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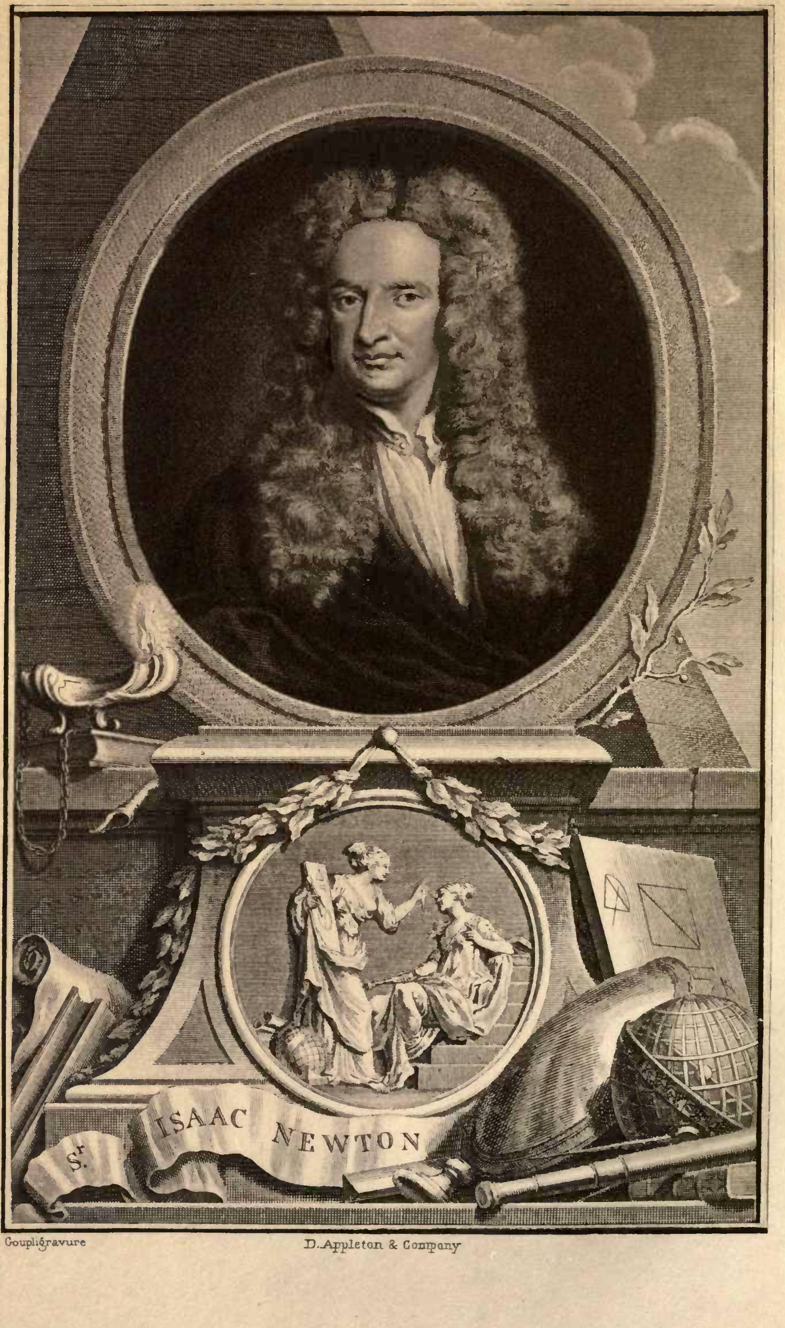

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Essays in Astronomy

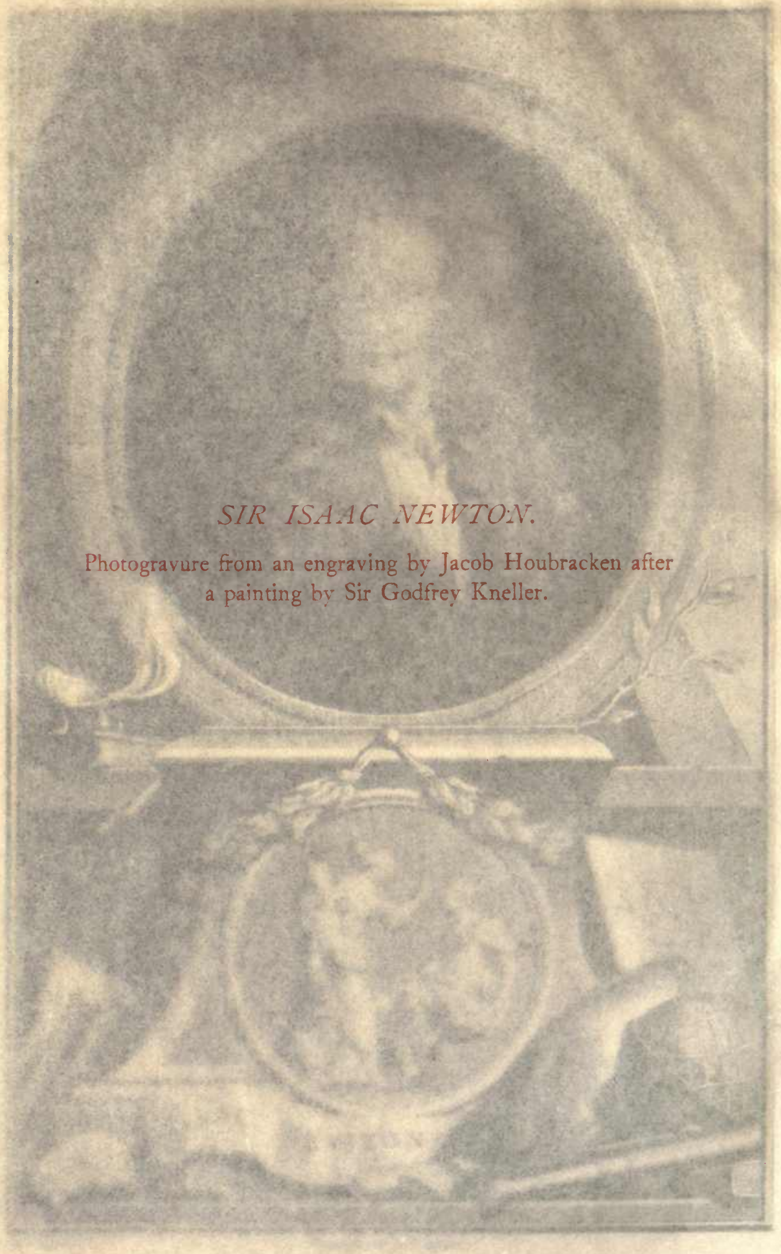
By

Ball, Harkness, Herschel, Huggins,
Laplace, Mitchel, Proctor,
Schiaparelli, and Others

With a Critical Introduction by
Edward Singleton Holden

Illustrated

New York
D. Appleton and Company
1900



SIR ISAAC NEWTON.

Photogravure from an engraving by Jacob Houbracken after
a painting by Sir Godfrey Kneller.

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ESSAYS ON ASTRONOMY

IT is the design of the series of which this volume is a part to give in complete form the greatest masterpieces in every department of literature, and in science, so far as practicable. In natural science the object may be attained by presenting, in their entirety, the World's Great Books. The case is not the same for the more exact sciences. There is no single book on astronomy, for example, that displays the amazing achievements of the past century in untechnical form. On the other hand, there are many scattered addresses and memoirs that present a single aspect of the advance in an authoritative and, at the same time, in a popular manner. A volume might now be written that would set forth the present state of scientific opinion on vital astronomical questions and exhibit the history of the successive steps by which our actual vantage-ground has been reached. A single volume of the sort would have the great advantage of symmetrical presentation. The whole of astronomy might be viewed from our present standpoint, and its different parts displayed in their just historical proportions. But there is no such book.

It has seemed that the special ends in view could be attained by extracting from the writings of the astronomers of the past and present generations—mostly from those of acknowledged masters of science—their own accounts of great discoveries made by themselves, or their luminous reviews of the discoveries of others, their colleagues. No account of the nebular hypothesis of Laplace from the hand of another could give the vital essence of his great generalization so vividly and succinctly as his own note, first printed in 1796 and subsequently amended in its details until it took its final shape. Here we have the matured reflections of a great philosopher precisely in the form in which they were marshalled in his own mind. His problem is to account for the present state of the solar system—how it came to be that which it is. We are bound to preserve the substance of his thoughts. Can they be better presented than in his very words? A paraphrase of Laplace's original would be as inappropriate as a prose version of one of Milton's immortal sonnets. The present volume gives Laplace's note in full.

Again, it would not be difficult to write out afresh a satisfactory account of the achievements of the spectroscope in determining the motions and the physical constitutions of the sun, the stars, the comets, and the nebulæ. It was a novel and little understood instrument when Dr. Huggins (now Sir William Huggins) first devoted it to researches on the heavenly bodies. In 1866, in the first flush of successful investigation, Dr. Huggins paused to make a report on the extraordinary results that he had obtained from the application of spectrum analysis to the heavens. Again, in 1891, a quarter of a century later, the veteran observer presented to the British Association for the Advancement of Science, at Cardiff, an elaborate account of the achievements of spectroscopists up to that time. Finally, in 1897, in a paper on the "New Astronomy," Sir William takes a retrospective view of the whole subject. All these papers from the same skilled hand are printed in this volume. Taken together, they exhibit, as no other writings can, the history of the immense advances that have been made in this novel department of astronomical research. The earlier paper sets forth the first results, and shows what was even then, a generation ago, to be looked for in the future. The later papers review the progress that has actually been made, and indicate the results that are safely ours, and the further advances that may be confidently expected. Here we have what we are seeking presented to us by a master hand, and we become, in some sense, witnesses of the processes of research, and appreciate the historical relations of its different parts.

It is to be noted that all the papers reprinted in this volume are given in their original forms and in their entirety (except those of the present writer, which have been condensed, and except that a single paragraph of Sir John Herschel's has been added to supplement an address that he delivered in 1841, which is here printed in full). Substance and form are both scrupulously preserved.

The successive chapters in this book have the great advantage, then, of setting forth the state of the questions of which they treat exactly as they were conceived at the time of writing by those best qualified to report upon them. In general, they accurately represent the present state of opinion, or, at least, they are suited to lead up to it, and to exhibit the historic development of the science.

This series of chapters is by no means addressed to professional astronomers, although it collects into a single volume many papers that astronomers will be glad to possess. On the contrary, the selections have been made especially to meet the wants of the general reader, and to furnish him with the best judgment on those questions of astronomy that appeal to us all: What is to be the future of our system? By what processes of evolution has it come to be what it is? How long may we expect the sun's heat to endure? What is the actual physical condition of the planets? Are they suited to maintain human life? Along with these fundamental questions others, less far-reaching, but not less interesting, are treated.

The dimensions of the solar system are discussed in an address delivered before the American Association for the Advancement of Science in 1894 by its president, Prof. William Harkness, Director of the United States Naval Observatory at Washington. The results of the great mathematical astronomers—Lagrange, Laplace, and others—upon the question of the mechanical stability of our system are set forth in a chapter by Mitchel, founder of the Cincinnati Observatory. The lectures and addresses of this remarkable man had the greatest influence in creating the universal and intelligent interest in astronomy that is so distinctive a note in our country. His lectures were attended by thousands, and are still remembered. The Cincinnati Observatory was built and equipped by voluntary contributions from his hearers, and it is largely to the impetus given by him that we owe the establishment of so many college and university observatories in the twenty years preceding our civil war. The chapter here reproduced from his addresses is an excellent example of his eloquent manner of presenting subjects difficult in themselves and foreign to ordinary thinking.

The papers upon "Atoms and Sunbeams," by Sir Robert Stawell Ball, formerly Astronomer-Royal for Ireland, and now Professor of Astronomy in the University of Cambridge, England, and upon the "Age of the Sun's Heat," by Sir William Thomson, now Lord Kelvin, professor in the University of Glasgow, present two aspects in the study of the central ruler of our system. In the latter chapter we have the reasons for Lord Kelvin's conclusions upon the past and future duration of the sun, and of the solar system and of our earth. The sun can not have existed in the past more than fifteen million or twenty million years, nor can its heat support life on the globe more than ten million years in the future.

The immense periods of past time called for by the geologist, and especially by the biologist, to account for observed changes in strata and in the species of animals, are thus denied to them by the mathematical astronomer. The history of the earth must be compressed into a comparatively few millions of years. On the other hand, the future of our planet, so far as it depends upon the solar heat, is strictly limited. There is no escape from the general conclusions of Lord Kelvin's paper. The number of hundreds of thousands of years during which the sun will furnish heat to the earth and to its populations can not be fixed with exactness. But it appears to be certain that our system is not self-sustaining, and that its life is not to be indefinitely long. It is an organism that, like every other organism known to us, must come to an end in consequence of the very laws that now keep it active. It must have had a beginning within a period that can not be fixed with certainty, but which can not, in any event, much exceed twenty million years; and it must end, so far as life is concerned, in a chaos of cold and dead globes at a calculable time in the future, when the sun has radiated away its store of heat; unless that heat is recreated by the operation of forces now totally unknown to us.

At the annual meeting of the ancient *Accademia dei Lincei* in Rome (Galileo's Academy), in 1889, in the presence of the King and Queen of Italy, the famous astronomer of Milan—Schiaparelli—was called upon to report on his remarkable conclusions regarding the rotation and the physical constitution of the planet Mercury. His paper is printed as a chapter in this volume. In it he announced that the planet Mercury revolved about the sun, turning always one face to it, as the moon always turns one face to the earth. In the one case as in the other, the efficient cause has been the action of tides, which operate to reduce the rate of axial rotation. Working always in one direction —to retard and never to accelerate—the tides may, in time, retard the motion of rotation of a planet (as they have the motion of our own satellite) until it presents a single face to its central body. This face of Mercury alone receives solar light and heat; the other is condemned to perpetual darkness. The conditions of life (if so be that life exists) upon such a planet are as different as possible from those that we experience upon our own earth; and Schiaparelli has lucidly expounded the consequences of such conditions as exist on Mercury.

As early as 1882 M. Schiaparelli confided to one of his correspondents his discovery in the verses that follow:

> "Cynthiæ ad exemplum versus Cyllenius axe
> Æternum noctem sustinet, atque diem:
> Altera perpetuo facies comburitur æstu,
> Abdita pars tenebris altera Sole caret."

A little later he announced, after a comparison between observations made at Milan, Washington, and Brussels, that the planet Venus was in like case. Venus, too, revolves like our moon, turning once on its axis during one revolution about its central body (in 225 days for Venus, 88 days for Mercury, and 29 days for the moon).

The observations of Mr. Percival Lowell, in Arizona, seem to confirm Schiaparelli's conclusions. There is nothing in the observations made at the Lick Observatory to throw doubt upon them. The question is very difficult. If the results of M. Schiaparelli are finally conclusively established, we shall have an entirely novel set of circumstances in the family of planets. A mathematical investigation of the effects of tidal retardation in the case of the moon shows that certain effects are possible, and that the rotation of a satellite to the earth (the moon), or to the sun (a planet), may go through strange alterations during its life history. Not only may the period of rotation be changed, but the distance from the central body may vary enormously. Our moon now turns a single face to the earth. Schiaparelli interprets his observations as showing that two out of the eight planets of our system are likewise in this stage of evolution. These and other planets will in the future pass through very various circumstances.

A chapter is also devoted to the presentation of Professor Schiaparelli's conclusions as to the planet Mars. The views of Schiaparelli, who is an observer of the highest competence, and a skilled mathematician and physicist, must not be confounded with the reports of his work that have been made from time to time by sanguine popular writers who have decided *a priori* that the Universe was created to be peopled by human beings, and who have distorted Schiaparelli's guarded and cautious words into definite pronouncements that Mars, at least, is inhabited by a race of high intelligence. Nor does the mass of writing in newspapers and ephemeral publications upon the question of life in Mars deserve any scientific consideration whatever. Weight of a certain kind it has. There is no doubt that thousands of readers get, and must get, their acquaintance with scientific conclusions from newspaper paragraphs compiled at third and fourth hand by persons without accurate and sufficient technical knowledge. Many such persons are now fully satisfied that somehow and somewhere, some one, probably Schiaparelli, has decided in a scientific manner, first, that Mars is a planet very much like our own earth, with a sufficient atmosphere, abundant water, and all the conditions for habitability; second, that the "canals" on the planet are without doubt the work of human hands, and a convincing proof of intelligent engineering; and, third, that we are now on the point of communicating by some kind of electric signalling with our cosmic brothers. An ardent desire that all this should be true is taken as convincing proof that it has veritably been proved.

The judgment of the majority of competent astronomers on these points is very different. They know how very far we still are from a determination of the question whether Mars, or any other planet, is, in fact, inhabited. They know that this question, universally interesting as it is, by no means represents the present inquiry of science, which is: Are any of the planets habitable? Are they fit to be inhabited? Even this limited inquiry is still so far from an answer that it is almost idle to speculate on the larger question. Unless it can be proved—as it certainly has not been—that Mars is a planet where conditions prevail that are favourable to human existence, it is a simple waste of effort to inquire whether such life is now in existence. No one can say with any kind of scientific certainty whether a human being transported to Mars could live even for an instant. It is more than probable that he could not.

On account of the intense interest that such questions excite, a few remarks are here devoted to a review of the case as it stands. In the first place, we may safely say that Mars has little or no atmosphere, basing this conclusion, at the outset, upon its capacity of reflecting the solar light that falls upon its surface, as compared with the like capacity of other bodies of the solar system. The moon reflects $\frac{17}{100}$ of the light falling upon it—about as much as sandstone rocks. Mercury reflects $\frac{13}{100}$. These bodies have little or no atmosphere. Venus reflects (from the outer surface of its envelope of clouds) $\frac{50}{100}$ of the incident light. Jupiter ($\frac{62}{100}$), Saturn ($\frac{52}{100}$), Uranus ($\frac{64}{100}$), Neptune ($\frac{46}{100}$), are all surrounded by extensive atmospheres and all have high reflecting powers. The corresponding number for Mars ($\frac{26}{100}$) is so small as to indicate that this planet has little atmosphere, if any. Again, the planet's surface has been under careful scrutiny for many years, and observers are all but unanimous in their report that no clouds are visible over the surface. The centre of the disks of bodies with extensive atmospheres (the sun, Jupiter, Saturn, etc.) is always brighter than the edges. The centre of the moon, which has no atmosphere, is not so bright as the edge. Mars is like the moon in this respect, and not like Jupiter. Finally, the only satisfactory spectroscopic observations of the planet (made independently at the Lick Observatory and at the Allegheny Observatory) show no evidence whatever of an atmosphere to Mars and no sign of water-vapour about the planet. If there is any atmosphere at all, it can hardly be more dense than the earth's atmosphere at the high summits of the Himalaya Mountains—not enough to support human life, therefore. As there is no evidence of the presence of water-vapour and of clouds, etc., it follows that there is little or no water on the planet's surface. The spectrum of Mars and the spectrum of the moon are identical in every respect. This could not be true if Mars had any considerable atmosphere.

The important and long-continued observations of Schiaparelli on Mars led him to announce that the planet was provided with an elaborate system of water-courses (" oceans, seas, lakes, canals, etc."), and the authority of this distinguished observer is the chief support of those who maintain that the planet is fit for human habitation.

Complete explanations of all the phenomena presented by the planet can not be given in the light of our present knowledge. This is not to be wondered at, in spite of the industry and ability of the observers who have spent years in studying its surface. The case is much the same for the planets Mercury, Venus, Jupiter, Saturn, Uranus, Neptune. We know very little of the real conditions that prevail on their surfaces. We know comparatively little of the interior of the earth on which we live, and next to nothing about the interior of other planets. There is every reason to believe that complete explanations will be forthcoming in time. It is, at any rate, certain that the conclusions of Schiaparelli, named above, can not be accepted without serious modification. In the telescope the main body of the planet is reddish, and there are many permanent dark markings of a grayish-blue colour. The polar caps are sometimes dazzlingly white.

When Sir William Herschel was examining Mars in the eighteenth century he called the red areas of Mars "land" and the greenish and bluish areas "water." It was a general opinion in his day that all the planets were created to be useful to man. Astronomers of the eighteenth century set out with this belief very much as the philosophers of Ptolemy's time set out with the fundamental theorem that the earth was the centre of the motions of the planets. For example, Herschel maintained that the sun was cool and habitable underneath its envelope of fire. He says (1795), "The sun appears to be nothing else than a very eminent, large, and lucid planet, most probably also inhabited by beings whose organs are adapted to the peculiar circumstances of that vast globe." It is certain that the sun is not inhabited by any beings with organs. This conclusion is now as obvious as that no beings inhabit the carbons of an electric street lamp. Herschel's guess that the red areas on Mars were land and the blue areas water had no more foundation than his guess that the sun might be inhabited.

The next careful studies of Mars were made by Maedler about 1840. He also called the red areas of the disk land and the dark areas water. In this he followed Herschel. There was no reason why he should not have called the red areas water and the dark areas land. He had absolutely no evidence on the point. The same is true of later observers down to the first observations of Schiaparelli about 1877. Schiaparelli gave reasons for these names, though his reasons are not convincing. He pointed out that the narrow dark streaks (" canals ") generally ended in large dark areas (" oceans ") or in smaller dark areas (" lakes "). The narrow, dark streaks (very seldom less than sixty miles wide) are quite straight. They can not be rivers, then. If they are water at all, the name " canal " is not inappropriate, though sixty or one hundred miles is a very wide canal. If they are water, then the large dark areas must be seas. The narrow, dark streaks are not water, however, because it was discovered by Dr. Schaeberle at the Lick Observatory that the so-called " seas " sometimes had so-called " canals " crossing them. A sea traversed by a canal is an absurdity. It is maintained by a few recent observers of Mars that some of the dark areas are water, and some are not so. The bluish-green colour of the dark spots is said to " suggest vegetation." But who can know what colours the vegetation on Mars may have?

The foregoing very brief abstract of a long history proves that the dark areas on Mars are not water. The red areas are not known to be land. The spectroscopic and other evidence proves that Mars has little or no atmosphere —little or no water-vapour—no clouds. It is not yet known what is the real nature of the red areas and of the dark areas. It is one of the many unsolved problems of astronomy to discover the answer to this fundamental question. There is no doubt that the red areas and the large dark areas have a real existence, since some of the markings on Mars have been seen for more than two centuries.

It is not certain that all the " canals " that have been mapped really exist. Some of them are probably mere optical illusions. If they were real streaks on the planet's surface (like wide fissures, broad water-courses, etc.) they would always appear broadest when they were at the centre of the disk and would always be narrower when they were at the edges. The laws of perspective demand this. It is found by observation that the reverse is frequently true.

The distance of Mars from the sun is one and a half times the earth's distance. The heat received by the earth from the sun is to the heat received by Mars as $(1.5)^2 = 2.25$ to 1. Mars receives less than half as much sun heat as the earth. If the earth had no more atmosphere than the moon, the earth's temperature would be like that of the moon. If the earth had no denser atmosphere than that on the summits of the Himalayas, the temperature of the earth would always be below zero. Human life could not exist here. The case is the same with Mars. The temperature of the whole surface of the planet must be extremely low, even in its equatorial regions. The temperature at the poles of Mars must be several hundred degrees (Fahrenheit) below zero when the pole is turned away from the sun, and below zero even when the pole is turned toward the sun.

Before going further it is worth while to consider the circumstances under which Mars is seen by an observer on the earth. The mean distance of the moon from the earth is 240,000 miles. If it is viewed through a field glass magnifying four times, it is virtually brought within 60,000 miles of the observer. The nearest approach of Mars to the earth is 35,000,000 miles. The planet can very seldom be viewed to advantage with a magnifying power so high as five hundred. If such a power is employed when Mars is nearest, the planet is virtually brought within 70,000 miles. It follows, therefore, that we never see Mars so advantageously even with the largest telescopes as we may see the moon in a common field glass. If the reader will examine the moon with a field glass magnifying four times, he will have a realizing sense of the best conditions under which it is possible to see Mars, and he will be surprised that so much is known of the planet. The industry and fidelity of observers can only be appreciated after such an experiment.

Observations upon the polar caps of Mars must be interpreted in the light of the foregoing facts—namely, that Mars has little or no water-vapour, and that its temperature is appallingly low. The main facts of observation are as follows: Cassini, the royal astronomer of France, discovered in 1666 that Mars sometimes had dazzling white circular patches near his poles. In 1783 Sir William Herschel observed these patches to wax and wane, and he called them "snow" caps, thus begging the question as to their real nature. Herschel's observations and those of all later observers show that these caps wax and wane with the Martian seasons. In the Martian polar summer they are smallest, or they even vanish; in the Martian polar winter they are largest. As Herschel began with the conviction that all planets were analogous to the earth and were meant to be inhabited, his conclusion was that the polar winter condensed water-vapour into snow, and that the polar summer melted this snow, and so on. A more scientific conclusion would have been that some vapour was condensed and subsequently dissipated by the solar heat. It is practically certain that the phenomena of the waxing and waning of the caps depend on solar heat.

If the caps are snow condensed from water-vapour, the layer of snow must be exceedingly thin, because when these caps are melted no clouds appear. When snow melts on the earth, clouds are formed and our atmosphere is charged with the vapour of water. No clouds are seen on Mars, and no water-vapour is to be found above its surface by any spectroscopic test.

The polar caps must be formed by the vapour of some other substance than water. It is worth while to inquire whether they may not be carbon dioxide in a solid state. This substance is a heavy gas (carbonic-acid gas) at ordinary temperatures. It would lie at the bottom of valleys and fill cañons or ravines. At a temperature of about one hundred Fahrenheit below zero it is a colourless liquid.

At temperatures such as must obtain at the pole of Mars turned away from the sun it becomes a snowlike solid. Caps of carbon dioxide would wax and wane at the poles of Mars under variations of solar heat such as obtain at these poles, very much as caps of snow and ice wax and wane in our arctic regions, which, under all circumstances, are at a higher temperature than the poles of Mars.

There is so far no observational proof that the polar caps of Mars are formed of carbon dioxide. There is convincing proof that they are not formed of water. The question as to the nature of the polar caps is still open.

The question of life in other worlds is vitally and profoundly interesting to all mankind, and it is discussed in several chapters of this volume. But the inquiries of science must at present be limited to a determination of more humble questions. Are the planets habitable? is the present problem; later, we may perhaps be able to attack the question whether they are, in fact, inhabited.

A chapter on "Sidereal Astronomy, Old and New," gives an account of researches that led to an inventory of the stellar universe, and of some of the main results attained, and serves as an introduction to two memorable addresses by Sir John Herschel on the occasion of awarding the gold medal of the Royal Astronomical Society to Bessel and to Francis Baily for their great catalogues of stars. The true significance of the work of the observing astronomer is not grasped until it is understood to how many and various uses the results of his patient and laborious watchings may be applied. These addresses of Herschel are, moreover, models of lucid and eloquent exposition.

Two papers on the "Mathematical Theories of the Earth," by Professor Robert Simpson Woodward, of Columbia University, and on the "Wanderings of the North Pole," by Sir Robert Ball, exhibit the astronomical relations of our planet in an unfamiliar light, and describe one of the capital discoveries of modern times—the variability of terrestrial latitudes. Incidentally, they serve to display the vitality of the "old" exact astronomy. The "new" astronomy, which avails itself of photographic and spectroscopic aids, appeals more directly to popular imagination, and its results can be presented to a popular audience through a series of striking photographic pictures, but it is helpless unless it employs the rigid methods of its elder sister science; and new triumphs in many fields await the votary of the old astronomy. The recent discovery of variable terrestrial latitudes is not the least of them.

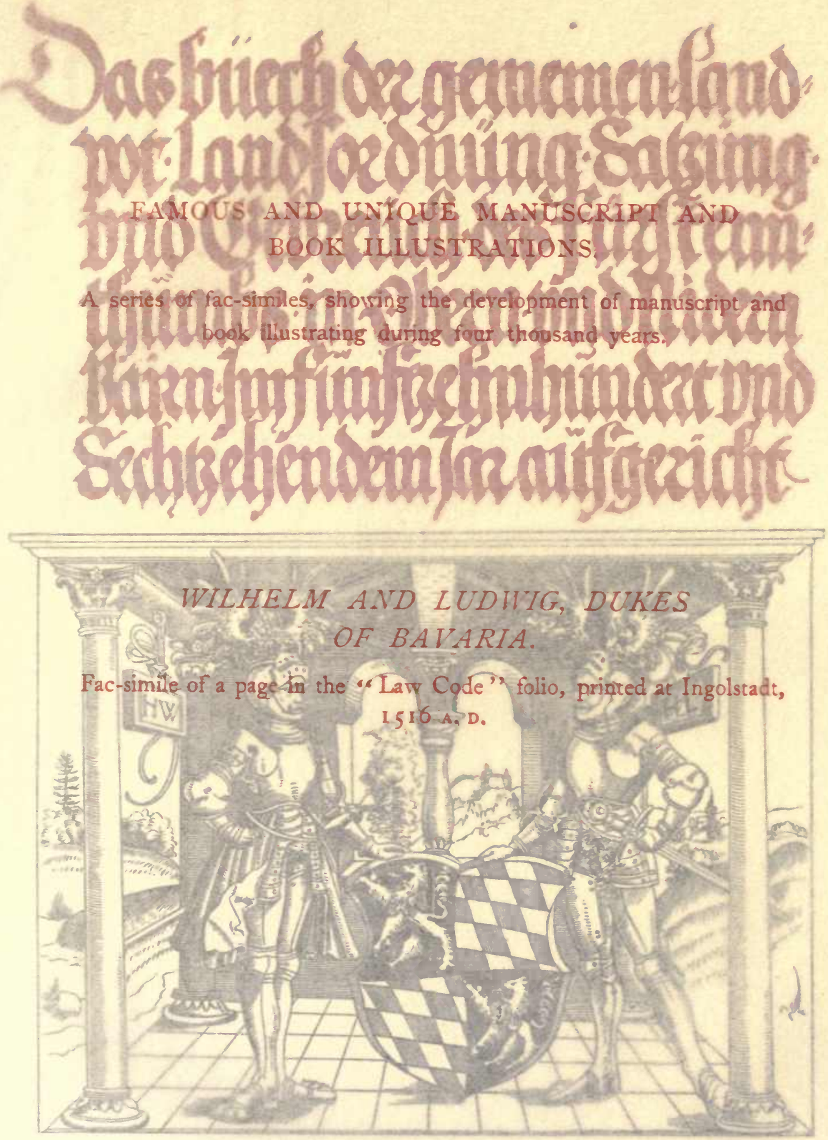
The papers on the "History of the Astronomical Telescope," by Professor Charles Hastings, of Yale University; on "An Astronomer's Life in a Modern Observatory," by Dr. David Gill, royal astronomer at the Cape of Good Hope; on "Photography as the Servant of Astronomy," and on the "Beginnings of American Astronomy," by the writer, present the history of astronomy in this country and elsewhere, and exhibit certain little-understood aspects of modern astronomical activity. These chapters of actualities will, it is believed, be especially welcome to readers who are familiar with the results of astronomical work, but who are not acquainted with its details.

Laplace's Nebular Hypothesis—a household word—was first proposed in 1796. He mentioned the suggestions of Buffon with respect. It is an interesting fact that Buffon possessed a copy of the "Principia" of Swedenborg, in which the Swedish philosopher had expounded (1734) a hypothesis to account for the genesis of the solar system. The cosmologies of Swedenborg, Buffon, and Laplace stand in a kind of historic relation to each other. The theory propounded by Laplace must, of course, be regarded as an independent and original generalization of a master mind. His obligations to his predecessors were vanishingly small.

The astronomical question that touches us most intimately and deeply is that of life in other worlds. We know our own life here in its thousand forms. We see the firmament studded with stars, each star a sun, and each sun presumably accompanied by its retinue of planets. Our own sun, a fairly insignificant star, has eight planets in its train, and one of them we know to swarm with life. How is it with the planets that accompany those millions of other suns? The instructed mind refuses to accept their shining as accessory to our little existence. Man was the king and centre of the universe. The telescope discrowned him. To the eye of reason he became a mere ephemeral being, living out a short and troubled life on the surface of one of the smaller bodies of a vast universe. And the universe itself is limited. We have seen that it is to end in darkness, as it began. The immensity of man's fall in dignity has often been insisted upon. It is sometimes forgotten that, after all, it was by a man that man was discrowned. If he is not the master of his own fate, at least he has foreseen it. Has he companions of like nature on the planets of his own system, or in the worlds that accompany each of the brilliant stars of a winter's night? The subject is of immense intellectual interest, and it is treated in two chapters of this volume: by the late Richard Proctor, on a "New Theory of Life in Other Worlds," and by the Reverend Father Searle, some time Professor of Astronomy in the Catholic University of America, on the "Habitability of the Planets."

A glance over the table of contents will show that the present volume contains essays on a great variety of subjects, written at first hand, mostly by the great astronomers. Place has been found for chapters on the sun, the planets, meteorites, stellar systems, and on our own earth; on the magnitude and stability of the solar system; on the nebular hypothesis; on sidereal astronomy; on photography and spectroscopy in their astronomical relations; on the history of American astronomy, and of the telescope in all lands; on the special aims and methods of the modern astronomer; and, finally, on the theory of life in other worlds than ours. Taken together, they present a vivid picture of the present state of scientific opinion, and they afford a historical perspective that is of high value.

EDWARD S. HOLDEN.



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pot · Landſordnüng · Satzüng ·
vnd Gebreüch · des Fürſten-
thümbs · in · Obern · vnd Nidern
Bairn · Im fünfftzehenhündert vnd
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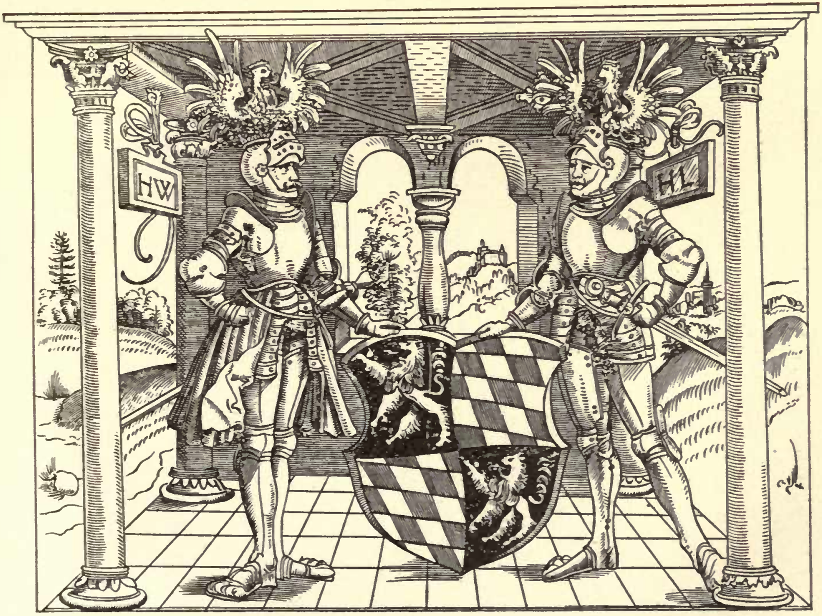

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AND

THE WANDERINGS OF THE NORTH POLE

BY

SIR ROBERT STAWELL BALL

ATOMS AND SUNBEAMS[1]

IN recent years an important change has taken place in the manner in which many physical problems are approached. The philosopher who now seeks an explanation of great natural phenomena not unfrequently finds much assistance from certain remarkable discoveries as to the ultimate constitution of matter. Many an obscure question in physics has been rendered clear when some of the properties of molecules have been brought to light. No doubt our knowledge of the natural history of the molecule is still vastly wanting in detail. It must, however, be admitted that we have traced an outline of that wonderful chapter in Nature which is specially serviceable in the question which I now propose to discuss.

The problem before us may be stated in the following terms: We have to illustrate how the sun is enabled to maintain its tremendous expenditure of light and heat without giving any signs of approaching exhaustion. It will be found that the atomic theory of the constitution of matter exhibits the mechanism of the process by which that capacity of the great luminary for supplying the radiation so vital to the welfare of mankind is sustained from age to age.

Let me here anticipate an objection which may not improbably be raised. Those who have paid attention to this subject are aware that the remarkable doctrine first propounded by Helmholtz removed all real doubt from the matter. It is to this eminent philosopher we owe an explanation of what at first seemed to be a paradox. He ex-

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[1] From the " Fortnightly Review," October, 1893.

plained how, notwithstanding that the sun radiates its heat so profusely, no indications of the inevitable decline of heat can be as yet discovered. If the sun had been made of solid coal from centre to surface, and if that coal had been burned for the purpose of sustaining the radiation, it can be demonstrated that a few thousand years of solar expenditure at the present rate would suffice to exhaust all the heat which the combustion of that great sphere of fuel could generate. We know, however, that the sun has been radiating heat, not alone for thousands of years, but for millions of years. The existence of fossil plants and animals would alone suffice to demonstrate this fact. We have thus to account for the extremely remarkable circumstance that our great luminary has radiated forth already a thousand times as much heat as could be generated by the combustion of a sphere of coal as big as the sun is at present, and yet, notwithstanding this expenditure in the past, physics declares that for millions of years to come the sun may continue to dispense light and heat to its attendant worlds with the same abundant prodigality. To have shown how the apparent paradox could be removed is one of the most notable achievements of the great German philosopher.

What Helmholtz did was to refer to the obvious fact that the expenditure of heat by radiation must necessarily lead to shrinkage of the solar volume. This shrinkage has the effect of abstracting from a store of potential energy in the sun and transforming what it takes into the active form of heat. The transformation advances pari passu with the radiation, so that the loss of heat arising from the radiation is restored by the newly produced heat derived from the latent reservoir. Such is an outline of the now famous doctrine universally accepted among physicists. It fulfils the conditions of the problem, and when tested by arithmetical calculation it is not found wanting.

But the genuine student of Nature loves to get to the heart of a great problem like this; he loves to be able to follow it, not through mere formulæ or abstract principles, but so as to be able to visualize its truth and feel its certainty. He will, therefore, often desire something in addition to the bare presentation of the theory as above stated. It may be no doubt sufficient for the mathematician to know that the total potential energy in the sun, due to the dispersed nature of its materials, is so vast that as contraction brings the materials, on the whole, somewhat nearer together, the potential energy thus surrendered is transformed into a supply of heat quite adequate to compensate for the losses arising from the radiation by which the contraction was produced. The student who admits—and who is there that does not admit?—the doctrine of the conservation of energy knows that in this argument he is on thoroughly reliable ground. At the same time the argument does not actually offer any very clear conception, or indeed any conception at all, of the precise modus operandi by which, as the active potential energy vanishes, its equivalent in available heat appears. I have always felt that this was the unsatisfactory part of an otherwise perfect theory. It was, therefore, with much interest that I became acquainted a short time ago with a development of the molecular theory of gases which afforded precisely what seemed wanted to make every link in the chain of the great argument distinctly perceptible. I make no doubt that the notions which have occurred to me on this subject must have presented themselves to others also. I have, however, not read in print or heard in conversation any use made of the illustration that I am going to set forth. I feel, therefore, confident that even if it be known at all, it is certainly not generally known among the large and ever-increasing circle of readers to whom the great questions of physics are of interest.

The division of matter into the three forms of solids, liquids, and gases has acquired in these days a special significance now that the constitution of matter is becoming in some degree understood. First let it be noted that, though matter is capable of subdivision to a certain extent, yet that there is a limit beyond which subdivision could not be carried. This statement touches upon the ancient controversy as to the infinite divisibility of matter. Even still we can find the statement in some of our old text-books that there is no particle of matter so small that it could not be again subdivided into half. No doubt, so far as most ordinary experience goes, this statement may be unquestionable. It is quite true that we do not often reduce matter to fragments so small that each of them shall be insusceptible of further conceivable division. But, to illustrate the natural principle now under consideration, let us take the example of a body which is itself composed of but a single element. Think, for instance, of a diamond, which is, as we all know, a portion of crystallized carbon. It is true that the reduction of diamonds to powder is a laborious process. Still, diamond dust has to be produced in the finishing of the rough stone, and this element will serve the purpose of our present argument better than a substance of a composite nature. Each particle of the diamond dust is, of course, as much a particle of carbon as was the original crystal. We may, however, suppose that by a repetition of the process a reduction of the diamond dust to powder still finer is accomplished. The grains thus obtained may have become so minute that they have ceased to be visible to the unaided eye, and require a microscope to render them perceptible; but even after this comminution each of these particles is still a veritable diamond. It possesses the properties, optical, chemical, and mechanical, of the original gem, from which it differs merely in the attribute of size. Even when the disintegration has been carried to such a point that each individual particle can be only just perceived by the keenest power of the most powerful microscope, there is still no indication that the particles cease to possess the characteristics of the original body. These facts being undoubted, it was perhaps not unnatural to suppose that the reduction could be carried on indefinitely, and that even if the smallest fragment of diamond which could be seen in a powerful microscope were reduced to a millionth part, and each of those to a million more, yet that the ultimate particles thus reached would be diamonds still. Now, however, we know that that is not the case. The smallest particle visible under a microscope might indeed be crushed into a thousand parts, and each one of those parts, though wholly inappreciable to our sense of touch or vision, would nevertheless be a genuine diamond. If, however, the subdivision be carried on until the particles produced are, roughly speaking, one-millionth part of the bulk of the smallest objects which could be seen in the microscope, we then approach the limits of partition of which the diamond would be susceptible. We now know that there is an atom of diamond so small that it must refuse to undergo any further division. This ultimate atom, be it observed, is not an infinitely small quantity. It has definite dimensions; it possesses a definite weight. All such diamond atoms are precisely alike in weight, and probably in other characteristics. It might be thought that if this atom has finite dimensions, it is, at all events, conceivable that it should admit of further subdivision. In a certain sense this is, no doubt, the case. The diamond atom is made up of parts and, being so made, it is, of course, conceivable that those parts could be separated. The important point to notice is, that no means known to us could produce this separation, while it is perfectly certain that if the decomposition of the atom of diamond into distinct parts could be effected, those parts would not be diamonds at all, nor anything in the least resembling diamonds.

What we have said as regards the element carbon may be extended to every other elementary substance. Sulphur is familiarly known in a form of extreme subdivision, and each little particle of sulphur could be further comminuted to a certain point beyond which any further partition would be impossible. So, too, any composite body—such, for example, as a lump of sugar—admits of being decomposed into molecules so small that any further separation would be impossible if the molecule were still to remain sugar. No doubt, a separation of the molecule of any composite body into constituent atoms of other elements is not alone possible, but is incessantly taking place.

The first step in our knowledge of the constitution of matter has been taken when we have come to recognise that every body is composed of a multitude of extremely, but not infinitely, small molecules. The next point relates to the condition in which these molecules are found. At first it might be thought that in a solid, at all events, the little particles must be clustered together in a compact mass. If we depended merely on sensible evidence it would seem that a lump of iron, if constituted from molecules at all, must be simply a cohering mass of particles, just as a multitude of particles of sand unite to form a lump of sandstone. But the truth is far more wonderful than such a belief would imply. Were the sensibility of our eyes so greatly increased as to make them a few million times more powerful than our present organs, then, indeed, the display of the texture of solid matter would be an astonishing revelation. It would be seen that the diamond atoms, which, when aggregated in sufficient myriads, form the perfect gem, were each in a condition of rapid movement of the most complex description; each molecule would be seen swinging to and fro with the utmost violence among the neighbouring molecules. It would be seen quivering all over under the influence of the shocks which it would receive from the vehement encounters with other molecules which occur millions of times in each second. Such would be the minute anatomy of the diamond. The well-known properties of such gems seem, at first sight, wholly at variance with the curious structure we have assigned to them. Surely, it may be said that the hardness and the impenetrability so characteristic of the diamond refute at once the supposition that it is no more than a cluster of rapidly moving particles. But the natural philosopher now knows that his explanation of the qualities of the diamond holds the field against all other explanations. The well-known impenetrability of the diamond seems to arise from the fact that when you try to press a steel point into the stone you fail to do so because the rapidly moving molecules of the gem batter the end of the steel point with such extraordinary vehemence that they refuse to allow it to penetrate or even to mark the crystallized surface. When you cut glass with a diamond it is quite true that the edge, which seems so intensely hard, is really composed of rapidly moving atoms. But the glass which is submitted to the operation is also merely a mass of moving molecules, and what seems to happen is, that, as the diamond is pressed forward, its several particles, by their superior vigour, drive the little particles of glass out of the way. We do not see the actual details of the myriad encounters in which the diamond atoms are victorious over the glassy molecules; we only discern the broad result that the diamond has done its work, and that the glass has been cut.

It may well be asked how we know that matter is constituted of molecules in intensely rapid movement. The statement seems at the first glance to be so utterly at variance with our ordinary experience that we demand, and rightly demand, some convincing proof on the matter. There are many arguments by which the required demonstration can be forthcoming. The one which I shall give is not perhaps the most conclusive, but it has the advantage of being one of the simplest and the most readily intelligible.

Let us see if we can not prove at once that the molecules in, let us say, a piece of iron must be in movement. Suppose that the iron is warmed so that it radiates heat to a perceptible extent. We know that the heat which, in this case, affects our nerves has been transmitted from its origin by ethereal undulations. Those undulations have, undoubtedly, been set in motion by the iron, and yet the parts of the metal seem quite motionless relatively to each other, notwithstanding that they possess the power of setting the ether into vibration. It is impossible that such vibrations could be produced were it not that there is in the iron a something which vibrates in such a manner as to communicate the necessary pulses to the ether. It therefore follows that in the texture of the solid iron there must be some molecular movement, timed in such a way as to impart to the ether the actual vibrations which we find it to possess. The argument in this case may be illustrated by the analogous phenomena presented in the case of sound. As we listen to the notes of a violin, what we actually perceive are vibrations communicated through the air to the auditory apparatus. We can trace these aërial vibrations back to their source, and we find they originate from the quivering of the violin under the influence of the bow of the performer. Were it not for these vibrations of the instrument the aërial vibrations would not be produced, and the corresponding sounds would not be heard. Far more delicate than the atmospheric waves of sound are the ethereal waves corresponding to light or to heat, but none the less must these latter also originate from the impulse of some vibrating mass. It is thus apparent that a hot piece of iron, however still it may seem, must be animated by an excessively rapid molecular movement. Nor is the validity of this conclusion impaired even if the iron be at ordinary temperature. We know that a body which is no hotter than the surrounding bodies is still incessantly radiating heat to them and receiving heat from them in return. Thus we are led to the conviction that a piece of iron, whatever be its temperature, must consist of atoms in a state of lively movement. The important conclusion thus drawn with regard to iron may be equally stated with respect to every other solid, or, indeed, every other body, whether solid, liquid, or gaseous. All matter of every description is not only known to be composed of molecules, but it is also now certain that those molecules are incessantly performing movements of a very complex type.

A closer study of this subject will be necessary for our present purpose, and it will be convenient to examine matter in that state in which it is exhibited in its very simplest type from the molecular point of view. This condition is not presented, as might at first be supposed, when the matter is solid, like a diamond, or like a piece of iron. Even in a liquid the complexity of molecular constitution, though somewhat less than in the case of a solid, is still notably greater than in matter which has the gaseous form. The air that we breathe is matter almost of the most simple kind, so far as molecular constitution is concerned. It should, however, be noted that, as air consists of a mixture, it would be better for our purpose to think of a gas isolated from any other element. Let us take the case of oxygen, the most important constituent of our atmosphere.

Like every other element, oxygen is composed of molecules, and those molecules are in a state of rapid motion. It might be expected that the affinity by which the different molecules were allied in the case of a gas should be of the simplest nature, and this is indeed found to be the case. Notwithstanding that oxygen is an invisible body, and notwithstanding that the molecules are so excessively minute as to be severally quite inappreciable to our senses, yet we have been able to learn a great deal with regard to the constitution of the molecules of this gas. The mental eye of the philosopher shows him that, though the oxygen with which a jar is filled appears to be perfectly quiescent, yet that quiescence has there no real existence. He knows that oxygen consists of myriads of molecules identical in weight and in other features, and darting about one among the other with velocities which vary perhaps between those of express trains and those of rifle bullets. He sees that each little molecule hurries along quite freely for a while until it happens to encounter some other molecule equally bent on its journey, and then a collision takes place. Perhaps it would be more correct to say that what usually happens is that the two impinging molecules make a very close approach; then each of them so vehemently attracts the other as to make it swerve out of its course and start it off along a path, inclined, it may be, even at a right angle to that which it previously pursued. The molecules in a gas at ordinary pressures are so contiguous that these encounters take place incessantly; in fact, we are able to show that each individual molecule will probably experience such adventures some millions of times in the course of each second. We are able to calculate the average velocity with which the several molecules move when the gas has a certain temperature. We know how to determine the average length of the free path which each molecule traverses in the interval between two consecutive encounters. We are able to trace how all these circumstances would vary if, instead of oxygen gas, we took nitrogen, or hydrogen, or any other body in the same molecular state. It is, in fact, characteristic of every gas that each molecule wanders freely, subject only to those incessant encounters with other similar wanderers by which its path is so frequently disturbed. If two gases be placed in the same vessel, one being laid over the other, it will presently be found that the two gases begin to blend; ere long one gas will have diffused uniformly through the latter, so that the two will have become a perfect mixture just as the oxygen and nitrogen have done in our own atmosphere. The molecular theory of gases explains at once the actual character of the operation by which diffusion is effected. Across the boundary which initially separates the two gases certain molecules are projected from either side, and this process of interchange goes on until the molecules become uniformly distributed throughout.

There is, indeed, nothing more remarkable than the fact that information so copious and so recondite can be obtained in a region which lies altogether beyond the direct testimony of the senses. Just as the astronomer staggers our powers of conception by the description of appalling distances and stupendous periods of time, and relies with confidence on the evidence which convinces him of the reality of his statements, so the physicist avails himself of a like potent method of research to study distances so minute and times so brief that the imagination utterly fails to realize them.

In the case of a liquid, the freedom enjoyed by the molecules is considerably more restricted than in the case of a gas. It would seem that in the denser fluid there can be no intervals of undisturbed travel permitted to a molecule; it is almost incessantly in a state of encounter with some other similar object. When a molecule in a liquid breaks away from its association with one group, it is only because it has entered into alliance with another. As, however, two liquids will very frequently blend if so placed that diffusion be possible, we have a proof that, though the transference of a particular molecule through the liquid may be comparatively slow, yet it will gradually exchange association with one group for association with another, and may in this way travel throughout any distance to which the liquid extends.

In the case of a solid there is still further limitation imposed on the mobility of each separate molecule. It is now no longer permitted to make excursions throughout the entire volume of the body. Each molecule is in rapid motion, it is true, but those movements are confined to gyrations within minutely circumscribed limits. Two solids placed in contact do not generally diffuse one into the other, the incapacity for diffusion being the direct consequence of the inferior degree of mobility possessed by the molecules in this condition of matter.

It is known that the immediate effect of the application of heat is to increase the velocities with which the molecules move. Apply heat, for instance, to the water in a kettle; the moving molecules of water are thereby stimulated to even greater activity, and it will occasionally happen that the velocity thus acquired by a molecule becomes so great that the little particle will swing clear away from the influence of the other molecules with which it had been associated. When this takes place in the case of a sufficient number of molecules, they dart freely from the surface of the liquid, thus producing the effect which in our ordinary language we describe as giving off steam. If, therefore, a volume of gas be heated, the velocities with which its molecules are animated will be in general increased. As the molecular velocities throughout the extent of the gas are, on the whole, augmented, it is quite plain that the intensities of the shocks experienced by the molecules in their several encounters will be also accentuated. The more rapidly moving particles will strike each against the other with increased violence, and the contemplation of this single fact leads us close to one of Nature's greatest secrets.

Let us think of the abounding heat which is dispensed to us from the sun. That heat comes, as we know, in the form of undulations imparted to the ether by the heated matter in the sun, and transmitted thence across space for the benefit of the earth and its inhabitants. I have already explained that these vibrations in the ether must take their rise from molecular movements, and it is important to notice that the character of the vibrations in the ether enable us to learn to some extent the precise description of molecular movements which alone would be competent to produce the particular vibrations corresponding to radiant heat. At first it might be thought that it was the rapid movements of translation of the molecules themselves, as entire if extremely minute bodies, which caused the ethereal vibration, but this is not so. We must carefully observe that there is another kind of molecular motion besides that which the molecule possesses as a whole. We have hitherto been occupied only with the movements of each molecule as a little projectile pursuing its zigzag course, each turn of the zigzag being the result of an encounter with some similar molecule belonging to the same medium. But we have now to observe that the molecule itself is by no means to be regarded as a simple rigid particle; indeed, if it were so, it is certain that we should receive no heat at all from the sun. We have the best reasons for believing that the molecule of matter, so far from resembling a simple rigid particle, is an elaborate structure, whose parts are in some degree capable of independent movement. It will not, indeed, be necessary for us to adopt the splendid hypothesis of Lord Kelvin, which supposes that molecules of matter are merely vortex rings in that perfect fluid, the ether. It seems difficult to doubt that this doctrine represents the facts, but if any one should reject it, then I have only to say that its assumption is not required

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for our present argument. All that is necessary for us is to regard each molecule as somewhat resembling an elastic structure made of parts which can quiver like springs, and so arranged as to be susceptible of many different modes of vibration. We are to suppose that each molecule, in addition to the energy which it possesses in virtue of its movement of translation as a whole, has also a store of energy corresponding to the oscillations of its electric springs. We can, in fact, in some cases determine the ratio which exists between the amount of energy which is, on the average, possessed by molecules in consequence of their velocities of translation, and the amount of energy which they possess in consequence of the vibrations by which their several parts are animated. It is these internal molecular vibrations which are of essential importance in our present inquiry. It is believed that the radiation of light, or of heat, generally takes rise in the impulses given to ether by the internal molecular vibrations. Do we not know that the essential characteristic of those ethereal movements which correspond to radiant light and heat is that they have the nature of oscillations? Such could not be imparted by mere rectilinear movements of the molecules as a whole. They must be due to those internal oscillations by which the actual molecules are animated.

No doubt it is difficult to realize that much can be learned with regard to the performances that actually go on in the internal parts of a molecule, especially when it is remembered that each molecule in its entirety is so extremely minute as to be entirely beyond the reach of our organs of sense. It is, nevertheless, impossible to doubt that the statements just made correspond to the veritable facts of Nature. It would be impracticable here to go into any complete detail with regard to the evidence on this subject; I can only sketch an outline of it. Let us take, perhaps as the simplest case, that presented by hydrogen.

At the ordinary temperature of the air hydrogen is, of course, invisible; this means that the vibrations in the interior of the molecules are not sufficiently vehement to im-

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part pulses to the ether with the energy that would be required to produce visual effects. Now, let us suppose that the hydrogen is heated. The effect of heating is to impart additional speed to the molecules of the gas, and consequently when the molecules happen to come together their encounter is more violent. The effect of such an occurrence on one of these little elastic bodies is to set it quivering with greater vehemence in those particular modes of vibration for which it is tuned. If the temperature of the gas has been raised sufficiently high, as it can be by the aid of electricity, then the internal energy acquired by the molecules, in consequence of the increased vehemence of their collisions, has become so great that they are able to impart pulses to the ether with sufficient intensity to affect our nerves of vision; thereupon we declare that the hydrogen is now so hot as to have become luminous. Suppose we employ a spectroscope for the purpose of studying the particular character of the light which the glowing hydrogen dispenses. It will appear that the spectrum consists of a definite number of bright lines. We know that each one of these lines corresponds to a particular period of vibration of the ether, and hence we see that the light emitted by the hydrogen does not consist of vibrations of all periods indiscriminately, but only of certain particular waves which are in unison with the oscillations to which the internal parts of the molecule of hydrogen are adapted. Had we examined the spectrum of some other gas in a state of incandescence we should have found a wholly different system of lines from those pertaining to hydrogen. This demonstrates that the molecules of one gas differ essentially from those of another in respect to the character of the internal vibrations which they are adapted to perform. The extraordinary activity of the movements which take place within the molecules may be appreciated from the following facts: We know that the wave corresponding to one of the hydrogen lines has a length of about the forty-thousandth of an inch; we also know that in a single second of time light travels over a space of a hundred and eighty-six thousand miles; a simple calculation will, therefore, assure us that certain vibrations in the molecules of hydrogen corresponding to this particular undulation must take place with such an extraordinary frequency that about four hundred and sixty millions of millions of them are performed in each second of time.

Provided with these conceptions, we shall now, I think, be able to see without difficulty how it is that the sun's heat is sustained. We may, for our present purpose, think of the great luminary as a mass of glowing gas. It is quite true that the physical condition of the matter in the interior of the tremendous globe can hardly be that which we ordinarily consider as gaseous. But this need not affect our argument. It is undoubtedly true that those portions of the solar atmosphere from which the light and heat are mainly dispensed are gaseous in their character, or, at all events, come sufficiently near to matter in the gaseous state to permit the application of the line of argument with which we have hitherto been engaged. In consequence of the vast mass of the sun the gravitation with which it draws all bodies toward it is very much greater than the gravitation on the surface of the earth. On our globe we know that the effect of gravitation is to impart to any body near the surface velocity directed toward the earth's centre at the rate of thirty-two feet per second. The sun is more than three hundred thousand times as massive as the earth; we can not, however, assert that the gravitation is increased in the same proportion, because, on account of the vast size of the sun, a particle at its surface is more than a hundred times farther away from the solar centre than a body on the surface of the earth is from the terrestrial centre. It can, however, be shown that, taking these various matters into account, the actual intensity of gravitation at the solar surface is sufficient to tend to impart to all objects an increase of velocity toward the sun's centre at the rate of four hundred and fifty-seven feet per second. This would apply not only to a meteorite, or other considerable mass, which is falling into the sun; it would be equally true of an object as small as a molecule. Every one of the myriads of gaseous molecules in the outer regions of the solar atmosphere must be constantly acted upon by this attractive force, which tends in the course of each second to add to them a downward velocity at that rate per second which has already been stated. It is quite true that to a great extent the effect of this attraction is masked by counteracting tendencies. In particular we may mention that, inasmuch as the density of the solar atmosphere increases as the sun's centre is approached, the flying molecule generally finds itself more obstructed by encounters with other molecules when it is descending than when it is ascending. We may here contrast the condition of the atmosphere on the earth with the condition of the solar atmosphere. Each molecule in our air, being acted upon by terrestrial gravitation, has thereby a tendency to fall downward with a velocity continually increasing at the rate of thirty-two feet per second. As, however, the terrestrial atmosphere has long since reached a stable condition, in which it undergoes no further contraction, the effect of gravitation in adding velocity to the molecules is so completely masked by the counteracting tendencies that, on the whole, there is no continual increase of molecular velocities downward due to gravitation. Were such an increase at present going on, we should necessarily find that the terrestrial atmosphere was decreasing in volume, and ever becoming more condensed in its lower strata. It is, however, well known that no such changes as are here implied are taking place. The essential difference between the earth and the sun, so far as the matter now before us is concerned, is to be found in the fact that, as the sun has not yet passed into the form of a rigid body, it is still contracting at a rate very much greater than that at which a body grown so cold as the earth draws its particles closer together. The molecules in the solar photosphere accordingly yield to a certain extent to the gravitation which constantly seeks to draw them down. The counteracting tendencies can not in the sun, as they do in the earth, mask the direct and obvious effect of gravitation. The consequence is that the intense attraction which is capable of adding velocity to the molecules at the phenomenal rate of four hundred and fifty-seven feet per second is permitted to accomplish something, and thus increase the average speeds with which the molecules hurry along. To express the matter a little more accurately, we should say that the downward velocity imparted by gravitation, being compounded with the velocities otherwise possessed by the molecules, tends, on the whole, to increase the rate at which they move.

We shall now be able to discern what actually takes place as the sun contracts by dispersing heat, and in consequence of its decline in bulk finds a store of energy liberated which it is permitted to use for the purpose of sustaining its radiating capacity. Owing to the intense heat which prevails in the photosphere, the molecules must there be in very rapid movement; their mutual encounters must be of the utmost vehemence, and their internal vibrations, which are the consequences of the shocks in the encounters, must be correspondingly energetic. It is, as we have seen, these internal molecular vibrations which set the ether in motion, and thus dispense solar heat and light far and wide through the universe. But this the molecules can only do at the expense of the energy which they possess in virtue of their internal vibrations. Unless, therefore, the internal molecular energy were to be in some way recuperated from time to time, the radiating power must necessarily flag. It is now plain that the necessary recuperation takes place in the successive encounters. A molecule whose internal energy of vibration is becoming exhausted by the effort of setting the ether into vibration presently impinges against some other molecule, and in consequence of the blow is again set into active vibration which permits it to carry on the work of radiation anew, until its declining energies have again to be sustained by some similar addition arising from a fresh collision. Of course, we know that the internal molecular energy thus acquired can not be created out of nothing. If the molecule receives such accessions of internal energy, it must be at the expense of the energy which is elsewhere. Obviously the only possible source of such energy must be found in the movement of the molecule as a whole—that is to say, in the velocity of translation with which it rushes about among the other molecules. Thus we see that the immediate effect of expenditure of heat or light by radiation is to diminish the internal energies of the molecules. These energies are restored by the transference of energy obtained from the general velocities of the molecules regarded as moving projectiles. It follows that the velocities of the several particles must on the whole tend to decline; in other words, that the temperature tends to fall. What we have to discover is the agent which at present prevents the solar temperature from falling. We want, therefore, to ascertain the means by which the molecular velocities are preserved at the same average value, notwithstanding that there is a constant tendency for these velocities to abate in consequence of the losses of light and heat by radiation. We have already explained how the gravitation of the sun constantly tends to impart additional downward velocity to the molecules in its atmosphere. This is precisely the action which we now require. The contraction of the sun tends to an augmentation of the molecular velocities, and this augmentation just goes to supply the loss of velocities which is the consequence of the radiation. A complete explanation of the maintenance of the sun's heat is thus afforded. Observation, no doubt, seems to show that the capacity for radiation is at present sensibly constant, and this being so, we see that the gain of molecular velocities from gravitation and their losses from radiation are at present just adapted to neutralize each other. Nothing, however, that has as yet been said demonstrates that the efficiency of the sun for radiating light and heat must always be preserved exactly at its present value.

It is quite possible that if we had the means of studying the sun's heat for a hundred thousand years, we might find that the capacity for radiation was slightly decreasing, or, it may be, that it would be slightly increasing, for it is at least conceivable that the gain of molecular velocity due to gravitation may, on the whole, exceed the loss due to the dispersal of energy by radiation. On the other hand, it is, of course, possible that the acquisition of velocity by gravitation, though nearly sufficient to countervail the expenditure by radiation, may not be quite enough, in which case the sun's temperature would be slowly declining.

It must not, however, be supposed that the argument which we have been here following attributes eternal vigour to the great luminary. It will be noted that it is of the essence of the argument that the contraction is still in progress. If the contraction were to cease, then the restitution of velocity by gravitation would cease also, and the speedy dispersal of the existing heat by radiation would presently produce bankruptcy in the supply of sunbeams. Indeed, such bankruptcy must arrive in due time, when, after certain millions of years, the sun has so far contracted that it ceases to be a gaseous mass. The vast accumulated store of energy which is now being drawn upon, to supply the current radiation, will then yield such supplies no longer. Once this state has been reached, a few thousand years more must witness the extinction of the sun altogether as a source of light, and the great orb, at present our splendid luminary, will then pass over into the ranks of the innumerable host of bodies which were once suns, but are now suns no longer.

THE WANDERINGS OF THE NORTH POLE[1]

ON a recent visit to Cambridge, Professor Barnard, the discoverer of the fifth satellite of Jupiter, exhibited at the Cavendish Laboratory his most interesting collection of photographs made at the Lick Observatory. These pictures were obtained by a six-inch photographic lens of three feet focus, attached to an ordinary equatorial, the telescope of which was used as a guider when it was desired to obtain a picture of the stars with a long exposure. Among the advantages of this process may be reckoned the large field that is thereby obtained, many of the plates that he exhibited being as much as four degrees on the edge. I am, however, not now going to speak of Barnard's marvellous views of the Milky Way, nor of the plate on which a comet was discovered, nor of the vicissitudes of Holmes's comet, nor of that wonderful picture in which Swift's comet actually appears to be producing, by a process of gemmation, an offshoot which is evidently adapted for an independent cometary existence. The picture to which I wish specially to refer in connection with our immediate subject is one in which the instrument was directed toward the celestial pole. In this particular case the clockwork which is ordinarily employed to keep the stars acting at the same point of the plate was dispensed with. The telescope, in fact, remained fixed while the heavens rotated in obedience to the diurnal motion. Under these circumstances each star, as minute after minute passed by, produced an image on a different part of the

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[1] From the "Fortnightly Review," August, 1893.

plate; the consequence of which was that the record which the star was found to have left, when the picture was developed, was that of a long trail instead of a sharply defined point. As each star appears to describe a circle in the sky around the pole, and as, in the vicinity of the pole, these circles were small enough to be included in the plate, this polar photograph exhibits a striking spectacle. It displayed a large number of concentric circles, or rather, I should say, of portions of circles, for the exposures having lasted for about four hours, about one sixth of each circumference was completed during that time. The effect thus produced was that of a number of circular arcs of varying sizes, and of different degrees of brightness. Most conspicuous among them was the trail produced by the actual Pole Star itself. It is well known, of course, that though the situation of the pole is conveniently marked by the fortunate circumstance that a bright star happened during the present century to lie in the immediate vicinity of the veritable pole, yet, of course, this star is not actually at the pole, and consequently, like all the other stars, Polaris itself must be revolving in a circle whereof the centre lies at the true pole. The brighter the star the brighter is the trail which it produces, so that the circle made by Polaris is much more conspicuous than the circles produced by the other stars of inferior lustre. It is, however, to be noted that some of the faint stars lie much closer to the pole than Polaris itself. There is, indeed, one very minute object so close to the pole that the circle in which its movements are performed seems very little more than a point when represented on the screen on which the slide was projected. The interesting circumstance was noted that there appeared to be occasional interruptions to the continuity of the circular arcs. This was due to the fact that clouds had interposed during the intervals represented by the interruptions. A practical application is thus suggested, which has been made to render useful service at Harvard College Observatory. Every night, and all night long, a plate is there exposed to this particular part of the sky, and the degree

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in which the Pole Star leaves a more or less complete trail affords an indication of the clearness or cloudiness of the sky throughout the course of the night. From the positions of the parts where the trail has been interrupted it is possible not only to learn the amount of cloudiness that has prevailed, but the particular hours during which it has lasted. This interesting system of concentric polar circles affords us perhaps the most striking visual representation that could possibly be obtained of the existence of that point in the heavens which we know as the pole. The picture thus exhibited was a striking illustration of the Copernican doctrine that the diurnal stellar movement was indeed only apparent, being, of course, due to the rotation of the earth on its axis.

Suppose that a photograph, like that I have been describing, were to be taken at intervals of a century, it would be found that the centre of the system of circles—that is to say, the veritable pole itself—was gradually changing on the heavens. I do not by this mean that the stars themselves would be found to have shifted their places relatively to each other. No doubt there is some effect of this kind, but it is an insignificant one, and need not at present concern us. The essential point to be noticed is, that the stars which happen to lie in the vicinity of the pole would have a changed relation to the pole in consequence of the fact that this latter point is itself in incessant movement. At the present time the pole is advancing in such a direction that it is getting nearer to the Pole Star, so that the actual circle which the Pole Star is describing is becoming less and less. The time will come when the circle which this star performs will have reached its lowest dimensions, but still the pole will be moving on its way, and then, of course, the dimensions of the circle traversed by the Pole Star will undergo a corresponding increase. As hundreds of years and thousands of years roll by the pole will retreat farther and farther from the Pole Star, so that in the course of a period as far on in the future as the foundation of Rome was in the past, the pole will be no longer sufficiently near the Pole Star to enable the latter to render to astronomers the peculiar services which it does at present.

Looking still further ahead, we find that in the course of about twelve thousand years the pole will have gained a position as remote as it possibly can from that position which it now occupies. This most critical point in the heavens will then lie not far from the star Vega, the brightest point in the northern sky, and then it will commence to return, so that after the lapse of about twenty-five thousand years the pole will be found again in the same celestial neighbourhood where it is to-night, having, in the meantime, traversed a mighty circle through the constellations. In all this there is no novelty; these movements of the pole are so conspicuous that they were detected long before the introduction of accurate instruments. They were discovered so far back as the time of Hipparchus, and the cause of them was assigned by Newton as one of the triumphs of his doctrine of universal gravitation. In giving the title of "The Wanderings of the North Pole" to this paper I did not, however, intend to discourse of the movements to which I have hitherto referred. They are so familiar that every astronomer has to attend to them practically in the reduction of almost every observation of the place of a celestial body. It was, however, necessary to make the reference which I have done to this subject in order that the argument on which we are presently to enter should be made sufficiently clear. It must be noted that the expression "the north pole" is ambiguous. It may mean either of two things, which are quite distinct. In the case we have already spoken of, I understand by the north pole that point on the celestial sphere which is the centre of the system of concentric circles described by the circumpolar stars. The other sense in which the north pole is used is the terrestrial one; it denotes that point on this earth which has been the goal of so many expeditions, and to reach which has been the ambition of so many illustrious navigators. We have a general notion that the terrestrial north pole lies in a desolate region of eternal ice, somewhat relieved by the circumstance that for six months of the year the frozen prospect is brightened by perpetual day, though, on the other hand, during the remaining six months of the year this region is the abode of perpetual night. The north pole is that hitherto unattainable point on our globe on which, if an observer could take his station, he would find that the phenomena of the rising and the setting of the stars, so familiar elsewhere, were non-existent. Each star viewed from the coign of vantage offered by the north pole would move round and round in a horizontal circle; and the system of concentric circles would be directly overhead. In midsummer the sun would seem to revolve around, remaining practically at the same elevation above the horizon for a few days, until it slowly began to wend its way downward in a spiral. In a couple of months it would draw near the horizon, and as day after day passed by the luminary would descend lower and lower until its edge grazed the horizon all round. The setting of the sun for the long winter would then be about to commence, and gradually less and less of the disk would remain perceptible. Finally, the sun would disappear altogether, though for many days afterward a twilight glow would travel round the whole hemisphere, ever getting less and less, until at last all indications of the sun had vanished. The utter darkness of winter would then ensue for months, mitigated only so far as celestial luminaries were concerned by starlight or occasional moonlight. Doubtless, however, the fitful gleams of the aurora would often suffice to render the surrounding desolation visible. Then as the spring drew near, if, indeed, such a word as spring be at all applicable to an abode of utter dreariness, a faint twilight would be just discernible. The region thus illuminated would move round and round the horizon each twenty-four hours, gradually becoming more and more conspicuous, until at last the edge of the sun appeared. Then, by a spiral movement inverse to that with which its descent was accomplished, the great luminary would steal above the horizon, there to continue for a period of six months until the beginning of the ensuing winter. Indeed, the actual duration of apparent summer would be somewhat protracted in consequence of the effect of refraction in raising the sun visually above the horizon when in reality it was still below. The result would be to lengthen the summer at one end and to anticipate it at the other. Such would be the astronomical conditions at the north pole; that anomalous point, from whence every other locality on the globe lies due south, that mysterious point which up to the present seems never to have been approached by man within a distance less than four hundred miles, unless, indeed, as is not improbably the case, the preglacial man who lived in the last genial period found a temperate climate, and enjoyable conditions even at the latitude of 90°.

For our present purpose it will be necessary to get a very clear idea as to the precise point on the earth which we mean when we speak of the north pole. As our knowledge of it is almost entirely derived from astronomical phenomena it is necessary to assign the exact locality of the pole by a strict definition depending on astronomical facts. Supposing that Nansen does succeed in his expedition, as every one hopes that he will, and does penetrate within that circle of four hundred miles' radius where the foot of civilized man has never yet trod, how is he to identify that particular spot on this globe which is to be defined by the north pole? It was for this purpose that at the beginning of this paper I referred to that photograph of the concentric circles which illustrated so forcibly the position of the pole in the heavens. Imagine that your eye was placed at the centre of the earth, and that you had a long, slender tube from that centre to the surface through which you could look out at the celestial sphere; if that tube be placed in such a way that, when looking from the centre of the earth through this tube your vision was directed exactly to that particular point of the heavens which is the centre of the circle now described by the Pole Star and the other circumpolar stars, then that spot in which the end of the tube passes out through the surface of the earth is the north pole. Imagine a stake to be driven into the earth at the place named, then the position of that stake is the critical spot on our globe which has been the object of so much scientific investigation and of so much maritime enterprise. The reader must not think that I am attempting to be hyperaccurate in this definition of the north pole; no doubt, in our ordinary language we often think of the pole as something synonymous with the polar regions, an ill-defined and most vaguely known wilderness of ice. For scientific purposes it is, however, essential to understand that the pole is a very definitely marked point, and we must assign its position accurately, not merely to within miles, but even to within feet. Indeed, it is a truly extraordinary circumstance that, considering no one, with the possible exception just referred to, has ever yet been within so many hundreds of miles of the pole, we should be able to locate it so precisely that we are absolutely certain of its position to within an area not larger than that covered by a small town, or even by a good-sized drawing-room.

We have seen that the north pole in the sky is in incessant movement, and that the travels which it accomplishes in the course of many centuries extend over a wide sweep of the heavens; this naturally suggests the question, Does the pole in the earth move about in any similar manner, and if so, what are the nature and extent of its variation? Here is the point about which those modern researches have been made which it is my special object to discuss in this paper. Let us first see clearly the issue that is raised. At the time of the building of the Pyramids the pole in the heavens was in quite a different place from its present position; the Pole Star had not at that time the slightest title to be called a pole star; in fact, the point around which the heavens revolved lay in a wholly different constellation. It was certainly not far from the star Alpha Draconis about 3000 B. C., and we could indicate its position quite definitely if we had any exact knowledge as to the date of the Pyramids' erection. It is, however, plain that the difference was so patent between the celestial pole at the time of the Pyramids and the celestial pole of later centuries, that it could not be overlooked in attentive observation of the heavens. As the north pole in the sky was, therefore, so different in the time of the Pharaohs from the north pole in the time of Victoria, it is proper to ask whether there was a like difference, or any difference at all, between the terrestrial pole at the time of the building of the Pyramids and that terrestrial pole in whose quest Nansen is just setting off. If Pharaoh had despatched a successful expedition to the north pole and driven a post in there to mark it, and if Nansen were now successful, would he find that the north pole in the earth which he was to mark occupied the same position or a different position from that which had been discovered thousands of years previously? At first one might hastily say that there must be such a difference, for it will be remembered that I have defined the north pole in the earth as that point through which the tube passes which would permit an eye placed at the centre of the earth to view the north pole in the sky. If, therefore, the north pole in the sky had undergone a great change in its position, it might seem obvious that the tube from the earth's centre to its surface which would now conduct the vision from that centre to the north celestial pole would emerge at a different point of the earth's crust from that which it formerly occupied. We have here to deal with the case that arises not unfrequently in astronomy, in which a fact of broad general truth requires a minute degree of qualification; indeed, it is not too much to say that it is in this qualification of broad general truths that many of the greatest discoveries in physical science have consisted. And such is the case in the present instance. There is a broad general truth and there is the qualification of it. It is the qualification that constitutes the essential discovery which it is my object herein to set forth. But before doing so it will be necessary for me to lay down the broad general truth that the north pole of the earth as it existed in the time of the Pharaohs appears to be practically the same as the north pole of the earth now. It seems perfectly certain that at any time within the last ten thousand years the north pole might have been found within a region on the earth's surface not larger than Hyde Park. Indeed, the limits might be drawn much more closely. It is quite possible that many an edifice in London occupies an area sufficiently great to cover the holes that would be made by all the posts that might be driven to mark the precise sites of the north pole on the earth not only for the last five thousand or ten thousand years, but probably for periods much more ancient still. It is very likely that the north pole at the time of the Glacial epoch was practically indistinguishable from the north pole now; in fact, the constancy, or sensible constancy I should, perhaps, rather say, of the situation of this most critical point in our globe is one of the most astonishing facts in terrestrial physics.

Let us, then, assume this broad, general fact of the permanency in the position of the north pole, and deduce the obvious consequences it implies with regard to the earth's movement. At this point we find the convenience of the time-honoured illustration in our geography books which likens the earth to an orange. Let us thrust a knitting-needle through the orange along its shortest diameter to represent the axis about which the earth rotates. Not only does the earth perform one revolution about this axis in the space of each sidereal day, but the axis itself has a movement. If the earth's axis always remained fixed, or never had any motion except in a direction parallel to itself, then the point to which it was directed on the sky would never change. We have, however, seen that the pole in the sky is incessantly altering its position; we are therefore taught that the direction of the earth's axis of rotation is constantly changing. To simulate the movement by the orange and knitting-needle we must imagine the orange to rotate around its axis once in that period of twenty-three hours and fifty-six minutes, which is well known as the length of the sidereal day; while at the same time the knitting-needle, itself bearing, of course, the orange with it, performs a conical movement with such extreme slowness that not less than twenty-five thousand years is occupied in making the circuit. The movement, as has often been pointed out, is like that of a peg-top which rotates rapidly on its axis while at the same time the axis itself has a slow revolving motion. Thus the phenomena which are presented in the rotation of the earth demonstrate that the axis about which the earth rotates occupies what is, at all events, approximately a fixed position in the earth, though not a fixed position in space. We can hardly be surprised at this result; it merely implies that the earth acts like a rigid body on the whole, and does not permit the axis about which it is turning to change its position.

It will now be easily understood how it comes to pass that the position of the north pole upon the earth has not appreciably changed in the course of thousands of years. The axis around which the earth rotates has retained a permanent position relative to the earth itself; it has, however, continuously changed, it is at this moment changing, and it will continue to change with regard to its direction in space. So far our knowledge extended up to within the last few years, but in these modern days a closer inquiry has been made into this, as into so many other physical subjects, and the result has been to disclose the important fact that, though the phenomena as just described are very nearly true, they must receive a certain minute qualification. Complete examination of this subject is desirable, not only on account of its natural importance, but also because it illustrates the refinements of which modern astronomical processes are susceptible. I have stated the broad, general fact that the position of the terrestrial pole undergoes no large or considerable fluctuation. But while we admit that no large fluctuation is possible, it is yet very proper to consider whether there may not be a small fluctuation. It is certain that the position of the pole as it would be marked by a post driven into the earth to-day can not differ by a mile from the position in which the same point would be marked last year or next year. But does it differ at all? Is it absolutely exactly the same? Would there be a difference not indeed of miles, but of yards or of feet, between the precise position of the pole on the earth determined at successive intervals of time? Would it be the same if we carried out our comparisons not merely between one year and another, but day after day, week after week, month after month? No doubt the more obvious phenomena proclaim in the most unmistakable manner that the position of the pole is substantially invariable. If, therefore, there be any fluctuations in its positions, those could only be disclosed by careful scrutiny of minute phenomena which were too delicate to be detected in the coarser methods of observation. There is indeed a certain presumption in favour of the notion that absolute constancy in the position of the pole need not be expected. Almost every statement of astronomical doctrine requires its qualification, and it would seem indeed unlikely that when sufficient refinement was introduced into the measurements the position of the pole in the earth should appear to be absolutely unalterable. Until a very recent period the evidence on the subject was almost altogether negative; it was no doubt recognised that there might be some fluctuations in the position of the pole, but it was known that they would only be extremely small, and it was believed that in all probability those fluctuations must be comprised within those slender limits which are too much affected by inevitable errors of observation to afford any reliable result. Perseverance in this interesting inquiry has been at last rewarded; and, as in so many similar cases, we are indebted to the labours of many independent workers for the recent extension of our knowledge. We are, however, at present most interested by the labours of Mr. Chandler, a distinguished American astronomer, who has made an exhaustive examination into the subject. The result has been to afford a conclusive proof that the terrestrial pole does undergo movement. Mr. Chandler has been so successful as to have determined the law of those polar movements, and he has found that when they are taken into consideration an important improvement in certain delicate astronomical inquiries is the result. These valuable investigations merit, in the highest degree, the attention, not only of those who are specially devoted to astronomical and mathematical researches, but of that large and ever-increasing class who are anxious for general knowledge with regard to the physical phenomena of our globe.

At first sight it might seem difficult indeed to conduct the investigation of this question. Here is a point on the earth's surface, this wonderful north pole, which, so far as we certainly know, has never yet been approached to within four hundred miles, and yet we are so solicitous about the position of this pole and about its movement that we demand a knowledge of its whereabouts with an accuracy which at first apears wholly unattainable. It sounds almost incredible when we are told that a shift in the position of the north pole to the extent of twenty yards, or even of twenty feet, is appreciable, notwithstanding that we have never been able to get nearer to it than from one end of England to the other. Indeed, as a matter of fact, our knowledge of the movements of the pole are derived from observations made not alone hundreds but even many thousands of miles distant. It is in such observatories as those at Greenwich or Berlin, Pulkowa or Washington, that the determinations have been made by which changes in the position of the pole can be ascertained with a delicacy and precision for which those would hardly be prepared who were not aware of the refinement of modern astronomical methods. I do not, however, imply that the observations conducting to the discoveries now about to be considered have been exclusively obtained at the observatories I have named. There are a large number of similar institutions over the globe which have been made to bear their testimony. Tens of thousands of different observations have been brought together, and by discussing them it has been found possible to remove a large part of the errors by which such work is necessarily affected, and to elicit from the vast mass those grains of truth which could not have been discovered had it not been for the enormous amount of material that was available. Mr. Chandler has discussed these matters in a remarkable series of papers, and it will be necessary for me now to enter into some little detail, both as regards the kind of observations that have been made, and the results to which astronomers have been thereby conducted.

Greenwich Observatory lies more than two thousand miles from the north pole, and yet if the pole were to shift by as much as the width of Regent Street, the fact that it had done so would be quite perceptible at Greenwich. Let me endeavour to explain how such a measurement could be achieved. In finding the latitude at any locality we desire, of course, to know the distance between the locality and the equator, expressed in angular magnitude. But though this is distinctly the definition of latitude, it does not at once convey the idea as to how this element can be ascertained. How, for instance, would an astronomer at Greenwich be able to learn the angular distance of the observatory from the equator? The equator is not marked on the sky, and it is obvious that the observer must employ a somewhat indirect process to ascertain what he wants. Here, again, we have to invoke the aid of that celestial pole to which I have so often referred. Think of that point on the sky which is the common centre of the circles exhibited on Professor Barnard's photograph. That point is not indeed marked by any special star, but it is completely defined by the circumstance that it is the centre of the track performed by the circumpolar stars. We thus obtain a clear idea of this definite point in the sky, and the horizon is a perfectly definite line, at all events from any station where the sea is visible. It is not difficult to imagine that by suitable measurements we can ascertain the altitude of this point in the heavens above the horizon. That altitude is the latitude of the place; it is, in fact, the very angle which lies between the locality on the earth and the equator. It is quite true that as the pole is implied by these circles rather than directly marked by them, the measurement of the altitude can not be effected quite directly.

The actual process is to take the Pole Star, or some one of the other circumpolar stars, and to measure the greatest height to which it ascends above the horizon and the lowest altitude to which it declines about twelve hours later. The former of these is as much above the pole as the latter is below it, so between them we are able to ascertain the altitude of the pole with a high degree of accuracy. It is true that in a fixed observatory such as Greenwich there is no visible sea horizon, and even if there were it would not provide so excellent a method as is offered by the equivalent process of first observing the star directly and then observing its reflection from a dish of mercury. In this way the altitude of the star above the horizon is determined with the utmost precision. The practical astronomer will, however, remember that, of course, he has to attend to the effects of atmospheric refraction, which invariably shows a star higher up than it ought to be. This can be allowed for, and in this way the latitude of the observatory is ascertained with all needful accuracy. When the highest degree of precision is sought for, and it is only observations with a very high degree of precision which are available for our present purpose, a considerable number of stars have to be employed, and very many observations have to be taken at different seasons of the year, so as to eliminate as far as possible all sources of casual error. When, however, due attention has been paid to those precautions which the experience of astronomers suggests, the result that is obtained is characterized by extraordinary precision. How great that precision may be I must endeavour to explain. The latitude of every important observatory is obtained from a large number of observations, and it would be unlikely that it was more than one or two tenths of a second different from the actual mean value. Now, a tenth of a second on the surface of the earth corresponds to a distance of about ten feet, and this means that the latitude of the observatory or, as we must now speak very precisely, the latitude of the centre of the meridian circle in the observatory, is known to a degree of precision represented by a few paces. It will thus be seen that, with the accuracy attainable in our modern observations, it would often be an appreciable blunder to mistake the latitude of one wall of the observatory for that of the opposite wall; in other words, we know accurately to within the tenth of a second, or within not much more than the tenth of a second, the distance from the centre of the transit circle at Greenwich down to the earth's equator. But, of course, the distance from the pole to the equator is 90°, and this being so, it follows that the distance from the north pole of the earth to the centre of the transit circle at Greenwich Observatory has been accurately ascertained to within one or two tenths of a second. If any change took place in the distance between the pole and the meridian circle at Greenwich, then it must be manifested by the changes of latitude. We shall now be able to understand how any movement of the pole, or rather of the position which it occupies in the earth, would be indicated at Greenwich. Suppose, for instance, that the pole actually advanced toward Great Britain, and that it moved to a distance of, let us say, thirty feet, the effect of this would be to produce a diminution of the distance between the pole and Greenwich—that is to say, there must be an increase in the distance from Greenwich to the equator. This would correspond to a change in the latitude of Greenwich; that latitude would diminish by three tenths of a second, which is a magnitude quite large enough to be recognisable by the observations I have already indicated as proper for the determination of latitude. A shift of the pole to a distance of sixty feet would be a conspicuous alteration announced in every observatory in Europe provided with instruments of good modern construction.

