threshold between life and non-life; it is, rather, a succession of in-

> stabilities of which we are only now beginning to identify certain stages.[12]

In a later section, he again suggests caution:

> But let us have no illustions. If today we look into the situation where the analogy with the life sciences is the most striking --even if we discovered within biological systems some operations distant from the state of equilibrium--our research would still leave us quite unable to grasp the extreme complexity of the simplest of organisms.[13]

One thing is clear. Whatever of scientific value may be deduced from Prigogine's analysis, he has not solved the problem of harmonizing entropy with evolution and he has certainly not shown that life can evolve from non-living chemicals. His dissipative structures do not constitute either the required program or the required mechanism to enable any kind of permanently increased order to be produced in an open system. However, he should perhaps be commended for trying. Maybe next he can work on a perpetual motion machine!

The problem of the origin of life can really only be resolved by recognition of the omnipotent Creator. The only alternative to belief in special creation is credulous faith in impotent Chance.

> We are faced with the idea that genesis was a statistically unlikely event. We are also faced with the certainty that it occurred. Was there a temporary repeal of the second law that permitted a "fortuitous concourse of atoms?" If so, study of the Repealer and Genesis is a subject properly left to theologians. Or we may hold with the more traditional scientific attitude that the origin of life is beclouded merely because we don't know enough about the composition of the atmosphere and other conditions on the earth many eons ago.[14]

Yes, not knowing how life could be formed would indeed becloud the understanding of the origin of life! The problem is why this should be called the scientific attitude when all the scientific evidence continues to support special creation.

References

[1]Victor F. Weisskopf, "The Frontiers and Limits of Science," American Scientist. Vol 65, July-August 1977, p.409.

[2]Henry M. Morris, The Scientific Case for Creation (San Diego: Creation-Life, 1977) pp.

[3]Freeman Dyson, "Honoring Dirac," Science, Vol. 185, Sept. 27, 1974, p.1161. Dyson is at Princeton's Institute for Advanced Study.

[4]Stanley W. Angrist and Loren G. Hepler, Order and Chaos,

(New York: Basic Books, Inc., 1967), pp.203-203.

[5]Henry M. Morris, "Entropy and Open Systems" (ICR Impact Series No. 40) Acts and Facts, October 1976.

[6]Jerry R. Gergman, "Albert Szent-Gyorgyi's Theory of Syntropy and Creationism" (ICR Impact Series No. 54), Acts and Facts, December, 1977.

[7]"Chemistry: The Flow of Life" (Newsweek; October 24, 1977) October 17, 1977, p.87.

[8]Chemical and Engineering News; October 17, 1977, p.4.

[9]Newsweek, Ibid.

[10]Ilya Prigogine, Gregoire Nicolis and Agnes Babloyants, "Thermodynamics of Evolution," Physics Today (Vol. 25, November 1972), p.25.

[11]Henry M. Morris, A New Concept of Flow in Rough Conduits (Minneapolis, University of Minnesota, 1951, 157 pp.)

[12]Ilya Prigogine, "Can Thermodynamics Explain Biological Order?" Impact of Science on Society, Vol. XXIII, No.3, 1973, p.169.

[13]Ibid, p.178.

[14]Angrist and Hepler, op. cit, p.205.

THERMODYNAMICS OF EVOLUTION

Prigogine, Ilya, et al; *Physics Today,* 25:23-28, November 1972.

Coherent behavior dissipation and life. Although it is customary to associate biological order with the formation of low-entropy ordered structures such as macromolecules, membranes and cells as a whole, this is only part of the problem. Equally important is functional order, which in actual living cells is ensured by a great number of coupled biochemical pathways. Biologists of development have always been puzzled that these metabolic processes are associated with a great amount of energy dissipation. As an example (A. Zotin, R.S. Zotina, J. Theor, Biol. 17:57, (1967) the rate of heat production in chicken eggs, which, roughly speaking, should furnish a measure of the entrophy production, is about 6 kcal/day/gm at the fourth day of development. At the 16th day, it drops to about 1 kcal/day/gm. These rather high figures raise the question of how living systems have acquired the ability to dissipate intensely. One of our main points here shall be that an increase in dissipation is possible

for nonlinear systems driven far from equilibrium. Such systems may be subject to a succession of unstable transitions that lead to spatial order and to increasing entropy production. (p. 24)

• Game Theory and Evolution

AN OPTIMAL STRATEGY OF EVOLUTION

Slobodkin, Lawrence B., and Rapoport, Anantol; *Quarterly Review of Biology,* 49:181-200, 1974.

Abstract. Admissable game-theory models of evolution must be restricted to the class of "existential games" in which there is no way of using the winnings ("payoff") for any purpose other than continuing the game for as long as possible. The optimal strategy in such a game is to minimize the stakes played. Organisms and populations seem to be organized consistently with this strategy. The evolution of this organization does not involve group selection or any other deviation from accepted evolutionary theory.