Until the last few years there was not much reason to think that the pole exhibited any unequivocal indications of movement. No doubt, displacements resembling those which have now been definitely ascertained have existed for many years, but they were too small to produce any appreciable effect, except with instruments of a more refined description than those with which the earlier observatories were equipped. It was obvious that the pole did not make movements of anything like a hundred yards in extent; had it done so the resulting variations in latitude would have been conspicuous enough to have obtained notice many years ago. The actual movements which the pole does make are of that small character which require very minute discussion of the observations to establish them beyond reach of cavil. There is, however, one striking method of confirming such observations as have been made which leaves no doubt of the accuracy of the results to which they point. Suppose, for instance, that the great observatories in Europe indicate at a certain time that their latitudes have all increased; this necessarily implies that the equator has receded from them, and that, therefore, the north pole has approached Europe. If, however, the north pole has approached Europe it must have retreated from those regions on the opposite side of the world—say, for instance, the Sandwich Islands. Observations in the Sandwich Islands should, therefore, indicate, if our reasoning has been correct, that the pole has retreated from them, and that the equator has, therefore, advanced in such a way that the latitudes of localities in the Sandwich Islands have diminished. The various observations which have been brought together by the diligence of Mr. Chandler, including those which he has himself made with an ingenious apparatus of his own design, have been submitted to this test, and they have borne it well. The result has been that it is now possible to follow the movements of the pole with a considerable degree of completeness. Professor Chandler has tracked the pole month after month, year after year, through a period of more than a century of exact observations, and he has succeeded in determining the movements which this point undergoes. Let me here endeavour to describe the result at which he has arrived.

In that palæocrystic ocean which arctic travellers have described, where the masses of ice lie heaped together in the wildest confusion, lies this point which is the object of so much speculation. Let us think of this tract, or a portion of it, to be levelled to a plain, and at a particular centre let a circle be drawn the radius of which is about thirty feet; it is in the circumference of this circle that the pole of the earth is constantly to be found. In fact, if at different times, month after month and year after year, the position of the pole was ascertained as the extremity of that tube from which an eye placed at the centre of the earth would be able to see the pole of the heavens, and if the successive positions of this pole were marked by pegs driven into the ground, then the several positions in which the pole would be found must necessarily trace out the circumference of the circle that has been thus described. The period in which each revolution of the pole around the circle takes place is about four hundred and twenty-seven days; the result, therefore, of these investigations shows, when the observations are accurate, that the north pole of the earth is not, as has been so long supposed, a fixed point, but that it revolves around in the earth, accomplishing each revolution in about two months more than the period that the earth requires for the performance of each revolution around the sun.

The discovery of the movement of the pole which I have here described must be regarded as a noteworthy achievement in astronomy, nor is the result to which it leads solely of interest in consequence of the lesson it teaches us with regard to the circumstances of the earth's rotation. It has a higher utility, which the practical astronomer will not be slow to appreciate, and of which he has, indeed, already experienced the benefit. There are several astronomical investigations in which the latitude of the observatory enters as a significant element. Latitude is, in fact, at every moment employed as an important factor in many astronomical determinations: to take one of the most simple cases, suppose that we are finding the place of a planet in the observatory, we deduce its position by measuring its zenith distance, and then to obtain the declination the latitude of the observatory has, of course, to be considered. Now, astronomers have hitherto been in the habit of accepting the determination of their latitude which had been established by a protracted series of observations, and treating it as if it were a constant. This method will be no longer admissible when astronomical work of the highest class is demanded. No doubt, from the sailor's point of view, an alteration in latitude which at most amounts to a shift of sixty feet, not a quarter, perhaps, of the length of his vessel, is immaterial. But in the more refined parts of astronomical work these discoveries can no longer be overlooked; indeed, Mr. Chandler has shown that many discrepancies by which astronomers had been baffled can be removed when note is taken of the circumstance that the latitude of the observatory is in an incessant condition of transformation in accordance with the law which his labours have expounded. It will ere long be necessary in every observatory where important work is being done to obtain for every day the correction to the mean value of the latitude, in order to obtain the value appropriate for that day.

There are also other grounds of a somewhat profounder character on which the discoveries now made are eminently instructive. Those who are interested in the physics of our globe often discuss the question as to whether the internal heat, which the earth certainly possesses, is sufficiently intense to render the deep-seated portions of our globe more or less fluid. On the other hand, the effects of pressure, especially of such pressures as are experienced in the depths hundreds and thousands of miles below the surface, must go far to consolidate the materials to form what must be sensibly a rigid body. The question, therefore, arises, Is the earth to be regarded as a rigid mass, or is it not? The phenomena of the tides had already to some extent afforded information on this subject, and now Mr. Chandler's investigation adds much further light, for it is certain from his result that the earth can not be a rigid body. It is quite true that, even though the earth were rigid, the pole might go round in a circle, and that circle might have a thirty-feet radius, but in such a case the period would be only about three quarters of the four hundred and twenty-seven days which he has found. In the interest, therefore, of the theoretical astronomer, as well as on the other grounds which I have set forth, Mr. Chandler's investigations must be regarded as a most important contribution to modern astronomy.

THE AGE OF THE SUN'S HEAT

BY

SIR WILLIAM THOMSON (LORD KELVIN)

THE AGE OF THE SUN'S HEAT[1]

THE second great law of thermodynamics involves a certain principle of irreversible action in Nature. It is thus shown that, although mechanical energy is indestructible, there is a universal tendency to its dissipation, which produces gradual augmentation and diffusion of heat, cessation of motion, and exhaustion of potential energy through the material universe.[2] The result would inevitably be a state of universal rest and death, if the universe were finite and left to obey existing laws. But it is impossible to conceive a limit to the extent of matter in the universe; and therefore science points rather to an endless progress, through an endless space, of action involving the transformation of potential energy into palpable motion and thence into heat, than to a single finite mechanism, running down like a clock, and stopping forever. It is also impossible to conceive either the beginning or the continuance of life, without an overruling creative power; and, therefore, no conclusions of dynamical science regarding the future condition of the earth can be held to give dispiriting views as to the destiny of the race of intelligent beings by which it is at present inhabited.

The object proposed in the present article is an application of these general principles to the discovery of probable limits to the periods of time, past and future, during which the sun can be reckoned on as a source of

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[1] From "Macmillan's Magazine," March, 1862.

[2] See "On a Universal Tendency in Nature to the Dissipation of Mechanical Energy," "Proceedings of the Royal Society of Edinburgh," April 19, 1852; or the "Philosophical Magazine," October, 1852; also "Mathematical and Physical Papers," vol. i, art. 59.

heat and light. The subject will be discussed under three heads:

I. The secular cooling of the sun.

II. The present temperature of the sun.

III. The origin and total amount of the sun's heat.

I. THE SECULAR COOLING OF THE SUN.—How much the sun is actually cooled from year to year, if at all, we have no means of ascertaining, or scarcely even of estimating in the roughest manner. In the first place we do not know that he is losing heat at all. For it is quite certain that some heat is generated in his atmosphere by the influx of meteoric matter; and it is possible that the amount of heat so generated from year to year is sufficient to compensate the loss by radiation. It is, however, also possible that the sun is now an incandescent liquid mass, radiating away heat, either primitively created in his substance, or, what seems far more probable, generated by the falling in of meteors in past times, with no sensible compensation by a continuance of meteoric action.

It has been shown [3] that, if the former supposition were true, the meteors by which the sun's heat would have been produced during the last 2,000 or 3,000 years must have been all that time much within the earth's distance from the sun, and must therefore have approached the central body in very gradual spirals; because, if enough of matter to produce the supposed thermal effect fell in from space outside the earth's orbit, the length of the year would have been very sensibly shortened by the additions to the sun's mass which must have been made. The quantity of matter annually falling in must, on that supposition, have amounted to $\frac{1}{47}$ of the earth's mass, or to $\frac{1}{15000000}$ of the sun's; and therefore it would be necessary to suppose the "zodiacal light" to amount to at least $\frac{1}{5000}$ of the sun's mass, to account in the same way for a future supply of 3,000 years' sun-heat. When these conclusions were

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[3] "On the Mechanical Energies of the Solar System," "Transactions of the Royal Society of Edinburgh," April, 1854, and "Philosophical Magazine," December, 1854.

first published it was pointed out that "disturbances in the motions of visible planets" should be looked for, as affording us means for estimating the possible amount of matter in the zodiacal light; and it was conjectured that it could not be nearly enough to give a supply of 30,000 years' heat at the present rate. These anticipations have been to some extent fulfilled in Le Verrier's great researches on the motion of the planet Mercury, which have recently given evidence of a sensible influence attributable to matter circulating, as a great number of small planets, within his orbit round the sun. But the amount of matter thus indicated is very small; and, therefore, if the meteoric influx taking place at present is enough to produce any appreciable portion of the heat radiated away, it must be supposed to come from matter circulating round the sun, within very short distances of his surface. The density of this meteoric cloud would have to be supposed so great that comets could scarcely have escaped as comets actually have escaped, showing no discoverable effects of resistance, after passing his surface within a distance equal to ⅛ of his radius. All things considered, there seems little probability in the hypothesis that solar radiation is at present compensated, to any appreciable degree, by heat generated by meteors falling in; and, as it can be shown that no chemical theory is tenable,[4] it must be concluded as most probable that the sun is at present merely an incandescent liquid mass cooling.

How much he cools from year to year becomes therefore a question of very serious import, but it is one which we are at present quite unable to answer. It is true we have data on which we might plausibly found a probable estimate, and from which we might deduce, with at first sight seemingly well-founded confidence, limits, not very wide, within which the present true rate of the sun's cooling must lie. For we know, from the independent but concordant investigations of Herschel and Pouillet, that the sun radiates every year from his whole surface about

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[4] "Mechanical Energies of the Solar System."

6×10^{30} (six million million million million million) times as much heat as is sufficient to raise the temperature of one pound of water by 1° C. We also have excellent reason for believing that the sun's substance is very much like the earth's. Stokes's principles of solar and stellar chemistry have been for many years explained in the University of Glasgow, and it has been taught as a first result that sodium does certainly exist in the sun's atmosphere, and in the atmospheres of many of the stars, but that it is not discoverable in others. The recent application of these principles in the splendid researches of Bunsen and Kirchhof (who made an independent discovery of Stokes's theory) has demonstrated with equal certainty that there are iron and manganese, and several of our other known metals, in the sun. The specific heat of each of these substances is less than the specific heat of water, which indeed exceeds that of every other known terrestrial body, solid or liquid. It might, therefore, at first sight seem probable that the mean specific heat[5] of the sun's whole substance is less, and very certain that it can not be much greater, than that of water. If it were equal to the specific heat of water we should only have to divide the preceding number (6×10^{30}), derived from Herschel's and Pouillet's observations, by the number of pounds (4.3×10^{30}) in the sun's mass, to find 1.4° C. for the present annual rate of cooling. It might therefore seem probable that the sun cools more, and almost certain that he does not cool less, than a centigrade degree and four tenths annually. But, if this estimate were well founded, it would be equally just to assume that the

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[5] The "specific heat" of a homogeneous body is the quantity of heat that a unit of its substance must acquire or must part with, to rise or to fall by 1° in temperature. The main specific heat of a heterogeneous mass, or of a mass of homogeneous substance, under different pressures in different parts, is the quantity of heat which the whole body takes or gives in rising or in falling 1° in temperature, divided by the number of units in its mass. The expression, "mean specific heat" of the sun, in the text, signifies the total amount of heat actually radiated away from the sun, divided by his mass, during any time in which the average temperature of his mass sinks by 1°, whatever physical or chemical changes any part of his substance may experience.

sun's expansibility[6] with heat does not differ greatly from that of some average terrestrial body. If, for instance, it were the same as that of solid glass, which is about $\frac{1}{40000}$ on bulk, or $\frac{1}{120000}$ on diameter, per 1° C. (and for most terrestrial liquids, especially at high temperatures, the expansibility is much more), and if the specific heat were the same as that of liquid water, there would be in 860 years a contraction of one per cent on the sun's diameter, which could scarcely have escaped detection by astronomical observation. There is, however, a far stronger reason than this for believing that no such amount of contraction could have taken place, and therefore for suspecting that the physical circumstances of the sun's mass render the condition of the substances of which it is composed, as to expansibility and specific heat, very different from that of the same substances when experimented on in our terrestrial laboratories. Mutual gravitation between the different parts of the sun's contracting mass must do an amount of work, which can not be calculated with certainty, only because the law of the sun's interior density is not known. The amount of work performed on a contraction of one tenth per cent of the diameter, if the density remained uniform throughout the interior, would, as Helmholtz showed, be equal to 20,000 times the mechanical equivalent of the amount of heat which Pouillet estimated to be radiated from the sun in a year. But in reality the sun's density must increase very much toward his centre, and probably in varying proportions, as the temperature becomes lower and the whole mass contracts. We can not, therefore, say whether the work actually done by mutual gravitation during a contraction of one tenth per cent of the diameter

[6] The "expansibility in volume," or the "cubical expansibility," of a body, is an expression technically used to denote the proportion which the increase or diminution of its bulk, accompanying a rise or fall of 1° in its temperature, bears to its whole bulk at some stated temperature. The expression, "the sun's expansibility," used in the text, may be taken as signifying the ratio which the actual contraction, during a lowering of his mean temperature by 1° C., bears to his present volume.

would be more or less than the equivalent of 20,000 years' heat; but we may regard it as most probably not many times more or less than this amount. Now, it is in the highest degree improbable that mechanical energy can in any case increase in a body contracting in virtue of cooling. It is certain that it really does diminish very notably in every case hitherto experimented on. It must be supposed, therefore, that the sun always radiates away in heat something more than the Joule-equivalent of the work done on his contracting mass, by mutual gravitation of its parts. Hence, in contracting by one tenth per cent in his diameter, or three tenths per cent in his bulk, the sun must give out something either more, or not greatly less, than 20,000 years' heat; and thus, even without historical evidence as to the constancy of his diameter, it seems safe to conclude that no such contraction as that calculated above (one per cent in 860 years) can have taken place in reality. It seems, on the contrary, probable that, at the present rate of radiation, a contraction of one tenth per cent in the sun's diameter could not take place in much less than 20,000 years, and scarcely possible that it could take place in less than 8,600 years. If, then, the mean specific heat of the sun's mass, in its actual condition, is not more than ten times that of water, the expansibility in volume must be less than $\frac{1}{4000}$ per 100° C. (that is to say, less than $\frac{1}{10}$ of that of solid glass), which seems improbable. But although from this consideration we are led to regard it as possible that the sun's specific heat is considerably more than ten times that of water (and, therefore, that his mass cools considerably less than 100° C. in 700 years, a conclusion which, indeed, we could scarcely avoid on simply geological grounds), the physical principles we now rest on fail to give us any reason for supposing that the sun's specific heat is more than 10,000 times that of water, because we can not say that his expansibility in volume is probably more than $\frac{1}{400}$ per 1° C. And there is, on other grounds, very strong reason for believing that the specific heat is really much less than

10,000. For it is almost certain that the sun's mean temperature is even now as high as 14,000° C.; and the greatest quantity of heat that we can explain, with any probability, to have been by natural causes ever acquired by the sun (as we shall see in the third part of this article), could not have raised his mass at any time to this temperature, unless his specific heat were less than 10,000 times that of water.

We may therefore consider it as rendered highly probable that the sun's specific heat is more than ten times, and less than 10,000 times, that of liquid water. From this it would follow with certainty that his temperature sinks 100° C. in some time from 700 years to 700,000 years.

What, then, are we to think of such geological estimates as 300,000,000 years for the "denudation of the Weald"? Whether is it more probable that the physical conditions of the sun's matter differ 1,000 times more than dynamics compel us to suppose they differ from those of matter in our laboratories; or that a stormy sea, with possibly Channel tides of extreme violence, should encroach on a chalk cliff 1,000 times more rapidly than Mr. Darwin's estimate of one inch per century?

II. THE PRESENT TEMPERATURE OF THE SUN.—At his surface the sun's temperature can not, as we have many reasons for believing, be incomparably higher than temperatures attainable artificially in our terrestrial laboratories.

Among other reasons it may be mentioned that the sun radiates out heat from every square foot of his surface at only about 7,000 horse power.[7] Coal, burning at a rate of a little less than a pound per two seconds, would generate the same amount; and it is estimated (Rankine,

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[7] One horse power in mechanics is a technical expression (following Watt's estimate) used to denote a rate of working in which energy is involved at the rate of 33,000 foot pounds per minute. This, according to Joule's determination of the dynamical value of heat, would, if spent wholly in heat, be sufficient to raise the temperature of 23¾ pounds of water by 1° C. per minute.

"Prime Movers," p. 285, edition 1852) that, in the furnaces of locomotive engines, coal burns at from one pound in thirty seconds to one pound in ninety seconds per square foot of grate-bars. Hence heat is radiated from the sun at a rate not more than from fifteen to forty-five times as high as that at which heat is generated on the grate-bars of a locomotive furnace, per equal areas.

The interior temperature of the sun is probably far higher than that at his surface, because direct conduction can play no sensible part in the transference of heat between the inner and outer portions of his mass, and there must be an approximate convective equilibrium of heat throughout the whole, if the whole is fluid. That is to say, the temperatures, at different distances from the centre, must be approximately those which any portion of the substance, if carried from the centre to the surface, would acquire by expansion without loss or gain of heat.

III. The Origin and Total Amount of the Sun's Heat.—The sun being, for reasons referred to above, assumed to be an incandescent liquid now losing heat, the question naturally occurs, How did this heat originate? It is certain that it can not have existed in the sun through an infinity of past time, since, as long as it has so existed, it must have been suffering dissipation, and the finiteness of the sun precludes the supposition of an infinite primitive store of heat in his body.

The sun must, therefore, either have been created as an active source of heat at some time of not immeasurable antiquity, by an overruling decree; or the heat which he has already radiated away, and that which he still possesses, must have been acquired by a natural process, following permanently established laws. Without pronouncing the former supposition to be essentially incredible, we may safely say that it is in the highest degree improbable, if we can show the latter to be not contradictory to known physical laws. And we do show this and more, by merely pointing to certain actions going on before us at present, which, if sufficiently abundant at some past time, must have given the sun heat enough to account for all we know of his past radiation and present temperature.

It is not necessary at present to enter at length on details regarding the meteoric theory, which appears to have been first proposed in a definite form by Mayer, and afterward independently by Waterston; or regarding the modified hypothesis of meteoric vortices, which the writer of the present article showed to be necessary, in order that the length of the year, as known for the last 2,000 years, may not have been sensibly disturbed by the accessions which the sun's mass must have had during that period, if the heat radiated away has been always compensated by heat generated by meteoric influx.

For reasons mentioned in the first part of the present article, we may now believe that all theories of complete, or nearly complete, contemporaneous meteoric compensation must be rejected; but we may still hold that "meteoric action . . . is . . . not only proved to exist as a cause of solar heat, but it is the only one of all conceivable causes which we know to exist from independent evidence."[8]

The form of meteoric theory which now seems most probable, and which was first discussed on true thermodynamic principles by Helmholtz,[9] consists in supposing the sun and his heat to have originated in a coalition of smaller bodies, falling together by mutual gravitation, and generating, as they must do according to the great law demonstrated by Joule, an exact equivalent of heat for the motion lost in collision.

That some form of the meteoric theory is certainly the true and complete explanation of solar heat can scarcely be doubted, when the following reasons are considered:

1. No other natural explanation, except by chemical action, can be conceived.

2. The chemical theory is quite insufficient, because the most energetic chemical action we know, taking place be-

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[8] "Mechanical Energies of the Solar System." Note, p. 351.

[9] Popular lecture delivered on February 7, 1854, at Königsberg, on the occasion of the Kant commemoration.

tween substances amounting to the whole sun's mass, would only generate about 3,000 years' heat.[10]

3. There is no difficulty in accounting for 20,000,000 years' heat by the meteoric theory.

It would extend this article to too great a length, and would require something of mathematical calculation, to explain fully the principles on which this last estimate is founded. It is enough to say that bodies, all much smaller than the sun, falling together from a state of relative rest, at mutual distances all large in comparison with their diameters, and forming a globe of uniform density equal in mass and diameter to the sun, would generate an amount of heat which, accurately calculated according to Joule's principles and experimental results, is found to be just 20,000,000 times Pouillet's estimate of the annual amount of solar radiation. The sun's density must, in all probability, increase very much toward his centre, and therefore a considerably greater amount of heat than that must be supposed to have been generated if his whole mass was formed by the coalition of comparatively small bodies. On the other hand, we do not know how much heat may have been dissipated by resistance and minor impacts before the final conglomeration; but there is reason to believe that even the most rapid conglomeration that we can conceive to have probably taken place could only leave the finished globe with about half the entire heat due to the amount of potential energy of mutual gravitation exhausted. We may, therefore, accept, as a lowest estimate for the sun's initial heat, 10,000,000 times a year's supply at the present rate, but 50,000,000 or 100,000,000 as possible, in consequence of the sun's greater density in his central parts.

The considerations adduced above, in this paper, regarding the sun's possible specific heat, rate of cooling, and superficial temperature, render it probable that he must have been very sensibly warmer 1,000,000 years ago than now; and, consequently, if he has existed as a luminary for 10,000,000 or 20,000,000 years, he must have radiated away

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[10] "Mechanical Energies of the Solar System." Note, p. 351.

considerably more than the corresponding number of times the present yearly amount of loss.

It seems, therefore, on the whole most probable that the sun has not illuminated the earth for 100,000,000 years, and almost certain that he has not done so for 500,000,000 years. As for the future, we may say, with equal certainty, that inhabitants of the earth can not continue to enjoy the light and heat essential to their life for many million years longer unless sources now unknown to us are prepared in the great storehouse of creation.

THE PAST AND FUTURE OF OUR EARTH

AND

A NEW THEORY OF LIFE IN OTHER WORLDS

BY

RICHARD ANTHONY PROCTOR

(Born 1837; died 1888)

THE PAST AND FUTURE OF OUR EARTH[1]

> " Ut his exordia primis
> Omnia, et ipse tener Mundi concreverit orbis.
> Tum durare solum, et discludere Nerea ponto
> Cœperit, et rerum paullatim sumere formas."
> VIRGIL.

THE subject with which I am about to deal[2] is associated by many with questions of religion. Let me premise, however, that I do not thus view it myself. It seems to me impossible to obtain from science any clear ideas respecting the ways or nature of the Deity, or even respecting the reality of an Almighty personal God. Science deals with the finite, though it may carry our thoughts to the infinite. Infinity of space and of matter occupying space, of time and of the processes with which time is occupied, and infinity of energy as necessarily implied by the infinities of matter and of the operations affecting matter—these infinities science brings clearly before us. For science directs our thoughts to the finites to which these infinites correspond. It shows us that there can be no conceivable limits to space or time, and though finiteness of matter or of operation may be conceivable, there is manifest incongruity in assuming an infinite disproportion between unoccupied and occupied space, or between void

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[1] From " Our Place among Infinities," D. Appleton and Company.

[2] This essay presents the substance of a lecture delivered in New York on April 3, 1874, being the first of a subsidiary series in which, of set purpose (and in accordance with the request of several esteemed friends), I dealt less with the direct teachings of astronomy, which had occupied me in a former series, than with ideas suggested by astronomical facts, and more particularly by the discoveries made during the last quarter of a century.

time and time occupied with the occurrence of events of what sort soever. So that the teachings of science bring us into the presence of the unquestionable infinities of time and of space, and the presumable infinities of matter and of operation—hence, therefore, into the presence of infinity of energy. But science teaches us nothing about these infinities, as such. They remain none the less inconceivable, however clearly we may be taught to recognise their reality. Moreover, these infinites, including the infinity of energy, are material infinities. Science tells us nothing of the infinite attributes of an Almighty Being; it presents to us no personal infinites, whether of Power, Beneficence, or Wisdom. Science may suggest some ideas on these points, though we perceive daily more and more clearly that it is unsafe to accept as her teaching ideas which commonly derive their colouring from our own prepossessions. And assuredly, as respects actual facts, Science in so far as she presents personal infinity to us at all, presents it as an inconceivable, like those other inconceivable infinities, with the finites corresponding to which her operations are alone directly concerned. To speak in plain terms—so far as science is concerned, the idea of a personal God is inconceivable,[3] as are all the attributes which religion recognises in such a Being. On the other hand, it should be admitted as distinctly, that Science no more disproves the existence of infinite personal power or wisdom than she disproves the existence of infinite material

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[3] I mean these words to be understood literally. To the man of science, observing the operation of second causes in every process with which his researches deal, and finding no limit to the operation of such causes however far back he may trace the chain of causation, the idea of a first cause is as inconceivable in its relation to observed scientific facts as is the idea of infinite space in its relation to the finite space to which the observations of science extend. Yet infinite space must be admitted; nor do I see how even that man of science who would limit his thoughts most rigidly to facts, can admit that all things are of which he thinks, without having impressed upon him the feeling that, in some way he can not understand, these things represent the operation of Infinite Purpose. Assuredly we do not avoid the inconceivable by assuming as at least possible that matter exists only as it affects our perceptions.

energy (which on the contrary must be regarded as probable) or the existence of infinite space or time (which must be regarded as certain).

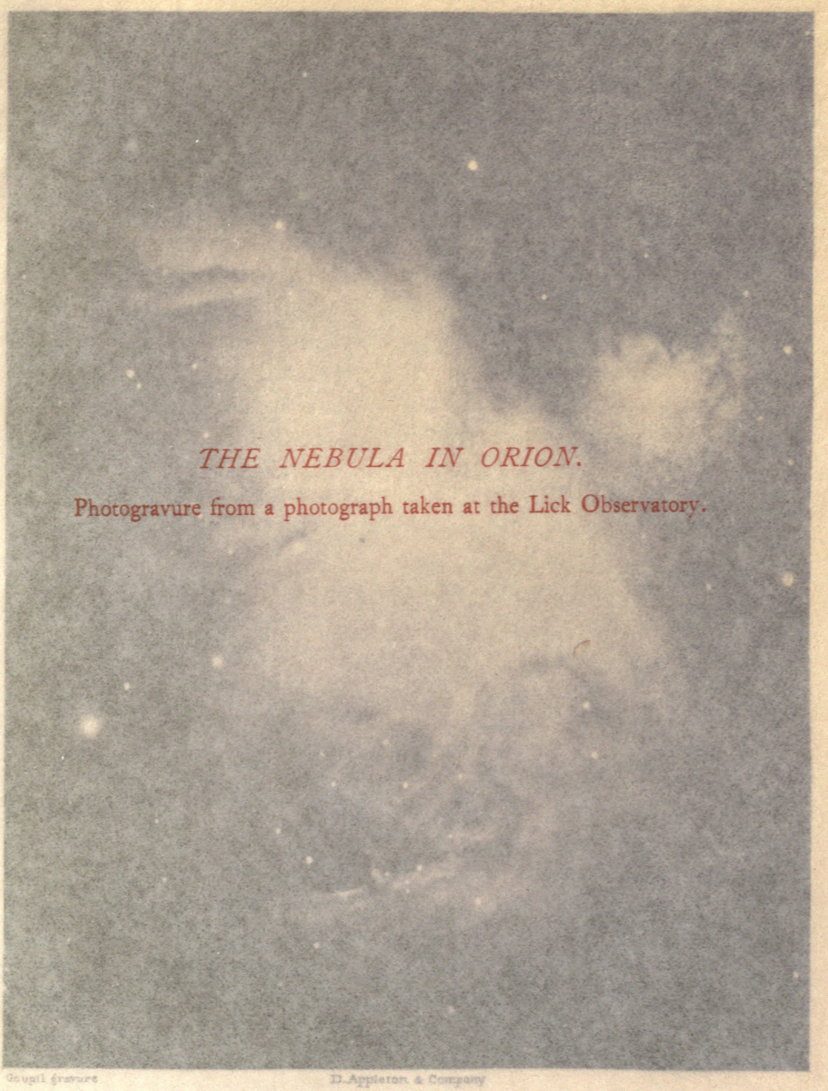
So much premised, we may proceed to inquire into the probable past and future of our earth, as calmly as we should inquire into the probable past and future of a pebble, a weed, or an insect, of a rock, a tree, or an animal, of a continent, or of a type—whether of vegetable or of animal life. The beginning of all things is not to be reached, not appreciably to be even approached, by a few steps backward in imagination, nor the end of all things by a few steps forward. Such a thought is as unfounded as was the fear of men in old times that by travelling too far in any direction they might pass over the earth's edge and be plunged into the abyss beyond, as unreasonable as was the hope that by increase of telescopic range astronomers could approach the imagined "heavens above the crystalline."

In considering the probable past history of the earth, we are necessarily led to inquire into the origin of the solar system. I have already sketched two theories of the system, and described the general facts on which both theories are based. The various planets circle in one direction around the sun, the sun rotating in the same direction, the satellite families (with one noteworthy but by no means inexplicable exception) travelling round their primaries in the same direction, and all the planets whose rotation has been determined still preserving the same direction of circulation (so to speak). These relations seem to point, in a manner there is no mistaking, to a process of evolution by which those various parts of the solar system which now form discrete masses were developed from a former condition characterized by a certain unity as respects the manner of its circulation. One theory of this process of evolution, Laplace's, implies the contraction of the solar system from a great rotating nebulous mass; according to the other theory, the solar system, instead of contracting to its present condition, was formed by a process of accretion, due to the indrawing of great flights of meteoric and cometic matter.

I need not here enter at length, for I have already done so elsewhere, into the astronomical evidence in favour of either theory; but it will be well to present briefly some of the more striking facts.

Among the various forms of nebulæ (or star-cloudlets) revealed by the telescope, we find many which seem to accord with our ideas as to some of the stages through which our solar system must have passed in changing from the nebulous condition to its present form. The irregular nebulæ—such, for instance, as that wonderful nebula in the Sword of Orion—show by their enormous extension the existence of sufficient quantities of gaseous matter to form systems as large and as massive as our own, or even far vaster. We know from the teachings of the spectroscope that these irregular nebulæ do really consist of glowing gas (as Sir W. Herschel long since surmised), hydrogen and nitrogen being presumably present, though the spectrum of neither gas appears in its complete form (one line only of each spectrum being shown, instead of the sets of lines usually given by these gases). An American physicist has suggested that hydrogen and nitrogen exist in the gaseous nebulæ in an elementary condition, these gases really being compound, and he suggests further that all our so-called elements may have been derived from those elementary forms of hydrogen and nitrogen. In the absence of any evidence from observation or experiment, these ideas must be regarded as merely speculative; and I think that we arrive here at a point where speculation helps us as little as it does in attempting to trace the evolution of living creatures across the gap which separates the earliest forms of life from the beginning itself of life upon the earth. Since we can not hope to determine the real beginning of the earth's history, we need not at present attempt to pass back beyond the earliest stage of which we have any clear information.

Passing from the irregular nebulæ, in which we see



THE NEBULA IN ORION.

Photogravure from a photograph taken at the Lick Observatory.

Goupil gravure

D. Appleton & Company

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chaotic masses of gaseous matter occupying millions of millions of cubic miles and scattered as wildly through space as clouds are scattered in a storm-swept air, we come to various orders of nebulæ in which we seem to find clear evidence of a process of evolution. We see first the traces of a central aggregation. This aggregation becomes more and more clearly defined, until there is no possibility of mistaking its nature as a centre having power (by virtue of the quantity of matter contained in it) to influence the motions of the matter belonging to the rest of the nebula. Then, still passing be it remembered from nebula to nebula, and only inferring, not actually witnessing, the changes described—we see a subordinate aggregation, wherein, after a while, the greater portion of the mass of the nebula outside the central aggregation becomes gathered, even as Jupiter contains the greater portion of the mass of the solar system outside the central sun.[4] Next we see a second subordinate aggregation, inferior to the first, but comprising, if we judge from its appearance, by far the greater portion of what remained after the first aggregation had been formed—even as Saturn's mass far exceeds the combined mass of all the planets less than himself, and so comprises far the greater portion of the solar system after account has been taken of Jupiter and the sun.[5] And we may infer that the other parts of nebulæ contain smaller aggregations not perceptible to us, out of which the smaller planets of the developing system are hereafter to be formed.

Side views of some of these nebulæ indicate a flatness of figure agreeing well with the general tendency of the members of the solar system toward the medial plane of that system. For the solar system may be described as flat, and if the nebulæ I have been dealing with (the spiral nebulæ with aggregations) were globular we could not recognise in them the true analogues of our solar system in the

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[4] The mass of Jupiter exceeds, in the proportion of five to two, the combined mass of all the remaining planets.

[5] The mass of Saturn exceeds, in the proportion of nearly three to one, the combined mass of all the planets smaller than himself.

earlier stages of its history. But the telescope reveals nebulæ manifestly corresponding in appearance to the great whirlpool nebula of Lord Rosse, as it would appear if it is a somewhat flattened spiral and could be viewed nearly edgewise.

And here I may pause to note that, although, in thus inferring progressive changes where in reality we have but various forms of nebulæ, I have been adopting an assumption and one which no one can hope either to verify or to disprove, yet it must be remembered that these nebulæ by their very figure indicate that they are not at rest. If they consist of matter possessing the attribute of gravitation—and it would be infinitely more daring to assert that they do not than that they do—then they must be undergoing processes of change. Nor can we conceive that discrete gaseous masses in whorls spirally arranged around a great central aggregation (taking one of the earlier stages) could otherwise change than by aggregating toward their centre, unless we admit motions of revolution (in orbits more or less eccentric) the continuance of which would necessarily lead, through collisions, to the rapid growth of the central aggregation, and to the formation and slower growth of subordinate gatherings.

I have shown elsewhere how the formation of our solar system, in the manner supposed, would explain what Laplace admitted that he could not explain by his theory—the peculiar arrangement of the masses forming the solar system. The laws of dynamics tell us that no matter what the original configuration or motion of the masses, probably gaseous, forming the nebula, the motions of these masses would have greater and greater velocity the nearer the masses were to the central aggregation, each distance indicating certain limits between which the velocities must inevitably lie. For example, in our solar system, supposing the central sun had already attained very nearly his full growth as respects quantity of matter, then the velocity of any mass whatever belonging to the system would at Jupiter's distance be less than twelve miles per second, whereas at the distance of the earth, the largest planet travelling inside the orbit of Jupiter, the limit of the velocity would be more than twice as great. Hence we can see with what comparative difficulty an aggregation would form close to the central one, and how the first subordinate aggregation would lie at a distance where the quantity of matter was still great but the average velocity of motion not too great. Such an aggregation once formed, the next important aggregation would necessarily lie far outside, for within the first there would now be two disturbing influences preventing the rapid growth of these aggregations. The third and fourth would be outside the second. Between the first aggregation and the sun only small planets, like the earth and Venus, Mars, Mercury, and the asteroids, could form; and we should expect to find that the largest of the four small planets would be in the middle of the space belonging to the family (as Venus and the earth are actually placed), while the much smaller planets Mercury and Mars travel next on either side, one close to the sun and the other next to Jupiter, the asteroids indicating the region where the combined disturbing influences of Jupiter and the sun prevented any single planet from being developed.

But I should require much more time than is now at my command to present adequately the reasoning on which the theory of accretion is based. And we are not concerned here to inquire whether this theory, or Laplace's theory of contraction, or (which I hold to be altogether more probable than either) a theory involving combined processes of accretion and contraction, be the true hypothesis of the evolution of the solar system. Let it suffice that we recognise as one of the earliest stages of our earth's history her condition as a rotating mass of glowing vapour, capturing then as now, but far more actively then than now, masses of matter which approached near enough, and growing by these continual indraughts from without. From the very beginning, as it would seem, the earth grew in this way. This firm earth on which we live represents an aggregation of matter not from one portion of space, but from all space.

All that is upon and within the earth, all vegetable forms and all animal forms, our bodies, our brains, are formed of materials which have been drawn in from those depths of space surrounding us on all sides. This hand that I am now raising contains particles which have travelled hither from regions far away amid the northern and southern constellations, particles drawn in toward the earth by processes continuing millions of millions of ages, until after multitudinous changes the chapter of accidents has so combined them, and so distributed them in plants and animals, that after coming to form portions of my food they are here present before you. Passing from the mere illustration of the thought, is not the thought itself striking and suggestive, that not only the earth on which we move, but everything we see or touch, and every particle in body and brain, has sped during countless ages through the immensity of space?

The great mass of glowing gas which formed our earth in the earliest stage of its history was undergoing two noteworthy processes—first, the process of cooling by which the mass was eventually to become at least partially solid, and secondly a process of growth due to the gathering in of meteoric and cometic matter. As respects the latter process, which will not hereafter occupy our attention, I must remark that many astronomers appear to me to give far less consideration to the inferences certainly deducible from recent discoveries than the importance of these discoveries would fairly warrant. It is now absolutely certain that hour by hour, day by day, and year by year, the earth is gathering in matter from without. On the most moderate assumption as to the average weight of meteors and shooting stars, the earth must increase each year in mass by many thousands of tons. And when we consider the enormous, one may almost say the awful, time-intervals which have elapsed since the earth was in a gaseous condition, we can not but perceive that the process of accretion now going on indicates the existence of only the merest residue of matter (ungathered) compared with that which at the beginning of those time-intervals was freely moving around the central aggregation. The process of accretion which now does not sensibly increase the earth's mass was then a process of actual growth. Jupiter and Saturn might then no longer be gathering in matter appreciably increasing their mass, although the quantity of matter gathered in by them must have been far larger than all that the other forming earth could gather in equal times. For those planets were then as now so massive that any possible increment from without was as nothing compared with the mass they had already attained. We have to throw back into yet more awful time-depths the birth and growth of those giant orbs. And even those depths of time are as nothing compared with the intervals which have elapsed since the sun himself began to be. Yet it is with time-intervals measurable by hundreds of millions of years that we have to deal in considering only our earth's history—nay, two or three hundred millions of years only carry us back to a period when the earth was in a stage of development long sequent to the gaseous condition we are now considering. That the supply of meteoric and cometic matter not gathered in was then enormously greater than that which still exists within the solar domain, appears to me not a mere fanciful speculation, nor even a theoretical consideration, but as nearly a certainty as anything not admitting of mathematical demonstration can possibly be. That the rate of ingathering at that time enormously exceeded the present rate may be regarded as certain. That the increase resulting from such ingathering during the hundreds of millions of years that it has been in operation since the period when the earth first existed as a gaseous mass, must have resulted in adding a quantity of matter forming no inconsiderable aliquot part of the earth's present mass, seems to me a reasonable inference, although it is certain that the present rate of growth continued even for hundreds of millions of years would not appreciably affect the earth's mass.[6] And

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[6] It is, perhaps, hardly necessary to explain that I refer here not to absolute but to relative increase. The absolute increase of mass would

it is a thought worthy of consideration, in selecting between Laplace's theory of contraction and the theory of accretion, that accretion being a process necessarily exhaustive, we are able to trace it back through stages of gradually increasing activity without limit until we reach that stage when the whole of the matter now forming our solar system was as yet unformed. Contraction may alternate with expansion, according to the changing condition of a forming system; but accretion is a process which can only act in one direction; and as accretion is certainly going on now, however slowly, we have but to trace back the process to be led inevitably, in my judgment, to regard our system as having its origin in processes of accretion—though it seems equally clear that each individual orb of the system, if not each subordinate scheme within it, has also undergone a process of contraction from a former nebulous condition.

In this early gaseous stage our earth was preparing, as it were, to become a sun. As yet her gaseous globe probably extended beyond the smaller aggregation out of which the moon was one day to be formed. This may be inferred, I think, from the law of the moon's rotation. It is true that a moon independently created, and started on the moon's present course, with a rotation-period nearly equalling its period of revolution, would gradually have acquired a rotation-period exactly equalling the mean period of revolution. But there is no reason in Nature why there should have been any such near approach; whereas, if we suppose the moon's gaseous globe to have been originally entangled within the outskirts of the earth's, we see that the peculiar relation in question would have prevailed from the beginning of the moon's existence as a separate body. The laws of dynamics show us, moreover, that although the conditions under which the moon moved and rotated must have undergone considerable changes since her first formation, yet that since those changes took place very

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amount to many millions of tons, but the earth would not be increased by the billionth part of her present mass.

slowly, the rotation of the moon would be gradually modified, pari passu, so that the peculiar relation between the moon's rotation and revolution would continue unimpaired.[7]

In her next stage, our earth is presented to us as a sun. It may be that at that time the moon was the abode of life, our earth affording the supplies of light and heat necessary for the wants of creatures living on the moon. But whether this were so or not, it may be safely assumed that when the earth's contracting gaseous globe first began to have liquid or solid matter in its constitution, the earth must have been a sun so far as the emission of heat and light were concerned. I must warn you, however, against an undue regard for analogy which has led some astronomers to say that all the members of the solar system have passed or will pass through exactly similar stages. That our earth once gave out light and heat, as the sun does now, may be admitted as probable; and we may believe that later the earth presented the characteristics which we now recognise in Jupiter; while hereafter it may pass through a stage comparable with that through which our moon is now passing. But we must remember that the original quantity of matter in any orb passing through such stages must very importantly modify the actual condition of the orb in each of those stages, as well, of course, as the duration of each stage; and it may even be that no two orbs in the universe were ever in the same or very nearly the same condition, and that no change undergone by one has corresponded closely with any change undergone by another.

We know so little respecting the sun's actual condition, that even if we could be assured that in any past stages of her history the earth was nearly in the same state, we

[7] On the theory of evolution some such view of the origin of the moon's rotation must be adopted, unless the matter be regarded as the result of a strange chance. If we believe, on the contrary, that the arrangement was specially ordained by the Creator, we are left to wonder what useful purpose a relation so peculiar and so artificial can have been intended to subserve.

should nevertheless remain in almost complete ignorance as to the processes to which the earth's orb was at that time subject. In particular we have no means of forming an opinion as to the manner in which the elementary constituents of the earth's globe were situated when she was in the sunlike stage. We may adopt some general theory of the sun's present condition; for example, we may accept the ingenious reasoning by which Professor Charles A. Young has supported his theory that the sun is a gigantic bubble;[8] but we should be far from having any exact idea of the processes actually taking place within the solar globe, even if we were absolutely certain that that or some other general theory were the true one.

Assuming that our earth, when in the sunlike stage, was a gaseous mass within a liquid non-permanent shell, we can see that as the process of cooling went on the showers forming the shell would attain a greater and greater depth, the shell thus becoming thicker, the space within the shell

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[8] "The eruptions which are all the time" (Anglice, "always") "occurring on the sun's surface," says Professor Young, "almost compel the supposition that there is a crust of some kind which restrains the imprisoned gases, and through which they force their way with great violence. This crust may consist of a more or less continuous sheet of rain—not of water, of course, but of materials whose vapours are shown by means of the spectroscope to exist in the solar atmosphere, and whose condensations and combinations are supposed to furnish the solar heat. The continuous outflow of the solar heat is equivalent to the supply that would be developed by the condensation from steam to vapour of a layer about five feet thick over the whole surface of the sun per minute. As this tremendous rain descends, the velocity of the falling drops would be increased by the resistance of the dense gases underneath, the drops would increase until continuous sheets would be formed; and the sheets would unite and form a sort of bottomless ocean, resting upon the compressed vapours beneath and pierced by innumerable ascending jets and bubbles. It would have nearly a constant depth in thickness, because it would re-evaporate at the bottom nearly as fast as it would grow by the descending rains above, though probably the thickness of this sheet would continually increase at some slow rate, and its whole diameter diminish. In other words, the sun, according to this view, is a gigantic bubble, whose walls are gradually thickening and its diameter diminishing at a rate determined by its loss of heat. It differs, however, from ordinary bubbles in the fact that its skin is constantly penetrated by blasts and jets from within."

becoming less, the whole earth contracting until it became entirely liquid; or rather these changes would progress until no considerable portion of the earth would be gaseous, for doubtless long before this stage was reached large portions of the earth would have become solid. As to the position which the solid parts of the earth's globe would assume when the first processes of solidification took place, we must not fall into the mistake of judging from the formation of a crust of ice on freezing water that these solid parts would form a crust upon the earth. Water presents an exception to other substances, in being denser in the liquid form than as a solid. Some metals and alloys are like water in this respect; but with most earthy substances, "and notably," says Dr. Sterry Hunt, "the various minerals and earthy compounds like those which may be supposed to have made up the mass of the molten globe, the case is entirely different. The numerous and detailed experiments of Sainte-Claire Deville, and those of Delesse, besides the earlier ones of Bischof, unite in showing that the density of fused rocks is much less than that of the crystalline products resulting from their slow cooling, these being, according to Deville, from one seventh to one sixteenth heavier than the fused mass, so that if formed at the surface they would, in obedience to the laws of gravity, tend to sink as soon as formed."[9]

Nevertheless, inasmuch as solidification would occur at the surface, where the radiation of heat would take place most rapidly, and as the descending solid matter would be gradually liquefied, it seems certain that for a long time the solid portions of the earth, though not forming a solid crust, would occupy the exterior parts of the earth's globe. After a time, the whole globe would have so far cooled that a process of aggregation of solid matter around the centre of the earth would take place. The matter so aggregated consisted probably of metallic and metalloidal compounds denser than the material forming the crust of the earth.

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[9] It is as yet doubtful how far the recent experiments of Mallet affect this reasoning.

Between the solid centre and the solidifying crust there would be a shell of uncongealed matter, gradually diminishing in amount, but a portion probably retaining its liquid condition even to the present time, whether existing in isolated reservoirs, or whether, as Scrope opines, it forms still a continuous sheet surrounding the solid nucleus. One strange fact of terrestrial magnetism may be mentioned in partial confirmation of the theory that the interior of the earth is of this nature—a great solid mass, separated from the solid crust by a viscous plastic ocean: the magnetic poles of the earth are changing in position in a manner which seems only explicable on the supposition that there is an interior solid globe rotating under the outer shell, but at a slightly different rate, gaining or losing one complete rotation in the course of about six hundred and fifty years.

Be this as it may, we find in this theory an explanation of the irregularities of the earth's surface. The solid crust, contracting at first more rapidly than the partially liquid mass within, portions of this liquid matter would force their way through and form glowing oceans outside the crust. Geology tells us of regions which, unless so formed, must have been produced in the much more startling manner conceived by Meyer, who attributed them to great meteoric downfalls.[10] At a later stage, when the crust,

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[10] There is very little new under the sun. In dealing with the multitudinous lunar craters, which were certainly formed in ages when unattached meteors were enormously greater in number and size than at present, I mentioned as a consideration not to be overlooked the probability that some of the meteoric matter falling on the moon when she was plastic with intensity of heat might be expected to leave traces which we could discern; and although none of the larger lunar craters could be so formed, yet some of the smaller craters in these lunar regions where craters overlap like the rings left by raindrops which have fallen on a plastic surface, might be due to meteoric downfall. I find that Meyer had far earlier advanced a similar idea in explanation of those extensive regions of our earth which present signs of having been in a state of igneous fluidity. Again, two or three years ago, Sir W. Thomson startled us all by suggesting the possibility that vegetable life might have been introduced upon our earth by the downfall of fragments of old worlds. Several years before, Dr. Sterry Hunt had pointed to evidence which tends to show that large meteoric globes had fallen on the earth, and he showed further that some meteors con-

having hitherto cooled more rapidly than the interior, began to have a slower rate of cooling, the retreating nucleus left the crust to contract upon it, corrugating in the process, and so forming the first mountain ranges upon the spheroidal earth, which preceding processes had left partially deformed and therefore ready to become in due time divided into oceans and continents.

At this stage the earth must have been surrounded by an atmosphere much denser than that now existing, and more complex in constitution. We may probably form the most trustworthy opinion of the nature of the earth's atmosphere and the probable condition of the earth's surface at this early epoch by following the method of reasoning employed by Dr. Sterry Hunt. It will be remembered that he conceives an intense heat applied to the earth as at present existing, and infers the chemical results. It is evident that such a process would result in the oxidation of every form of carbonaceous matter; all carbonates, chlorides, and sulphates would be converted into silicates—carbon, chlorine, and sulphur being separated in the form of acid gases. These gases, with nitrogen, an excess of oxygen, and enormous quantities of aqueous vapour, would form an atmosphere of great density. In such an atmosphere condensation would only take place at a temperature far above the present boiling point; and the lower level of the slowly cooling crust would be drenched with a heated solution of hydrochloric acid, whose decomposing action, aided by its high temperature, would be exceedingly rapid. The primitive igneous rock on which these heavy showers fell probably resembled in composition certain furnace-slags or basic volcanic glasses. Chlorides of the various

tain hydrocarbons and certain metallic compounds indicating processes of vegetation. Dr. Hunt tells me that, in his opinion, some of the meteors whose fragments have fallen on the earth in historic times were once covered with vegetation, since otherwise, according to our present chemical experience, the actual condition of these meteoric fragments would be inexplicable. He does not regard them as fragments of a considerable orb comparable even with the least of the planets, but still, whatever their dimensions may have been, he considers that vegetable life must have formerly existed upon them.

bases would be formed, and silica would be separated under the decomposing action of the heated showers until the affinities of the hydrochloric acid were satisfied. Later, sulphuric acid would be formed in large quantities by the combination of oxygen with the sulphurous acid of the primeval atmosphere. After the compounds of sulphur and chlorine had been separated from the air, carbonic acid would still continue to be an important constituent of the atmosphere. This constituent would gradually be diminished in quantity, during the conversion of the complex aluminous silicates into hydrated silicate of alumina, or clay, while the separated lime, magnesia, and alkalies would be changed into bicarbonates, and carried down to the sea in a state of solution.

Thus far the earth was without life; at least no forms of life, vegetable or animal, with which we are familiar, could have existed while the processes hitherto described were taking place. The earth during the long series of ages required for these changes was in a condition comparable with the condition through which Jupiter and Saturn are apparently at present passing. A dense atmosphere concealed the surface of the earth, even as the true surface of Jupiter is now concealed. Enormous cloud-masses were continually forming and continually pouring heavy showers on the intensely heated surface of the planet, throughout the whole of the enormous period which elapsed between the time when first the earth had a surface, and the time when the atmosphere began to resemble in constitution the air we breathe. Even when vegetable life, such as we are familiar with, was first possible, the earth was still intensely heated, and the quantity of aqueous vapour and cloud always present in the air must have been far greater than at present.

It has been in vain, thus far, that men have attempted to lift the veil which conceals the beginning of life upon the earth. It would not befit me to express an opinion on the controversy whether the possibility of spontaneous generation has, or has not, been experimentally verified.

That is a question on which experts alone can give an opinion worth listening to; and all that can here be noted is that experts are not agreed upon the subject. As a mere speculation it may be suggested that, somewhat as the elements when freshly released from chemical combination show for a short time an unusual readiness to enter into new combinations, so it may be possible that, when the earth was fresh from the baptism of liquid fire to which her primeval surface had for ages been exposed, certain of the substances existing on her surface were for the time in a condition fitting them to pass to a higher order of existence, and that then the lower forms of life sprang spontaneously into existence on the earth's still throbbing bosom. In any case, we need not feel hampered by religious scruples in considering the possibility of the spontaneous generation of life upon the earth. It would be straining at a gnat and swallowing a camel if we found a difficulty of that sort here, after admitting, as we are compelled by clearest evidence to admit, the evolution of the earth itself and of the system to which the earth belongs, by purely natural processes. The student of science should view these matters apart from their supposed association with religious questions, apart in particular from interpretations which have been placed upon the Bible records. We may be perfectly satisfied that the works of God will teach us aright if rightly studied. Repeatedly it has been shown that ideas respecting creation which had come to be regarded as sacred because they were ancient, were altogether erroneous, and it may well be so in this matter of the creation of life.[11]

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[11] It is not for me to undertake to reconcile the Bible account of creation with the results which science is bringing gradually more clearly before us. It seems to me unfortunate, in fact, that such reconciliation should be thought necessary. But it must be conceded, I suppose, by all, that it is not more difficult to reconcile modern biological theories of evolution with the Bible record than it is to reconcile with that record the theory of the evolution of the solar system. Yet strangely enough many oppose the biological theories (not without anger), who readily admit that some form or other of the nebular hypothesis of the solar system must be adopted in order to explain the peculiarities of structure presented by that system.

Whatever opinion we form on these points, it seems probable that vegetable life existed on the earth before animal life, and also that primeval vegetation was far more luxuriant than the vegetation of our own time. Vast forests were formed, of which our coal-fields, enormous as is their extent, represent merely a small portion preserved in their present form through a fortuitous combination of exceptional conditions. By far the greater portion of those forest masses underwent processes of vegetable decay effectually removing all traces of their existence. What escaped, however, suffices to show the amazing luxuriance with which vegetation formerly throve over the whole earth.

In assuming the probability that vegetable life preceded animal life, I may appear to be opposing myself to an accepted paleontological doctrine, according to which animal and vegetable life began together upon the earth. But I would remind you that the actual teaching of the ablest, and therefore the most cautious, paleontologists on this point, amounts merely to this, that if the geological record as at present known be assumed to be coeval with the commencement of life upon the globe, then animals and plants began their existence together. In a similar way the teachings of geology and paleontology as to the nature of the earliest known forms of life and as to the succession of faunæ and floræ, depend on an admittedly imperfect record. Apart, however, from this consideration, I do not think it would serve any useful purpose if I were to attempt, I will not say to discuss, for that is out of the question, but to speak of the geological evidence respecting that portion of the past history of our earth which belongs to the interval between the introduction of life upon the surface and the present time. In particular, my opinion on the interesting question, whether all the forms of life upon the earth, including the various races of man, came into being by processes of evolution, could have no weight whatever. I may remark that, even apart from the evidence which the most eminent biologists have brought to bear on this question, it seems to me illogical to accept evolution as sufficient to explain the history of our earth during millions of years prior to the existence of life, and to deny its sufficiency to explain the development of life (if one may so speak), upon the earth. It seems even more illogical to admit its operation up to any given stage in the development of life, and there to draw a hard-and-fast line beyond which its action can not be supposed to have extended.[12] Nor can I understand why it should be considered a comforting thought, that at this or that epoch in the history of the complex machine of life, some imperfection in the machinery compelled the intervention of God—thus presented to our contemplation as Almighty, but very far from being All-wise.

There is, however, one aspect in which the existence of life has to be considered as intimately associated with the future history of our earth. We perceive that the abundance of primeval vegetation during long ages, aided by other processes tending gradually to reduce the amount of carbonic-acid gas in the air, must have led to a gradual change in the constitution of the atmosphere. At a later epoch, when animal life and vegetable life were more equally proportioned, a state of things existed which, so far as can be judged, might have lasted many times as long as it has already lasted had not man appeared upon the scene. But it seems to me impossible to consider what is actually taking place on the earth at present, without perceiving that within periods, short indeed by comparison with geological eras, and still shorter compared with the intervals to which the astronomical history of our earth has introduced us, the condition of the earth as an abode of

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[12] Since I thus spoke, a new and as it seems to me an even more illogical limit has been suggested for the operation of the process of evolution as affecting the development of life, and this by an advocate of the general doctrine of evolution. I refer to the opinion advanced by Mr. J. Fiske, of Harvard University, that "no race of organisms can in future be produced through the agency of natural selection and direct adaptation, which shall be zoölogically distinct from, and superior to, the human race."

life will be seriously modified by the ways and works of man. It is only in the savage state that man is content to live upon the produce of the earth, taking his share, as it were, of what the earth (under the fruitful heat of the sun, which is her life) brings forth—day by day, month by month, year by year, and century by century. But civilized man is not content to take his share of the earth's income, he uses the garnered wealth which is the earth's capital—and this at a rate which is not only ever increasing, but is increasing at an increasing rate. The rapid consumption of coal is but a single instance of his wasteful expenditure of the stores which during countless ages have been gathered together, seemingly for the use of man. In this country (America), I need not dwell upon the fact that, in many other ways, man is consuming, if not wasting, supplies of earth-wealth which can not be replaced. It is not merely what is found within the earth, but the store of wealth which clothes the earth's surface, which is thus being exhausted. Your mighty forests seem capable of supplying all the timber that the whole race of man could need for ages; yet a very moderate computation of the rate at which they are being cut down, and will presumably continue to be, by a population increasing rapidly in numbers and in the destructive capabilities which characterize modern civilization, would show that America will be denuded of its forest-wealth in about the same period which we in England have calculated as probably limiting the effective duration of our stores of coal. That period—a thousand or twelve hundred years—may seem long compared with the life of individual men, long even compared with the duration of any nation in the height of power; but though men and nations pass away, the human race continues, and a thousand years are as less than a day in the history of that race. Looking forward to that future day, seemingly so remote, but (on the scale upon which we are at present tracing our earth's history) in reality the to-morrow of our earth, we see that either a change in their mode of civilization will be forced on the human race, or else it will then have become possible, as your Ericsson has already suggested, to make the sun's daily heat the mainspring of the machinery of civilization.

But turning from those portions of the past and future of our earth which, by comparison with the astronomical eras of her history, may be regarded as present, let us consider, so far as known facts permit, the probable future of the earth after astronomical eras comparable with those which were presented to us when we considered her past history.

One of the chief points in the progression of the earth toward her present condition was the gradual passing away of the heat with which formerly her whole globe was instinct. We have now to consider whether this process of cooling is still going on, and how far it is likely to extend. In this inquiry we must not be misled by the probable fact, for such it seems, that during hundreds of thousands of years the general warmth of the surface of the earth has not appreciably diminished. In the first place, hundreds of thousands of years are the seconds of the time-measures we have now to deal with; and next, it is known that the loss of temperature which our earth is at present undergoing chiefly affects the interior parts of her globe. The inquiries of Mallet and others show that the present vulcanian energies of the earth are due in the main to the gradual withdrawal of the earth's nuclear parts from the surface crust, because of the relatively more rapid loss of heat by the former. The surface crust is thus left to contract under the action of gravity, and vulcanian phenomena —that is, volcanoes and earthquakes—represent the mechanical equivalent of this contraction. Here is a process which can not continue forever, simply because it is in its very nature exhaustive of the energy to which it is due. It shows us that the earth's nuclear regions are parting with their heat, and as they can not part with their heat without warming the surface-crust, which nevertheless grows no warmer, we perceive that the surface heat is maintained from a source which is being gradually exhausted. The fitness of the earth to be the abode of life will not only be affected directly in this way, but will be indirectly affected by the loss of that vulcanian energy which appears to be one of its necessary conditions. At present, the surface of the earth is like the flesh clothing the living body; it does not wear out because (through the life which is within it) it undergoes continual change. But even as the body itself is consumed by natural processes so soon as life has passed from it, so, when the internal heat of the earth, which is its life, shall have passed away, her surface will "grow old as doth a garment"; and with this inherent terrestrial vitality will pass away by slow degrees the life which is upon the earth.

In dealing with the past history of our earth, we recognised a time when she was a sun, rejoicing as a giant in the strength of youth; and later we considered a time when her condition resembled that of the planets Jupiter and Saturn, whose dense atmospheres seem to be still loaded with the waters which are to form the future oceans of those noble orbs. In considering our earth's future, we may recognise in the moon's actual condition a stage through which the earth will hereafter have to pass. When the earth's inherent heat has passed away and long ages have elapsed since she had been the abode of life, we may believe that her desert continents and frost-bound oceans will in some degree resemble the arid wastes which the astronomer recognises in the lunar surface. And yet it is not to be supposed that the appearance of the earth will ever be closely similar to that presented by the moon. The earth may part, as completely as the moon has, with her internal heat; the rotation of the earth may in hundreds of millions of years be slowed down by tidal action into agreement with the period in which the moon completes her monthly orbit; and every form of animal and vegetable life may perish from off the face of the earth: yet ineffaceable traces of the long ages during which her surface was clothed with life and instinct with inherent vitality, will distinguish her from the moon, where the era of life was incomparably shorter. Even if the speculations of Stanislas Meunier be just, according to which the oceans will gradually be withdrawn beneath the surface crust and even the atmosphere almost wholly disappear, there would forever remain the signs of changes brought about by rainfall and snowfall, by wind and storm, by river and glacier, by ocean waves and ocean currents, by the presence of vegetable life and of animal life during hundreds of millions of years, and even more potently by the fiery deluge poured continually on the primeval surface of our globe. By all these causes the surface of the earth has been so wrought upon as no longer to resemble the primary igneous rock which we seem to recognise in the scarred surface of our satellite.

Dare we look onward to yet later stages in the history of our earth? Truly it is like looking beyond death; for now imagination presents our earth to us as an inert mass, not only lifeless as at the beginning, but no longer possessing that potentiality of life which existed in her substance before life appeared upon her surface. We trace her circling year after year around the sun, serving no useful purpose according to our conceptions. The energy represented by her motions of rotation and revolution seems to be as completely wasted as are those parts (the whole save only one 230,000,000th portion) of the sun's light and heat, which, falling on no planet, seem to be poured uselessly into desert space. Long as has been, and doubtless will be, the duration of life upon the earth, it seems less than a second of time compared with those two awful time-intervals—one past, when as yet life had not begun, the other still to come, when all life shall have passed away.

But we are thus led to contemplate time-intervals of a yet higher order—to consider the eras belonging to the lifetime of the solar system itself. Long after the earth shall have ceased to be the abode of life other and nobler orbs will become in their time fit to support millions of forms as well of animal as of vegetable existence; and the later each planet is in thus "putting on life," the longer will be the duration of the life-supporting era of its own existence.

Even those time-intervals will pass, however, until every orb in turn has been the scene of busy life, and has then, each after its due life-season, become inert and dead. One orb alone will then remain, on which life will be possible—the sun, the source whence life had been sustained in all those worlds. And then, after the lapse, perchance, of a lifeless interval, compared with which all the past eras of the solar system were utterly insignificant, the time will arrive when the sun will be a fit abode for living creatures. Thereafter, during ages infinite to our conceptions, the great central orb will be (as now, though in another sense) the life of the solar system. We may even look onward to still more distant changes, seeing that the solar system is itself moving on an orbit, though the centre round which it travels is so distant that as yet it remains unknown. We see in imagination change after change, cycle after cycle, till

> "Drawn on paths of never-ending duty,
> The worlds—eternity begun—
> Rest, absorbed in ever-glorious beauty,
> On the Heart of the All-Central Sun."

But in reality it is only because our conceptions are finite that we thus look forward to an end even as we seek to trace events back to a beginning. The notion is inconceivable to us that absolutely endless series of change may take place in the future and have taken place in the past; equally inconceivable is the notion that series on series of material combinations, passing onward to ever-higher orders—from planets to suns, from suns to sun-systems, from sun-systems to galaxies, from galaxies to systems of galaxies, from these to higher and higher orders, absolutely without end—may surround us on every hand. And yet, as I set out by saying, these things are not more inconceivable than infinity of time and infinity of space, while the idea that time and space are finite is not merely inconceivable, but opposed directly to what the mind conceives of space and time. It has been said that progression necessarily implies a beginning and an end; but this is not so where the progression relates to absolute space or time. No one can indeed doubt that progression in space is of its very nature limitless. But this is equally true, though not less inconceivable, of time. Progression implies only relative beginning and relative ending; but that there should be an absolute beginning or an absolute end is not merely inconceivable, like absolute eternity, but is inconsistent with the necessary conditions of the progression of time as presented to us by our conceptions. Those who can may find relief in believing in absolutely void space and absolutely unoccupied time before some very remote but not infinitely remote epoch, which may in such belief be called the beginning of all things; but the void time before that beginning can have had no beginning, unless it were preceded by time not unoccupied by events, which is inconsistent with the supposition. We find no absolute beginning if we look backward; and looking forward we not only find an absolute end inconceivable by reason, but revealed religion—as ordinarily interpreted—teaches—that on that side lies an eternity not of void but of occupied time. The time-intervals, then, which have presented themselves to our contemplation in dealing with the past and future of our earth, being in their nature finite, however vast, are less than the shortest instant in comparison with absolute time, which—endless itself—is measured by endless cycles of change. And in like manner, the space seemingly infinite from which our solar system has drawn its materials—in other words, the universe as partially revealed to us in the study of the star-depths—is but the merest point by comparison with absolute space. The end, seemingly so remote, to which our earth is tending, the end infinitely more remote to which the solar system is tending, the end of our galaxy, the end of systems of such galaxies as ours—all these endings (each one of which presents itself in turn to our conceptions as the end of the universe itself) are but the beginnings of eras comparable with themselves, even as the beginnings to which we severally trace back the history of our planet, of the planetary system, and of galaxies of such systems, are but the endings of prior conditions which have followed each other in infinite succession. The wave of life which is now passing over our earth is but a ripple in the sea of life within the solar system; this sea of life is itself but as a wavelet on the ocean of eternal life throughout the universe. Inconceivable, doubtless, are these infinities of time and space, of matter, of motion, and of life. Inconceivable that the whole universe can be for all time the scene of the operation of infinite personal power, omnipresent, all-knowing. Utterly incomprehensible how Infinite Purpose can be associated with endless material evolution. But it is no new thought, no modern discovery, that we are thus utterly powerless to conceive or comprehend the idea of an Infinite Being, Almighty, All-knowing, Omnipresent, and Eternal, of whose inscrutable purpose the material universe is the unexplained manifestation. Science is in presence of the old, old mystery; the old, old questions are asked of her: " Canst thou by searching find out God? canst thou find out the Almighty unto perfection? It is as high as heaven; what canst thou do? deeper than hell; what canst thou know? " And Science answers these questions, as they were answered of old, " As touching the Almighty, we can not find him out."

A NEW THEORY OF LIFE IN OTHER WORLDS[1]

TWO opposite views have been entertained respecting life in other worlds. One is the theory which Brewster somewhat strangely described as the creed of the philosopher and the hope of the Christian, that nearly all the orbs which people space are the abode of life. Brewster, Chalmers, Dick, and a host of other writers, have adopted and enforced this view, Brewster going so far as to maintain the probability that life may exist upon the moon, dead though her surface seems, or beneath the glowing photosphere of the sun. But even where so extreme an opinion has not been entertained, the believers in the theory of a plurality of worlds have maintained that all the celestial orbs have been created to be, and are at this present time, the abodes of life, or else minister to the wants of creatures living in other orbs. It is worthy of notice that this view has been entertained even by astronomers, who, like the Herschels, have devoted their lives to the scientific study of the heavens. So completely has the theory been identified, as it were, with modern astronomy, that we find the astronomer passing from a statement respecting some observed fact about a planet, to the consideration of the bearing of the fact on the requirements of living creatures on the planet's surface, without expressing any doubt whatever as to the existence of such creatures. For example, Sir John Herschel, writing about the rings of Saturn, after discussing Lardner's supposed demonstration that the eclipses caused by the rings would

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[1] From "Our Place among Infinities," D. Appleton and Company.

last but for a short time,[2] says: "This will not prevent, however, some considerable regions of Saturn from suffering very long total interception of the solar beams, affording to our ideas but an inhospitable asylum to animated beings, ill compensated by the feeble light of the satellites; but we shall do wrong to judge of the fitness or unfitness of their condition from what we see around us, when perhaps the very combinations which convey to our minds only images of horror may be, in reality, theatres of the most striking and glorious displays of beneficent contrivance." And many other such cases might be cited.

Before passing to the opposite view of life in other worlds, a view commonly associated with the name of the late Dr. Whewell, I shall venture to quote a few passages from his "Bridgewater Treatise on Astronomy and General Physics," in which he writes very much like a supporter of the theory he subsequently opposed in his "Plurality of Worlds." Thus, speaking of the satellites in the solar system, he says: "There is one fact which immediately arrests our attention; the number of these attendant bodies appears to increase as we proceed to planets farther and farther from the sun. Such, at least, is the general rule. Mercury and Venus, the planets near the sun, have no attendants; the earth has but one. Mars, indeed, who is still farther removed, has none, nor have the minor planets, so that the rule is only approximately verified. But Jupiter, who is at five times the earth's distance, has four satellites; and Saturn, who is again at a distance nearly twice as great, has seven" (now eight) "besides that most extraordinary phenomenon, his ring, which for purposes of illumination is equivalent to many thousand satellites. Of Uranus it is

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[2] This is disproved, and the justice of Herschel's views demontrated in Chapter VII of my treatise on Saturn, in which work I give a table of the climatic relations in Saturn (for I also once adopted the theory criticised above), the time and place of sunrise and sunset in Saturnian latitudes in Saturnian spring, summer, autumn, and winter, and so on. Labour wasted, I fear, except as practice in Geometrical Astronomy.

difficult to speak, for his great distance renders it almost impossible to observe the smaller circumstances of his condition. It does not appear at all probable that he has a ring like Saturn; but he has at least four satellites which are visible to us, at the enormous distance of nine hundred millions of miles, and I believe that the astronomer will hardly deny that he may possibly have thousands of smaller ones circulating about him. But leaving conjecture, and taking only the ascertained cases of Venus, the earth, Jupiter, and Saturn, we conceive that a person of common understanding will be strongly impressed with the persuasion that the satellites are placed in the system with a view to compensate for the diminished light of the sun at greater distances." Then he presently adds, after considering the exceptional case of Mars, " No one familiar with such contemplations will, by one anomaly, be driven from the persuasion that the end which the arrangements of the satellites seem suited to answer is really one of the ends of their creation." Here is the theory of life in other worlds definitely adopted, and moreover presented in company with the extremest form of the teleological argument, and that, too, by Whewell, whose name afterward became associated with the extremest development of the doctrine of the paucity of worlds!

The Whewellite theory is tolerably well known, though certainly it is not held in very great favour. For my own part, I used, at one time, to think that Whewell only advanced it in jest; but now (perhaps because my own researches and study have led me to regard the Brewsterian theory as untenable) I recognise in Whewell's later views the result of longer and more careful study than he had given to the subject, when (nearly a quarter of a century earlier) he wrote his " Bridgewater Treatise."

Whatever opinion we form as to the theory advanced in the " Plurality of Worlds," we must admit that Whewell did good service to science in breaking the chains of old-fashioned ideas, and inaugurating freedom of discussion. The stock writers on astronomy had been repeating so often the imperfect analogies on which astronomers had earlier insisted, that the suggestions based on such analogies had come to be regarded as so many scientific facts. The earth is a planet, and Mars is a planet, therefore what we know about the earth may be inferred respecting Mars, no account being taken of the known difference in the condition of the two planets: accordingly, not only are the white spots at the Martian poles to be regarded as snow-covered regions, and the blue markings on his surface as seas, but we are to infer a similarity of climatic conditions and other habitudes, without entering into any close consideration of the probable extent of the planet's atmosphere, the heat received from the sun by Mars, and a variety of other relations respecting which we are at least as well informed as we are respecting the analogies in question. Jupiter, again, is a planet, and though he is so much larger than the earth that we might be disposed at the outset to regard him as a body of another order, we must be so guided by analogies (which, after all, may be imaginary) as to consider that his size only renders him so much the nobler an abode for such life as we are familiar with: and instead of being struck by the fact that Jupiter, unlike Mars, shows no polar snow-caps, we are to direct our attention to his belts, and to regard them as cloud-belts analogous to the tropical cloud-zone of the earth. Nor are we to inquire too closely whether the aspect of his equatorial belt, to say nothing of his other belts, corresponds in any degree with that which the cloud zone of our earth would present to observers on another planet. Let it suffice to note a few analogies, as thus: "The earth is a planet, Jupiter is a planet; the earth rotates and therefore has a day, Jupiter rotates and has a day; the earth has a year, Jupiter has a year; the earth has clouds, Jupiter has clouds; the earth has a moon, Jupiter has four moons; this done, every other consideration may be conveniently overlooked, and we may proceed to descant on the wonderful extent and dignity of this distant world, with as little question of its being inhabited as though we had seen with our own eyes the creatures which exist upon the planet's surface. So with Saturn, and the rest."

Whewell broke through all these old-fashioned methods. He dealt with the several planets on the true scientific principle long since enunciated by Descartes, taking nothing for granted that had not been proved. He showed how unlike the conditions prevailing in the other planets must be to those existing on the earth, and without pretending to demonstrate absolutely that none of the higher forms of life can exist on certain planets, he showed that at any rate the probabilities are in favour of that hypothesis. Passing on to the stars, he did good service by showing how much had been taken for granted by astronomers in their assumptions respecting those orbs; nor is the value of his work, in this field, by any means diminished by the circumstance that during recent years evidence which was wanting when Whewell wrote has been obtained, and the stars have been shown demonstratively to be suns. And lastly, he dealt in an independent and therefore instructive manner with the star-cloudlets or nebulæ, giving many strong reasons for doubting the views which were at that time repeated in every text-book of astronomy.

The conclusions to which Whewell was led were: (1) that no sufficient reason exists for believing in other worlds than ours; and (2) if the other planets are inhabited, it can only be, in all probability, by creatures belonging to the lowest orders of animated existence. He somewhat softened the harshness of these inferences by pointing out that our conceptions of the glories of God's kingdom need not be enfeebled by our doubts as to the existence of life in the planets of our own system, or of systems circling around other suns. "However destitute," he wrote, "planets, moon, and rings may be of inhabitants, they are at least vast scenes of God's presence, and of the activity with which he carries into effect everywhere the laws of Nature; and the glory of creation arises from its being, not only the product but the constant field of God's activity and thought, wisdom, and power." And, in passing, I may note that Sir David Brewster, when commenting somewhat angrily and contemptuously on this remark, failed really to grasp Whewell's meaning. Brewster was at great pains to show how large a portion of the glories of the heavens is invisible and useless to man; but Whewell was manifestly not referring to the glories of God as revealed to man, but as they exist in themselves. It must be admitted, indeed, even by those who prefer Brewster's theory, that he maintained it with much more warmth than was necessary in such a discussion. In presence of Whewell's philosophic, calm, and dispassionate force of reasoning, there was something almost ludicrous in the impassioned outbursts of the great physicist who took the doctrine of life in other worlds under his protection. "Where," says he, "is the grandeur, where the utility, where the beauty, where the poetry, of the two almost invisible stars which usurp the celestial names of Uranus and Neptune, and which have been seen by none but a very few even of the cultivators of astronomy? The seaman in the trackless ocean never seeks their guidance; to him they have not even the value of the Pole Star; they contribute nothing to the arts of terrestrial life: they neither light the traveller on his journey, nor mark by their feeble ray the happy hours which are consecrated to friendship and to love." All this is very pretty writing, but it is very little to the purpose, and while it has no bearing whatever on what Whewell had urged, it is a very long way from establishing what Brewster desired to prove—viz., that "Uranus and Neptune must have been created for other and nobler ends; to be the abodes of life and intelligence, the colossal temples where their Creator is recognised and worshipped; the remotest watch-towers of our system, from which his works may be better studied, and his distant glories more readily described."

Here, however, are two theories—opposed to each other, and not admitting of being reconciled. If we are to make a selection between them, to which shall we turn in preference? The balance of evidence is on the whole in favour of Whewell's (so at least the matter presents itself to me after careful and long-continued study); but certainly Brewster's is the theory which commends itself most favourably to the mind which would believe that God "hath done all things well," and that nothing that he has made was made in vain. Even those who, like myself, are indisposed to admit that the ways and works of God are to be judged by our conceptions of the fitness of things (though we may be altogether certain that all things are made in wisdom and fitness), would prefer to accept the Brewsterian theory, if decision were to be made between the two. For, what amount of evidence could reconcile us to the belief (even though it forced this belief upon us) that our earth alone of all the countless orbs which people space, is the abode of reasoning creatures, capable of recognising the glories of the universe, and of lauding the Creator of those wonders and of their own selves? Nevertheless we must be guided in these matters by evidence, not by sentiment —by facts, not by our feelings. It is well, therefore, to note that the decision does not lie between the two theories which have just been dealt with. Another theory, holding a position intermediate between those two, and combining in my judgment the evidence which favours one theory with the fitness characterizing the other, remains yet to be presented.

I propose to take, as the basis of the new theory of life in other worlds, the analogy which has commonly been regarded as affording the strongest evidence in favour of the Brewsterian theory—only I shall take a more extended view of the subject than has been customary.

Before introducing that Brewsterian argument, I may remark that the mere fact that our earth is an inhabited world is not in itself sufficient even to render probable the theory that there is life in other worlds than ours. An equally strong argument might be derived against that theory from the study of our moon—the only other planet of which we have obtained reliable information—for few can suppose that the moon is fit to be the abode of life.

Since, then, of the two planets we can examine, one—the earth—is inhabited, while the other—the moon—is probably not inhabited, the only evidence we have is almost equally divided between the Whewellite and Brewsterian theories, whatever balance remains in favour of the latter being too slight to afford any sufficient basis for a conclusion.

But while this reasoning is just, as applied to the mere fact that the earth is inhabited, it is by no means capable of overthrowing the evidence which is derived from the manner in which life exists on the earth. When we consider the various conditions under which life is found to prevail, that no difference of climatic relations or of elevation, of land or of air or of water, of soil in land, of freshness or saltness in water, of density in air, appears (so far as our researches have extended) to render life impossible, we are compelled to infer that the power of supporting life is a quality which has an exceedingly wide range in Nature. I refrain, it will be noticed, from using here the usual expression, and saying, as of yore, that "the great end and aim of all the workings of Nature is to afford scope and room for the support of life," because this mode of speaking may be misunderstood. We can see what Nature actually does, and we may infer, if we so please, that such or such is the end and aim of the God of Nature; nevertheless we must remember that the evidence we have belongs to the former relation, not to the latter. I am careful to dwell on this point because the longer I study such matters the more clearly do I recognise the necessity of most studiously limiting our statements to that which the evidence before us really establishes.

Passing beyond the evidence which the earth at present affords, we find that during many ages the earth has presented a similar scene. "Geology," I wrote four years ago, "teaches us of days when this earth was peopled with strange creatures such as now are not found upon its surface. We turn our thoughts to the epochs when these monsters throve and multiplied, and picture to ourselves the appearance which our earth then presented. Strange forms of vegetation clothe the scene which the mind's eye dwells upon. The air is heavily laden with moisture to nourish the abundant flora; hideous reptiles crawl over their slimy domain, battling with each other, or with the denizens of the forest; huge batlike creatures sweep through the dusky twilight which constituted the primeval day; weird monsters pursue their prey amid the depths of ocean: and we forget, as we dwell upon the strange forms which existed in those long-past ages, that the scene now presented by the earth is no less wonderful, and that the records of our time may, perhaps, seem one day as perplexing as we now find those of the geological eras." In the past, then, as in the present, this earth was inhabited by countless millions of living creatures, and during the enormous period which has elapsed since life first appeared on the surface of the earth, myriads, if not millions, of orders of living creatures have appeared, have lived the life appointed to their order, and have vanished, or exist only under modified forms. As each individual has had its period of life, so also has each race, and we may say with the poet (noting always that the personification of Nature is but a poetical idea, and does not present any real substantive truth):

"Are God and Nature then at strife,
That Nature lends such evil dreams?
So careful of the type she seems,
So careless of the single life.

. . . .

"'So careful of the type?' but no,
From scarpèd cliff and quarried stone
She cries, 'A thousand types are gone;
I care for nothing, all shall go.'"

Abundant life, in ever-varying forms, and under all-various conditions, continuing age after age during hundreds of thousands of years, such is what our earth presents to us when we turn our thoughts to its past history. And looking forward, a similar scene is presented to our contemplation. For many a long century, probably for hundreds of thousands of years, life will continue on the earth, unless some catastrophe (the occurrence of which we have as yet no reason to anticipate) should destroy life suddenly from off her surface.

So viewing this earth, we seem to find forced upon us the belief that the support of life is the object for which the earth was created, and thus we are led to regard the other orbs which, like her, circle around a central sun, as intended to be the abode of life. The only object which, so far as we can see, the earth has fulfilled during an indefinitely long period has been to present a field, so to speak, for the support of life, nor can we recognise any other purpose which she will fulfil in the future. If we admit this, and if we also believe that God made nothing without some purpose, of course we have no choice but to admit that the purpose with which the earth was made was the support of life. And reasoning from analogy, we infer that the other planets, as well those of our own system as those which we believe to exist, " wheeling in perpetual round," as attendants upon other suns, were similarly created to be the abode of life.[3]

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[3] I shall venture to quote here the once celebrated argument advanced by Dr. Bentley in favour of the plurality of worlds: " Considering," he says, " that the soul of one virtuous and religious man is of greater worth and excellency than the sun and all his planets, and all the stars in the heavens, their usefulness to man might be the sole end of their creation if it could be proved that they were as beneficial to us as the Pole Star formerly was for navigation, or as the moon is for producing the tides and lighting us on winter nights. But we dare not undertake to show what advantage is brought to us by those innumerable stars in the galaxy of other parts of the firmament, not discernible by naked eyes, and yet each many thousand times bigger than the whole body of the earth. If you say they beget in us a great idea and veneration of the mighty Author and Governor of such stupendous bodies, and excite and devote our minds to his adoration and praise, you say very truly and well. But would it not raise in us a higher apprehension of the infinite majesty and boundless beneficence of God, to suppose that those remote and vast bodies were formed, not merely upon our account, to be peeped at through an optic glass, but for different ends and nobler purposes? And yet who will deny that there are great multitudes of lucid stars even beyond the reach of the best

But, before we infer from the strength of this reasoning that the other planets are inhabited worlds, let us look somewhat more closely into the circumstances, or rather, instead of examining only a portion of the evidence, let us take a wider survey and examine all the evidence we possess. It may appear, at a first view, that already we are dealing with periods which, to our conceptions, are practically infinite. How long, compared with the brief span of human life, are the eras with which history deals! how enormous, even by comparison with these eras, appears the range of time (tens of thousands, if not hundreds of thousands, of years) since man first appeared upon this earth! and, according to the teachings of geology, we have to deal with a yet higher order of time in passing to the beginning of life upon our globe. From one million of years to ten millions! It is between such limits, say the most experienced geologists, that the choice lies. Surely we may be content with periods such as these, periods as utterly beyond our powers of conception as the duration of the Pyramids would be to creatures like the ephemeron, did such creatures possess the power of reason!

And yet, why should we stop at the beginning of life upon this earth? We have passed to higher and higher orders of time-intervals, but the series has no limit that we know of, while it possesses terms, recognisable by us,

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telescopes; and that every visible star may have opaque planets revolving about them which we can not discover? Now, if they were not created for our sakes it is certain and evident that they were not made for their own; for matter has no life or perception, is not conscious of its own existence, nor capable of happiness, nor gives the sacrifice of praise and worship to the author of its being. It remains, therefore, that all bodies were formed for the sake of intelligent minds; and as the earth was principally designed for the being and service and contemplation of men, why may not all other planets be created for the like uses, each for its own inhabitants which have life and understanding?" The objection to Dr. Bentley's argument resides, not in the belief which he expresses in the wisdom and beneficence of the Creator, but in the confidence with which he assumes that the Creator had such and such purposes—and not perhaps others such as we not only can not discover, but can not even conceive.

of higher order than those we have been dealing with. We know that in the far-off times before life appeared,

> "The solid Earth whereon we tread
> In tracts of fluent heat began,
> And grew to seeming-random forms,
> The seeming prey of cyclic storms."

Let us look back at that part of the earth's history, and see whether the long periods which we have contemplated may not be matched and more than matched by the æons which preceded them. When we thus

> "Contemplate all this work of Time
> The giant labouring in his youth,"

we see how far we have been from recognising the true breadth of the mighty waves on one of which the life upon this earth has been borne, we see that as yet we have not

> "Come on that which is, and caught
> The deep pulsations of the world—
> Æonian music measuring out
> The steps of time."

Taking as the extremest span of the past existence of life upon the earth ten millions of years, we learn from the researches of physicists that the age preceding that of life (the age during which the world was a mass of molten rock) lasted more than thirty-five times as long, since Bischof has shown that the earth would require three hundred and fifty millions of years to cool down from a temperature of 2,000° C. to 200° C. But far back beyond the commencement of that vast era, our earth existed as a nebulous mass, nor can we form even a conjecture as yet respecting the length of time during which that earlier stage of the earth's existence continued.

So much for the past. Of the future we know less. But still we recognise, not indistinctly, a time when all life will have ceased upon the earth. Whether by the process of refrigeration which is going on, or by the gradual exhaustion of the forces which at present reside in the earth, or by the change in the length of the day which we know to be slowly taking place, a time must come when the condition of our earth will no longer be suited for the support of life. Or it may be that Stanilas Meunier is right in his theory that as a planet grows older, the oceans, and even the atmosphere, are gradually withdrawn into the interior of the planet's globe, where space is formed for them by the cooling and contracting of the solid frame of the planet. But apart from all such considerations, we know that a process of exhaustion is taking place, even in the sun himself, whence all that exists upon the earth derives its life and daily nourishment. So that indirectly by the dying out of the source of life, if not directly by the dying out of life, this earth must one day become as bleak and desolate a scene as we believe the moon to be at this present time.

It is easy to recognise the bearing of these considerations upon the question of life in other worlds. We had been led, by the contemplation of the long continuance of life upon this earth, to regard the support of life as in a sense the object of planetary existence, and therefore to view the other planets as the abode of life. But we now see that the time during which life has existed on the earth has been a mere wavelet in the sea of our earth's lifetime, this sea itself being but a minute portion of the infinite ocean of time, while, as Tyndall has well remarked, in that infinite ocean, the history of man (the sole creature known to us that can appreciate the wonders of creation) is but the merest ripple. We learn, then, from the earth's history, a lesson the very reverse of that which before we had seemed so clearly to read there. It is not the chief, but only a minute portion of the earth's existence which has been characterized by the existence of life upon our globe; and if we adopted the teaching now brought before us, as readily as before we learned that other lesson, we should say, "It is not the chief, but only an utterly subordinate part of Nature's purpose, to provide for the existence and support of life."

We have been led by the study of the probable past history of the earth, and by the consideration of her probable future fortunes, to the conclusion that although life has existed on her surface for an enormously long period, and will continue for a corresponding period in the future, yet the whole duration of life must be regarded but as a wave on the vast ocean of time, while the duration of the life of creatures capable of reasoning upon the wonders which surround them, is but as a ripple upon the surface of such a wave. It matters little then whether we take life itself, without distinction of kind or order, or whether we take only the life of man, we still find a disproportion which must be regarded as practically infinite, between the duration of such life, and the duration of the preceding and following periods when there has been and will be no such life upon the earth.

But yet, in passing, I can not but point to the fact that in considering the usual arguments of life in other worlds, I might limit myself to the existence of rational beings. It would be difficult to show that mere life, without the power which man possesses of appreciating the wonders of the universe, is a more fitting final purpose in creation than the existence of lifeless but moving masses like the suns and their attendant planets. The insect or the fish, the bird or the mammal, the minutest miscroscopic animalcule or the mightiest cetacean, may afford suggestive indications of what we describe as beneficent contrivance; yet it is hard to see in what essential respect a universe of worlds beyond our own, inhabited only by such animals, would accord better with those ideas which the believers in the plurality of worlds entertain respecting the purpose of the Almighty, than a universe with none but vegetable life, or a universe with no life at all, yet replete with wonderful and wonderfully moving masses of matter. It is rational life alone to which the arguments of our Brewsters and Chalmers really relate. Nor would it be difficult to raise here another perplexing consideration, by inquiring what degree of cultivation of the intellect in human races accords with the "argument from admiration" which the followers of Brewster delight to employ. The savage engaged in the mere effort to support life or to combat his foes, knows nothing of the glories whereof science tells us. The wonders of Nature, so far as they affect him at all, tend to give ignoble and debasing ideas of the being or beings to whose power he attributes the occurrence of natural phenomena. Nor, as we advance in the scale of civilization, do we quickly arrive at the stage where the admiration of Nature begins to be an ordinary exercise even of a few minds. Still less do we arrive quickly, even in reviewing the progress of the most civilized races, at the stage when the generality of men give much of their thoughts to the natural wonders which surround them. Is it saying too much to assert that this stage has never yet been attained by any nation, even the most advanced and the most cultured? If we limit ourselves, however, to the existence merely of some few nations, among whom the study of Nature has been more or less in vogue, how brief in the history of this earth has been the period when such nations have existed! how brief the continuance of those among such nations which belong to the past, and whose whole history is thus known to us! how few even in such nations the men who have been so deeply impressed with the wonders of Nature as to be led to the utterance of their thoughts! If the life of man is but as a ripple where life itself is as a wave on the ocean of time, surely the life of man as the student and admirer of Nature is but as the tiniest of wave-crests upon the ripple of human life.

How, then, does all this bear upon the question of life in other worlds? The answer will be manifest if we apply to these considerations the same argument which Brewster and Chalmers have applied to the evidence which indicates the enormous duration of life upon the earth. Since this enormous duration, taking life even in its most general aspect, has been shown to be as a mere nothing by comparison with the practically infinite duration of the earth without life, the argument as respects life in any other world (at least, in any world of which antecedently we know nothing) must be directly reversed. It is far more probable that that world is now passing through a part of the stage preceding the appearance of life, or of the stage following the appearance of life, than that this particular epoch belongs to the period when that particular world is inhabited. If, indeed, we had some special reason for believing that this epoch to which terrestrial life belongs has some special importance as respects the whole universe, we might feel unwilling to consider the question of life in any other world independently of preconceptions derived from our experience in this world. But I apprehend that we have no reason whatever for so believing. It appears to me that such a belief—that is, the belief that life in this earth corresponds with a period special for the universe itself—is as monstrous as the old belief that our earth is the centre of the universe. It is, in fact, a belief which bears precisely the same relation to time that the last-mentioned belief bears to space. According to one belief, the minute space occupied by our earth was regarded as the central and most important part of all space, and the only part which the Creator had specially in his plans, so to speak, in creating the universe; according to the other, the minute time occupied by the existence of life on the earth is the central and most important part of all time, and the only part during which the Creator intended that living creatures should exist anywhere. Both ideas are equally untenable, though one only has been formally discarded.

This present time, then, is a random selection, so to speak, regarded with reference to the existence of life in any other world, and being a random selection, it is much more likely to belong to the period when there is no life there. Let me illustrate my meaning by an example. Suppose I know that a friend of mine, living at a distance, will be at home for six minutes exactly, some time between noon and ten on any given day, but that I have no means of forming any opinion as to when the six minutes will be. Then, if at any given moment, say at three, I ask myself the question, "Is my friend at home?" although I can not know, I can form an opinion as to the probability of his being so. There are six hundred minutes between noon and ten, and he is to be at home only six minutes, or the one-hundredth part of the time; accordingly, the chance that he is at home is one in a hundred, or speaking in a general way it is much more likely that he is not at home than that he is. And so precisely with any given planet, apart from any evidence we may have as to its condition—what we know about life on our earth teaches us that the probability is exceedingly minute that that planet is inhabited. The argument is the favourite argument from analogy. Thus: life on our earth lasts but a very short time compared with the duration of the earth's existence; therefore life in any given planet lasts but a very short time compared with the planet's existence; accordingly, the probability that that planet is inhabited at this present moment of time is exceedingly small, being, in fact, as the number of years of life to the number of years without life, or as one chance in many hundreds at the least.

This applies to the planets of our solar system only in so far as we are ignorant of their condition. We may know enough about some of them to infer either a much higher probability that life exists, or almost certainly that life can not exist. Thus we may view the condition of Venus or Mars as perchance not differing so greatly from that of our earth as to preclude the probability that many forms of life may exist on those planets. Or, on the other hand, we may believe from what we know about Jupiter and Saturn that both these planets are still passing through the fiery stages which belong to the youth of planet life; while in our moon we may see a world long since decrepit, and now utterly unfit to support any forms of animated existence. But even in the case of our solar system, though the evidence in some cases against the possibility of life is exceedingly strong, we do not meet with a single instance in which evidence of the contrary kind is forcible, still less decisive. So that in the solar system the evidence is almost as clear in favour of the conclusion above indicated as where we reason about worlds of whose actual condition we know nothing. As respects such worlds—that is, as respects the members of those systems of worlds which circle, as we believe (from analogy), around other suns than ours—the probability that any particular world is inhabited at this present time is exceedingly small.

But let us next consider what is the probability that there is life on some member or other of a scheme of worlds circling around any given sun. Here, again, the argument is from analogy, being derived from what we have learned or consider probable in the case of our own system. And I think we may adopt as probable some such view as I shall now present. Each planet, according to its dimensions, has a certain length of planetary life, the youth and age of which include the following eras: a sunlike state; a state like that of Jupiter or Saturn, when much heat but little light is evolved; a condition like that of our earth; and lastly, the stage through which our moon is passing, which may be regarded as planetary decrepitude. In each case of world existences the various stages may be longer or shorter, as the whole existence is longer or shorter, so that, speaking generally, the period of habitability bears the same proportion in each world to the whole period of its existence; or perhaps there is no such uniform proportion, while, nevertheless, there exists in all cases that enormous excess of the period when no life is possible over the period of habitability. In either case, it is manifest that regarding the system as a whole, now one, now another planet (or more generally, now one, now another member of the system) would be the abode of life, the smaller and shorter-lived having their turn first, then larger and larger members, until life has existed on the mightiest of the planets, and even at length upon the central sun himself. We need not concern ourselves specially with the peculiarities affecting the succession of life in the case of subordinate systems, or of the members of the asteroidal family, or in other cases where we have little real knowledge to guide us: the general conclusion remains the same, that life would appear successively in planet after planet, step by step from the smaller to the larger, until the approach of the last scene of all, when life would have passed from all the planets, and our sun would alone remain to be in due time inhabited, and then in turn to pass (by time intervals to us practically infinite) to decrepitude and death.

During all this progression, the intervals without life would in all probability be far longer than those when one or other planet was inhabited. In fact, the enormous excess of the lifeless periods for our earth over the period of habitability renders the conclusion all but certain that the lifeless gaps in the history of the solar system must last very much longer than the periods of life (in this or that planet) with which they would alternate.

If we apply this conclusion to the case of any given star or sun with its scheme of dependent worlds, we see that even for a solar system so selected at random the probability of the existence of life is small. It is, of course, greater than for a single world taken at random—just as if I had ten friends who were to be at home each for six minutes between noon and ten, the chance would be greater that some one of the number would be at home at a given moment of that interval than would be the chance that a given one of the number would be then at home; while yet even taking all the ten it would still be more likely than not that at that moment not one would be at home.

Thus when we look at any star, we may without improbability infer that at the moment that star is not supporting life in any one of those worlds which probably circle round it.

Have we then been led to the Whewellite theory that our earth is the sole abode of life? Far from it. For not only have we adopted a method of reasoning which teaches us to regard every planet in existence, every moon, every sun, every orb in fact in space, as having its period as the abode of life, but the very argument from probability which leads us to regard any given sun as not the centre of a scheme in which at this moment there is life, forces upon us the conclusion that among the millions on millions, nay, the millions of millions of suns which people space, millions have orbs circling round them which are at this present time the abode of living creatures. If the chance is one in a thousand in the case of each particular star, then in the whole number (practically infinite) of stars, one in a thousand has life in the system which it rules over: and what is this but saying that millions of stars are life-supporting orbs? There is then an infinity of life around us, although we recognise infinity of time as well as infinity of space as an attribute of the existence of life in the universe. And remembering that as life in each individual is finite, in each planet finite, in each solar system finite, and in each system of stars finite, so (to speak of no higher orders) the infinity of life itself demonstrates the infinity of barrenness, the infinity of habitable worlds implies the infinity of worlds not as yet habitable, or which have long since passed their period of inhabitability. Yet is there no waste, whether of time, of space, of matter, or of force; for waste implies a tending toward a limit, and therefore of these infinities, which are without limits, there can be no waste.

MATHEMATICAL THEORIES OF THE EARTH

BY

ROBERT SIMPSON WOODWARD

THE MATHEMATICAL THEORIES OF THE EARTH[1]

THE name of this section, which by your courtesy it is my duty to address to-day, implies a community of interest among astronomers and mathematicians. This community of interest is not difficult to explain. We can, of course, imagine a considerable body of astronomical facts quite independent of mathematics. We can also imagine a much larger body of mathematical facts quite independent of and isolated from astronomy. But we never think of astronomy in the large sense without recognising its dependence on mathematics, and we never think of mathematics as a whole without considering its capital applications in astronomy.

Of all the subjects and objects of common interest to us, the earth will easily rank first. The earth furnishes us with a stable foundation for instrumental work and a fixed line of reference, whereby it is possible to make out the orderly arrangement and procession of our solar system and to gain some inkling of other systems which lie within telescopic range. The earth furnishes us with a most attractive store of real problems; its shape, its size, its mass, its precession and nutation, its internal heat, its earthquakes and volcanoes, and its origin and destiny, are to be classed with the leading questions for astronomical and mathematical research. We must of course recognise the claims of our friends the geologists to that indefinable something

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[1] Vice-presidential address before the Section of Mathematics and Astronomy of the American Association for the Advancement of Science at the Toronto meeting, August, 1889. (From the "Proceedings of the American Association for the Advancement of Science," vol. xxxviii.)

called the earth's crust, but considered in its entirety and in its relations to similar bodies of the universe, the earth has long been the special province of astronomers and mathematicians. Since the times of Galileo and Kepler and Copernicus it has supplied a perennial stimulus to observation and investigation, and it promises to tax the resources of the ablest observers and analysts for some centuries to come. The mere mention of the names of Newton, Bradley, D'Alembert, Laplace, Fournier, Gauss, and Bessel, calls to mind not only a long list of inventions and discoveries, but the most important parts of mathematical literature. In its dynamical and physical aspects the earth was to them the principal object of research, and the thoroughness and completeness of their contributions toward an explanation of the "system of the world" are still a source of wonder and admiration to all who take the trouble to examine their works.

A detailed discussion of the known properties of the earth, and of the hypotheses concerning the unknown properties, is no fit task for a summer afternoon; the intricacies and delicacies of the subject are suitable only for another season and a special audience. But it has seemed that a somewhat popular review of the state of our mathematical knowledge of the earth might not be without interest to those already familiar with the complex details, and might also help to increase that general interest in science, the promotion of which is one of the most important functions of this association.

As we look back through the light of modern analysis, it seems strange that the successors of Newton, who took up the problem of the shape of the earth, should have divided into hostile camps over the question whether our planet is elongated or flattened at the poles. They agreed in the opinion that the earth is a spheroid, but they debated, investigated, and observed for nearly half a century before deciding that the spheroid is oblate rather than oblong. This was a critical question, and its decision marks perhaps the most important epoch in the history of the figure of the earth. The Newtonian view of the oblate form found its ablest supporters in Huygens, Maupertuis, and Clairaut, while the erroneous view was maintained with great vigour by the justly distinguished Cassinian school of astronomers. Unfortunately for the Cassinians, defective measures of a meridional arc in France gave colour to the false theory and furnished one of the most conspicuous instances of the deterring effect of an incorrect observation. As you well know, the point was definitely settled by Maupertuis's measurement of the Lapland arc. For this achievement his name has become famous in literature as well as in science, for his friend Voltaire congratulated him on having "flattened the poles and the Cassinis"; and Carlyle has honoured him with the title of "Earth-flattener."[2]

Since the settlement of the question of the form—progress toward a knowledge of the size of the earth has been consistent and steady, until now it may be said that there are few objects with which we have to deal whose dimensions are so well known as the dimensions of the earth. But this is a popular statement, and, like most such, needs to be explained in order not to be misunderstood. Both the size and shape of the earth are defined by the lengths of its equatorial and polar axes; and, knowing the fact of the oblate spheroidal form, the lengths of the axes may be found within narrow limits from simple measurements conducted on the surface quite independently of any knowledge of the interior constitution of the earth. It is evident, in fact, without recourse to mathematical details, that the length of any arc, as a degree of latitude or longitude on the earth's surface, must depend on the lengths of those axes. Conversely, it is plain that the measurement of such an arc and the determination of its geographical position constitute an indirect measurement of the axes. Hence it has happened that scientific as distinguished from practical geodesy has been concerned chiefly with such

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[2] Todhunter, "History of the Theories of Attraction and the Figure of the Earth," London, 1873, vol. i, art. 195.

linear and astronomical measurements, and the zeal with which the work has been pursued is attested by triangulations on every continent. Passing over the earlier determinations as of historical interest only, all of the really trustworthy approximations to the lengths of the axes have been made within the half century just passed. The first to appear of these approximations were the well-founded values of Airy,[3] published in 1830. These, however, were almost wholly overshadowed and supplanted eleven years later by the values of Bessel,[4] whose spheroid came to occupy a most conspicuous place in geodesy for more than a quarter of a century. Knowing as we now do that Bessel's values were considerably in error, it seems not a little remarkable that they should have been so long accepted without serious question. One obvious reason is found in the fact that a considerable lapse of time was essential for the accumulation of new data, but two other possible reasons of a different character are worthy of notice because they are interesting and instructive, whether specially applicable to this particular case or not. It seems not improbable that the close agreement of the values of Airy and Bessel, computed independently and by different methods —the greatest discrepancy being about one hundred and fifty feet—may have been incautiously interpreted as a confirmation of Bessel's dimensions, and hence led to their too ready adoption. It seems also not improbable that the weight of Bessel's great name may have been too closely associated in the minds of his followers with the weights of his observations and results. The sanction of eminent authority, especially if there is added to it the stamp of an official seal, is sometimes a serious obstacle to real progress. We can not do less than accord to Bessel the first place among the astronomers and geodesists of his day, but this is no adequate justification for the exaggerated estimate long entertained of the precision of the elements of his spheroid.

The next step in the approximation was the important

[3] "Encyclopædia Metropolitana."

[4] "Astronomische Nachrichten," No. 438, 1841.

one of Clarke[5] in 1866. His new values showed an increase over Bessel's of about half a mile in the equatorial semi-axis and about three tenths of a mile in the polar semi-axis. Since 1866, General Clarke has kept pace with the accumulating data and given us so many different elements for our spheroid that it is necessary to affix a date to any of his values we may use. The later values, however, differ but slightly from the earlier ones, so that the spheroid of 1866, which has come to be pretty generally adopted, seems likely to enjoy a justly greater celebrity than that of its immediate predecessor. The probable error of the axes of this spheroid is not much greater than the hundred thousandth part,[6] and it is not likely that new data will change their lengths by more than a few hundred feet.

In the present state of science, therefore, it may be said that the first order of approximation to the form and dimensions of the earth has been successfully attained. The question which follows naturally and immediately is, How much further can the approximation be carried? The answer to this question is not yet written, and the indications are not favourable for its speedy announcement. The first approximation, as we have seen, requires no knowledge of the interior density and arrangement of the earth's mass; it proceeds on the simple assumption that the sea surface is closely spheroidal. The second approximation, if it be more than a mere interpolation formula, requires a knowledge of both density and arrangement of the constituents of the earth's mass, and especially of that part called the crust. "All astronomy," says Laplace, "rests on the stability of the earth's axis of rotation."[7] In a similar sense we may say all geodesy rests on the direction of the plumb line. The simple hypothesis of a spheroidal form assumes that

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[5] "Comparison of Standards of Length," made at the ordnance office, Southampton, England, by Captain A. R. Clarke, R. E. Published by order of the Secretary of State for War, 1866.

[6] Clarke, Colonel A. R., "Geodesy," Oxford, 1880, p. 319.

[7] "Toute l'Astronomie repose sur l'invariabilité de l'axe de rotation de la Terre à la surface du sphéroïde terrestre et sur l'uniformité de cette rotation." ("Mécanique Céleste," Paris, 1882, tome v, p. 22.)

the plumb line is everywhere coincident with the normal to the spheroid, or that the surface of the spheroid coincides with the level of the sea. But this is not quite correct. The plumb line is not in general coincident with the normal, and the actual sea level or geoid must be imagined to be an irregular surface lying partly above and partly below the ideal spheroidal surface. The deviations, it is true, are relatively small, but they are in general much greater than the unavoidable errors of observation and they are the exact numerical expression of our ignorance in this branch of geodesy. It is well known, of course, that deflections of the plumb line can sometimes be accounted for by visible masses, but on the whole it must be admitted that we possess only the vaguest notions of their cause and a most inadequate knowledge of their distribution and extent.

What is true of plumb-line deflections is about equally true of the deviations of the intensity of gravity from what may be called the spheroidal type. Given a closely spheroidal form of the sea level and it follows from the law of gravitation, as a first approximation, without any knowledge of the distribution of the earth's mass, that the increase of gravity varies as the square of the sine of the latitude in passing from the equator to the poles. This is the remarkable theorem of Stokes,[8] and it enables us to determine the form or ellipticity of the earth by means of pendulum observations alone. It must be admitted, however, that the values of the ellipticity recently obtained in this way by the highest authorities, Clarke [9] and Helmert,[10] are far from satisfactory, whether we regard them in the light of their discrepancy or in the light of the different methods of computing them. In general terms we may say that the difficulty in the way of the use of pendulum observations still hinges on the treatment of local anomalies and on the question of reduction to sea level. At present,

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[8] Stokes, G. G., "Mathematical and Physical Papers," Cambridge University Press, 1880, vol. ii.

[9] "Geodesy," chap. xiv.

[10] Helmert, Dr. F. R., "Die Mathematischen und Physikalischen Theorieen der Höheren Geodäsie," Leipsic, 1880, 1884, ii Teil.

the case is one concerning which the doctors agree neither in their diagnosis nor in their remedies.

Turning attention now from the surface toward the interior, what can be said of the earth's mass as a whole, of its laws of distribution, and of the pressures that exist at great depths? Two facts—namely, the mean density and the surface density—are roughly known; a third fact—namely, the precession constant, or the ratio of the difference of the two principal moments of inertia to the greater of them—is known with something like precision. These facts lie within the domain of observation and require only the law of gravitation for their verification. Certain inferences, also, from these facts and others, have long been and still are held to be hardly less cogent and trustworthy, but before stating them it will be well to recall briefly the progress of opinion concerning this general subject during the past century and a half.

The conception of the earth as having been primitively fluid was the prevailing one among mathematicians before Clairaut published his "Théorie de la Figure de la Terre," in 1743. By the aid of this conception Clairaut proved the celebrated theorem which bears his name, and probably no idea in the mechanics of the earth has been more suggestive and fruitful. It was the central idea in the elaborate investigations of Laplace and received at his hands a development which his successors have found it about equally difficult to displace or to improve. From the idea of fluidity spring naturally the hydrostatical notions of pressure and level surfaces, or the arrangement of fluid masses in strata of uniform density. Hence follows, also, the notion of continuity of increase in density from the surface toward the centre of the earth. All of the principal mechanical properties and effects of the earth's mass—viz., the ellipticity, the surface density, the mean density, the precession constant, and the lunar inequalities—were correlated by Laplace[11] in a single hypothesis, involving only one assumption in addition to that of original fluidity and

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[11] "Mécanique Céleste," tome v, livre xi.

the law of gravitation. This assumption relates to the compressibility of matter and asserts that the ratio of the increment of pressure to the increment of density is proportional to the density. Many interesting and striking conclusions follow readily from this hypothesis, but the most interesting and important are those relative to density and pressure, especially the latter, whose dominance as a factor in the mechanics of celestial masses seems destined to survive whether the hypothesis stands or falls. The hypothesis requires that, while the density increases slowly from something less than three at the surface to about eleven at the centre of the earth, the pressure within the mass increases rapidly below the surface, reaching a value surpassing the crushing strength of steel at the depth of a few miles and amounting at the centre to no less than three million atmospheres. The inferences, then, as distinguished from facts, are that the mass of the earth is very nearly symmetrically disposed about its centre of gravity, that pressure and density except near the surface are mutually dependent, and that the earth in reaching this stage has passed through the fluid or quasi-fluid state.

Later writers have suggested other hypotheses for a continuous distribution of the earth's mass, but none of them can be said to rival the hypothesis of Laplace. Their defects lie either in not postulating a direct connection between density and pressure or in postulating a connection which implies extreme or impossible values for these and other mechanical properties of the mass.

It is clear, from the positiveness of his language in frequent allusions to this conception of the earth, that Laplace was deeply impressed with its essential correctness. "Observations," he says, "prove incontestably that the densities of the strata (couches) of the terrestrial spheroid increase from the surface to the centre," [12] and "the regu-

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[12] "Enfin il (Newton) regarde la terre comme homogène, ce qui est contraire aux observations, qui prouvent incontestablement que les densités des couches du sphéroïde terrestre croissent de la surface au centre." ("Mécanique Céleste," tome v, p. 9.)

larity with which the observed variation in length of a second's pendulum follows the law of squares of the sines of the latitudes proves that the strata are arranged symmetrically about the centre of gravity of the earth."[13] The more recent investigations of Stokes, to which allusion has already been made, forbid our entertaining anything like so confident an opinion of the earth's primitive fluidity or of a symmetrical and continuous arrangement of its strata. But, though it must be said that the sufficiency of Laplace's arguments has been seriously impugned, we can hardly think the probability of the correctness of his conclusions has been proportionately diminished.

Suppose, however, that we reject the idea of original fluidity. Would not a rotating mass of the size of the earth assume finally the same aspects and properties presented by our planet? Would not pressure and centrifugal force suffice to bring about a central condensation and a symmetrical arrangement of strata similar at least to that required by the Laplacian hypothesis? Categorical answers to these questions can not be given at present. But, whatever may have been the antecedent condition of the earth's mass, the conclusion seems unavoidable that at no great depth the pressure is sufficient to break down the structural characteristics of all known substances, and hence to produce viscous flow whenever and wherever the stress difference exceeds a certain limit, which can not be large in comparison with the pressure. Purely observational evidence, also, of a highly affirmative kind in support of this conclusion, is afforded by the remarkable results of Tresca's experiments on the flow of solids and by the abundant proofs in geology of the plastic movements and viscous flow of rocks. With such views and facts in mind the fluid stage, considered indispensable by Laplace, does not appear

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[13] " La régularité avec laquelle la variation observée des longueurs du pendule à secondes suit la loi du carré du sinus de la latitude prouve que ces couches sont disposées régulièrement autour du centre de gravité de la terre et que leur forme est à peu près elliptique et de révolution." (" Mécanique Celéste," tome v, p. 17.)

necessary to the evolution of a planet, even if it reach the extreme refinement of a close fulfilment of some such mathematical law as that of his hypothesis. If, as is here assumed, pressure be the dominant factor in such large masses, the attainment of a stable distribution would be simply a question of time. The fluid mass might take on its normal form in a few days or a few months, whereas the viscous mass might require a few thousand or a few million years.

Some physicists and mathematicians, on the other hand, reject both the idea of existence of great pressures within the earth's mass, and the notion of an approach to continuity in the distribution of density. As representing this side of the question the views of the late M. Roche, who wrote much on the constitution of the earth, are worthy of consideration. He tells us that the very magnitude of the central pressure computed on the hypothesis of fluidity is itself a peremptory objection to that hypothesis.[14] According to his conception, the strata of the earth from the centre outward are substantially self-supporting and unyielding. It does not appear, however, that he had submitted this conception to the test of numbers, for a simple calculation will show that no materials of which we have any knowledge would sustain the stress in such shells or domes. If the crust of the earth were self-supporting, its crushing strength would have to be about thirty times that of the best cast steel, or five hundred to one thousand times that of granite. The views of Roche on the distribution of the terrestrial densities appear equally extreme.[15] He prefers to consider the mass as made up of two distinct parts, an outer shell or crust whose thickness is about one sixth of the earth's radius, and a solid nucleus having little or no central condensation. The nucleus is conceived to be purely metallic, and to have about the same density as iron. To account for geological phenomena, he postulates

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[14] "Mémoire sur l'état intérieur du globe terrestre," par M. Édouard Roche; "Mémoires de la section des sciences de l'Académie des Sciences et Lettres de Montpellier," 1880–1884, tome x.

[15] Ibid.

a zone of fusion separating the crust from the nucleus. The whole hypothesis is consistently worked out in conformity with the requirements of the ellipticity, the superficial density, the mean density, and precession; so that to one who can divest his mind of the notion that pressure and continuity are important factors in the mechanics of such masses, the picture which Roche draws out of the constitution of our planet will present nothing incongruous.

In a field so little explored and so inaccessible, though hedged about as we have seen by certain sharply limiting conditions, there is room for a wide range of opinion and for great freedom in the play of hypothesis; and although the preponderance of evidence appears to be in favour of a terrestrial mass in which the reign of pressure is well-nigh absolute, we should not be surprised a few decades or centuries hence to find many of our notions on this subject radically defective.

If the problem of the constitution and distribution of the earth's mass is yet an obscure and difficult one after two centuries of observation and investigation, can we report any greater degree of success in the treatment of that still older problem of the earth's internal heat; of its origin and effects? Concerning phenomena always so impressive and often so terribly destructive as those intimately connected with the terrestrial store of heat, it is natural that there should be a considerable variety of opinion. The consensus of such opinion, however, has long been in favour of the hypothesis that heat is the active cause of many and a potent factor in most of the grander phenomena which geologists assign to the earth's crust; and the prevailing interpretation of these phenomena is based on the assumption that our planet is a cooling sphere whose outer shell or crust is constantly cracked and crumpled in adjusting itself to the shrinking nucleus.

The conception that the earth was originally an intensely heated and molten mass appears to have first taken something like definite form in the minds of Leibnitz and

Descartes.[16] But neither of these philosophers was armed with the necessary mathematical equipment to subject this conception to the test of numerical calculation. Indeed, it was not fashionable in their day, any more than it is with some philosophers in ours, to undertake the drudgery of applying the machinery of analysis to the details of a hypothesis. Nearly a century elapsed before an order of intellects capable of dealing with this class of questions appeared. It was reserved for Joseph Fournier to lay the foundation and build a great part of the superstructure of our modern theory of heat diffusion, his avowed desire being to solve the great problem of terrestrial heat. "The question of terrestrial temperatures," he says, "has always appeared to us one of the grandest objects of cosmological studies, and we have had it principally in view in establishing the mathematical theory of heat." [17] This ambition, however, was only partly realized. Probably Fournier underestimated the difficulties of his problem, for his most ingenious and industrious successors in the same field have made little progress beyond the limits he attained. But the work he left is a perennial index to his genius. Though quite inadequately appreciated by his contemporaries, the "Analytical Theory of Heat," which appeared in 1820, is now conceded to be one of the epoch-making books. Indeed, to one who has caught the spirit of the extraordinary analysis which Fournier developed and illustrated by numerous applications in this treatise, it is evident that he opened a field whose resources are still far from being exhausted. A little later Poisson took up the same class of questions and published another great work on the mathematical theory of heat.[18] Poisson narrowly missed being

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[16] Protogée, ou de la formation et des révolutions du globe, par Leibnitz, ouvrage traduite . . . avec une introduction et des notes par le Dr. Bertrand de Saint-Germain, Paris, 1859.

[17] "La question des températures terrestres nous a toujours paru un des plus grands objets des études cosmologiques, et nous l'avions principalement en vue en établissant la théorie mathématique de la chaleur." ("Annales de Chimie et de Physique," 1824, tome xxvii, p. 159.)

[18] "Théorie Mathematique de la Chaleur," Paris, 1835.

the foremost mathematician of his day. In originality, in wealth of mathematical resources, and in breadth of grasp of physical principles he was the peer of the ablest of his contemporaries. In lucidity of exposition it would be enough to say that he was a Frenchman, but he seems to have excelled in this peculiarly national trait. His contributions to the theory of heat have been somewhat overshadowed in recent times by the earlier and perhaps more brilliant researches of Fournier, but no student can afford to take up that enticing though difficult theory without the aid of Poisson as well as Fournier.

It is natural, therefore, that we should inquire what opinions these great masters in the mathematics of heat diffusion held concerning the earth's store of heat. I say opinions, for unhappily this whole subject is still so largely a matter of opinion that, in discussing it, one may not inappropriately adopt the famous caution of Marcus Aurelius, "Remember that all is opinion." It does not appear that Fournier reached any definite conclusion on this question, though he seems to have favoured the view that the earth in cooling from an earlier state of incandescence reached finally through convection a condition in which there was a uniform distribution of heat throughout its mass. This is the consistentior status of Leibnitz, and it begins with the formation of the earth's crust, if not with the consolidation of the entire mass. It thus affords an initial distribution of heat and an epoch from which analysis may start, and the problem for the mathematician is to assign the subsequent distribution of heat and the resulting mechanical effects. But no great amount of reflection is necessary to convince one that the analysis can not proceed without making a few more assumptions. The assumptions which involve the least difficulty, and which for this reason partly have met with most favour, are that the conductivity and thermal capacity of the entire mass remain constant, and that the heat conducted to the surface of the earth passes off by the combined process of radiation, convection, and conduction, without producing any sensible effect on surrounding space. These or similar assumptions must be made before the application of theory can begin. In addition, two data are essential to numerical calculations—namely, the diffusivity, or ratio of the conductivity of the mass to its thermal capacity, and the initial uniform temperature. The first of these can be observed, approximately, at least; the second can only be estimated at present. With respect to these important points which must be considered after the adoption of the consistentior status, the writings of Fournier afford little light. He was content, perhaps, to invent and develop the exquisite analysis requisite to the treatment of such problems.

Poisson wrote much on the whole subject of terrestrial temperatures, and carefully considered most of the troublesome details which lay between his theory and its application. While he admitted the nebular hypothesis and an initial fluid state of the earth, he rejected the notion that the observed increase of underground temperature is due to a primitive store of heat. If the earth was originally fluid by reason of its heat, a supposition which Poisson regarded quite gratuitous, he conceived that it must cool and consolidate from the centre outward;[19] so that according to this view the crust of our planet arrived at a condition of stability only after the supply of heat had been exhausted. But Poisson was not at a loss to account for the observed temperature gradient in the earth's crust. Always fertile in hypotheses, he advanced the idea that there exists, by reason of interstellar radiations, great variations in the temperature of space, some vast regions being comparatively cool and others intensely hot, and that the present store of terrestrial heat was acquired by a journey of the solar system through one of the hotter regions. "Such is," he says, "in my opinion, the true cause of the augmentation of temperature which occurs as we descend below the surface of the globe."[20] This hypothesis was the result of Pois-

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[19] "Théorie Mathématique de la Chaleur," Supplément de, Paris, 1837.

[20] "Telle est, dans mon opinion, la cause véritable de l'augmentation

son's mature reflection, and as such is well worthy of attention. The notion that there exist hot foci in space was advanced also in another form in 1852 by Rankine, in his interesting speculation on the reconcentration of energy. But whatever we may think of the hypothesis as a whole, it does not appear to be adequate to the case of the earth unless we suppose the epoch of transit through the hot region exceedingly remote and the temperature of that region exceedingly high. The continuity of geological and paleontological phenomena is much better satisfied by the Leibnitzian view of an earth long subject to comparatively constant surface conditions, but still active with the energy of its primitive heat.

Notwithstanding the indefatigable and admirable labours of Fournier and Poisson in this field, it must be admitted that they accomplished little more than the preparation of the machinery with which their successors have sought and are still seeking to reap the harvest. The difficulties which lay in their way were not mathematical but physical. Had they been able to make out the true conditions of the earth's store of heat, they would undoubtedly have reached a high grade of perfection in the treatment of the problem. The theory as they left it was much in advance of observation, and the labours of their successors have therefore necessarily been directed largely toward the determination of the thermal properties of the earth's crust and mass.

Of those who in the present generation have contributed to our knowledge and stimulated the investigation of this subject, it is hardly necessary to say that we owe most to Sir William Thomson. He has made the question of terrestrial temperatures highly attractive and instructive to astronomers and mathematicians, and not less warmly interesting to geologists and paleontologists. Whether we are prepared to accept his conclusions or not, we must all

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de température qui a lieu sur chaque verticale à mesure que l'on s'abaisse au-dessous de la surface du globe." ("Théorie Mathématique de la Chaleur," Supplément de, p. 15.)

acknowledge our indebtedness to the contributions of his master hand in this field, as well as in most other fields of terrestrial physics. The contribution of special interest to us in this connection is his remarkable memoir on the secular cooling of the earth.[21] In this memoir he adopts the simple hypothesis of a solid sphere whose thermal properties remain invariable while it cools by conduction from an initial state of uniform temperature, and draws therefrom certain striking limitations on geologic time. Many geologists were startled by these limitations, and geologic thought and opinion have since been widely influenced by them. It will be of interest, therefore, to state a little more fully and clearly the grounds from which his arguments proceed. Conceive a sphere having a uniform temperature initially, to cool in a medium which instantly dissipates all heat brought by conduction to its surface, thus keeping the surface at a constant temperature. Suppose we have given the initial excess of the sphere's temperature over that of the medium. Suppose also that the capacity of the mass of the sphere for the diffusion of heat is known, and known to remain invariable during the process of cooling. This capacity is called diffusivity, and is a constant which can be observed. Then from these data the distribution of temperature at any future time can be assigned, and hence also the rate of temperature increase, or the temperature gradient, from the surface toward the centre of the sphere can be computed. It is tolerably certain that the heat conducted from the interior to the surface of the earth does not set up any reaction which in any sensible degree retards the process of cooling. It escapes so freely that, for practical purposes, we may say it is instantly dissipated. Hence, if we can assume that the earth had a specified uniform temperature at the initial epoch, and can assume its diffusivity to remain constant, the whole history of cooling is known so soon as we determine the diffusivity and the temperature gradient at any point.

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[21] "Transactions of the Royal Society of Edinburgh," 1862. Thomson and Tait's "Natural Philosophy," vol. i, part ii, Appendix D.

Now, Sir William Thomson determined a value for the diffusivity from measurements of the seasonal variations of underground temperatures, and numerous observations of the increase of temperature with depth below the earth's surface gave an average value for the temperature gradient. From these elements, and from an assumed initial temperature of 7,000° Fahr., he infers that geologic time is limited to something between 20,000,000 and 400,000,000 years. He says: "We must allow very wide limits in such an estimate as I have attempted to make; but I think we may with much probability say that the consolidation can not have taken place less than 20,000,000 years ago, or we should have more underground heat than we actually have, nor more than 400,000,000 years ago, or we should not have so much as the least observed underground increment of temperature. That is to say, I conclude that Leibnitz's epoch of emergence of the consistentior status was probably between those dates." These conclusions were announced twenty-seven years ago and were republished without modification in 1883. Recently, also, Professor Tait, reasoning from the same basis, has insisted with equal confidence on cutting down the upper limit of geologic time to some such figures as 10,000,000 or 15,000,000 years.[22] As mathematicians and astronomers, we must all confess to a deep interest in these conclusions and the hypothesis from which they flow. They are very important if true. But what are the probabilities? Having been at some pains to look into this matter, I feel bound to state that, although the hypothesis appears to be the best which can be formulated at present, the odds are against its correctness. Its weak links are the unverified assumptions of an initial uniform temperature and a constant diffusivity. Very likely these are approximations, but of what order we can not decide. Furthermore, if we accept the hypothesis, the odds appear to be against the present attainment of trustworthy numerical results, since the data for calculation, obtained mostly from observations

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[22] "Recent Advances in Physical Science," London, 1876.

on continental areas, are far too meagre to give satisfactory average values for the entire mass of the earth. In short, this phase of the case seems to stand about where it did twenty years ago, when Huxley warned us that the perfection of our mathematical mill is no guarantee of the quality of the grist, adding that, "as the grandest mill will not extract wheat flour from peascods, so pages of formulæ will not get a definite result out of loose data."[23]

When we pass from the restricted domain of quantitative results concerning geologic time to the freer domain of qualitative results of a general character, the contractional theory of the earth may be said still to lead all others, though it seems destined to require more or less modification if not to be relegated to a place of secondary importance. Old, however, as is the notion that the great surface irregularities of the earth are but the outward evidence of a crumpling crust, it is only recently that this notion has been subjected to mathematical analysis on anything like a rational basis. About three years ago Mr. T. Mellard Reade[24] announced the doctrine that the earth's crust from the joint effect of its heat and gravitation should behave in a way somewhat analogous to a bent beam, and should possess at a certain depth a "level of no strain" corresponding to the neutral surface in a beam. Above the level of no strain, according to this doctrine, the strata will be subjected to compression and will undergo crumpling, while below that level the tendency of the strata to crack and part is overcome by pressure which produces what Reade calls "compressive extension," thus keeping the nucleus compact and continuous. A little later the same idea was worked out independently by Mr. Charles Davison,[25] and it has since received elaborate mathematical

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[23] "Geological Reform" ("The Anniversary Address to the Geological Society for 1869").

[24] Reade, T. Mellard, "Origin of Mountain Ranges," London, 1886.

[25] "On the Distribution of Strain in the Earth's Crust resulting from Secular Cooling, with Special Reference to the Growth of Continents and the Formation of Mountain Chains." By Charles Davison,

treatment at the hands of Darwin,[26] Fisher,[27] and others. The doctrine requires for its application a competent theory of cooling, and hence can not be depended on at present to give anything better than a general idea of the mechanics of crumpling and a rough estimate of the magnitudes of the resulting effects. Using Thomson's hypothesis, it appears that the stratum of no strain moves downward from the surface of the earth at a nearly constant rate during the earlier stages of cooling, but more slowly during later stages; its depth is independent of the initial temperature of the earth; and if we adopt Thomson's value of the diffusivity, it will be about two and a third miles below the surface in 100,000,000 years from the beginning of cooling, and a little more than fourteen miles below the surface in 700,000,000 years. The most important inference from this theory is that the geological effects of secular cooling will be confined for a very long time to a comparatively thin crust. Thus if the earth is 100,000,000 years old, crumpling should not extend much deeper than two miles. A test to which the theory has been subjected, and one which some[28] consider crucial against it, is the volumetric amount of crumpling shown by the earth at the present time. This is a difficult quantity to estimate, but it appears to be much greater than the theory can account for.

The opponents of the contractional theory of the earth, believing it quantitatively insufficient, have recently revived and elaborated an idea first suggested by Babbage[29] and Herschel in explanation of the greater folds and movements of the crust. This idea figures the crust as being in a state bordering on hydrostatic equilibrium, which can

with a note by G. H. Darwin. "Philosophical Transactions," vol. clxxviii (1887), A, pp. 231–249.

[26] Ibid.

[27] Fisher, Rev. Osmond, "Physics of the Earth's Crust," second edition, London, 1889, chap. viii.

[28] Notably, Rev. Osmond Fisher. See his "Physics of the Earth's Crust," chap. viii.

[29] Appendix to the "Ninth Bridgewater Treatise" (by C. Babbage), second edition, London, 1838.

not be greatly disturbed without a readjustment and consequent movement of the masses involved. According to this view, the transfer of any considerable load from one area to another is followed sooner or later by a depression over the loaded area and a corresponding elevation over the unloaded one, and in a general way it is inferred that the elevation of continental areas tends to keep pace with erosion. The process by which this balance is maintained has been called isostasy,[30] and the crust is said to be in an isostatic state. The dynamics of the superficial strata with the attendant phenomena of folding and faulting are thus referred to gravitation alone, or to gravitation and whatever opposing force the rigidity of the strata may offer. In a mathematical sense, however, the theory of isostasy is in a less satisfactory state than the theory of contraction. As yet we can see only that isostasy is an efficient cause if once set in action, but how it is started and to what extent it is adequate remain to be determined. Moreover, isostasy does not seem to meet the requirements of geological continuity, for it tends rapidly toward stable equilibrium, and the crust ought therefore to reach a state of repose early in geologic time. But there is no evidence that such a state has been attained, and but little if any evidence of diminished activity in crustal movements during recent geologic time. Hence we infer that isostasy is competent only on the supposition that it is kept in action by some other cause tending constantly to disturb the equilibrium which would otherwise result. Such a cause is found in secular contraction, and it is not improbable that these two seemingly divergent theories are really supplementary.

Closely related to the questions of secular contraction and the mechanics of crust movements are those vexed questions of earthquakes, volcanism, the liquidity or solidity of the interior, and the rigidity of the earth's mass as

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[30] Dutton, Captain C. E., "On some of the Greater Problems of Physical Geology," "Bulletin Philosophical Society of Washington," vol. xi, pp. 51–64.

a whole; all questions of the greatest interest, but still lingering on the battlefields of scientific opinion. Many of the "thrice-slain" combatants in these contests would fain risk being slain again; and whether our foundation be liquid or solid, or, to speak more precisely, whether the earth may not be at once highly plastic under the action of long-continued forces and highly rigid under the action of periodic forces of short period, it is pretty certain that some years must elapse before the arguments will be convincing to all concerned. The difficulties appear to be due principally to our profound ignorance of the properties of matter subject to the joint action of great pressure and great heat. The conditions which exist a few miles beneath the surface of the earth are quite beyond the reach of laboratory tests as hitherto developed, but it is not clear how our knowledge is to be improved without resort to experiments of a scale in some degree comparable with the facts to be explained. In the mean time, therefore, we may expect to go on theorizing, adding to the long list of dead theories which mark the progress of scientific thought with the hope of attaining the truth not so much by direct discovery as by the laborious process of eliminating error.

When we take a more comprehensive view of the problems presented by the earth, and look for light on their solution in theories of cosmogony, the difficulties which beset us are no less numerous and formidable than those encountered along special lines of attack. Much progress has recently been made, however, in the elaboration of such theories. Roche,[31] Darwin,[32] and others have done much to remove the nebulosity of Laplace's nebular hypothesis.

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[31] "Essai sur la Constitution et l'origine du système solaire," par M. Edouard Roche. "Mémoires de l'Académie des Sciences et Lettres de Montpellier," tome viii, 1873.

[32] "On the Precession of a Viscous Spheroid and on the Remote History of the Earth," "Phil. Trans.," part ii, 1879. "On the Secular Changes in the Elements of the Orbit of a Satellite revolving about a Tidally Distorted Planet," "Phil. Trans.," part ii, 1880. "On the Tidal Friction of a Planet attended by Several Satellites, and on the Evolution of the Solar System," "Phil. Trans.," part ii, 1881.

Poincaré[33] and Darwin[34] have gone far toward bridging the gaps which have long rendered the theory of rotating fluid masses incomplete. Poincaré has, in fact, shown us how a homogeneous rotating mass might, through loss of heat and consequent contraction, pass from the spheroidal form to the Jacobian ellipsoidal form, and thence, by reason of its increasing speed of rotation, separate into two unequal masses. Darwin, starting with a swarm of meteorites and gravitation as a basis, has reached many interesting and instructive results in the endeavour to trace out the laws of evolution of a planetary system.[35] But notwithstanding the splendid researches of these and other investigators in this field, it must be said that the real case of the solar system, or of the earth and moon, still defies analysis; and that the mechanics of the segregation of a planet from the sun, or of a satellite from a planet, if such an event has ever happened, or the mechanics of the evolution of a solar system from a swarm of meteorites, are still far from being clearly made out.

Time does not permit me to make anything but the briefest allusion to the comparatively new science of mathematical meteorology with its already considerable list of well-defined theories pressing for acceptance or rejection. Nor need I say more with reference to those older mathematical questions of the tides and terrestrial magnetism than that they are still unsettled. These and many other questions, old and new, might serve equally well to illustrate the principal fact that this address has been designed to emphasize, namely, that the mathematical theories of the earth already advanced and elaborated are by no means complete, and that no mathematical Alexander need yet pine for other worlds to conquer.

Speculations concerning the course and progress of sci-

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[33] Sur l'équilibre d'une masse fluide animée d'un mouvement de rotation. ("Acta Mathematica," vol. vii, 1885.)

[34] "On Figures of Equilibrium of Rotating Masses of Fluid," "Phil. Trans.," vol. clxxviii, 1887.

[35] "On the Mechanical Conditions of a Swarm of Meteorites and on Theories of Cosmogony," "Phil. Trans.," vol. clxxx, 1889.

ence are usually untrustworthy if not altogether fallacious. But, being delegated for the hour to speak to and for mathematicians and astronomers, it may be permissible to offer, in closing, a single suggestion, which will perhaps help us to orient ourselves aright in our various fields of research. If the curve of scientific progress in any domain of thought could be drawn, there is every reason to believe that it would exhibit considerable irregularities. There would be marked maxima and minima in its general tendency toward the limit of perfect knowledge; and it seems not improbable that the curve would show throughout some portions of its length a more or less definitely periodic succession of maxima and minima. Races and communities as well as individuals, the armies in pursuit of truth as well as those in pursuit of plunder, have their periods of culminating activity and their periods of placid repose. It is a curious fact that the history of the mathematical theories of the earth presents some such periodicity. We have the marked maximum of the epoch of Newton near the end of the seventeenth century, with the equally marked maximum of the epoch of Laplace near the end of the eighteenth cntury; and, judging from the recent revival of geodesy and astronomy in Europe, and from the well-nigh general activity in mathematical and geological research, we may hope, if not expect, that the end of the present century will signalize a similar epoch of productive activity. The minima periods which followed the epochs of Newton and Laplace are less definitely marked but not less noteworthy and instructive. They were not periods of placid repose; to find such one must go back into the night of the middle ages; but they were periods of greatly diminished energy, periods during which those who kept alive the spirit of investigation were almost as conspicuous for their isolation as for their distinguished abilities. Many causes, of course, contributed to produce these minima periods, and it would be an interesting study in philosophic history to trace out the tendency and effect of each cause. It is desired here, however, to call attention to only one cause which contributed to the somewhat general apathy of the periods mentioned, and which always threatens to dampen the ardour of research immediately after the attainment of any marked success or advance. I refer to the impression of contentment with and acquiescence in the results of science, which seems to find easy access to trained as well as untrained minds before an investigation is half completed or even fairly begun. That some such tacit persuasion of the completeness of the knowledge of the earth has at times pervaded scientific thought, there can be no doubt. This was notably the case during the period which followed the remarkable epoch of Laplace. The profound impression of the sufficiency of the brilliant discoveries and advances of that epoch is aptly described by Carlyle in the half humorous, half sarcastic language of Sartor Resartus. " Our theory of gravitation," he says, " is as good as perfect: Lagrange, it is well known, has proved that the planetary system, on this scheme, will endure forever; Laplace, still more cunningly, even guesses that it could not have been made on any other scheme. Whereby, at least, our nautical log-books can be better kept; and water transport of all kinds has grown more commodious. Of geology and geognosy we know enough; what with the labours of our Werners and Huttons, what with the ardent genius of their disciples, it has come about that now, to many a royal society, the creation of a world is little more mysterious than the cooking of a dumpling; concerning which last, indeed, there have been minds to whom the question—How the apples were got in—presented difficulties." This was written nearly sixty years ago, about the time the sage of Ecclefechan abandoned his mathematics and astronomy for literature to become the seer of Chelsea; but the force of its irony is still applicable, for we have yet to learn, essentially, " How the apples were got in," and what kind they are.

As to the future, we can only guess, less or more vaguely, from our experience in the past and from our knowledge of present needs. Though the dawn of that future is certainly not heralded by rosy tints of overconfidence among those acquainted with the difficulties to be overcome, the prospect, on the whole, has never been more promising. The converging lights of many lines of investigation are now brought to bear on the problems presented by our planet. There is ample reason to suppose that our day will witness a fair average of those happy accidents in science which lead to the discovery of new principles and new methods. We have much to expect from the elaborate machinery and perfected methods of the older and more exact sciences of measuring and weighing—astronomy, geodesy, physics, and chemistry. We have more to expect, perhaps, from geology and meteorology, with their vast accumulation of facts not yet fully correlated. Much, also, may be anticipated from that new astronomy which looks for the secrets of the earth's origin and history in nebulous masses or in swarms of meteorites. We have the encouraging stimulus of a very general and rapidly growing popular concern in the objects of our inquiries, and the freest avenues for the dissemination of new information; so that we may easily gain the advantage of a concentration of energy without centralization of personal interests. To those, therefore, who can bring the prerequisites of endless patience and unflagging industry, who can bear alike the remorseless discipline of repeated failure and the prosperity of partial success, the field is as wide and as inviting as it ever was to a Newton or a Laplace.

THE ROTATION AND PHYSICAL CONSTITUTION OF THE PLANET MERCURY

AND

THE PLANET MARS

BY

GIOVANNI VIRGINIO SCHIAPARELLI

THE ROTATION AND PHYSICAL CONSTITUTION OF THE PLANET MERCURY[1]

AMONG the older planets no one is so difficult to observe as Mercury; and none presents so many obstacles to the investigation of its orbit as well as to the study of its physical nature. With respect to the orbit it is enough to say that Mercury is the only planet whose motions it has been declared to be impossible up to the present time to subject to the laws of universal gravitation; and the theory of whose orbit, though elaborated by the sagacious mind of a Leverrier, still presents notable discrepancies with observations. As to its physical nature, very little is known, and of that little it may be said that nearly all of it rests upon observations now a century old, made at Lilienthal by the famous Schroeter.

The telescopic examination of this planet is, in fact, most difficult. So close is its orbit to the sun that Mercury never appears in the sky far enough away from that great luminary to admit of its examination in complete darkness—at least not in our latitudes. Observations which are made in the period of twilight, before the rising or after the setting of the sun, are rarely successful, because under such circumstances the planet is always near to the horizon, and so subject to disturbances and unequal refraction in the lowest atmospheric strata, as to present for the most part in the telescope that uncertain and flaming

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[1] A discourse delivered at the meeting of the Royal Academy of the Lincei, December 8, 1889, in the presence of the King and Queen of Italy. Translated by SARA CARR UPTON.

aspect which strikes the naked eye as a bright scintillation; for that very reason the ancients called it Stilbon, which means the scintillating. Observations by night being therefore impossible, and twilight observations being rarely successful, no other way remains but to make the attempt in full daylight, in the continual presence of the sun, and through an atmosphere constantly illuminated.

Certain trials made in 1881 persuaded me that it would be possible not only to see the markings on Mercury in full daylight, but also to obtain a series of sufficiently connected and continuous observations of these spots. In the beginning of 1882 I determined to make a regular study of the planet; and in the eight following years I have had the telescope directed upon Mercury several hundreds of times, usually to little purpose, and with the loss of much time; sometimes on account of atmospheric disturbance, which during the day is often very great (especially in the summer months); and again on account of insufficient transparency of the air. Nevertheless, by employing the necessary patience, I succeeded in seeing the spots on the planet with greater or less precision on more than one hundred and fifty occasions, and also in making at such times some rather satisfactory drawings. For this purpose I used at first the smaller telescope of our observatory, made by Merz, which was often found to be inadequate for observations so difficult as these. But in the mean time the new great equatorial refractor had been installed in the observatory of Milan. It may be called the most perfect work which has yet come from the workshops of Munich. By its aid I was enabled to pursue the work with greater success, and to attain more complete and more certain results. And in regard to this refractor I can not recall without lively emotions of gratitude the warm interest shown by your Majesties, now eleven years ago, when it was a question of providing that noble instrument for the Milan Observatory. Nor is it possible for me to forget the generous eagerness with which this academy, and Quintius Sella of glorious memory at the head of it, supported the proposition with an authoritative vote, and the large majority by which it was honoured in both branches of Parliament. The new facts concerning the planet Mercury which this telescope has revealed, I consider as the most important and most precious results which have been so far obtained by its aid; so that to give the first account of these new things at this time and in this place seems to me the fulfilment of a duty.

I will first speak of the rotation of the planet, which I have found to be very different from what has been believed up to the present time, on the faith of the few and insufficient observations made a hundred years ago with imperfect telescopes. The manner and chief peculiarities of this rotation, which it has taken me many years of observation to establish, may be described in few words, by saying that Mercury revolves around the sun in a manner similar to that in which the moon revolves around the earth. As the moon describes its orbit around the earth, showing to us always very nearly the same face and the same spots, so Mercury in its orbit around the sun constantly presents to that great luminary very nearly the same hemisphere of its surface.

I have said almost the same hemisphere, and not exactly the same hemisphere. Mercury, in fact, like the moon, presents the phenomenon of libration. Observing the full moon with a small telescope at very different epochs, we shall find that, in general, the same spots occupy the central region of its disk; but, studying more minutely these central spots, and the relations of their distances from the eastern border of the moon, and from the western border, we shall soon ascertain (as did Galileo, now two hundred and fifty years ago, for the first time) that they oscillate by sensible amounts, now toward the right hand and again toward the left. This phenomenon is named the libration in longitude, and arises chiefly because the point toward which the moon perpetually and almost exactly[2] directs one of its

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[2] That is, taking no account of the slight inclination of the lunar equator with respect to the plane of its orbit, and supposing the

diameters is not the centre of the earth, neither is it the centre of the lunar elliptical orbit, but that one of the two foci of that orbit which is not occupied by the earth. This point is called by astronomers the upper focus. To any one stationed at this point the moon would therefore show always the same aspect. To us, who are, instead, on the average forty-two thousand kilometres distant from the point, the moon shows itself in slightly different aspects at different times, turning toward us now more of its eastern regions, now more of its western.

Exactly similar is the way in which Mercury presents itself to the sun during the various phases of its revolution about that body. One of the diameters of the planet is constantly directed not toward that focus of its elliptical orbit which is occupied by the sun, but toward the other focus—toward the upper focus. Now, these two foci being distant from each other not less than one fifth of the whole diameter of the orbit of Mercury, the libration of the planet is very great; and that point of Mercury which receives the solar rays vertically is projected on the surface of the planet, and oscillates along its equator in an arc which has an amplitude of forty-seven degrees—that is, more than an eighth of the whole circumference of the equator itself. The complete period of going and returning is equal to the time employed by Mercury in moving around its orbit—that is, almost eighty-eight terrestrial days. Mercury, therefore, is continually oriented to the sun as a magnetic needle to a piece of iron; but it does not point thither so constantly as not to permit of a certain oscillatory motion of the planet eastward and westward, similar to the moon's oscillation with respect to the earth.

This oscillatory motion is of the greatest importance in respect to the physical condition of the planet. Let us suppose for a moment that this motion did not exist in fact, and that Mercury always presented the same hemisphere

moon's motion in this orbit to be the so-called simple elliptic motion, in which the perturbations of the true anomaly are disregarded as well as certain terms that are of the order of the square of the eccentricity.

to the light and heat of the sun, the other hemisphere remaining wrapped in perpetual night. That point of the planet's surface which lies at the central pole of the illuminated hemisphere would eternally have the sun vertically above it; all other places upon Mercury which are reached by the sun's rays would always see the sun above the same point of the horizon, at the same altitude, without any apparent motion or sensible change of place whatever. Therefore such places would have no alternation of day and night, and no vicissitude of seasons.

Remaining thus perpetually in presence of the sun, with the stars always invisible, Mercury having no moon, it is difficult to understand how the dwellers in the regions of perpetual day could make any determinations of time (or have any chronology).

Upon Mercury things are nearly in this case, but not entirely so. The oscillating motion to which we have seen the body of the planet is subjected with respect to the sun would be attributed by an observer on the planet to the sun itself, just as we are used to attribute to the sun the diurnal motion of rotation which actually belongs to the earth. While, therefore, to us the sun seems to revolve continually from east to west, and thus determines the period of night and day to be twenty-four hours, an observer on Mercury would see the sun describe an arc of forty-seven degrees, with an alternating motion to and fro, upon the celestial vault; and this arc would remain always in the same position with respect to the horizon of the observer. A complete cycle of such double oscillations of the sun would last almost exactly eighty-eight terrestrial days; and according as the arc of the solar oscillatory motion aforesaid is all above the spectator's horizon, or all below that horizon, or partly above and partly below it, there would be different appearances and a different distribution of heat and of light. Accordingly, in those regions of Mercury where the arc of solar oscillation remains entirely below the local horizon, the sun will never be seen, and there will be continual darkness. In such regions, which occupy nearly three eighths of all the planet, the dense and eternal night can never be abated except by occasional sources of light, such as refraction and atmospheric twilights, by polar auroras, or similar phenomena, to which may be added the faint light afforded by the stars and planets. Another region of Mercury which also comprises three eighths of the whole surface will have the entire arc of solar oscillation above the horizon, and it will be continually exposed to the rays of the sun, without any variation other than that of their greater or less obliquity during the various phases of the period of eighty-eight days: for such a region no night will be possible. And, lastly, there are other regions, comprising in all a fourth part of the whole planet, for which the arc of the apparent oscillation of the sun is in part above the horizon, and part below. For these places alone alternations of light and darkness will be possible. In these privileged regions the entire period of eighty-eight terrestrial days will be divided into two intervals: one all light, the other all darkness; the duration of each will be equal at certain places; in others, instead, light or darkness will prevail in greater or less degree, according to the position of the place upon the surface of Mercury, and according as a larger or smaller portion of the arc before described remains above its horizon.

Upon a planet where affairs are so ordered the possibility of organic life depends upon the existence of an atmosphere capable of distributing the solar heat over different regions so as to modify the extraordinary excesses of heat and of cold. The presence of such an atmosphere upon Mercury was conjectured by Schroeter a century ago; in my observations I find more evident indications of it, which concur in making the probability of its existence almost a certainty.

One of the first proofs springs from the continually observed fact that the markings on the planet, visible for the most part when they are found in the central regions of the disk, are less visible or even disappear as they approach its circular borders. I have been able to convince myself that this does not occur simply from the greater obliquity of the line of sight due to perspective, but from the fact that in that perimetral position there is actually a greater hindrance to the vision, and this seems to be due to nothing else but the greater length of the path which the visual rays coming from the non-central spot must pass through in the atmosphere of Mercury to reach the eye. And from this I conclude that the atmosphere of Mercury is less transparent than that of Mars, and that it more nearly resembles, in that regard, that of the earth.

In addition to this, the circular border of the planet, where the spots become less visible, always appears more luminous than the rest of the disk. It is often irregularly luminous, in certain parts more so, in others less; and sometimes along this edge rather brilliant white areas may be seen which remain visible for several consecutive days, but which nevertheless are, in general, changeable, now in one portion and now in another. I attribute this fact to condensations in the interior of the atmosphere of Mercury, which must reflect the solar rays outward toward celestial space, and more strongly as they become more opaque. Such white areas are also often seen in the more central parts of the disk, but in that case they are not so brilliant as upon the border.

But there is still more. The dark spots of the planet, although permanent in form and arrangement, are not always equally apparent, but are sometimes more intense and sometimes more faint; and it also happens that some of these markings occasionally become entirely invisible. This I can not attribute to any more obvious cause than to atmospheric condensations similar to our terrestrial clouds, which prevent more or less completely any view of the true surface of Mercury in any portion. The clouded regions of the earth must present an absolutely identical appearance to a person viewing them from the depths of celestial space.

Concerning the nature of the surface of Mercury very little can be ascertained from the observations so far made. Thus we have to note that three eighths of its surface remain inaccessible to the solar rays, and hence to our vision also; and there is very little hope of ever knowing anything about it with certainty. But, nevertheless, it will be easy to reach precise and certain knowledge of the portion visible to us. The dark spots, even when they are not obscured by atmospheric condensation in the manner mentioned above, appear always under the form of bands of extremely light shadings, which under ordinary circumstances can only be observed with much difficulty and great attention. Upon more favourable occasions these shadings have a warm brown tint like sepia, which nevertheless is never greatly different from the general colour of the planet. This is usually of a light rose tint, tending toward a copper colour. It is most difficult to give a satisfactory graphic representation of such vague and diffused forms or bands specially from the want of fixity of the edges which always leaves room for a certain choice. Such indeterminate edges, however, I have reason to believe, in most cases are mere appearances arising from insufficient optical power of the instrument; because the more beautiful is the image and the more perfect the vision, the more manifest is the tendency of the shadings to dissolve into a number of minute particles. And there is no doubt that by using a more powerful telescope all would appear resolved into minuter forms; exactly as with a simple opera-glass we may see those irregular and indistinct masses of shading which every one can see with the naked eye upon the moon resolved into much smaller parts.

Considering the difficulty of making a proper study of the dark spots of Mercury, it is not easy to express a well-founded opinion on their nature. They might simply depend upon the diverse material and structure of the solid superficial strata, as we know to be the case with the moon. But if any one, taking into account the fact that there exists an atmosphere upon Mercury capable of condensation and perhaps also of precipitation, should hold the opinion that there was something in those dark spots analogous to our seas, I do not think that a conclusive argument to the contrary could be advanced. And as those spots are not grouped in great masses, but are dispersed about in areas and zones of no great width, much ramified, and alternated with clear spaces with some uniformity, it might be concluded that no vast oceans and great continents exist upon Mercury, but that the liquid and solid areas mingle with each other in reciprocal ways and with frequent vicissitudes, thus giving rise to a condition of things very different from that which exists upon the earth, and one which perhaps we might envy.

At any rate, we have in the case of Mercury (as in Mars), a world which is sufficiently diverse from our own; which receives light and heat from the sun, not only in a greater amount but in a different manner than the earth; and where life, if so be life exists there, finds conditions so different from those to which we are accustomed that we can scarcely imagine them. The perpetual presence of the sun almost vertically above certain regions, and its perpetual absence from other regions, appears to us to be something intolerable. But we must recollect that such a contrast should produce an atmospheric circulation which is at the same time stronger, more rapid, and more regular than that which sows the elements of life on the earth; and that on this account it may come about that an equilibrium of temperature is produced quite as complete as ours, and possibly even more so.

Mercury is conspicuously distinguished from the other planets by the manner of its revolution around the sun, turning always the same face toward it. All the other planets (at least so far as is ascertained in the cases which it has been possible to determine) revolve rapidly around their axes in the space of a few hours. Mercury's manner of revolution, however, which is unique among the planets, seems, on the other hand, quite usual for the satellites; such at least is true in all the cases where it has been possible to investigate the rotation motion of a satellite. That our own satellite has always in the memory of man shown to the earth the same hemisphere, is certain also from historical testimony, because Dante speaks of "Cain and the Thorns," and among the smaller works of Plutarch there is one entitled "Of the Face to be seen in the Disk of the Moon." That the satellites of Jupiter show always the same face to their chief planet is probable as to the first three, and for the fourth it is absolutely proved by the observations of Auwers and Engelmann. The same fact had been discovered by William Herschel in the case of Japetus, the eighth and most distant satellite of Saturn. That which would seem to be the general rule for the satellites is therefore, as exemplified in the case of Mercury, the exception among the planets.

Such an exception, however, seems not without cause, and it is probably connected with the fact of Mercury's great proximity to the sun, and perhaps also with the other fact that Mercury is without satellites. In my opinion, it depends also upon the way in which Mercury was generated at the time when the solar system took its present form. The singularity of Mercury constitutes, therefore, a new document to add to those which must be considered in the study of the solar and planetary cosmogony.

THE PLANET MARS[1]

MANY of the first astronomers who studied Mars with the telescope had noted on the outline of its disk two brilliant white spots of rounded form and of variable size. In process of time it was observed that while the ordinary spots upon Mars were displaced rapidly in consequence of its daily rotation, changing in a few hours both their position and their perspective, the two white spots remained sensibly motionless at their posts. It was concluded rightly from this that they must occupy the poles of rotation of the planet, or at least must be found very near to them. Consequently they were given the name of polar caps or spots. And not without reason is it conjectured that these represent upon Mars that immense mass of snow and ice which still to-day prevents navigators from reaching the poles of the earth. We are led to this conclusion not only by the analogy of aspect and of place, but also by another important observation.

As things stand, it is manifest that if the above-mentioned white polar spots of Mars represent snow and ice they should continue to decrease in size with the approach of summer in those places and increase during the winter. Now this very fact is observed in the most evident manner. In the second half of the year 1892 the southern polar cap was in full view; during that interval, and especially in the months of July and August, its rapid diminution from week to week was very evident to those observ-

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[1] From "Natura ed Arte," February 15, 1893. Translated by WILLIAM H. PICKERING.

ing with common telescopes. This snow (for we may well call it so), which in the beginning reached as far as latitude 70°, and formed a cap of over 2,000 kilometres (1,200 miles) in diameter, progressively diminished, so that two or three months later little more of it remained than an area of perhaps 300 kilometres (180 miles) at the most, and still less was seen in the last days of 1892. In these months the southern hemisphere of Mars had its summer, the summer solstice occurring upon October 13th. Correspondingly the mass of snow surrounding the northern pole should have increased; but this fact was not observable, since that pole was situated in the hemisphere of Mars which was opposite to that facing the earth. The melting of the northern snow was seen in its turn in the years 1882, 1884, and 1886.

These observations of the alternate increase and decrease of the polar snows are easily made even with telescopes of moderate power, but they become much more interesting and instructive when we can follow assiduously the changes in their more minute particulars, using larger instruments. The snowy regions are then seen to be successively notched at their edges; black holes and huge fissures are formed in their interiors; great isolated pieces many miles in extent stand out from the principal mass and, dissolving, disappear a little later. In short, the same divisions and movements of these icy fields present themselves to us at a glance that occur during the summer of our own arctic regions, according to the descriptions of explorers.

The southern snow, however, presents this peculiarity: The centre of its irregularly rounded figure does not coincide exactly with the pole, but is situated at another point, which is nearly always the same, and is distant from the pole about 300 kilometres (180 miles) in the direction of the Mare Erythræum. From this we conclude that when the area of the snow is reduced to its smallest extent the south pole of Mars is uncovered, and therefore, perhaps, the problem of reaching it upon this planet is easier than upon the earth. The southern snow is in the midst of a huge dark spot, which with its branches occupies nearly one third of the whole surface of Mars, and is supposed to represent its principal ocean. Hence the analogy with our arctic and antarctic snows may be said to be complete, and especially so with the antarctic one.

The mass of the northern snow cap of Mars is, on the other hand, centred almost exactly upon its pole. It is located in a region of yellow colour, which we are accustomed to consider as representing the continent of the planet. From this arises a singular phenomenon which has no analogy upon the earth. At the melting of the snows accumulated at that pole during the long night of ten months and more the liquid mass produced in that operation is diffused around the circumference of the snowy region, converting a large zone of surrounding land into a temporary sea and filling all the lower regions. This produces a gigantic inundation, which has led some observers to suppose the existence of another ocean in those parts, but which does not really exist in that place, at least as a permanent sea. We see then (the last opportunity was in 1884) the white spot of the snow surrounded by a dark zone, which follows its perimeter in its progressive diminution, upon a circumference ever more and more narrow. The outer part of this zone branches out into dark lines, which occupy all the surrounding region, and seem to be distributary canals by which the liquid mass may return to its natural position. This produces in these regions very extensive lakes, such as that designated upon the map by the name of Lacus Hyperboreus; the neighbouring interior sea called Mare Acidalium becomes more black and more conspicuous. And it is to be remembered as a very probable thing that the flowing of this melted snow is the cause which determines principally the hydrographic state of the planet and the variations that are periodically observed in its aspect. Something similar would be seen upon the earth if one of our poles came to be located suddenly in the centre of Asia or of Africa. As things stand at present, we may find a miniature image of these conditions in the flooding that is observed in our streams at the melting of the Alpine snows.

Travellers in the arctic regions have frequent occasion to observe how the state of the polar ice at the beginning of the summer, and even at the beginning of July, is always very unfavourable to their progress. The best season for exploration is in the month of August, and September is the month in which the trouble from the ice is the least. Thus in September our Alps are usually more practicable than at any other season. And the reason for it is clear—the melting of the snow requires time; a high temperature is not sufficient; it is necessary that it should continue, and its effect will be so much the greater, as it is the more prolonged. Thus, if we could slow down the course of our season so that each month should last sixty days instead of thirty, in the summer, in such a lengthened condition, the melting of the ice would progress much further, and perhaps it would not be an exaggeration to say that the polar cap at the end of the warm season would be entirely destroyed. But one can not doubt, in any case, that the fixed portion of such a cap would be reduced to much smaller size than we see it to-day. Now, this is exactly what happens on Mars. The long year, nearly double our own, permits the ice to accumulate during the polar night of ten or twelve months, so as to descend in the form of a continuous layer as far as parallel 70°, or even farther. But in the day which follows, of twelve or ten months, the sun has time to melt all, or nearly all, of the snow of recent formation, reducing it to such a small area that it seems to us no more than a very white point. And perhaps this snow is entirely destroyed; but of this there is at present no satisfactory observation.

Other white spots of a transitory character and of a less regular arrangement are formed in the southern hemisphere upon the islands near the pole, and also in the opposite hemisphere whitish regions appear at times surrounding the north pole and reaching to 50° and 55° of latitude.

They are, perhaps, transitory snows, similar to those which are observed in our latitudes. But also in the torrid zone of Mars are seen some very small white spots more or less persistent; among others one was seen by me in three consecutive oppositions (1877–1882) at the point indicated upon our chart by longitude 268° and latitude 16° north. Perhaps we may be permitted to imagine in this place the existence of a mountain capable of supporting extensive ice fields. The existence of such a mountain has also been suggested by some recent observers upon other grounds.

As has been stated, the polar snows of Mars prove in an incontrovertible manner that this planet, like the earth, is surrounded by an atmosphere capable of transporting vapour from one place to another. These snows are, in fact, precipitations of vapour, condensed by the cold and carried with it successively. How carried with it if not by atmospheric movement? The existence of an atmosphere charged with vapour has been confirmed also by spectroscopic observations, principally those of Vogel, according to which this atmosphere must be of a composition differing little from our own, and, above all, very rich in aqueous vapour. This is a fact of the highest importance, because from it we can rightly affirm with much probability that to water and to no other liquid is due the seas of Mars and its polar snows. When this conclusion is assured beyond all doubt, another one may be derived from it of not less importance —that the temperature of the Arean climate, notwithstanding the greater distance of that planet from the sun, is of the same order as the temperature of the terrestrial one. Because, if it were true, as has been supposed by some investigators, that the temperature of Mars was on the average very low (from 50° to 60° below zero), it would not be possible for water vapour to be an important element in the atmosphere of that planet, nor could water be an important factor in its physical changes, but would give place to carbonic acid, or to some other liquid whose freezing point was much lower.

The elements of the meteorology of Mars seem, then, to have a close analogy to those of the earth. But there are not lacking, as might be expected, causes of dissimilarity. From circumstances of the smallest moment Nature brings forth an infinite variety in its operations. Of the greatest influence must be different arrangement of the seas and the continents upon Mars and upon the earth, regarding which a glance at the map will say more than would be possible in many words. We have already emphasized the fact of the extraordinary periodical flood, which at every revolution of Mars inundates the northern polar region at the melting of the snow. Let us now add that this inundation is spread out to a great distance by means of a network of canals, perhaps constituting the principal mechanism (if not the only one) by which water (and with it organic life) may be diffused over the arid surface of the planet. Because on Mars it rains very rarely, or perhaps even it does not rain at all. And this is the proof.

Let us carry ourselves in imagination into celestial space, to a point so distant from the earth that we may embrace it all at a single glance. He would be greatly in error who had expected to see reproduced there upon a great scale the image of our continents with their gulfs and islands and with the seas that surround them which are seen upon our artificial globes. Then without doubt the known forms or part of them would be seen to appear under a vaporous veil, but a great part (perhaps one half) of the surface would be rendered invisible by the immense fields of cloud, continually varying in density, in form, and in extent. Such a hindrance, most frequent and continuous in the polar regions, would still impede nearly half the time the view of the temperate zones, distributing itself in capricious and ever-varying configurations. The seas of the torrid zone would be seen to be arranged in long parallel layers, corresponding to the zone of equatorial and tropical calms. For an observer placed upon the moon the study of our geography would not be so simple an undertaking as one might at first imagine.

There is nothing of this sort in Mars. In every climate and under every zone its atmosphere is nearly perpetually clear and sufficiently transparent to permit one to recognise at any moment whatever the contours of the seas and continents and, more than that, even the minor configurations. Not, indeed, that vapours of a certain degree of opacity are lacking, but they offer very little impediment to the study of the topography of the planet. Here and there we see appear from time to time a few whitish spots, changing their position and their form, rarely extending over a very wide area. They frequent by preference a few regions, such as the islands of the Mare Australe, and on the continents the regions designated on the map with the names of Elysium and Tempe. Their brilliancy generally diminishes and disappears at the meridian hour of the place, and is re-enforced in the morning and evening with very marked variations. It is possible that they may be layers of cloud, because the upper portions of terrestrial clouds where they are illuminated by the sun appear white. But various observations lead us to think that we are dealing rather with a thin veil of fog instead of a true nimbus cloud, carrying storms and rain. Indeed, it may be merely a temporary condensation of vapour under the form of dew or hoar frost.

Accordingly, as far as we may be permitted to argue from the observed facts, the climate of Mars must resemble that of a clear day upon a high mountain. By day a very strong solar radiation, hardly mitigated at all by mist or vapour; by night a copious radiation from the soil toward celestial space, and because of that a very marked refrigeration. Hence a climate of extremes, and great changes of temperature from day to night, and from one season to another. And as on the earth at altitudes of 5,000 and 6,000 metres (17,000 to 20,000 feet) the vapour of the atmosphere is condensed only into the solid form, producing those whitish masses of suspended crystals which we call cirrus clouds, so in the atmosphere of Mars it would be rarely possible (or would even be impossible) to find collections of cloud capable of producing rain of any consequence. The variation of the temperature from one season to another would be notably increased by their long duration, and thus we can understand the great freezing and melting of the snow, which is renewed in turn at the poles at each complete revolution of the planet around the sun.

As our chart demonstrates, in its general topography Mars does not present any analogy with the earth. A third of its surface is occupied by the great Mare Australe, which is strewn with many islands, and the continents are cut up by gulfs and ramifications of various forms. To the general water system belongs an entire series of small internal seas, of which the Hadriacum and the Tyrrhenum communicate with it by wide mouths, while the Cimmerium, the Sirenum, and the Solis Lacus are connected with it only by means of narrow canals. We shall notice in the first four a parallel arrangement, which certainly is not accidental, as also not without reason is the corresponding position of the peninsulas of Ausonia, Hesperia, and Atlantis. The colour of the seas of Mars is generally brown, mixed with gray, but not always of equal intensity in all places, nor is it the same in the same place at all times. From an absolute black it may descend to a light gray or to an ash colour. Such a diversity of colours may have its origin in various causes, and is not without analogy also upon the earth, where it is noted that the seas of the warm zone are usually much darker than those nearer the pole. The water of the Baltic, for example, has a light, muddy colour that is not observed in the Mediterranean. And thus in the seas of Mars we see the colour become darker when the sun approaches their zenith, and summer begins to rule in that region.

All of the remainder of the planet, as far as the north pole, is occupied by the mass of the continents, in which, save in a few areas of relatively small extent, an orange colour predominates, which sometimes reaches a dark-red tint, and in others descends to yellow and white. The variety in this colouring is in part of meteorological origin, in part it may depend on the diverse nature of the soil, but upon its real cause it is not as yet possible to frame any very well grounded hypothesis. Nevertheless, the cause of this predominance of the red and yellow tints upon the surface of ancient Pyrois is well known.[2] Some have thought to attribute this colouring to the atmosphere of Mars, through which the surface of the planet might be seen coloured, as any terrestrial object becomes red when seen through red glass. But many facts are opposed to this idea, among others that the polar snows appear always of the purest white, although the rays of light derived from them traverse twice the atmosphere of Mars under great obliquity. We must then conclude that the Arean continents appear red and yellow because they are so in fact.

Besides these dark and light regions, which we have described as seas and continents, and of whose nature there is at present scarcely left any room for doubt, some others exist, truly of small extent, of an amphibious nature, which sometimes appear yellowish like the continents, and are sometimes clothed in brown (even black in certain cases), and assume the appearance of seas, while in other cases their colour is intermediate in tint, and leaves us in doubt to which class of regions they may belong. Thus all the islands scattered through the Mare Australe and the Mare Erythræum belong to this category; so too the long peninsula called Deucalionis Regio and Pyrrhæ Regio, and in the vicinity of the Mare Acidalium the regions designated by the names of Baltia and Nerigos. The most natural idea, and the one to which we should be led by analogy, is to suppose these regions to represent huge swamps, in which the variation in depth of the water produces the diversity of colours. Yellow would predominate in those parts where the depth of the liquid layer was reduced to little or nothing, and brown, more or less dark, in those places where the water was sufficiently deep to absorb more light and to render the bottom more or less invisible. That the water of the sea, or any other deep and transparent

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[2] Pyrois I take to be some terrestrial region, although I have not been able to find any translation of the name.—TRANSLATOR.

water, seen from above, appears more dark the greater the depth of the liquid stratum, and that the land in comparison with it appears bright under the solar illumination, is known and confirmed by certain physical reasons. The traveller in the Alps often has occasion to convince himself of it, seeing from the summits the deep lakes with which the region is strewn extending under his feet as black as ink, while in contrast with them even the blackest rocks illumined by the sunlight appeared brilliant.[3]

Not without reason, then, have we hitherto attributed to the dark spots of Mars the part of seas, and that of continents to the reddish areas which occupy nearly two thirds of all the planet, and we shall find later other reasons which confirm this method of reasoning. The continents form in the northern hemisphere a nearly continuous mass, the only important exception being the great lake called the Mare Acidalium, of which the extent may vary according to the time, and which is connected in some way with the inundations which we have said were produced by the melting of the snow surrounding the north pole. To the system of the Mare Acidalium undoubtedly belong the temporary lake called Lacus Hyperboreus and the Lacus Niliacus. This last is ordinarily separated from the Mare Acidalium by means of an isthmus or regular dam, of which the continuity was only seen to be broken once for a short time in 1888. Other smaller dark spots are found here and there in the continental area which we may designate as lakes, but they are certainly not permanent lakes like ours, but are variable in appearance and size according to the seasons, to the point of wholly disappearing under certain circumstances. Ismenius Lacus, Lunæ Lacus, Trivium Charontis, and Propontis are the most conspicuous and durable ones. There are also smaller ones, such as Lacus Moeris

[3] This observation of the dark colour which deep water exhibits when seen from above is found already noted by the first author of antique memory, for in the " Iliad " (verses 770, 771 of book v) it is described how " the sentinel from the high sentry box extends his glance over the wine-coloured sea, *οἴνοπα πόντον*." In the version of Monti the adjective indicating the colour is lost.

and Fons Juventæ, which at their maximum size do not exceed 100 to 150 kilometres (60 to 90 miles) in diameter, and are among the most difficult objects upon the planet.

All the vast extent of the continents is furrowed upon every side by a network of numerous lines or fine stripes of a more or less pronounced dark colour, whose aspect is very variable. These traverse the planet for long distances in regular lines that do not at all resemble the winding courses of our streams. Some of the shorter ones do not reach 500 kilometres (300 miles), others, on the other hand, extend for many thousands, occupying a quarter or sometimes even a third of a circumference of the planet. Some of these are very easy to see, especially that one which is near the extreme left-hand limit of our map, and is designated by the name of Nilosyrtis. Others in turn are extremely difficult, and resemble the finest thread of spider's web drawn across the disk. They are subject also to great variations in their breadth, which may reach 200 or even 300 kilometres (120 to 180 miles) for the Nilosyrtis, while some are scarcely 30 kilometres (18 miles) broad.

These lines or stripes are the famous canals of Mars, of which so much has been said. As far as we have been able to observe them hitherto, they are certainly fixed configurations upon the planet. The Nilosyrtis has been seen in that place for nearly one hundred years, and some of the others for at least thirty years. Their length and arrangement are constant, or vary only between very narrow limits. Each of them always begins and ends between the same regions. But their appearance and their degree of visibility vary greatly, for all of them, from one opposition to another, and even from one week to another, and these variations do not take place simultaneously and according to the same laws for all, but in most cases happen apparently capriciously, or at least according to laws not sufficiently simple for us to be able to unravel. Often one or more become indistinct, or even wholly invisible, while others in their vicinity increase to the point of becoming conspicuous even in telescopes of moderate power. The first of our maps shows all those that have been seen in a long series of observations. This does not at all correspond to the appearance of Mars at any given period, because generally only a few are visible at once.[4]

Every canal (for now we shall so call them) opens at its ends either into a sea, or into a lake, or into another canal, or else into the intersection of several other canals. None of them have yet been seen cut off in the middle of the continent, remaining without beginning or without end. This fact is of the highest importance. The canals may intersect among themselves at all possible angles, but by preference they converge toward the small spots to which we have given the name of lakes. For example, seven are seen to converge in Lacus Phœnicis, eight in Trivium Charontis, six in Lunæ Lacus, and six in Ismenius Lacus.

The normal appearance of a canal is that of a nearly uniform stripe, black, or at least of a dark colour, similar to that of the seas, in which the regularity of its general course does not exclude small variations in its breadth and small sinuosities in its two sides. Often it happens that such a dark line opening out upon the sea is enlarged into the form of a trumpet, forming a huge bay, similar to the estuaries of certain terrestrial streams. The Margaritifer Sinus, the Aonius Sinus, the Auroræ Sinus, and the two horns of the Sabæus Sinus are thus formed, at the mouths

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[4] In a footnote the author refers to a drawing of Mars made by himself, September 15, 1892, and says: ". . . At the top of the disk the Mare Erythræum and the Mare Australe appear divided by a great curved peninsula, shaped like a sickle, producing an unusual appearance in the area called Deucalionis Regio, which was prolonged that year so as to reach the islands of Noachis and Argyre. This region forms with them a continuous whole, but with faint traces of separation occurring here and there in a length of nearly six thousand kilometres (four thousand miles). Its colour, much less brilliant than that of the continents, was a mixture of their yellow with the brownish gray of the neighbouring seas." The interesting feature of this note is the remark that it was an unusual appearance, the region referred to being that in which the central branch of the fork of the Y appeared. Since no such branch was conspicuously visible this year, it would therefore seem from the above that it was the opposition of 1892 that was peculiar, and not the present one.—TRANSLATOR.

of one or more canals, opening into the Mare Erythræum or into the Mare Australe. The largest example of such a gulf is the Syrtis Major, formed by the vast mouth of the Nilosyrtis, so called. This gulf is not less that 1,800 kilometres (1,100 miles) in breadth, and attains nearly the same depth in a longitudinal direction. Its surface is little less than that of the Bay of Bengal. In this case we see clearly the dark surface of the sea continued without apparent interruption into that of the canal. Inasmuch as the surfaces called seas are truly a liquid expanse, we can not doubt that the canals are a simple prolongation of them, crossing the yellow areas or continents.

Of the remainder, that the lines called canals are truly great furrows or depressions in the surface of the planet, destined for the passage of the liquid mass and constituting for it a true hydrographic system, is demonstrated by the phenomena which are observed during the melting of the northern snows. We have already remarked that at the time of melting they appeared surrounded by a dark zone, forming a species of temporary sea. At that time the canals of the surrounding region become blacker and wider, increasing to the point of converting at a certain time all of the yellow region comprised between the edge of the snow and the parallel of 60° north latitude into numerous islands of small extent. Such a state of things does not cease until the snow, reduced to its minimum area, ceases to melt. Then the breadth of the canals diminishes, the temporary sea disappears, and the yellow region again returns to its former area. The different phases of these vast phenomena are renewed at each return of the seasons, and we were able to observe them in all their particulars very easily during the oppositions of 1882, 1884, and 1886, when the planet presented its northern pole to terrestrial spectators. The most natural and the most simple interpretation is that to which we have referred, of a great inundation produced by the melting of the snows; it is entirely logical and is sustained by evident analogy with terrestrial phenomena. We conclude, therefore, that the canals are such in fact and not only in name. The network formed by these was probably determined in its origin in the geological state of the planet, and has come to be slowly elaborated in the course of centuries. It is not necessary to suppose them the work of intelligent beings, and, notwithstanding the almost geometrical appearance of all of their system, we are now inclined to believe them to be produced by the evolution of the planet, just as on the earth we have the English Channel and the channel of Mozambique.

It would be a problem not less curious than complicated and difficult to study the system of this immense stream of water, upon which perhaps depends principally the organic life upon the planet, if organic life is found there. The variations of their appearance demonstrated that this system is not constant. When they become displaced, or their outlines become doubtful and ill defined, it is fair to suppose that the water is getting low or is even entirely dried up. Then, in place of the canals there remains either nothing or at most stripes of yellowish colour differing little from the surrounding background. Sometimes they take on a nebulous appearance, for which at present it is not possible to assign a reason. At other times true enlargements are produced, expanding to 100, 200, or more kilometres (60 to 120 miles) in breadth, and this sometimes happens for canals very far from the north pole, according to laws which are unknown. This occurred in Hydaspes in 1864, in Simois in 1879, in Ackeron in 1884, and in Triton in 1888. The diligent and minute study of the transformations of each canal may lead later to a knowledge of the causes of these effects.