The implication of analyzing evolution as an existential game is to deny the idea of evolutionary momentum and to emphasize that the adaptedness must always be defined in an explicit environmental context.

It is asserted that adaptive responses will have a particular temporal relation to environmental perturbations. This assertion is a predictive statement about evolution--unlike the retrospective assertions that usually emerge from evolutionary analyses.

• Is Evolution a Tautology?

EVOLUTION A TAUTOLOGY

Macbeth, Norman; *Darwin Retried,* Gambit, Inc., Boston, 1971, pp. 47-48.

If we say that evolution is accomplished largely by natural

selection and that natural selection consists of differential reproduction, what have we done? Differential reproduction means that some species multiply by leaving more offspring than one-for-one, while others leave one-for-one and remain stable, and others leave less than one-for-one and dwindle or die out. Thus we have as Question: Why do some multiply, while others remain stable, dwindle, or die out? To which is offered as Answer: Because some multiply, while others remain stable, dwindle, or die out. The two sides of the equation are the same. We have a tautology. The definition is meaningless.

I regard this as a major discovery, a sort of lethal gene in the body of the central Darwinian doctrine; but I am not the first discoverer. It was formulated at least as early as 1959 by Professor C.H. Waddington of Edinburgh, a reputable member of the synthetic school, although his discovery seems to have had no impact on the biological fraternity. Waddington's statement is so staggering that it must be set forth in full:

> Darwin's major contribution was, of course, the suggestion that evolution can be explained by the natural selection of random variations. Natural selection, which was at first considered as though it were a hypothesis that was in need of experimental or observational confirmation, turns out on closer inspection to be a tautology, a statement of an inevitable although previously unrecognized relation. It states that the fittest individuals in a population (defined as those which leave most offspring) will leave most offspring. Once the statement is made, its truth is apparent. This fact in no way reduces the magnitude of Darwin's achievement, only after it was clearly formulated, could biologists realize the enormous power of the principle as a weapon of explanation.

Why do I find this staggering? Because a man who is astute enough to see that differential reproduction is a tautology is unable to see anything improper in a tautology. Because a man who reveres Darwin reduces Darwin's major contribution to a tautology, yet asserts that this does not reduce the magnitude of Darwin's achievement. Because a man who must know how weak natural selection is in explaining hard cases, and who has his finger on the reason for this weakness (the tautology), still speaks of the enormous power of natural selection as a "weapon of explanation." (pp.47-48)

TAUTOLOGY IN EVOLUTION AND ECOLOGY

Peters, Robert Henry; *American Naturalist,* 110:1-12, 1976.

Definition of the nature of scientific constructs can play as important a role in science as the original elaboration of an idea. Such clarification should eliminate confusion and lead to the con-

struct's proper application. Although Darwinian evolution remains one of the greatest unifying concepts in biology, its utility has been questioned. Difficulties in defining "fittest" in the phrase "survival of the fittest" have led some authors to conclude that the Darwinian theory is a meaningless formulation because fitness is apparently equivalent to survival (Waddington 1957; Coffin, cited in Scriven 1959). The testing of evolutionary theory against observation is, at best, extremely difficult (Slobodkin 1968; Orians 1973), and Scriven (1959) suggests that it is not to be expected. Birch and Ehrlich (1967) maintain that our theory of evolution is "not necessarily false" but "outside of empirical science."

Although each of these authors has found fault with the theory of evolution, none has indicated precisely where Darwinian theory may be applied and what may be expected from such an application. In this essay, I argue that the "theory of evolution" does not make predictions, so far as ecology is concerned, but is instead a logical formula which can be used only to classify empiricisms and to show the relationships which such a classification implies. Similar criticisms are then made of several ecological concepts. The essence of the argument is that these "theories" are actually tautologies and, as such, cannot make empirically testable predictions. They are not scientific theories at all. (p. 1)

• Homology and Evolution

REDUCTIONISM V. ORGANICISM

Thorpe, William; *New Scientist,* 43:635-638, 1969.

Problem of Homology. A further point of general agreement seemed to be that the animal has an active "strategy" in seeking new environments, and that with this conclusion, due largely to Waddington himself, one of the difficulties of the older more orthodox genetic approach is fading away. But there are one or two problems which still seem formidable. The first is that of homology. Geneticists tend nowadays to dismiss as unimportant, or else ignore altogether, the concept of homology which most zoologists, in any event until recently, regarded as absolutely fundamental to the idea of evolution. Sir Alister Hardy in his recent book The Living Stream (Collins, 1965) says "in the 1930s it all seemed so obvious, the same homologous structures must clearly be due to the same hereditary factors handed on genera-

tion after generation from the early ancestor with occasional changes by mutation, the wide variety of form seen in different animal groups being due to natural selection acting upon these factors or genes which were handed on, with mutational changes, from the original ancestral form."