But the most surprising phenomenon pertaining to the canals of Mars is their gemination, which seems to occur principally in the months which precede and in those which follow the great northern inundation—at about the times of the equinoxes. In consequence of a rapid process, which certainly lasts at most a few days, or even perhaps only a few hours, and of which it has not yet been possible to determine the particulars with certainty, a given canal changes its appearance and is found transformed through all its length into two lines or uniform stripes more or less parallel to one another, and which run straight and equal with the exact geometrical precision of the two rails of a railroad. But this exact course is the only point of resemblance with the rails, because in dimensions there is no comparison possible, as it is easy to imagine. The two lines follow very nearly the direction of the original canal and end in the place where it ended. One of these is often superposed as exactly as possible upon the former line, the other being drawn anew; but in this case the original line loses all the small irregularities and curvature that it may have originally possessed. But it also happens that both the lines may occupy opposite sides of the former canal and be located upon entirely new ground. The distance between the two lines differs in different geminations and varies from 600 kilometres (360 miles) and more down to the smallest limit at which two lines may appear separated in large visual telescopes—less than an interval of 50 kilometres (30 miles). The breadth of the stripes themselves may range from the limit of visibility, which we may suppose to be 30 kilometres (18 miles), up to more than 100 kilometres (60 miles). The colour of the two lines varies from black to a light red, which can hardly be distinguished from the general yellow background of the continental surface. The space between is for the most part yellow, but in many cases appears whitish. The gemination is not necessarily confined only to the canals, but tends to be produced also in the lakes. Often one of these is seen transformed into two short, broad dark lines parallel to one another and traversed by a yellow line. In these cases the gemination is naturally short and does not exceed the limits of the original lake.

The gemination is not shown by all at the same time, but when the season is at hand it begins to be produced here and there, in an isolated, irregular manner, or at least without any easily recognisable order. In many canals (such as the Nilosyrtis, for example) the gemination is lacking entirely, or is scarcely visible. After having lasted for some months, the markings fade out gradually and disappear until another season equally favourable for their formation. Thus it happens that in certain other seasons (especially near the southern solstice of the planet) few are seen, or even none at all. In different oppositions the gemination of the same canal may present different appearances as to width, intensity, and arrangement of the two stripes; also in some cases the direction of the lines may vary, although by the smallest quantity, but still deviating by a small amount from the canal with which they are directly associated. From this important fact it is immediately understood that the gemination can not be a fixed formation upon the surface of Mars and of a geographical character like the canals. The second of our maps will give an approximate idea of the appearance which these singular formations present. It contains all the geminations observed since 1882 up to the present time. In examining it it is necessary to bear in mind that not all of these appearances were simultaneous, and consequently that the map does not represent the condition of Mars at any given period; it is only a sort of topographical register of the observations made of this phenomenon at different times.[5]

The observation of the geminations is one of the greatest difficulty, and can only be made by an eye well practised in such work, added to a telescope of accurate construction and of great power. This explains why it is that it was not seen before 1882. In the ten years that have transpired since that time it has been seen and described at eight or ten observatories. Nevertheless, some still deny that these phenomena are real, and tax with illusion (or even imposture) those who declare that they have observed it.

Their singular aspect, and their being drawn with absolute geometrical precision, as if they were the work of

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[5] This map may be found in "La Planète Mars," by Flammarion, page 440.—TRANSLATOR.

rule or compass, have led some to see in them the work of intelligent beings, inhabitants of the planet. I am very careful not to combat this supposition, which includes nothing impossible. (Io mi guarderò bene dal combattere questa supposizione, la quale nulla include d' impossibile.) But it will be noticed that in any case the gemination can not be a work of permanent character, it being certain that in a given instance it may change its appearance and dimensions from one season to another. If we should assume such a work, a certain variability would not be excluded from it; for example, extensive agricultural labour and irrigation upon a large scale. Let us add, further, that the intervention of intelligent beings might explain the geometrical appearance of the gemination, but it is not at all necessary for such a purpose. The geometry of Nature is manifested in many other facts from which are excluded the idea of any artificial labour whatever. The perfect spheroids of the heavenly bodies and the ring of Saturn were not constructed in a turning lathe, and not with compasses has Iris described within the clouds her beautiful and regular arch. And what shall we say of the infinite variety of those exquisite and regular polyhedrons in which the world of crystals is so rich? In the organic world, also, is not that geometry most wonderful which presides over the distribution of the foliage upon certain plants, which orders the nearly symmetrical, starlike figures of the flowers of the field, as well as of the animals of the sea, and which produces in the shell such an exquisite conical spiral that excels the most beautiful masterpieces of Gothic architecture? In all these objects the geometrical form is the simple and necessary consequence of the principles and laws which govern the physical and physiological world. That these principles and these laws are but an indication of a higher intelligent Power we may admit, but this has nothing to do with the present argument.

Having regard, then, for the principle that in the explanation of natural phenomena it is universally agreed to begin with the simplest suppositions, the first hypotheses of the nature and cause of the geminations have for the most part put in operation only the laws of inorganic Nature. Thus, the gemination is supposed to be due either to the effects of light in the atmosphere of Mars, or to optical illusions produced by vapours in various manners, or to glacial phenomena of a perpetual winter, to which it is known all the planets will be condemned, or to double cracks in its surface, or to single cracks of which the images are doubled by the effect of smoke issuing in long lines and blown laterally by the wind. The examination of these ingenious suppositions leads us to conclude that none of them seem to correspond entirely with the observed facts, either in whole or in part. Some of these hypotheses would not have been proposed had their authors been able to examine the geminations with their own eyes. Since some of these may ask me directly, " Can you suggest anything better? " I must reply candidly, " No."

It would be far more easy if we were willing to introduce the forces pertaining to organic Nature. Here the field of plausible supposition is immense, being capable of making an infinite number of combinations capable of satisfying the appearances even with the smallest and simplest means. Changes of vegetation over a vast area, and the production of animals, also very small, but in enormous multitudes, may well be rendered visible at such a distance. An observer placed in the moon would be able to see such an appearance at the times in which agricultural operations are carried out upon one vast plain—the seedtime and the gathering of the harvest. In such a manner also would the flowers of the plants of the great steppes of Europe and Asia be rendered visible at the distance of Mars—by a variety of colouring. A similar system of operations produced in that planet may thus certainly be rendered visible to us. But how difficult for the Lunarians and the Areans to be able to imagine the true causes of such changes of appearance without having first at least some superficial knowledge of terrestrial nature! So also for us, who know so little of the physical state of Mars, and nothing of its organic world, the great liberty of possible supposition renders arbitrary all explanations of this sort and constitutes the gravest obstacle to the acquisition of well-founded notions. All that we may hope is that with time the uncertainty of the problem will gradually diminish, demonstrating if not what the geminations are, at least what they can not be. We may also confide a little in what Galileo called " the courtesy of Nature," thanks to which a ray of light from an unexpected source will sometimes illuminate an investigation at first believed inaccessible to our speculations, and of which we have a beautiful example in celestial chemistry. Let us therefore hope and study.

METEORITES AND STELLAR SYSTEMS

BY

GEORGE HOWARD DARWIN

METEORITES AND STELLAR SYSTEMS[1]

IT is only within the last few years that photographic processes have been so far perfected as to make it possible to photograph a faintly luminous celestial object. The success attained has already been so great that we are made aware of the existence of a multitude of stars which would never have been otherwise perceived, even with the finest telescope and under the purest air. The sensitized plate sums up the effects of light, so that under prolonged exposure even a very faint light at length produces its mark. In this respect the advantage is all on the side of the photograph as compared with the eye, for prolonged gazing is actually detrimental to the acuteness of vision.

The exposure necessary for an ordinary photograph in the broad daylight may be only a fraction of a second, but with the feeble light of the stars three or four hours are found to be necessary or advantageous. Fortunately for the astronomer the heavens move uniformly, and the instrument can be made to follow the object by clockwork. But as clocks are imperfect, the motion of the photographic telescope has to be constantly regulated by hand, so as to keep exact pace with a star, which is viewed through a second telescope attached to the first one. It may easily be conceived that it has required an enormous amount of skill and patience to attain to the present high degree of perfection. But the details of celestial photography are outside the scope of this paper, and I am only concerned with some of the conclusions which have been drawn from the photographic method.

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[1] From "The Century Magazine," October, 1890. By special permission of the Century Company.

Mr. Isaac Roberts, of Liverpool, has recently photographed a portion of the heavens, embracing about four square degrees in the constellation of Cygnus, and he estimates that his plate shows about sixteen thousand stars, none of which are, I believe, visible to the naked eye. A good idea may be formed of this picture by imagining a sheet of dark paper thoroughly splashed with whitewash. The recent advance of celestial photography is well illustrated by the fact that this same portion of the heavens, when photographed in 1885, appeared to contain only about five thousand stars. Thus four years has tripled the number.

Four square degrees comprise only about a ten-thousandth of the whole heavens, and if space were everywhere as thickly peopled as the constellation of the Swan, the whole number of stars photographically visible would reach the stupendous total of one hundred and sixty-seven millions. But the Milky Way runs through Cygnus, and this is a crowded portion of the heavens. Yet there is little doubt that a hundred millions of stars would already be perceptible if the whole heavens were surveyed with equal thoroughness.

These celestial photographs bring vividly before us the utter insignificance of this world and of ourselves; for our planet is of almost contemptible smallness, and our sun is certainly a star of no great magnitude.

And yet it is nearly twice as far from the sun's center to his surface as from here to the moon, and the planet Neptune is distant nearly three thousand million miles from the sun. The mind fails to grasp such a number of stars as a hundred million, and a limit to the perfection of celestial photography has certainly not yet been reached.

Each of these millions of stars has its history, and there are among them representatives of every stage of evolution. If they were not, even with the telescope, mere specks of light, we might see the whole process before us, and might study them like the objects in a museum.

Among the stars there are, however, small luminous clouds called nebulæ, which are not immeasurably small. They have, of course, been examined with all the finest telescopes for many years, and many strange vagaries in their structure have been noted.

It is true that we know the stars and nebulæ to be made of materials found on the earth, and we can estimate approximately how hot they are, and which are old in their history and which are young. All this has been discovered by means of that wonderful instrument, the spectroscope, but it can not show us their shapes and structures. Within the last few months, however, there is reason to hope that the telescopic photograph may really bring before us in an intelligible shape many objects from the celestial museum.

Notwithstanding the paucity of definite knowledge, many theories have been propounded as to the sequence of changes through which the solar system has passed. The most celebrated of these is that associated with the names of the great mathematician Laplace and of the philosopher Kant. It is remarkable that substantially the same theory should have been independently formulated by two men whose intellects were so different.

They both suggested that the matter which now forms the sun and the planets existed in primitive times as a globular nebula of highly rarefied gas in slow rotation, and their theory is accordingly generally known as the nebular hypothesis.

Every portion of this nebula of course attracted every other portion, and therefore there must have been a condensation at the centre, at which point a dense nucleus must ultimately have formed.

The rotation made the nebula fly out like a trundled mop, but the outward tendency was counteracted by attraction. This battle between the attraction due to gravitation and the repulsion due to rotation caused a flattening of the globe, so that it became orange-shaped.

The gas of which the nebula was composed possessed heat; the central part being probably very hot and the external part very cold, as estimated by terrestrial standards. As the energy of heat was gradually lost by radiation into space the globe shrank, and at the same time the central portion became still hotter.

In consequence of the shrinkage, the rate of rotation was increased. This mechanical effect may easily be illustrated thus: If I whirl a stone attached to a string and let the string wind itself up on my finger, the stone will whirl faster and faster as the string shortens.

Lastly, with increased rate of rotation the increased repulsion due to centrifugal force augmented the flattening of the globe. At length a time arrived when the globe was flattened until it became more like a disk than a globe, and gravitation was then no longer capable of holding it together in a single shape.

Everywhere in the nebula the gas was being pressed by the surrounding gas, attracted toward the centre of the nebula, and repelled by centrifugal force away from the axis of rotation. The attraction diminishes and the repulsion increases the farther we go from the centre. If at a place near the edge of the disklike globe the attraction and repulsion are just equal to one another, pressure is not called into play in keeping the gas in its place. At this distance from the centre, then, the gas which is outside does not press at all on that which is inside, and the inner gas may part company with the outer gas without disturbance to it.

In fact, according to the nebular hypothesis, when the flattening had reached a certain degree a ring separated itself from the equatorial regions. The central portion, thus relieved, regained a more globular shape, continued to contract and to spin quicker, until a second crisis supervened, when another ring was shed. A succession of rings was thus formed, and after the detachment of the last the central portion, continuing to contract, at length formed the sun.

Each ring, as soon as it was free, began to aggregate round some denser portion in its periphery. Subordinate nebulæ were thus formed, and they in their turn contracted and shed rings. The nucleus of the secondary nebulæ formed the planets, and their rings condensed into satellites.

This is an outline of the celebrated nebular hypothesis. I shall now show what an interesting confirmation this theory receives from a recent photograph.

There is in the constellation of Andromeda a nebula so remarkable that its nebulous character was recognised even long before the invention of the telescope.

This nebula was first photographed with conspicuous success, in October, 1888, by Mr. Roberts, and again on the 29th of the following December, 1888.

The result is of the greatest interest, for in it we actually see what Laplace pictured with his mind's eye. There is a bright central condensation surrounded by ring after ring, gradually dying away into faintness.

In one of the rings there is a region of greater brightness, which may fairly be interpreted as the centre of aggregation for a planet. At another place which is clearly more remote from the centre, although brought nearer by foreshortening, we have a brilliant round luminous ball—surely a planetary nebula already formed. At a much greater distance there is an elongated nebulosity, which we may conjecture to be a planetary nebula seen edgewise, but in a further state of advance than the other. It is worthy of notice that the remote planets Neptune and Uranus rotate about axes nearly in the plane of their orbits, and from the direction of elongation of this subordinate nebula it seems as though the like must be true here.

In 1848 Bond measured the positions of these two bright small nebulæ relatively to the large one, and they seem to have changed their positions since that date. This confirms the theory that they are planets, but it must be admitted that measurements with reference to an ill-defined object like a nebula are hard to make with precision.

I should suppose this to be the greatest triumph yet achieved by celestial photography, and I owe my sincere thanks to Mr. Roberts for allowing me to reproduce it.

But these pictures, while confirming the substantial truth of the nebular hypothesis, fail to clear up many of the obscurities which surround the evolution of a planetary system. There is one difficulty indeed so fundamental that it has led some astronomers virtually to throw over the whole theory, and it forms the special object of this essay to discuss it.

It is the very essence of the nebular hypothesis that the nebula should be formed of continuous gas, one part of which exercises a pressure on another part; for we have seen how gaseous pressure is instrumental in imparting the globular form to the whole, and how when the globe loses heat and shrinks it is just along that line where the pressure vanishes that the ring splits off.

Now, there is no perceptible trace in the solar system of that all-pervading gas from which the whole is supposed to have been evolved; for the planets do not suffer any sensible retardation in their motion round the sun, as would be the case if they were moving through even a highly rarefied gas.

On the other hand, there is evidence of abundance of solid bodies flying through space. When these bodies meet our atmosphere they glow up white-hot with friction, and are called falling stars or meteorites. Though they are generally dissipated into dust in their passage through the air, yet once in a while one of them owes its preservation to its greater size, and falls on the earth. We thus know them to be strange-looking stones, largely composed of iron.

The ring which surrounds the planet Saturn was obviously suggestive of the nebular hypothesis to the minds of Laplace and Kant. But it has been conclusively proved by the researches of Roche and Clerk Maxwell to consist of a swarm of loose stones—a shower of brickbats, as Maxwell was fond of calling it. And now within the last three years spectroscopic research has led Mr. Lockyer to suggest that the luminous gas, which undoubtedly forms the visible portion of the nebulæ, is simply gas volatilized from the solid state and rendered incandescent by the violent impact of meteoric stones.

These gases, he tells us, cool quickly, cease to be luminous, and condense again into the solid state, but the collisions being incessant, the whole nebula shines with a steady light. Mr. Lockyer supports his view by an elaborate comparison of the spectra of stars and nebulæ with those of actual meteorites, fused by the electric spark in the laboratory. I have not the knowledge of spectroscopy which would be necessary to examine his theory, but his general conclusions seem to be of the highest importance in the study of stellar systems.

All these lines of observation conspire to indicate that the immediate antecedent of the sun and planets was not a continuous gas, but a swarm of loose stones. And yet the nebular hypothesis seems as good as proved by this photograph of Mr. Roberts. Here, then, we find ourselves in a dilemma; on the one hand we have the meteoric theory denying the continuity of the matter which forms the nebulæ, while on the other hand the nebular hypothesis demands such continuity. I wish to emphasize this point: either a nebula is made of a cooling gas, such as hydrogen, nitrogen, oxygen, and the vapours of metals, or it is not so. The nebular hypothesis apparently says it must consist of gas, while this is denied by the strong evidence that it consisted of an enormous number of stones. It seems at first that either the nebular hypothesis or the meteoric theory must be untrue.

I believe, nevertheless, that there is a way in which these conflicting ideas may be brought into harmony and made to re-enforce one another, and the special object of this paper is to effect such a reconciliation. But before coming to that we must leave for a time the world of stars, and must consider the ultimate structure of a gas, such as the air we breathe. A gas is now known to consist of ultra-microscopic molecules, all exactly alike in weight, shape, and structure. Although they are invisible, they can be counted and timed; there are found to be millions in a cubic inch of air, moving indiscriminately in all directions, with great velocity. For example, in the air, at a temperature of 60° Fahr., their average speed is 1,570 feet a second—half as fast again as the velocity of sound. The temperature of a gas simply depends on the rate at which the molecules are moving. Millions of times in each second each one of these molecules happens by chance to strike one of its neighbours, and the two which have struck rebound from each other as though they were of India rubber, or at any rate after such an encounter they behave as though they were perfectly elastic. If we could watch the crowd we should see the individuals darting about in a zigzag course, being deflected into a new direction at each collision. I have often been reminded of this so-called kinetic theory of gases when watching the dance of a little swarm of house-flies as they zigzag about and sharply change their paths, when, for a second, two of them get entangled together. Perhaps this familiar example may help the reader to realize the dance of the molecules in a gas.

The incessant agitation of molecules is quite independent of winds and draughts, and when as many molecules are going in any one direction as in any other we consider the air to be calm. What we call a wind is when more molecules are going in the direction of the wind than in any other.

The molecules of a gas are not aimed at one another, and as a collision is all a matter of chance, it is clear that a molecule is sometimes nearly stopped, sometimes impelled faster, and sometimes merely deflected. Thus they are moving with all possible speeds, but the great majority are moving with about the average speed of the whole crowd.

According to this theory, the pressure of a gas is merely the cannonade of millions of molecules against the side of the vessel containing the gas; as the number of impacts per square inch and per second is enormous, the effect is indistinguishable from that of continuous pressure.

We are accustomed to make statistical inquiries into any question affecting groups of men, and the same method has to be applied to the collisions of the molecules of a gas. These very complex statistics have been profoundly studied by Maxwell, Clausius, and others, and they have shown how to compute from the temperature and density of a gas the average velocity of its molecules, the average frequency of collision, and the average distance travelled between successive collisions.

It will not be possible to go into these difficult questions at present, and it must be accepted that a gas is actually composed of constantly colliding particles or molecules. But when we look at a gas from this point of view we must take care not to confuse the single molecule with the gas of which it forms part. Gas is merely our word for a crowd of molecules, much as nation is a word for a crowd of men. National history is no more to be learned from the doings and character of a single man than the properties of a gas are to be learned from the doings and character of a single molecule. In "gas" and in "nation" the relationship of all to all is involved.

Now that the internal structure of common air has been explained, let us examine a little more closely its relation to ourselves.

If we were to shrink to a ten-millionth of our actual size, how different would air seem! It would then seem to consist of cannon-balls flying about in all directions, at rare intervals, and at a prodigious rate. And yet the supposed change in us would not have affected the nature of air, and it would still be a continuous gas to our former senses. Thus the description we should give of a gas is all a matter of the relative scales of largeness of ourselves and of the gas.

Now, this theory of a gas affords the idea by which I seek to reconcile the conflicting theories of the evolution of stellar systems. My suggestion is that celestial nebulæ are drawn on so large a scale that meteorites may be treated as molecules, and that the collisions of meteorites are so frequent that the whole swarm will behave as though it were a gas. The relationship of us men to this coarse-grained meteoric medium is exactly that of the ideal pygmies to common air.

But it is not enough to make such a suggestion as this; the details of the idea must be examined. We must consider what may be supposed to happen when two stones clash together, and must see whether they can come into collision often enough to make the swarm into a kind of gas.

In comparing the behaviour of meteorites to the molecules of a gas it will naturally occur to inquire whether they can be supposed to possess that high degree of elasticity which is necessary for a kinetic theory. I believe that this question may be answered in the affirmative. Meteoric stones move with speeds which are very great according to our terrestrial notions; and even without Mr. Lockyer's direct spectroscopic evidence we could not doubt that enough heat is generated in a collision to volatilize part of the solid matter of each stone, and to make the gas incandescent. Now, a sudden generation of gas at the point of contact of the two stones would be exactly like the explosion of a charge of gunpowder between them, and they would be blown apart with great violence. As far as regards their velocities after collision the result would be much the same as though they were highly elastic, although this virtual elasticity is of quite a different character from that tendency of a strained solid to recover its shape, which constitutes ordinary elasticity. I may call to mind, as an example of an abnormal elasticity of somewhat the same sort, how a leaden bullet bounds from the surface of the sea, although lead is a very inelastic solid, and water is not solid at all.

It is not claimed that these considerations prove absolutely that two meteorites would bound from each other as if they were very elastic, but it seems highly probable that they would do so, and the matter is not susceptible of strict proof. But granting the elasticity, there is another point to consider.

If two stones meet, the chance of their fracture is greater if they are great than if they are small, and the breakage may go on only until a certain size, dependent on the average velocity of the meteorites, is reached, after which it may become unimportant.

When the gases generated on collision cool they will condense into a metallic rain, and this may fuse with old meteorites. Some actual meteorites show signs of the fusion of many distinct nuclei. Thus there are both abstract reason and direct evidence in support of occasional fusions. The mean size of meteorites in a swarm probably depends on the balance between the opposing forces of breakage and fusion.

No doubt when two stones meet directly each is shattered to fragments. But glancing collisions must be indefinitely more frequent, and in these we may suppose that fracture is comparatively rare, and virtual elasticity great. The possible frequency of fracture undoubtedly does present a difficulty in the theory, for it would seem as though the whole swarm of stones must gradually degrade into dust. There must be some way out of this difficulty, for meteorites of considerable size fall upon the earth, and unless Mr. Lockyer has misinterpreted the spectroscopic evidence, the nebulæ do now consist of meteorites.

I hold that these considerations justify us in maintaining a rough similarity between meteoric stones and the molecules of a gas, as far as regards the actual collisions. If this is so, what is called the temperature of a gas must be translated as meaning the average energy of motion of the meteorites.

We must now go on to consider how often meteorites collide, and try to discover whether a swarm of meteorites possesses a fine enough texture to permit the applicability of the theory. For this part of the discussion numerical calculations are necessary. Calculations require numerical data, and these can only be derived from a known system, and of course the only one known with any precision is the solar system. The fineness of grain is obviously independent of the amount of flattening of the nebula which arises from rotation. In order, therefore, to simplify the matter, and to consider one thing at a time, the nebula is supposed to be one in which there is no rotation.

The weight of the sun in pounds is four with thirty zeros after it, and I suppose the sun to be broken up into that number of meteorites, each weighing one pound. If the meteorites are supposed to be of iron their exact size is known, because the dimensions of a pound of iron are known. This supposition as to the weight and size of the meteorites is merely adopted as a type, but it suffices for our present purpose. These one-pound iron stones are to be distributed in a swarm extending beyond the present orbit of Neptune. To give numerical precision, let us suppose that the swarm extends half as far again as Neptune's orbit—that is to say, let it extend to forty-five times the distance of the earth from the sun.

In this condition the nebula is of extreme tenuity, and if the stones are not then too sparse to make the swarm behave like a gas, it will, a fortiori, behave like a gas when the nebula has shrunk and the stones are more closely packed. The supposition made as to the extension of the solar swarm, therefore, puts the theory to a severe test.

In the case of a town, density of population means the number of people to the square mile, and for meteorites what we may still call the density of population is the number to the cubic mile. The swarm is not supposed to be rotating, and is therefore a perfect globe, and the layers of equal density of population are also spheres.

The stones will not be evenly distributed in space, but the density of population will be much greater toward the middle than toward the outside. The reason of this is that every stone is attracted toward the middle, and is only prevented from yielding to the tendency by the blows it receives from its neighbours. Think of a crowd struggling for tickets at a railway station, and you have a picture of what happens. The men squeeze and push and sway about, but the crowd remains of about the same density at each distance from its middle. So in the swarm the dance of the meteorites is incessant, but it arranges itself automatically into a steady condition, in which the density of population has no tendency to shift.

It is natural to ask why the stones should be moving at all, and how they acquired their great speed. This is a question that imperatively demands an answer, and we are able to answer it with certainty. They derive their speed from gravitation; they have fallen in from a great distance toward a centre of aggregation. A description of the way in which they may have come together will make it clear why they are moving, and will also give the reader some idea of how the actual velocity may be calculated in a swarm of given mass and size.

Imagine that somewhere in space there is an aggregation of meteorites—no matter how it got there—and conceive a stone released from a state of rest at a very great distance. Under the attraction of gravitation the stone falls toward the centre of aggregation, and on reaching the confines of the swarm it will have acquired a certain velocity. It then penetrates the swarm for some uncertain distance, until it happens to strike another meteorite. Henceforth its path is zigzag, as it happens to strike, and we need not suppose ourselves to watch it any longer, since it has become one of the swarm. It is, however, important to remark that the supposed visitant from outside space has imported energy of motion into the system, which energy it gradually communicates to its neighbours by collisions; it has also increased the mass of the swarm. When another stone is allowed to fall in, since it is attracted by a slightly greater mass, it arrives at the swarm with slightly greater speed than the first. So if we imagine the swarm to be increased by the addition of stone after stone, we see that in the course of accretion the energy of agitation of its constituent meteorites gradually increases. Also the volume of the globe throughout which (if anywhere) the swarm possesses the mechanical properties of a gas is at the same time gradually increased. We must suppose that at length all the stones in that part of space are exhausted, the materials of the nebula are collected, and it only remains for them to work out their future fate.

By this sort of reasoning we find out how fast the stones are moving, but it is proper to add that an important correction has to be applied to allow for the fact that at each collision some speed is lost. In the process of settling into the steady condition each stone retains only seven tenths of the velocity it would have had if it were a fresh arrival from space.

It will make no material difference in these results by what process the stones were brought together, and this account which I have just given of the formation of a swarm is not intended as a contribution to its history, but is only meant to render intelligible the mechanical principles involved, and to show in a general way how the matter may be subjected to calculation.

By such a line of argument as this I found that, when the solar swarm extended half as far again as the planet Neptune, in the central region the stones were moving at an average rate of three miles a second—two hundred times as fast as a fast train—but that in the outer portion of the swarm the velocity was less.

We have now to find out how often the stones came into collision, how far they travelled beween collisions, and whether the collisions were frequent enough to allow us to consider the whole nebula as a kind of gas, as is demanded by the nebular hypothesis.

To how small a pygmy would air still be air? The answer is that the pygmy must be just large enough to be struck so often that he loses the sensation of the individual blows, and is only aware of their average effect. It will insure this if he is struck hundreds or thousands of times in the average interval between two collisions of a molecule of air; or we may say that his bulk must be great enough to contain thousands of molecules, or his length thousands of times as great as the average path traversed by a molecule between two successive collisions. These conditions are amply satisfied in the relationship of the smallest microscopic animalcule to our air.

It must, however, be a giant who would not feel the individual blows of meteorites, but only realize their average effects. If we might consider the nebula, as a whole, to be a living being, we might say that if she is to behave like a gas she must realize herself as a gas, and so she must be the giant to whose perceptions the meteoric nebula is to be a gas; hence the giant must not be larger than the nebula itself.

It would not be easy to explain the exact reasoning by which a comparison is made between the dimensions of the giant and the texture of the nebula at every part of itself. It must suffice to say that the comparison is best clothed in a form which may appear something quite different, but which is really substantially the same.

Except at the moment of a collision, a meteorite is like a very small planet, and accordingly moves in a curved orbit, but at each collision it starts in a new orbit.

I say, then, that the nebula will behave sufficiently like a gas to allow the nebular hypothesis to be true, if the average path of a meteorite between two collisions is so short that the bit of orbit described departs very little from a straight line.

We now at length come to the numerical values to which we have been tending, and shall see how often the stones of the solar nebula came into collision with one another when the nebula extended in a swarm of one-pound iron meteorites half as far again from its centre as the present distance of Neptune from the sun.

In this case I find that at the middle of the swarm a meteorite would on the average come into collision every thirteen hours, and would travel 140,000 miles between collisions; at the distance of the small planets, called the asteroids, it would collide every seventeen hours and would travel 190,000 miles between; at the distance of Uranus the collisions would be at intervals of twenty-five days, and the path 6,000,000 miles; and lastly, at the distance of Neptune, the interval would be one hundred and ninety days and the path 28,000,000 miles.

I have said that the criterion we have to apply depends on the amount of curvature of the average path of a stone between two successive collisions. Now, it may be shown that the amount of departure from straightness is greater the farther we go from the middle of the swarm; and I find that even at the distance of Neptune the collisions are, speaking relatively, so frequent that gravity only suffices to draw the meteorite aside from the straight path by one sixty-sixth of the path it has traversed. The fraction one sixty-sixth is then the value of the criterion which was to be applied. Now one sixty-sixth is so small a fraction that it may be concluded that the meteoric swarm passes the proposed test, notwithstanding that the great extension which has been attributed to the nebula strains the hypothesis severely.

It follows, therefore, that if meteorites possess virtual elasticity, and if breakages are counterbalanced by fusions, then a swarm of meteorites provides a gaslike medium of a fine enough structure to satisfy the demands of the nebular hypothesis.

Some such numerical examination as the foregoing is necessary in order to assure us that the quality of a gas can have been imparted to a nebula in the suggested way, and so to lift the hypothesis out of the realms of mere conjecture.

We may conclude from this discussion that it is possible to justify the contention that the meteoric theory is reconcilable with the nebular hypothesis, and that we may accordingly hold the truth of both of them at the same time.

If space permitted I might go on to consider some of the conclusions fairly deducible from this view of a nebula, but it must suffice to say that this theory seems likely to prove fruitful in the further elucidation of this complex and necessarily speculative subject.

Up to this time we have been occupied with proving, or rendering probable, a modification in Laplace's theory.

But it would hardly be satisfactory to leave the matter at this point. I wish it were possible to gain an insight into the origin and previous history of these stones, but on these mysteries I have no suggestion to make. It is, however, possible to see pretty clearly what happened after the nebular stage, and how all this bears on the state of things of which we are witnesses to-day.

At the various centres of condensation which we now call sun, planets, and satellites the swarm of meteorites became denser and denser. The collisions were too frequent to let the gases cool and condense again, and thus by degrees the meteorites were entirely volatilized. Thus round these centres we should have at length a mass of glowing gas, and toward the middle fluids and solids. All this must have occurred comparatively early in the case of the sun, later at the planets, and last at the satellites.

Outside of these condensations there were numbers of free meteorites, but the majority of the stones which formed the swarm in primitive times were already absorbed, and the absorption still went on gradually.

The collisions among the free meteorites became rarer, because they were scattered more sparsely; and less violent, because at each successive collision some relative motion was lost. Finally, the collisions were nearly annulled. The residue of the meteoric swarm then consisted of sparse flights of meteorites, moving in streams. Such streams give us no evidence of their existence, except under special circumstances.

The zodiacal light is a lens-shaped luminosity, seen in the east or west shortly before sunrise or after sunset—not commonly in the latitude of England, but frequently in the south. It is probably due to the reflection of sunlight from millions of meteorites which have not yet been swallowed by the sun.

Again, if a stream of meteorites moves in a very elliptic orbit, at one part of its course it passes near the sun. In this part of its orbit the flight is packed into a smaller space than before, so that collisions are largely multiplied. Moreover, the flight dashes through a region thickly peopled with the meteorites which make the zodiacal light. It has been proved that there is an intimate relationship between comets and flights of meteorites, and Mr. Lockyer suggests that the luminosity of comets is caused jointly by the collisions internal to the flight of stones, and by those which occur as the flight ploughs its way through the zodiacal light.

But meteorites are still frequent far outside of the zodiacal light, although there may not be enough to reflect sunlight to a visible degree. Of this we have familiar evidence in the shooting star.

The orbits of several streams of meteorites are known, and each year, as on certain days the earth crosses those orbits, their existence is proved by volleys of falling stars, which emanate from known radiant points in the heavens.

But these are the dregs and sawdust of the solar system, and merely serve to give us a memento of the myriads which existed in early days, before the sun and the planets and their satellites were born.

In this paper I have attempted to touch on only a few points in a large subject. The attempt to reconstruct the history of stars and planets involves ideas grand in themselves; but the events to be recorded in that history relate to a past so remote that our conclusions can not but be speculative. Thus the value of the investigation of which I have given an account will appear very different to different minds. To some men of science it will stand condemned as altogether too speculative; others will think that it is better to risk error in the chance of winning truth. To me, at least, it seems that the line of thought flows in a true channel; that it may help to give a meaning to the observations of the astronomer and of the spectroscopist; and that many interesting problems may perhaps be solved with sufficient completeness to throw further light on the evolution of nebulæ and of planetary systems.

MAGNITUDE OF THE SOLAR SYSTEM

BY

WILLIAM HARKNESS

MAGNITUDE OF THE SOLAR SYSTEM[1]

NATURE may be studied in two widely different ways. On the one hand we may employ a powerful microscope which will render visible the minutest forms, and limit our field of view to an infinitesimal fraction of an inch situated within a foot of our own noses; or, on the other hand, we may occupy some commanding position, and from thence, aided perhaps by a telescope, we may obtain a comprehensive view of an extensive region. The first method is that of the specialist, the second is that of the philosopher, but both are necessary for an adequate understanding of Nature. The one has brought us knowledge wherewith to defend ourselves against bacteria and microbes, which are among the most deadly enemies of mankind, and the other has made us acquainted with the great laws of matter and force upon which rests the whole fabric of science. All Nature is one, but for convenience of classification we have divided our knowledge into a number of sciences which we usually regard as quite distinct from each other. Along certain lines, or, more properly, in certain regions, these sciences necessarily abut on each other, and just there lies the weakness of the specialist. He is like a wayfarer who always finds obstacles in crossing the boundaries between two countries, while to the traveller who gazes over them from a commanding eminence the case is quite different. If the boundary is

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[1] Presidential address delivered before the American Association for the Advancement of Science, at its Brooklyn meeting, August 16, 1894. Printed in "Astronomy and Astro-Physics," vol. xiii, No. 8; also in "American Journal of Science," vol. xlviii, September, 1894; and the "Smithsonian Report for 1894."

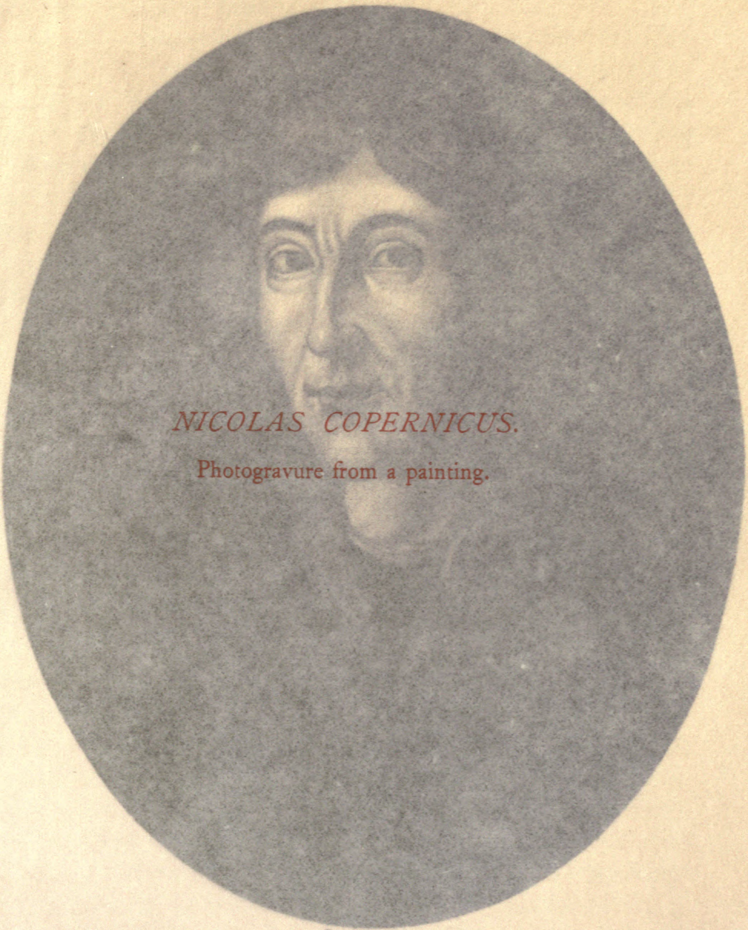
an ocean shore, there is no mistaking it; if a broad river or a chain of mountains, it is still distinct; but if only a line of posts traced over hill and dale, then it becomes lost in the natural features of the landscape, and the essential unity of the whole region is apparent. In that case the border-land is wholly a human conception of which Nature takes no cognizance, and so it is with the scientific border-land to which I propose to invite your attention this evening.

To the popular mind there are no two sciences further apart than astronomy and geology. The one treats of the structure and mineral constitution of our earth, the causes of its physical features and its history, while the other treats of the celestial bodies, their magnitudes, motions, distances, periods of revolution, eclipses, order, and of the causes of their various phenomena. And yet many, perhaps I may even say most, of the apparent motions of the heavenly bodies are merely reflections of the motions of the earth, and in studying them we are really studying it. Furthermore, precession, nutation, and the phenomena of the tides depend largely upon the internal structure of the earth, and there astronomy and geology merge into each other. Nevertheless, the methods of the two sciences are widely different, most astronomical problems being discussed quantitatively by means of rigid mathematical formulæ, while in the vast majority of cases the geological ones are discussed only qualitatively, each author contenting himself with a mere statement of what he thinks. With precise data the methods of astronomy lead to very exact results, for mathematics is a mill which grinds exceeding fine; but, after all, what comes out of a mill depends wholly upon what is put into it, and if the data are uncertain, as is the case in most cosmological problems, there is little to choose between the mathematics of the astronomer and the guesses of the geologist.

If we examine the addresses delivered by former presidents of this association, and of the sister—perhaps it would be nearer the truth to say the parent—association on the other side of the Atlantic, we shall find that they have generally dealt either with the recent advances in some broad field of science, or else with the development of some special subject. This evening I propose to adopt the latter course, and I shall invite your attention to the present condition of our knowledge respecting the magnitude of the solar system; but in so doing it will be necessary to introduce some considerations derived from laboratory experiments upon the luminiferous ether, others derived from experiments upon ponderable matter, and still others relating both to the surface phenomena and to the internal structure of the earth, and thus we shall deal largely with the border-land where astronomy, physics, and geology merge into each other.

The relative distances of the various bodies which compose the solar system can be determined to a considerable degree of approximation with very crude instruments as soon as the true plan of the system becomes known, and that plan was taught by Pythagoras more than five hundred years before Christ. It must have been known to the Egyptians and Chaldeans still earlier, if Pythagoras really acquired his knowledge of astronomy from them, as is affirmed by some of the ancient writers, but on that point there is no certainty. In public Pythagoras seemingly accepted the current belief of his time, which made the earth the centre of the universe, but to his own chosen disciples he communicated the true doctrine that the sun occupies the centre of the solar system and that the earth is only one of the planets revolving around it. Like all the world's greatest sages, he seems to have taught only orally. A century elapsed before his doctrines were reduced to writing by Philolaus of Crotona, and it was still later before they were taught in public for the first time by Hicetas, or as he is sometimes called Nicetas, of Syracuse. Then the familiar cry of impiety was raised, and the Pythagorean system was eventually suppressed by that now called the Ptolemaic, which held the field until it was overthrown by Copernicus almost two thousand years later. Pliny tells us that Pythagoras believed the distances to the sun and moon to be, respectively, 252,000 and 12,600 stadia, or, taking the stadium at 625 feet, 29,837 and 1,492 English miles; but there is no record of the method by which these numbers were ascertained.

After the relative distances of the various planets are known, it only remains to determine the scale of the system, for which purpose the distance between any two planets suffices. We know little about the early history of the subject, but it is clear that the primitive astronomers must have found the quantities to be measured too small for detection with their instruments, and even in modern times the problem has proved to be an extremely difficult one. Aristarchus, of Samos, who flourished about 270 B. C., seems to have been the first to attack it in a scientific manner. Stated in modern language, his reasoning was that when the moon is exactly half full the earth and sun, as seen from its centre, must make a right angle with each other, and by measuring the angle between the sun and moon, as seen from the earth at that instant, all the angles of the triangle joining the earth, sun, and moon would become known, and thus the ratio of the distance of the sun to the distance of the moon would be determined. Although perfectly correct in theory, the difficulty of deciding visually upon the exact instant when the moon is half full is so great that it can not be accurately done, even with the most powerful telescopes. Of course, Aristarchus had no telescope, and he does not explain how he effected the observation, but his conclusion was that at the instant in question the distance between the centres of the sun and moon as seen from the earth is less than a right angle by one thirtieth part of the same. We should now express this by saying that the angle is 87°, but Aristarchus knew nothing of trigonometry, and in order to solve his triangle he had recourse to an ingenious but long and cumbersome geometrical process, which has come down to us, and affords conclusive proof of the condition of Greek mathematics at that time. His conclusion was that the sun is nineteen times farther



NICOLAS COPERNICUS.

Photogravure from a painting.

Goupilgravure

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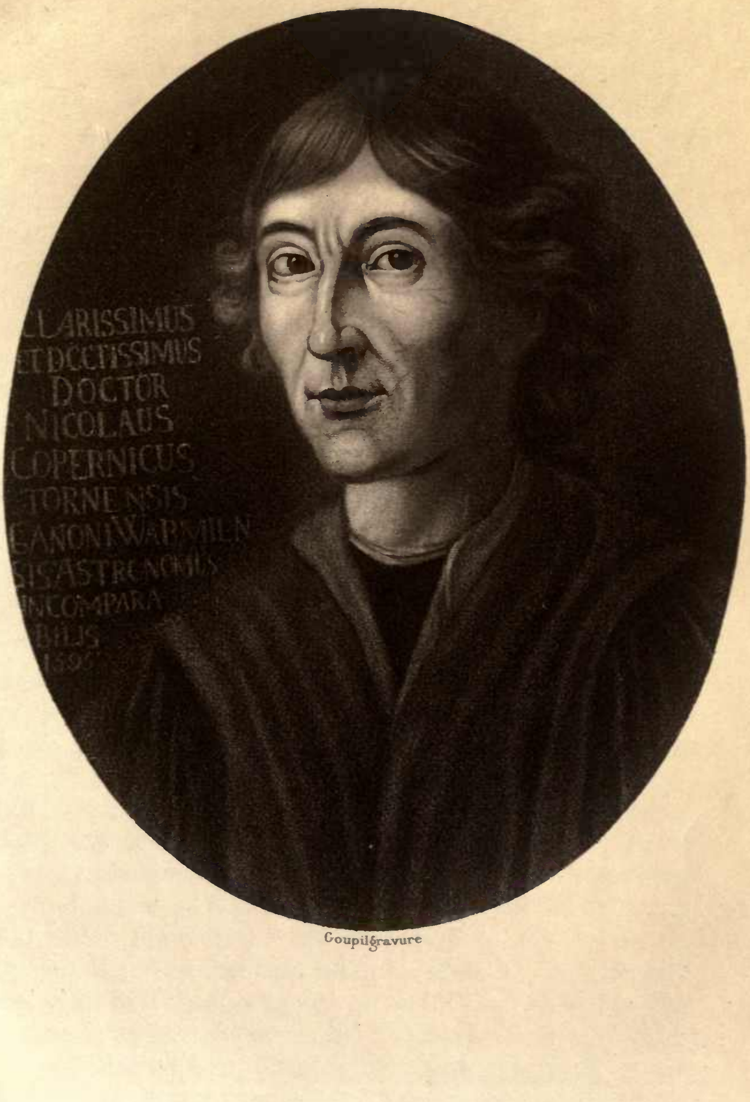

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from the earth than the moon, and if we combine that result with the modern value of the moon's parallax—viz., 3,422.38″, we obtain for the solar parallax 180″, which is more than twenty times too great.

The only other method of determining the solar parallax known to the ancients was that devised by Hipparchus about 150 B. C. It was based on measuring the rate of decrease of the diameter of the earth's shadow cone by noting the duration of lunar eclipses, and as the result deduced from it happened to be nearly the same as that found by Aristarchus, substantially his value of the parallax remained in vogue for nearly two thousand years, and the discovery of the telescope was required to reveal its erroneous character. Doubtless this persistency was due to the extreme minuteness of the true parallax, which we now know is far too small to have been visible upon the ancient instruments, and thus the supposed measures of it were really nothing but measures of their inaccuracy.

The telescope was first pointed to the heavens by Galileo in 1609, but it needed a micrometer to convert it into an accurate measuring instrument, and that did not come into being until 1639, when it was invented by William Gascoigne. After his death, in 1644, his original instrument passed to Richard Townley, who attached it to a fourteen-foot telescope at his residence in Townley, Lancashire, England, where it was used by Flamsteed in observing the diurnal parallax of Mars during its opposition in 1672. A description of Gascoigne's micrometer was published in the "Philosophical Transactions" in 1667, and a little before that a similar instrument had been invented by Auzout, in France, but observatories were fewer then than now, and, so far as I know, J. D. Cassini was the only person besides Flamsteed who attempted to determine the solar parallax from that opposition of Mars. Foreseeing the importance of the opportunity, he had Richer despatched to Cayenne some months previously, and when the opposition came he effected two determinations of the parallax: one being by the diurnal method, from his own observations in Paris, and the other by the meridian method, from observations in France by himself, Römer, and Picard, combined with those of Richer at Cayenne. This was the transition from the ancient instruments with open sights to telescopes armed with micrometers, and the result must have been little short of stunning to the seventeenth-century astronomers, for it caused the hoary and gigantic parallax of about 180″ to shrink incontinently to 10″, and thus expanded their conception of the solar system to something like its true dimensions. More than fifty years previously Kepler had argued from his ideas of the celestial harmonies that the solar parallax could not exceed 60″, and a little later Horrocks had shown on more scientific grounds that it was probably as small as 14″; but the final death blow to the ancient values—ranging as high as 2′ or 3′—came from these observations of Mars by Flamsteed, Cassini, and Richer.

Of course, the results obtained in 1672 produced a keen desire on the part of astronomers for further evidence respecting the true value of the parallax, and as Mars comes into a favourable position for such investigations only at intervals of about sixteen years, they had recourse to observations of Mercury and Venus. In 1677 Halley observed the diurnal parallax of Mercury, and also a transit of that planet across the sun's disk, at St. Helena, and in 1681 J. D. Cassini and Picard observed Venus when she was on the same parallel with the sun, but although the observations of Venus gave better results than those of Mercury, neither of them was conclusive, and we now know that such methods are inaccurate even with the powerful instruments of the present day. Nevertheless, Halley's attempt by means of the transit of Mercury ultimately bore fruit in the shape of his celebrated paper of 1716, wherein he showed the peculiar advantages of transits of Venus for determining the solar parallax. The idea of utilizing such transits for this purpose seems to have been vaguely conceived by James Gregory, or perhaps even by Horrocks, but Halley was the first to work it out completely, and long after his death his paper was mainly instrumental in inducing the governments of Europe to undertake the observations of the transits of Venus in 1761 and 1769, from which our first accurate knowledge of the sun's distance was obtained.

Those who are not familiar with practical astronomy may wonder why the solar parallax can be got from Mars and Venus, but not from Mercury or the sun itself. The explanation depends upon two facts: Firstly, the nearest approach of these bodies to the earth is for Mars 33,874,000 miles, for Venus 23,654,000 miles, for Mercury 47,935,000 miles, and for the sun 91,239,000 miles. Consequently, for us Mars and Venus have very much larger parallaxes than Mercury or the sun, and of course the larger the parallax the easier it is to measure. Secondly, even the largest of these parallaxes must be determined within far less than one tenth of a second of the truth, and while that degree of accuracy is possible in measuring short arcs, it is quite unattainable in long ones. Hence, one of the most essential conditions for the successful measurement of parallaxes is that we shall be able to compare the place of the near body with that of a more distant one situated in the same region of the sky. In the case of Mars that can always be done by making use of a neighbouring star, but when Venus is near the earth she is also so close to the sun that stars are not available, and consequently her parallax can be satisfactorily measured only when her position can be accurately referred to that of the sun, or, in other words, only during her transits across the sun's disk. But even when the two bodies to be compared are sufficiently near each other, we are still embarrassed by the fact that it is more difficult to measure the distance between the limb of a planet and a star or the limb of the sun than it is to measure the distance between two stars, and since the discovery of so many asteroids that circumstance has led to their use for the determination of the solar parallax. Some of these bodies approach within 75,230,000 miles of the earth's orbit, and as they look precisely like stars, the increased accuracy of pointing on them fully makes up for their greater distance as compared with Mars or Venus.

After the Copernican system of the world and the Newtonian theory of gravitation were accepted it soon became evident that trigonometrical measurements of the solar parallax might be supplemented by determinations based on the theory of gravitation, and the first attempts in that direction were made by Machin in 1729 and T. Mayer in 1753. The measurement of the velocity of light between points on the earth's surface, first effected by Fizeau in 1849, opened up still other possibilities, and thus for determining the solar parallax we have at our command no less than three entirely distinct classes of methods, which are known respectively as the trigonometrical, the gravitational, and the photo-tachymetrical. We have already given a summary sketch of the trigonometrical methods as applied by the ancient astronomers to the dichotomy and shadow cone of the moon, and by the moderns to Venus, Mars, and the asteroids, and we shall next glance briefly at the gravitational and photo-tachymetrical methods.

The gravitational results which enter directly or indirectly into the solar parallax are six in number, to wit: First, the relation of the moon's mass to the tides; second, the relation of the moon's mass and parallax to the force of gravity at the earth's surface; third, the relation of the solar parallax to the masses of the earth and moon; fourth, the relation of the solar and lunar parallaxes to the moon's mass and parallactic inequality; fifth, the relation of the solar and lunar parallaxes to the moon's mass and the earth's lunar inequality; sixth, the relation of the constants of nutation and precession to the moon's parallax.

Respecting the first of these relations it is to be remarked that the tide-producing forces are the attractions of the sun and moon upon the waters of the ocean, and from the ratio of these attractions the moon's mass can readily be determined. But unfortunately the ratio of the solar tides to the lunar tides is affected both by the depth of the sea and by the character of the channels through which the water flows, and for that reason the observed ratio of these tides requires multiplication by a correcting factor in order to convert it into the ratio of the forces. The matter is further complicated by this correcting factor varying from port to port, and in order to get satisfactory results long series of observations are necessary. The labour of deriving the moon's mass in this way was formerly so great that for more than half a century Laplace's determination from the tides at Brest remained unique; but the recent application of harmonic analysis to the data supplied by self-registering tide gauges is likely to yield abundant results in the near future.

Our second gravitational relation—viz., that connecting the moon's mass and parallax with the force of gravity at the earth's surface—affords an indirect method of determining the moon's parallax with very great accuracy if the computation is carefully made, and with a fair approximation to the truth even when the data are exceedingly crude. To illustrate this, let us see what could be done with a railroad transit such as is commonly used by surveyors, a steel tape, and a fairly good watch. Neglecting small corrections due to the flattening of the earth, the centrifugal force at its surface, the eccentricity of its orbit and the mass of the moon, the law of gravitation shows that if we multiply together the length of the seconds-pendulum, the square of the radius of the earth, and the square of the length of the sidereal month, divide the product by four, and take the cube root of the quotient, the result will be the distance from the earth to the moon. To find the length of the seconds-pendulum we would rate the watch by means of the railroad transit, and then making a pendulum out of a spherical leaden bullet suspended by a fine thread, we would adjust the length of the thread until the pendulum made exactly three hundred vibrations in five minutes by the watch. Then, supposing the experiment to be made here or in New York city, we would find that the distance from the point of suspension of the thread to the centre of the bullet was about 39⅛ inches, and dividing that by the number of inches in a mile—viz., 63,360—we would have for the length of the seconds-pendulum $\frac{1}{1620}$ of a mile. The next step would be to ascertain the radius of the earth, and the quickest way of doing so would probably be, first, to determine the latitude of some point in New York city by means of the railroad transit; next to run a traverse survey along the old post-road from New York to Albany, and finally to determine the latitude of some point in Albany. The traverse survey should surely be correct to one part in three hundred, and as the distance between the two cities is about 2°, the difference of latitude might be determined to about the same percentage of accuracy. In that way we would find the length of 2° of latitude to be about 138 miles, whence the earth's radius would be 3,953 miles. It would then only remain to observe the time occupied by the moon in making a sidereal revolution around the earth, or, in other words, the time which she occupies in moving from any given star back to the same star again. By noting that to within one quarter of her own diameter we would soon find that the time of revolution is about 27.32 days, and multiplying that by the number of seconds in a day—viz., 86,400—we would have for the length of the sidereal month 2,360,000 seconds. With these data the computation would stand as follows: The radius of the earth, 3,953 miles, multiplied by the length of a sidereal month, 2,360,000 seconds, and the product squared gives 87,060,000,000,000,000,000. Multiplying that by one fourth of the length of the seconds-pendulum—viz., $\frac{1}{6480}$ of a mile—and extracting the cube root of the product, we would get 237,700 miles for the distance from the earth to the moon, which is only about 850 miles less than the truth, and certainly a remarkable result considering the crudeness of the instruments by which it might be obtained. Nevertheless, when all the conditions are rigorously taken into account these data are to be regarded as determining the relation between the moon's mass and parallax, rather than the parallax itself.

Our third gravitational relation—to wit, that existing between the solar parallax, the solar attractive force, and the masses of the earth and moon—is analogous to the relation existing between the moon's mass and parallax and the force of gravity at the earth's surface, but it can not be applied in exactly the same way on account of our inability to swing a pendulum on the sun. We are therefore compelled to adopt some other method of determining the sun's attractive force, and the most available is that which consists in observing the perturbative action of the earth and moon upon our nearest planetary neighbours, Venus and Mars. From this action the law of gravitation enables us to determine the ratio of the sun's mass to the combined masses of the earth and moon, and then the relation in question furnishes a means of comparing the masses so found with trigonometrical determinations of the solar parallax. Thus it appears that notwithstanding necessary differences in the methods of procedure, the analogy between the second and third gravitational relations holds not only with respect to their theoretical basis, but also in their practical application, the one being used to determine the relation between the mass of the moon and its distance from the earth, and the other to determine the relation between the combined masses of the earth and moon and their distance from the sun.

Our fourth gravitational relation deals with the connection between the solar parallax, the lunar parallax, the moon's mass, and the moon's parallactic inequality. The important quantities are here the solar parallax and the moon's parallactic inequality, and although the derivation of the complete expression for the connection between them is a little complicated, there is no difficulty in getting a general notion of the forces involved. As the moon moves around the earth she is alternately without and within the earth's orbit. When she is without, the sun's attraction on her acts with that of the earth; when she is within, the two attractions act in opposite directions. Thus in effect the centripetal force holding the moon to the earth is alternately increased and diminished, with the result of elongating the moon's orbit toward the sun and compressing it on the opposite side. As the variation of the centripetal force is not great, the change of form of the orbit is small; nevertheless, the summation of the minute alternations thereby produced in the moon's orbital velocity suffices to put her sometimes ahead and sometimes behind her mean place to an extent which oscillates from a maximum to a minimum, as the earth passes from perihelion to aphelion, and averages about 125″ of arc. This perturbation of the moon is known as the parallactic inequality, because it depends on the earth's distance from the sun, and can therefore be expressed in terms of the solar parallax. Conversely, the solar parallax can be deduced from the observed value of the parallactic inequality, but unfortunately there are great practical difficulties in making the requisite observations with a sufficient degree of accuracy. Notwithstanding the ever-recurring talk about the advantages to be obtained by observing a small, well-defined crater instead of the moon's limb, astronomers have hitherto found it impracticable to use anything but the limb, and the disadvantage of doing so, as compared with observing a star, is still further increased by the circumstance that in general only one limb can be seen at a time, the other being shrouded in darkness. If both limbs could always be observed, we should then have a uniform system of data for determining the place of the centre, but under existing circumstances we are compelled to make our observations half upon one limb and half upon the other, and thus they involve all the systematic errors which may arise from the conditions under which these limbs are observed, and all the uncertainty which attaches to irradiation, personal equation, and our defective knowledge of the moon's semidiameter.

Our fifth gravitational relation is that which exists between the solar parallax, the lunar parallax, the moon's mass, and the earth's lunar inequality. Strictly speaking, the moon does not revolve around the earth's centre, but both bodies revolve around the common centre of gravity of the two. In consequence of that, an irregularity arises in the earth's orbital velocity around the sun, the common centre of gravity moving in accordance with the laws of elliptic motion, while the earth, on account of its revolution around that centre, undergoes an alternate acceleration and retardation which has for its period a lunar month, and is called the lunar inequality of the earth's motion. We perceive this inequality as an oscillation superposed on the elliptic motion of the sun, and its semiamplitude is the measure of the angle subtended at the sun by the interval between the centre of the earth and the common centre of gravity of the earth and moon. Just as an astronomer on the moon might use the radius of her orbit around the earth as a base for measuring her distance from the sun, so we may use this interval for the same purpose. We find its length in miles from the equatorial semidiameter of the earth, the moon's parallax, and the moon's mass, and thus we have all the data for determining the solar parallax from the inequality in question. In view of the great difficulty which has been experienced in measuring the solar parallax itself, it may be asked, Why should we attempt to deal with the parallactic inequality, which is about twenty-six per cent smaller? The answer is, Because the latter is derived from differences of the sun's right ascension, which are furnished by the principal observatories in vast numbers, and should give very accurate results on account of their being made by methods which insure freedom from constant errors. Nevertheless, the sun is not so well adapted for precise observations as the stars, and Dr. Gill has recently found that heliometer measurements upon asteroids which approach very near to the earth yield values of the parallactic inequality superior to those obtained from right ascensions of the sun.

Our sixth gravitational relation is that which exists between the moon's parallax and the constants of precession and nutation. Every particle of the earth is attracted both by the sun and by the moon, but in consequence of the polar flattening the resultant of these attractions passes a little to one side of the earth's centre of gravity. Thus a couple is set up, which, by its action upon the rotating earth, causes the axis thereof to describe a surface which may be called a fluted cone, with its apex at the earth's centre. A top spinning with its axis inclined describes a similar cone, except that the flutings are absent and the apex is at the point upon which the spinning occurs. For convenience of computation we resolve this action into two components, and we name that which produces the cone the luni-solar precession, and that which produces the flutings the nutation. In this phenomenon the part played by the sun is comparatively small, and by eliminating it we obtain a relation between the luni-solar precession, the nutation, and the moon's parallax which can be used to verify and correct the observed values of these quantities.

In the preceding paragraph we have seen that the relation between the quantities there considered depends largely upon the flattening of the earth, and thus we are led to inquire how and with what degree of accuracy that is determined. There are five methods—viz., one geodetic, one gravitational, and three astronomical. The geodetic method depends upon measurements of the length of a degree on various parts of the earth's surface; and with the data hitherto accumulated it has proved quite unsatisfactory. The gravitational method consists in determining the length of the seconds-pendulum over as great a range of latitude as possible, and deducing therefrom the ratio of the earth's polar and equatorial semidiameters by means of Clairaut's theorem. The pendulum experiments show that the earth's crust is less dense on mountain plateaus than at the seacoast, and thus for the first time we are brought into contact with geological considerations. The first astronomical method consists in observing the moon's parallax from various points on the earth's surface; and as these parallaxes are nothing else than the angular semidiameter of the earth at the respective points, as seen from the moon, they afford a direct measure of the flattening. The second and third astronomical methods are based upon certain perturbations of the moon which depend upon the figure of the earth, and should give extremely accurate results; but unfortunately very great difficulties oppose themselves to the exact measurement of the perturbations. There is also an astronomico-geological method which can not yet be regarded as conclusive on account of our lack of knowledge respecting the law of density which prevails in the interior of the earth. It is based upon the fact that a certain function of the earth's moments of inertia can be determined from the observed values of the coefficients of precession and nutation, and could also be determined from the figure and dimensions of the earth if we knew the exact distribution of matter in its interior. Our present knowledge on that subject is limited to a superficial layer not more than ten miles thick, but it is usual to assume that the deeper matter is distributed, according to Lagrange's law, and then by writing the function in question in a form which leaves the flattening indeterminate, and equating the expression so found to the value given by the precession and nutation, we readily obtain the flattening. As yet these methods do not give consistent results, and so long as serious discrepancies remain between them there can be no security that we have arrived at the truth.

It should be remarked that in order to compute the function of the earth's moments of inertia which we have just been considering, we require not only the figure and dimensions of the earth and the law of distribution of density in its interior, but also its mean and surface densities. The experiments for determining the mean density have consisted in comparing the earth's attraction with the attraction either of a mountain or of a known thickness of the earth's crust or of a known mass of metal. In the case of mountains the comparisons have been made with plumb lines and pendulums; in the case of known layers of the earth's crust they have been made by swinging pendulums at the surface and down in mines; and in the case of known masses of metal they have been made with torsion balances, fine chemical balances, and pendulums. The surface density results from a study of the materials composing the earth's crust, but notwithstanding the apparent simplicity of that process it is doubtful if we have yet attained as accurate a result as in the case of the mean density.

Before quitting this part of our subject it is important to point out that the luni-solar precession can not be directly observed, but must be derived from the general precession. The former of these qualities depends only upon the action of the sun and moon, while the latter is affected in addition by the action of all the planets, and to ascertain what that is we must determine their masses. The methods of doing so fall into two great classes, according as the planets dealt with have or have not satellites. The most favourable case is that in which one or more satellites are present, because the mass of the primary follows immediately from their distances and revolution times; but even then there is a difficulty in the way of obtaining very exact results. By extending the observations over sufficiently long periods the revolution times may be ascertained with any desired degree of accuracy; but all measurements of the distance of a satellite from its primary are affected by personal equation, which we can not be sure of completely eliminating, and thus a considerable margin of uncertainty is brought into the masses. In the cases of Mercury and Venus, which have no satellites, and to a certain extent in the case of the earth also, the only available way of ascertaining the masses is from the perturbations produced by the action of the various planets on each other. These perturbations are of two kinds, periodic and secular. When sufficient data have been accumulated for the exact determination of the secular perturbations they will give the best results, but as yet it remains advantageous to employ the periodic perturbations also.

Passing now to the photo-tachymetrical methods, we have first to glance briefly at the mechanical appliances by which the tremendous velocity of light has been successfully measured. They are of the simplest possible character, and are based either upon a toothed wheel or upon a revolving mirror.

The toothed-wheel method was first used by Fizeau, in 1849. To understand its operation, imagine a gun-barrel with a toothed wheel revolving at right angles to its muzzle in such a way that the barrel is alternately closed and opened as the teeth and the spaces between them pass before it. Then, with the wheel in rapid motion, at the instant when a space is opposite the muzzle let a ball be fired. It will pass out freely, and after traversing a certain distance let it strike an elastic cushion and be reflected back upon its own path. When it reaches the wheel, if it hits a space it will return into the gun-barrel, but if it hits a tooth it will be stopped. Examining the matter a little more closely, we see that, as the ball requires a certain time to go and return, if during that time the wheel moves through an odd multiple of the angle between a space and a tooth the ball will be stopped, while if it moves through an even multiple of that angle the ball will return into the barrel. Now, imagine the gun-barrel, the ball, and the elastic cushion to be replaced, respectively, by a telescope, a light-wave, and a mirror. Then if the wheel moved at such a speed that the returning light-wave struck against the tooth following the space through which it issued, to an eye looking into the telescope all would be darkness. If the wheel moved a little faster and the returning light-wave passed through the space succeeding that through which it issued, the eye at the telescope would perceive a flash of light, and if the speed was continuously increased a continual succession of eclipses and illuminations would follow each other according as the returning light was stopped against a tooth or passed through a space farther and farther behind that through which it issued. Under these conditions the time occupied by the light in traversing the space from the wheel to the mirror and back again would evidently be the same as the time required by the wheel to revolve through the angle between the space through which the light issued and that through which it returned, and thus the velocity of light would become known from the distance between the telescope and the mirror, together with the speed of the wheel. Of course the longer the distance traversed and the greater the velocity of the wheel the more accurate would be the result.

The revolving-mirror method was first used by Foucault in 1862. Conceive the toothed wheel of Fizeau's apparatus to be replaced by a mirror attached to a vertical axis and capable of being put into rapid rotation. Then it will be possible so to arrange the apparatus that light issuing from the telescope shall strike the movable mirror and be reflected to the distant mirror, whence it will be returned to the movable mirror again, and being thrown back into the telescope will appear as a star in the centre of the field of view. That adjustment being made, if the mirror were caused to revolve at a speed of some hundred turns per second it would move through an appreciable angle while the light was passing from it to the distant mirror and back again, and, in accordance with the laws of reflection, the star in the field of the telescope would move from the centre by twice the angle through which the mirror had turned. Thus the deviation of the star from the centre of the field would measure the angle through which the mirror turned during the time occupied by light in passing twice over the interval between the fixed and revolving mirrors, and from the magnitude of that angle, together with the known speed of the mirror, the velocity of the light could be calculated.

In applying either of these methods the resulting velocity is that of light when traversing the earth's atmosphere, but what we want is its velocity in space, which we suppose to be destitute of ponderable material, and in order to obtain that the velocity in the atmosphere must be multiplied by the refractive index of air. The correct velocity so obtained can then be used to find the solar parallax, either from the time required by light to traverse the semidiameter of the earth's orbit, or from the ratio of the velocity of light to the orbital velocity of the earth.

Any periodic correction which occurs in computing the place of a heavenly body or the time of a celestial phenomenon is called by astronomers an equation, and as the time required by light to traverse the semidiameter of the earth's orbit first presented itself in the guise of a correction to the computed times of the eclipses of Jupiter's satellites, it has received the name of the light equation. The earth's orbit being interior to that of Jupiter, and both having the sun for their centre, it is evident that the distances between the two planets must vary from the sum to the difference of the radii of their respective orbits, and the time required by light to travel from one planet to the other must vary proportionately. Consequently, if the observed times of the eclipses of Jupiter's satellites are compared with the times computed upon the assumption that the two planets are always separated by their mean distance, it will be found that the eclipses occur too early when the earth is at less than its mean distance from Jupiter, and too late when it is farther off, and from large numbers of such observations the value of the light equation has been deduced.

The combination of the motion of light through our atmosphere with the orbital motion of the earth gives rise to the annual aberration, all the phases of which are computed from its maximum value, commonly called the constant of aberration. There is also a diurnal aberration due to the rotation of the earth on its axis, but that is quite small and does not concern us this evening. When aberration was discovered the corpuscular theory of light was in vogue, and it offered a charmingly simple explanation of the whole phenomenon. The hypothetical light corpuscles impinging upon the earth were thought to behave precisely like the drops in a shower of rain, and you all know that their apparent direction is affected by any motion on the part of the observer. In a calm day, when the drops are falling perpendicularly, a man standing still holds his umbrella directly over his head, but as soon as he begins to move forward he inclines his umbrella in the same direction, and the more rapidly he moves the greater must be its inclination in order to meet the descending shower. Similarly, the apparent direction of oncoming light corpuscles would be affected by the orbital motion of the earth, so that in effect it would always be the resultant arising from combining the motion of the light with a motion equal and opposite to that of the earth. But since the falsity of the corpuscular theory has been proved that explanation is no longer tenable, and as yet we have not been able to replace it with anything equally satisfactory based on the now universally accepted undulatory theory. In accordance with the latter theory we must conceive the earth as ploughing its way through the ether, and the point which has hitherto baffled us is whether or not in so doing it produces any disturbance of the ether which affects the aberration. In our present ignorance on that point we can only say that the aberration constant is certainly very nearly equal to the ratio of the earth's orbital velocity to the velocity of light, but we can not affirm that it is rigorously so.

The luminiferous ether was invented to account for the phenomena of light, and for two hundred years it was not suspected of having any other function. The emission theory postulated only the corpuscles which constitute light itself, but the undulatory theory fills all space with an imponderable substance possessing properties even more remarkable than those of ordinary matter, and to some of the acutest intellects the magnitude of this idea has proved an almost insuperable objection against the whole theory. So late as 1862 Sir David Brewster, who had gained a worldwide reputation by his optical researches, expressed himself as staggered by the notion of filling all space with some substance merely to enable a little twinkling star to send its light to us; but not long after Clerk Maxwell removed that difficulty by a discovery coextensive with the undulatory theory itself. Since 1845, when Faraday first performed his celebrated experiment of magnetizing a ray of light, the idea that electricity is a phenomenon of the ether had been steadily growing, until at last Maxwell perceived that if such were the fact the rate of propagation of an electro-magnetic wave must be the same as the velocity of light. At that time no one knew how to generate such waves, but Maxwell's theory showed him that their velocity must be equal to the number of electric units of quantity in the electro-magnet unit, and careful experiments soon proved that that is the velocity of light. Thus it was put almost beyond the possibility of doubt that the ether gives rise to the phenomena of electricity and magnetism, as well as to those of light, and perhaps it may even be concerned in the production of gravitation itself. What could be apparently more remote than these electric quantities and the solar parallax? And yet we have here a relation between them, but we make no use of it because as yet the same relation can be far more accurately determined from experiments upon the velocity of light.

Now, let us recall the quantities and methods of observation which we have found to be involved, either directly or indirectly, with the solar parallax. They are, the solar parallax, obtained from transits of Venus, oppositions of Mars, and oppositions of certain asteroids; the lunar parallax, found both directly and from measurements of the force of gravity at the earth's surface; the constants of precession, nutation, and aberration, obtained from observations of the stars; the parallactic inequality of the moon; the lunar inequality of the earth, usually obtained from observations of the sun, but recently found from heliometer observations of certain asteroids; the mass of the earth, found from the solar parallax and also from the periodic and secular perturbations of Venus and Mars; the mass of the moon, found from the lunar inequality of the earth and also from the ratio of the solar and lunar components of the ocean tides; the masses of all the planets obtained from observations of their satellites whenever possible, and when no satellites exist, then from observations of their mutual perturbations, both periodic and secular; the velocity of light, obtained from experiments with revolving mirrors and toothed wheels, together with laboratory determinations of the index of refraction of atmospheric air; the light equation, obtained from observations of the eclipses of Jupiter's satellites; the figure of the earth, obtained from geodetic triangulations, measurements of the length of the seconds-pendulum in various latitudes, and observations of certain perturbations of the moon; the mean density of the earth, obtained from measurements of the attractions of mountains, from pendulum experiments in mines, and from experiments on the attraction of known masses of matter made either with torsion balances or with the most delicate chemical balances; the surface density of the earth, obtained from geological examinations of the surface strata; and, lastly, the law of distribution of density in the interior of the earth, which in the present state of geological knowledge we can do little more than guess at.

Here, then, we have a large group of astronomical, geodetic, geological, and physical quantities which must all be considered in finding the solar parallax, and which are all so entangled with each other that no one of them can be varied without affecting all the rest. It is therefore impossible to make an accurate determination of any one of them apart from the remainder of the group, and thus we are driven to the conclusion that they must all be determined simultaneously. Such has not been the practice of astronomers in the past, but it is the method to which they must inevitably resort in the future. A cursory glance at an analogous problem occurring in geodesy may be instructive. When a country is covered with a net of triangles it is always found that the observed angles are subject to a certain amount of error, and a century ago it was the habit to correct the angles in each triangle without much regard to the effect upon adjacent triangles. Consequently the adjustment of the errors was imperfect, and in computing the interval between any two distant points the result would vary somewhat with the triangles used in the computation—that is, if one computation was made through a chain of triangles running around on the right-hand side, another through a chain of triangles running straight between the two points, and a third through a chain of triangles running around on the left-hand side, the results were usually all different. At that time things were less highly specialized than now, and all geodetic operations were yet in the hands of first-rate astronomers, who soon devised processes for overcoming the difficulty. They imagined every observed angle to be subject to a small correction, and as these corrections were all entangled with each other through the geometrical conditions of the net, by a most ingenious application of the method of least squares they determined them all simultaneously in such a way as to satisfy the whole of the geometrical conditions. Thus the best possible adjustment was obtained, and no matter what triangles were used in passing from one point to another, the result was always the same. That method is now applied to every important triangulation, and its omission would be regarded as proof of incompetency on the part of those in charge of the work.

Now, let us compare the conditions existing respectively in a triangulation net and in the group of quantities for the determination of the solar parallax. In the net every angle is subject to a small correction, and the whole system of corrections must be so determined as to make the sum of their weighted squares a minimum and at the same time satisfy all the geometrical conditions of the net. Like the triangles, the quantities composing the group from which the solar parallax must be determined are all subject to error, and therefore we must regard each of them as requiring a small correction, and all these corrections must be so determined as to make the sum of their weighted squares a minimum, and at the same time satisfy every one of the equations expressing the relations between the various components of the group.

Thus it appears that the method required for adjusting the solar parallax and its related constants is in all respects the same as that which has so long been used for adjusting systems of triangulation; and as the latter method was invented by astronomers, it is natural to inquire, Why have they not applied it to the fundamental problem of their own science? The reasons are various, but they may all be classed under two heads: First, an inveterate habit of overestimating the accuracy of our own work as compared with that of others; and, second, the unfortunate effect of too much specialization.

The prevailing opinion certainly is that great advances have recently been made in astronomy, and so they have in the fields of spectral analysis and in the measurement of minute quantities of radiant heat; but the solution of the vast majority of astronomical problems depends upon the exact measurement of angles, and in that little or no progress has been made. Bradley, with his zenith sector a hundred and fifty years ago, and Bessel and Struve, with their circles and transit instruments seventy years ago, made observations not sensibly inferior to those of the present day, and indeed it would have been surprising if they had not done so. The essentials for accurately determining star-places are a skilled observer, a clock, and a transit circle, the latter consisting of a telescope, a divided circle, and four micrometer microscopes. Surely no one will claim that we have to-day any more skilful observers than were Bessel, Bradley, and Struve, and the only way in which we have improved upon the telescopes made by Dollond one hundred and thirty years ago is by increasing their aperture and relatively diminishing their focal distance. The most famous dividing engine now in existence was made by the elder Repsold seventy-five years ago; but as the errors of divided circles and their micrometer microscopes are always carefully determined, the accuracy of the measured angles is quite independent of any small improvement in the accuracy of the divisions or of the micrometer screws. Only in the matter of clocks has there been some advance, and even that is not very great. On the whole, the star-places of to-day are little better than those of seventy-five years ago, but even yet there is great room for improvement. One of the commonest applications of these star-places is to the determination of latitude, but it is very doubtful if there is any point on the face of the earth whose latitude is known certainly within one tenth of a second.

Looking at the question from another point of view, it is notorious that the contact observations of the transits of Venus in 1761 and 1769 were so discordant that from the same observations Encke and E. J. Stone got respectively for the solar parallax 8.59″ and 8.91″. In 1870 no one thought it possible that there could be any difficulty with the contact observations of the then approaching transits of 1874 and 1882, but we have found from sad experience that our vaunted modern instruments gave very little better results for the last pair of transits than our predecessors obtained with much cruder appliances in 1761 and 1769.

The theory of probability and uniform experience alike show that the limit of accuracy attainable with any instrument is soon reached; and yet we all know the fascination which continually lures us on in our efforts to get better results out of the familiar telescopes and circles which have constituted the standard equipment of observatories for nearly a century. Possibly these instruments may be capable of indicating somewhat smaller quantities than we have hitherto succeeded in measuring with them, but their limit can not be far off, because they already show the disturbing effects of slight inequalities of temperature and other uncontrollable causes. So far as these effects are accidental they eliminate themselves from every long series of observations, but there always remains a residuum of constant error, perhaps quite unsuspected, which gives us no end of trouble. Encke's value of the solar parallax affords a fine illustration of this. From the transits of Venus in 1761 and 1769 he found 8.58″ in 1824, which he subsequently corrected to 8.57″, and for thirty years that value was universally accepted. The first objection to it came from Hansen in 1854, a second followed from Leverrier in 1858, both based upon facts connected with the lunar theory, and eventually it became evident that Encke's parallax was about one fourth of a second too small.

Now please observe that Encke's value was obtained trigonometrically, and its inaccuracy was never suspected until it was revealed by gravitational methods, which were themselves in error about one tenth of a second, and required subsequent correction in other ways. Here, then, was a lesson to astronomers, who are all more or less specialists, but it merely enforced the perfectly well-known principle that the constant errors of any one method are accidental errors with respect to all other methods, and therefore the readiest way of eliminating them is by combining the results from as many different methods as possible. However, the abler the specialist the more certain he is to be blind to all methods but his own, and astronomers have profited so little by the Encke-Hansen-Leverrier incident of thirty-five years ago that to-day they are mostly divided into two great parties, one of whom holds that the parallax can be best determined from a combination of the constant of aberration with the velocity of light, and the other believes only in the results of heliometer measurements upon asteroids. By all means continue the heliometer measurements and do everything possible to clear up the mystery which now surrounds the constant of aberration; but why ignore the work of predecessors who were quite as able as ourselves? If it were desired to determine some one angle of a triangulation net with special exactness, what would be thought of a man who attempted to do so by repeated measurements of the angle in question while he persistently neglected to adjust the net? And yet until very recently astronomers have been doing precisely that kind of thing with the solar parallax. I do not think there is any exaggeration in saying that the trustworthy observations now on record for the determination of the numerous quantities which are functions of the parallax could not be duplicated by the most industrious astronomer working continuously for a thousand years. How, then, can we suppose that the result properly deducible from them can be materially affected by anything that any of us can do in a lifetime unless we are fortunate enough to invent methods of measurement vastly superior to any hitherto imagined? Probably the existing observations for the determination of most of these quantities are as exact as any that can ever be made with our present instruments, and if they were freed from constant errors they would certainly give results very near the truth. To that end we have only to form a system of simultaneous equations between all the observed quantities and then deduce the most probable values of these quantities by the method of least squares. Perhaps some of you may think that the value so obtained for the solar parallax would depend largely upon the relative weights assigned to the various quantities, but such is not the case. With almost any possible system of weights the solar parallax will come out very nearly 8.809″ ± 0.0057″, whence we have for the mean distance between the earth and the sun 92,797,000 miles, with a probable error of only 59,700 miles; and for the diameter of the solar system, measured to its outermost member, the planet Neptune, 5,578,400,000 miles.

THE STABILITY OF THE SOLAR SYSTEM

BY

ORMSBY McKNIGHT MITCHEL

(Born 1810; died 1862)

THE
STABILITY OF THE SOLAR SYSTEM [1]

WHEN, by the application of a single great law, the mind had succeeded in resolving the difficult problems presented by the motions of the earth and its satellite, the moon, it rose to the examination of the higher and more complicated questions of the stability of the entire sytem of planets, satellites, and comets, which are found to pursue their courses round the sun. The number of bodies involved in this investigation, their magnitudes and vast periods of revolution, their great distances from the observer, and the exceeding delicacy of the required observations, combined with the high interest which attaches itself to the final results, have united to render this investigation the most wonderful which has ever employed the energies of the human mind.

To comprehend the dignity and importance of this great subject, let us rapidly survey the system, and moving outward to its known boundaries, mark the number and variety of worlds involved in the investigation. Beginning, then, at the great centre, the grand controlling orb, the sun, we find its magnitude such as greatly to exceed the combined masses of all its attendant planets. Indeed, if these could all be arranged in a straight line on the same side of the sun, so that their joint effect might be exerted on that body, the centre of gravity of the entire system, thus located, would scarcely fall beyond the limits of the sun's surface. At a mean distance of 36,000,000 miles from the sun we meet the nearest planet, Mercury, revolving in an

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[1] From "Planetary and Stellar Worlds."

orbit of considerable eccentricity, and completing its circuit around the sun in a period of about 88 of our days. This world has a diameter of only 3,140 miles, and is the smallest of the old planets. Pursuing our journey, at a distance of 68,000,000 miles from the sun, we cross the orbit of the planet Venus. Her magnitude is nearly equal to that of the earth. Her diameter is 7,700 miles, and the length of her year is nearly 225 of our days. The next planet we meet is the earth, whose mean distance from the sun is 95,000,000 miles. The peculiarities which mark its movements and those of its satellite have been already discussed. Leaving the earth, and continuing our journey outward, we cross the orbit of Mars, at a mean distance from the sun of 142,000,000 miles. This planet is 4,100 miles in diameter, and performs its revolution around the sun in about 687 days, in an orbit but little inclined to the plane of the ecliptic. Its features, as we shall see hereafter, are more nearly like those of the earth than any other planet. Beyond the orbit of Mars, and at a mean distance from the sun of about 250,000,000 miles, we encounter a group of small planets, eight in number, presenting an anomaly in the system, and entirely different from anything elsewhere to be found. These little planets are called asteroids. Their orbits are, in general, more eccentric, and more inclined to be ecliptic, than those of the other planets; but the most remarkable fact is this, that their orbits are so nearly equal in size that, when projected on a common plane, they are not inclosed the one within the other, but actually cross each other.

We shall return to an examination of these wonderful objects hereafter. At a mean distance of 485,000,000 miles from the sun we cross the orbit of Jupiter, the largest and most magnificent of all the planets. His diameter is nearly 90,000 miles. He is attended by four moons, and performs his revolution round the sun in a period of nearly twelve years. Leaving this vast world, and continuing our journey to a distance of 890,000,000 miles from the sun, we cross the orbit of Saturn, the most wonderful of all the planets. His diameter is 76,068 miles, and he sweeps round the sun in a period of nearly 29½ years. He is surrounded by several broad, concentric rings, and is accompanied by no fewer than seven satellites or moons. The interplanetary spaces, we perceive, are rapidly increasing. The orbit of Uranus is crossed at a mean distance from the sun of 1,800,000,000 miles. His diameter is 35,000 miles and his period of revolution amounts to rather more than 84 of our years. He is attended by six moons, and pursues his journey at a slower rate than any of the interior planets. Leaving this planet, we reach the boundary of the planetary system at a distance of about 3,000,000,000 miles from the sun. Here revolves the last discovered planet, Neptune, attended by one, probably by two moons, and completing his vast circuit about the sun in a period of 164 of our years. His diameter is eight times greater than the earth's, and he contains an amount of matter sufficient to form one hundred and twenty-five worlds such as ours.

Here we reach the known limit of the planetary worlds, and standing at this remote point and looking back toward the sun, the keenest vision of man could not descry more than one solitary planet along the line we have traversed. The distance is so great that even Saturn and Jupiter are utterly invisible, and the sun itself has shrunk to be scarcely greater than a fixed star.

There are certain great characteristics which distinguish this entire scheme of worlds. They are all nearly globular, they all revolve on axes, their orbits are all nearly circular, they all revolve in the same direction around the sun, the planes of their orbits are but slightly inclined to each other, and their moons follow the same general laws. With a knowledge of these general facts, it is proposed to trace the reciprocal influences of all these revolving worlds, and to learn, if it be possible, whether this vast scheme has been so constructed as to endure while time shall last, or whether the elements of its final dissolution are not contained within itself, either causing the planets, one by one, to drop into the sun, or to recede from this great centre, released from its influence, to pursue their lawless orbits through unknown regions of space.

Before proceeding to the investigation of the great problem of the stability of the universe, let us examine how far the law of gravitation extends its influence over the bodies which are united in the solar system. A broad and distinct line must be drawn between those phenomena for which gravitation must render a satisfactory account, and those other phenomena for which it is in no wise responsible. In the solar system we find, for example, that all the planets revolve in the same direction around the sun, in orbits slightly elliptical, and in planes but little inclined to each other. Neither of these three peculiarities is in any way traceable to the law of gravitation.

Start a planet in its career, and, no matter what be the eccentricity of its orbit, the direction of its movement, or the inclination of the plane in which it pursues its journey, once projected, it falls under the empire of gravitation, and ever afterward this law is accountable for all its movements. We are not, therefore, to regard the remarkable constitution of the solar system as a result of any of the known laws of Nature.

If the sun were created, and the planetary worlds formed and placed at the disposal of a being possessed of less than infinite wisdom, and he were required so to locate them in space, and to project them in orbits, such that their revolutions should be eternal, even with the assistance of the known laws of motion and gravitation, this finite being would fail to construct his required system.

Let it be remembered that each and every one of these bodies exerts an influence upon all the others. There is no isolated object in the system. Planet always planet, and satellite bends the orbit of satellite, until the primitive curves described lose the simplicity of their character, and perturbations arise, which may end in absolute destruction. There is no chance work in the construction of our mighty system. Every planet has been weighed and poised, and placed precisely where it should be. If it were possible to drag Jupiter from its orbit, and cause him to change places with the planet Venus, this interchange of orbits would be fatal to the stability of the entire system. In contemplating the delicacy and complexity of the adjustment of the planetary worlds, the mind can not fail to recognise the fact that, in all this intricate balancing, there is a higher object to be gained than the mere perpetuity of the system.

If stability had been the sole object it might have been gained by a far simpler arrangement. If God had so constituted matter that the sun might have attracted the planets, while these should exert no influence over each other—that the planets might have attracted their satellites, while these were free from their reciprocal influences—then, indeed, a system would have been formed whose movements would have been eternal and whose stability would have been independent of the relative positions of the worlds and the character of their orbits. Give to them but space enough in which to perform their revolutions around the sun, so that no collisions might occur, freed from this only danger, and every planet and every satellite will pursue the same undeviating track throughout the ceaseless ages of eternity.

If this statement be true, it may be demanded why such a system was not adopted. It is impossible for us to assign all the reasons which led to the adoption of the present complicated system. Of one thing, however, we are certain: If God designed that in the heavens his glory and his wisdom should be declared, and that in the study of his mighty works his intelligent creatures should rise higher and higher toward his eternal throne, then, indeed, has the present system been admirably constituted for the accomplishment of this grand design. To have acquired a knowledge of a system constituted of independent planets, free from all mutual perturbations, would have required scarcely no effort to the mind, when compared with that put forth in the investigation of the present complex construction of the planetary system. The mind would have lost the opportunity of achieving its greatest triumphs, while the evidence of infinite wisdom displayed in the arrangement and counterpoising of the present system would have been lost forever. There is one other thought which here suggests itself with so much force that I can not turn away from it. We speak of gravitation as some inherent quality or property of matter, as though matter could not exist in case it were deprived of this quality. This is, however, a false idea. Matter might have existed independent of any quality which should cause distant globes to influence each other. The force called gravitation, even admitting that it must have an existence, no special law of its action could have forced itself on matter to the exclusion of all other laws. Why does this force diminish as the square of the distance at which it operates increases? There are almost an infinite number of laws according to which an attraction might have exerted itself, but there is no one which would have rendered the planets fit abodes for sentient beings, such as now dwell on them, and which would at the same time have guaranteed the perpetuity of the system. Admitting, then, that matter can not be matter without exerting some influence on all other matter (which I am unwilling to admit), in the selection of the law of the inverse square of the distance there is the strongest evidence of design.

If we rise above the law of gravitation to the Great Author of Nature, and regard the laws of motion and of gravitation nothing more than the uniform expressions of his will, we perceive at once the impossibility of constructing the universe in such manner that the sun should attract the planets, without these attracting each other; or that the planets should attract their satellites without, in turn, being reciprocally influenced by their satellites; for this would be equivalent to saying that the will of the same Almighty Being should exert itself, and not exert, at the same moment, which is impossible. As there is but one God, so there is but one kind of matter, governed by one law, applied by infinite wisdom to the formation of suns and systems without number, crowding the illimitable regions of space, all moving harmoniously, fulfilling their high destiny, and all sustained by the single arm of divine Omnipotence.

We now proceed to an examination of the great question, Is the system of worlds by which we are surrounded, and of which our earth and its moon form a part, so constructed that, under the operation of the known laws of Nature, it shall forever endure, without ever passing certain narrow limits of change, which do not in any way involve its stability?

It is well known that the planets revolve in elliptical orbits of small eccentricity—that under the action of the primitive impulse by which they were projected in their orbits they would have moved off in a straight line, with a velocity proportioned to the intensity of the impulse, and which would have endured forever; but being seized by the central attraction of the sun, at the moment of starting in their career, the joint action of these two forces bends the planet from its straight direction and causes it to commence a curvilinear path, which carries it round the sun.

The question which first presents itself is this: If the central force lodged in the sun has the power to cause a planet to diverge from the straight line in which, but for this, it would have moved, if it draw it into a curved path, will not this central force, which is ever active, finally overcome entirely the impulsive force originally given to the planet, draw it closer and closer to the sun in each successive revolution, in a spiral orbit, until, finally, the planet shall fall into the sun and be destroyed forever? This question arises independent of the extraneous influence which the planets exert over each other. It refers to a solitary globe revolving around the sun, under the influence of a central force which varies its action as does the law of gravitation. The problem has been submitted to the most rigorous mathematical examination, and a result has been obtained which settles the question in the most absolute manner. The amount by which the central force, in a moment of time, overcomes the effect produced by the primitive impulse, is a quantity infinitely small, and of the second order. If it were found to be infinitely small in each moment of time, then might it accumulate so that, at the end of a vast period, it might become finite and appreciable. But because it is of the second order of infinitely small quantities, before it can become an infinitely small quantity of the first order a period equal to infinite ages must roll by, and to make a finite appreciable quantity out of this, an infinite cycle of years must roll round an infinite number of times.

Such is the answer given by analysis to this wonderful question. " Is there no change? " demands the astronomer. " Yes," answers the all-seeing analysis. " When will it become appreciable? " asks the astronomer. " At the end of a period infinitely long, repeated an infinite number of times," is the reply.

Having settled this important question, it remains now to examine whether the mutual attractions of the planets on each other may not, in the end, change permanently the form of their orbits, and lead ultimately to the destruction of the system. To comprehend more readily the nature of the examination, let us review the points involved in the permanency of our orbit.

Take, for example, our own planet, the earth. It now revolves in an elliptic orbit, whose magnitude is determined by the length of its longer axis and by its eccentricity. These elements are readily deduced from observation. If it were possible to construct this orbit of some material like wire, which would permit us to take it up and locate it in space at will, to enable us to give it the position now occupied by the actual orbit of the earth, we must first carry its focus to the sun's centre; we must then turn its longer axis around this centre as a fixed point until the nearest vertex of the wire orbit shall fall upon that point of the earth's orbit which is at this time nearest to the sun. Having accomplished this, the axes will coincide in their entire length, and to make the orbits coincide we must revolve the artificial one around the now common axis until its plane shall fall upon the actual orbit of the earth.

If, now, change should ever come in the absolute coincidence of these two orbits, regarding the iron one as fixed and permanent, the orbit of Nature may vary from it in any one or all of the following ways: 1. The natural orbit, all other things remaining the same, may leave the fixed orbit by a variation of eccentricity—that is, it may become more or less nearly circular. 2. The planes of the orbits remaining coincident, the curves may separate from each other, in consequence of an angular movement of the longer axis of the natural orbit, by means of which the vertex of the natural curve shall be carried to the right or to the left of the vertex of the fixed one. 3. While these causes are operating to produce change, an increase of deviation may be occasioned by the fact that the two planes may become inclined to each other, thus causing the natural orbit to lie partly above and partly below the fixed one. These, then, are the several ways in which the orbits of the planets may change; and to settle the question of stability, we must ascertain whether these changes actually exist, and whether any of them, in case they do exist, and are progressing constantly in the same direction, will ever prove fatal to the permanency of the system, finally accomplishing its absolute destruction, or rendering it unfit for the sustentation of that life which now exists upon the planet.

By a close examination of this great subject, both theoretically and practically, it is found that the system is so constituted that not a single planet or satellite revolves in an orbit absolutely invariable. Theory demonstrates that such changes must exist, and observation confirms this great truth by showing that they actually do exist.

Draw, in imagination, a straight line from the sun's centre through the perihelion, or nearest point to the sun of the earth's orbit, and let it be extended to the outermost limits of the entire system. On this locate the perihelion points of the orbits of all the planets, and in these points fix the planets themselves. They are now all on the same side of the sun, the longer axes of their orbits are in the same direction, and they are located at their nearest distance from the sun, or in perihelion. The planes of the orbits are inclined to each other under their proper angles, and they all intersect in a common line of nodes passing through the sun's centre. Now give the entire group of planets their primitive impulse, and at the same instant they start in their respective orbits round the sun. Now, in case no perturbations existed, the perihelion-points, the inclinations, and the lines of nodes would remain fixed forever, and although millions of years might pass away before the planets would again resume their primitive position with reference to each other, yet the time would come when a final restoration would be effected.

At the end of 164 years Neptune will have completed its revolution round the sun, and will return to its starting point. All the other planets will have performed several revolutions, but each, on reaching the point of departure, will find the perihelion of its orbit changed in position, the inclination altered, and the line of nodes shifted. These changes continue until the longer axes of the orbits, which once coincided, radiate from the sun in all directions. The lines of nodes, once common, now diverge under all angles, the inclinations increasing or decreasing, and even the figures of the orbits undergoing constant mutation; and the grand question arises, whether these changes, no matter how slow, are ever to continue progressing in the same direction until all the original features of the system shall be effaced, and the possibility of return to the primitive condition destroyed forever.

Such a problem would seem to be far too deep and complicated ever to be grasped by the human intellect. It is true that no single mind was able to accomplish its complete solution, but the advance made by one has been steadily increased by another, until finally not a question remains unanswered. The solution is complete, yielding results of the most wonderful character.

We shall examine this great problem in detail, and begin with the figure of the orbit of any planet—our earth, for example.

The amount of heat received from the sun by the earth depends, other things being the same, on the minor axis of its elliptic orbit. Any change in the eccentricity operates directly to increase or decrease the shorter axis, and consequently to increase or decrease the mean annual amount of heat received from the sun. Now, we know that animal and vegetable life is adjusted in such a way that it requires almost exact uniformity in the mean annual amount of heat which it shall enjoy. An increase or decrease of two or three degrees in temperature would make an entire revolution in the animals and plants belonging to the region experiencing such a change. If, then, it be true that the eccentricity of the earth's orbit is actually changing under the combined action of the other planets, may this change continue so far as to subvert the order of Nature on its surface? This question has been answered in the most satisfactory manner.

It is found that the greatest axes of the planetary orbits are subjected to slight and temporary variations, returning in comparatively short periods to their primitive values. This important fact guarantees the permanency of the periodic times, so that it becomes possible to deduce, with the utmost precision, the periodic times of the planets, from the mean of a large number of revolutions. That of the earth is now so accurately known, and so absolutely invariable, that we know what it will be a million of years hence, should the system remain as it now is, as perfectly as at the present moment. But neither of these elements secures the stability of the eccentricity or of the minor axis. Lagrange, however, demonstrated a relation between the masses of the planets, their major axes and eccentricities—such that, while the masses remain constant and the axes invariable, the eccentricity can only vary its value through extremely narrow limits. These limits have been assigned beyond which the change can never pass, and within these narrow bounds we find that the orbits of all the planets are slowly vibrating backward and forward in periods which actually stun the imagination.

This remarkable law for the preservation of the system would not hold in any other organization. It demands orbits nearly circular, with planes nearly coincident with the periodic times related as are those of the planets, and the planets themselves located as they actually are. No interchange of orbits is admissible; but, constituted as the system now is, the perpetuity is absolutely certain, so far as the change of eccentricity is concerned.

Let us now examine the changes which affect the position of the major axis in its own plane. The perihelion of every orbit is found to be slowly advancing. Nor is this distance ever to be changed into a retrograde motion. The movement is ever progressive in the same direction, and the perihelion-points of all the orbits are slowly sweeping round the sun. That of the earth's orbit accomplishes its revolution in 111,000 years! How wonderful the fact that such discoveries should be made by man, whose entire life is but a minute fraction of these vast periods of time!

Owing to a retrograde motion in the vernal equinox, carrying it around in the opposite direction in 25,868 years, the perihelion and equinox pass each other once in 20,984 years. Knowing their relative position at this moment, and their rates of motion, it is easy to compute the time of their coincidence. Their last coincidence took place 4,089 years before the Christian era, or about the epoch usually assigned for the creation of man. The effect of the coincidence of the perihelion with the vernal equinox is to cause an exact equality in the length of spring and summer, compared with autumn and winter. In other language, the sun will occupy exactly half a year in passing from the vernal to the autumnal equinox, and the other half in moving from the autumnal to the vernal equinox.

At present the line of equinoxes divides the earth's elliptic orbit into two unequal portions. The smaller part is passed over in the fall and winter, causing the earth to be nearer the sun at this season than in summer, and making a difference in the length of the two principal seasons, summer and winter, of some seventeen and a half days. This inequality, which is now in favour of summer, will eventually be destroyed, and the time will come when the earth will be farthest from the sun during the winter and nearest in the summer. But at the end of a great cycle of more than 20,000 years all the changes will have been gone through, and in this respect a complete compensation and restoration will have been effected.

This epoch of subordinate restoration will find the perihelion of the earth's orbit located in space far distant from the point primitively occupied. Five of these grand revolutions of 20,984 years must roll round before the slow movement of the perihelion shall bring it back to its starting point. One hundred and ten thousand years will then restore the axis of the earth's orbit, and the equinoctial line, nearly to their relative positions to each other, and to the same region of absolute space occupied at the beginning of this grand cycle. If, now, we direct our attention to the other planets, we find their perihelion-points all slowly advancing in the same direction. That of the orbit of Jupiter performs its revolution round the sun in 186,207 years, while the perihelion of Mercury's orbit occupies more than 200,000 years in completing its circuit round the sun. To effect a complete restoration of the planetary orbits to their original position with reference to their perihelion-points will require a grand compound cycle amounting to millions of years. Yet the time will come when all the orbits will come again to their primitive positions, to start once more on their ceaseless journeys.

In the changes of the eccentricities, it will be remembered, the stability of the system was involved. Should these changes be ever progressive, no matter how slowly, a time would finally come when the original figure of the orbit would be destroyed, the planet either falling into the sun, or sweeping away into unknown regions of space. But a limit is assigned, beyond which the change can never pass. Some of the planetary orbits are becoming more circular, others growing more elliptical; but all have their limits fixed. The earth's orbit, for example, should the present rate of decrease of eccentricity continue, in about half a million years will become an exact circle. There the progressive motion of the changes stops, and it slowly commences to recover its ellipticity. This is not the case with the motion of the perihelions. Their positions are in no way involved in the well-being of a planet or in its capacity to sustain the life which exists on its surface; and since the stability of the system is not endangered by progressive change, it ever continues in the same direction, until the final restoration is effected, by an entire revolution about the sun.

Let us now examine the inclinations of the planetary orbits. Here it is found that there is no guarantee for the stability of the system, provided the angles under which the orbits of the planets are inclined to each other do not remain nearly the same forever. If changes are found to exist, by which the inclinations are made to increase, without stopping and returning to their primitive condition, then is the perpetuity of the system rendered impossible. Its fair proportions must slowly wear away, the harmony which now prevails be destroyed, and chaos must come again.

Commencing again with the earth, we find that from the earliest ages the inclination of the earth's equator to the ecliptic has been decreasing. Since the measure of Eratosthenes, 2,078 years ago, the decrease has amounted to about 23′ 44″, or about half a second every year. Should the decrease continue, in about 85,000 years the equator and ecliptic would coincide, and the order of Nature would be entirely changed; perpetual spring would reign throughout the year, and the seasons would be lost forever. Of this, however, there is no danger. The diminution will reach its limit in a comparatively short time, when the decrease of inclination will change into an increase, and thus slowly rocking backward and forward in thousands of years, the seasons shall ever preserve their appointed places, and seedtime and harvest shall never fail. These changes of inclination are principally due to the perturbations of Venus, and, arising from configurations, will be ultimately entirely compensated.

The angles under which the planetary orbits are inclined to each other are in a constant state of mutation. The orbit of Jupiter at this time forms an angle with the ecliptic of 4,731 seconds, and this angle is decreasing at such a rate that in about 20,000 years the planes would actually coincide. This would not affect the well-being of the planets or the stability of the system; but should the same change now continue, the angle between the orbits might finally come to fix them even at right angles to each other, and a subversion of the present system would result.

A profound investigation of the problem of the planetary inclinations, accomplished by Lagrange, resulted in the demonstration of a relation between the masses of the planets, the principal axes of their orbits, and the inclinations, such that, although the angles of inclination may vary, the limits are narrow, and they are all found slowly to oscillate about their mean positions, never passing the prescribed limits, and securing in this particular the perpetuity of the system.

Here, again, we are presented with the remarkable fact that whenever mutation involves stability, this mutation is of a compensatory character, always returning upon itself, and in the long run correcting its own effects. If all this mighty system was organized by chance, how happens it that the angular motions of the perihelia of the planetary orbits are ever progressive, while the angular motions of the planes of the orbits are vibrating? Design, positive and conspicuous, is written all over the system in characters from which there is no escape.

We now proceed to an examination of the lines in which the planes of the planetary orbits cut each other, or the lines in which they intersect a fixed plane. These are called the lines of nodes. They all pass through the sun's centre, and, in case they ever were coincident, they now radiate from a common point in all directions.

Here is an element in no degree involving in its value the stability of the system, and from analogy we already begin to anticipate that its changes, whatever they may be, will probably progress always in the same direction. This is actually the case. The nodes of the planetary orbits are all slowly retrograding on a fixed plane; and in vast periods, amounting to thousands of years, accomplish revolutions which in the end return them to their primitive positions.

Thus are we led to the following results: Of the two elements which fix the magnitude of the planetary orbits, the principal axes, and the eccentricity, the axes remain invariable, while the eccentricity oscillates between narrow and fixed points. In the long run, therefore, the magnitudes of the orbits are preserved.

Of the three elements which give position to the planetary orbits—viz., the place of the perihelion, the lines of nodes, and the inclinations—the first two ever vary in the same direction, and accomplish their restoration at the end of vast periods of revolution, while the inclinations vibrate between narrow and prescribed limits.

One more point, and we close this wonderful investigation. The last question which presents itself is this: May not the periodic times of the planets be so adjusted to each that the results of certain configurations may ever be repeated without any compensation, and thus, by perpetual accumulation, finally effect a destruction of the system?

If the periodic times of two neighbourng planets were exact multiples of the same quantity, or if the one was double the other, or in any exact ratio, then the contingency above alluded to would arise, and there would be perturbations which would remain uncompensated. A near approach to this condition of things actually exists in the system, and gave great trouble to geometers. It was found, in comparing observations, that the mean periods of Jupiter and Saturn were not constant—that one was on the decrease while the other was on the increase. This discovery seemed to disprove the great demonstration which had fixed as invariable the major axes of the planetary orbits, and guaranteed the stability of the mean motions. It was not until after Laplace had instituted a long and laborious research that the phenomenon was traced to its true origin, and was found to arise from the near commensurability of the periodic times of Jupiter and Saturn—five of Jupiter's periods being nearly equal to two of Saturn's. In case the equality were exact, it is plain that if the two planets set out from the same straight line drawn from the sun, at the end of a cycle of five of Jupiter's periods, or two of Saturn's, they would be again found in the same relative positions, and whatever effect the one planet had exerted over the other would again be repeated under the same precise circumstances. Hence would arise derangements which would progress in the same direction, and eventually lead to the permanent derangement of the system.

But it happens that five of Jupiter's periods are not exactly equal to two of Saturn's, and in this want of equality safety is found. The difference is such that the point of conjunction of the planets does not fall at the same points of their orbits, but at the end of each cycle is in advance by a few degrees. Thus the conjunction slowly works round the orbits of the planets, and in the end the effect produced on one side of the orbit is compensated for on the other, and a mean period of revolution comes out for both planets, which is invariable. In the case of Jupiter and Saturn, the entire compensation is not effected until after a period of nearly a thousand years.

A similar inequality is found to exist between the earth and Venus, with a period much shorter, and producing results much less easily observed. In no instance do we find the periods of any two planets in an exact ratio. They are all incommensurable with each other, and in this peculiar arrangement we find the stability of the entire system is secured.

So far, then, as the organization of the great planetary system is concerned, we do not find within itself the elements of its own destruction. Mutation and change are everywhere found—all is in motion, orbits expanding or contracting, their planes rocking up and down, their perihelia and nodes sweeping in opposite directions round the sun—but the limits of all these changes are fixed; these limits can never be passed, and at the end of a vast period, amounting to many millions of years, the entire range of fluctuation will have been accomplished, the entire system—planets, orbits, inclinations, eccentricities, perihelia, and nodes—will have regained their original values and places, and the great bell of eternity will have then sounded " One."

Having reached the grand conclusion of the stability of the system of planets in their reciprocal influences, and that no element of destruction is found in the organization, we propose next to inquire whether the same features are stamped on the subordinate groups composing the planetary system. As our limits will not permit us to enter into a full examination of all the subordinate groups, we shall confine our remarks to our own earth and its satellite, Jupiter and his satellites, and to Saturn, his rings and moons. We shall, in this examination, find it practicable to answer, to some extent, the inquiry as to whether either of these systems has received any shock from external causes. We know nothing as to the future, and can, in this particular, only form our conjectures as to what is to be from what has been.

We commence our inquiry by an examination of two questions—viz., Is the velocity of rotation of the earth on its axis absolutely invariable? Has the relation between the earth and moon ever been disturbed by any external cause? There is nothing so important to the well-being of our planet and its inhabitants as absolute invariability in the period of its axial rotation. The sidereal day is the great unit of measure for time, and is of the highest consequence in all astronomical investigations. If causes are operating, either to increase or decrease the velocity of rotation, a time will come when the earth will cease to rotate, or else acquire so great a velocity as to destroy its figure, and, in the end, scatter its particles in space.

It is difficult to ascertain from theory a perfectly satisfactory answer to the question of the invariable velocity of rotation of the earth, but Laplace has demonstrated that the length of the day has not varied by the hundredth of one second during the last 2,000 years—that is, the length of the day is neither greater nor less than it was 2,000 years ago by the hundredth of a second. The reasoning leading to this remarkable result is simple, and may be readily comprehended by all. Two thousand years ago the duration of the moon's period of revolution around the earth was accurately determined and was expressed in days and parts of a day. The measure of the same period has been accomplished in our own time, and is expressed in days and parts of a day. Now, all the causes operating to change the moon's period of revolution are known, and may be applied. When this is done, it is found that the moon's period now and 2,000 years ago agree precisely, being accomplished in the same number of days and parts of a day—which would be impossible if the unit of a measure, the day, had varied ever so slightly.

The extraordinary relation existing between the moon's period in her orbit and the time occupied in her axial rotation gives us the opportunity of ascertaining whether our system has received any external shock. These two periods are so accurately adjusted that in all respects an exact equality exists. The moon ever turns the same hemisphere to the earth, and ever will, unless some external cause should arise to disturb the perfect harmony which now reigns. It is not my purpose to explain why it is that this phenomenon exists. I merely desire to state that this delicate balancing of periods furnishes an admirable evidence that, for several thousand years, at least no shock has been received by the earth and its satellite. Steadily have they moved in their orbits, subject only to the influence of causes originating in the constitution of the mighty system of which they constitute a part.

Moving out to a more complex system, we find in the remarkable arrangement of the satellites of Jupiter a delicate test for the action of sudden and extraneous causes. Here we find the periodic times of the satellites so related that 1,000 periods of the first added to 2,000 periods of the third will be precisely equal to 3,000 periods of the second. This delicate balancing of periods would be destroyed by the action of any external shock, such as might be experienced from the collision of a comet sweeping through the system. Thus far we know that no disturbance has entered, and a knowledge of facts will now pass down to posterity, which will give the means of ascertaining exactly the influence of all disturbing causes which do not form a part of the great system.

The last subordinate group, and the most extraordinary one to which I will at this time direct your attention, is that of Saturn and his rings. Here we find a delicacy of adjustment and equilibrium far exceeding anything yet exhibited in our examinations. This great planet is surrounded certainly by two, probably by three, immense rings, which are formed of solid matter, in all respects like that constituting the central body. These wonderful appendages are nowhere else to be found throughout the entire solar system, at least with certainty. Their existence has elsewhere been suspected, but around Saturn they are seen with a perfection and distinctness which defies all scepticism as to their actual existence. The diameter of the outer ring is no less than 176,000 miles. Its breadth is 21,000 miles, while its thickness does not exceed 100 miles. The inner ring is separated from the outer one by a space of about 1,800 miles, its breadth 34,000 miles, its inner edge being about 20,000 miles from the surface of the planet. Its thickness is the same as that of the outer ring. These extraordinary objects are rotating in the same direction as the planet, and with a velocity so great that objects on the extreme edge of the outer ring are carried through space with the amazing velocity of nearly 50,000 miles an hour, or nearly fifty times more swiftly than the objects on the earth's equator.

What power of adjustment can secure the stability of these stupendous rings? No solid bond fastens them to the planet; isolated in space, they hold their places, and revolving with incredible velocity around an imaginary axis, they accompany their planet in its mighty orbit round the sun. Such is the exceeding delicacy with which this system is adjusted, that, the slightest external cause once deranging the equilibrium, no readjustment would be effected. The rings would be thrown on the body of the planet, and the system would be destroyed.

To understand the extraordinary character of this system we will explain a little more fully the three different kinds of equilibrium. The first is called an equilibrium of instability, and is exemplified in the effort to balance a rod on the tip of the finger. The slightest deviation from the exact vertical increases itself constantly, until the equilibrium is destroyed. In case the same rod be balanced on its centre on the finger, it presents an example of an equilibrium of indifference—that is, if it be swayed slightly to the one side or the other, there is no tendency to restore itself, or to increase its deviation. It remains indifferent to any change. Take the same rod, and suspend it like a pendulum. Now cause it to deviate from the vertical to the right or left, and it returns of itself to the condition of equilibrium. This is an equilibrium of stability. We have already seen that this is the kind of equilibrium which exists in the planetary system. There are constant deviations, but a perpetual effort is making to restore the object to its primitive condition.

Now, in case the rings of Saturn are homogeneous, equally thick, and exactly concentric with the planet, their equilibrium is one of instability. The smallest derangement would find no restorative power, and would even perpetuate and increase itself, until the system is destroyed. For a long time it was believed that the rings were equally thick, and concentric with the planet, but when it was discovered that such features would produce an equilibrium of instability, and that there existed no guarantee for the permanency of this exquisite system, an analytic examination was made, which led to this singular result—viz., to change the equilibrium of instability into one of stability, all that is necessary is to make the ring thicker or denser in some parts than in others, and to cause its centre of position to be without the centre of the planet, and to perform around that centre a revolution in a minute orbit. Finding these conditions analytically, it now became a matter of deep interest to ascertain whether these conditions actually existed in Nature. The occasional disappearance of the ring, in consequence of its edge being presented to the eye of the observer, gave a capital opportunity of determining whether it was of uniform thickness. On these rare occasions, in the most powerful telescopes, the ring remains visible edgewise, and looks like a slender fibre of silver light drawn across the diameter of the planet. In the gradual wasting away of the two extremities of the ring it has been remarked that the one remains visible longer than the other. As the ring is swiftly revolving, neither extremity can, in any sense, be regarded as fixed, and hence sometimes the one, sometimes the other, fades first from the sight. An exactly uniform thickness in the ring would render such a phenomenon impossible, and hence we conclude that the first condition of stability is fulfilled—the rings are not equally thick throughout.

The micrometer was now applied to detect an eccentricity in the central point of the ring. Recent examinations by Struve and Bessel have settled this question in the most satisfactory manner. The centre of the ring does not coincide with that of the planet, and it is actually performing a revolution around the centre of the planet in a minute orbit, thus forming the second delicate condition of equilibrium. The analogy of the great system is unbroken in the subordinate one. For more than two hundred years have these wonderful circles of light whirled in their rapid career under the eye of man, and freed from all external action they are so poised that millions of years shall in nowise affect their beautiful organization. Their graceful figures and beautiful light shall greet the eye of the student when ten thousand years shall have rolled away.

Thus do we find that God has built the heavens in wisdom, to declare his glory, and to show forth his handiwork. There are no iron tracks, with bars and bolts, to hold the planets in their orbits. Freely in space they move, ever changing, but never changed; poised and balancing; swaying and swayed; disturbing and disturbed, onward they fly, fulfilling with unerring certainty their mighty cycles. The entire system forms one grand complicated piece of celestial machinery—circle within circle, wheel within wheel, cycle within cycle—revolution so swift as to be completed in a few hours; movements so slow that their mighty periods are only counted by millions of years. Are we to believe that the Divine Architect constructed this admirably adjusted system to wear out, and to fall in ruins, even before one single revolution of its complex scheme of wheels had been performed? No. I see the mighty orbits of the planets slowly rocking to and fro, their figures expanding and contracting, their axes revolving in their vast periods; but stability is there. Every change shall wear away, and after sweeping through the grand cycle of cycles, the whole system shall return to its primitive condition of perfection and beauty.

THE NEW PLANET, EROS

BY

EDMUND LEDGER

THE NEW PLANET, EROS[1]

ON the 13th of August, 1898, a little planet was discovered at the Urania Observatory of Berlin by Herr G. Witt, to which he has since given the name of Eros. Its discovery has been the great astronomical sensation of the past twelve months, because its orbit passes out of the zone in which all the other small planets move into a very remarkable proximity to the earth—a proximity which will cause Eros to be of the highest value in connection with some of the most important problems of astronomy.

In order to appreciate the method of its discovery, and the reasons which make that discovery so important, it will be well briefly to recall what had been previously achieved in the same line of research. Copernicus had proved and Kepler and others had drawn attention to the greatness of the gap between the orbits of Mars and Jupiter. Then, in 1781, Sir William Herschel detected the planet Uranus at a distance from the sun agreeing with the next term of a series which Titius and Bode had noticed as almost exactly representing the distances of the other planets, except that for one term, between those which corresponded with Mars and Jupiter, there was no planet known.

While twenty-four astronomers were arranging a search for such a missing member of the solar system, Piazzi (who was not one of them) unexpectedly detected at Palermo, on the 1st of January, 1801, a little planet, afterward named Ceres. Three more were found by two out of the twenty-four astronomers in the course of the next six years. A

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[1] From the "Nineteenth Century," April, 1899.

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fifth was found in 1845, after an interval of thirty-eight years; then the progress became rapid. Since 1846 no year has passed without such a discovery. In 1868 the total reached 100; in 1879, 200; in 1890, 300; in 1895, 400; and now it is nearly 450. After a few of the brighter ones had been detected the search for others became a wearisome process. New star charts had to be constructed, with great labour, so as to include the fainter stars. If a small star was noticed with the telescope which could not be found in them, it was carefully watched, and if it exhibited an orbital movement it was entered on the list of planets, and the various elements of its orbit were calculated and recorded.

But when many astronomers were inclined to look upon all this work as well-nigh profitless, and too wearisome to be continued, photography with startling suddenness did away with all need of star charts and of any comparison of observations with them. Upon a photographic plate suitably exposed, Herr Max Wolf, of Heidelberg, found that a little planet had recorded its place on the 22d of December, 1891. There was no need to compare the plate with any chart of stars. The planet had asserted its right to the name of Wanderer by moving on a little way in its orbit among the stars during the exposure of the plate. The stars left their traces in dots (the effect of the rotatory motion of the earth having been duly compensated); the planet left its trail in a little straight line drawn by it in the direction of its motion.

In the next year Max Wolf found thirteen more, and in the same year Charlois ten, by this new method. And since the early part of 1892, out of one hundred discoveries of such planets, only three or four have been made by the old method of eye observation. Once more, however, the great abundance of these photographic discoveries of planet after planet began to make astronomers despair of the possibility of so keeping count of their orbits and positions as to be able to determine whether the little trails, of which several were sometimes found upon the same photographic plate, indicated the presence of planets previously seen or hitherto unknown. It would, indeed, have been quite impossible to do so had it not been for the unremitting industry of German computers, as evidenced year by year in the " Berliner Jahrbuch."

These little bodies were even termed astronomical nuisances. But one of them—the 433d—has at last proved to be a great astronomical treasure. It has proved that it would have been most unwise to have neglected any of these minute portions of our solar system. Some—e. g., Hilda (No. 153), Thule (No. 279), and one which is still unnamed (No. 361)—approach so near to the orbit of Jupiter that they will be of much use in the accurate determination of that great planet's mass. Others are of especial interest in the comparison of their very oval orbits with those of certain periodic comets. But by far the most important are those whose orbits lie nearest to that of the earth. Only three or four, however, such as Æthra (No. 132), Brucia (No. 323), and Ingeborg (No. 391), have hitherto been found which approach the earth, even to a very moderate extent, within the distance of that part of the somewhat oval orbit of Mars in which he is at his farthest from the sun; and they do so only in a small portion of their orbits.

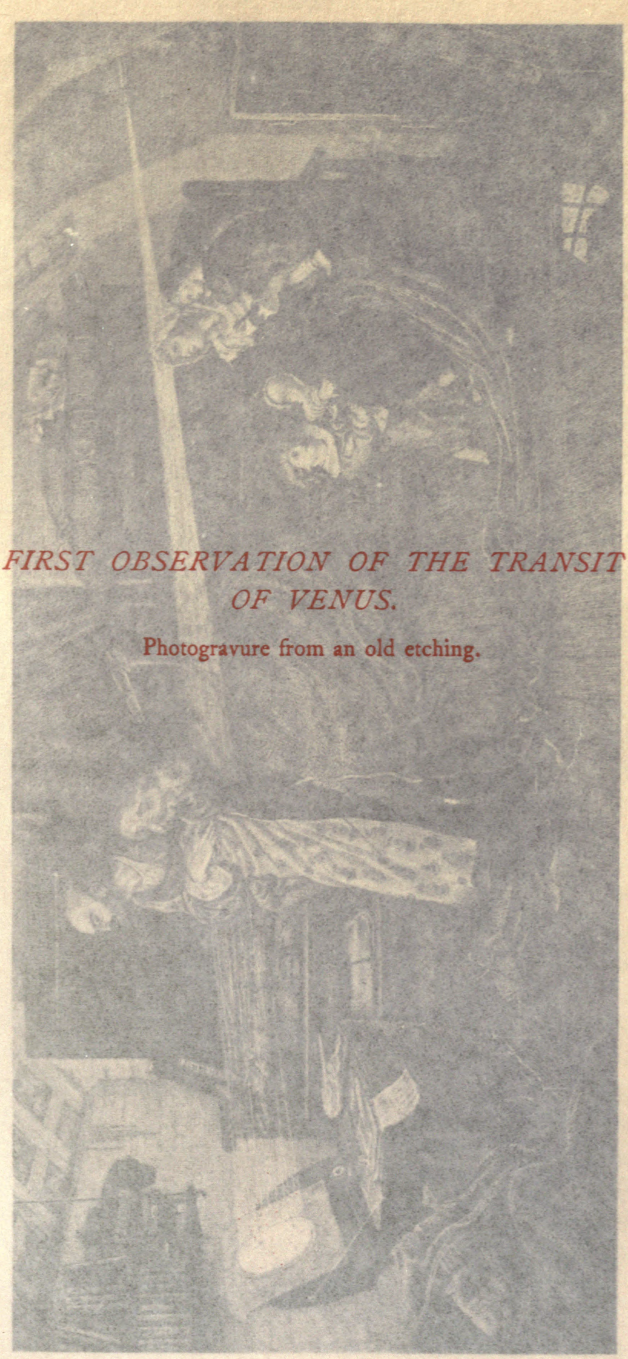
But in the case of Eros we meet with something utterly different and unexpected. A new planet has been discovered whose average distance from the sun is less than that of Mars; a planet which at times comes within a distance from the earth not much more than one third of the nearest distance within which Mars ever approaches it.

On the 13th of August, 1898, Herr Witt exposed a photographic plate with the hope of obtaining upon it the trail of another previously known minor planet. He succeeded, but upon the plate there was also a second, fainter trail—faint and of unusual length because of the rapidity with which the planet had moved. This indicated an unusual orbit. Further observations were at once made. From them Herr Berberich calculated what proved to be a most surprising orbit. The path of Mars up to this time had practically formed the boundary beyond which minor planets had hardly transgressed. This new planet came 45,000,000 miles within the mean distance of Mars. With the exception of the moon it is by far the nearest celestial neighbour of the earth, the nearest approach even of Venus to the earth being not much less than twice as great.

But let us now ask, Why should the near approach of Eros to the earth attach an extraordinary value to our acquaintance with it? Is it because we may hope to see the details of its surface, or to set up some communication between it and the earth? By no means. If we may judge by the amount of light which it reflects to us, we may conclude that its diameter is probably less than twenty miles. The largest telescope, therefore, will barely reveal in it any disk of measurable breadth. On the contrary, the great value of this little Eros depends upon its enabling us to measure the scale upon which the whole universe around us is constructed with an accuracy much surpassing any that has been previously attained.

Our estimate, for instance, of the distance of any star, or of the size of the orbits of any pair of double stars, in fact, all our measurements in the celestial spaces, depend upon our knowledge of the distance of the earth from the sun. To determine that distance a direct trigonometrical method, such as is used in surveying, and such as may be applied to find the distance of the moon from the earth, can not be used, as no instruments can be constructed of the necessary delicacy. But there is a remarkable proportion connected with the movements of the planets in their orbits, discovered by Kepler and more fully investigated by the genius of Newton, which enables us at once to determine the distance of the sun, if only we can measure the distance of any one of the other planets from the earth.

It was at one time hoped that this might be accurately determined in the case of Venus by observations made on those rare occasions when it passes in transit across the sun's disk. But the glare of the sun's light, the ill-defined



FIRST OBSERVATION OF THE TRANSIT OF VENUS.

Photogravure from an old etching.

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edge of the sun's disk, and the atmosphere of Venus itself, combine to deprive such observations of the necessary accuracy. Apart from some other methods, involving long periods of time and highly complicated theoretical investigations in their use, attention was therefore next given to an attempt to obtain the distance of the planet Mars when it makes its nearest approaches to the earth. It was, however, found to be difficult to measure the exact position of the centre of its disk. Whereupon it was suggested that some of the nearer minor planets, although they would be farther from the earth, and their distance from it proportionately more difficult to determine, might more than compensate for this disadvantage by the great accuracy with which the positions of their starlike telescopic images might be observed. And this was found to be the case. The most accurate value of the sun's distance known at the present time is believed to be that which has been skilfully deduced in this way from observations of certain of the nearer minor planets by Dr. Gill, H. M. Astronomer at the Cape of Good Hope.

The new planet, Eros, is of the utmost value for such observations, because the accuracy of the result which they afford is proportionate to the nearness to the earth of the planet that is observed. The method of calculation employed depends upon the ratio of the planet's distance to the distance between two observers simultaneously looking at it from two widely separated points upon the earth's surface, or to the distance through which an observer may himself be moved, by the rotation of the earth upon its axis, between two observations made, the one soon after sunset, and the other shortly before sunrise. The movements of the earth and the planet in their orbits in the interval (as also if in the first case the two observations made are not exactly simultaneous) can be allowed for, and will not affect the final result. An observer may be moved between such an evening and early-morning observation when this latter (termed the diurnal) method is employed, provided he be near to the equator, to a position which may be separated by about 7,000 miles, supposed to be measured in a straight line drawn through the earth, from his previous place. The effect would be the same in altering the apparent direction in which the planet would be seen as if he were looking at it one moment from Jamaica, and then were suddenly transported to see it from Aden.

The difference in the directions in which the planet is seen from two such standpoints, as compared with the positions of the stars around it, which are so distant that no change is produced in their apparent places, is in such cases large enough to be capable of very accurate measurement, and will be so much the larger and more easily measurable the nearer any planet employed is to the earth.

It was a few years ago supposed that the diurnal method would prove to be the most satisfactory possible, because in it the same observer and the same instrument can be employed for all the observations. We are inclined to think that this may ultimately prove to be the case if an observatory suitably equipped, and situated near to the equator, can be employed. Dr. Gill has, however, introduced such improvements into the other method that it has been chiefly used under his superintendence for the last published[2] and most accurate result that has yet been obtained—viz., from observations of the minor planets Victoria, Sappho, and Iris, made in 1888 and 1889 at the Cape of Good Hope, and at Yale, Leipsic, Göttingen, Bamberg, and Oxford (Radcliffe) Observatories. But it is very interesting that it is also found that observations (nearly 3,500 in number) made at the same time by the diurnal method upon the planet Victoria at the Cape, although that observatory is unfavourably situated for the use of this method, in a latitude thirty-four degrees south of the equator, gave almost precisely the same result—a value of very nearly 92,875,000 miles as the distance of the sun from the earth.

This is a very trustworthy value, but it nevertheless involves an uncertainty of about 50,000 miles, or possibly

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[2] "Annals of the Cape Observatory," vol. vi, published in 1897.

somewhat more. It was obtained by the observation of planets selected for their suitability of position and because their orbits were very accurately known, which did not, however, come within a distance equal to six times that of the nearest approach which Eros may make to the earth. There is every reason, therefore, to hope that our future observations of Eros may give us this all-important unit for all our celestial measurings—the distance of the earth from the sun—with an accuracy six times greater than any which has hitherto been secured.

It is very fortunate that Eros, when at its nearest approach to the sun, is also almost in the plane of the earth's orbit. If the earth is at the same time in the corresponding part of its own orbit, the planet's approximation to the earth is consequently in nowise hindered by its being elevated above, or depressed below, the earth's orbit. But the two are only simultaneously in these positions once in about every thirty years, and it is very unfortunate that an exceedingly favourable concurrence of such positions occurred in January, 1894, so that the next occasion will not be until January, 1924. In the latter part, however, of 1900, and in the beginning of 1901, the earth and Eros will come within about 31,000,000 miles, and this, their nearest approach to one another before the year 1924, will enable observations of much importance to be made, which ought to suffice for a decided improvement in the accuracy of our present estimate of the distance of the sun.

A few further statements with reference to this very remarkable planet may be of interest. Its mean distance from the sun is 5,500,000 miles less than that of Mars. That distance ranks, therefore, not with those of all the other little planets as between that of Mars and that of Jupiter, but as between the earth's and that of Mars. Owing, however, to the ovalness of its orbit, it passes in one part of its circuit about 11,000,000 miles beyond the maximum distance of Mars, which is, nevertheless, a comparatively slight excess. Its period of revolution round the sun is 643 days, that of Mars being 687. There is no fear of its ever colliding with Mars, because the two orbits, where they would otherwise intersect, are separated by an interval of about 21,000,000 miles, owing to the difference of their tilts, or inclinations to the ecliptic.

Besides its great usefulness for the purpose already explained, the perturbations of its motion by the earth's attraction will afford another indirect and theoretically very interesting method of determining the distance of the sun, by its enabling a comparison to be made between the masses of the earth and the sun. The perturbations of the motion of Eros by Mars will, in addition, be very valuable to astronomers. Certain recondite effects of its proximity in reference to the moon may also prove to be important. The great alterations in its distance from the earth at different times will afford an excellent test as to whether the light received from it varies exactly as the inverse square of its distance from us, or meets with any hindrance, or absorbing medium, in its passage. The comparison of its light with the phases of its little disk corresponding to its positions relatively to the earth and the sun will also be instructive.

The discovery of Eros has afforded a most important proof of the value of stellar photographs carefully kept and preserved. For the more accurate determination of the elements of its orbit it was very desirable to obtain, if possible, records of its exact position in previous years. Very careful search was therefore made among the many plates preserved at the Harvard College Observatory, in America, in order to see if its faint trace could be found upon some of those which had been exposed in 1896. It seemed almost impossible to detect it. But at last success rewarded a search which proved most trying to the eyes. Mrs. Fleming, well known for her splendid work in connection with stellar spectra in the Harvard Observatory, detected the trail upon a plate dated the 5th of June, 1896. It was soon found upon other plates of that year, as its probable position could not be more precisely calculated; then on others of 1893 and 1894; upon thirteen plates in all. Its orbit is consequently known at the present time with great accuracy.

There is no reason to suppose that Eros is a body recently drawn, by the attraction of the earth, into its present orbit. Its near approach to the earth is by no means near enough for such an event as that to have occurred. It has doubtless escaped previous observation because its light has only exceeded that of an eighth-magnitude star (or, for the purpose of photography, as it seems to be wanting in violet rays, that of a ninth-magnitude star) for about two months in the last eleven years. On the comparatively rare occasions of its very nearest approach it will be barely within the range of visibility to the naked eye.

In conclusion, it may be noticed that the proximity of the orbit of this little neighbour to that of the earth may afford one more argument against the hypothesis put forward by Olbers of the supposed origin of the minor planets by the explosion of a larger planet, a hypothesis which for a while met with much acceptance. When only four minor planets had been discovered, three at least of their orbits tended to support his hypothesis. It has, however, since been discarded by nearly all the highest authorities in astronomy, to the regret, no doubt, of those whom the idea of any celestial catastrophe seems to fascinate, whether it be the possible collision of two suns, the destruction of the earth by a comet, or the blowing of a planet into pieces by its own inherent forces. Even if such an event could have occurred, and have produced the minor planets, it must have been at an exceedingly remote epoch; otherwise, by the laws of mechanics every fragment would have continued, once in each of its subsequent revolutions round the sun, to pass again and again through the point of explosion. Millions of years would have been required to enable the mutual perturbing attractions of the fragments upon each other so to change their orbits as to have effaced all trace of the point where the catastrophe took place. Apart, however, from this, and apart from the fact that it is difficult to conceive how the orbits could have been spread over seven eighths of the vast interval which separates those of Mars and Jupiter, apart also from the great extension of the region in which they move which is involved in the newly found orbit of Eros, another argument against the hypothesis seems to be conclusive.

It appears impossible to conceive of such an amount of explosive energy in any globe as should not only hurl away a number of ejected portions in the directions and with the velocities which would produce such widely differing orbits, but such as should break up and suitably project the whole mass of the globe, leaving no fragment of any importance unprojected. A cannon-ball, as Proctor has remarked in his "Old and New Astronomy," "might be driven by a certain charge of gunpowder to a distance of two or three miles, but a thousand times that charge would not scatter the fragments of the cannon (if the ball had been tightly driven in) over a similar distance all round the place of explosion. Nothing known about our earth's interior, nothing which we can infer about the interior of any other planet formed by processes such as we recognise in the development of the solar system as at present understood, suggests the possibility that a millionth part of the force necessary to shatter a planet, as Olbers's theory requires, can ever be generated or accumulated within the planet's interior."[3]

Rather may we see in a planet such as Eros a portion of the primeval solar nebula unused in the formation either of Mars or of the earth. The minor planets are probably no fragments of a larger planet previously existing, but the fragments that might have helped to form a larger planet had it not been for the influence of the mighty globe of Jupiter. We may see in them one more instance of the effect of that process of tidal action which Professor Darwin has of late so wonderfully applied to show how the matter of the moon may, in bygone time, have been disrupted from the then viscous earth, in the form of a succession of lumps

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[3] "Old and New Astronomy," p. 563.

broken off by centrifugal effect from the summits of great tidal waves—a hypothesis which is found to be of ever-widening application, as, for instance, to the genesis of double stars, and to the temporary outburst of such stars as that which Kepler saw in 1572 in Cassiopeia.

The attraction of the globe of Jupiter, as the solar nebula contracted within his orbit, may well have produced such tides in its mass as, in place of allowing a greater quantity of matter or a nebulous ring to be more quietly detached at some subsequent epoch, so as to form another large globe, may have caused many and many a smaller portion to have been broken off and left behind. These portions we may now see in the hundreds of minor planets which have so far been discovered. After a while, it may be supposed that the influence of Jupiter was so far left behind by the continued contraction of the solar nebula that the formation of larger globes, such as those of Mars and the earth, Venus and Mercury, began again.

However this may be, let us hope that in the succession of celestial photographs now being continuously secured other similar fragments may ere long be revealed whose orbits may be as interesting as that of Eros, whether they may revolve within, or, like it, outside of the orbit of the earth. Let us hope that some of them may approach the earth even more closely than Eros. If so, they will be still more useful rewards of the unwearied industry of observers and computers, and of the skill displayed in astronomical photography.

SIDEREAL ASTRONOMY: OLD AND NEW

PHOTOGRAPHY THE SERVANT OF ASTRONOMY

THE BEGINNINGS OF AMERICAN ASTRONOMY

BY

EDWARD SINGLETON HOLDEN

SIDEREAL ASTRONOMY: OLD AND NEW[1]

I. THE DATA IT HAS COLLECTED

WHEN did astronomy have its beginnings on the earth? There have been many learned attempts to answer this question. They all have led to the conclusion that long before the historic period there was a large common stock of knowledge; so large, in fact, that one distinguished writer finds it simplest to ascribe the origin of astronomy to the teaching of an extinct race: " Ce peuple ancien qui nous á tout appris—excepté son nom et son existence," his commentator adds.

Astronomy is older than the first records of any nation. In order that the records might exist, it was first necessary to divide the years and times by astronomical observations. On the other hand, I believe the travellers of to-day have found no tribe so degraded as to be without some knowledge of the sort.

It is extremely doubtful if animals notice special celestial bodies. Birds seem to be inspired by the approach of day and not by the actual presence of the sun. It is a question whether dogs " bay the moon " or only the moon's light. A friend maintains that her King Charles spaniel watched the progress of an occultation of Venus by the crescent moon with the most vivid interest. This is the only case which I have been able to collect in which the attention of animals has been even supposed to have been held by a celestial phenomenon. The actions of the most ignorant savages during a total solar eclipse, compared with

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[1] From the " Century Magazine," August and September, 1888. By special permission of the Century Company.

those of animals, throw much light on the question of whereabouts in the scale of intelligence the attention begins to be directed to extra-terrestrial occurrences. The savages are appalled by the disappearance of the sun itself, while animals seem to be concerned with the advent of darkness simply.

I am told that the Eskimos of Smith's Sound have names for a score or more of stars, and that their long sledge journeys are safely made by the guidance of these stars alone. I have myself seen a Polynesian islander embark in a canoe, without compass or chart, bound for an island three days' sail distant. His course would need to be so accurately laid that at the end of his three days he should find himself within four or five miles of his haven; if he passed the low coral island at a greater distance it could not be seen from his frail craft. There can be little doubt but that he used the sun by day and the stars by night to hold his course direct.

There must have been centuries during which such knowledge was passed from man to man by word of mouth, woven into tales and learned as a part of the lore of the sailor, the hunter, or the tiller of the soil. No one can say how early this knowledge of the sky was put into the formal shape of maps, globes, or catalogues. Eudoxus is said to have constructed a celestial globe B.C. 366. Globes would naturally precede maps, and maps mere lists or catalogues.

The prototype of all sidereal catalogues is the "Almagest" of Ptolemy (A. D. 150), which includes not only the observations of Ptolemy, but those of the great Hipparchus (B. C. 127). It contains the description of 1,022 stars, their positions, and their brightness. Here we meet for the first time the name magnitude of a star. Ptolemy divides all the stars into magnitudes—degrees of brightness. Sirius, Capella, are of the first magnitude; the faintest stars visible to the eyes are of the sixth. But Ptolemy has gone further, and divides each magnitude into three parts. The moderns divide each class into ten parts—that is, decimally.

SCALE OF MAGNITUDES

In assigning magnitudes in this way, we have unconsciously adopted a scale. A star of the third magnitude is brighter than one of the fourth. How much brighter? Sirius and the brightest stars are about one hundred times more brilliant than the very faintest stars which can be seen with the naked eye. In general a star of any magnitude, as fifth, is four tenths as bright as the star of the next brighter magnitude, as fourth. Ten fifth-magnitude stars taken together are as bright as four fourth-magnitude stars, and so on. This relation between the brightness of stars of consecutive magnitudes gives us a means of computing the total amount of light received from stars. For example, there are ten stars in our sky as bright as the brilliant star Vega, or Alpha Lyræ, which we see in our zenith during the summer months. The collective light of these ten first-magnitude stars is 10 times that of Vega. The 37 second-magnitude stars are together 7.4 times as bright as Vega; the 128 third-magnitude stars are 10.2 times as bright; and so on down to the 4,328 sixth-magnitude stars, which, taken together, are 22.1 times as bright. Taking all the stars visible to us without a telescope and adding their brilliancy, we find that all the naked-eye stars give us a light 67.6 times as bright as that from Vega. Now, the stars of the seventh and eighth magnitudes have been counted; there are 13,593 of the seventh, 57,960 of the eighth, and they too send light to us, although they are individually invisible. All the seventh-magnitude stars taken together give us 27.8 times as much light as Vega, and the eighth give us 47.4 as much; so that we have from both of these classes 75.2 times the light of Vega—that is, more light comes to us from stars so faint as to be individually invisible than from the less numerous and brighter stars that we see with the naked eye. We may recollect that more than half of the light of a starlit night comes from the collective lustre of stars, each of which is totally invisible except in the telescope.

METHODS OF NAMING THE STARS

In Ptolemy's "Almagest," and for fifteen centuries later, there were two, and but two, ways of designating a particular star. A few of the brighter stars had special names.

By far the greater number were described by their situation in their constellation. The brightest star in Taurus was the eye of the Bull, and so for others, as the belt and sword of Orion. This was all very well for the brighter stars, and it did not require that the boundaries of the constellations should be very accurately fixed. There was no mistaking Regulus, Cor Leonis—the heart of the lion. But when we come to the small pairs of stars which make the paws of the Great Bear, or to some of the stars in the windings of Serpens, then it is evident that Ptolemy must have had accurately bounded constellations laid down on charts or globes. Not a single ancient globe or chart has come down to us. The oldest extant are but Arabian copies of the tenth century.

Where, then, do we derive our figures of the constellations? If any one of my readers will ask some astronomical friend to show him a copy of Flamsteed's "Atlas Cœlestis" he will see the beautiful and spirited drawings of the constellation figures, and be charmed and delighted with their vigour and character. Who could have drawn these outlines, instinct with life? Who of the ancients knew the whole character of the timid hare, or who could draw Andromeda, and put a modern resignation in her chained despair? These figures were drawn by a master indeed, for they are from the hand of Albert Dürer himself. If we follow the history of how he came to make them for an edition of Ptolemy, and think of him patiently fitting his marvellously free outlines to match the stars in the sky and the crabbed descriptions in Ptolemy's book, the pleasure does not diminish. About 1603 Bayer introduced the practice of designating the brighter stars of each constellation by the letters of the Greek alphabet, so that Cor Leonis or

Regulus became *a* Leonis; Aldebaran became *a* Tauri, and so on. As the number of the well-determined stars has vastly increased, the practice of referring to them by their numbers in some well-known catalogue has come into vogue; so that *a* Leonis, for example, might be known as Bradley, 1406, from its number in Bradley's catalogue; or as Lalande, 19,755, and so on. It is not to be denied that astronomical nomenclature in this direction could be greatly improved.

URANOMETRIES

The word uranometry has received a limited technical meaning in astronomy. It is used to denote a description of the fixed stars which are visible to the naked eye only. The description of each star places it in its proper constellation, assigns its latitude and longitude, and gives its brightness or magnitude. Variable stars, which change their brightness periodically—and there are many such—are treated separately.

Ptolemy's "Almagest" (1,022 stars) was an incomplete uranometry, since there were more than 3,000 stars visible to him. Al-Sûfi's revision of it, in the tenth century, added no stars, but simply revised the magnitudes given by Ptolemy. Bayer (1603) gave 1,200 stars. None of the very important works of Flamsteed (1753), Harris (1725), Wollaston (1811), Harding (1822), were complete—that is, no one gave every star down to a certain brightness. It was reserved for Argelander (1843) to give in the "Uranometria Nova" the position of brightness of every star visible to the naked eye at Bonn. This was a picture of the sky; changes could no longer occur without detection. This work gave the places of 3,256 stars, from first to sixth magnitudes, and very careful eye-estimates of their magnitudes. Argelander's work has been repeated by Heis (1872). The southern sky has been treated in the same way by Dr. Gould, in the "Uranometria Argentina" (1879), containing 6,694 southern and 991 northern stars, of magnitudes between the first and seventh. Houzeau, during a residence in Jamaica, made a uranometry which embraces every star in both hemispheres, and which has a special value owing to the fact that the estimates of magnitude were all made by a single person, and are therefore consistent.

We have, then, a complete picture of our sky, as seen with the naked eye, based on eye-estimates of the brightness of the stars. It should be said that the magnitudes so determined are extremely accurate, approaching closely to the exactness which can be reached with the best photometers, or instruments for measuring the relative brightness of stars.

THE "HARVARD PHOTOMETRY"

Up to 1877, when Professor Pickering became director of the Harvard University Observatory, there was no single observatory devoted to photometry as a chief end. The important works of this nature had been done as a part of other duties. Professor Pickering turned the whole strength of the observatory in this direction, and by means of new methods and new instruments he and his assistants have just completed a work of the first importance—the "Harvard Photometry." It contains the positions and the measured brightness of 4,260 stars visible at Cambridge, together with a comparison with the magnitudes of all other observers. The actual number of single observations is 95,000. Each one of these consists in a direct photometric comparison of the relative brightness of a star with one of the polar stars. The polar stars are always visible; the stars to be measured were taken as they crossed the meridian; and these direct measures, suitably combined, give the relative brightness of each of the stars of the list. We have now a sure basis for all future work, and a perfect picture of the sky at this time.

THE NUMBER OF THE STARS

The total number of stars one can see will depend very largely upon the clearness of the atmosphere and the keenness of the eye. There are in the whole celestial sphere about 6,000 stars visible to an ordinarily good eye. Of these, however, we can never see more than a fraction at any one time, because a half of the sphere is always below the horizon. If we could see a star in the horizon as easily as in the zenith, a half of the whole number, or 3,000, would be visible on any clear night. But stars near the horizon are seen through so great a thickness of atmosphere as greatly to obscure their light, and only the brightest ones can there be seen. As a result of this obscuration, it is not likely that more than 2,000 stars can ever be taken in at a single view by any ordinary eye. About 2,000 other stars are so near the south pole that they never rise in our latitudes. Hence, out of 6,000 supposed to be visible, only 4,000 ever come within the range of our vision, unless we make a journey toward the equator.

As telescopic power is increased, we still find stars of fainter and fainter light. But the number can not go on increasing forever in the same ratio as with the brighter magnitudes, because, if it did, the whole sky would be a blaze of starlight. If telescopes with powers far exceeding our present ones were made, they would no doubt show new stars of the twentieth and twenty-first, etc., magnitudes. But it is highly probable that the number of such successive orders of stars would not increase in the same ratio as is observed in the eighth, ninth, and tenth magnitudes, for example. The enormous labour of estimating the number of stars of such classes will long prevent the accumulation of statistics on this question; but this much is certain, that in special regions of the sky, which have been searchingly examined by various telescopes of successively increasing apertures, the number of new stars found is by no means in proportion to the increased instrumental power. If this is found to be true elsewhere, the conclusion may be that, after all, the stellar system can be experimentally shown to be of finite extent and to contain only a finite number of stars. In the whole sky an eye of average power will see about 6,000 stars, as I have just said. With a telescope this number is greatly increased, and the most powerful telescopes of modern times will show more than 60,000,000 stars. Of this number not one out of one hundred has ever been catalogued at all.

In Argelander's "Durchmusterung" of the stars of the northern heavens, there are recorded as belonging to the northern hemisphere:

10	stars	between the	1.0	magnitude and the	1.9	magnitude.
37	"	"	2.0	" "	2.9	"
128	"	"	3.0	" "	3.9	"
310	"	"	4.0	" "	4.9	"
1,016	"	"	5.0	" "	5.9	"
4,328	"	"	6.0	" "	6.9	"
13,593	"	"	7.0	" "	7.9	"
57,960	"	"	8.0	" "	8.9	"
237,544	"	"	9.0	" "	9.5	"

In all, 314,926 stars, from the first to the 9½ magnitudes, are contained in the northern sky, or about 600,000 in both hemispheres. All of these can be seen with a three-inch object-glass.

THE CHARTS OF THE BERLIN ACADEMY

In 1824 Bessel wrote to the Academy of Berlin somewhat as follows:

"It is of the highest astronomical interest that every fixed star in the sky should be known, and its position fixed. Completeness in this task is unattainable; but when we once have maps of all the stars down to a certain magnitude, then the object will be attained. The limit I set is at those stars which can just be plainly seen in one of Fraunhofer's excellent comet-seekers [2]—that is, at about the ninth or tenth magnitude."

Bessel then gives briefly the reasons why such a complete list would be valuable, in addition to its importance as a finished picture of the sky so far as it went; and continues:

"For all these reasons I have often expressed my hope that we might have such a complete list, if even over only a portion of the sky; and I think the time of an astronomer,

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[2] A telescope with about three inches aperture, magnifying ten times.

and of an observatory, could not be better spent than in aiding a systematic attempt to carry out this plan. I myself designed the instruments of the Königsberg Observatory for such a purpose, and since 1821 I have observed as many as possible of the stars from 15° north to 15° south of the equator. In all there are 36,000 observations of 32,000 stars. If the stars are equally numerous over the whole sky, there are 125,000 such. I am about to carry on these zones up to 45° from the equator."

With this introduction Bessel unfolds his plan, which was to have 24 astronomers join in an undertaking to make the 24 separate charts required to extend round the whole 24 hours, and in width over the 30° from 15° north to 15° south of the equator. He himself made a small chart as a beginning, "to break the path," and as a model. The Academy welcomed Bessel's plan, and the work began in 1825.

The first two charts were received in 1828, and the work on the others continued slowly. One of these charts has a great history. It had been engraved but not yet distributed, and was lying in the Berlin Observatory for examination. On the evening of September 23, 1846, Le Verrier's letter, giving the place of a new planet, Neptune, was received in Berlin. The planet had never been seen, but its existence had been predicted from the otherwise inexplicable motions of Uranus. The predicted place of the planet fell within the limit of the lately finished chart, which was taken to the telescope. In very truth there was an eighth-magnitude star in the sky which was not on the chart. This star was in motion; it had the planetary light and disk; it was, in fact, Neptune. The proposal of Bessel had borne splendid fruit. Besides this major planet, many of the minor planets (asteroids) were discovered by these maps. Finally, in 1859, thirty-five years after Bessel's letter, this series was finished. But before it was finished a greater undertaking was begun, of which we must give a short account. One thing must be continually kept in sight. Every one of the systematic "Durchmusterungen," as the Germans say—we have no word for them—is the direct outcome of Bessel's original proposition.

ARGELANDER'S "DURCHMUSTERUNG"

Argelander was Bessel's pupil. In the great zones of Königsberg, Bessel had pointed the telescope on the stars as they passed, and Argelander read the verniers which showed their position. Finally, Argelander had an observatory of his own at Bonn, and his two young assistants, Drs. Krueger and Schoenfeld, were all to him that he had been to Bessel. The years 1852 to 1862 were spent in the tremendous task of observing every star plainly visible in such a comet-seeker as we have described, over more than half of the whole heavens. The telescope was pointed and fixed in position. The time of the passage of every star over a wire in the field of view was noted; the part of the wire crossed by the star was also noted, and finally the brightness of the star.

Not counting the time for the computations, the observations alone lasted seven years and one month; 1,797 hours were spent in observing the comet-seeker zones on 625 nights; and 227 other nights were used in part or wholly in revision zones to correct errors of one nature or another, or to solve doubts.

In the comet-seeker zones 850,000 single observations were made, or on the average 473 stars per hour, or 8 per minute. In specially rich parts of the Milky Way more than 16 stars per minute were often observed, and the richest zone had 1,226 stars in the hour, or 20½ per minute—one every 3 seconds. Counting all the observations together, there were no less than 1,065,000, and this million of observations gave the positions and the brightness of 324,198 stars—that is, the position and brightness of every star plainly visible in the telescope used, from the north pole down to 2° south of the equator.

The very enumeration of the observations makes one fatigued. Only the astronomer can know the multifarious nature of the calculations connected with the observations themselves. Millions on millions of figures had to be made, and made correctly; and, finally, every star had to be engraved on charts, and engraved correctly both as to position and magnitude.

How this work could have been finished in ten years one does not see. That Argelander and his two assistants had the courage to persevere in this tremendous task is itself a marvel. But the work is done, is printed, and is in daily use by scores of astronomers. Its value will never be less. It will remain forever as a picture of the sky, available for every purpose.

Mr. Proctor has done a very useful work in representing the results of Argelander's "Durchmusterung" in a single chart. For every star in Argelander's catalogue Mr. Proctor has laid down a dot, correct as to position and magnitude—324,198 dots in all. The resulting map is photographed down so that the individual dots are, in general, hard to distinguish, but the law of aggregation of the stars is all the better brought out. The map is most interesting, not only in relation to the mere positions and brilliancy of the stars, but as showing, better than any other means can, the apparently capricious manner in which the stars are spread over the surface of the sky. Some evidences of law can be made out, and, in the original, the great features of the Milky Way come forth in a most striking manner. It must be remembered that this map contains, besides the stars visible to the naked eye, all those visible in an ordinary three-inch telescope.

SCHOENFELD'S "DURCHMUSTERUNG"

Argelander's original plan was to extend his observations to 23° south of the equator. Professor Schoenfeld, his successor at Bonn, and his aid in the original undertaking, in 1885 completed the plan projected by Bessel in 1824, and so nobly followed at Bonn from 1852 to 1860. From 1876 to 1884 he has catalogued the stars from 2° to 23° south of the equator, and the work is just finished. Soon we shall have this new "Durchmusterung," with its charts, showing the position and brightness of 133,658 southern stars.

It is most desirable that this enumeration should be extended over the whole southern sky. So long ago as 1866 the work was begun in the southern hemisphere, but apparently it was abandoned, though there is reason to believe that the observatory of the Argentine Republic at Cordoba may begin anew. Professor Stone, at Cincinnati, has partly completed the zone between 23° and 31° (south).

A recognition of the enormous advantages which photography would have over ordinary visual methods of charting is now leading several observatories to attempt the cataloguing of stars from photographic negatives.

The difficulties are many, but success seems to be tolerably certain, and the observatories of Harvard University and of Paris have already produced wonderful results in this direction. The observatory of the Cape of Good Hope, also, has seriously begun a southern "Durchmusterung" by photographic methods.

SYSTEMATIC OBSERVATORIES OF THE STARS IN ZONES

These "Durchmusterungen" are most important. They give us an index to the stars of the whole sky. But it is clear that the positions of the separate stars can not be accurate when so many as eight or ten per minute are observed. What the astronomer wants is the accurate position of a star—its latitude and longitude, as it were. We shall see how much pains is necessary to fix the position of a single star with real precision. Scores of observations are needed, and each observation requires at least five minutes to make and an hour to calculate. When we say that many thousand stars have their positions known with this high precision, we shall be giving a feeble idea of the amount of labour devoted to this question.

But it is impossible to fix the position of every one of the 600,000 stars of the "Durchmusterungen" with this last degree of precision, and yet it is important to know very closely the place of each star. The positions of all faint comets, of asteroids, etc., are known by referring them to neighbouring stars. We must know the positions of these stars. These positions are determined by a special kind of observations—zone observations, so called. A telescope is fixed in the meridian so that it can only move north and south. A divided circle is attached to this, the indications of which give the altitude of the stars seen in the field. One observer at the telescope moves it slowly up and down until some star enters the field. The motion is stopped. The transit of the star is observed over spider lines stretched in the field, while a second observer reads the altitude of this star from the divided circle. In this way it is possible to obtain very accurate positions, and by confining the work to a narrow zone the observations are increased as to number, and the subsequent computations are much simplified.

Before the days of the Berlin charts, or of the "Durchmusterungen," Lalande, in Paris (1870), had fixed the places of more than 50,000 stars in this way, and the Abbé Lacaille (1751) had made a special expedition to the Cape of Good Hope to determine the places of 9,766 southern stars. Bessel took up the same research in the years 1821–'33, and his results are given in two magnificent catalogues, which include 62,000 of the most important stars from 15° south to 45° north of the equator. He made 75,011 single observations, employing 868 hours in observing alone—that is, about 84 stars per hour were observed. Argelander read the altitudes of the stars from the circle while Bessel observed their transits. One of Argelander's first works, when he took charge of the observatory at Bonn, was to continue this series of zones from 45° up to 80° north of the equator—that is, to within 10° of the pole. In this region he made 26,424 observations of 22,000 stars, or 83 stars per hour. Not content with this extension of Bessel's zones to the north, Argelander next began a series of southern zones from 15° to 31° south of the equator. This task he also completed, with 23,250 observations of 17,600 stars, or 83 stars per hour.

Bessel and Argelander alone had pushed their zones from 31° south to 80° north of the equator, making nearly 125,000 separate observations and fixing the positions of 101,600 stars. We have no space to speak of the 38,000 observations made at the Naval Observatory in Washington in the years 1846–'49, or of the zones observed by Lieutenant Gilliss, of our navy, in Chile (1850), which covered the region for 25° round the south pole (27,000 stars). It is most unfortunate for the credit of American astronomers, as well as for the good of the science, that these collections are not yet suitably published.

One would think that the 100,000 stars of Bessel and Argelander would have been sufficient for the needs of astronomy. But the German Astronomical Society, at its meeting in Bonn, in 1867, deliberately resolved upon the task of accurately determining the position of every star as bright as the ninth magnitude contained in Argelander's "Durchmusterung."

The veteran Argelander presided at this meeting, and it is interesting to note how serious the undertaking appeared to be to him. No one knew better how gigantic a task it was. The plan was well laid. A set of 539 very-well-determined stars was assumed as fundamental, and the society resolved that the position of the stars to be determined should be referred to these. The sky was cut up into zones five degrees wide, and various observatories undertook to finish one or more of these zones. The Polar Zone (90° to 80° north of the equator) had lately been completed by Carrington, in England, and did not need revision.

The observatories of Kazan (80°–75°), Dorpat (75°–70°), Christiania (70°–65°), Helsingfors (65°–55°), Harvard University (55°–50°), Bonn (50°–40°), Lund (40°–35°), Leyden (35°–30°), Cambridge, England (30°–25°), Berlin (25°–15°), Leipsic (15°–5°), Albany (5°–1°), Nikolaief (1°–2° south), joined in the work, and to-day it is nearly completed.

But this is only a beginning. Schoenfeld's "Durchmusterung" to 23° south will soon be printed, and it is the intention of the German Astronomical Society to push the zones to this point, to join on to the great series of southern zones printed by our countryman, Dr. B. A. Gould, at the National Observatory of the Argentine Republic. Dr. Gould is himself a pupil of Argelander, and his magnificent work may be fairly called an outcome of the spirit of Bessel, the master; 105,000 observations of some 73,000 stars, from 23° south to 65° south of the equator, have been printed by Dr. Gould as part of the results of fourteen years' labour in a foreign country. Thus from the north to the south poles the labours of Carrington, Argelander, Bessel, Gould, and Gilliss[3] have given us an almost complete catalogue of accurate positions of nearly all the principal stars. Besides this we shall shortly have the region from 80° north to 2° south completely reobserved, and by 1900 the region to 23° south will be done also.

SPECIAL CATALOGUES OF STARS

Besides these gigantic undertakings there have been scores of separate catalogues pretending to greater precision even, the very names of which we can not mention. The observatories of Greenwich, Oxford, Edinburgh, Paris, Pultowa, Dorpat, Bonn, Berlin, Palermo, Washington, Harvard University, Melbourne, Cape of Good Hope, and many others have issued such accurate collections.

It is also necessary to say that a certain small number of stars—several thousands—have had their positions and motions determined with extreme precision; and of these again, a few hundreds of the brightest stars have been observed for so long, and so many times, that their resulting positions are now almost as accurate as they can be made, and their motions so well known as to admit of very little improvement by the work of the next generation. These are our fundamental stars, so called.

Such, then, are our data: a few hundred stars determined with the last degree of precision, a few thousand nearly as well, 200,000 with considerable accuracy, and

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[3] Two Germans, one Englishman, two Americans.

nearly half a million separate stars known by the approximate positions of the "Durchmusterungen," or additional to these from the southern zones. We can add to these too the 200,000 or more stars laid down in the ecliptic charts of Paris, Vienna, and Clinton (New York), which serve as nets to catch the minor planets just now, but which have an incalculable value as accurate pictures of the sky at a given instant.

The brightness of some 10,000 stars is very accurately known, and that of nearly half a million has been very approximately fixed. Lastly, the distances of some fifteen of the brighter stars from the earth are known with tolerable certainty, and that of a few more with a good degree of approximation.

These are the materials available—mighty monuments to human ingenuity, skill, patience, devotion. But what further problems will they solve for us? What far-reaching conclusions can be drawn? In a succeeding article I shall try to show to what results a combination of the data so painfully accumulated may lead, and what conclusions may safely be drawn even now.

The science of the positions and the motions of the stars is not so young as that other science so well described by Professor Langley in his admirable articles on "The New Astronomy" ("The Century" for September, October, December, 1884, and March, 1885), but it has its modern period as well as the historical one which has been here set forth. The old astronomy has set itself to solve such problems as these: What is the rate at which the whole solar system is moving on through space? What are the distances and what are the masses of the stars? What is the shape of the stellar cluster to which our sun belongs? Are the stars in general broken up into subordinate universes? or do they, as a whole, form one mighty system, with one common motion?

Some of these and other such questions are answered; some seem almost unanswerable; some are still in the way of solution.

II. THE RESULTS THAT IT HAS ATTAINED

In the preceding chapter we collected the data which the ancient and the modern astronomy has placed at our disposition. We saw that a few hundred of the stars have their positions fixed with the last degree of precision; a few thousand are known nearly as well; half a million have their places approximately known, and half of these last are tolerably well determined. The brightness of some ten thousand stars is well known, while the brightness of nearly half a million is known with fair approximation. The distances of a few stars (about fifteen) are known with precision; the distances of a few more are approximately known.

These are the data which have been amassed by the observing astronomers of the modern period, beginning with Bradley (1750). In the present paper we are to see some of the general conclusions which may be drawn from these data. What are the distances, what are the dimensions, of the stars? What is the orbit in which our sun, with its group of planets, is travelling? What stars are our nearest neighbours and travelling with us? Are stars in general aggregated into systems of comparatively small size, or are the stars as a whole collected into one vast system, bound together by a common bond, and endowed with a common motion?

The stellar universe, as we see it at any moment, is quite complete. Change does not seem to belong to the region of fixed stars. Yet every one of the millions of observations has been made to fix a position so accurately that the slow changes which must be going on may not escape us; so that the laws of these changes can be formulated. If we know that a star retains its position invariably, if we know positively that its brightness and colour remain the same, it becomes for these very reasons a most useful standard of reference, but it does not, as yet, help us to solve the problem of the stellar universe. We must seek a clew elsewhere, among the stars where changes are manifest, so that the unknown laws of these changes may be unfolded.

PROPER MOTIONS OF STARS

As we said, nothing appears to be more invariable or unalterable than the region of the fixed stars, and, in a general sense, nothing is more so. But when we come to a closer view all is change there as well as elsewhere.

Since Rome was built the apparent situation of Sirius has changed more than a diameter of the moon, Arcturus has moved more than three such angular diameters, and so with other stars.

If gravitation is truly universal, if all the stars are bound together in one system by this law, as we believe, then no star can move without affecting every other. As one moves all must move. The real motion of any star is along some line or curve; we see this real motion projected on the ground of the heavens as an apparent change of its latitude and longitude. Knowing the latitude and longitude of the star now, by observation, we may compare these with the positions of twenty, fifty, or a hundred years ago. It is possible to allow by calculation for every one of the complex changes produced in the apparent position of a star by every cause not in the star itself. Each one of the several observations, when reduced to a common epoch, should give the same position, except for the small and unavoidable errors of observation and the proper motion of the stars.

For example, here are the observations made by Dr. Gould in the last twelve years on a southern star, all reduced to what they would have been if all had been made on January 1, 1875:

Year of Observation.	Right ascension.	South declination.
1873	$23^h\ 58^m\ 0^s.92$.	$37^\circ\ 58'\ 13''.9$
1876	2 .19.	20 .9
1881	4 .63.	34 .1
1885	6 .60.	42 .0

These do not agree. They ought not to differ by more than $0^s.20$ or 3″[4] if the star were at rest. If we assume that the star is moving in right ascension by $0^s.482$ and in declination by 2″.45 yearly, and apply these numbers, the positions will harmonize.

1873 is two years before 1875, and we add twice $0^s.482$ and twice 2″.45; and subtract for the other intervals. The observations thus corrected give

For 1873..........	$23^h\ 58^m\ 1^s.88$..........	37° 58′ 18″.8
1876..........	1 .71..........	18 .4
1881..........	1 .74..........	19 .4
1885..........	1 .78..........	17 .5

and are harmonious within the errors of observation. If we assume that this star is as near to the earth as the very nearest of all the stars, it is certainly moving no less than 600,000,000 miles per year. Yet it will require more than 3,000 years for it to move from its present place by so much as one diameter of the moon.

The calculation that has been outlined here for one star has been performed for several thousands of the better-known stars, especially for the 3,222 stars which were most carefully determined by Bradley in 1750. For each one of these the proper motion has been determined with the greatest nicety. The results at first sight are interesting only in a very special way. No. 1, for example, may be moving 21″ in a century along a path inclined by 10° to the equator. No. 2 moves 44″ in a century along another path inclined by another angle, and so on to No. 3,222. Here seem to be three thousand isolated facts, each one useful in its narrow relations, but each having no connection with any other.

Let us suppose for a moment that the sun, with the solar system, and the earth, our point of view, are moving onward in space, and imagine how such a motion would affect the appearance of a universe of stars scattered all about us. If the sun alone has a motion, all the stars to-

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[4] Errors of observation of this magnitude may exist.

ward which we are moving will appear to be retreating en masse from the point in the sky toward which our course is directed. The nearer stars will move most rapidly; those more distant, less so.

In the same way the stars from which we are retreating will appear to crowd together and approach each other. It is as if one were riding on the rear of a railroad train and watching the rails over which one had just passed. As one recedes from any point the rails at that point seem to come nearer and nearer together. If we were passing through a forest we should see the trunks of the trees from which we were going apparently moving nearer and nearer to each other, while those at the sides would retain their distance apart and those in front would be moving wider and wider apart.

Here is a case in which we are sensible of our own motion and observe the effects of that motion in the positions of the fixed objects about us. We may turn the question about, and inquire whether the observed motions of the stars indicate any real motion of our own.

The outline of the problem is here much as it presented itself to Sir William Herschel in 1782. The details are extremely complicated. It is certain that we are not passing along through space among a vast number of fixed stars. Each star has a motion peculiar to itself. It also is moving along a vast orbit, and this real motion of the star is evident to our instruments. Combined with the veritable motion of the star itself is the parallactic motion produced by the shifting of our own point of view as the earth sweeps forward through space.

It is for analysis to separate the effects of these two motions and to determine what is the real direction and the real amount of the solar motion. The processes of the analysis can not be given here, but fortunately it is easy to exhibit both the data and the results graphically. This has been well done by M. Flammarion in his edition of Dien's Star-Atlas.

The circle marked "Northern Hemisphere" gives the positions of those northern stars which are known to have a proper motion. The size of the dot representing each star gives the magnitude (i. e., brilliancy) of the star. The arrows attached to the star represent the directions in which the stars move on the surface of the sky by their proper motions. The lengths of the arrows represent the velocities with which the stars move. At the time of making the map the stars are in the positions marked by the dots. At the end of 50,000 years they will be at the ends of their respective arrows.

Thus the data are all presented graphically. Notice what variety there is. Notice, too, the striking fact that some of the largest proper motions belong to some of the smallest stars. One would think that the brighter stars would be the nearer, and therefore that on the average they would have the larger proper motions. For evidence on this point I have compiled the paragraph which follows from Argelander's list of the 250 stars with the best-known proper motions. I have chosen the fainter magnitude classes in order to get a sufficient number of stars:

"Seventy-seven stars between sixth and seventh magnitudes have a proper motion of 0".54 yearly; 80 stars between seventh and eighth magnitudes have a proper motion of 0".56 yearly; 58 stars between eighth and ninth magnitudes have a proper motion of 0".71 yearly."

That is, the proper motions do not seem to diminish as the numerical magnitude diminishes.

But to return to the plate. In the constellation Hercules, not far from the bright star Vega, which is near our zenith in the summer sky, is the point toward which the sun is moving. In the corresponding position on the map of the southern hemisphere is a similar point; it is the point from which we come. All over the map are arrows not attached to any stars. These show the direction and the velocity of that part of the proper motion due to the motion of the solar system alone. In general the arrows belonging to the stars should agree in length and in direction with these unattached arrows—and in general they do, for the latter were derived from computations based on the former. But there are many exceptional cases; and, at first glance, it is the exceptions which seem to be the rule.

There is no space to refer to special cases except in passing; but we should note a pair of stars marked 21,258 (of Lalande's catalogue) and 1,830 (of Groombridge's catalogue). They were about 15° apart in 1880. In 50,000 years they will be more than 200 diameters of the moon apart, while now they are not more than 30 such angular diameters. Proper motion alone will in time change the whole aspect of the sky.

So much for the map. Mathematical analysis gives the same results in numbers. It declares that the apex of solar motion is in right ascension 260° and in declination 36° north, which defines the point in Flammarion's map marked by the figure like the sun; and analysis further declares that the amount of the solar motion in 100 years, if viewed from a point at the average distance of the 3,222 Bradley stars, would be 5°.05.

If we know this average distance in miles, we can assign our own velocity in miles. With our best present knowledge, it follows that the sun, the earth, and the whole solar system are moving through space at the rate of

586,000,000 miles per year.
1,600,000 " " day.
67,000 " " hour.
18½ " " second.

The earth moves about the sun in its own orbit at about the same rate of 19 miles per second, while sun, earth, and orbit move along in space another 19 miles.

We can now go back to the stars themselves, and subtract from the observed proper motion of each star that portion (motus parallacticus) which is due to the motion of the solar system, and leave that portion which is due to the star's own motion (motus peculiaris).

Is there anything common to the truly proper motions of the stars? In the first place, it may be said that, so far as we know up to this time, these motions are, in general, not curved. They are practically straight lines. They have no common centre. There is no great central body around which revolve the suns of all other systems. If there be such a body it will be many centuries before we shall know it; and we may certainly say that, so far as our knowledge goes, there is none.

SYSTEMATIC MOTIONS OF THE FIXED STARS PARALLEL TO THE MILKY WAY

But if we are obliged to consider the motions of all the stars to be practically in right lines, and not in closed orbits, there is no reason why we should not examine the question of whether the stars as a whole do not have some systematic motion—whether there is not among this variety some unity. The most natural hypothesis to start with is that the stars have a vast rotation in planes parallel to the Milky Way. We already have good data for examining this, and in a few years, when the zones of the "Astronomische Gesellschaft" are complete, much material will be added. Without some assumption of the sort, that the stars rotate in planes parallel to the Milky Way, it is hardly possible to explain the existence of the Milky Way itself. It would necessarily disintegrate more and more with the lapse of time, and it would be a pure accident that we happen to live at an epoch when this disintegration has not been accomplished. The investigation of this possible rotation has been carried out by two pupils of Professor Gyldén and of Professor Schoenfeld respectively. While the result in one case is fairly against the hypothesis of such a rotation, in the other it is somewhat in its favour. The doubt in the matter arises solely from the deficiency of the data, and this will soon be supplied. In the mean time it should be an answer to those objectors who ask what is the use of another new catalogue of stars, that this catalogue, and every other catalogue, goes a certain way toward providing the means for solving the very greatest problem that can be presented to the human mind by natural objects.

Look at the Milky Way stretching across the summer sky with the bright star Vega burning near it. Think that the few proper motions laboriously determined by Halley and Maskelyne enabled Herschel to announce that the sun, the earth, and every planet is moving toward a spot—near Vega—which he could point out. Think, too, that the smallest efforts of every faithful observer, the world over, go to the solution of the question, How do all these thousands of stars that I see move in space? Are they bound up with that Milky Way in one fate? Or is that permanent shining track, which seems unchanged since Job and the patriarchs looked upon it—is that doomed to destruction? The finger of analysis can point out the fate of those myriads of shining stars, and man becomes fit to live under their influence when his mind adds the beauty of law to the wayward beauty of their shining.

SPECTROSCOPIC PROPER MOTIONS—MOTIONS IN THE LINE OF SIGHT

The observation of a star's position is really nothing but the determination of the place where the line joining eye and star pierces the celestial sphere. The determination of its proper motion is nothing but the determination of the rate at which its apparent position changes. If a star is moving directly toward us, or directly away from us, its apparent place in the sky will remain unchanged. But we have in the spectroscope a means of measuring the motion of a star in the line of sight. The principle of the method is simple. The application of it is most difficult. Every one has noticed, in travelling upon an express train, the sudden clang of the bell of a train passing in the contrary direction; and how the note, the pitch, of the sound of this bell rapidly changes from high back to low again. Nothing is more certain than that the bell has but one essential pitch. Why, then, does it change? The engineer of the passing train hears his own bell giving always the same note, and this note is determined by the length of the sound waves that reach his ear. Suppose them to come at the rate of about 500 per second to him. He is always moving at the same rate as his bell. But to us in the other train the case is different. When the bell is just opposite us, 500 waves come to us per second; when we are approaching the passing train more than 500 come to us (not only the 500 sent out by the Bell, but those others which we meet by our velocity); as we leave the passing train less than 500 waves overtake us per second. Hence the pitch (the number of waves per second) varies. The same thing happens in the case of light. In the spectrum of a star there are certain dark lines the presence of which is due to hydrogen in the star's atmosphere. If the star is at rest with respect to us, these lines are not displaced in its spectrum; a definite number of waves per second (say *A*) come to us from the spectrum on both sides of these lines. If the star is approaching us, more waves than *A* reach us; if the star is receding, fewer waves reach us. The pitch of the line, so to say, is altered; and the spectroscope can measure this change of pitch.

When this is done with respect to the principal stars the most interesting results follow.

Vega (*α* Lyræ) is found to be approaching us at the rate of 10 miles per second. Castor is approaching us at 18 miles, Arcturus at 5 miles, etc.; while Pollux is receding from us 1 mile per second, Aldebaran is receding 30 miles, and so on. After years the aspect of our sky will change. We shall have new glories in the galaxy, and after thousands of years these again will leave us. There is ceaseless change here as everywhere.[5]

PARALLAXES OF THE STARS

The ancients placed all the fixed stars on the inner surface of a vast sphere which turned about the earth's centre

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[5] Later observations, particularly those at Potsdam and at the Lick Observatory, have given more accurate velocities than those originally printed, and the later values have been inserted here.

once each day. They had absolutely no way of even guessing how far off this sphere might be. In 1618 Kepler's guess was 4,000,000 times as far as the sun; in 1698 Huygens placed Sirius 28,000 times as far as the sun; in 1841 Picard showed that the errors of observation with the instruments of his time were as great as the parallaxes of the stars themselves, and that therefore the problem was indeterminate to him; in 1806 Delambre concluded that the same thing remained true, notwithstanding the improvements of the instruments in the meanwhile. It was not till 1836 that W. Struve and Bessel really determined the parallax, and hence the distance, of two different stars *α* Lyræ and 61 Cygni.