This same view of homology was championed by J.B.S. Haldane in 1932, by Sir Gavin de Beer in 1938, and I suggest in much more recent writing by zoologists. It seems, however, that recent developments in genetical theory can no longer envisage the fairly static gene-pool or "gene-cluster" which is maintained intact over long periods of evolutionary time and is responsible for the slowly changing development of organs such as the wings, limbs and mouth parts of insects, or the fore and hind limbs of mammals and birds through all their marvellous structural adaptations of which we have fossil and recent evidence. Now it seems that these constant systems may exist only in our imagination and the genetic control of the development of such homologous organs may shift relatively rapidly while the organ remains the same! This seems to me to raise a quite fantastic difficulty, and if the orthodox genetical argument leaves us with this riddle, it becomes increasingly hard to my mind, to see how it is that life has progressed beyond its simplest forms--since increase in complexity or each major change in an organ makes life that much more precarious. And coming to a group I happen to know particularly well--I find the fantastic development of the beautiful and complex patterning of male birds very hard to account for as merely part of the isolation mechanisms which keep one species separate from another. They seem to have gone too far and developed too big a momentum of their own to be explicable on that basis (see John Thoday, Chance and Purpose in Theoria to Theory, vol. 2, p. 29, and together with this Sir Alister Hardy's reply, Theoria to Theory, vol. 2, p. 312).

Not being a geneticist myself I might feel more hesitation in putting forward difficulties of this kind had it not been for the apparently extraordinary state of flux in which genetics finds itself at the present moment. As we see from the discussions, geneticists are presently living in hazardous times--not least because the definition of so many of their basic concepts is becoming more and more difficult. Let me remind you of Waddington's words "I think we are going to see extraordinary changes in our ideas about evolution quite soon." And I also draw your attention to the extraordinary genetical happenings which seem to be involved, and to take place very rapidly, in the production of what is called "an established strain". Also in this connection, I would refer to the remarkable conclusions (quoted by Waddington) concerning the work of Forbes Robertson in his laboratory. (p. 637)

• The Possibility of Life beyond the Earth

THE "CAT-FLAP" EFFECT

Michel, Aime; *FSR,* 25:3-5, September/October 1979.

At the end of last June (1979) there was held in Las Vegas the first world congress of engineers to discuss the problem of interstellar galactic propulsion.

Convened by the most eminent American engineering association, this congress had as its objective the examination of all the possible means for converting into reality something that, only fifteen or twenty years ago, still seemed to pertain to the realms of science fiction: namely travel from star to star (ie. from sun to sun) across the Galaxy. I have no knowledge as yet of the papers that were read there, but there were expected to be several hundreds of them. It may well be that at that conference there was far more talk of the difficulties than of the means for achieving so formidable an enterprise. Nevertheless, it remains a fact that, if we date the birth of modern science from the discovery of the quanta (Max Planck, 1900), then it has taken only 79 years from that date for engineers to be considering that the time has come to examine the means available for effecting this Great Voyage --- the prelude to the Diaspora of mankind (terrestrial) throughout the Galaxy.

* * * * *

On the other hand a number of astronomers and physicists (Stephen Dole and G. K. O'Neill for example) have calculated --- and have done so quite separately --- that, within the framework of only those means that are available to present-day science, a minimum of one million years, and a maximum of ten million years, would elapse between the first interstellar voyage and to total occupation of the Galaxy.

In other words, (a) we are, at this moment, already studying the means for leaving our solar system and, (b) even if we admit that this Great Departure can only become possible in a few centuries from now, the entire Galaxy will be occupied by mankind (terrestrial) --- or, rather, by its descendants --- in from one million to ten million years from now.

* * * * *

It is from this point onwards that I now request the reader's close attention, for I am about to introduce the Cat Flap Effect which, I think, will alter radically all our previous speculations about "extraterrestrial" life.

Up till now, the big preoccupation of the astronomers dedicated to the Search for Extraterrestrial Intelligences (S.E.T.I.) has

been to find the most plausible solution for Drake's Equation, as set out, for example, in 1973 by Sagan in his article "On the Detectivity of Advanced Galactic Civilizations" in Icarus No. 19, p. 350:-

> N being the number of galactic "civilizations" capable of communicating with us, this equation is written as
>
> $$N = RL$$

where L is the presumed lifetime of a civilization, and R is the product of six highly speculative factors of probability, such as the "probable" number of stars having a planetary system; the "probable" number of planets "suitable for life"; the "probable" number of those planets where life has appeared; the "probable" number of those planets that have given birth to an intelligent species, etc., etc.

All these "probabilities," about which we know nothing whatsoever, are highly uncertain and so naturally they are --- like every probability --- inferior to 1. The product of them can therefore naturally only tend towards zero.