It is familiar to all that the distances of even the nearest stars are not to be conceived when they are expressed in miles or familiar units. No star is so near to us as 200,000 times 93,000,000 miles. We have to express these distances in terms of the time required for light to pass from star to earth. For 61 Cygni that time is 2,377 days, or 6½ years. It was the elder Herschel who put these immense distances before us in the true light, by showing that if to-day the star were blotted out of existence its mild light would shine on for years, until the last ray that left it had finally ended its long journey and reached the earth, more than six years afterward.

But all stars are not equally distant. The light from one star may be 10, from another 100, from another 1,000 years old when it reaches us. We must no longer regard the study of the stars as a study of their contemporaneous existence. It is rather the ancient history of the universe which is exhibited to us by the vault of heaven. Assiduous observers have determined the parallaxes of about a score of stars. The first stars to be examined were either the brightest (as in the case of Vega), or those of large proper motion (as 61 Cygni). In general, the brightest stars should be the nearest, one would think, and yet the very largest parallaxes belong to the fainter stars. Similarly the star with the greatest proper motion has a very small parallax.

By treating all the certain data in various ways, Professor Gyldén has come to the conclusion that the average parallax of a star of the first magnitude is about 0".084, or that the average distance of our brightest star is 160,000,000,000,000,000 miles. But to make further steps in the problem of the "construction of the heavens," we must know more than the average parallax of the brightest stars. We must be able to assign the average parallax of stars of each order of magnitude, and this in both hemispheres.

This task is now undertaken for stars down to the fourth magnitude by two observers who have already distinguished themselves in this field—Dr. Gill, Royal Astronomer at the Cape of Good Hope, and Dr. Elkin, now at Yale University Observatory. These gentlemen have devoted their energies to this one problem, which will require perhaps ten years for its solution in the form that they have chosen for it. Dr. Ball, Royal Astronomer for Ireland, is systematically searching for stars of large parallax and incidentally proving many stars to have small parallax—a fact which it is just as important to know as its converse.

The next dozen years will show immense strides in our knowledge of the stellar distances of individual stars, and it may well be that some general relation between distance, brightness, and proper motion of situation in the sky will result from the great increase of data.

DISTANCES OF STARS OF EACH MAGNITUDE

The golden time for astronomers will come when the parallaxes of enough stars have been determined for them to be able to say that the distance of an average third, fourth, sixth, or tenth magnitude star is so many, or so many, times the sun's distance. That time has not yet come, nor will it have come even when the great work begun by Messrs. Gill and Elkin has been ended. There is no certain way of assigning the stellar distances but by measurements such as they are making. But it is a fair procedure to make certain assumptions as to stellar distances, to work out the logical consequences of these assumptions, and to compare these consequences with known facts. An agreement with the facts will, in some degree, support the assumptions. If we assume the stars to be of equal brilliancy one with another, we have one basis of computation. If we suppose them, further, to be equally distributed in space on the average, we have another basis. These conditions lead at once to the following table:

Magnitudes.	Relative distances.
1	1.00
2	1.54
3	2.36
4	3.64
5	5.59
6	8.61
7	13.23
8	20.35

We can test these assumptions to some extent. If they are true, then the ratio of the actual number of stars of any brightness to the actual number of stars of the next lower grade of brightness, raised to the two-thirds power, should be 0.400. Using the stars of the sixth and seventh magnitudes, this number results 0.426; of the seventh and eighth, it results 0.4003, etc. The two hypotheses are in the main not far from correct, and therefore the relative distances above given are not very far wrong for stars down to the eighth magnitude. There is strong reason to believe that the fainter stars, from eleventh to fifteenth magnitudes, do not follow the same law. We have seen that the average distance of a first-magnitude star is 160,000,000,000,000,000 miles. Multiply this by 20.35 and you have the best estimate now available of the distance of an eighth-magnitude star. It is inconceivable, but no more so than the first number. Light would require 600 years and more to reach us from such stars.

DISTRIBUTION OF THE STARS OVER THE SURFACE OF THE CELESTIAL SPHERE

The real question to be solved is, How are the stars distributed throughout solid space itself? To solve this question completely the distance of every star from the earth must be measured (which is a simple impossibility), or else we must find some law which connects the brightness, or the proper motion, or the position of a star with its distance. Suppose that ten stars of each magnitude from the brightest down to the faintest are selected—say 150 or 160 in all—and that the parallax of each individual star is determined. This would be a tremendous labour in itself, and would require the work of several observers for a score of years. But suppose this work done. Suppose that the average distances of the ten stars of each group resulting from the measures were I, II, III, IV, V — — — — — — — XIII, XIV, XV, XVI. Would any general relation exist between the magnitudes 1 — — — — 16 and the corresponding distances I — — — — XVI? From the measures that we already possess this is by no means sure. In fact, the evidence seems to be directly opposed to this conclusion. The average measured parallax of five first-magnitude stars is about 0".27; of three fourth-magnitude stars about 0".13; of three fifth-magnitude stars about 0".31; of seven sixth-magnitude stars about 0".21. That is, the parallax does not seem materially to decrease as the brilliancy diminishes from the first to the sixth magnitude. If, instead of comparing the magnitudes with the distances, we compare the proper motions, there seems to be no evident agreement. The stars with the largest proper motions do not in general have the largest parallaxes (and hence the smallest distances). We have not enough determinations of parallax to decide whether the region of the sky in which a star is situated has any relation to its distance; so that for the present we are not sure that a series of measures even so extensive as the one we have imagined would solve the question of the relation between magnitude, or proper motion, and parallax. Such a series would go a great way toward deciding whether the question was solvable or not. It would add enormously to the very small number of certain facts bearing on the subject of the constitution of the stellar system. And it is to the great credit of this generation of astronomers that such a series has actually been begun (for stars of from first to fourth magnitudes) by Messrs. Gill and Elkin at the Cape of Good Hope and New Haven respectively, as has been mentioned already.

In the absence of definite knowledge with regard to the distribution of the stars in space, much labour has been expended on the study of what we may call stellar statistics—the statistics of the distribution of the stars on the surface of the celestial vault. This distribution of the stars is known when once we have a map of their positions, which it is comparatively easy to make. A more rapid method of studying this distribution may be employed—that of star gauging, so called by Herschel, its inventor. This consists essentially in counting the number of stars visible in the field of the telescope as it is directed to various known portions of the sky. The mere number of stars visible at each pointing may be laid down on a map, like the soundings on a hydrographic chart. The data are easily gathered. How are they to be interpreted? We may briefly indicate one obvious method. Suppose that we have made such star gauges with telescopes of five different powers over the same areas in the sky. The largest telescope will show all the stars, say, down to and including the fifteenth magnitude; the next smaller those to the fourteenth; the next to the thirteenth, the twelfth, the eleventh (the actual distribution of the individual stars from first to tenth magnitudes is known by the " Durchmusterungen "). In any area the difference between all the " Durchmusterung " stars (from one to tenth magnitude) and the number seen in telescope I (the smallest of the five supposed) will give the number of the eleventh-magnitude stars in that region.

The difference between the counts by telescope I (which shows all stars down to and including the eleventh magnitude) and telescope II (which shows all to twelfth magnitude) will give the actual number of twelfth-magnitude stars. Combining the results of the telescopes II and III we should have the number of thirteenth-magnitude stars for this region, and so on for the fourteenth and fifteenth magnitudes. Thus the actual number of the stars of each magnitude in this area (and similarly for other areas) will be known. We may interpret these figures somewhat in this way: Take a map which shall have spaces on it for the whole sky, and devote this map to exhibiting the results of our gauges for the fifteenth-magnitude stars. Wherever there are 100 of these to the square degree lay on one tint of colour; wherever there are 200, two tints; 300, three tints, and so on. The final map will exhibit to the eye the results of our gauges for the fifteenth-magnitude stars. Where the tint is deep, there are more stars; where it is light, fewer. Another such map must be made for the fourteenth-magnitude stars; another for the thirteenth, and so on. Now place these fifteen maps side by side before you, and it will be possible to obtain at once a number of definite conclusions. Here the stars that we call fifteenth and those that we call fourteenth are really connected together in space. Why? Because this long ray of many fifteenth-magnitude stars on one map is matched by this other long ray of just the same position and shape of the fourteenth-magnitude stars. The thirteenth, too, we will say, is similar. But the ninth, tenth, eleventh, and twelfth do not in their distribution at all resemble the fainter stars in this region, but they do resemble each other. In this way, passing from region to region, the general peculiarities of each region may be made out, and much light may be thrown on the vital question, How many magnitudes of stars exist at the same distance from us? Are the stars of the so-called ninth, tenth, and eleventh magnitudes all really at the same distance from us, and are their differences in brightness simply due to differences in size, or are they really at different distances?

A large amount of evidence upon these fundamental points already exists, and more is being accumulated, and it appears possible that a skilful use of it may throw much light on the real question. The new photographic processes will be of immense importance for this investigation. We have not the space to go further into this method of research, but we may just refer in passing to one interesting form of it. We have already elaborate maps of certain portions of the sky showing the position and magnitude of every star down to the thirteenth. These are the maps used for the discovery of asteroids. From each of these maps we can make thirteen others, each one of which shall show the stars of one magnitude only. Now compare these thirteen derived maps, and see what the evidence is that the stars of any two magnitudes are connected or independent. This method is capable of bringing out most interesting conclusions when it is thoroughly carried out, as it has not yet been to any large degree. The local arrangements of stars can be adequately studied in this way, and it is not too much to expect that the typical forms of stellar systems—distorted by perspective, of course—may be exhibited here.

MASSES OF BINARY AND OTHER STARS

The binary systems are those composed of two stars which are connected with each other by a mutual gravitation. They revolve about a common centre of gravity in orbits which can be calculated. In some few cases the parallax of these stars is known; and in every such case the sum of the masses of the two stars becomes known in terms of the mass of our own sun. It is especially noteworthy that in every known case the mass of the binary system is not very different from the mass of our own sun. That is to say, all the stars whose masses are known at all are such bodies as our sun is: they shine with light like his; they are of the same order of magnitude mass.

The term " hypothetical parallax " is applied to a parallax computed for a binary star on the supposition that the mass of the binary, although unknown, may be hypothetically assumed to be the same as the sun's mass. So far as we can judge, these hypothetical parallaxes must be provisionally accepted as essentially correct.

If we can assume that the intrinsic brilliancy of the fixed stars is the same for each star, which does not seem to be a very violent supposition, several interesting conclusions follow which can only be stated here.

If it be true that for the stars, taken one with another, a square mile of surface shines with an equal light for each star, then among stars of known distances some must be at least 270 times as great in diameter as others. This is about the proportion of the sun to Mercury. Also it follows that binary stars whose colours are alike must be composed of stars of like size; and also, that on the average the brightest star of any cluster is about four times as large as the smallest star of the cluster. No star is more than 200,000 times farther than the nearest fixed star. Other assumptions which might serve as a basis for computation will give other results; but for the present we have to content ourselves with some such assumption, and in the infinite variety of circumstances choose that one as general which seems to be the most likely a priori, and which leads to results which agree with actual observation.

THE CLUSTER OF STARS TO WHICH OUR SUN BELONGS

The "Uranometria Nova" of Argelander gave the positions of the lucid stars of the northern sky, and it has been supplemented by the "Uranometria Argentina" of Dr. Gould, which covers the southern sky. With the stellar statistics of the whole sky before him Dr. Gould was in a position to draw some extremely interesting conclusions with respect to the arrangement of the brighter stars in space, and to the situation of our solar system in relation to them. The outline of his reasoning can be given here, but the numerical evidence upon which his conclusions are founded must be omitted. In the first place, it is assumed that in general the stars that are visible to the naked eye (the lucid stars) are distributed at approximately equal distances one from another, and that on the average they are of approximately equal brilliancy. If we make a table of the number of stars of each separate magnitude in the whole sky we shall find that there are proportionately many more of the brighter ones (from first to fourth magnitudes) than of the fainter (from fourth to seventh magnitudes)—that is, there is an " unfailing and systematic excess of the observed number of the brighter stars." We can not suppose, taking one star with another, that the difference between their apparent brightness arises simply from real difference in size, but we must conclude that the stars from the first to fourth magnitudes (some 500) are really nearer to us than the fainter stars. It therefore follows that these brighter stars form a system whose separation from that of those of the fainter stars is marked by the change of relative numerical frequency.

What, then, is the shape of this system? and have we any independent proof of its existence? Sir John Herschel and Dr. Gould have pointed out that there is in the sky a belt of brighter stars which is very nearly a great circle of the sphere. This belt is plainly marked, and it is inclined about 80° to the Milky Way, which it crosses near Cassiopea and the Southern Cross. Taking all the stars down to 4.0 magnitude, Dr. Gould shows that they are more symmetrically arranged with reference to this belt than they are with reference to the Milky Way. In fact, the belt has 264 stars on one side of it and 263 on the other, while the corresponding numbers for the Milky Way are 245 and 282. From this and other reasons it is concluded that this belt contains brighter stars because it contains the nearest stars, and that this set of nearer and brighter stars is distinctively the cluster to which our sun belongs. Leaving out the brighter stars which may be accidentally projected among the true stars belonging to this cluster, Dr. Gould concludes that our sun belongs to a cluster of about 400 stars; that it lies in the principal plane of the cluster (since the belt of bright stars is a great, not a small circle); and that this solar cluster is independent of the vast congeries of stars which we call the Milky Way.

We know that the sun is moving in space. It becomes a question whether this motion is one common to the solar cluster and to the sun, or only the motion of the sun in the solar cluster. The motion has been determined on the supposition that the sun is moving and that its motion is not systematically shared by the stars which Dr. Gould assigns to the solar cluster. But a very important research will be to investigate the solar motion without employing these 400 stars as data.

In what has gone before I have tried to exhibit some of the main questions in purely sidereal astronomy; to show some of the more important results already reached, and especially to indicate the directions along which present researches are tending. It is impossible to give a complete view in this or in any other single branch of astronomy, for they are all indissolubly bound together.

The methods of the new astronomy have taught us that in the condition of the variable stars, where the intense glow has cooled to a red heat, we can see the future of our own sun as well as its past in the brilliant white and violet of the brightest and youngest stars. It requires the profound mathematical analysis of Gyldén to interpret his equations so as to explain to the new astronomy exactly how the phenomena of the rotation of variable stars produce the effects which are observed by its methods.

Professor Langley measures the light and heat of the moon by the new methods; Professor Darwin interprets the mathematical theory of the tides so as to trace back the origin of that heat to the remote time when the earth and moon formed one mass, and rotated in less than an eighth part of our present day. All the parts of the complex science are intimately connected, and no one can be separately treated without losing sight of many lines of research of the greatest promise and importance.

But I hope that enough has been said to show that the old astronomy is not idle; that it has its new side; and that its energies are addressed to the solution of problems of the highest significance. In broad terms, it is the noble aim of the new astronomy to trace the life-history of an individual star, and of the old to show how all these single stars are bound together to make a universe. There is no antagonism in their objects. Each is incomplete without the other.

PHOTOGRAPHY THE SERVANT OF ASTRONOMY[1]

IN order to appreciate the present state of astronomy, its new methods, its novel instruments, its recondite problems, it is necessary to glance at its condition a half century ago. The great astronomers, Bessel and W. Struve, were then contending in friendly rivalry to found the science on a sure basis. They had a perfectly definite object, and that object has been attained through their efforts, and through the efforts of the school of young men whom they trained either directly or indirectly—Argelander, Schoenfeld, Krueger, Auwers, Winnecke, Wagner, Schiaparelli in Europe, Walker, Coffin, Hubbard, Gould in America.

The attention of astronomers was then almost exclusively directed to the question of the motions of the heavenly bodies, as determined by the law of universal gravitation. The vast catalogues of stars which have been made in the past half century, as well as the accurate discussion and rediscussion of the older observations of Bradley (1750), at Greenwich, were all undertaken for this sole object. The school of mathematical astronomers founded by Euler, Laplace, Lagrange, Gauss, utilized these observations to the utmost. The examination of the surfaces of the planets was an entirely secondary question, and was largely left to amateur astronomers. The surface of the sun was studied only in the crudest manner, simply for the enumeration of the solar spots.

The fact that these spots were periodic was only established in 1851. Sir John Herschel was almost the only

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[1] From the "Overland Monthly," November, 1886.

astronomer by profession who devoted himself to observations not "of precision."

In this fifty years, an entirely new science has arisen—astrophysics—which is, indeed, the daughter of astronomy, but the cousin-german of chemistry, technics, physics.

This new science always had its cultivators, even before it had a name. The elder Herschel set himself the problem "to find out the construction of the heavens," and this is the problem of astrophysics, in contradistinction to the problem of exact astronomy—"to find out how the heavenly bodies move." The modern form of Herschel's phrase is, "to determine the present constitution and the evolution history of the stars, the comets, the sun, the planets."

We must regard Sir William Herschel as the founder of the science. He has had great followers: Schroeter, Sir John Herschel, Beer, Maedler, Fraunhofer, Kirchhoff, Bunsen, Lassell, Bond, De la Rue, Rutherfurd, Draper, Schiaparelli, Vogel, Janssen, Lockyer, Young, Langley, Pickering, not to speak of a host of other familiar names.

To-day there are several observatories devoted exclusively to the new science and their number is growing. This should be so. There are too many astronomical observatories idle. If the charm of the new fields is enough to make them efficient in forwarding the science as a whole, we must welcome the new impetus. But there is a note of warning to which we must give attention. We must keep strictly before us the means by which the older astronomy has arrived at its proud position as the chief of the physical sciences. For hundreds, yes, thousands of years, one principle has run through all of astronomy. Assiduous observations must be made according to well-considered plans, matured after deep reflection. The results of these observations must be compared with a theory expressed rigorously in the terms of mathematics. The differences between observation and theory must be treated by a profound analysis, so to derive corrections to the provisional theory. This provisional theory will in its turn become the basis of comparison with Nature, and so on, until the ideal is reached by successive approximations. This ideal is simple, and in many researches it has been attained already. It is to push the successive approximations until we can predict the position or the motion of a heavenly body as accurately as we can observe it. When this stage is reached we may leave the special problem in hand, until the methods of observation are themselves improved.

If astrophysics will accept this ideal and strive for it, there is no future so brilliant that we may not claim it for her portion. If this straight and narrow way is departed from, although the new science is followed never so assiduously, no essential progress can be expected, and real harm is sure to follow.

Astrophysics has three well-marked lines of research—namely: spectrum analysis (now a quarter of a century old), celestial photometry (half a century), celestial photography (dating back exactly forty-six years). Schiaparelli's theory of meteor-streams and their connection with comets belongs to this science in so far as it throws light upon the material out of which comets are built; and every part of physics which treats of the action of one body upon another body at a distance, whether through gravitation, heat, magnetism, electricity, has close relations to it. But the three main paths are spectroscopy, celestial photometry, and celestial photography. It is of the latter path that I wish to speak in this paper. We shall follow it assiduously at the Lick Observatory, and we shall have unrivalled opportunities to do so.

Spectroscopy in certain of its lines we shall also follow, and our opportunities in this branch also are unique. Photometry is so thoroughly done at the Harvard College Observatory that it would be a waste of energy for another American observatory to devote any great part of its time to such researches.

I assume that some slight explanation of the differences between a photographic telescope and an ordinary one will not be superfluous. The object-glass of an ordinary telescope brings the rays by which we see (those having a wave-length of about 6,000 ten-millionths of a millimetre) to an accurate focus. These can not be photographed except by special plates and with special difficulty. The rays which affect the photographic salts of silver have a wave-length of about 4,000 ten-millionths of a millimetre, and to bring these to a focus the two lenses of the ordinary achromatic object-glass must be supplemented by a third lens. This third lens is so arranged that it can be placed in front of (and close against) the ordinary objective, and it turns the telescope from a seeing instrument into a camera. It is also necessary to say that if the telescope remains fixed, while a bright star is passing across its field of view, the image of the star will pass across the sensitive plate, and will leave a "trail" which is the visible representative of the direction of the star's diurnal motion. Equatorial stars as faint as the eighth or ninth magnitude will give trails.

If, on the contrary, we attach an accurate driving clock to the telescope, and cause it to follow the star in its motion from east to west, rising to setting, we shall have instead of a trail a bright point, the photographic image. If we wish to make a picture of the sky, we must register the stars by such points as these. The trails have, however, various advantages, one of which is that they can not be mistaken for dust or for pin holes on the plate itself. The position of the dots in latitude and longitude can be very accurately measured. The latitude of the star can be even better determined from its trail, but its longitude must then be determined by special devices, which I need not describe. In the ordinary methods of observing, the astronomer views the visual images of the heavenly bodies, and either examines their surfaces, or determines their position with reference to adjacent bodies (as, for example, the positions of satellites relative to their planet), by means of extremely accurate and refined micrometers, forming a part of the eyepiece of his telescope.

To utilize photographic plates fully, and especially to make them a substitute for micrometric measures, it is necessary to contrive elaborate measuring engines to take the place of the costly micrometers, ordinarily used with telescopes. These engines measure the positions of the dots or trails on the plates, after these have been removed from the telescope.

Mr. Rutherfurd first made a satisfactory engine of this kind; it was then improved upon in the design of Professor Harkness adopted by the United States Transit of Venus Commission, in 1874, and the Lick Observatory owns the finest specimen of this class, which was made for it under the supervision of Professor Harkness.

The very first essay in astronomical photography was that of Professor John William Draper, of New York, who, in the year 1840, took a satisfactory daguerreotype of the moon. The experiments of Dr. Draper were repeated by George Bond, Director of the Harvard College Observatory, in 1850, and a lunar daguerreotype made by him was exhibited at London in 1851, at the World's Fair, where it attracted much attention.

During the years 1853 to 1857, Mr. De la Rue, of London, made lunar daguerreotypes and photographs, some of great excellence. In 1864 Dr. Lewis Rutherfurd, of New York, made an eleven-and-a-half-inch objective, which was corrected only for the photographic rays, and by means of this he obtained the finest photographs of the moon which had yet been made. Dr. Henry Draper, about the same time, made a fifteen-inch reflecting telescope with which he also took excellent lunar photographs. These latter have been enlarged to three and even to four feet in diameter, from the original picture of about two inches and a half. A long-focus telescope is of great advantage in these researches. The pictures in the principal focus of the Melbourne reflector are some six inches in diameter, and I have seen a few of these of great excellence. Such pictures can be enlarged, in printing, from six to twelve times.

The photographs of the moon in the focus of the Lick equatorial will be six inches in diameter, and will probably stand an enlargement of twelve times, so as to be six feet finally.

Lunar photographs have not advanced our knowledge in any important degree up to this time, however.

Solar daguerreotypes were first taken by Foucault and Fizeau, in 1845, at Paris, on the advice of Arago. In 1857 Mr. De la Rue contrived the photoheliograph for the Kew Observatory, by which solar photographs have been taken since that time daily at Kew and Greenwich.

M. Janssen, of Mendon, near Paris, about 1878, succeeded in making his exquisite photographs of the sun on glass, which show an astonishing amount of detail. I understand that these are chiefly made by means of a six-inch refractor, and I have never been able to comprehend how so much detail can be shown with an objective of such a small separating power, nor to rid myself of an impression that some, at least, of these details are due to atmospheric disturbances.

If the exposures are made extremely short ($\frac{1}{2000}$ to $\frac{1}{1000}$ of a second), very successful results can be obtained in solar photography. There is, undoubtedly, an important field of research still open here, especially with large objectives of great separating power.

The first photographs of a solar eclipse were made by Busch, at Königsberg, in 1851, and by Bartlett at West Point, in 1854. The eclipse photographs of Secchi and De la Rue, in 1860, were of high scientific importance, since they established beyond doubt the fact that the solar protuberances were really appendages of the sun, and not of the moon.

I believe the first photograph of the spectrum of the sun at a solar eclipse was taken at the Egyptian eclipse of 1882, by Professor Schuster, and also by the party under Mr. Lockyer. Very perfect photographs of the solar spectrum were taken at the total eclipse of 1883 in the Pacific Ocean, by the English parties and by the French parties, and the subject does not now present any great difficulties.

Photography served a very useful purpose in its application to the transits of Venus of 1874 and 1882.

According to Professor Pickering, the first daguerreotype of a star was taken at Harvard College Observatory, on July 17, 1850, under the direction of the elder Bond. The star Vega was satisfactorily daguerreotyped, and later the double star Castor gave an elongated image, which was plainly due to its two components. The sensitiveness of the daguerreotype plates then in use was so small that even such bright stars as these gave faint images, and no impression whatever was obtained from the Pole Star, no matter how long the exposure. These experiments were repeated with various stars and clusters, but finally the work was abandoned on account of photographic difficulties. In 1857 the younger Bond resumed the research. At this time the collodion process had greatly reduced the time of exposure, and the plates were of much greater sensitiveness. An impression of the double star Zeta Ursæ Majoris was obtained in eight seconds. A trail was obtained from the image of the bright star Vega. The faintest star photographed was the companion of Epsilon Lyræ, which is of the sixth magnitude—that is, just visible to the naked eye.

A series of measures was made of the relative positions and distances of the various double stars photographed, in order to see whether measures made upon a photographic plate could be used to replace those made in the ordinary manner at the telescope. It was found that a single measure made upon the plate was about of the same value as a single measure made by an astronomer with the ordinary micrometer. Professor Bond pointed out very clearly how photographic images might be used to determine accurately the relative brightness of stars, and also what the advantages of photography were for the permanent registration of star positions. Mr. De la Rue and Dr. Rutherfurd soon after repeated these experiments of Professor Bond, and a very extended investigation was undertaken in 1864 by Dr. Rutherfurd, and continued by him for many years. Most of the principal clusters in the northern heavens were photographed, as well as most of the brighter double stars. These researches have never been fully utilized for the following reason: the photographs were measured in the most careful manner on a measuring engine, in which the distances of one star from another were determined by means of a very accurate screw. After the series of measures had been continued for several years, it was discovered that the screw itself had worn considerably, so that the value of its revolutions was not the same as it had formerly been. It was impossible to discover at what time this wear commenced, or how it progressed, and therefore these excellent photographs have remained undiscussed up to the present time.[2] The distances, which must be accurately measured, are about $\frac{1}{50000}$ of an inch. The faintest stars shown in Dr. Rutherfurd's eleven-inch telescope are about of the ninth magnitude. The plates used by Dr. Rutherfurd were, I believe, exclusively wet plates.

Dr. Henry Draper attacked the same problem in 1880, using, however, the most sensitive dry plates then available. In 1881, with an eleven-inch refractor constructed by the Clarks, he obtained a photograph of the nebula in Orion, in which one of the stars is shown whose magnitude is not more than 14½. This star is barely visible with a telescope of the same aperture as that with which the photograph was taken. The photographic plate now had become as efficient an instrument of research as the eye itself. M. Janssen also photographed the nebula in Orion in 1881, but the best of all such photographs has been made by Mr. Common, of England, with his three-foot silver-on-glass reflector.

Dr. B. A. Gould, in his expedition to the southern hemisphere (1870–'84), carried with him a photographic lens of eleven inches aperture, and during his entire stay of more than ten years employed all the available time at his command in accumulating negatives of the principal southern double stars and clusters. These photographs have not yet been discussed, and Dr. Gould has discovered that there are signs that the films on the negatives are now

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[2] They are now (1900) being remeasured and rediscussed at Columbia University.

beginning to deteriorate. Probably this extensive and important series will soon receive discussion.[3]

During the years 1882–'86 many observatories have undertaken some researches in stellar photography. The Royal Astronomer at the Cape of Good Hope, Dr. Gill, has undertaken to make a map of the whole southern heavens by photographic means only. The Rev. T. E. Espin, of Liverpool, has published a catalogue of the magnitudes of 500 stars, determined by means of photography alone. The most extensive investigation is that of the brothers Paul and Prosper Henry, of the Observatory of Paris. Important investigations have also been made at the Astrophysical Observatory of Potsdam and at two Physical observatories in Hungary.

In 1863 Dr. Huggins, of London, obtained a photographic image of the spectrum of Sirius, but no lines were visible in this spectrum. The first successful photograph of the spectrum of a star was obtained by Dr. Henry Draper in 1872. Both of these astronomers succeeded in 1876 in obtaining valuable spectrum photographs of the brightest stars. In 1882 they both obtained a photograph of the spectrum of the nebula in Orion. Since 1882 many astronomers and observatories have devoted themselves to photographic researches, but little has been published, except by the Observatory of Harvard College. Here the years 1882–'85 were spent in very elaborate experiments, preliminary to undertaking larger and more important researches. A very large number of photographs have been taken of the regions lying about the north celestial pole. The photographic telescope employed is eight inches in aperture. The chief results up to now have been the establishing the relative brightness of 117 stars within one degree of the pole. A second research in progress is the determination of the relative brilliancy of all the brighter stars. Other experiments are in hand, but, as they all relate to photometry or to spectroscopy, they may be passed over here,

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[3] This work has lately been published under the direction of Dr. S. C. Chandler.

after merely calling attention to them as the most important researches of the kind.

In 1882 Dr. Gill, at the Cape of Good Hope, succeeded in photographing the great comet of that year, and in doing this he proved the practicable possibility of making star maps, which should contain all the stars down to the tenth magnitude. In 1885 the Royal Society granted £300 to the Cape of Good Hope Observatory for photographic purposes. Dr. Gill has set himself to the solution of two problems: First, that of securing as soon as possible a complete photographic map of the southern heavens, containing every star visible down to the tenth magnitude, so as to continue the "Durchmusterung" of Argelander; secondly, to test the possibility of photographing the solar corona daily without an eclipse, by the method first suggested by Dr. Huggins. For the first purpose Dr. Gill makes use of one of Dallmeyer's rapid rectilinear combinations, composed of two concavo-convex achromatic combinations of six inches aperture. This camera is mounted on an equatorial stand, and is pointed by means of a telescope of forty-five inches focal length and three and a half inches aperture. The exposures are an hour long when the sky is clear. Each plate is six inches square, and covers an area of about thirty-six degrees. Every such area is photographed twice, so as to render it impossible to confound the images of faint stars with minute dust specks. In this way a great portion of the sky has already been photographed in duplicate.

The same observatory has recently obtained a much more powerful optical apparatus through the generosity of Mr. James Nasmyth, who has purchased a specially corrected photographic objective of nine inches aperture and nine feet focal length, made by Mr. Grubb, of Dublin. The field of this Nasmyth lens will be much more limited than that of the Dallmeyer apparatus, but it is expected to obtain from it a photograph of all stars to the twelfth or thirteenth magnitude inclusive within a circle of a radius of one or one and a half degree.

Mr. Roberts, in England, has erected a reflector of twenty inches aperture and of one hundred inches focus for stellar photography alone, and has made considerable progress in the work of charting the northern heavens. The size of the field of Mr. Roberts's telescope is two degrees in declination and one and a half degree in right ascension. The time of exposure is fifteen minutes in a clear sky. The companion to the Pole Star is just visible in four seconds under the best circumstances. Mr. Roberts refers to an important difficulty, which is, that in most photographic plates there are small specks in the film, many of which look like stars, and which are extremely difficult to distinguish from stars even when they are viewed through a microscope. Dr. Gill, at the Cape of Good Hope, avoids this difficulty by taking two photographs of the same field successively, giving to each an exposure of one hour. At Paris three exposures of an hour each are made, on the same plate.

Mr. Common's experiments commenced in 1879. At this time, using dry plates with his three-foot reflector, he took successful pictures of the Pleiades, with one and a half minute exposure, showing all the stars to the eighth and ninth magnitude. In 1882 he devoted his time to photographing the nebula in Orion, and has obtained wonderful results.

After making such a splendid success with his three-foot reflector, Mr. Common is now making one of five feet in aperture. There is no doubt that a mirror of this aperture can be accurately figured by the optician. The difficulties in using it come from unequal flexure of its various parts and from their differing temperatures. Difficulties of this nature have never yet been successfully overcome for reflectors of more than thirty-six inches of aperture, but Mr. Common's great mechanical skill and knowledge and experience lead us to hope that he may succeed in this important undertaking.

In September, 1884, Dr. Lohse used the eleven-inch refractor of the Potsdam Observatory to photograph the star cluster in Perseus. An exposure of forty-five minutes was given, and stars as faint as the tenth and eleventh magnitude were registered.

A number of other star clusters have also been photographed by Dr. Lohse. The Savilian Observatory at Oxford (England) has undertaken to study two constellations (Lyra and Cassiopeia) by photography on plates one degree square.

The early experiments at the Paris Observatory, 1884, were made with a telescope with an aperture of $\frac{16}{100}$ of a metre (6.3 inches), and they were so successful that it was decided to make a larger instrument specially for photography, and soon an objective of $\frac{34}{100}$ of a metre aperture (13.4 inches) and $3\frac{43}{100}$ metres focal length (134 inches) was made. Parallel to this photographic telescope, one of about the same focus and of $\frac{24}{100}$ of a metre (9.5 inches) aperture is placed as a directing telescope. In May, 1885, the new photographic telescope was first brought into use, and a few of the important results that have been reached by it are mentioned below. Stars down to the fifteenth magnitude are photographed with an exposure of one hour, the plates being something more than two degrees square. From one to two thousand stars are shown to each square degree with this exposure, using dry plates. On these plates three separate exposures of an hour each are given, the instrument being moved between each exposure, so as to change the position of the image on the plate about five seconds of arc each time. The three images of the same star thus form a little triangle. By means of this telescope a new and very faint nebula has been discovered in the Pleiades, which would never have been discovered if we depended on the eye alone. Admirable photographs of Saturn have been taken by direct enlargement of the primary image, through a non-achromatic eyepiece, which gives a magnifying power of eleven times. Hyperion, the faintest satellite of Saturn, a difficult object in the twenty-six-inch telescope at Washington, has been photographed with an exposure of thirty minutes, and the satellite of

Neptune can be taken in any part of its orbit. With an exposure of one hour the eleventh- and fifteenth-magnitude stars have an actual diameter of about $\frac{1}{1000}$ of an inch—that is, in arc about one and a half second. Stars of the fifth or sixth magnitude are about one minute in diameter with long exposures. With a properly limited exposure, these also are of extremely minute dimensions.

The proper exposure for a first-magnitude star, like Sirius or Vega, is not more than $\frac{5}{1000}$ of a second. For a star just visible to the naked eye, half a second is sufficient. For stars of the tenth magnitude, twenty seconds; of the twelfth, two minutes; of the thirteenth, five minutes; of the fourteenth, thirteen minutes; and for the faintest visible, an hour and twenty-three minutes. These results are, of course, a minimum, and but approximate.

So far as is known, the growth of the image of a star upon the photographic plate is equal, and concentric with the point of the plate, on which the image of the star falls. The faintest stars on these Paris plates are, as was said, arranged in little groups of three. As the brighter stars on these plates are examined, it is found that the size of each of the images increases until they overlap; this continues until the complete images of any one bright star are very much larger than the original small triangle. The appearance is as if three circles nearly as large as the resulting image had been struck from the centre of what would have been (in the case of a smaller star) the three separate stars of a group. It is the intention of the director of the Observatory of Paris to use this photographic telescope to continue the construction of ecliptic charts. And it is suggested by the director that by means of the co-operation of six or eight observatories the whole heavens should be charted in a similar way. There are 41,000 square degrees in the whole heavens, and if six square degrees can be registered on a plate (with one hour's exposure), 7,000 such plates must be made, requiring at least 7,000 hours. To avoid mistakes, at least two exposures must be given for each region, or 14,000 plates and 14,000 hours are necessary.

If we allow 100 clear nights in a year (which is a fair allowance for all observatories except the Lick Observatory, where we can count on at least 200), it would require 140 years at any one observatory to do this work, or 14 at ten observatories. I personally doubt whether the strict adherence to a plan, which is indispensable to success, could be maintained at so many establishments for so long a period. The whole subject is yet in too unsettled a state to warrant an international undertaking of such magnitude at present. A number of years must be spent in tentative researches before the right paths are struck out. I give some of the most obvious directions for these trials in what follows.

The two hundred and sixty or more small planets (asteroids) which lie between Mars and Jupiter have all been discovered by the slow process of comparing a star map, night after night, with the heavens. A star not on the map is either an omitted star to be inserted, or a minor planet, known or unknown. A photographic objective of twelve inches aperture will show a trail for a star of the magnitude of the brighter asteroids with an exposure of half an hour. An hour's exposure will probably show the trail of the faintest asteroids (twelve to thirteen magnitude). One of the immediate results of the application of photography will undoubtedly be to greatly increase the number of known asteroids.

There are reasons to believe in the existence of a major planet exterior to Neptune. If such a planet exists it is not likely to be brighter than the tenth magnitude, and its motion will be very slow. Hence it is unlikely, at least, that such a planet can be discovered by its trail on the plate. The method of three exposures on the same plate employed at Paris probably would not disclose the existence of a trans-Neptunian planet, though it would suffice for the detection of Neptune itself in most parts of its orbit. Probably the surest way to detect such a body, if it exists, would be to take photographs of the same region on successive days. Such plates would then have to be laboriously compared, star by star. Doubtful cases would require a third night's work to be done in order to decide.

A blue-print of two such plates will enable all the brighter stars to be quickly compared and disposed of. The real labour will then be confined to the stars less bright than the faintest which can be blue-printed. The problem of the constitution of the stellar universe must be studied, it seems, by some kind of celestial statistics derived from counts or gauges of the stars. Nearly all the conclusions we have so far reached are based on the counts made by Sir William Herschel. I have myself spent much time in continuing these. All such work is now useless. Photographic maps will give us all the requisite data, and will throw much light, too, on another closely connected problem—the extinction of light in space—provided only that all negatives taken for this object are made strictly comparable in every respect. This proviso is of the utmost importance, and very difficult to be lived up to in any work done by co-operating observatories. It is just possible that photometric measures of the photographs of a very eccentric asteroid can now be made with sufficient delicacy to settle the question whether light is, or is not, extinguished in space.

The precision of the photographic images of stars is so great that there is no doubt that measures of the negatives of double stars, of star clusters and groups, will, at least in most instances, take the place of the painful and laborious micrometric measures which are now employed by observers. The photographs have their own errors, and many of them; but these are all susceptible of investigation.

The shrinkage of the gelatine films of the negatives is likely to prove a grave difficulty in the application of photography to exact astronomy, but this can always be detected by photographing a network of lines on glass. Very serious difficulties of this kind have lately been met with by Professor Pritchard, of Oxford, in his researches on the (photographic) parallax of 61 Cygni.

But photographic plates have also many capital advantages. For example, the photographic impress of a star gives really its mean or average position, freed from those accidental and transitory variations of place which are due to variations of atmospheric refraction—a constant source of error. The saving of time is also important.

An exposure of an hour has given (at the Paris Observatory) a map of 5,000 stars in four square degrees in the constellation Cygnus. The best maps we now have give 170 of the brightest stars only in this place. To map 5,000 stars by the eye alone would require several years. The writer spent all the time he could spare from routine observations during four years with the twenty-six-inch equatorial, at Washington, in a study of the nebula in Orion. Every important result reached by that study, and very many not comprised in it, was attained by Mr. Common's photograph (subsequently taken), which required an exposure of forty minutes only.

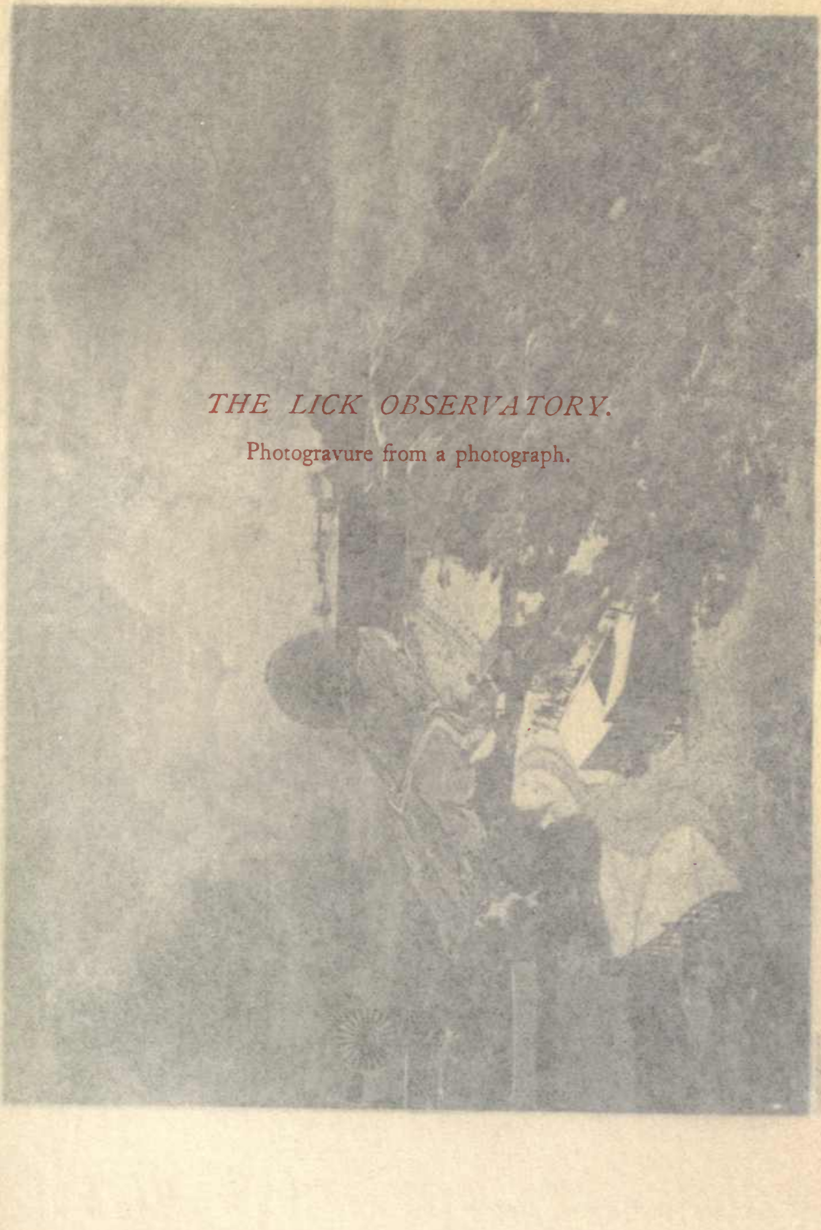
Another important advantage of the new methods is that they do not require highly skilled observers. It required a Bessel or a Struve to determine the parallax of 61 Cygni or of Vega. But photographic exposures can be made, and glass negatives successfully measured by well-trained assistants, after the plan of observation has once been thoroughly thought out. This is no slight benefit. The skill of the astronomer is reserved for real difficulties, and the merely laborious work can be done in duplicate, if necessary, by younger men.

Again, the chemical plate is sensitive to a whole series of rays, which produce no effect on the human eye. Only half of the faintest stars of any photographic map are visible to the eye in the same telescope. Photographic methods thus increase the range of our vision immensely; they also increase its sharpness. The photographic plate will register the sum of all the impressions it receives. It does not tire, as the eye does, and refuse to pay attention for more than a small fraction of a second, but it will faithfully record every ray of light that falls upon it, even for hours, and finally it will produce its automatic register, so that the eye can see it, and so that this can be measured, if necessary, again and again. The permanence of the records is of the greatest importance, and, so far as we know, it is complete when the best modern plates are employed.

We can hand down to our successors a picture of the sky locked in a box. What would we not give for such a record bequeathed to us by Hipparchus or by Galileo!

It will be of interest to briefly state here how far the equipment of the Lick Observatory will fit it to engage in this important branch of research. It is known that the situation of the observatory is the finest in the world, both as to the number of clear days and as to the quality of steady atmosphere. The observatory will be completely equipped for all micrometric work, and also for all spectroscopic researches.

Mr. Lick's bequest for the observatory was $700,000, of which nearly $200,000 will remain after the observatory is completed The income from this sum must support the observatory for the present. Although the whole plan of the observatory has been made with direct reference to keeping its running expenses low, it is clear that the company of astronomers will have to be kept small. The work of these observers must be concentrated on the large equatorial, and even then their energies will not be sufficient to utilize every moment. It is not our intention to jealously guard the immense scientific opportunity for ourselves, for California, or even for the United States. The real gift of Mr. Lick was to the world. We mean to put the large telescope at the disposition of the world, by inviting its most distinguished astronomers to visit us one at a time, and to give them the use of the instrument during certain specific hours of the twenty-four. Each day there will be certain hours set apart when the observatory staff will relinquish the use of the equatorial to distinguished specialists who will come upon our invitation from the United



THE LICK OBSERVATORY.

Photogravure from a photograph.

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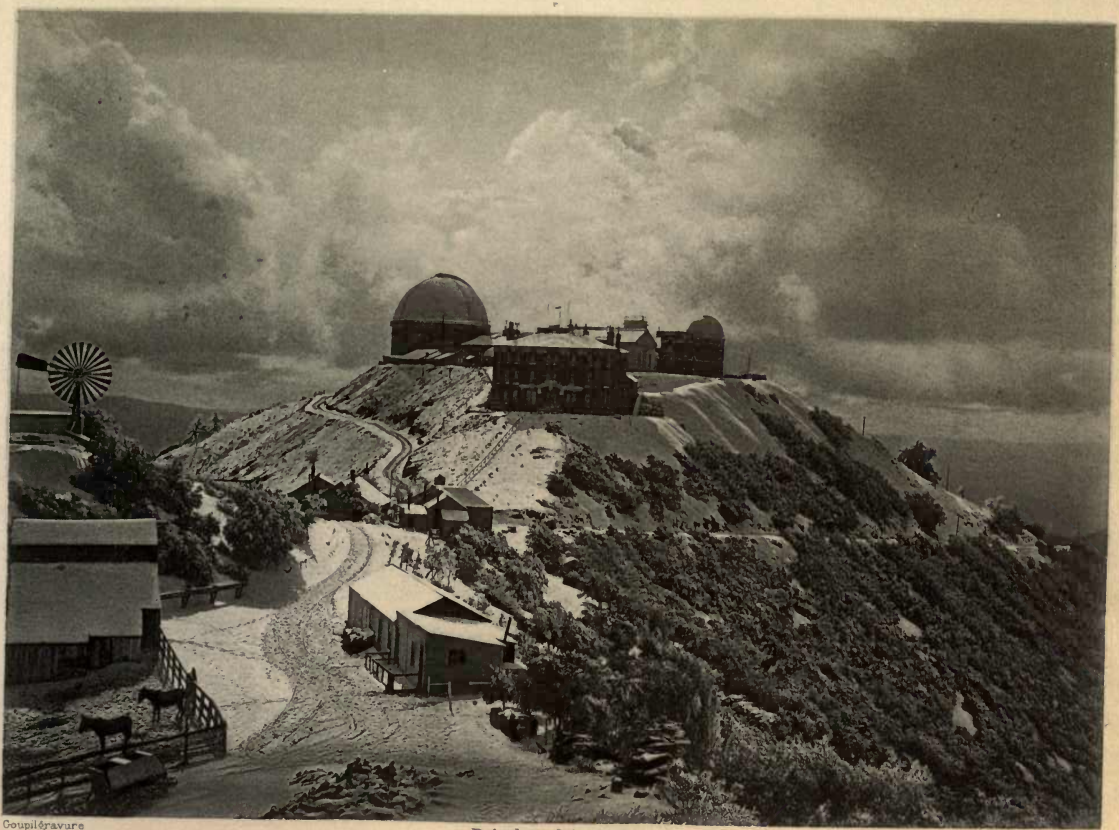

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States and from Europe to solve or to attack some one of the many unsolved problems of astronomy. In this way we hope to make the gift of Mr. Lick one which is truly a gift to science, and not merely a gift to California and to its university.

Even under such circumstances it will be impossible to utilize the instrumental outfit to the full. It was clearly the duty of the Lick trustees to make this observatory perfect in every respect, and to provide it with all the instruments necessary to a complete equipment. This they have done economically and wisely. So far as I can judge, there is nothing that should be altered. The instruments are all necessary, and they are mounted in the most perfect manner.

Each one is directly subordinate to the large equatorial and accessory to it. Nothing has been purchased, and no work has been done, which does not directly tend to make the observations made by the large equatorial either more complete or more immediately useful. The cost of the whole observatory may fairly be said to be the cost of the great telescope in place and entirely ready for work. The objective itself has cost $52,000. The photographic lens will add $13,000 to this. The mounting which is to carry the tube of nearly 60 feet in length is to be made and delivered for $42,000. The dome, of 70 feet interior diameter, will be built in San Francisco, and I have no doubt that it will be materially better than any now made. The chief novelty will be the adoption of Mr. Grubb's ingenious plan for placing the observer in a proper position with reference to his telescope. We have to recollect that the eyepiece of the telescope may be about 5 feet from the floor of the dome when the telescope is pointed to the zenith, or it may be 35 feet when the telescope is in the horizontal position. The ordinary observing chair, which is convenient enough when it is not more than 16 feet high, becomes a cumbrous and inconvenient affair when it is extended to 35 feet. Mr. Grubb proposed to remedy this by raising the whole floor of the dome, like an elevator, to the proper height. The whole floor will be raised vertically a distance of 16½ feet by four hydraulic jacks. The ascent is made in eight minutes with a perfectly parallel motion. The water supply for this purpose comes from the watershed of the dome itself. The last mechanical difficulty is now overcome, and it is expected that the steel dome will be mounted during the present year, or at least in the spring of 1887. The contract price for the dome delivered and erected is $56,800, and for the moving floor $14,250. The sum of these items is $178,000, and if this is increased by others not named here it will raise the cost of the large instrument in place to $200,000. The preparation of the top of the mountain to receive the buildings, the erection of the buildings themselves and the observers' houses, and above all the provision of an adequate water supply, have been covered by the remaining $300,000.

With faithfulness on the part of the company of astronomers to which this magnificent equipment is confided, and with the generous support of the friends of science in California, much may be expected to follow from this splendid gift to America and to the world.

THE BEGINNINGS OF AMERICAN ASTRONOMY[1]

IT is impossible, even in the briefest sketch, not to emphasize the debt of American science and learning to the intelligent interest and patronage of our early Presidents—Washington, John Adams, Jefferson, Madison, Monroe, John Quincy Adams. The powerful impetus given by them and through them has shaped the liberal policy of our Governments, national and State, toward education and toward science. Sir Lyon Playfair, in his address to the British Association for the Advancement of Science (1885), has recognised this influence in the truest and most graceful way. He said: "In the United Kingdom we are just beginning to understand the wisdom of Washington's 'Farewell Address to his Countrymen' (1796) when he said: 'Promote, as an object of primary importance, institutions for the increase and diffusion of knowledge; in proportion as the structure of a government gives force to public opinion, it is essential that public opinion should be enlightened.'"

Until the Revolution (1776) American science was but English science transplanted, and it looked to the Royal Society of London as its censor and patron. Winthrop, Franklin, and Rittenhouse were, more or less, English astronomers. Franklin was the sturdiest American of the three. As early as 1743 he suggested the formation of the American Philosophical Society of Philadelphia. John Adams founded the American Academy of Arts and Sciences in Boston in 1780. These two societies, together

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[1] From "Science," June 18, 1897.

with Harvard College (founded in 1636), Yale College (1701), the University of Virginia (founded by Jefferson in 1825), and the United States Military Academy at West Point (1801), were the chief foci from which the light of learning spread. Other colleges were formed or forming all over the Eastern and Middle States during the early years of the century.

The leading school of pure science was the Military Academy at West Point, and it continued to hold this place until the civil war of 1861. From its corps of professors and students it gave two chiefs to the United States Coast Survey; and the army, particularly the corps of engineers, provided many observers to that scientific establishment, besides furnishing a large number of professors and teachers of science to the colleges of the country. The observatory of the academy was founded by Bartlett in 1841, and much work was done there, only a small part of which is published. The Coast Survey was a school of practice for army officers, and their experience was utilized in numerous boundary surveys during the period 1830–'50. Colonel J. D. Graham, for example, was astronomer of the survey of the boundary between Texas and the United States in 1839–'40; commissioner of the Northeast boundary survey, 1840–'43; astronomer of the Northwest boundary survey, 1843–'47; of the boundary between the United States and Canada, 1848–'50; of the survey of the boundary between Pennsylvania and Virginia, 1849–'50; of the boundary survey between Mexico and the United States, 1850–'51. The names of Bonneville, Talcott, Cram, Emory, and other army officers are familiar in this connection, and their work was generally of a high order. It was in such service that Talcott invented or re-invented the Zenith Telescope, now universally employed for all delicate determinations of latitude. The mechanical tact of Americans has served astronomy well. The sextant was invented by Thomas Godfrey, of Philadelphia, in 1730, a year before Hadley brought forward his proposal for such an instrument.[2] The

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[2] In 1700 Sir Isaac Newton sent drawings and descriptions of a

chronograph of the Bonds, the Zenith Telescope of Talcott, and the break-circuit chronometer of Winlock are universally used to-day The diffraction gratings of Rutherfurd were the best to be had in the world till they were replaced by those of Rowland. The use of a telescope as a collimator was first proposed by Rittenhouse. The pioneer opticians of the United States were Holcomb (1826), Fitz (1846 or earlier), Clark (1845), Spencer (1851). Only the Clarks have a world-wide reputation. Würdemann, instrument maker to the United States Coast Survey (1834), had a decided influence on observers and instrument makers throughout the United States, as he introduced extreme German methods and models among us where extreme English methods had previously prevailed. The system of rectangular land surveys which proved to be so convenient for the public lands east of the Rocky Mountains was devised and executed by Mansfield, a graduate of the Military Academy.

The list of army officers who became distinguished in civil life as professors in the colleges of the country is a very long one. Courtenay (class of 1821 at West Point) was professor of mathematics at the University of Pennsylvania, 1834–'36, at the University of Virginia, 1842–'43, and was the author of admirable text-books. Norton (class of 1831) became professor at New Haven, and wrote a very useful text-book of astronomy in 1839; and the list could be much extended. The excellent training in mathematics at West Point (chiefly in French methods) early made itself felt throughout the whole country. The mathematical text-books of Peirce, of Harvard, and of Chauvenet, of the Naval Academy, brought the latest learning of Europe to American students. Mitchel (class of 1829 at West Point) was the only graduate who became a professional

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reflecting sextant to Halley for his advice. At Halley's death these were found among his papers. Hadley's device (1731) was undoubtedly derived from Newton's manuscripts. The Royal Society of London granted two hundred pounds to Godfrey for his invention, which his brother, Captain Godfrey, had previously put into practical use in the West Indies.

astronomer (1842–'61). His direct service to practical observing astronomy is small, but his lectures (1842–'48), the conduct of the Cincinnati Observatory (1845–'59), and his publication of the "Sidereal Messenger" (1846–'48), together with his popular books, excited an intense and widespread public interest in the science, and indirectly led to the foundation of many observatories. He was early concerned in the matter of using the electric current for longitude determinations, and his apparatus was only displaced because of the superior excellence of the chronograph devised by the Bonds. His work was done under immense disadvantages, in a new community (Ohio), but the endowment of astronomical research in America owes a large debt to his energy and efforts.

The navy and the United States Naval Academy (founded by Bancroft in 1845, at the suggestion of Chauvenet) were very active in astronomical work. Chauvenet (Yale College, 1840) published a text-book of trigonometry, in 1850, which had an important share in directing attention to rigid, elegant, and general methods of research. His "Astronomy" (1863) is a handbook for all students. Walker, Gilliss, Coffin, Hubbard, Ferguson, Keith, Yarnall, Winlock, Maury, Wilkes, were all connected with the navy more or less intimately. Walker's career was especially brilliant; he graduated at Harvard College in 1825, and established the observatory of the Philadelphia High School in 1840. He was the leading spirit in the United States Naval Observatory at Washington (1845–'47), and introduced modern methods into its practice at the beginning. From the observatory he went to the Coast Survey to take charge of its longitude operations, and he continued to direct and expand this department until his death, in 1853. To him, more than to any single person, is due the idea of the telegraphic method ("the American method") of determining differences of longitude. His assistant in this work was Gould, who succeeded to the charge of it in 1853. His researches extended to the field of mathematical astronomy also, and his theory of the planet Neptune (then newly discovered) marks an important step forward. His investigations and those of Peirce were conducted in concert, and attracted general and deserved attention.

The exploring expedition of Wilkes required corresponding observations to be made in America, and during the period 1838–'42 William Bond, at Dorchester, and Lieutenant Gilliss, at Washington, maintained such a series with infinite assiduity and with success. The results of Gilliss's astronomical expedition to the southern hemisphere (Chile, 1849–'52) were most creditable to him and to the navy, though his immediate object—the determination of the solar parallax—was not attained.

The Coast Survey began its work in 1817 under Hassler, a professor from West Point, who impressed upon the establishment a thoroughly scientific direction. Bache, his successor (a grandson of Benjamin Franklin), was a graduate of West Point in the class of 1825, and took charge of the Survey in 1843. He is the true father of the institution, and gave it the practical efficiency and high standard which characterized its work. He called around him the flower of the army and navy, and was ably seconded by the permanent corps of civilian assistants—Walker, Saxton, Gould, Dean, Blunt, Pourtales, Boutelle, Hilgard, Schott, Goodfellow, Cutts, Davidson, and others.

Silliman's (and Dana's) "American Journal of Science" had been founded at New Haven in 1818, and served as a medium of communication among scientific men. A great step forward was made in the establishment of the "Astronomical Journal" by Dr. Gould on his return from Europe at the close of 1849.[3] Silliman's "Journal" was chiefly concerned with the non-mathematical sciences, though it has always contained valuable papers on mathematics, astronomy, and physics, especially from the observers of Yale College—Olmsted, Herrick, Bradley, Twining, Norton, Newton, Lyman, and others. In Mason, who died in 1840

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[3] The "Astronomische Nachrichten" had been founded in Altona, by Schumacher, in 1821.

at the age of twenty-one, the country lost a practical astronomer of the highest promise.[4] Gould's "Journal" was an organ devoted to a special science. It not only gave a convenient means of prompt publication, but it immediately quickened research and helped to enforce standards already established and to form new ones. The "Astronomical Notices" of Brünnow (1858–'62) might have been an exceedingly useful journal with an editor who was willing to give more attention to details, but, in spite of Brünnow's charming personality and great ability, it had comparatively little influence on the progress of the science.

The translation of the "Mécanique Céleste" of Laplace by Nathaniel Bowditch, the supercargo of a Boston ship (1815–'17), marks the beginning of an independent mathematical school in America. The first volume of the translation appeared in 1829; at that time there were not more than two or three persons in the country who could read it critically. The works of the great mathematicians and astronomers of France and Germany—Laplace, Lagrange, Legendre, Olbers, Gauss, W. Struve, Bessel—were almost entirely unknown.

Bowditch's translation of the "Mécanique Céleste," and, still more, his extended commentary, brought this monumental work to the attention of students and within their grasp. His "Practical Navigator"[5] contained the latest and best methods for determining the position of a ship at sea, expressed in simple rules. American navigators had no superiors in the first half of this century. Nantucket whalers covered the Pacific, Salem ships swarmed in the Indies, and the clipper ships made passages round the Horn to San Francisco, which are a wonder to-day. Part of their success is due to the bold enterprise of their

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[4] See the "International Review," vol. x, p. 585.

[5] First edition, 1802. Sumner's method in navigation (1843)—a very original and valuable contribution from a Boston sea-captain—and Maury's "Wind and Current Charts," begun in 1844, are two other notable contributions from a young country to an art as old as commerce.

captains (who were said to carry deck-loads of studding-sail booms to replace those carried away), but an important part depended on their skill as observers with the sextant. One of the sister ships to the one of which Bowditch was supercargo was visited at Genoa by a European astronomer of note (Baron de Zach), who found that the latest methods of working lunar distances to determine the longitude were known to all on board, sailors as well as officers. His bewilderment reached its climax when the navigator called the negro cook from the galley and bade him expound the methods of determining the longitude to the distinguished visitor.

On Bowditch's own ship there was "a crew of twelve men, every one of whom could take and work a lunar observation as well, for all practical purposes, as Sir Isaac Newton himself." Such crews were only to be found on American ships in the palmy days of democracy. All were cousins or neighbours, and each had a "venture" in the voyage. But these anecdotes may serve as illustrations of the intellectual awakening which came about as soon as our young country was relieved from the pressure of the two wars of 1776 and 1812. An early visitor, Baron Hyde de Neuville (1805), felt "an unknown something in the air," "a new wind blowing." This new spirit, born of freedom, entered first into practical life, as was but natural; science next felt its impulse, and, last of all, literature was born. Emerson hailed it (in 1837) "as the sign of an indestructible instinct." "Perhaps the time is already come," he says, "when the sluggard intellect of this country will look from under its iron lids and fill the postponed expectation of the world with something better than the exertions of mechanical skill. Our day of dependence, our long apprenticeship to the learning of other lands, draws to a close. The millions that around us are rushing into life can not always be fed with the sere remains of foreign harvests."

Benjamin Peirce, a graduate of Harvard in the class of 1829, had been concerned with the translation of the "Mécanique Céleste," and was early familiar with the best mathematical thought of Europe. He became professor in Harvard College in 1833, and, after the death of Bowditch, in 1838, he was easily the first mathematical astronomer in the country. His instruction was precisely fitted to develop superior intelligences, and this was his prime usefulness. Just such a man was needed at that time. Besides his theoretical researches on the orbits of the planets (especially Uranus and Neptune) and of the moon, his study of the theory of perturbations, and his works on pure mathematics and mechanics, he concerned himself with questions of practical astronomy, although the observations upon which he depended were the work of others. He was the consulting astronomer of the "American Ephemeris and Nautical Almanac" from its foundation in 1849, and its plans were shaped by him to an important degree. His relative, Lieutenant Davis, United States Navy (the translator of Gauss's "Theoria Motus Corporum Cœlestium") (1857), was placed in charge of the "Ephemeris," and the members of its staff—Runkle, Ferrel, Wright, Newcomb, Winlock, and others—most effectively spread its exact methods by example and precept. Professor Peirce undertook the calculations relating to the sun, Mars, and Uranus in the early volumes of the "Ephemeris." As a compliment to her sex, Miss Maria Mitchell was charged with those of Venus, Mercury was computed by Winlock, Jupiter by Kendall, Saturn by Downes, Neptune by Sears Walker.

The Smithsonian Institution was founded in 1846, and Joseph Henry was called from Princeton College to direct it. There never was a wiser choice. His term of service (1846–'78) was so long that his ideals became firmly fixed within the establishment, and were impressed upon his contemporaries and upon a host of younger men. The interests of astronomy were served by the encouragement of original research through subsidies and otherwise, by the purchase of instruments for scientific expeditions, by the free exchange of scientific books between America and Europe, and by the publication of the results of recondite investigations. It is by these and like services that the institution is known and valued among the wide community of scientific men throughout the world.

But this enumeration of specific benefits does not convey an adequate idea of the immense influence exercised by the institution upon the scientific ideals of the country. It was of the first importance that the beginnings of independent investigation among Americans should be directed toward right ends and by high and unselfish aims. In the formation of a scientific and, as it were, a moral standard a few names will ever be remembered among us; and no one will stand higher than that of Henry. His wise, broad, and generous policy, and his high personal ideals, were of immense service to his colleagues and to the country.

The establishment of a national observatory in Washington was proposed by John Quincy Adams in 1825; but it was not until 1844 that the United States Naval Observatory was built by Lieutenant Gilliss, of the navy, from plans which he had prepared. By what seems to have been an injustice Gilliss was not appointed to be its first director.[6] This place fell to Lieutenant M. F. Maury. Gilliss had been on detached service for some years, and a rigid construction of rules required that he should be sent to sea, and not remain to launch the institution which he had built and equipped.

The first corps of observers at Washington (1845) contained men of first-class ability—Walker, Hubbard, Coffin. Gilliss's work as astronomer to the Wilkes Exploring Expedition (1838–'42), at his little observatory on Capitol Hill, had shown him to be one of the best of observers, as well as one of the most assiduous. His study and experience in planning and building the Naval Observatory had broadened his mind. To the men just named, with Peirce, Gould, and Chauvenet, and to their coadjutors and pupils, we owe the introduction of the methods of Gauss, Bessel, and Struve into the United States, and it is for this reason

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[6] He was, however, director during the years 1861–'65.

that American astronomy is the child of German and not of English science.

The most natural evolution might seem to have been for Americans to follow the English practice of Maskelyne and Pond. But the break caused by the War of Independence, by the War of 1812, and by the years necessary for our youthful governments to consolidate (1776–1836), allowed our young men of science to make a perfectly unbiased choice of masters. The elder Bond (William Cranch Bond, born 1789, director of Harvard College Observatory, 1840–'59) was one of the older school and received his impetus from British sources during a visit to England in 1815.

In estimating the place of the elder Bond among scientific men it is necessary to take into account the circumstances which surrounded him. He was born in the first year of the French Revolution (1789); he was absolutely self-taught; practically no astronomical work was done in America before 1838. While Admiral Wilkes was seeking for coadjutors to prosecute observations in the United States during the absence of his exploring expedition he was indeed fortunate in finding two such men as Bond and Gilliss. Their assiduity was beyond praise, and it led each of them to important duties. Bond became the founder and director of the Observatory of Harvard College, while Gilliss is the father of the United States Naval Observatory at Washington, as well as of that of Santiago de Chile, the oldest observatory in South America. Cambridge, though the seat of the most ancient university in America, was but a village in 1839. The college could afford no salary to Bond, but only the distinction of a title, "Astronomical Observer to the University," and the occupancy of the Dana house, in which his first observatory was established. His work there, as elsewhere, was well and faithfully done, and it led the college authorities to employ him as the astronomer of the splendid observatory which was opened for work in 1847. At that time the two largest telescopes in the world were those of the Imperial Observatory of

Russia (Pultowa) and its companion at Cambridge. Each of these instruments has a long and honourable history. Their work has been very different. Who shall say that one has surpassed the other? We owe to Bond and his son the discovery of an eighth satellite to Saturn, of the dusky ring to that planet, the introduction of stellar photography, the invention of the chronograph, by which the electric current is employed in the registry of observations, the conduct of several chronometric expeditions between Liverpool and Boston to determine the transatlantic longitude, and a host of minor discoveries and observations.

Gilliss visited France for study in 1835, before he took up his duties at Washington. The text-books of Bond and Gilliss were the " Astronomies " of Vince (1797–1808) and of Pearson (1824–'29). The younger Bond (George Phillips Bond, born 1825; Harvard College, 1844; director of the Harvard College Observatory, 1859–'65) and his contemporaries, on the other hand, were firmly grounded in the German methods, then, as now, the most philosophical and thorough.

It was not until 1850, or later, that it was indispensable for an American astronomer to read the German language and to make use of the memoirs of Bessel, Encke, and Struve and the text-books of Sawitsch and Brünnow.[1] This general acquaintance with the German language and methods came nearly a generation later in England. The traditions of Piazzi and Oriani came to America with the Jesuit Fathers of Georgetown College (1844), of whom Secchi and Sestini are the best known.

The dates of the foundation of a few observatories of the United States may be set down here. Those utilized for the observation of the transit of Venus in 1769 were temporary stations merely. The first college observatory was that of Chapel Hill, North Carolina (1831); Williams College followed (1836); Hudson Observatory (Ohio) (1838); the Philadelphia High School (1840); the Dana

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[1] Dr. Bowditch learned to read German in 1818, at the age of forty-five.

House Observatory of Harvard College (1840); West Point (1841); the United States Naval Observatory (1844); the Georgetown College Observatory (1844); the Cincinnati Observatory (1845); the new observatory of Harvard College (1846); the private observatory of Dr. Lewis M. Rutherfurd in New York city (1848); the observatory at Ann Arbor (1854); the Dudley Observatory at Albany (1856); and that of Hamilton College (1856).

These dates and the summary history just given will serve to indicate the situation of astronomy in the United States during the first half of the present century. A little attention to the dates will enable the reader to place an individual or an institution on its proper background. It must constantly be kept in mind that the whole country was very young, and that public interest in astronomical matters was neither educated nor very general. The data here set down will have a distinct value as a contribution to the history of astronomy in America. The developments of later years have been so amazing that we forget that the first working observatories were founded so late as 1845.

American science is scarcely more than half a century old. The day will soon come—it is now here—when we shall look back with wonder and gratitude to ask who were the men who laid the wide and deep foundations which already maintain so noble an edifice.

STELLAR PARALLAX

BY

Sir JOHN FREDERICK WILLIAM HERSCHEL

(Born 1792; died 1871)

STELLAR PARALLAX[1]

GENTLEMEN: The report of the council has placed before you so ample a view of the state of the society, of its labours during the last year, of the accessions of its members, and of the many and severe losses it has had to deplore, that little is left for me to add, except my congratulations on its continued and increasing prosperity. It would be inexpressibly gratifying to me if I could persuade myself that my own exertions in its chair had contributed, even in a small degree, to that prosperity; but, alas! I have felt only too sensibly how very feebly and inefficiently, especially during the last year, owing to a variety of causes, but chiefly to residence at a distance from London, I have been able to fill that most honourable office.

The immediate object of my now addressing you, gentlemen, is to declare the award by your council of the gold medal of this society to our eminent associate, M. Bessel, for his researches on the annual parallax of that remarkable double star 61 Cygni—researches which it is the opinion of your council have gone so far to establish the existence and to measure the quantity of a periodical fluctuation, annual in its period and identical in its law with parallax, as to leave no reasonable ground for doubt as to the reality of such fluctuation, as something different from mere instrumental or observational error: an inequality, in short, which, if it is not parallax, is so inseparably mixed up with

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[1] Address on presenting the gold medal of the Royal Astronomical Society to Prof. F. W. Bessel, Director of the Observatory of Königsberg. (From "Monthly Notices of the Royal Astronomical Society," 1841.)

that effect as to leave us without any criterion by which to distinguish them. Now, in such a case, parallax stands to us in the nature of a vera causa, and the rules of philosophizing will not justify us in referring the observed effect to an unknown and, so far as we can see, an inconceivable cause, when this is at hand ready to account for the whole effect.

I say in the nature of a vera causa, since each particular star must of necessity be of some parallax. Every real, existing material body must enjoy that indefeasible attribute of body—viz., definite place. Now, place is defined by direction and distance from a fixed point. Every body, therefore, which does exist, exists at a certain definite distance from us and at no other, either more or less. The distance of every individual body in the universe from us is, therefore, necessarily admitted to be finite.

But though the distance of each particular star be not in strictness infinite, it is yet a real and immense accession to our knowledge to have measured it in any one case. To accomplish this has been the object of every astronomer's highest aspirations ever since sidereal astronomy acquired any degree of precision. But hitherto it has been an object which, like the fleeting fires that dazzle and mislead the benighted wanderer, has seemed to suffer the semblance of an approach only to elude his seizure when apparently just within his grasp, continually hovering just beyond the limits of his distinct apprehension, and so leading him on in hopeless, endless, and exhausting pursuit.

The pursuit, however, though eager and laborious, has been far from unproductive, even in those stages where its immediate object has been baffled.

The fact of a periodical fluctuation of some kind in the apparent places of the stars was recognised by Flamsteed, and erroneously attributed to parallax. The nearer examination of this phenomenon with far more delicate instruments, infinitely greater refinement of methods, and clearer views of the geometrical relations of the subject, rewarded Bradley with his grand discoveries of aberration and nutation, and enabled him to restrict the amount of possible parallax of the stars observed by him within extremely narrow limits.

Bradley failed to detect any appreciable parallax, though he considered 1″ as an amount which would not have escaped his notice. And since his time this quantity has been assumed as a kind of conventional limit, which it might be expected to attain, but hardly to surpass. But this was rather because, in the best observations from Bradley's time forward, 1″ has been a tolerated error; a quantity for which observation and mechanism, joined to atmospheric fluctuations and uncertainties of reduction, could not be held rigidly accountable even in mean results, than from any reason in the nature of the case, or any distinct perception of its reality. If parallax were to be detected at all by observations of the absolute places of the stars, it could only emerge as a "residual phenomenon," after clearing away all the effects of the uranographical corrections as well as of refraction, when it would remain mixed up with whatever uncertainties might remain as to the coefficients of the former, with the casual irregularities of the latter, and with all the forms of instrumental and observational error. Now, these have hitherto proved sufficient, even in the observation of zenith stars, quite to overlay and conceal that minute quantity of which astronomers were in search.

It is not my intention, gentlemen, to enter minutely into the history of the attempts of various astronomers on this problem, whether by the discussion of observations of one star, or by the combination of those of pairs of stars opposite in right ascension; nor with the occasional gleams of apparent success which, however, have always proved illusory, which have attended these attempts. For such a history, and, indeed, for a complete and admirably drawn-up monograph of the whole subject, I must refer to a paper lately read to this society by Mr. Main, and which is now in process of publication in the forthcoming volume of our "Memoirs." In whatever reference I may have to make to the history of the subject, I must take this opportunity to acknowledge my obligations to the author of this paper, as well as for his exceedingly luminous exposition of the results of those more successful attempts on the problem by Henderson, Struve, and Bessel, which I shall now proceed more especially to consider.

It would be wrong, however, not to notice that the first indication of some degree of impression beginning to be made on the problem seems to be found in Struve's discussion of the differences of right ascension of circumpolar stars in 1819, 1820, and 1821. The only positive result, indeed, of these observations is that, in the case of twenty-seven stars examined, none has a parallax amounting to half a second. But below this there certainly do seem to be indications in the nature of a real parallax, which might at least suffice to raise the sinking hopes of astronomers and excite them to further efforts.

But the time arrived when the problem was to be attacked from a quarter offering the greater advantages, and exposed to few or none of those unmanageable sources of irregular error to which the determination of absolute places are liable. I mean by the measurement of the distances of such double stars as consist of individuals so different in magnitude as to authorize a belief of their being placed at very different distances from the eye; or, as Struve expresses it, optically and not physically double. This, in fact, was the original notion which led to the micrometrical measurements of double stars; but not only was anything like a fair trial of the method precluded by the imperfections of all the micrometers in use until recently, but the interesting phenomena of another kind, which began to unfold themselves in the progress of those measurements, led attention off altogether from this their original application, which thus lay dormant and neglected, until the capital modern improvements, both in the optical and mechanical parts of refracting telescopes, and the great precision which it was found practicable, by their aid, to attain in these delicate measurements, revived the idea of giving this method, what it never before had, a fair trial. The principle on which the determination of parallax by means of micrometrical observations of a double star turns, is extremely simple. If we conceive two stars very nearly in a line with the eye, but of which one is vastly more remote than the other, each, by the effect of parallax, will appear to describe annually a small ellipse about the mean place as its centre. These two ellipses, however, though similar in form, will differ in dimension, that described by the more remote star being comparatively much smaller; consequently, the apparent places being similarly situated in each, their apparent distance on the line joining these apparent places will both oscillate in angular position and fluctuate in length, thus giving rise to an annual relative alternate movement between the individuals both in position and distance, which is greater the greater the difference of the parallaxes.

Thus it is not the absolute parallax of either, but the differences of their parallaxes, which is effectively measured by this method—i. e., by repeating the measurements of their mutual distance at all times of the year. But, on the other hand, aberration, nutation, precession, and refraction act equally on both stars, or so very nearly so as to leave only an exceedingly small fraction of these corrections bearing on the results. And when the stars are very unequal in magnitude, there is a presumption that the difference of their parallaxes is very nearly equal to the whole parallax of the nearer one.

The selection of a star for observation involves many considerations. In that pitched on by M. Bessel (61 Cygni) the large star so designated is, in fact, a fine double star; nay, one that has been ascertained to be physically double. It is in every respect a highly remarkable star. The mutual distance of its individuals is great, being about 16¼″. Now, this being necessarily less than the axis of their mutual orbit, affords in itself a presumption that the star is a near one. And this presumption is increased by the unusually great proper motion of this binary system, which amounts to nearly 5″ per annum, and which has been made by Sir James South the subject of particular inquiry, and found to be not participated in by several small surrounding stars, which, therefore, are not physically connected with it. Moreover, the angular rotation of the two, one about the other, has been well ascertained.

Now, it fortunately happens that of these small surrounding stars there are two very advantageously situated for micrometrical comparison with either of the individuals of the binary star, or with the middle point between them. The one of these (*a*), at a distance of 7′ 42″, is situated nearly at right angles to the direction of the double star; the other (*b*), at a distance of 11′ 46″, nearly in that direction. Considering *a* and *b* as fixed points, then, and measuring at any instant of time their distances from *c*, the middle point of the double star, the situation of *c* relative to *a* and *b* is ascertained; and if this be done at every instant, the relative locus of *c*, or the curve described by it on the plane of the heavens with respect to the fixed base line *a b*, will become known.

Now, on the hypothesis of parallax, that locus ought to be an ellipse of one certain calculable eccentricity and no other, and its major and minor axes ought to hold with respect to the points, *a*, *b*, certain calculable positions and no other. Hence it follows that the distance *a c* and *b c* will each of them be subject to annual increase and diminution; and that, first, in a given and calculable ratio the one to the other; and, secondly, so that the maxima and minima of the one distance *a c* shall be nearly contemporaneous with the mean values of the other distance (*b c*), and vice versa.

Thus we have, in the first place, several particulars independent of mere numerical magnitudes; and, in the second place, several distinct relations a priori determined, to which those numerical values must conform, if it be true that any observed fluctuations in these distances (*a c*, *b c*) be really parallactic. So that if they be found in such conformity, and the above-mentioned maxima and minima do observe that interchangeable law above stated; and if, moreover, all due care be proved to have been taken to eliminate every instrumental source of annual fluctuation—there becomes accumulated a body of probability in favour of the resulting parallax which can not but impress every reasonable mind with a strong degree of belief and conviction.

Now, all these circumstances have been found by M. Bessel, in his discussion of the measures taken by him (which have been very carefully and rigorously examined by Mr. Main in the paper alluded to, as have also M. Bessel's formulæ and calculations, for in such matters nothing must remain unverified), to prevail in a very signal and satisfactory manner. Not one case of discordance, in so many independent particulars, has been found to subsist; and this, of itself, is high ground of probability. But we may go much further. Mr. Main has projected graphically the deviations of the distances *a c* and *b c* from their mean quantities (after clearing them of the effects of proper motion and of the minute differences of aberration, etc.). Taking the time for an abscissa, and laying down the deviations in the distances so cleared as ordinates, two curves are obtained, the one for the star *a*, the other for the star *b*. Each of these curves ought alternately to lie for half a year above and for half a year below its axis. It does so. Each of them ought to intersect its axis at those dates when the maximum and minimum of the other above and below the axis occurs. With only a slight degree of hesitation at one crossing, it does so. The points of intersection with the axis ought to occur at dates in like manner calculated a priori; and so they do within very negligible limits of error. And, lastly, the general forms, magnitudes, and fixtures of the curves ought to be identical with those of curves similarly projected by calculation on an assumed resulting parallactic coefficient. This is the final and severe test; Mr. Main has applied it, and the results have been placed before you: oculis subjecta fidelibus. If all this does not carry conviction along with it, it seems difficult to say what ought to do so.

The only thing that can possibly be cavilled at is the shortness of the period embraced by the observations—viz., from August, 1837, to the end of March, 1840. But this interval admits of five intersections of each curve with its axis; of two maxima and two minima in its excursions on either side; and of ample room for trying its agreement in general form with the true parallactic curves. Under such circumstances it is quite out of the question to declare the whole phenomenon an accident or an illusion. Something has assuredly been discovered, and if that something be not parallax, we are altogether at fault, and know not what other cause to ascribe it to.

The instrument with which Bessel made these most remarkable observations is a heliometer of large dimensions, and with an exquisite object-glass by Fraunhofer. I well remember to have seen this object-glass at Münich before it was cut, and to have been not a little amazed at the boldness of the maker who would devote a glass, which at that time would have been considered in England almost invaluable, to so hazardous an operation. Little did I then imagine the noble purpose it was destined to accomplish. By the nature and construction of this instrument, especially when driven by clockwork, almost every conceivable error which can effect a micrometrical measure is destroyed, when properly used; and the precautions taken by M. Bessel in its use have been such as might be expected from his consummate skill. The only possible apparent opening for an annually fluctuating error seems to be in the correction for temperature of its scale. But this correction has been ascertained by M. Bessel by direct observation, in hot and cold seasons, and applied. Nor could this cause destroy the evidence arising from the simultaneous observation of the two companion stars, since a wrong correction for temperature would affect both their distances proportionally, leaving the apparent parallactic movement still unaccounted for.

The resulting parallax is an extremely minute quantity, only $\frac{31}{100}$ of a second, which would place the star in question at a distance from us of nearly 670,000 times that of the sun![2] Such is the universe in which we exist, and which we have at length found the means to subject to measurement, at least in one of its members, probably nearer to us than the rest.

It becomes necessary for me now to refer to two series of researches on this important subject, which have been held by your council to merit very high and honourable mention; though neither of them, separately, for reasons which I shall state, would have been considered as carrying that weight of probability in favour of its conclusions, which would justify any immediate decision of the nature which they have come to in the case of M. Bessel's. I allude to M. Struve's inquiries, by the method of micrometric measures, into the parallax of α Lyræ, and to Mr. Henderson's, by that of meridian observations, on the parallax of α Centauri.

α Lyræ is accompanied by a very minute star, at the distance of about 43″. That this star is unconnected with α by any physical relation is clear from the fact, ascertained by Sir James South and myself, that it does not participate in the proper motion of the large star. The mutual angular distance of these stars has been made by M. Struve the subject of a very extensive series of micrometric measures with the celebrated Dorpat achromatic, bearing this object steadily in view, and working it out to a conclusion of the very same kind, and though materially inferior in the degree and nature of its evidence to that of Bessel, yet certainly entitled to high consideration. M. Struve's observations on this star, and for this purpose, extend from November, 1835, to August, 1838, and are distributed over sixty nights, averaging twenty per annum; and from their combination, according to the principle of probabilities, he concludes a parallax of 0.261″. Mr. Main has subjected these observations to an analysis and graphical projection pre-

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[2] The orbit described by the two stars of 61 Cygni about each other will, therefore, be about fifty times the diameter of the earth's about the sun, or two and a half times that of Uranus.

cisely similar in principle to those I have explained in the case of 61 Cygni. The curves so projected have been subjected to your inspection, and that inspection certainly does leave a very strong impression of a real and tolerably well-ascertained parallax having been detected in this star. But at the same time an impression no less decided, owing to irregularities in the march of the curve, when compared with the true parallactic curve, is created: that the errors of observation are far from being eliminated; that, on the contrary, they bear such a proportion to the parallax itself as to leave room for some degree of hesitation, and to justify an appeal to a longer series of observations, and to concurrent evidence from other quarters, before declaring any positive opinion. The evidence of this kind, in short, is not equal to that afforded by the similar projection of Bessel's observations of either of his two comparison stars. And to this it must be added that only one star of comparison existing in the line of *a* Lyræ, the possible effect of temperature and annual instrumental variation is not eliminated from the result in the way in which it is from the measures of 61 Cygni; while all that great mutual support which the observations of parallaxes of the two comparison stars afford each other in the latter case is altogether wanting in the former. These considerations, without any underestimation of the great importance and value of M. Struve's researches, yet formed essential drawbacks on the immediate admission of his results.

In a word, I conceive the question of discovery as between these illustrious but most generous and amicable rivals may be thus fairly stated. M. Struve's meridian observations in 1819–'21 seem to have made the first impression on the general problem, but too slight to authorize more than a hope that it would yield at no distant day. His micrometric measures of *a* Lyræ commenced more than a year earlier, and have extended altogether over a longer period than M. Bessel's of 61 Cygni. From their commencement they afford indications of parallax, and these indications accumulating with time have amounted to a high degree of probability, and rendered the supposition of parallax more admissible than that of instrumental or casual errors producing the same influence on the measures. On the other hand, M. Bessel's measures, commencing a year later and continued on the whole through somewhat less time, have exhibited a compact and consistent body of evidence drawn from two distinct systems of measures mutually supporting each other, and so steadily bearing on their object as to leave no more reasonable doubt of the truth than in the case of many things which we look upon as, humanly speaking, certain. And this conviction, once obtained, reacts on our belief in the other results, and induces us to receive and admit it on the evidence adduced for it; which, without such conviction so obtained, we might hesitate to do until after longer corroboration of the same kind.

The other series of observations to which I must now call your attention are those of Mr. Henderson, made at the Cape of Good Hope, on the great star *α* Centauri, the third star in brightness which the heavens offer to our view. It is a magnificent double star consisting of two individuals, the one of a high and somewhat brownish orange, the other of a fine yellow colour, and each of which I consider fairly entitled to be classed in the first magnitude.[3] Their distance is at present about 15″ asunder, but it is rapidly diminishing, and in no great lapse of time they will probably occult one another, their angular motion being comparatively small. Their apparent distance was formerly much greater—how much we can not say for want of observations, but probably the major axis of their mutual orbit is little short of a minute of space. They, therefore, afford strong indications of being very near our system. Add to which their proper motion is very considerable, and participated in by both, which proves their connection as a binary system; and an additional presumption in favour

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[3] I have seen both their images projected on a screen of three thicknesses of stout paper, the eye being on the opposite side of the screen from that on which the images were depicted.

of their proximity may be drawn from their situation in what, from general aspect, I gather to be the nearest region of the Milky Way, among an immensity of large stars.

Mr. Henderson observed these stars with great care both in right ascension and declination with the very fine transit, and (in spite of certain grievous defects in the axis) the otherwise really good and finely divided mural circle of the Royal Observatory in that colony. Since his return to England he has reduced these observations with a view to parallax, and the result is the apparent existence of that element to what, after what has been said, we must now call the great and conspicuous amount of a full second. Mr. Main, to whom I am so largely indebted for allowing me to draw so freely on his labours, has also discussed these results, and comes to the conclusion that (as might, perhaps, be expected) the right ascension observations afford a trace, but an equivocal one, of parallax, but that in declination (I use his words) " the law of parallax is followed remarkably well. There is scarcely an exception to the proper change of sign, according to the change of sign of the coefficients of parallax. This is quite as much as can reasonably be expected in a series of individual results obtained from any meridional instrument for observing zenith distances. We can not expect to find the periodical function regularly exhibited by the differences. On the whole, therefore, we should say that, in addition to the claims of *a* Centauri on our attention with relation to its parallax, arising from its forming a binary system, its great proper motion, and its brightness, it derives now much additional importance, in this point of view, from the investigation of Mr. Henderson. This we are at least entitled to assume until some distinct reason, independent of parallax, shall have been assigned for the changes in the declinations. Such I do not consider impossible, having before my eyes the results which Dr. Brinkley derived, in the cases of certain stars, from the Dublin circle. For the present it must be considered that the star well deserves a rigorous examination by all the methods which the author himself has so well pointed out; and that, in the event of a parallax at all comparable with that assigned by Mr. Henderson being found, he will deserve the merit of its first discovery, and the warmest thanks of astronomers, as an extender of the knowledge which we possess of our connection with the sidereal system."

With this view of Mr. Henderson's labours I fully agree, and await with highly excited interest the result of Mr. Maclear's larger and complete series of observations on this star, both with the old circle and that more perfect one with which the munificence of Government has recently supplied the observatory. Should a different eye and a different circle continue to give the same results, we must, of course, acquiesce in the conclusion, and the distinct and entire merit of the first discovery of the parallax of a fixed star will rest indisputably with Mr. Henderson. At present, however, we should not be justified in so far anticipating a decision which time alone can stamp with the seal of absolute authenticity.

Gentlemen of the Astronomical Society, I congratulate you and myself that we have lived to see the great and hitherto impassable barrier to our excursions into the sidereal universe—that barrier against which we have chafed so long and so vainly (æstuantes angusto limite mundi)—almost simultaneously overleaped at three different points. It is the greatest and most glorious triumph which practical astronomy has ever witnessed. Perhaps I ought not to speak so strongly; perhaps I should hold some reserve in favour of the bare possibility that it may be all an illusion, and that further researches, as they have repeatedly before, so may now, fail to substantiate this noble result. But I confess myself unequal to such prudence under such excitement. Let us rather accept the joyful omens of the time, and trust that, as the barrier has begun to yield, it will speedily be effectually prostrated. Such results are among the fairest flowers of civilization. They justify the vast expenditure of time and talent which have led up to them; they justify the language which men of science hold, or ought to hold, when they appeal to the governments of their respective countries for the liberal devotion of the national means in furtherance of the great objects they propose to accomplish. They enable them not only to hold out, but to redeem their promises, when they profess themselves productive labourers in a higher and richer field than that of mere material and physical advantages. It is then when they become (if I may venture on such a figure without irreverence) the messengers from Heaven to earth of such stupendous announcements as must strike every one who hears them with almost awful admiration, that they may claim to be listened to when they repeat in every variety of urgent instance that these are not the last of such announcements which they shall have to communicate; that there are yet behind, to search out and declare, not only secrets of Nature which shall increase the wealth or power of man, but truths which shall ennoble the age and country in which they are divulged, and, by dilating the intellect, react on the moral character of mankind. Some truths are things quite as worthy of struggles and sacrifices as many of the objects for which nations contend, and exhaust their physical and moral energies and resources. They are gems of real and durable glory in the diadems of princes, and conquests which, while they leave no tears behind them, continue forever unalienable.

It must be needless for me to express a hope that these researches will be followed up. Already we have to congratulate astronomy on the resolution taken by one of our great academic institutions to furnish its observatory with a heliometer of the same description as Bessel's; nor can we fear but that the research will speedily be extended to other stars, offering varieties of magnitude and other indications to draw attention to them.

On the whole, then, the award of our medal, which the council have agreed on, seems to me, under the circumstances, fully justified. I will now request the foreign secretary to convey it to our distinguished associate; and in so doing I will add our hope that, in the painful and distressing visitation with which it has pleased Providence recently to try him, he may find occasion to withdraw his mind awhile from that melancholy contemplation to receive with satisfaction such a tribute to this his last and perhaps his greatest achievement, accompanied as it is by the truest regard for his private worth and the most respectful sympathy for his present distress.

"If we ask to what end magnificent establishments are maintained by states and sovereigns, furnished with masterpieces of art, and placed under the direction of men of first-rate talent and high-minded enthusiasm, sought out for those qualities among the foremost in the ranks of science, if we demand cui bono? for what good a Bradley has toiled, or a Maskelyne or a Piazzi has worn out his venerable age in watching, the answer is, not to settle mere speculative points in the doctrine of the universe; not to cater for the pride of man by refined inquiries into the remoter mysteries of Nature; not to trace the path of our system through space, or its history through past and future eternities. These, indeed, are noble ends, and which I am far from any thought of depreciating; the mind swells in their contemplation, and attains in their pursuit an expansion and a hardihood which fit it for the boldest enterprise. But the direct practical utility of such labours is fully worthy of their speculative grandeur. The stars are the landmarks of the universe; and, amid the endless and complicated fluctuations of our system, seem placed by its Creator as guides and records, not merely to elevate our minds by the contemplation of what is vast, but to teach us to direct our actions by reference to what is immutable in his works. It is, indeed, hardly possible to over-appreciate their value in this point of view. Every well-determined star, from the moment its place is registered, becomes to the astronomer, the geographer, the navigator, the surveyor, a point of departure which can never deceive or fail him, the same forever and in all places, of a delicacy so extreme as to be a test for every instrument invented by man, yet equally adapted for the most ordinary purposes; as available for regulating a town clock as for conducting a navy to the Indies; as effective for mapping down the intricacies of a petty barony as for adjusting the boundaries of transatlantic empires. When once its place has been thoroughly ascertained and carefully recorded, the brazen circle with which that useful work was done may moulder, the marble pillar may totter on its base, and the astronomer himself survive only in the gratitude of posterity; but the record remains, and transfuses all its own exactness into every determination which takes it for a groundwork, giving to inferior instruments—nay, even to temporary contrivances, and to the observations of a few weeks or days—all the precision attained originally at the cost of so much time, labour, and expense."

THE HISTORY OF THE TELESCOPE

BY

CHARLES S. HASTINGS

THE HISTORY OF THE TELESCOPE[1]

THERE is no instrument which has done so much to widen the scope of human knowledge, to extend our notions of the universe, and to stimulate intellectual activity as has the telescope, unless the microscope be regarded as a successful rival. But even admitting a parity in scientific importance, the former instrument is incomparably more interesting in its history, in the same degree that its history is more simple and more comprehensible. To trace its development from a curious toy in the hands of its discoverer, for we shall see that this term is more appropriate than inventor, to the middle of this century, is to be brought into contact with most of the great philosophers, from the time of the Renaissance, who have achieved greatness in physical science, Galileo, Torricelli, Huygens, Cassini, Newton, Halley, Kepler, Euler, Calaiault, the Herschels, father and son, Fraunhofer, Gauss—from only a portion of the list of great names. Its growth toward perfection has constantly carried with it increased precision in the applied sciences of navigation and of all branches of engineering. It would be easy to show that even pure mathematics would be in a far less forward state had there been no problems of astronomy and physics which were first suggested by the employment of the telescope. It is to this history that I venture to invite your attention this evening. I purpose to review succinctly the origin and development of this potent aid in the study of

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[1] Address delivered at the dedication of the Goodsell Observatory of Carleton College, Northfield, Minn., June 11, 1891. (From the "Sidereal Messenger," August, 1891, vol. x, pp. 335-354.)

Nature, to name some of the more important achievements depending upon it, and to trace its gradual improvement to the magnificent and complicated instrument which constitutes the modern equatorial. After this sketch I shall try to give an idea of the imperfections which the conscientious artisan has to contend with in attaining perfection, and to make clear the methods which have been employed in reducing these imperfections in the noble instrument now erected at this institution,[2] and explain why its possessors are so hopeful of gratifying success.

Galileo learned, in 1609, while visiting Venice, that a marvellous instrument had been invented the preceding year in Holland, which would enable an observer to see a distant object with the same distinctness as if it were only at a small fraction of its real distance. It required but little time for the greatest physicist of his age to master the problem thus suggested to his mind, and after his return to Padua, where he held the position of professor of mathematics in the famous university of that city, he set himself earnestly to work making telescopes. Such was his success that in August of the same year he sent to the Venetian senate a more perfect instrument than they had been able to procure from Holland; and in January of the next year, by means of a telescope magnifying thirty times, he discovered the four satellites of Jupiter. This brilliant discovery was followed by that of the mountains in the moon; of the variable phases of Venus, which established the Copernican theory of the solar system as incontestable, and of the true nature of the Milky Way, together with many others of less philosophical importance. Though Galileo did not change the character of the telescope as it was known to its discoverer in Holland, he made it much more perfect; and above all, made the first and most fertile application of the instrument to increase the bounds of human knowledge, so that it is inevitable that his name should be indissolubly connected with the instrument. Thus the form which he used is to this day known as the Galilean telescope.

[2] Carleton College.

Considering the enormous interest excited throughout intellectual Europe by the invention of the telescope, it seems surprising that its early history is so confused. Less than two years after it was first heard of, a discovery, perhaps the greatest of a thousand years in the domain of natural philosophy, had been made by its means. Notwithstanding these facts, the three contemporary, or nearly contemporary, investigators assign the honour to three different persons, and if we should write out the names of all those to whom more modern writers have attributed the invention, the list would be a long one. The surprise will not be boundless, however, if we consider the task before a historian in the next century who undertakes to justly apportion the honour of the invention of the telephone among its numerous claimants. The analogy, though suggested in the obvious fact that the telephone is to hearing just what the telescope is to sight, may be made much closer if we could imagine the future historian deprived of all but verbal description, that contemporary diagrams and models were wholly wanting. Under such conditions it is difficult to believe that the historian would easily escape antedating the discovery of the telephone proper on account of descriptions, generally imperfect, of the acoustic telephone. But this would fairly represent the condition of the material at the command of an investigator of the present day into a question of science of the early part of the seventeenth century. No wonder, then, that the invention has been attributed to Archimedes, to Roger Bacon, to Porta, and to many others who have written on optics; but to find the name of Satan in the list is certainly surprising. Still we read that a very learned man of the seventeenth century, named Arias Montanus, finds in the fourth chapter of Matthew, eighth verse, evidence that Satan possessed, and probably invented, a telescope; otherwise, how could he have " shown him all the kingdoms of the world and the glory of them "? [3] It seems

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[3] The history of the telescope is admirably treated in Poggendorff's " Geschichte der Physik," from which the statements above are taken.

to be well established now, however, that Franz Lippershey, or Lippersheim, a spectacle-maker at Middleberg, was the real inventor of the telescope, and that Galileo's first telescope, avowedly suggested by news of the Hollander's achievement, was an independent invention.

That this discovery was really an accident we may be quite sure, for not only was there no developed theory of optics at that time, but even the law of refraction, which lies at the basis of such theory, was quite unknown. So, too, it seems to me quite certain that Galileo's invention must have been empirical and guided by somewhat precise information, such as that the instrument consisted essentially of two lenses, of which one was a magnifying and the other a diminishing lens. At least, that Galileo's telescope was like that of the Hollander; that, theoretically considered, it is not so simple as that made of two magnifying lenses, as is evinced by the fact that Kepler, the first philosopher to establish an approximate theory of optical instruments, only two years later invented the latter and prevailing form; and finaly, that Galileo published no contributions to the theory of optics, seem quite sufficient reasons for such a belief. But, in any case, Galileo's merit is in no wise lessened by having failed to do what could not be done at that time, and the value of his discoveries in emancipating men's minds from authority in matters of pure reason is incalculable.

No other discoveries of great moment were made until over a generation after Galileo proved the existence of spots on the sun in 1611. This cessation of activity was doubtless owing to the difficulty of securing telescopes of greater efficiency than that possessed by Galileo, and which he would hardly have left until its powers of discovery had been fully exhausted in his own hands. By the middle of the seventeenth century, however, several makers of lenses had so far improved the methods of grinding and polishing, that telescopes notably superior in power to that of Galileo were procurable. Of these Torricelli, Divini, and Campani, all Italians; Auzout, who constructed a telescope six hundred feet in length, though no means was ever found for directing such an enormous instrument toward the heavens; but above all, Huygens—have won distinction as telescope makers. The last-named philosopher discovered, by means of a telescope of his construction, the largest satellite of Saturn in 1655, thus adding a fifth member to the list of planetary bodies unknown to the ancients. But his most important astronomical discovery, made also in 1655, was the nature of the rings of Saturn. This object had greatly puzzled Galileo, to whose small telescope the planet appeared to consist of a larger sphere flanked on either side by a smaller one; but when in the course of the orbital motion of Saturn the rings entirely disappeared he was wholly unable to suggest an explanation. This planet had thus presented a remarkable problem to all astronomical observers for more than forty years, and the records of the efforts to solve it during that interval afford us a most excellent means of judging the progress in practical optics. Huygens announced these discoveries early in 1656, but that relating to the ring was given in the form of an anagram, the solution of which was first published in 1659. This discovery was contested in Italy by Divini, but was finally confirmed by members of the Florentine Academy with one of Divini's own telescopes.

A few years later the famous astronomer Cassini, having come to Paris from Italy as royal astronomer, commenced a series of brilliant discoveries with telescopes made by Campani, of Rome. With these, varying in length from 35 feet to 136 feet, he discovered four satellites to Saturn in addition to the one discovered by Huygens. The whole number was increased by Herschel's discovery of two smaller ones in 1789, a hundred and five years after Cassini's last discovery, and again by Bond's discovery of an eighth in 1848. The Saturnian system, to which the telescope has doubtless been directed more frequently than to anything else, thus serves as a record of the successive improvements of the telescope. Highly significant is the fact that the discoveries of the eighteenth century were made with a reflecting telescope, the others all being with refracting instruments.

Cassini's discovery in 1684 of the two satellites now known as Tethys and Dione, was not accepted as conclusive until long afterward, when Pound, 1718, with a telescope 123 feet in length, which Huygens had made and presented to the Royal Society, saw all five. This particular instrument is of especial interest, because it is the only one of those of the last half of the seventeenth century which has been carefully compared with modern instruments. Moreover, it is without doubt quite equal in merit to any of that period. But we find that, although it had a diameter of six inches, its performance was hardly better than that of a perfect modern telescope of four inches in diameter, and, perhaps, four feet and a half in length, while in regard to convenience in use the modern compact instrument is incomparably superior.

Another notable discovery of this period was that of the duplicity of the rings of Saturn by the Ball brothers in 1665, though its independent discovery by Cassini ten years later first attracted the attention of astronomers. The earlier discovery was made by means of a telescope thirty-eight feet long, which seems to have been of English manufacture. We must regard Cassini's discovery of the third and fourth satellites of Saturn, however, as marking the very farthest reach of the old form of telescope; a century was to elapse and an entirely new form of telescope was to be developed before another considerable addition to our knowledge of the aspect of the heavenly bodies was to be made. It is true larger telescopes were made, and Huygens invented a means by which they could be used without tubes, but notwithstanding this improvement they proved so cumbersome as to be impracticable.

The older opticians had found that if they attempted to increase the diameter of a telescope they were obliged to increase its length in a much more rapid ratio to secure distinct vision. The reason of this was not clearly understood, but it was supposed to be owing to the fact that a wave front, changed in curvature by passing through a spherical surface, is no longer strictly spherical. This deviation in shape of the refracted wave from a true sphere is called spherical aberration. When the refracting surfaces are large and of considerable curvature, this soon becomes very serious, but by using small curvature, which, in a telescope, obviously corresponds to great length, the effects of the error can be made insensible. Newton's discovery of the composite nature of light and of the phenomenon of dispersion enabled him to explain the true cause of indistinctness in short telescopes—namely, that the refraction by the objective varies for different colours; consequently, if the ocular is placed for one particular colour, it will not be in the right position for any of the others, whence the image of a star or planet will seem to be surrounded by a fringe of coloured light. Newton found this source of indistinctness in the image, which is now known as chromatic aberration, many hundred times as serious as the spherical aberrations. As he was persuaded by his experiments that this obstacle to further improvement in the refracting telescope was insuperable, he turned his attention to a form of telescope which had been suggested a number of years earlier, in which the image was to be formed by reflection from a concave mirror, and constructed a small one with his own hands which is still in the possession of the Royal Society. This little instrument seems to have been of about the same power as Galileo's instrument with which he discovered the satellites of Jupiter, but it was hardly more than six inches in length.

Since that time the reflecting telescope has had a remarkable history of development in the hands of a number of most skilful mechanicians, who have also for the most part been distinguished by their discoveries in physical astronomy; we may therefore advantageously depart from the chronological treatment and follow the history of this type of instrument. This course is the more natural because we may probably regard the supremacy of the reflector (undisputed a century ago) as passed away forever.

Even after Newton's invention was made public, little was done toward the improvement of telescopes for half a century, until Hadley presented a reflector of his own construction to the Royal Society in 1723, which was found to be equal to the Huygens refractor of 123 feet in length. From this time we may date the beginning of the superiority of reflectors. A few years later Short commenced his career as a practical optician, and for thirty years he was unapproached in the excellence of his instruments. During this time many telescopes, more powerful than the best of the previous century and infinitely more convenient in use, had been made and scattered throughout Europe, but during this period also there was a singluar dearth of telescopic discovery. Perhaps men thought that the harvest had already been gathered; or, perhaps, we may find the explanation in that the great cost of telescopes so restricted their use that the impulse to discovery by their means was confined to a very small class. In view of the remarkable manner in which the standstill in this branch of science was finally followed by a brilliant period of discovery, rivalled alone by that of Galileo, we might well regard the latter cause as the chief one.

William Herschel was born in 1738, in Hanover. In 1755 he left his native country, and going to England, secured a position as organist in Octagon Chapel, Bath, where we find him in 1766. Here he became so profoundly interested in the views of the heavens which a borrowed telescope of moderate power yielded, that he tried to purchase one in London. The cost of a satisfactory instrument proving beyond his command, he determined to construct one with his own hands. Thus he entered upon a course which was to reflect honour upon himself, his country, and his age, and which was to add more to physical astronomy than any other one man has added before or since. With almost inconceivable industry and perseverance he cast, ground, and polished more than four hundred mirrors for telescopes, varying in diameter from six to forty-eight inches. This in itself would imply a busy life in any artisan, but when we remember that all this was merely subsidiary to his main work of astronomical discovery, we can not withhold our admiration.

Fortunately for science as well as for himself, he made early in his career a discovery of the very first importance, which attracted the attention of all Christendom. On the night of March 13, 1781, Herschel was examining small stars in the constellation of Gemini with one of his telescopes of a little more than six inches in diameter, when he perceived one that appeared "visibly larger than the rest." This proved to be a new world, now known as Uranus. The discovery led in the following year to his appointment as astronomer to the king, George III, with a salary sufficient to enable him to devote his whole time to astronomy.

One of the fruits of this increased leisure was the construction of a telescope far more powerful than had been dreamed of by his predecessors—namely, a telescope four feet in diameter and forty feet in length. Commenced in 1785, Herschel dated its completion as August 28, 1789, when he discovered by its means a sixth satellite of Saturn, and, less than a month later, a seventh, even closer to the planet and smaller than the sixth. We may regard this achievement as marking the limit of progress in the reflecting telescope, for, although at least one as large is now in use, and one even half as large again has been constructed, it is more than doubtful whether they were ever as perfect as Herschel's at its best.

There has been one improvement, however, in the reflecting telescope since the time of Herschel which ought not to be left unnoticed here—namely, that of replacing the heavy metal mirror by one of glass, made even more highly reflective than the old mirrors by a thin coating of silver deposited by chemical methods upon the polished glass. The great advantage of this modern form of reflector lies not so much in the greater lightness and rigidity of the material as in that the surface when tarnished can be renewed by the simple process of replacing the old silver film by a new one; whereas in the metal reflectors a tarnished surface required a repetition of the most difficult and critical portion of the whole process of construction. The construction is also so comparatively simple that an efficient reflector is far less expensive than are refracting telescopes of like power, so that this may be regarded as particularly the amateur's telescope. On the other hand, such telescopes are, like their predecessors, extremely inconstant, and they require much more careful attention to keep them in working order. It is for these reasons, doubtless, that silver-on-glass reflectors have done so little for the advancement of astronomical discovery. In astronomical photography, however, they promise to do much; and, indeed, at the present date by far the best photographs we have of any nebulæ have been made by Mr. Common's magnificent reflector of three feet in diameter, and by the twenty-inch reflector of Mr. Roberts.

We must go back now to a quarter of a century before Herschel discovered the new planet, to the very year, indeed, when that great astronomer first set foot on English soil, in order to trace the history of another form of telescope which has remained unrivalled for the last half century in the more difficult fields of astronomical research, and which to-day finds its most perfect development in the instruments at Mount Hamilton, at Pultowa, at Vienna, and at Washington.

Newton had declared that, as a result from his experiments, separation of white light into its constituent colours was an inevitable accompaniment of deviation by refraction, and consequently the shortening of the unwieldy refractors was impracticable. The correctness of the experiments remained unquestioned for nearly a century; but a famous German mathematician, Euler, did question his conclusion. His argument was that since the eye does produce colourless images of white objects it might be possible by the proper selection of curves to so combine lenses of glass and of water as to produce a telescope free from the colour defect. Although Euler's premise was an error, since the eye is not free from dispersion, his efforts had the effect of leading to much more critical study of the phenomena involved. In this John Dolland, an English optician, met with brilliant success. Repeating an experiment of Newton's with a prism of water opposed by a prism of glass he found that deviation of light could be produced without accompanying dispersion into prismatic colours. More than this, he found that the two varieties of glass, then as now common in England—crown or common window glass, and flint glass, which is characterized by the presence of a greater or less quantity of lead oxide—possessed very different powers in respect to dispersion; thus, of two prisms of these two varieties of glass which would deflect the light by the same angle, that made of flint glass would form a spectrum nearly twice as long as the other; hence, if a prism of crown glass deflecting a transmitted beam of light, say ten degrees, were combined with one of flint glass which would deflect the beam of light five degrees in the opposite direction, there would remain a deflection of five degrees without division into colour. It also follows that a positive lens of crown combined with a negative lens of flint of half the power would yield a colourless image. Such combinations of two different substances are called achromatic systems. It is a singular fact, worth noting in passing, that more than twenty years before Dolland's success, Mr. Chester More Hall had invented and made achromatic telescopes, but this remained unknown to the world of science until after Dolland's telescopes became famous.

For a long time this ingenious invention remained fruitless for astronomical discovery (though they were early applied to meridian instruments), on account of the impossibility of securing sufficiently large and perfect pieces of glass, more particularly of flint glass. Not until after the beginning of this century was any real advance in this branch of the arts exhibited. Even then success appeared, not in England or France, where most strenuous efforts had been made to improve the quality of optical glass, but in Switzerland. There a humble mechanic, a watchmaker named Guinaud, spent many years in efforts, long unfruitful, to make large pieces of optical glass. What degree of success he attained there during twenty years of experiment we do not know, though from the fact that during that period good achromatic telescopes of more than five inches in diameter were unknown, we must conclude that his success was limited. In 1805 he joined the optical establishment of Fraunhofer and Utzschneiden in Munich. Here he remained nine years, and with the increased means at his disposal, and the aid of Fraunhofer, he perfected his methods so far that the production of large disks of homogeneous glass became only a matter of time and cost—that is to say, all of the large pieces of optical glass which have since been produced, whether in Germany, France, or England, have been made by direct heirs of the practical secrets of this Swiss watchmaker.

Fraunhofer was a genius of a high order. Although he died at the early age of thirty-nine, he had not only brought the achromatic telescope to a degree of optical perfection which made it a rival of the most powerful of the reflector type, and so far improved its method of mounting that his system has replaced all others; but he also made some capital discoveries in the domain of physical optics. His great achievement was the construction of an achromatic telescope 9.6 inches in diameter, with which the elder Struve made at Dorpat his remarkable series of discoveries and measurements of double stars. The character of Struve's work demonstrates the excellence of the telescope, and shows us that it is to be ranked as the equal of all but the very best of its predecessors. Indeed, it may fairly be concluded that not more than one or two telescopes, and those made and used by Herschel, had ever been of greater power, while in convenience for use the new refractor was vastly superior.

For a long time Fraunhofer and his successors, Merz and Mahler, from whom the great telescopes of Pultowa and of the Harvard Observatory were procured, remained unrivalled in this field of optics. But they have been followed by a number of skilful constructors whose products have, since the middle of the century, been scattered all over the world. In Germany, Steinheil and Schröder; in France, Canchois, Martin, and the Henry brothers; in England, Cook and Grubb; and in this country the Clarks and Brashear, have each produced one or more great telescopes which has rendered his name familiar to all readers of astronomical history. Of these the Clarks, father and son, have beyond a doubt won the first place, whether determined by the character of the discoveries made by means of their instruments or by the fact that the two most powerful telescopes in existence were made by them—namely, the new refractor of thirty inches in diameter at Pultowa, and the great refractor of three feet diameter of the Lick Observatory in California. The most notable discoveries made with their telescopes are the satellites of Mars and the companion to Sirius; but besides these there is a long list of double stars of the most difficult character discovered by the makers themselves, by Dawes in England, by Burnham in our own country, and by a number of other observers.

We ought not to terminate our review of the development of the telescope without a reference to the parallel development of the mounting of great telescopes. Indeed, did this not lead us too far from the immediate aim in view, we might find a great deal of interest and be brought into agreeable contact with some of the cleverest mechanicians and engineers of two centuries by tracing its course. We should meet with Huygens, as the inventor of the aërial telescope, and perhaps consider the claims of his contemporary, Robert Hook, as a rival inventor, for we may be sure that nothing which brings us to a study of that curious and able philosopher would fail to possess interest. We should find Herschel confronted with the problem as to how he should use his great forty-foot telescope, and the study of his solution would guide us in valuing the results of the subsequent efforts of Lassel and Rosse. The same line of study would bring us to Grubb's clever and interesting equatorial mounting of that anachronism, the four-foot Melbourne reflector. But we should find nothing of very notable interest in the mounting of refractors, after the time of Huygens and Hook, until Fraunhofer invented a type of mounting for the famous Dorpat equatorial, which still remains in its essential features as the type in universal use. With the increase in size of the telescopes to be directed toward the heavens, however, the number and complexity of the mechanical problems to be solved has been vastly increased, so that they have taxed the best powers of some of the ablest mechanicians. The Repsolds, of Germany, and Sir Howard Grubb, of Dublin, have specially distinguished themselves in this field of activity. But it seems to me that none have shown greater fertility of resources, greater skill in the solution of every problem affecting the comfort and efficacy of the observer, and greater taste, combined with accurate workmanship, than have the celebrated firm which has mounted the telescope at Mount Hamilton and that at Carleton College.

We come now to a consideration of the present state of the art of lens-making. We ask why such a very large proportion of the telescopes in existence are bad; why there was a time, brief it is true, during which the glass-maker was certainly in advance of the demands of telescope-makers; and why, finally, the first of the great modern objectives was in the hands of the most skilful optician in Great Britain for seven years, and even then this maker asserted that it was incomplete.

These questions can not be answered in a word, but we can, at least, gain much in perspicuity by recognising that the reasons are of two distinct kinds—namely, purely technical and theoretical; and by regarding them briefly in succession.

The art of lens-making can be certainly traced back to the thirteenth century, though the methods at a much later day than that were so rude that, as we have seen, Galileo had the utmost difficulty in making a lens good enough to bear a magnifying power of thirty times. At the present day there is little difficulty in selecting a spectacle glass which would rival that most famous of all telescopes. Not until after another generation of effort was there such notable improvement in the technique of lens-making that further astronomical discovery was possible. The reasons for this slow progress are to be found in the extremely critical requirements for a good lens. A departure by a fraction of $\frac{1}{1000000}$ part of an inch from a correct geometrical surface will greatly impair the performance of an objective. But even at this day the limit of accurate measurement may be set at about $\frac{1}{1000000}$ of an inch, while it is quite probable that ten times that value was vanishingly small to the artisans of a century or more ago. It was necessary, therefore, to devise a method of polishing—for it is a comparatively simple matter to grind a surface accurately—which should keep the surface true within a limit far transcending the range of measurements. Huygens is the first who seems to have done this, by polishing upon a paste which was formed to the glass and then dried, and by using only the central portion of a large lens. In Italy Campani developed a system which he most jealously guarded as a secret until his death, consisting of polishing with a dry powder on paper cemented to the grinding tools. This method still survives in Paris to the exclusion of almost all others, and it is probably the best for work which does not demand the highest scientific precision.

Newton, however, was the first to introduce a method which has since been developed to a state of surprising delicacy. Casting about for a means which should be sufficiently " tender," to use his own expression, for polishing the soft speculum metal, he fixed upon pitch, shaped to the mirror while warm, as a bed to hold the polishing powder. But the enormous value of this substance lies not so much in the comparative immunity which it gives from scratching, but in the fact that under slowly changing forces it is a liquid, but under those of short duration it behaves like a hard and brittle solid. Thus it is possible to slowly alter the shape of a lens while polishing, in any desired direction. It was only after the practical recognition of this fact that really excellent lenses were much more than a question of good fortune. The perfecting of this method belongs without doubt to the English of the last century and the early part of this. In the "Philosophical Transactions" we find many long papers relating to this art, contributed by skilful and successful amateurs. We may therefore regard the technique of the art of lens-making as practically complete at the middle of this century and as common property, so that success no longer depends upon the holding of some special or secret method.

We are now (after this, I fear, somewhat dry discussion of a necessary point) in a condition to explain the differences between the processes pursued by most telescope-makers and that of the maker of the Carleton College telescope.

This is the ordinary method: After securing perfect pieces of glass, crown and flint, as like as possible to those generally used, and having fixed upon the general shape of the lenses, a guess is made as to the proper radii of the four surfaces to determine the desired focal length and corrections both for colour and spherical aberration. The success of this guess has much to do with the necessary outlay of labour, and therefore past experience is of great value as a guide. After working the four surfaces to the dimensions provisionally adopted so far as to admit of fairly good seeing through the objective, an examination of the errors is made. Should the errors of colour be so small that their final correction will not make the telescope more than from three to ten per cent greater or less than the desired focal length, the crown lens will probably be completed in accordance with the provisional figures. Then the flint lens will be modified in such a direction as will tend to correct the observed errors of colour and figure, until, by a purely tentative process, the colour error is practically negligible and the error of figure is small. Then follows a process when the qualities of skill, conscientiousness, and perseverance have full scope. This process, first introduced, or at least made public, by Foucault, is known as local correcting. It consists in slowly polishing away portions of the lens surfaces so that errors in the focal image become so small, not that they can not be detected, but that one can not determine whether they are on the one side of truth or the other. Local correcting has always seemed to me to be eminently unscientific and unnecessary. It is a process of making errors which ought not to exist.

Mr. Brashear's method is essentially different from this. Before the glasses are touched every dimension and constant of the finished objective is known with great accuracy. His whole aim is to make the surfaces geometrically perfect; and by ingenious polishing machinery, which embodies twelve years of his thought and experience, he is enabled to do this with truly astonishing exactness. All the surfaces which admit of investigation—usually three in his ordinary construction—are made rigidly true without regard to the character of the focal image. This leaves only one surface which is known to be very nearly a sphere, but probably deviating slightly within in the direction of a prolate or oblate spheroid. A glance at the character of the focal image will determine this point. Then the polishing machine is adapted to bring about a change in the proper direction, and after action during a measured interval of time, the image is again examined, and from the observed change in character the necessary time for complete correction by the same or contrary action may be deduced. It will be observed that by this means it is quite possible to correct errors which are much too small to betray their nature, since a step in the wrong direction carries with it no consequences of the slightest moment, since any step may be retraced.

When we learn that Mr. Brashear's telescope objectives have always had a focal length differing only from $\frac{1}{10}$ to $\frac{1}{180}$ of one per cent of the value prescribed, we have a suggestion of the success of his efforts. But adding to that the fact that he is absolutely untrammelled by purely mechanical considerations, either as to the shape of his lenses or the character of his materials, leaving these questions to be decided alone by the requirements of the astronomer, it seems to me that we may fairly accord to him the merit of the most important improvements introduced into his art for a very long period.

I shall not venture to demand much of your time in considering the purely theoretical difficulties in telescope construction, not merely because the subject has already taxed our patience, but because it would be of almost too technical a character did we allow ourselves to regard anything but the most general features.

The obvious requirements are that in a good objective the light coming from a point in the object should be concentrated at a point in the image; but this, combined with a prescribed focal length, may be reduced to three conditions: First, a fixed focal length; second, freedom from colour error; third, freedom from spherical aberration for a particular colour or wave-length of light. Now let us catalogue what provisions we have for satisfying these conditions. They are, four surfaces, which must be spherical but may have any radii we please, the two thicknesses of the two lenses, and the distance which separates the lenses—that is, seven elements which may be varied to suit our requirements. As a matter of fact, however, on account of the cost of the material and the fact that glass is perfectly transparent, for powerful telescopes we must make the lenses as thin as possible; and we shall find also that separating the lenses introduces errors away from the axis which are, to say the least, undesirable. We have left, therefore, only the four radii as arbitrary constants. These, however, are more than enough to meet the three requirements. To make the problem determinate we must add another condition. The suggestion of this fourth condition and carrying the problem to its solution is the work of the great mathematicians who have directed their thought to it. Clairault proposed to make the fourth condition that the two adjacent surfaces should fit together and the lenses be cemented. This condition would be, doubtless, of great value were it possible to cement large lenses without changing their shapes to a degree which would quite spoil their performance. Sir John Herschel published a very important paper in 1821, in which he made the fourth condition that the spherical aberration should vanish, not only for objects at a very great distance, but also for those at a moderate distance. In this paper he computed a table, afterward greatly extended by Prof. Baden-Powell, for the avowed purpose of aiding the practical optician. It was this feature undoubtedly which brought his construction, not at all a good one, as we shall see, into more general use than any other for some time. But, as all Herschel's tables were derived from calculations which wholly disregarded the thickness of the lenses, I am quite unable to see how they could have been of any material aid, and am inclined to suspect that the discredit with which opticians have received the dicta of mathematicians concerning their instruments may have been due in part to this very fact. It is a singular fact, for which I have in vain sought the explanation, that Fraunhofer's objectives are of just such a form as to comply with the Herschelian solution, although they must have been made quite independently.

Gauss made the fourth condition that another colour or wave-length of light should be also free from spherical aberration. This seems to have been a tour de force as a mathematician, not as a sober suggestion of an improvement in construction, for in a point of fact the construction is very bad. It was generally believed that this condition could not be fulfilled; therefore Gauss, who was particularly fond of doing what all the rest of the world believed impossible, straightway did it. There has been only one effort to carry out this suggestion of Gauss, and that forty years later by Steinheil, but it proved a disappointment. A much larger objective made by Clark a few years ago, of the general form of Gauss's objective, probably does not meet the Gaussian condition; at least, this condition is extremely critical, and I believe it is not asserted that the objective was ever thoroughly investigated. It has been the father of no others.

It is hardly surprising, since none of these forms have any real merit, that the practical optician has, following the line of least resistance, adopted a form which costs him less labour than those heretofore mentioned, and is quite as good. By making the curve equi-convex the trouble and expense of making one pair of tools are saved, although this would hardly appear a satisfactory reason for choice of a particular form to the astronomer, who simply demands the best possible instrument of research.

The reason for so much futile work on the theory of the telescope objective is not far to seek. It had always been tacitly assumed that the condition of colour correction, one of those which serves to determine the values of the arbitrary constants, was readily determinable—in fact, one of the donne of the problem, whereas it is just this datum which has offered peculiar difficulties. Fraunhofer brought all the resources at the command of his genius to bear upon this point, and frankly failed, although in the effort he made a splendid discovery, which has assured a permanence to his fame no less than that of the history of science itself—the discovery of the dark, or Fraunhofer, lines in solar and stellar spectra. Gauss proposed the condition that the best objective is that which produces the most perfect concentration of light about the place of the geometrical image of a point, just as the best rifle practice is that which produces the maximum concentration of hits about the centre of the target. That this is a false guide appears at once from the consideration that if we take even as much as ten per cent of the light from an object, and diverted from the image so far that it can not be found, the telescope may still be practically perfect; all of Herschel's did much worse than this. But if you take that same ten per cent and concentrate it very close about the image, the telescope will be absolutely worthless.

The true difficulty with most of the theories is this: There is no recognition of the relative weight or importance of unavoidable errors. The optician is confronted at the very outset by the fact that absolute elimination of colour error is impossible, for certain physical reasons which we have not time for considering further. He can reduce the colour error of the old single-lens type of telescopes hundreds of times, and hence the length of the telescope tens of times. It is this fact which prevents the still further shortening of telescopes, which keeps the ratio of length to diameter not less than fifteen to one in large telescopes. This restriction being recognised, let us revise our limiting conditions. They now become, first, fixed focal length; second, best colour correction; third, freedom from spherical aberration for a particular wave-length of light. We therefore have still one arbitrary constant undetermined. How shall we fix its value, and thus solve the problem completely? Surely there is only one rational guide. Consider the residual errors and make the fourth condition such as to reduce these errors as far as possible. Now the only remaining errors are secondary colour error and spherical aberration for colours other than that for which it is eliminated, or, more scientifically, chromatic difference of spherical aberration. Which of these is the gravest defect? Our answer must depend upon the use to which the objective is to be put. If it is a high-power microscope objective, it is certainly the second. If it is an objective to be used for photographing at considerable angular distances from the axis, our question loses its physical significance, since we have excluded the consideration of eccentric refraction. But if the objective is to be for a visual telescope, there is no question that the defect of secondary colour error is incomparably the most serious. Our fourth and determining condition must, therefore, be better colour correction.

These are therefore the considerations which have served as guides in the construction of the Carleton College objective. First, the selection of the materials which, in the present condition of the art of optical glass-making, possess in the highest degree the desired physical properties; second, a general discussion of every possible combination of these two pieces of glass and a selection of the forms which yield the best attainable results. This conscientious strife after scientific perfection, the unexcelled skill with which the results of analysis have been interpreted into the reality of substance, the gratifying identity of predicted and realized values of physical characteristics—all of these have led some of those who have watched the growth of this new instrument of research with the most solicitous attention to the belief that although not the most powerful in existence, it may well be the most perfect great telescope yet made. Let us therefore congratulate the possessors of this noble instrument, wish them Godspeed in their search after knowledge, while we remind them that although no astronomer can ever make another discovery which will rival that made by the insignificant tube first directed toward the heavens by the Paduan philosopher, yet no mind can weigh the importance of any truth, however trivial in appearance, which may be added to that store which we call " science."

RESULTS
OF SPECTRUM ANALYSIS
APPLIED TO HEAVENLY BODIES

CELESTIAL SPECTROSCOPY

THE NEW ASTRONOMY

BY

WILLIAM HUGGINS

THE RESULTS OF SPECTRUM ANALYSIS APPLIED TO THE HEAVENLY BODIES[1]

AN important invention or discovery seldom, if ever, remains sterile and alone. It gives birth to other discoveries. The telescope and the microscope have led to remarkable discoveries in astronomy and in minute anatomy and physiology, which would not have been possible without those instruments. The observation that a magnetic body, free to move, arranges itself nearly north and south, has not only contributed immensely to the extension of commerce and of geographical discovery, but also has founded the important science of terrestrial magnetism.

This evening I have to bring before you some additions to our knowledge in the department of astronomy, which have followed from a comparatively recent discovery. The researches of Kirchhoff have placed in the hands of the astronomer a method of analysis which is specially suitable for the examination of the heavenly bodies. So unexpected and important are the results of the application of spectrum analysis to the objects in the heavens that this method of observation may be said to have created a new and distinct branch of astronomical science.

Physical astronomy, the imperishable and ever-growing monument to the memory of Newton, may be described as the extension of terrestrial dynamics to the heavens. It seeks to explain the movements of the celestial bodies on the supposition of the universality of an attractive force similar to that which exists upon the earth.

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[1] Delivered before the British Association for the Advancement of Science, August 23, 1866.

The new branch of astronomical science which spectrum analysis may be said to have founded has for its object to extend the laws of terrestrial physics to the other phenomena of the heavenly bodies, and it rests upon the now established fact that matter of a nature common to that of the earth, and subject to laws similar to those which prevail upon the earth, exists throughout the stellar universe.

The peculiar importance of Kirchhoff's discovery to astronomy becomes obvious if we consider the position in which we stand to the heavenly bodies. Gravitation and the laws of our being do not permit us to leave the earth; it is, therefore, by means of light alone that we can obtain any knowledge of the grand array of worlds which surround us in cosmical space. The starlit heavens is the only chart of the universe we have, and in it each twinkling point is the sign of an immensely vast though distant region of activity.

Hitherto the light from the heavenly bodies, even when collected by the largest telescope, has conveyed to us but very meagre information, and in some cases only of their form, their size, and their colour. The discovery of Kirchhoff enables us to interpret symbols and indications hidden within the light itself, which furnish trustworthy information of the chemical, and also to some extent of the physical, condition of the excessively remote bodies from which the light has emanated.

We are indebted to Newton for the knowledge that the beautiful tints of the rainbow are the common and necessary ingredients of ordinary light. He found that when white light is made to pass through a prism of glass it is decomposed into the beautiful colours which are seen in the rainbow. These colours when in this way separated from each other form the spectrum of the light. Let this white plate represent the transverse section of a beam of white light travelling toward you. Let now a prism be interposed in its path. The beam of white light is not turned aside as a whole, but the coloured lights composing it are deflected differently, each in proportion to the rapidity of its vibrations. An obvious consequence will be that, on emerging from the prism, the coloured lights which formed the white light will separate from each other, and in place of the white light which entered the prism we shall have its spectrum—that is, the coloured lights which composed it—in a state of separation from each other. Wollaston and Fraunhofer discovered that when the light of the sun is decomposed by a prism, the rainbow colours which form its spectrum are not continuous, but are interrupted by a large number of dark lines. These lines of darkness are the symbols which indicate the chemical constitution of the sun. It was not until recently, in the year 1859, that Kirchhoff taught us the true nature of these lines. He himself immediately applied his method of interpretation to the dark lines of the solar spectrum, and was rewarded by the discovery that several of the chemical elements which exist upon the earth are present in the solar atmosphere.

It is my intention to bring before you this evening the results of the extension of this method of analysis to the heavenly bodies other than the sun. These researches have been carried on in my observatory during the last four years. In respect of a large part of these investigations—viz., those of the moon, the planets, and fixed stars—I have had the great pleasure of working conjointly with the very distinguished chemist and philosopher, Dr. William A. Miller. Half a century ago Fraunhofer recognised several of the solar lines in the light of the moon, Venus, and Mars, and also in the spectra of several stars. Recently Donati, Janssen, Secchi, Rutherfurd, and the Astronomer Royal have observed lines in the spectra of some stars. Before I describe the results of our observations I will state, in a few words, the principles of spectrum analysis upon which our interpretation of the phenomena we have observed has been based, and also the method of observing which we have employed.

When light which has emanated from different sources is decomposed by a prism, the spectra which are obtained may differ in several important respects from each other.

All the spectra which may present themselves can be conveniently arranged in three general groups. A spectrum illustrating each of these three orders is placed upon the diagram:

1. The special character which distinguishes spectra of the first order consists in that the continuity of the coloured band is unbroken either by dark or bright lines. By means of the electric lamp, Mr. Ladd will throw a spectrum of this order upon the screen. We learn from such a spectrum that the light has been emitted by an opaque body, and almost certainly by matter in the solid or liquid state. A spectrum of this order gives to us no knowledge of the chemical nature of the incandescent body from which light comes. In the present case the light is emitted by the white-hot carbon points of the electric lamp. A spectrum in all respects similar would be formed by the light from incandescent iron, or lime, or magnesia.

2. Spectra of the second order are very different. These consist of coloured lines of light separated from each other. From such a spectrum we may learn much. It informs us that the luminous matter from which the light has come is in the state of gas. It is only when a luminous body is free from the molecular trammels of solidity and liquidity that it can exhibit its own peculiar power of radiating some coloured rays alone. Hence substances, when in a state of gas, may be distinguished from each other by their spectra. Each element, and every compound body that can become luminous in the gaseous state without suffering decomposition, is distinguished by a group of lines peculiar to itself. These green lines are produced by silver in a state of gas, and only by silver gas. It is obvious that if the groups of lines characterizing the different terrestrial substances be known, a comparison of these as standard spectra with the spectrum of light from an unknown source will show whether any of these terrestrial substances exist in the source of the light.

3. The third order consists of the spectra of incandescent solid or liquid bodies, in which the continuity of

the coloured light is broken by dark lines. These dark spaces are not produced by the source of the light. They tell us of vapours through which the light has passed on its way, and which have robbed the light, by absorption, of certain definite colours or rates of vibration; such spectra are formed by the light of the sun and stars.

Kirchhoff has shown that if vapours of terrestrial substances come between the eye and an incandescent body they cause groups of dark lines, and, further, that the group of dark lines produced by each vapour is identical in the number of the lines and in their position in the spectrum with the group of bright lines of which its light consists when the vapour is luminous.

Mr. Ladd will throw upon the screen the spectrum of incandescent carbon points which contain sodium. Observe in addition to the continuous spectrum of the incandescent carbon a bright-yellow band, which indicates the presence of sodium. Now a piece of metallic sodium will be introduced into the lamp. The sodium will be vapourized by the heat, and will fill the lamp with its vapour. This vapour absorbs, quenches the light that it emits when luminous. There will thus be produced a black line exactly in the place where the bright-yellow line was seen.

It is evident that Kirchhoff by this discovery has furnished us with the means of interpreting the dark lines of the solar spectrum. For this purpose it is necessary to compare the bright lines in the spectra of the light of terrestrial substances, when in the state of gas, with the dark lines in the solar spectrum. When a group of bright lines coincides with a similar group of dark lines, then we know that the terrestrial substance producing the bright lines is present in the atmosphere of the sun; for it is this substance, and this substance alone, which, by its own peculiar power of absorption, can produce that particular group of dark lines. In this way Kirchhoff discovered the presence of several terrestrial elements in the solar atmosphere.

METHODS OF OBSERVATION

I now pass to the special methods of observation by which, in our investigations, we have applied these principles of spectrum analysis to the light of the heavenly bodies. I may here state that several circumstances unite to make these observations very difficult and very irksome. In our climate, on few only even of those nights in which the stars shine brilliantly to the naked eye, is the air sufficiently steady for these extremely delicate observations. Further, the light of the star is feeble. This difficulty has been met, in some measure, by the employment of a large telescope. The light of a star falling upon the surface of an object-glass of eight inches aperture is gathered up and concentrated at the focus into a minute and brilliant point of light.

Another inconvenience arises from the apparent movement of the stars, caused by the rotation of the earth, which carries the astronomer and his instruments with it. This movement was counteracted by a movement given by clockwork to the telescope in the opposite direction. In practice, however, it is not easy to retain the image of a star for any length of time exactly within the jaws of a slit only the $\frac{1}{300}$ of an inch apart. By patient perseverance these difficulties have been overcome, and satisfactory results obtained. We considered that the trustworthiness of our results must rest chiefly upon direct and simultaneous comparison of terrestrial spectra with those of celestial objects. For this purpose we contrived the following apparatus.

By an outer tube the instrument is attached to the eye end of the telescope, and is carried round with it by the clock motion. Within this outer tube a second tube slides, carrying a cylindrical lens. This lens is for the purpose of elongating the round pointlike image of the star into a short line of light, which is made to fall exactly within the jaws of a nearly closed slit. Behind the slit an achromatic lens (and at the distance of its own focal length) causes the pencils to emerge parallel. They then pass into two prisms of dense flint glass. The spectrum which results from the decomposition of the light by the prisms is viewed through a small achromatic telescope. This telescope is provided with a micrometer screw, by which the lines of the spectrum may be measured.

The light of the terrestrial substances which are to be compared with the stellar spectra is admitted into the instrument in the following manner:

Over one half of the slit is fixed a small prism, which receives the light reflected into it by the movable mirror placed above the tube. The mirror faces a clamp of ebonite, provided with forceps to contain fragments of the metals employed. These metals are rendered luminous in the state of gas by the intense heat of the sparks from a powerful induction coil. The light from the spark reflected into the instrument by means of the mirror and the little prism passes on to the prisms in company with that from the star. In the small telescope the two spectra are viewed in juxtaposition, so that the coincidence and relative positions of the bright lines in the spectrum of the spark with dark lines in the spectrum of the star can be accurately determined.

MOON AND PLANETS

I now pass to the results of our observations.

I refer in a few words only to the moon and planets. These objects, unlike the stars and nebulæ, are not original sources of light. Since they shine by reflecting the sun's light, their spectra resemble the solar spectrum, and the only indications in their spectra which may become sources of knowledge to us are confined to any modifications which the solar light may have suffered, either in the atmosphere of the planets or by reflection at their surfaces.

Moon.—On the moon the results of our observations have been negative. The spectra of the various parts of the moon's surface, when examined under different conditions of illumination, showed no indication of an atmosphere about the moon. I also watched the spectrum of a star as the dark edge of the moon advanced toward the star and then occulted it. No signs of a lunar atmosphere presented themselves.

Jupiter.—In the spectrum of Jupiter lines are seen which indicate the existence of an absorptive atmosphere about this planet. In this diagram these lines are presented as they appeared when viewed simultaneously with the spectrum of the sky, which, at the time of observation, reflected the light of the setting sun. One strong band corresponds with some terrestrial atmospheric lines, and probably indicates the presence of vapours similar to those which are about the earth. Another band has no counterpart among the lines of absorption of our atmosphere, and tells us of some gas or vapour which does not exist in the earth's atmosphere.

Saturn.—The spectrum of Saturn is feeble, but lines similar to those which distinguish the spectrum of Jupiter were detected. These lines are less strongly marked in the ansæ of the rings, and show that the absorptive power of the atmosphere about the rings is less than that of the atmosphere which surrounds the ball. A distinguished foreigner present at the meeting, Janssen, has quite recently found that several of the atmospheric lines in this part of the spectrum are produced by aqueous vapour. It appears to be very probable that aqueous vapour exists in the atmospheres of Jupiter and Saturn.

Mars.—On one occasion some remarkable groups of lines were seen in the more refrangible part of the spectrum of Mars. These may be connected with the source of the red colour which distinguishes this planet.

Venus.—Though the spectrum of Venus is brilliant, and the lines of Fraunhofer were well seen, no additional lines affording evidence of an atmosphere about Venus were detected. The absence of lines may be due to the circumstance that the light is probably reflected, not from the planetary surface, but from clouds at some elevation above it. The light which reaches us in this way by reflection from clouds would not have been exposed to the absorbent action of the lower and denser strata of the planet's atmosphere.

THE FIXED STARS

The fixed stars, though immensely more remote and less conspicuous in brightness than the moon and planets, yet, because they are original sources of light, furnish us with fuller indications of their nature.

To each succeeding age the stars have been a beauty and a mystery. Not only children, but the most thoughtful of men, often repeat the sentiment expressed in the well-known lines:

> "Twinkle, twinkle, little star;
> How I wonder what you are!"

The telescope was appealed to in vain, for in the largest instruments the stars remain diskless—brilliant points merely.

The stars have indeed been represented as suns, each upholding a dependent family of planets. This opinion rested upon a possible analogy alone. It was not more than a speculation. We possessed no certain knowledge from observation of the true nature of those remote points of light. This long and earnestly coveted information is at last furnished by spectrum analysis. We are now able to read in the light of each star some indications of its nature. Since I have not a magician's power to convert this theatre into an observatory, and so exhibit to you the spectra of the stars themselves, I have provided photographs of careful drawings. These photographs Mr. Ladd will exhibit upon the screen by means of the electric lamp. I will take first the spectra of two bright stars which we have examined with great care.

The upper one represents the spectrum of Aldebaran, and the other that of Betelgeux, the star marked ***a*** in the constellation of Orion.

The positions of all these dark lines, about eighty in each star, were determined by careful and repeated measures. These measured lines form but a small part of the numerous fine lines which may be seen in the spectra of these stars.

Beneath the spectrum of each star are represented the bright lines of the metals which have been compared with it. These terrestrial spectra appeared in the instrument as you now see them upon the screen, in juxtaposition with the spectrum of the star. By such an arrangement it is possible to determine with great accuracy whether or not any of these bright lines actually coincide with any of the dark ones. For example:

This closely double line is characteristic of sodium. You see that it coincides, line for line, with a dark line similarly double in the star. The vapour of sodium is therefore present in the atmosphere of the star, and sodium forms one of the elements of the matter of this brilliant but remote star.

These three lines in the green are produced, so far as we know, by the luminous vapour of magnesium alone. These lines agree in position exactly line for line with three dark stellar lines. The conclusion, therefore, appears well founded that another of the constituents of this star is magnesium.

Again, there are two strong lines peculiar to the element hydrogen; one line has its place in the red part of the spectrum, the other at the blue limit of the green. Both of these correspond to dark lines of absorption in the spectrum of the star. Hydrogen, therefore, is present in the star.

In a similar way, other elements, among them bismuth, antimony, telurium, and mercury, have been shown to exist in the star.

Now, in reference to all those elements, the evidence does not rest upon the coincidence of one line, which would be worth but little, but upon the coincidence of a group of two, three, or four lines, occurring in different parts of the spectrum. Other corresponding lines are probably also present, but the faintness of the star's light limited our comparisons to the stronger lines of each element.

What elements do the numerous other lines in the star represent? Some of them are probably due to the vapours of other terrestrial elements, which we have not yet compared with these stars. But may not some of these lines be the signs of primary forms of matter unknown upon the earth? Elements new to us may here show themselves which form large and important series of compounds, and therefore give a special character to the physical conditions of these remote systems. In a similar manner the spectra of terrestrial substances have been compared with several other stars. The results are given in the diagrams. Five or six elements have been detected in Betelgeux. Ten other elements do not appear to have a place in the constitution of this star.

β Pegasi contains sodium, magnesium, and perhaps barium.

Sirius contains sodium, magnesium, iron, and hydrogen.

α Lyræ (Vega) contains sodium, magnesium, and iron.

Pollux contains sodium, magnesium, and iron.

About sixty other stars have been examined, all of which appear to have some elements in common with the sun and earth, but the selective grouping of the elements in each star is probably peculiar and unique.

A few stars, however, stand out from the rest, and appear to be characterized by a peculiarity of great significance. These stars are represented by Betelgeux and *β* Pegasi. The general grouping of the lines of absorption in these stars is peculiar, but the remarkable and exceptional feature of their spectra is the absence of the two lines which indicate hydrogen, one line in the red and the other in the green. These lines correspond to Fraunhofer's C and F. The absence of these lines in some stars shows that the lines C and F are not due to the aqueous vapour of the atmosphere.

We hardly venture to suggest that the planets which may surround these suns probably resemble them in not possessing the important element hydrogen. To what forms of life could such planets be adapted? Worlds without water! A power of imagination like that possessed by Dante would be needed to people such planets with living creatures.

It is worthy of consideration that, with these few exceptions, the terrestrial elements which appear most widely diffused through the host of stars are precisely some of those which are essential to life such as it exists upon the earth—namely, hydrogen, sodium, magnesium, and iron. Besides, hydrogen, sodium, and magnesium represent the ocean, which is an essential part of a world constituted like the earth.

We learn from these observations that in plan of structure the stars, or at least the brightest of them, resemble the sun. Their light, like that of the sun, emanates from intensely white-hot matter, and passes through an atmosphere of absorbent vapours. With this unity of general plan of structure there exists a great diversity among the individual stars. Star differs from star in chemical constitution. May we not believe that the individual peculiarities of each star are essentially connected with the special purpose which it subserves, and with the living beings which may inhabit the planetary worlds by which it may possibly be surrounded?

When we had obtained this new information respecting the true nature of the stars, our attention was directed to the phenomena which specially distinguish some of the stars.

COLOURS OF THE STARS

When the air is clear, especially in southern climes, the twinkling stars do not all resemble diamonds; here and there may be seen in beauteous contrast richly coloured gems.

The colour of the light of the stars which are bright to the naked eye is always some tint of red, orange, or yellow. When, however, a telescope is employed, in close companionship with many of these ruddy and orange stars, other fainter stars become visible, the colour of which may be blue, or green, or purple.

Now it appeared to us to be probable that the origin of these differences of colour among the stars may be indicated by their spectra.

Since we had found that the source of the light of the stars is incandescent solid or liquid matter, it appeared to be very probable that at the time of its emission the light of all the stars is white alike. The colours observed among them must then be caused by some modification suffered by the light after its emission.

Again, it was obvious that if the dark lines of absorption were more numerous or stronger in some part of the spectrum, then those colours would be subdued in power, relatively to the colour in which few lines only occur. These latter colours, remaining strong, would predominate, and give to the light, originally white, their own tints. These suppositions have been confirmed by observations.

Mr. Ladd will throw upon the screen the spectrum of Sirius, which may be taken as an illustration of the stars the light of which is white.

As might be expected, the spectra of these stars are remarkable for their freedom from strong groups of absorption lines. The dark lines, though present in great number, are all, with one exception, very thin and faint, and too feeble to modify the original whiteness of the light. The one exception consists of three very strong single lines: one line corresponding to Fraunhofer's C, one to F, and the other near G. Two of these certainly indicate the presence of hydrogen. This peculiarity, which seems invariably connected with colourless stars, is very suggestive, and invites speculation. May it be a sign of a temperature of extreme fierceness?

Let us now examine the spectrum of an orange star.

This diagram represents the spectrum of the brighter of the two stars which form the double star α Herculis. In the spectrum of this star the green and blue parts of the light, and also the deep red, are subdued with strong groups of lines, while the orange and yellow rays preserve nearly their original intensity, and therefore predominate in the star's light.

The question yet remained to be answered, Would the faint telescopic stars, which are blue, green, and purple, and which are never found alone in the heavens, but always under the protection of a strong ruddy or orange star, furnish spectra in accordance with this theory?

With some little difficulty, and by means of a special arrangement of the spectrum apparatus, we succeeded in observing the spectra of the components of some double stars. There will now be thrown upon the screen the well-known double star β Cygni. In a large telescope the colours of the two stars are beautifully contrasted, as they now appear upon the screen. The spectra of these stars are now shown. The upper spectrum represents the orange star, the lower one that of its beautiful blue but feeble companion. In the orange star you observe that the dark lines are strongest and most closely grouped in the blue and violet parts of the spectrum, and the orange rays therefore, which are comparatively free from lines, predominate.

In the delicate blue companion, the strongest groups of lines are found in the yellow, orange, and in part of the red. In the arrangement of these groups of lines we have a sufficient cause for the predominance of the other portions of the spectrum which unite in the eye to give the blue purple colour of the light of this star.

We have, therefore, shown that the colours of the stars are produced by the vapours existing in their atmosphere. The chemical constitution of a star's atmosphere will depend upon the elements existing in the star and upon its temperature.

VARIABLE STARS

The brightness of many of the stars is found to be variable. From night to night, from month to month, or from season to season, their light may be observed to be continually changing, at one time increasing, at another time diminishing. The careful study of these variable stars by numerous observers has shown that their continual changes do not take place in an uncertain or irregular manner. The greater part of these remarkable objects wax and wane in accordance with a fixed law of periodic variations which is peculiar to each.

We have been seeking for some time to throw light upon this strange phenomenon by means of observation of their spectra. If in any case the periodic variation of brightness is associated with physical changes occurring in the star, we might obtain some information by means of the prism. Again, if the diminution in brightness of a star should be caused by the interposition of a dark body, then, in that case, if the dark body be surrounded with an atmosphere, its presence might possibly be revealed to us by the appearance of additional lines of absorption in the spectrum of the star when at its minimum. One such change in the spectrum of a variable star we believe we have already observed.

Betelgeux is a star of a moderate degree of variability. When this star was at its maximum brilliancy, in February last, we missed a group of lines, the exact position of which we had determined with great accuracy by micrometric measurement some two years before.

We have observed the spectra of several variable stars at different phases of their periodic variation, but our results are not yet complete.

It is worthy of notice that the variable stars which have a ruddy or an orange tint possess spectra analogous to that of Betelgeux and β Pegasi.

As an example of this group of variable stars, Mr. Ladd will throw upon the screen the spectrum of μ Cephei when at its maximum.

TEMPORARY STARS

With the variable stars modern opinion would associate the remarkable phenomena of the so-called new stars which occasionally, but at long intervals, have suddenly appeared in the sky. But in no case has a permanently bright star been added to the heavens. The splendour of all these objects was temporary only, though whether they died out or still exist as extremely faint stars is uncertain. In case of the two modern temporary stars, that seen by Mr. Hind in 1845, and the bright star recently observed in Corona, though they have lost their ephemeral glory, they still continue as stars of the tenth and eleventh magnitudes.

The old theories respecting these strange objects must be rejected. We can not believe with Tycho Brahe that objects so ephemeral are new creations, nor with Riccioli that they are stars brilliant on one side only, which have been suddenly turned round by the Deity. The theory that they have suddenly darted toward us with a velocity greater than that of light, from a region of remote invisibility, will not now find supporters.

On the 12th of May last a star of the second magnitude suddenly burst forth in the constellation of the Northern Crown. Thanks to the kindness of the discoverer of this phenomenon, Mr. Birmingham, of Tuam, I was enabled, conjointly with Dr. Miller, to examine the spectrum of this star on the 16th of May, when it had not fallen much below the third magnitude.

I ought to state that Mr. Barker, of London, Canada West, who announced an observation of this star on the 14th of May in the "Canadian Free Press," now claims to have seen the star on May 4th, and states that it increased in brilliancy up to May 10th, when it was at its maximum.

The spectrum of this star consists of two distinct spectra. One of these is formed by these four bright lines. The other spectrum is analogous to the spectra of the sun and stars.

These two spectra represent two distinct sources of light. Each spectrum is formed by the decomposition of light which is independent of the light which gives birth to the other spectrum.

The continuous spectrum, crowded with groups of dark lines, shows that there exists a photosphere of incandescent solid or liquid matter. Further, that there is an atmosphere of cooler vapours, which give rise by absorption to the groups of dark lines.

So far the constitution of this object is analogous to that of the sun and stars; but in addition there is the second spectrum, which consists of bright lines. There is, therefore, a second and distinct source of light, and this must be, as the character of the spectrum shows, luminous gas. Now the position of the two principal bright lines of this spectrum informs us that one of the luminous gases is hydrogen. The great brightness of these lines shows that the luminous gas is hotter than the photosphere. These facts, taken in connection with the suddenness of the outburst of light in the star, and its immediate very rapid decline in brightness from the second magnitude down to the eighth magnitude in twelve days, suggested to us the startling speculation that the star had become suddenly inwrapped in the flames of burning hydrogen. In consequence, it may be, of some great convulsion, enormous quantities of gas were set free. A large part of this gas consisted of hydrogen, which was burning about the star in combination with some other element. This flaming gas emitted the light represented by the spectrum of bright lines. The increased brightness of the spectrum of the other part of the star's light may show that this fierce gaseous conflagration had heated to a more vivid incandescence the solid matter of the photosphere. As the free hydrogen became exhausted the flames gradually abated, the photosphere became less vivid, and the star waned down to its former brightness.

We must not forget that light, though a swift messenger, requires time to pass from the star to us. The great physical convulsion which is new to us is already an event of the past with respect to the star itself. For years the star has existed under the new conditions which followed this fiery catastrophe.

NEBULÆ

I now pass to objects of another order.

When the eye is aided by a telescope of even moderate power, a large number of faintly luminous patches and spots come forth from the darkness of the sky, which are in strong contrast with the brilliant but pointlike images of the stars. A few of these objects may be easily discerned to consist of very faint stars closely aggregated together. Many of these strange objects remain, even in the largest telescopes, unresolved into stars, and resemble feebly shining clouds, or masses of phosphorescent haze. During the last one hundred and fifty years the intensely important question has been continually before the mind of astronomers, " What is the true nature of these faint, cometlike masses? "

The interest connected with an answer to this question has much increased since Sir William Herschel suggested that these objects are portions of the primordial material out of which the existing stars have been fashioned; and further, that in these objects we may study some of the stages through which the suns and planets pass in their development from luminous cloud.

The telescope has failed to give any certain information of the nature of the nebulæ. It is true that each successive increase of aperture has resolved more of these objects into bright points, but at the same time other fainter nebulæ have been brought into view, and fantastic wisps and diffused patches of light have been seen, which the mind almost refuses to believe can be due to the united glare of innumerable suns still more remote.

Spectrum analysis, if it could be successfully applied to objects so excessively faint, was obviously a method of investigation specially suitable for determining whether any essential physical distinction separates the nebulæ from the stars.

I selected for the first attempt, in August, 1864, one of the class of small but comparatively bright nebulæ. My surprise was very great on looking into the small telescope of the spectrum apparatus to perceive that there was no appearance of a band of coloured light, such as a star would give, but in place of this there were three isolated bright lines only.

This observation was sufficient to solve the long-agitated inquiry in reference to this object at least, and to show that it was not a group of stars, but a true nebula.

A spectrum of this character, so far as our knowledge at present extends, can be produced only by light which has emanated from matter in the state of gas. The light of this nebula, therefore, was not emitted from incandescent solid or liquid matter, as is the light of the sun and stars, but from glowing or luminous gas.

It was of importance to learn, if possible, from the position of these bright lines, the chemical nature of the gas or gases of which this nebula consists.

Measures taken by the micrometer of the most brilliant of the bright lines showed that this line occurs in the spectrum very nearly in the position of the brightest of the lines in the spectrum of nitrogen. The experiment was then made of comparing the spectrum of nitrogen directly with the bright lines of the nebula. I found that the brightest of the lines of the nebula coincided with the strongest of the group of lines which are peculiar to nitrogen. It may be, therefore, that the occurrence of this one line only indicates a form of matter more elementary than nitrogen, and which our analysis has not yet enabled us to detect.

In a similar manner the faintest of the lines was found to coincide with the green line of hydrogen.

The middle line of the three lines which form the spectrum of the nebula does not coincide with any strong line in the spectra of about thirty of the terrestrial elements. It is not far from the line of barium, but it does not coincide with it. Besides these bright lines, there was also an exceedingly faint continuous spectrum. The spectrum had no apparent breadth, and must, therefore, have been formed by a minute point of light. The position of this faint spectrum, which crossed the bright lines about the middle of their length, showed that the bright point producing it was situated about the centre of the nebula. Now, this nebula possesses a minute but bright nucleus. We learn from this observation that the matter of the nucleus is almost certainly not in a state of gas, as is the material of the surrounding nebula. It consists of opaque matter, which may exist in the form of an incandescent fog of solid or liquid particles.

The new and unexpected results arrived at by the prismatic examination of this nebula showed the importance of examining as many as possible of these remarkable bodies. Would all the nebulæ give similar spectra? Especially it was of importance to ascertain whether those nebulæ which the telescope had certainly resolved into a close aggregation of bright points would give a spectrum indicating gaseity.

The observation with the prism of these objects is extremely difficult, on account of their great faintness. Besides this, it is only when the sky is very clear and the moon is absent that the prismatic arrangement of their light is even possible. During the last two years I have examined the spectra of more than sixty nebulæ and clusters. These may be divided into two great groups. One group consists of the nebulæ which give a spectrum similar to the one I have already described, or else of one or two only of the three bright lines. Of the sixty objects examined, about one third belong to the class of gaseous bodies. The light from the remaining forty nebulæ and clusters becomes spread out by the prism into a spectrum which is apparently continuous.

I will exhibit upon the screen a few of the more remarkable of the nebulæ which are gaseous in their constitution.

This photograph is from a drawing by Lord Rosse of a small nebula in Aquarius. (I. H. IV.)

We have here a gaseous system which reminds the observer of Saturn and his rings. The ring is seen edgewise.

The three bright lines represent the spectrum into which the light of this object is resolved by the prism.

In this other nebula we find probably an analogous general form of structure. In consequence of the nebula lying in a different position to us, its ring is seen, not edgewise, but open on the flat. The spectrum consists of three bright lines.

The arrangement of the streams of light in the object now on the screen suggests a spiral structure. This nebula is remarkable as the only one in which, in addition to the three bright lines, a fourth line was also seen.

The most remarkable, and possibly the nearest to our system, of the nebulæ presenting a ring formation, is the well-known annular nebula in Lyra. The spectrum consists of one bright line only. When the slit of the instrument crosses the nebula, the line consists of two brighter portions, corresponding to the sections of the ring. A much fainter line joins them, which shows that the faint central portion of the nebula has a similar constitution.

A nebula remarkable for its large extent and peculiar form is that known as the dumb-bell nebula. The spectrum of this nebula consists of one line only. A prismatic examination of the light from different parts of this object shows that it is throughout of a similar constitution.

The most widely known, perhaps, of all the nebulæ is the remarkable cloudlike object in the sword-handle of Orion.

This object is also gaseous. Its spectrum consists of three bright lines. Lord Rosse informs me that the bluish-green matter of the nebula has not been resolved by his telescope. In some parts, however, he sees a large number of very minute red stars, which, though apparently connected with the irresolvable matter of the nebula, are yet doubtless distinct from it. These stars would be too faint to furnish a visible spectrum.

I now pass to some examples of the other great group of nebulæ and clusters.

All the true clusters, which are resolved by the telescope into distinct bright points, give a spectrum, which does not consist of separate bright lines, but is apparently continuous in its light. There are many nebulæ which furnish a similar spectrum.

I take, as an example of these nebulæ, the great nebula in Andromeda, which is visible to the naked eye, and is not seldom mistaken for a comet. The spectrum of this nebula, though apparently continuous, has some suggestive peculiarities. The whole of the red and part of the orange are wanting. Besides this character, the brighter parts of the spectrum have a very unequal and mottled appearance.

It is remarkable that the easily resolved cluster in Hercules has a spectrum precisely similar. The prismatic connection of this cluster with the nebula in Andromeda is confirmed by telescopic observation. Lord Rosse has discovered in this cluster dark streaks or lines similar to those which are seen in the nebula in Andromeda.

In connection with these observations, it was of great interest to ascertain whether the broad classification afforded by the prism of the nebulæ and clusters would correspond with the indications of resolvability furnished by the telescope. Would it be found that all the unresolved nebulæ are gaseous, and that those which gave a continuous spectrum are clusters of stars?

Lord Oxmantown has examined all the observations of the sixty nebulæ and clusters in my list, which have been made with the great reflecting telescope erected by his father, the Earl of Rosse.

The results are given in this table:

	Continuous spectrum.	Gaseous spectrum.
Clusters	10	0
Resolved, or resolved ?	5	0
Resolvable, or resolvable?	10	6
Blue or green, no resolvability, no resolvability seen.	0	4
	6	5
	31	15
Not observed by Lord Rosse	10	4
	41	19

Considering the great difficulty of successful telescopic observation of these objects, the correspondence between the results of prismatic and telescopic observation may be regarded as close and suggestive.

Half of the nebulæ which give a continuous spectrum have been resolved, and about one third more are probably resolvable; while of the gaseous nebulæ none have been certainly resolved, according to Lord Rosse.

The inquiry now presents itself upon us, What superstructure of interpretation have we a right to raise upon the new facts with which the prism has furnished us?

Is the existence of the gaseous nebulæ an evidence of the reality of that primordial nebulous matter required by the theories of Sir William Herschel and Laplace?

Again, if we do not accept the view that these nebulæ are composed of portions of the original elementary matter out of which suns and planets have been elaborated, what is the cosmical rank and relation which we ought to assign to them?

As aids to a future determination of these great questions I will refer in a few words to some other observations.

COMETS

There are objects in the heavens which occasionally, and under some conditions, resemble closely some of the nebulæ. In some positions in their orbits some of the comets appear as round vaporous masses, and, except by their motion, can not be distinguished from nebulæ. Does this occasional general resemblance indicate a similarity of nature? If such be the case, if the material of the comets is similar to that of the nebulæ, then the study of the wonderful changes which comets undergo in the neighbourhood of the sun may furnish useful information for a more correct interpretation of the structure and condition of the nebulæ. In 1864 Donati found that the spectrum of a comet visible in that year consisted of bright lines.

Last January a small telescopic comet was visible. Its appearance in a large telescope is represented on the screen.

It was a nearly circular, very faint vaporous mass. Nearly in the centre a small and rather dim nucleus was seen. When this object was viewed in the spectroscope, two spectra were distinguished—a very faint continuous spectrum of the coma showing that it was visible by reflecting solar light. About the middle of this faint spectrum a bright point was seen. This bright point is the spectrum of the nucleus, and shows that its light is different from that of the coma. This short, bright line indicates that the nucleus of this comet was self-luminous, and further, the position of this line of the spectrum suggests that the material of the comet was similar to the matter of which the gaseous nebulæ consist.

MEASURES OF THE INTRINSIC BRIGHTNESS OF THE NEBULÆ

It appeared to me that some information of the nature of the nebulæ might be obtained from observations of another order. If physical changes of the magnitude necessary for the conversion of the gaseous bodies into suns are now in progress in the nebulæ, surely this process of development would be accompanied by marked changes in the intrinsic brightness of their light, and in their size.

Now, since the spectroscope shows these bodies to be continuous masses of gas, it is possible to obtain an approximate measure of their real brightness. It is known that as long as a distant object remains of sensible size, its brightness remains unaltered. By a new photometric method I found the intrinsic intensity of the light of three of the gaseous nebulæ in terms of a sperm candle burning at the rate of 158 grains per hour:

Nebula No. 4,628, $\frac{1}{1508}$ part of the intensity of the candle.

Annular nebula, Lyra, $\frac{1}{6032}$ part of the intensity of the candle.

Dumb-bell nebula, $\frac{1}{19604}$ part of the intensity of the candle.

These numbers represent not the apparent brightness only, but the true brightness of these luminous masses, except so far as it may have been diminished by a possible power of extinction existing in cosmical space, and by the absorption of our atmosphere. It is obvious that similar observations, made at considerable intervals of time, may show whether the light of these objects is undergoing increase or diminution, or is subject to a periodic variation.

If the dumb-bell nebula, the feeble light of which is not more than $\frac{1}{20000}$ part of that of a candle, be in accordance with popular theory a sun-germ, then it is scarcely possible to put in an intelligible form the enormous number of times by which its light must increase before this faint nebula, feebler now in its glimmering than a rushlight, can rival the dazzling splendour of our sun.

MEASURES OF THE NEBULÆ

Some of the nebulæ are sufficiently defined in outline to admit of accurate measurement. By means of a series of micrometric observations, it may be possible to ascertain whether any considerable alteration in size takes place in nebulæ.

METEORS

Mr. Alexander Herschel has recently succeeded in subjecting another order of the heavenly bodies to prismatic analysis. He has obtained the spectrum of a bright meteor, and also the spectra of some of the trains which meteors leave behind them. A remarkable result of his observations appears to be that sodium, in the state of luminous vapour, is present in the trains of most meteors.

CONCLUSION

In conclusion, the new knowledge that has been gained from the observations with the prism may be summed up as follows:

1. All the brighter stars, at least, have a structure analogous to that of the sun.

2. The stars contain material elements common to the sun and earth.

3. The colours of the stars have their origin in the

chemical constitution of the atmospheres which surround them.

4. The changes in brightness of some of the variable stars are attended with changes in the lines of absorption of their spectra.

5. The phenomena of the star in Corona appear to show that in this object, at least, great physical changes are in operation.

6. There exist in the heavens true nebulæ. These objects consist of luminous gas.

7. The material of comets is very similar to the matter of the gaseous nebulæ, and may be identical with it.

8. The bright point of the star clusters may not be in all cases stars of the same order as the separate bright stars.

It may be asked what cosmical theory of the origin and relations of the heavenly bodies do these new facts suggest? It would be easy to speculate, but it appears to me that it would not be philosophical to dogmatize at present on a subject of which we know so very little. Our views of the universe are undergoing important changes; let us wait for more facts, with minds unfettered by any dogmatic theory, and therefore free to receive the obvious teaching, whatever it may be, of new observations.

Star differs from star in glory, each nebula and each cluster has its own special features; doubtless in wisdom, and for high and important purposes, the Creator has made them all.

CELESTIAL SPECTROSCOPY[1]

IT is now many years since this association has done honour to the science of astronomy in the selection of its president. Since Sir George Airy occupied the chair, in 1851, and the late Lord Wrottesley nine years later, in 1860, other sciences have been represented by the distinguished men who have presided over your meetings. The very remarkable discoveries in our knowledge of the heavens which have taken place during this period of thirty years—one of amazing and ever-increasing activity in all branches of science—have not passed unnoticed in the addresses of your successive presidents; still, it seems to me fitting that I should speak to you to-night chiefly of those newer methods of astronomical research which may have led to those discoveries, and which have become possible by the introduction since 1860 into the observatory of the spectroscope and the modern photographic plate.

In 1866 I had the honour of bringing before this association, at one of the evening lectures, an account of the first fruits of the novel and unexpected advances in our knowledge of the celestial bodies which followed rapidly upon Kirchhoff's original work on the solar spectrum and the interpretation of its lines.

Since that time a great harvest has been gathered in the same field by many reapers. Spectroscopic astronomy has become a distinct and acknowledged branch of the science, possessing a large literature of its own, and observatories specially devoted to it. The more recent discovery of the gelatine dry plate has given a further great impetus

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[1] From "Report of the British Association for the Advancement of Science" (1891).

to this modern side of astronomy, and has opened a pathway into the unknown of which even an enthusiast thirty years ago would scarcely have dared to dream.

In no science, perhaps, does the sober statement of the results which have been achieved appeal so strongly to the imagination, and make so evident the almost boundless powers of the mind of man. By means of its light alone to analyze the chemical nature of a far-distant body; to be able to reason about its present state in relation to the past and future; to measure within an English mile or less per second the otherwise invisible motion which it may have toward us or from us; to do more, to make even that which is darkness to our eyes light, and from vibrations which our organs of sight are powerless to perceive, to evolve a revelation in which we see mirrored some of the stages through which the stars may pass in their slow evolutional progress —surely the record of such achievements, however poor the form of words in which they may be described, is worthy to be regarded as the scientific epic of the present century.

I do not purpose to attempt a survey of the progress of spectroscopic astronomy from its birth at Heidelberg in 1859, but to point out what we do know at present, as distinguished from what we do not know, of a few only of its more important problems, giving a prominent place, in accordance with the traditions of this chair, to the work of the last year or two.

In the spectroscope itself advances have been made by Lord Rayleigh by his discussion of the theory of the instrument, and by Professor Rowland in the construction of concave gratings.

Lord Rayleigh has shown that there is not the necessary connection, sometimes supposed, between dispersion and resolving power, as, besides the prism or grating, other details of construction and of adjustment of a spectroscope must be taken into account.

The resolving power of the prismatic spectroscope is proportional to the length of path in the dispersive medium. For the heavy flint glass used in Lord Rayleigh's experiments the thickness necessary to resolve the sodium lines came out 1.02 centimetre. If this be taken as a unit, the resolving power of a prism of similar glass will be in the neighbourhood of the sodium lines equal to the number of centimetres of its thickness. In other parts of the spectrum the resolving power will vary inversely as the third power of the wave-length, so that it will be eight times as great in the violet as in the red. The resolving power of a spectroscope is therefore proportional to the total thickness of the dispersive material in use, irrespective of the number, the angles, or the setting of the separate prisms into which, for the sake of convenience, it may be distributed.

The resolving power of a grating depends upon the total number of lines on its surface and the order of spectrum in use, about one thousand lines being necessary to resolve the sodium lines in the first spectrum.

As it is often of importance in the record of observations to state the efficiency of the spectroscope with which they were made, Professor Schuster has proposed the use of a unit of purity as well as of resolving power, for the full resolving power of a spectroscope is realized in practice only when a sufficiently narrow slit is used. The unit of purity also is to stand for the separation of two lines differing by $\frac{1}{1000}$ of their own wave-length; about the separation of the sodium pair at D.

A further limitation may come in from the physiological fact that, as Lord Rayleigh has pointed out, the eye when its full aperture is used is not a perfect instrument. If we wish to realize the full resolving power of a spectroscope, therefore, the emergent beam must not be larger than about one third of the opening of the pupil.

Up to the present time the standard of reference for nearly all spectroscopic work continues to be Ångström's map of the solar spectrum, and his scale based upon his original determinations of absolute wave-length. It is well known, as was pointed out by Thalen in his work on the spectrum of iron in 1884, that Ångström's figures are slightly too small, in consequence of an error existing in a standard metre used by him. The corrections for this have been introduced into the tables of the wave-lengths of terrestrial spectra collected and revised by a committee of this association from 1885 to 1887. Last year the committee added a table of corrections to Rowland's scale.

The inconvenience caused by a change of standard scale is, for a time at least, considerable; but there is little doubt that in the near future Rowland's photographic map of the solar spectrum, and his scale based on the determinations of absolute wave-length by Peirce and Bell, or the Potsdam scale based on original determinations by Müller and Kempf, which differs very slightly from it, will come to be exclusively adopted.

The great accuracy of Rowland's photographic map is due chiefly to the introduction by him of concave gratings, and of a method for their use, by which the problem of the determination of relative wave-lengths is simplified to measures of near coincidences of the lines in different spectra by a micrometer.

The concave grating and its peculiar mounting, in which no lenses or telescope are needed, and in which all the spectra are in focus together, formed a new departure of great importance in the measurement of spectral lines. The valuable method of photographic sensitizers for different parts of the spectrum has enabled Professor Rowland to include in his map the whole visible solar spectrum, as well as the ultra-violet portion as far as it can get through our atmosphere. Some recent photographs of the solar spectrum, which include A, by Mr. George Higgs, are of great technical beauty.

During the past year the results of three independent researches have appeared, in which the special object of the observers has been to distinguish the lines which are due to our atmosphere from those which are truly solar—the maps of M. Thollon, which, owing to his lamented death just before their final completion, have assumed the character of a memorial to him; maps by Mr. Becker; and sets of photographs of a high and a low sun by Mr. McClean.

At the meeting of this association in Bath, Mr. Janssen gave an account of his own researches on the terrestrial lines of the solar spectrum, which owe their origin to the oxygen of our atmosphere. He discovered the remarkable fact that while the intensity of one class of bands varies as the density of the gas, other diffuse bands vary as the square of the density. These observations are in accordance with the work of Egoroff and of Olszewski, and of Liveing and Dewar on condensed oxygen. In some recent experiments Olszewski, with a layer of liquid oxygen thirty millimetres thick, saw, as well as four other bands, the band coincident with Fraunhofer's A; a remarkable instance of the persistence of absorption through a great range of temperature. The light which passed through the liquid oxygen had a light-blue colour resembling that of the sky.

Of not less interest are the experiments of Ångström which show that the carbonic acid and aqueous vapour of the atmosphere reveal their presence by dark bands in the invisible infra-red region, at the positions of bands of emission of these substances.

It is now some thirty years since the spectroscope gave us for the first time certain knowledge of the nature of the heavenly bodies, and revealed the fundamental fact that terrestrial matter is not peculiar to the solar system, but that it is common to all the stars which are visible to us.

In the case of a star, such as Capella, which has a spectrum almost identical with that of the sun, we feel justified in concluding that the matter of which it is built up is similar, and that its temperature is also high, and not very different from the solar temperature. The task of analyzing the stars and nebulæ becomes, however, one of very great difficulty when we have to do with spectra differing from the solar type. We are thrown back upon the laboratory for the information necessary to enable us to interpret the indications of the spectroscope as to the chemical nature, the density and pressure, and the temperature of the celestial masses.

What the spectroscope immediately reveals to us are the waves which were set up in the ether filling all interstellar space, years or hundreds of years ago, by the motion of the molecules of the celestial substances. As a rule it is only when a body is gaseous and sufficiently hot that the motions within its molecules can produce bright lines and a corresponding absorption. The spectra of the heavenly bodies are, indeed, to a great extent absorption spectra, but we have naturally to study them through the corresponding emission spectra of bodies brought into the gaseous form and rendered luminous by means of flames or of electric discharges. In both cases, unfortunately, it has been shown recently by Professors Liveing and Dewar, Wullner, E. Wiedemann, and others, that there appears to be no certain direct relation between the luminous radiation as shown in the spectroscope and the temperature of the flame, or of the gaseous contents of the vacuum tube—that is, in the usual sense of the term as applied to the mean motion of all the molecules. In both cases the vibratory motions within the molecules to which their luminosity is due are almost always much greater than would be produced by encounters of molecules having motions of translation no greater than the average motions which characterize the temperature of the gases as a whole. The temperature of a vacuum tube through which an electric discharge is taking place may be low, as shown thermometrically, quite apart from the consideration of the extreme smallness of the mass of gas, but the vibrations of the luminous molecules must be violent in whatever way we suppose them to be set up by the discharge; if we take Schuster's view that comparatively few molecules are carrying the discharge, and that it is to the fierce encounters of these alone that the luminosity is due, then if all the molecules had similar motions, the temperature of the gas would be very high.

So in flames where chemical changes are in progress, the vibratory motions of the molecules which are luminous may be, in connection with the energy set free in these changes, very different from those corresponding to the mean temperature of the flame. Under the ordinary conditions of terrestrial experiments, therefore, the temperature or the mean vis viva of the molecules may have no direct relation to the total radiation, which, on the other hand, is the sum of the radiation due to each luminous molecule. These phenomena have recently been discussed by Ebert from the standpoint of the electro-magnetic theory of light.

Very great caution is therefore called for when we attempt to reason by the aid of laboratory experiments to the temperature of the heavenly bodies from their radiation, especially on the reasonable assumption that in them the luminosity is not ordinarily associated with chemical changes or with electrical discharges, but is due to a simple glowing from the ultimate conversion into molecular motion of the gravitational energy of shrinkage.

In a recent paper Stas maintains that electric spectra are to be regarded as distinct from flame spectra, and, from researches of his own, that the pairs of lines of the sodium spectrum other than D are produced only by disruptive electric discharges. As these pairs of lines are found reversed in the solar spectrum, he concludes that the sun's radiation is due mainly to electric discharges. But Wolf and Diacon, and later Watts, observed the other pairs of lines of the sodium spectrum when the vapour was raised above the ordinary temperature of the Bunsen flame. Recently Liveing and Dewar saw easily, besides D, the citron and green pairs, and sometimes the blue pair and the orange pair, when hydrogen charged with sodium vapour was burning at different pressures in oxygen. In the case of sodium vapours, therefore, and presumably in all other vapours and gases, it is a matter of indifference whether the necessary vibratory motion of the molecules is produced by electric discharges or by flames. The presence of lines in the solar spectrum which we can only produce electrically is an indication, however, as Stas points out, of the high temperature of the sun.