We see thus that, despite our immense desire to know, S.E.T.I. is a well-nigh desperate piece of research. In fact, all the conferences and all the publications on this theme have only ended in a mass of contradictory and unverifiable suppositions, and you can see convincing proof of this merely by reading through the bibliography.

But the two facts noted above, namely that (a) we men, after less than a hundred years of advanced science, are already studying the means of setting out for the stars, and that (b) a few millions of years will suffice for us to occupy the whole Galaxy, these two facts render Drake's Equation totally useless and totally obsolete.

For it is in fact useless for us to know (even very vaguely) how many stars have "suitable" planets, how many of them have produced life, etc. (see what we have said above), inasmuch as, if only one single planet in the whole of our Galaxy were more advanced than ours is, this would suffice for some portion of the Galaxy, or indeed for the whole of it, to be occupied already.

The Cat-Flap, is the technology of the first inter-stellar voyage: the first civilization to cross the Cat-Flap point explodes, literally, into Galactic Space, and occupies it totally, within a negligible period of time (i.e. between one and ten million years) compared to the cosmological durations of time which are registered not in millions of years, but in billions.

* * * *

The numerous and uncertain questions raised by Drake's Equation are thus replaced by one single, solitary question, to wit: has anyone else yet crossed the Cat-Flap?

This question, which at first sight looks as uncertain as the R factors in Drake's Equation, can (and should) be put in a far more striking fashion, namely as follows: What is the Probability that we are the most evolved beings in the Galaxy?

For, once the question has been put in these terms, the reply given by astrophysics is inclined towards the view that the probability of such a situation is virtually nil.

In fact there are tens of billions of stars of the same type as the Sun, and the majority of them are older than the Sun -- older than the Sun by billions (not millions) of years.

Consequently it is virtually infinitely improbable that we could be the first species to reach the Cat-Flap threshold. Indeed, on the contrary, it is infinitely probable that the Cat-Flap was passed by somebody else billions of years ago and that, during the whole of the vast period of time that has elapsed since then, the Galaxy in its entirety has been occupied.

* * * * *

If we accept this (and let us remember that the only alternative is that we are the most evolved of all beings in Space) then we must at once face up to certain consequences which totally alter the whole of our perspectives, historical, philosophical, and indeed religious too.

We would, for example, have to face up to the fact that "somebody" (or "something") was already around when the Earth came into existence; that this "somebody" was present (but, indeed, were "they" merely present?) throughout the whole evolution of life on Earth, the appearance of man, and the whole of his history.

And furthermore we must face up to yet another consequence: how are we going to be able to imagine for ourselves what that "somebody" or "something" is like, when 90% of all that we know at this very moment in 1979 was unknown to our own grandparents less than one century ago?

It is not surprising that the astrophysicists have detected nothing in Space resembling any sort of human activity such as we would define as "intelligent." What would indeed be surprising would be for the contrary to be the case. What, pray, is our own "intelligence" likely to be in a few billions of years from now, if men of my own generation now no longer recognise the world that they knew in the days of their own childhood?

. . . . .

Compiler's Comment: This charming article, translated from the French by Gordon Creighton, does not come to grips with the fact that the probability of life existing on any of the billions of other fertile planets in the cosmos may in fact be zero. It is fashionable to believe otherwise---perhaps because terrestrial life is so rich in variety and quantity and the number of extraterrestrial opportunities is so large. Nevertheless, we may in truth be alone in the universe. If we are not, Michel's analysis seems valid.

INDEX

William R. Corliss is a freelance writer and the author of 20 full-length books on space technology, energy, man-machine systems, and scientific mysteries. Under contracts from NASA, the National Science Foundation, the Energy Research and Development Administration and other organizations, he has also written over 50 booklets and articles on various facets of science and technology; including astronomy, computers, microelectronics, automation, and communication systems. In addition to this Handbook, Corliss has compiled ten closely related sourcebooks and four other handbooks of scientific anomalies in the fields of geophysics, geology, archeology, astronomy, biology, and psychology.

Corliss received his undergraduate education in physics at Rensselaer Polytechnic Institute and obtained the M.S. degree in physics from the University of Colorado in 1953. After nine years of work in industry, he began freelance writing in 1963. The Sourcebook Project was formed in 1974—the same year that the first sourcebook, *Strange Phenomena*, appeared. Corliss lives in Glen Arm, Maryland, with his wife and four children.

John C. Holden is a freelance illustrator, artist, and consulting scientist. He has conducted research and published extensively on various problems of earth tectonics. His scientific illustrations reflect his professional past as a geophysicist, oceanographer, and exploration geologist for several U.S. research agencies and various mining, coal, and oil companies.

The Sourcebook Project publishes several books dealing with the frontiers of science. For further information write:

The Sourcebook Project
Glen Arm, Maryland 21057