We must not forget that the light from the heavenly bodies may consist of the combined radiations of different layers of gas at different temperatures, and possibly be further complicated to an unknown extent by the absorption of cooler portions of gas outside.

Not less caution is needed if we endeavour to argue from the broadening of lines and the coming in of a continuous spectrum as to the relative pressure of the gas in the celestial atmospheres. On the one hand, it can not be gainsaid that in the laboratory the widening of the lines in a Plucker's tube follows upon increasing the density of the residue of hydrogen in the tube, when the vibrations are more frequently disturbed by fresh encounters; and that a broadening of the sodium lines in a flame at ordinary pressure is produced by an increase of the quantity of sodium in the flame; but it is doubtful if pressure, as distinguished from quantity, does produce an increase of the breadth of the lines. An individual molecule of sodium will be sensibly in the same condition, considering the relatively enormous number of the molecules of the other gases, whether the flame is scantily or copiously fed with the sodium salt. With a small quantity of sodium vapour the intensity will be feeble except near the maximum of the lines; when, however, the quantity is increased, the comparative transparency on the sides of the maximum will allow the light from the additional molecules met with in the path of the visual ray to strengthen the radiation of the molecules farther back, and so increase the breadth of the lines.

In a gaseous mixture it is found, as a rule, that at the same pressure or temperature, as the encounters with similar molecules become fewer, the spectral lines will be affected as if the body were observed under conditions of reduced quantity or temperature.

In their recent investigation of the spectroscopic behaviour of flames under various pressures up to forty atmospheres, Professors Liveing and Dewar have come to the conclusion that though the prominent feature of the light emitted by flames at high pressure appears to be a strong continuous spectrum, there is not the slightest indication that this continuous spectrum is produced by the broadening of the lines of the same gases at low pressure. On the contrary, photometric observations of the brightness of the continuous spectrum, as the pressure is varied, show that it is mainly produced by the mutual action of the molecules of a gas. Experiments on the sodium spectrum were carried up to a pressure of forty atmospheres without producing any definite effect on the width of the lines which could be ascribed to the pressure. In a similar way the lines of the spectrum of water showed no signs of expansion up to twelve atmospheres; though more intense than at ordinary pressure, they remained narrow and clearly defined.

It follows, therefore, that a continuous spectrum can not be considered, when taken alone, as a sure indication of matter in the liquid or the solid state. Not only, as in the experiments already mentioned, such a spectrum may be due to gas when under pressure, but, as Maxwell pointed out, if the thickness of a medium, such as sodium vapour, which radiates and absorbs different kinds of light, be very great, and the temperature high, the light emitted will be of exactly the same composition as that emitted by lampblack at the same temperature, for the radiations which are feebly emitted will be also feebly absorbed, and can reach the surface from immense depths. Schuster has shown that oxygen, even in a partially exhausted tube, can give a continuous spectrum when excited by a feeble electric discharge.

Compound bodies are usually distinguished by a banded spectrum; but on the other hand such a spectrum does not necessarily show the presence of compounds—that is, of molecules containing different kinds of atoms—but simply of a more complex molecule, which may be made up of similar atoms, and be therefore an allotropic condition of the same body. In some cases, for example, in the diffuse bands of the absorption spectrum of oxygen, the bands may have an intensity proportional to the square of the density of the gas, and may be due either to the formation of more complex molecules of the gas with increase of pressure, or it may be to the constraint to which the molecules are subject during their encounters with one another.

It may be thought that at least in the coincidences of bright lines we are on the solid ground of certainty, since the length of the waves set up in the ether by a molecule, say of hydrogen, is the most fixed and absolutely permanent quantity in Nature, and is so of physical necessity, for with any alteration the molecule would cease to be hydrogen.

Such would be the case if the coincidence were certain; but an absolute coincidence can be only a matter of greater or less probability, depending on the resolving power employed, on the number of the lines which correspond and on their characters. When the coincidences are very numerous, as in the case of iron and the solar spectrum, or the lines are characteristically grouped, as in the case of hydrogen and the solar spectrum, we may regard the coincidence as certain; but the progress of science has been greatly retarded by resting important conclusions upon the apparent coincidence of single lines, in spectroscopes of very small resolving power. In such cases, unless other reasons supporting the coincidence are present, the probability of a real coincidence is almost too small to be of any importance, especially in the case of a heavenly body which may have a motion of approach or of recession of unknown amount.

But even here we are met by the confusion introduced by multiple spectra, corresponding to different molecular groupings of the same substance; and further, to the influence of substances in vapour upon each other; for when several gases are present together, the phenomena of radiation and reversal by absorption are by no means the same as if the gases were free from each other's influence, and especially is this the case when they are illuminated by an electric discharge.

I have said as much as time will permit, and I think indeed sufficient, to show that it is only by the laborious and slow process of most cautious observations that the foundations of the science of celestial physics can be surely laid. We are at present in a time of transition when the earlier and, in the nature of things, less precise observations are giving place to work of an order of accuracy much greater than was formerly considered attainable with objects of such small brightness as the stars.

The accuracy of the earlier determinations of the spectra of the terrestrial elements is in most cases insufficient for modern work on the stars as well as on the sun. They fall much below the scale adopted in Rowland's map of the sun, as well as below the degree of accuracy attained at Potsdam by photography in a part of the spectrum for the brighter stars. Increase of resolving power very frequently breaks up into groups, in the spectra of the sun and stars, the lines which had been regarded as single, and their supposed coincidences with terrestrial lines fall to the ground. For this reason many of the early conclusions, based on observations as good as it was possible to make at the time with the less powerful spectroscopes then in use, may not be found to be maintained under the much greater resolving power of modern instruments.

The spectroscope has failed as yet to interpret for us the remarkable spectrum of the aurora borealis. Undoubtedly in this phenomenon portions of our atmosphere are lighted up by electric discharges; we should expect, therefore, to recognise the spectra of the gases known to be present in it. As yet we have not been able to obtain similar spectra from these gases artificially, and especially we do not know the origin of the principal line in the green, which often appears alone, and may have therefore an origin independent of that of the other lines. Recently the suggestion has been made that the aurora is a phenomenon produced by the dust of meteors and falling stars, and that near positions of certain auroral lines to lines or flutings of manganese, lead, barium, thallium, iron, etc., are sufficient to justify us in regarding meteoric dust in the atmosphere as the origin of the auroral spectrum. Liveing and Dewar have made a conclusive research on this point by availing themselves of the dust of excessive minuteness thrown off from the surface of electrodes of various metals and meteorites by a disruptive discharge and carried forward into the tube of observation by a more or less rapid current of air or other gas. These experiments prove that metallic dust, however fine, suspended in a gas will not act like gaseous matter in becoming luminous with its characteristic spectrum in an electric discharge, similar to that of the aurora. Professor Schuster has suggested that the principal line may be due to some very light gas which is present in too small a proportion to be detected by chemical analysis or even by the spectroscope in the presence of the other gases near the earth, but which at the height of the auroral discharges is in a sufficiently greater relative proportion to give a spectrum. Lemstrom, indeed, states that he saw this line in the silent discharge of a Holtz machine on a mountain in Lapland. The lines may not have been obtained in our laboratories from the atmospheric gases, on account of the difficulty of reproducing in tubes with sufficient nearness the conditions under which the auroral discharges take place.

In the spectra of comets the spectroscope has shown the presence of carbon presumably in combination with hydrogen, and also sometimes with nitrogen; and in the case of comets approaching very near the sun, the lines of sodium, and other lines which have been supposed to belong to iron. Though the researches of Professor H. A. Newton and of Professor Schiaparelli leave no doubt of the close connection of comets with corresponding periodic meteor swarms, and therefore of the probable identity of cometary matter with that of meteorites, with which the spectroscopic evidence agrees, it would be perhaps unwise at present to attempt to define too precisely the exact condition of the matter which forms the nucleus of the comet. In any case the part of the light of the comet which is not reflected solar light can scarcely be attributed to a high temperature produced by the clashing of separate meteoric stones set up within the nucleus by the sun's disturbing force. We must look rather to disruptive electric discharges produced probably by processes of evaporation due to increased solar heat, which would be amply sufficient to set free portions of the occluded gases into the vacuum of space. May it be that these discharges are assisted, and indeed possibly increased, by the recently discovered action of the ultra-violet part of the sun's light? Hertz has shown that ultra-violet light can produce a discharge from a negatively electrified piece of metal, while Hallwachs and Righi have shown further that ultra-violet light can even charge positively an unelectrified piece of metal—phenomena which Lenard and Wolf associate with the disengagement from the metallic surfaces of very minute particles. Similar actions on cometary matter, unscreened as it is by an absorptive atmosphere, at least of any noticeable extent, may well be powerful when a comet approaches the sun, and help to explain an electrified condition of the evaporated matter which would possibly bring it under the sun's repulsive action. We shall have to return to this point in speaking of the solar corona.

A very great advance has been made in our knowledge of the constitution of the sun by the recent work at the Johns Hopkins University by means of photography and concave gratings, in comparing the solar spectrum, under the great resolving power, directly with the spectra of the terrestrial elements. Professor Rowland has shown that the lines of thirty-six terrestrial elements at least are certainly present in the solar spectrum, while eight others are doubtful. Fifteen elements, including nitrogen as it shows itself under an electric discharge in a vacuum tube, have not been found in the solar spectrum. Some ten other elements, inclusive of oxygen, have not yet been compared with the sun's spectrum.

Rowland remarks that of the fifteen elements named as not found in the sun, many are so classed because they have few strong lines, or none at all, in the limit of the solar spectrum as compared by him with the arc. Boron has only two strong lines. The lines of bismuth are compound and too diffuse. Therefore, even in the case of these fifteen elements, there is little evidence that they are really absent from the sun.

It follows that if the whole earth were heated to the temperature of the sun, its spectrum would resemble very closely the solar spectrum.

Rowland has not found any lines common to several elements, and in the case of some accidental coincidences more accurate investigation reveals some slight difference of wave-length or a common impurity. Further, the relative strength of the lines in the solar spectrum is generally, with a few exceptions, the same as that in the electric arc, so that Rowland considers that his experiments show "very little evidence" of the breaking up of the terrestrial elements in the sun.

Stas, in a recent paper, gives the final results of eleven years of research on the chemical elements in a state of purity, and on the possibility of decomposing them by the physical and chemical forces at our disposal. His experiments on calcium, strontium, lithium, magnesium, silver, sodium, and thallium show that these substances retain their individuality under all conditions, and are unalterable by any forces that we can bring to bear upon them.

Professor Rowland looks to the solar lines which are unaccounted for as a means of enabling him to discover such new terrestrial elements as still lurk in raw minerals and earths, by confronting their spectra directly with that of the sun. He has already resolved yttrium spectroscopically into three compounds, and actually into two. The comparison of the results of this independent analytical method with the remarkable but different conclusions to which M. Lecoq de Boisbaudran and Mr. Crookes have been led respectively, from spectroscopic observation on these bodies when glowing under molecular bombardment in a vacuum tube, will be awaited with much interest. It is worthy of remark that as our knowledge of the spectrum of hydrogen in its complete form came to us from the stars, it is now from the sun that chemistry is probably about to be enriched by the discovery of new elements.

In a discussion in the Bakerian lecture for 1885 of what we knew up to that time of the sun's corona, I was led to the conclusion that the corona is essentially a phenomenon similar in the cause of its formation to the tails of comets—namely, that it consists for the most part probably of matter going from the sun under the action of a force, possibly electrical, which varies as the surface, and can therefore in the case of highly attenuated matter easily master the force of gravity even near the sun. Though many of the coronal particles may return to the sun, those which form the long rays or streamers do not return; they separate and soon become too diffused to be any longer visible, and may well go to furnish the matter of the zodiacal light, which otherwise has not received a satisfactory explanation. And further, if such a force exist at the sun, the changes of terrestrial magnetism may be due to direct electric action, as the earth moves through lines of inductive force.

These conclusions seem to be in accordance broadly with the lines along which thought has been directed by the results of subsequent eclipses. Professor Schuster takes an essentially similar view, and suggests that there may be a direct electric connection between the sun and the planets. He asks further whether the sun may not act like a magnet in consequence of its revolution about its axis. Professor Bigelow has recently treated the coronal forms by the theory of spherical harmonics, on the supposition that we see phenomena similar to those of free electricity, the rays being lines of force, and the coronal matter discharged from the sun, or at least arranged or controlled by these forces. At the extremities of the streams for some reasons the repulsive power may be lost, and gravitation set in, bringing the matter back to the sun. The matter which does leave the sun is persistently transported to the equatorial plane of the corona; in fact, the zodiacal light may be the accumulation at great distances from the sun along this equator of similar material. Photographs on a larger scale will be desirable for the full development of the conclusions which may follow from this study of the curved forms of the coronal structure. Professor Schaeberle, however, considers that the coronal phenomena may be satisfactorily accounted for on the supposition that the corona is formed of streams of matter ejected mainly from the spot zones with great initial velocities, but smaller than 382 miles a second; further, that the different types of the corona are due to the effects of perspective on the streams from the earth's place at the time relatively to the plane of the solar equator.

Of the physical and the chemical nature of the coronal matter we know very little. Schuster concludes, from an examination of the eclipses of 1882, 1883, and 1886, that the continuous spectrum of the corona has the maximum of actinic intensity displaced considerably toward the red when compared with the spectrum of the sun, which shows that it can only be due in small part to solar light scattered by small particles. The lines of calcium and of hydrogen do not appear to form part of the normal spectrum of the corona. The green coronal line has no known representative in terrestrial substances, nor has Schuster been able to recognise any of our elements in the other lines of the corona.

The spectra of the stars are almost infinitely diversified, yet they can be arranged, with some exceptions, in a series in which the adjacent spectra, especially in the photographic region, are scarcely distinguishable, passing from the bluish-white stars like Sirius, through stars more or less solar in character, to stars with banded spectra, which divide themselves into two apparently independent groups, according as the stronger edge of the bands is toward the red or the blue. In such an arrangement the sun's place is toward the middle of the series.

At present a difference of opinion exists as to the direction in the series in which evolution is proceeding: whether by further condensation white stars pass into the orange and red stages, or whether these more coloured stars are younger and will become white by increasing age. The latter view was suggested by Johnstone Stoney in 1867.

About ten years ago Ritter, in a series of papers, discussed the behaviour of gaseous masses during condensation, and the probable resulting constitution of the heavenly bodies. According to him, a star passes through the orange and red stages twice, first during a comparatively short period of increasing temperature which culminates in the white stage, and a second time during a more prolonged stage of gradual cooling. He suggested that the two groups of banded stars may correspond to those different periods, the young stars being those in which the stronger edge of the dark band is toward the blue, the other banded stars, which are relatively less luminous and few in number, being those which are approaching extinction through age.

Recently a similar evolutional order has been suggested, which is based upon the hypothesis that the nebulæ and stars consist of colliding meteoric stones in different stages of condensation.

More recently the view has been put forward that the diversified spectra of the stars do not represent the stages of an evolutional progress, but are due for the most part to differences of original constitution.

The few minutes which can be given to this part of the address are insufficient for a discussion of these different views. I purpose, therefore, to state briefly, and with reserve, as the subject is obscure, some of the considerations from the characters of their spectra which appeared to me to be in favour of the evolutional order in which I arranged the stars from their photographic spectra in 1879. This order is essentially the same as Vogel had previously proposed in his classification of the stars in 1874, in which the white stars, which are most numerous, represent the early adult and most persistent stage of stellar life, the solar condition that of full maturity and of commencing age; while in the orange and red stars with banded spectra we see the setting in and advance of old age. But this statement must be taken broadly, and not as asserting that all stars, however different in mass and possibly to some small extent in original constitution, exhibit one invariable succession of spectra.

In the spectra of the white stars the dark metallic lines are relatively inconspicuous and occasionally absent, at the same time that the dark lines of hydrogen are usually strong, and more or less broad, upon a continuous spectrum, which is remarkable for its brilliancy at the blue end. In some of these stars the hydrogen and some other lines are bright and sometimes variable.

As the greater or less prominence of the hydrogen lines, dark or bright, is characteristic of the white stars as a class, and diminishes gradually with the incoming and increase in strength of the other lines, we are probably justified in regarding it as due to some conditions which occur naturally during the progress of stellar life and not to a peculiarity of original constitution.

To produce a strong absorption-spectrum a substance must be at the particular temperature at which it is notably absorptive; and, further, this temperature must be sufficiently below that of the region behind from which the light comes for the gas to appear, so far as its special rays are concerned, as darkness upon it. Considering the high temperature to which hydrogen must be raised before it can show its characteristic emission and absorption, we shall probably be right in attributing the relative feebleness or absence of the other lines, not to the paucity of the metallic vapours, but rather to their being so hot relatively to the substance behind them as to show feebly, if at all, by reversion. Such a state of things would more probably be found, it seems to me, in conditions anterior to the solar stage. A considerable cooling of the sun would probably give rise to banded spectra due to compounds, or to more complex molecules, which might form near the condensing points of the vapours.

The sun and stars are generally regarded as consisting of glowing vapours surrounded by a photosphere where condensation is taking place, the temperature of the photospheric layer from which the greater part of the radiation comes being constantly renewed from the hotter matter within.

At the surface the convection currents would be too strong, producing a considerable commotion, by which the different gases would be mixed and not allowed to retain the inequality of proportions at different levels due to their vapour densities.

Now, the conditions of the radiating photosphere and those of the gases above it, on which the character of the spectrum of a star depends, will be determined, not alone by the temperature, but also by the force of gravity in these regions; this force will be fixed by the star's mass and its stage of condensation, and will become greater as the star continues to condense.

In the case of the sun the force of gravity has already become so great at the surface that the decrease of the density of the bases must be extremely rapid, passing in the space of a few miles from atmospheric pressure to a density infinitesimally small; consequently the temperature gradient at the surface, if determined solely by expansion, must be extremely rapid. The gases here, however, are exposed to the fierce radiation of the sun, and unless wholly transparent would take up heat, especially if any solid or liquid particles were present from condensation or convection currents.

From these causes, within a very small extent of space at the surface of the sun, all bodies with which we are acquainted should fall to a condition in which the extremely tenuous gas could no longer give a visible spectrum. The insignificance of the angle subtended by this space as seen from the earth should cause the boundary of the solar atmosphere to appear defined. If the boundary which we see be that of the sun proper, the matter above it will have to be regarded as in an essentially dynamical condition—

an assemblage, so to speak, of gaseous projectiles for the most part falling back upon the sun after a greater or less range of flight. But in any case it is within a space of relatively small extent in the sun, and probably in the other solar stars, that the reversion which is manifested by dark lines is to be regarded as taking place.

Passing backward in the star's life, we should find a gradual weakening of gravity at the surface, a reduction of the temperature gradient so far as it was determined by expansion, and convection currents of less violence producing less interference with the proportional quantities of gases due to their vapour densities, while the effects of eruptions would be more extensive.

At last we might come to a state of things in which, if the stars were hot enough, only hydrogen might be sufficiently cool relatively to the radiation behind to produce a strong absorption. The lower vapours would be protected, and might continue to be relatively too hot for their lines to appear very dark upon the continuous spectrum; besides, their lines might be possibly to some extent effaced by the coming in under such conditions in the vapours themselves of a continuous spectrum.

In such a star the light radiated toward the upper part of the atmosphere may have come from portions lower down of the atmosphere itself, or at least from parts not greatly hotter. There may be no such great difference of temperature of the low and less low portions of the star's atmosphere as to make the darkening effect of absorption of the protected metallic vapours to prevail over the illuminating effect of their emission.

It is only by a vibratory motion corresponding to a very high temperature that the bright lines of the first spectrum of hydrogen can be brought out, and by the equivalence of absorbing and emitting power that the corresponding spectrum of absorption should be produced; yet for a strong absorption to show itself, the hydrogen must be cool relatively to the source of radiation behind it, whether this be condensed particles or gas. Such conditions, it seems to me, should occur in the earlier rather than in the more advanced stages of condensation.

The subject is obscure, and we may go wrong in our mode of conceiving of the probable progress of events, but there can be no doubt that in one remarkable instance the white-star spectrum is associated with an early stage of condensation.

Sirius is one of the most conspicuous examples of one type of this class of stars. Photometric observations, combined with its ascertained parallax, show that this star emits from forty to sixty times the light of our sun, even to the eye, which is insensible to ultra-violet light, in which Sirius is very rich, while we learn from the motion of its companion that its mass is not much more than double that of our sun. It follows that unless we attribute to this star an improbably great emissive power, it must be of immense size, and in a much more diffuse and therefore an earlier condition than our sun; though probably at a later stage than those white stars in which the hydrogen lines are bright.

A direct determination of the relative temperature of the photospheres of the stars might possibly be obtained in some cases from the relative position of maximum radiation of their continuous spectra. Langley has shown that through the whole range of temperature on which we can experiment, and presumably at temperatures beyond the maximum of radiation, power in solid bodies gradually shifts upward in the spectrum from the infra-red through the red and orange, and that in the sun it has reached the blue.

The defined character, as a rule, of the stellar lines of absorption suggests that the vapours producing them do not at the same time exert any strong power of general absorption. Consequently we should probably not go far wrong, when the photosphere consists of liquid or solid particles, if we could compare select parts of the continuous spectrum between the stronger lines or where they are fewest. It is obvious that if extended portions of different stellar spectra were compared, their true relation would be obscured by the line-absorption.

The increase of temperature, as shown by the rise in the spectrum of the maximum of radiation, may not always be accompanied by a corresponding greater brightness of a star as estimated by the eye, which is an extremely imperfect photometric instrument. Not only is the eye blind to large regions of radiations, but even for the small range of light that we can see, the visual effect varies enormously with its colour. According to Professor Langley, the same amount of energy which just enables us to perceive light in the crimson at A would in the green produce a visual effect 100,000 times greater. In the violet the proportional effect would be 1,600, in the blue 62,000, in the yellow 28,000, in the orange 14,000, and in the red 1,200. Captain Abney's recent experiments make the sensitiveness of the eye for the green near F to be 750 times greater than for red about C. It is for this reason, at least in part, that I suggested in 1864, and have since shown by direct observation, that the spectrum of the nebula in Andromeda, and presumably of similar nebulæ, is in appearance only wanting in the red.

The stage at which the maximum radiation is in the green, corresponding to the eye's greatest sensitiveness, would be that in which it could be most favourably measured by eye-photometry. As the maximum rose into the violet and beyond, the star would increase in visual brightness, but not in proportion to the increase of energy radiated by it.

The brightness of a star would be affected by the nature of the substance by which the light was chiefly emitted. In the laboratory solid carbon exhibits the highest emissive power. A stellar stage in which radiation comes, to a large extent, from a photosphere of the solid particles of this substance would be favourable for great brilliancy. Though the stars are built up of matter essentially similar to that of the sun, it does not follow that the proportion of the different elements is everywhere the same. It may be that the substances condensed in the photospheres of different stars may differ in their emissive powers, but probably not to a great extent.

All the heavenly bodies are seen by us through the tinted medium of our atmosphere. According to Langley, the solar stage of stars is not really yellow, but, even as gauged by our imperfect eyes, would appear bluish-white if we could free ourselves from the deceptive influences of our surroundings.

From these considerations it follows that we can scarcely infer the evolutional stages of the stars from a simple comparison of their eye-magnitudes. We should expect the white stars to be, as a class, less dense than the stars in the solar stage. As great mass might bring in the solar type of spectrum at a relatively earlier time, some of the brightest of these stars may be very massive and brighter than the sun—for example, the brilliant star Arcturus. For these reasons the solar stars should not only be denser than the white stars, but perhaps, as a class, surpass them in mass and eye-brightness.

It has been shown by Lane that, so long as a condensing gaseous mass remains subject to the laws of a purely gaseous body, its temperature will continue to rise.

The greater or less breadth of the lines of absorption of hydrogen in the white stars may be due to variations of the depth of the hydrogen in the line of sight, arising from the causes which have been discussed. At the sides of the lines the absorption and emission are feebler than in the middle, and would come out more strongly with a greater thickness of gas.

The diversities among the white stars are nearly as numerous as the individuals of the class. Time does not permit me to do more than to record that in addition to the three subclasses into which they have been divided by Vogel, Scheiner has recently investigated minor differences as suggested by the character of the third line of hydrogen near G. He has pointed out, too, that so far as his observations go the white stars in the constellation of Orion stand alone, with the exception of Algol, in possessing a dark line in the blue which has apparently the same position as a bright line in the great nebula of the same constellation; and Pickering finds in his photographs of the spectra of the stars dark lines corresponding to the principal lines of the bright-line stars, and the planetary nebulæ with the exception of the chief nebular line. The association of white stars with nebular matter in Orion, in the Pleiades, in the region of the Milky Way, and in other parts of the heavens, may be regarded as falling in with the view that I have taken.

In the stars possibly farther removed from the white class than our sun, belonging to the first division of Vogel's third class, which are distinguished by absorption bands with their stronger edge toward the blue, the hydrogen lines are narrower than in the solar spectrum. In these stars the density-gradient is probably still more rapid, the depth of hydrogen may be less, and possibly the hydrogen molecules may be affected by a larger number of encounters with dissimilar molecules. In some red stars with dark hydrocarbon bands the hydrogen lines have not been certainly observed; if they are really absent it may be because the temperature has fallen below the point at which hydrogen can exert its characteristic absorption; besides, some hydrogen will have united with the carbon. The coming in of the hydrocarbon bands may indicate a later evolutional stage, but the temperature may still be high, as acetylene can exist in the electric arc.

A number of small stars more or less similar to those which are known by the names of their discoverers, Wolf and Rayet, have been found by Pickering in his photographs. These are remarkable for several brilliant groups of bright lines, including frequently the hydrogen lines and the line D_3, upon a continuous spectrum strong in blue and violet rays, in which are also dark lines of absorption. As some of the bright groups appear in his photographs to agree in position with corresponding bright lines in the planetary nebulæ, Pickering suggests that these stars should be placed in one class with them, although the brightest nebular line is absent from these stars. The simplest conception of their nature would be that each star is surrounded by a nebula, the bright groups being due to gaseous matter outside the star. Mr. Roberts, however, has not been able to bring out any indication of nebulosity by prolonged exposure. The remarkable star η Argus may belong to this class of the heavenly bodies.

In the nebulæ the elder Herschel saw portions of the fiery mist or "shining fluid" out of which the heavens and the earth had been slowly fashioned. For a time this view of the nebulæ gave place to that which regarded them as external galaxies, cosmical "sand heaps," too remote to be resolved into separate stars; though, indeed, in 1858, Mr. Herbert Spencer showed that the observations of nebulæ up to that time were really in favour of an evolutional progress.

In 1864 I brought the spectroscope to bear upon them; the bright lines which flashed upon the eye showed the source of the light of a number of them to be glowing gas, and so restored these bodies to what is probably their true place, as an early stage of sidereal life.

At that early time our knowledge of stellar spectra was small. For this reason, partly, and probably also under the undue influence of theological opinions then widely prevalent, I unwisely wrote in my original papers in 1864 that "in these subjects we no longer have to do with a special modification of our own type of sun, but find ourselves in presence of objects possessing a distinct and peculiar plan of structure." Two years later, however, in a lecture before this association, I took a truer position. "Our views of the universe," I said, "are undergoing important changes; let us wait for more facts, with minds unfettered by any dogmatic theory, and therefore free to receive the obvious teaching, whatever it may be, of new observations."

Let us turn aside for a moment from the nebulæ in the sky to the conclusions to which philosophers had been irresistibly led by a consideration of the features of the solar system. We have before us in the sun and planets obviously not a haphazard aggregation of bodies, but a system resting upon a multitude of relations pointing to a common physical cause. From these considerations Kant and Laplace formulated the nebular hypothesis, resting it on gravitation alone, for at that time the science of the conservation of energy was practically unknown. These philosophers showed how, on the supposition that the space now occupied by the solar system was once filled by a vaporous mass, the formation of the sun and planets could be reasonably accounted for.

By a totally different method of reasoning, modern science traces the solar system backward step by step to a similar state of things at the beginning. According to Helmholtz, the sun's heat is maintained by the contraction of his mass, at the rate of about 220 feet a year. Whether at the present time the sun is getting hotter or colder we do not certainly know. We can reason back to the time when the sun was sufficiently expanded to fill the whole space occupied by the solar system, and was reduced to a great glowing nebula. Though man's life, the life of the race, perhaps, is too short to give us direct evidence of any distinct stages of so august a process, still the probability is great that the nebular hypothesis, especially in the more precise form given to it by Roche, does express broadly, notwithstanding some difficulties, the succession of events through which the sun and planets have passed.

The nebular hypothesis of Laplace requires a rotating mass of fluid which at successive epochs became unstable from excess of motion, and left behind rings, or more probably perhaps lumps of matter, from the equatorial regions.

The difficulties to which I have referred have suggested to some thinkers a different view of things, according to which it is not necessary to suppose that one part of the system gravitationally supports another. The whole may consist of a congeries of discrete bodies, even if these bodies be the ultimate molecules of matter. The planets may have been formed by the gradual accretion of such discrete bodies. On the view that the material of the condensing solar system consisted of separate particles or masses, we have no longer the fluid pressure which is an essential part of Laplace's theory. Faye, in his theory of evolution from meteorites, has to throw over this fundamental idea of the nebular hypothesis, and he formulates instead a different succession of events in which the outer planets were formed last—a theory which has difficulties of its own.

Professor George Darwin has recently shown, from an investigation of the mechanical condition of a swarm of meteorites, that on certain assumptions a meteoric swarm might behave as a coarse gas, and in this way bring back the fluid pressure exercised by one part of the system on the other, which is required by Laplace's theory. Our chief assumption consists in supposing that such inelastic bodies as meteoric stones might attain the effective elasticity of a high order which is necessary to the theory through the sudden volatilization of a part of their mass at an encounter, by which what is virtually a violent explosive is introduced between the two colliding stones. Professor Darwin is careful to point out that it must necessarily be obscure as to how small a mass of solid matter can take up a very large amount of energy in a small fraction of a second.

Any direct indications from the heavens themselves, however slight, are of so great value that I should perhaps in this connection call attention to a recent remarkable photograph by Mr. Roberts of the great nebula in Andromeda. On this plate we seem to have presented to us some stage of cosmical evolution on a gigantic scale. The photograph shows a sort of whirlpool disturbance of the luminous matter which is distributed in a plane inclined to the line of sight, in which a series of rings of bright matter separated by dark spaces, greatly foreshortened by perspective, surround a large undefined central mass. The parallax of this nebula has not been ascertained, but there can be little doubt that we are looking upon a system very remote, and therefore of a magnitude great beyond our power of adequate comprehension. The matter of this nebula, in whatever state it may be, appears to be distributed, as in so many other nebulæ, in rings or spiral streams, and to suggest a stage in a succession of evolutional events not inconsistent with that which the nebular hypothesis requires. To liken this object more directly to any particular stage in the formation of the solar system would be to compare things great with small, and might be indeed to introduce a false analogy; but, on the other hand, we should err through an excess of caution if we did not accept the remarkable features brought to light by this photograph as a presumptive indication of a progress of events in cosmical history following broadly upon the lines of Laplace's theory.

The old view of the original matter of the nebulæ, that it consisted of a "fiery mist"—

> ". . . a tumultuous cloud
> Instinct with fire and nitre"—

fell at once with the rise of the science of thermodynamics. In 1854 Helmholtz showed that the supposition of an original fiery condition of the nebulous stuff was unnecessary, since in the mutual gravitation of widely separated matter we have a store of potential energy sufficient to generate the high temperature of the sun and stars. We can scarcely go wrong in attributing the light of the nebulæ to the conversion of the gravitational energy of shrinkage into molecular motion.

The idea that the light of comets and of nebulæ may be due to a succession of ignited flashes of gas from the encounters of meteoric stones was suggested by Professor Tait, and was brought to the notice of this association in 1871 by Sir William Thomson in his presidential address.

The spectrum of the bright-line nebulæ is certainly not such a spectrum as we should expect from the flashing by collisions of meteorites similar to those which have been analyzed in our laboratories. The strongest lines of the substances which in the case of such meteorites would first show themselves—iron, sodium, magnesium, nickel, etc.—

are not those which distinguish the nebular spectrum. On the contrary, this spectrum is chiefly remarkable for a few brilliant lines, very narrow and defined upon a background of a faint continuous spectrum, which contains numerous bright lines, and probably some lines of absorption.

The two most conspicuous lines have not been interpreted; for though the second line falls near, it is not coincident with a strong double line of iron. It is hardly necessary to say that though the near position of the brightest line to the bright double line of nitrogen, as seen in a small spectroscope in 1864, naturally suggested at that early time the possibility of the presence of this element in the nebulæ, I have been careful to point out, to prevent misapprehension, that in more recent years the nitrogen line and subsequently a lead line have been employed by me solely as fiducial points of reference in the spectrum.

The third line we know to be the second line of the first spectrum of hydrogen. Mr. Keeler has seen the first hydrogen line in the red, and photographs show that this hydrogen spectrum is probably present in its complete form, or nearly so, as we first learned to know it in the absorption spectrum of the white stars.

We are not surprised to find associated with it the line D_3 near the position of the absent sodium lines, probably due to the atom of some unknown gas, which in the sun can only show itself in the outbursts of highest temperature, and for this reason does not reveal itself by absorption in the solar spectrum.

It is not unreasonable to assume that the two brightest lines, which are of the same order as the third line, are produced by substances of a similar nature, in which a vibratory motion corresponding to a very high temperature is also necessary. These substances, as well as that represented by the line D_3, may be possibly some of the unknown elements which are wanting in our terrestrial chemistry between hydrogen and lithium, unless, indeed, D_3 be on the lighter side of hydrogen.

In the laboratory we must have recourse to the electric discharge to bring out the spectrum of hydrogen; but in a vacuum tube, though the radiation may be great, from the relative fewness of the luminous atoms or molecules or from some other cause, the temperature of the gas as a whole may be low.

On account of the large extent of the nebulæ, a comparatively small number of luminous molecules or atoms would probably be sufficient to make the nebulæ as bright as they appear to us. On such an assumption the average temperature may be low, but the individual particles, which by their encounters are luminous, must have motions corresponding to a very high temperature, and in this sense be extremely hot.

In such diffuse masses, from the great mean length of free path, the encounters would be rare but correspondingly violent, and tend to bring about vibrations of comparatively short period, as appears to be the case if we may judge by the great relative brightness of the more refrangible lines of the nebular spectrum.

Such a view may perhaps reconcile the high temperature which the nebular spectrum undoubtedly suggests with the much lower mean temperature of the gaseous mass, which we should expect at so early a stage of condensation, unless we assume a very enormous mass; or that the matter coming together had previously considerable motion, or considerable molecular agitation.

If the hydrogen shown by the spectroscope in the nebulæ and in the atmospheres of the stars is retained by the bodies, we should be able to assign approximately an inferior limit for the force of gravity at their surfaces, provided we assume that the gas is in the uncombined state, and always exists in some greater proportion than in the free space about them.

The inquisitiveness of the human mind does not allow us to remain content with the interpretation of the present state of the cosmical masses, but suggests the question—

> "What see'st thou else
> In the dark backward and abysm of time?"

What was the original state of things? how has it come about that by the side of aging worlds we have nebula in a relatively younger stage? Have any of them received their birth from dark suns, which have collided into new life, and so belong to a second or later generation of the heavenly bodies?

During the short historic period, however, there is no record of such an event; still, it would seem to be only through the collision of dark suns, of which the number must be increasing, that a temporary rejuvenescence of the heavens is possible, and by such ebbings and flowings of stellar life that the inevitable end to which evolution in its apparently uncompensated progress is carrying us can, even for a little, be delayed.

We can not refuse to admit as possible such an origin for nebulæ. In considering, however, the formation of the existing nebulæ, we must bear in mind that, in the part of the heavens within our ken, the stars still in the early and middle stages of evolution exceed greatly in number those which appear to be in an advanced condition of condensation. Indeed, we find some stars which may be regarded as not far advanced beyond the nebular condition.

It may be that the cosmical bodies which are still nebulous owe the lateness of their development to some conditions of the part of space where they occur, such as conceivably a greater original homogeneity, in consequence of which condensation began less early. In other parts of space condensation may have been still further delayed or even have not yet begun. It is worthy of remark that these nebulæ group themselves about the Milky Way, where we find a preponderance of the white-star type of stars, and almost exclusively the bright-line stars which Pickering associates with the planetary nebulæ. Further, Dr. Gill concludes, from the rapidity with which they impress themselves upon the plate, that the fainter stars of the Milky Way also, to a large extent, belong to this early type of stars. At the same time other types of stars occur also over this region, and the red hydrocarbon stars are found in certain parts: but possibly these stars may be before or behind the Milky Way, and not physically connected with it.

If light matter be suggested by the spectrum of these nebulæ, it may be asked further, as a pure speculation, whether in them we are witnessing possibly a later condensation of the light matter which had been left behind, at least in a relatively greater proportion, after the first growth of the worlds into which the heavier matter condensed, though not without some entanglement of the higher substances. The wide extent and great diffuseness of this bright-line nebulosity over a large part of the constellation of Orion may be regarded perhaps as pointing in this direction. The diffuse nebulous matter streaming round the Pleiades may possibly be another instance, though the character of its spectrum has not yet been ascertained.

In the planetary nebulæ, as a rule, there is a sensible increase of the faint continuous spectrum, as well as a slight thickening of the bright lines toward the centre of the nebula, appearances which are in favour of the view that these bodies are condensing gaseous masses.

Professor George H. Darwin, in his investigation of the equilibrium of a rotating mass of fluid, found, in accordance with the independent researches of Poincare, that when a portion of the central body becomes detached through increasing angular velocity, the portion should bear a far larger ratio to the remainder than is observed in the planets and satellites of the solar system; even taking into account heterogeneity from the condensation of the parent mass.

Now this state of things, in which the masses, though not equal, are of the same order, does seem to prevail in many nebulæ, and to have given birth to a large class of binary stars. Mr. See has recently investigated the evolution of bodies of this class, and points out their radical differences from the solar system in the relatively large mass-ratios of the component bodies, as well as the high eccentricities of their orbits brought about by tidal friction which would play a more important part in the evolution of such a system. Considering the large number of these bodies, he suggests that the solar system should perhaps no longer be regarded as representing celestial evolution in its normal form—

> "A goodly Paterne to whose perfect mould
> He fashioned them . . ."

but rather as modified by conditions which are exceptional.

It may well be that in the very early stages condensing masses are subject to very different conditions, and that condensation may not always begin at one or two centres, but sometimes set in at a large number of points, and proceed in the different cases along very different lines of evolution. Besides its more direct use in the chemical analysis of the heavenly bodies, the spectroscope has given to us a great and unexpected power of advance along the lines of the older astronomy. In the future a higher value may, indeed, be placed upon this indirect use of the spectroscope than upon its chemical revelations.

By no direct astronomical methods could motions of approach or of recession of the stars be even detected, much less could they be measured. A body coming directly toward us or going directly from us appears to stand still. In the case of the stars we can receive no assistance from change of size or of brightness. The stars show no true disks in our instruments, and the nearest of them is so far off that if it were approaching us at the rate of a hundred miles in a second of time, a whole century of such rapid approach would not do more than increase its brightness by the one-fortieth part.

Still, it was only too clear that, so long as we were unable to ascertain directly those components of the stars' motions which lie in the line of sight, the speed and direction of the solar motion in space, and many of the great problems of the constitution of the heavens, must remain more or less imperfectly known. Now, the spectroscope has placed in our hands this power, which, though so essential, appeared almost in the nature of things to lie forever beyond our grasp; it enables us to measure directly and under favourable circumstances to within a mile per second, or even less, the speed of approach or of recession of a heavenly body. This method of observation has the great advantage for the astronomer of being independent of the distance of the moving body, and is therefore as applicable and as certain in the case of a body on the extreme confines of the visible universe, so long as it is bright enough, as in the case of a neighbouring planet.

Doppler had suggested as far back as 1841 that the same principle, on which he had shown that a sound should become sharper or flatter if there were an approach or a recession between the ear and the source of the sound, would apply equally to light; and he went on to say that the difference of colour of some of the binary stars might be produced in this way by their motions. Doppler was right in that the principle is true in the case of light, but he was wrong in the particular conclusion which he drew from it. Even if we suppose a star to be moving with a sufficiently enormous velocity to alter sensibly its colour to the eye, no such change would actually be seen, for the reason that the store of invisible light beyond both limits of the visible spectrum, the blue and the red, would be drawn upon, and the light-waves invisible to us would be exalted or degraded so as to take the place of those raised or lowered in the visible region, and the colour of the star would remain unchanged. About eight years later Fizeau pointed out the importance of considering the individual wave-lengths of which white light is composed. It is, indeed, Doppler's principle which underlies the early determination of the velocity of light by Roemer; but this method, in its converse form, can scarcely be regarded as of practical value for the motions in the line of sight of binary stars. As soon, however, as we had learned to recognise the lines of known substances in the spectra of the heavenly bodies, Doppler's principle became applicable as the basis of a new and most fruitful method of investigation. The measurement of the small shift of the celestial lines from their true positions, as shown by the same lines in the spectrum of a terrestrial substance, gives to us the means of ascertaining directly in miles per second the speed of approach or of recession of the heavenly body from which the light has come.

An account of the first application of this method of research to the stars, which was made in my observatory in 1868, was given by Sir Gabriel Stokes from this chair at the meeting at Exeter in 1869. The stellar motions determined by me were afterward confirmed by Professor Vogel in the case of Sirius, and in the case of other stars by Mr. Christie, now Astronomer Royal, at Greenwich; but necessarily, in consequence of the inadequacy of the instruments then in use for so delicate an inquiry, the amounts of these motions were but approximate.

The method was shortly afterward taken up systematically at Greenwich and at the Rugby Observatory. It is to be greatly regretted that, for some reasons, the results have not been sufficiently accordant and accurate for a research of such exceptional delicacy. On this account, probably, as well as that the spectroscope at that early time had scarcely become a familiar instrument in the observatory, astronomers were slow in availing themselves of this new and remarkable power of investigation. That this comparative neglect of so truly wonderful a method of ascertaining what was otherwise outside our powers of observation has greatly retarded the progress of astronomy during the last fifteen years is but too clearly shown by the brilliant results which within the last two years have followed fast upon the recent masterly application of this method by photography at Potsdam, and by eye with the needful accuracy at the Lick Observatory. At last this use of the spectroscope has taken its true place as one of the most potent methods of astronomical research. It gives us the motions of approach and of recession, not in angular measures, which depend for their translation into actual velocities upon separate determinations of parallactic displacements, but at once in terrestrial units of distance.

This method of work will doubtless be very prominent in the astronomy of the near future, and to it probably we shall have to look for the more important discoveries in sidereal astronomy which will be made during the coming century.

In his recent application of photography to this method of determining celestial motions, Professor Vogel, assisted by Dr. Scheiner, considering the importance of obtaining the spectrum of as many stars as possible on an extended scale without an exposure inconveniently long, wisely determined to limit the part of the spectrum on the plate to the region for which the ordinary silver-bromide gelatine plates are most sensitive—namely, to a small distance on each side of G, and to employ as the line of comparison the hydrogen line near G, and recently also certain lines of iron. The most minute and complete mechanical arrangements were provided for the purpose of securing the absolute rigidity of the comparison spectrum relatively to that of the star, and for permitting temperature adjustments and other necessary ones to be made.

The perfection of these spectra is shown by the large number of lines, no fewer than 250 in the case of Capella, within the small region of the spectrum on the plate. Already the motions of about fifty stars have been measured with an accuracy, in the case of the large number of them, of about an English mile per second.

At the Lick Observatory it has been shown that observations can be made directly by eye with an accuracy equally great. Mr. Keeler's brilliant success has followed in great measure from the use of the third and fourth spectra of a grating with 14,438 lines to the inch. The marvellous accuracy attainable in his hands on a suitable star is shown by observations on three nights of the star Arcturus, the greatest divergence of his measures being not greater than six tenths of a mile per second, while the mean of the three nights' work agreed with the mean of five photographic determinations of the same star at Potsdam to within one tenth of an English mile. These are determinations of the motions of a sun so stupendously remote that even the method of parallax practically fails to fathom the depth of intervening space, and by means of light-waves which have been, according to Elkin's nominal parallax, nearly two hundred years upon their journey.

Mr. Keeler with his magnificent means has accomplished a task which I attempted in vain in 1874, with the comparatively poor appliances at my disposal, of measuring the motions in the line of sight of some of the planetary nebulæ. As the stars have considerable motions in space, it was to be expected that nebulæ should possess similar motions, for the stellar motions must have belonged to the nebulæ out of which they have been evolved. My instrumental means, limiting my power of detection to motions greater than twenty-five miles per second, were insufficient. Mr. Keeler has found in the examination of ten nebulæ motions varying from two miles to twenty-seven miles, with one exceptional motion of nearly forty miles.

For the nebula of Orion, Mr. Keeler finds a motion of recession of about ten miles a second. Now, this motion agrees with what it should appear to have from the drift of the solar system itself, so far as it has been possible at present to ascertain the probable velocity of the sun in space. This grand nebula, of vast extent and of extreme tenuity, is probably more nearly at rest relatively to the stars of our system than any other celestial object we know; still, it would seem more likely that even here we have some motion, small though it may be, than that the motions of the matter of which it is formed were so absolutely balanced as to leave this nebula in the unique position of absolute immobility in the midst of whirling and drifting suns and systems of suns.

The spectroscopic methods of determining celestial motions in the line of sight has recently become fruitful in a new but not altogether unforeseen direction, for it has, so to speak, given us a separating power far beyond that of any telescope the glass-maker and the optician could construct, and so enabled us to penetrate into mysteries hidden in stars apparently single, and altogether unsuspected of being binary systems. The spectroscope has not simply added to the list of the known binary stars, but has given to us for the first time a knowledge of a new class of stellar systems, in which the components are in some cases of nearly equal magnitude and in close proximity, and are revolving with velocities greatly exceeding the planetary velocities of our system.

The K line in the photographs of Mizar, taken at the Harvard College Observatory, was found to be double at intervals of fifty-two days. The spectrum was therefore not due to a single source of light, but to the combined effect of two stars moving periodically in opposite directions in the line of sight. It is obvious that if two stars revolve round their common centre of gravity in a plane not perpendicular to the line of sight, all the lines in a spectrum common to the two stars will appear alternately single or double.

In the case of Mizar and the other stars to be mentioned, the spectroscopic observations are not as yet extended enough to furnish more than an approximate determination of the elements of their orbits.

Mizar especially, on account of its relatively long period, about 105 days, needs further observations. The two stars are moving each with a velocity of about 50 miles a second, probably in elliptical orbits, and are about 143,000,000 miles apart. The stars of about equal brightness have together a mass about forty times as great as that of our sun.

A similar doubling of the lines showed itself in the Harvard photographs of β Aurigæ at the remarkably close interval of almost exactly two days, indicating a period of revolution of about four days. According to Vogel's later observations, each star has a velocity of nearly 70 miles a second, the distance between the stars being little more than 7,500,000 miles, and the mass of the system 4.7 times that of the sun. The system is approaching us at the speed of about 16 miles a second.

The telescope could never have revealed to us double stars of this order. In the case of β Aurigæ, combining Vogel's distance with Pritchard's recent determination of the star's parallax, the greatest angular separation of the stars as seen from the earth would be $\frac{1}{200}$ part of a second of arc, and therefore very far too small for detection by the largest telescopes. If we take the relation of aperture to separating power usually accepted, an object-glass of about 80 feet in diameter would be needed to resolve this binary star. The spectroscope which takes no note of distance magnifies, so to speak, this minute angular separation 4,000 times; in other words, the doubling of the lines, which is the phenomenon that we have to observe, amounts to the easily measurable quantity of twenty seconds of arc.

There were known, indeed, variable stars of short period, which it had been suggested might be explained on the hypothesis of a dark body revolving about a bright sun in a few days; but this theory was met by the objection that no such systems of closely revolving suns were known to exist.

The Harvard photographs of which we have been speaking were taken with a slitless form of spectroscope, the prisms being placed, as originally by Fraunhofer, before the object-glass of the telescope. This method, though it possesses some advantages, has the serious drawback of not permitting a direct comparison of the star's spectrum with the terrestrial spectra. It is obviously unsuited to a variable star like Algol, where one star only is bright, for in such a case there would be no doubling of the lines, but only a small shift to and fro in the spectrum of the lines of the bright star as it moved in its orbit alternately toward and from our system, which would need for its detection the fiducial positions of terrestrial lines compared directly with them.

For such observations the Potsdam spectrograph was well adapted. Professor Vogel found that the bright star Algol did oscillate backward and forward in the visual direction in a period corresponding to the known variation of its light. The explanation which had been suggested for the star's variability, that it was partially eclipsed at regular intervals of 68.8 hours by a dark companion large enough to cut off nearly five sixths of its light, was therefore the true one. The dark companion, no longer able to hide itself by its obscureness, was brought out into the light of direct observation by means of its gravitational effects.

Seventeen hours before minimum Algol is receding at the rate of about 24½ miles a second, while seventeen hours after minimum it is found to be approaching with a speed of about 28½ miles. From these data, together with those of the variation of its light, Vogel found, on the assumption that both stars have the same density, that the companion, nearly as large as the sun but with about one fourth his mass, revolves with a velocity of about 55 miles a second. The bright star, of about twice the size and mass, moves about the common centre of gravity with the speed of about 26 miles a second. The system of the two stars, which are about 3,250,000 miles apart, considered as a whole, is approaching us with a velocity of 24 miles a second. The great difference in luminosity of the two stars, not less than fifty times, suggests rather that they are in different stages of condensation, and dissimilar in density.

It is obvious that if the orbit of a star with an obscure companion is sufficiently inclined to the line of sight, the companion will pass above or below the bright star and produce no variation of its light. Such systems may be numerous in the heavens. In Vogel's photographs, Spica, which is not variable, by a small shifting of its lines reveals a backward and forward periodical pulsation due to orbital motion. As the pair whirl round their common centre of gravity, the bright star is sometimes advancing, at others receding. They revolve in about four days, each star moving with a velocity of about 56 miles a second in an orbit probably nearly circular, and possess a combined mass of rather more than two and a half times that of the sun. Taking the most probable value for the star's parallax, the greatest angular separation of the stars would be far too small to be detected with the most powerful telescopes.

If in a close double star the fainter companion is of the white-star type, while the bright star is solar in character, the composite spectrum would be solar with the hydrogen lines unusually strong. Such a spectrum would in itself afford some probability of a double origin, and suggest the existence of a companion star. In the case of a true binary star the orbital motions of the pair would reveal themselves in a small periodical swaying of the hydrogen lines relatively to the solar ones.

Professor Pickering considers that his photographs show ten stars with composite spectra; of these, five are known to be double. The others are: τ Persei, ζ Aurigæ, δ Sagittarii, 31 Ceti, and β Capricorni. Perhaps β Kyræ should be added to this list.

In his recent classical work on the rotation of the sun, Duner has not only determined the solar rotation for the equator, but for different parallels of latitude up to 75°. The close accordance of his results shows that these observations are sufficiently accurate to be discussed with the variation of the solar rotation for different latitudes, which had been determined by the older astronomical methods from the observations of the solar spots.

Though I have already spoken incidentally of the invaluable aid which is furnished by photography in some of the applications of the spectroscope to the heavenly bodies, the new power which modern photography has put into the hands of the astronomer is so great, and has led already, within the last few years, to new acquisitions or knowledge of such vast importance that it is fitting that a few sentences should be specially devoted to this subject.

Photography is no new discovery, being about half a century old; it may excite surprise, and indeed possibly suggest some apathy on the part of astronomers, that though the suggestion of the application of photography to the heavenly bodies dates from the memorable occasion when, in 1839, Arago, announcing to the Académie des Sciences the great discovery of Niepce and Daguerre, spoke of the possibility of taking pictures of the sun and moon by the new process, yet that it is only within a few years that notable advances in astronomical methods and discovery have been made by its aid.

The explanation is to be found in the comparative unsuitability of the earlier photographic methods for use in the observatory. In justice to the early workers in astronomical photography, among whom Bond, De la Rue, J. W. Draper, Rutherfurd, and Gould hold a foremost place, it is needful to state clearly that the recent great successes in astronomical photography are not due to greater skill, nor, to any great extent, to superior instruments, but to the very great advantages which the modern gelatine dry plate possesses for use in the observatory over the methods of Daguerre, and even over the wet collodion film on glass which, though a great advance on the silver plate, went but a little way toward putting into the hands of the astronomer a photographic surface adapted fully to his wants.

The modern silver-bromide gelatine plate, except for its grained texture, meets the needs of the astronomer at all points. It possesses extreme sensitiveness; it is always ready for use; it can be placed in any position; it can be exposed for hours; lastly, it does not need immediate development, and for this reason can be exposed again to the same object on succeeding nights, so as to make up by several instalments, as the weather may permit, the total time of exposure which is deemed necessary.

Without the assistance of photography, however greatly the resources of genius might overcome the optical and mechanical difficulties of constructing large telescopes, the astronomer would have to depend in the last resource upon his eye. Now, we can not by the force of continued looking bring into view an object too feebly luminous to be seen at the first and keenest moment of vision. But the feeblest light which falls upon the plate is not lost, but it is taken in and stored up continuously. Each hour the plate gathers up 3,600 times the light energy which is received during the first second. It is by this power of accumulation that the photographic plate may be said to increase, almost without limit, though not in separating power, the optical means at the disposal of the astronomer for the discovery or the observation of faint objects.

Two principal directions may be pointed out in which photography is of great service to the astronomer. It enables him within the comparatively short time of a single exposure to secure permanently, with great exactness, the relative positions of hundreds or even of thousands of stars, or the minute features of nebulæ or other objects, or the phenomena of a passing eclipse, tasks which by means of the eye and hand could only be accomplished, if at all, after a very great expenditure of time and labour. Photography puts it in the power of the astronomer to accomplish in the short span of his own life, and so enter into their fruition, great works which otherwise must have been passed on by him as a heritage of labour to succeeding generations.

The second great service which photography renders is not simply an aid to the powers the astronomer already possesses. On the contrary, the plate, by recording light-waves which are both too small and too large to excite vision in the eye, brings him into new regions of knowledge, such as the infra-red and the ultra-violet parts of the spectrum, which must have remained forever unknown but for artificial help.

The present year will be memorable in astronomical history for the practical beginning of the "Photographic Chart and Catalogue of the Heavens," which took their origin in an International Conference which met in Paris, in 1887, by the invitation of M. l'Amiral Mouchex, director of the Paris Observatory.

The richness in stars down to the ninth magnitude of the photographs of the comet of 1882, taken at the Cape Observatory under the superintendence of Dr. Gill, and the remarkable star charts of the brothers Henry which followed two years later, astonished the astronomical world. The great excellence of these photographs, which was due mainly to the superiority of the gelatine plate, suggested to these astronomers a complete map of the sky, and a little later gave birth in the minds of the Paris astronomers to the grand enterprise of an "International Chart of the Heavens." The actual beginning of the work this year is in no small degree due to the great energy and tact with which the director of the Paris Observatory has conducted the initial steps, through the many delicate and difficult questions which have unavoidably presented themselves in an undertaking which depends upon the harmonious working in common of many nationalities, and of no fewer than eighteen observatories in all parts of the world. The three years since 1887 have not been too long for the detailed organization of this work, which has called for several elaborate preliminary investigations on special points in which our knowledge was insufficient, and which have been ably carried out by Professors Vogel and Bakhuyzen, Dr. Trepied, Dr. Scheiner, Dr. Gill, the Astronomer Royal, and others. Time also was required for the construction of the new and special instruments.

The decisions of the conference in their final form provide for the construction of a great photographic chart of the heavens, with exposures corresponding to forty minutes' exposure at Paris, which it is expected will reach down to stars of about the fourteenth magnitude. As each plate is to be limited to four square degrees, and as each star, to avoid possible errors, is to appear on two plates, over 22,000 photographs will be required. For the more accurate determination of the positions of the stars, a réseau with lines at distances of five millimetres apart is to be previously impressed by a faint light upon the plate, so that the image of the réseau will appear together with the images of the stars when the plate is developed. This great work will be divided, according to their latitudes, among eighteen observatories provided with similar instruments, though not necessarily constructed by the same maker. Those in the British dominions and at Tacubaya have been constructed by Sir Howard Grubb.

Besides the plates to form the great chart, a second set of plates for a catalogue is to be taken, with a shorter exposure, which will give stars to the eleventh magnitude only. These plates, by a recent decision of the permanent committee, are to be pushed on as actively as possible, though, as far as may be practicable, plates for the charts are to be taken concurrently. Photographing the plates for the catalogue is but the first step in this work, and only supplies the data for the elaborate measurements which have to be made, which are, however, less laborious than would be required for a similar catalogue without the aid of photography.

Already Dr. Gill has nearly brought to conclusion, with the assistance of Professor Kapteyn, a preliminary photographic survey of the southern heavens.

With an exposure sufficiently long for the faintest stars to impress themselves upon the plate, the accumulating action still goes on for the brighter stars, producing a great enlargement of their images from optical and photographic causes. The question has occupied the attention of many astronomers whether it is possible to find a law connecting the diameters of these more or less over-exposed images with the relative brightness of the stars themselves. The answer will come out undoubtedly in the affirmative, though at present the empirical formulæ which have been suggested for this purpose differ from each other. Captain Abney proposes to measure the total photographic action, including density as well as size, by the obstruction which the stellar image offers to light.

A further question follows as to the relation which the photographic magnitudes of stars bears to those determined by eye. Visual magnitudes are the physiological expression of the eye's integration of that part of the star's light which extends from the red to the blue. Photographic magnitudes represent the plate's integration of another part of the star's light—namely, from a little below where the power of the eye leaves off in the blue to where the light is cut off by the glass, or is greatly reduced by want of proper corrections when a refracting telescope is used. It is obvious that the two records are taken by different methods in dissimilar units of different parts of the star's light. In the case of certain coloured stars the photographic brightness is very different from the visual brightness; but in all stars changes, especially of a temporary character, may occur in the photographic or the visual region, unaccompanied by similar changes in the other part of the spectrum. For these reasons it would seem desirable that the two sets of magnitudes should be tabulated independently, and be regarded as supplementary of each other.

The determination of the distances of the fixed stars from the small apparent shift of their positions when viewed from widely separated positions of the earth in its orbit is one of the most refined operations of the observatory. The great precision with which this minute angular quantity, a fraction of a second of arc only, has to be measured, is so delicate an operation with the ordinary micrometer—though, indeed, it was with this instrument that the classical observations of Sir Robert Ball were made—that a special instrument, in which the measures are made by moving the two halves of a divided object-glass, known as a heliometer, has been pressed into this service, and quite recently, in the skilful hands of Dr. Gill and Dr. Elkin, has largely increased our knowledge in this direction.

It is obvious that photography might be here of good service, if we could rely upon measurements of photographs of the same stars taken at suitable intervals of time. Professor Pritchard, to whom is due the honour of having opened this new path, aided by his assistants, has proved by elaborate investigations that measures for parallax may be safely made upon photographic plates, with, of course, the advantages of leisure and repetition; and he has already by this method determined the parallax for twenty-one stars with an accuracy not inferior to that of values previously obtained by purely astronomical methods.

The remarkable successes of astronomical photography, which depend upon the plate's power of accumulation of a very feeble light acting continuously through an exposure of several hours, are worthy to be regarded as a new revelation. The first chapter opened when, in 1880, Dr. Henry Draper obtained a picture of the nebula of Orion; but a more important advance was made in 1883, when Dr. Common, by his photographs, brought to our knowledge details and extensions of this nebula hitherto unknown. A further disclosure took place in 1885, when the brothers Henry showed for the first time in great detail the spiral nebulosity issuing from the bright star Maia of the Pleiades, and shortly afterward nebulous streams about the other stars of this group. In 1886 Mr. Roberts, by means of a photograph to which three hours' exposure had been given, showed the whole background of this group to be nebulous. In the following year Mr. Roberts more than doubled for us the great extension of the nebular region which surrounds the trapezium in the constellation of Orion. By his photographs of the great nebula in Andromeda he has shown the true significance of the dark canals which had been seen by the eye. They are in reality spaces between successive rings of bright matter, which appeared nearly straight, owing to the inclination in which they lie relatively to us. These bright rings surround an undefined central luminous mass. I have already spoken of this photograph.

Some recent photographs by Mr. Russell show that the great rift in the Milky Way in Argus, which to the eye is void of stars, is in reality uniformly covered with them. Also quite recently Mr. George Hale has photographed the solar prominences by means of a grating, making use of the lines H and K.

The heavens are richly but very irregularly inwrought with stars. The brighter stars cluster into well-known groups upon a background formed of an enlacement of streams and convoluted windings and intertwined spirals of fainter stars, which becomes richer and more intricate in the irregularly rifted zone of the Milky Way.

We, who form part of the emblazonry, can only see the design distorted and confused; here crowded, there scattered, at another place superposed. The groupings due to our position are mixed up with those which are real.

Can we suppose that each luminous point has no other relation to those near it than the accidental neighbourship of grains of sand upon the shore, or of particles of the windblown dust of the desert? Surely every star from Sirius and Vega down to each grain of the light-dust of the Milky Way has its present place in the heavenly pattern from the slow evolving of its past. We see a system of systems, for the broad features of clusters and streams and spiral windings which mark the general design are reproduced in every part. The whole is in motion, each point shifting its position by miles every second, though from the august magnitude of their distances from us and from each other it is only by the accumulated movements of years or of generations that some small changes of relative position reveal themselves.

The deciphering of this wonderfully intricate constitution of the heavens will be undoubtedly one of the chief astronomical works of the coming century. The primary task of the sun's motion in space, together with the motions of the brighter stars, has been already put well within our reach by the spectroscopic method of the measurement of star motions in the line of sight.

From other directions information is accumulating: from photographs of clusters and parts of the Milky Way, by Roberts in this country, Barnard at the Lick Observatory, and Russell at Sydney; from the counting of stars, and the detection of their configurations, by Holden and by Backhouse; from the mapping of the Milky Way by eye, at Parsonstown; from photographs of the spectra of stars, by Pickering at Harvard and in Peru; and from the exact portraiture of the heavens in the great international star chart which begins this year.

I have but touched some only of the problems of the newer side of astronomy. Of the many others which would claim our attention if time permitted I may name the following: The researches of the Earl of Rosse on lunar radiation, and the work on the same subject and on the sun by Langley; observations of lunar heat with an instrument of his own invention by Mr. Boys, and observations of the variation of the moon's heat with its phase by Mr. Frank Very; the discovery of the ultra-violet part of the hydrogen spectrum, not in the laboratory, but from the stars; the confirmation of this spectrum by terrestrial hydrogen in part by W. H. Vogel, and in its all but complete form by Cornu, who found similar series in the ultra-violet spectra of aluminium and thallium; the discovery of a simple formula for the hydrogen series by Balmer; the important question as to the numerical spectral relationship of different substances, especially in connection with their chemical properties, and the further question as to the origin of the harmonic and other relations between the lines and the groupings of lines of spectra. On these points contributions during the past year have been made by Rudolf von Koversligethy, Ames, Hartley, Deslandres, Rydberg, Grunwald, Kayser and Runge, Johnstone Stoney, and others; the remarkable employment of interference phenomena by Professor Michelson for the determination of the size, and distribution of light within them, of the images of objects which, when viewed in a telescope, subtend an angle less than that subtended by the light-wave at a distance equal to the diameter of the objective; a method applicable not alone to celestial objects, but also to spectral lines, and other questions of molecular physics.

Along the other lines there has not been less activity; by newer methods, by the aid of larger or more accurately constructed instruments, by greater refinement of analysis, knowledge has been increased, especially in precision and minute exactness.

Astronomy, the oldest of the sciences, has more than renewed her youth. At no time in the past has she been so bright with unbounded aspirations and hopes. Never were her temples so numerous, nor the crowd of her votaries so great. The British Astronomical Association, formed within the year, numbers already about six hundred members. Happy is the lot of those who are still on the eastern side of life's meridian!

Already, alas! the original founders of the newer methods are falling out—Kirchhoff, Ångström, D'Arrest, Secchi, Draper, Becquerel; but their places are more than filled; the pace of the race is gaining, but the goal is not and never will be in sight.

Since the time of Newton our knowledge of the phenomena of Nature has wonderfully increased, but man asks, perhaps more earnestly now than then, What is the ultimate reality behind the reality of the perceptions? Are they only the pebbles of the beach with which we have been playing? Does not the ocean of ultimate reality and truth lie beyond?

THE NEW ASTRONOMY: A PERSONAL RETROSPECT[1]

WHILE progress in all branches of knowledge has been rapid beyond precedent during the past sixty years, in at least two directions this knowledge has been so unexpected and novel in character that two new sciences may be said to have arisen: the new medicine, with which the names of Lister and of Pasteur will remain associated; and the new astronomy, of the birth and early growth of which I have now to speak.

The New Astronomy, unlike the old astronomy to which we are indebted for skill in the navigation of the seas, the calculation of the tides, and the daily regulation of time, can lay no claim to afford us material help in the routine of daily life. Her sphere lies outside the earth. Is she less fair? Shall we pay her less court because it is to mental culture in its highest form, to our purely intellectual joys, that she contributes? For surely in no part of Nature are the noblest and most profound conceptions of the human spirit more directly called forth than in the study of the heavens and the host thereof.

> "That with the glorie of so goodly sight
> The hearts of men . . .
> . . . may lift themselves up hyer."

May we not rather greet her in the words of Horace: "O matre pulchra filia pulchrior"?

As it fell to my lot to have some part in the early development of this new science, it has been suggested to me

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[1] From the "Nineteenth Century," June, 1897.

that the present Jubilee year of retrospect would be a suitable occasion to give some account of its history from the standpoint of my own work.

Before I begin the narrative of my personal observations it is desirable that I should give a short statement of the circumstances which led up to the birth of the new science in 1859, and also say a few words of the state of scientific opinion about the matters of which it treats, just before that time.

It is not easy for men of the present generation, familiar with the knowledge which the new methods of research of which I am about to speak have revealed to us, to put themselves back a generation, into the position of the scientific thought which existed on these subjects in the early years of the Queen's reign. At that time any knowledge of the chemical nature and of the physics of the heavenly bodies was regarded as not only impossible of attainment by any methods of direct observation, but as, indeed, lying altogether outside the limitations imposed upon man by his senses, and by the fixity of his position upon the earth.

It could never be, it was confidently thought, more than a matter of presumption, whether even the matter of the sun, and much less that of the stars, were of the same nature as that of the earth, and the unceasing energy radiated from it due to such matter at a high temperature. The nebular hypothesis of Laplace at the end of the last century required, indeed, that matter similar to that of the earth should exist throughout the solar system; but then this hypothesis itself needed for its full confirmation the independent and direct observation that the solar matter was terrestrial in its nature. This theoretical probability in the case of the sun vanished almost into thin air when the attempt was made to extend it to the stellar hosts; for it might well be urged that in those immensely distant regions an original difference of the primordial stuff as well as other conditions of condensation were present, giving rise to groups of substances which have but little analogy with those of our earthly chemistry.

About the time of the Queen's accession to the throne the French philosopher Comte put very clearly in his "Cours de Philosophie Positive" the views then held of the impossibility of direct observations of the chemical nature of the heavenly bodies. He says:

"On conçoit en effet, que nous puissions conjecturer, avec quelque espoir de succès, sur la formation du système solaire dont nous faisons partie, car il nous présente de nombreux phénomènes parfaitement connus, susceptibles peut-être de porter un témoignage décisif de sa véritable origine immédiate. Mais quelle pourrait être, au contraire, la base rationnelle de nos conjectures sur la formation des soleils eux-mêmes? Comment confirmer ou infirmer à ce sujet, d'àpres les phénomènes, aucune hypothèse cosmogonique, lorsqu'il n'existe vraiment en ce genre aucun phénomène exploré, ni même, sans doute, EXPLORABLE?" (The capitals are mine.)

We could never know for certain, it seemed, whether the matter and the forces with which we are familiar are peculiar to the earth, or are common with it to the midnight sky,

"All sow'd with glistering stars more thicke than grasse,
Whereof each other doth in brightnesse passe."

For how could we extend the methods of the laboratory to bodies at distances so great that even the imagination fails to realize them?

The only communication from them which reaches us across the gulf of space is the light which tells us of their existence. Fortunately this light is not so simple in its nature as it seems to be to the unaided eye. In reality it is very complex; like a cable of many strands, it is made up of light-rays of many kinds. Let this light-cable pass from air obliquely through a piece of glass, and its separate strand-rays all go astray, each turning its own way, and then go on apart. Make the glass into the shape of a wedge or prism, and the rays are twice widely scattered.

"First the flaming red
Sprung vivid forth: the tawny orange next;
And next delicious yellow; by whose side
Fell the kind beams of all-refreshing green.
Then the pure blue, that swells autumnal skies,
Ethereal played; and then, of sadder hue,
Emerged the deepened indigo, as when
The heavy-skirted evening droops with frost;
While the last gleamings of refracted light
Died in the fainting violet away."

Within this unravelled starlight exists a strange cryptography. Some of the rays may be blotted out, others may be enhanced in brilliancy. These differences, countless in variety, form a code of signals, in which is conveyed to us, when once we have made out the cipher in which it is written, information of the chemical nature of the celestial gases by which the different light-rays have been blotted out, or by which they have been enhanced. In the hands of the astronomer a prism has now become more potent in revealing the unknown than even was said to be "Agrippa's magic glass."

It was the discovery of this code of signals, and of its interpretation, which made possible the rise of the new astronomy. We must glance, but very briefly, at some of the chief steps in the progress of events which slowly led up to this discovery.

Newton, in his classical work upon the solar spectrum, failed through some strange fatality to discover the narrow gaps wanting in light, which, as dark lines, cross the colours of the spectrum and constitute the code of symbols. His failure is often put down to his using a round hole in place of a narrow slit, through the overlapping of the images of which the dark lines failed to show themselves. Though Newton did use a round hole, he states distinctly in his "Optics" that later he adopted a narrow opening in the form of a long parallelogram—that is, a true slit—at first one tenth of an inch in width, then only one twentieth of an inch, and at last still narrower. These conditions under which Newton worked were such as should have shown him the dark lines upon his screen. Professor Johnson has recently repeated Newton's experiments under strictly similar conditions, with the result that the chief dark lines were well seen. For some reason Newton failed to discover them. A possible cause may have been the bad annealing of his prism, though he says that it was made of good glass and free from bubbles.

The dark lines were described first by Wollaston in 1792, who strangely associated them with the boundaries of the spectral colours, and so turned contemporary thought away from the direction in which lay their true significance. It was left to Fraunhofer, in 1815, by whose name the dark lines are still known, not only to map some six hundred of them, but also to discover similar lines, but differently arranged, in several stars. Further, he found that a pair of dark lines in the solar spectrum appeared to correspond in their position in the spectrum, and in their distance from each other, to a pair of bright lines which were nearly always present in terrestrial flames. This last observation contained the key to the interpretation of the dark lines as a code of symbols; but Fraunhofer failed to use it, and the birth of astrophysics was delayed. An observation by Forbes at the eclipse of 1836 led thought away from the suggestive experiments of Fraunhofer, so that in the very year of the Queen's accession the knowledge of the time had to be summed up by Mrs. Somerville in the negation, "We are still ignorant of the cause of these rayless bands."

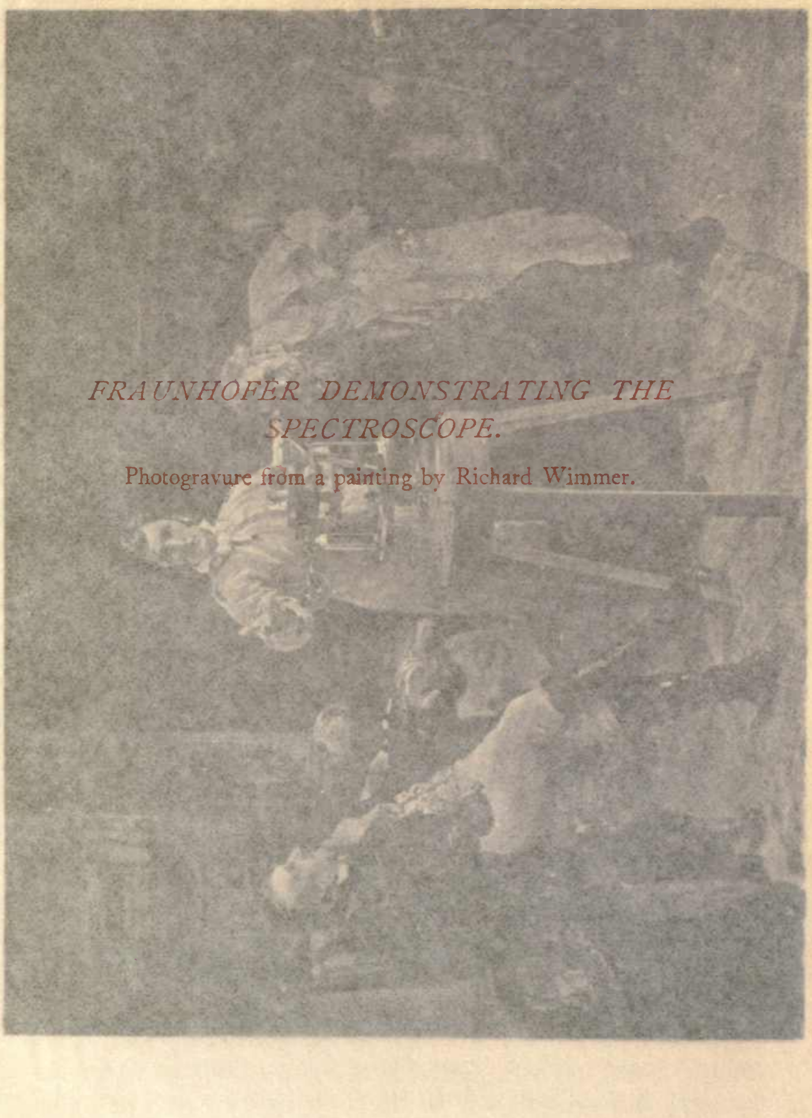
Later on, the revelation came more or less fully to many minds. Foucault, Balfour Stewart, and Ångström prepared the way. Prophetic guesses were made by Stokes and by Lord Kelvin. But it was Kirchhoff who, in 1859, first fully developed the true significance of the dark lines; and by his joint work with Bunsen on the solar spectrum proved beyond all question that the dark lines in the spectrum of the sun are produced by the absorption of the vapours of the same substances, which when suitably heated give out corresponding bright lines; and, further, that many of the solar absorbing vapours are those of substances found upon the earth. The new astronomy was born.

At the time that I purchased my present house, Tulse Hill was much more than now in the country and away from the smoke of London. It was after a little hesitation that I decided to give my chief attention to observational astronomy, for I was strongly under the spell of the rapid discoveries then taking place in microscopical research in connection with physiology.

In 1856 I built a convenient observatory opening by a passage from the house, and raised so as to command an uninterrupted view of the sky except on the north side. It consisted of a dome twelve feet in diameter, and a transit room. There was erected in it an equatorially mounted telescope by Dollond of five inches aperture, at that time looked upon as a large rather than a small instrument. I commenced work on the usual lines, taking transits, observing and making drawings of planets. Some of Jupiter now lying before me, I venture to think, would not compare unfavourably with drawings made with the larger instruments of the present day.

About that time Mr. Alvan Clark, the founder of the American firm famous for the construction of the great object-glasses of the Lick and the Yerkes Observatories, then a portrait painter by profession, began, as an amateur, to make object-glasses of large size for that time, and of very great merit. Specimens of his earliest work came into the hands of my friend Mr. Dawes and received the high approval of that distinguished judge. In 1858 I purchased from Mr. Dawes an object-glass by Alvan Clark of eight inches diameter, which he parted with to make room for a lens of a larger diameter by a quarter of an inch, which Mr. Clark had undertaken to make for him. I paid the price that it had cost Mr. Dawes—namely, £200. This telescope was mounted for me equatorially and provided with a clock motion by Mr. Cooke, of York.

I soon became a little dissatisfied with the routine character of ordinary astronomical work, and in a vague way sought about in my mind for the possibility of research upon the heavens in a new direction or by new methods.



FRAUNHOFER DEMONSTRATING THE SPECTROSCOPE.

Photogravure from a painting by Richard Wimmer.

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Rud. Wimmer 1897

It was just at this time, when a vague longing after newer methods of observation for attacking many of the problems of the heavenly bodies filled my mind, that the news reached me of Kirchhoff's great discovery of the true nature and the chemical constitution of the sun from his interpretation of the Fraunhofer lines.

This news was to me like the coming upon a spring of water in a dry and thirsty land. Here at last presented itself the very order of work for which in an indefinite way I was looking—namely, to extend his novel methods of research upon the sun to the other heavenly bodies. A feeling as of inspiration seized me: I felt as if I had it now in my power to lift a veil which had never before been lifted; as if a key had been put into my hands which would unlock a door which had been regarded as forever closed to man—the veil and door behind which lay the unknown mystery of the true nature of the heavenly bodies. This was especially work for which I was to a great extent prepared, from being already familiar with the chief methods of chemical and physical research.

It was just at this time that I happened to meet at a soirée of the Pharmaceutical Society, where spectroscopes were shown, my friend and neighbour, Dr. W. Allen Miller, Professor of Chemistry at King's College, who had already worked much on chemical spectroscopy. A sudden impulse seized me to suggest to him that we should return home together. On our way home I told him of what was in my mind, and asked him to join me in the attempt I was about to make, to apply Kirchhoff's methods to the stars. At first, from considerations of the great relative faintness of the stars, and the great delicacy of the work from the earth's motion, even with the aid of a clockwork, he hesitated as to the probability of our success. Finally he agreed to come to my observatory on the first fine evening, for some preliminary experiments as to what we might expect to do upon the stars.

At that time a star spectroscope was an instrument unknown to the optician. I remember that for our first trials we had one of the hollow prisms filled with bisulphide of carbon, so much in use then, and which, in consequence of a small leak, smelled abominably. To this day this pungent odour reminds me of star spectra!

Let us look at the problem which lay before us. It is difficult for any one, who has now only to give an order for a star spectroscope, to understand in any true degree the difficulties which we met with in attempting to make such observations for the first time. From the sun, with which the Heidelberg professors had to do—which, even bright as it is, for some parts of the spectrum has no light to spare—to the brightest stars is a very far cry. The light received at the earth from a first-magnitude star, as Vega, is only about the $\frac{1}{40000000000}$ part of that received from the sun.

Fortunately, as the stars are too far off to show a true disk, it is possible to concentrate all the light received from the star upon a large mirror or object-glass, into the telescopic image, and so increase its brightness.

We could not make use of the easy method adopted by Fraunhofer of placing a prism before the object-glass, for we needed a terrestrial spectrum, taken under the same conditions, for the interpretation, by a simultaneous comparison with it of the star's spectrum. Kirchhoff's method required that the image of a star should be thrown upon a narrow slit simultaneously with the light from a flame or from an electric spark.

These conditions made it necessary to attach a spectroscope to the eye end of the telescope, so that it would be carried with it, with its slit in the focal plane. Then, by means of a small reflecting prism placed before one half of the slit, light from a terrestrial source at the side of the telescope could be sent into the instrument together with the star's light, and so form a spectrum by the side of the stellar spectrum, for convenient comparison with it.

This was not all. As the telescopic image of a star is a point, its spectrum will be a narrow line of light without appreciable breadth. Now, for the observation of either dark or of bright lines across the spectrum a certain breadth is absolutely needful. To get breadth, the pointlike image of the star must be broadened out. As light is of first importance, it was desirable to broaden the star's image only in the one direction necessary to give breadth to the spectrum; or, in other words, to convert the stellar point into a short line of light. Such an enlargement in one direction only could be given by the device, first employed by Fraunhofer himself, of a lens convex or concave in one direction only, and flat, and so having no action on the light, in a direction at right angles to the former one.

When I went to the distinguished optician, Mr. Andrew Ross, to ask for such a lens, he told me that no such lenses were made in England, but that the spectacle lenses then very occasionally required to correct astigmatism—first used, I believe, by the then Astronomer Royal, the late Sir George Airy—were ground in Berlin. He procured for me from Germany several lenses; but not long afterward a cylindrical lens was ground for me by Browning. By means of such a lens, placed within the focus of the telescope, in front of the slit, the pointlike image of a star could be widened in one direction so as to become a very fine line of light, just so long as, but no longer than, was necessary to give to the spectrum a breadth sufficient for distinguishing any lines by which it may be crossed.

It is scarcely possible at the present day, when all these points are as familiar as household words, for any astronomer to realize the large amount of time and labour which had to be devoted to the successful construction of the first star spectroscope. Especially was it difficult to provide for the satisfactory introduction of the light for the comparison spectrum. We soon found, to our dismay, how easily the comparison lines might become instrumentally shifted, and so be no longer strictly fiducial. As a test we used the solar lines as reflected to us from the moon—a test of more than sufficient delicacy with the resolving power at our command.

Then it was that an astronomical observatory began, for the first time, to take on the appearance of a laboratory. Primary batteries, giving forth noxious gases, were arranged outside one of the windows; a large induction coil stood mounted on a stand on wheels so as to follow the positions of the eye end of the telescope, together with a battery of several Leyden jars; shelves with Bunsen burners, vacuum tubes, and bottles of chemicals, especially of specimens of pure metals, lined its walls.

The observatory became a meeting place where terrestrial chemistry was brought into direct touch with celestial chemistry. The characteristic light-rays from earthly hydrogen shone side by side with the corresponding radiations from starry hydrogen, or else fell upon the dark lines due to the absorption of the hydrogen in Sirius or in Vega. Iron from our mines was line-matched, light for dark, with stellar iron from opposite parts of the celestial sphere. Sodium, which upon the earth is always present with us, was found to be widely diffused through the celestial spaces.

This time was, indeed, one of strained expectation and of scientific exaltation for the astronomer, almost without parallel; for nearly every observation revealed a new fact, and almost every night's work was red-lettered by some discovery. And yet, notwithstanding, we had to record that " the inquiry in which we have been engaged has been more than usually toilsome; indeed, it has demanded a sacrifice of time very great when compared with the amount of information which we have been able to obtain."

Soon after the close of 1862 we sent a preliminary note to the Royal Society, " On the Lines of some of the Fixed Stars," in which we gave diagrams of the spectra of Sirius, Betelgeux, and Aldebaran, with the statement that we had observed the spectra of some forty stars, and also the spectra of the planets Jupiter and Mars. It was a little remarkable that on the same day on which our paper was to be read, but some little time after it had been sent in, news arrived there from America that similar observations on some of the stars had been made by Mr. Rutherfurd. A very little later similar work on the spectra of the stars was undertaken in Rome by Secchi, and in Germany by Vogel.

In February, 1863, the strictly astronomical character of the observatory was further encroached upon by the erection, in one corner, of a small photographic tent furnished with baths and other appliances for the wet collodion process. We obtained photographs, indeed, of the spectra of Sirius and Capella; but from want of steadiness and more perfect adjustment of the instruments, the spectra, though defined at the edges, did not show the dark lines as we expected. The dry collodion plates then available were not rapid enough; and the wet process was so inconvenient for long exposures, from irregular drying, and draining back from the positions in which the plates had often to be put, that we did not persevere in our attempts to photograph the stellar spectra. I resumed them with success in 1875, as we shall see further on.

At that time no convenient maps of the spectra of the chemical elements, which were then but imperfectly known, were available for comparison with the spectra of the stars. Kirchhoff's maps were confined to a few elements, and were laid down on an arbitrary scale, relatively to the solar spectrum. It was not always easy, since our work had to be done at night when the solar spectrum could not be seen, to recognise with certainty even the lines included in Kirchhoff's maps. To meet this want, I devoted a great part of 1863 to mapping, with a train of six prisms, the spectra of twenty-six of the elements; using as a standard scale the spark-spectrum of common air, which would be always at hand. The lines of air were first carefully referred to those of purified oxygen and nitrogen. The spectra were obtained by the discharge of a large induction coil furnished with a condenser of several Leyden jars. I was much assisted by specimens of pure metals furnished to me by Dr. W. A. Miller and Dr. Matthiessen. My paper on this subject, and its accompanying maps, appeared in the volume of the Transactions of the Royal Society for 1864.

During the same time, whenever the nights were fine, our work on the spectra of the stars went on, and the results were communicated to the Royal Society in April, 1864, after which Dr. Miller had not sufficient leisure to continue working with me. The general accuracy of our work, so far as it was possible with the instruments at our disposal, is shown by the good agreement of the spectra of Aldebaran and Betelgeux with the observations of the same stars made later in Germany by Vogel.

It is obviously unsafe to claim for spectrum comparisons a greater degree of accuracy than is justified by the resolving power employed. When the apparent coincidences of the lines of the same substance are numerous, as in the case of iron; or the lines are characteristically grouped, as are those of hydrogen, of sodium, and of magnesium, there is no room for doubt that the same substances are really in the stars. Coincidence with a single line may be little better than trusting to a bruised reed; for the stellar line may, under greater resolving power, break up into two or more lines, and then the coincidence may disappear. As we shall see presently, the apparent position of the star line may not be its true one, in consequence of the earth's or the star's motion in the line of sight. Our work, however, was amply sufficient to give a certain reply to the wonder that had so long asked in vain of what the stars were made. The chemistry of the solar system was shown to prevail, essentially at least, wherever a star twinkles. The stars were undoubtedly suns after the order of our sun, though not at all at the same evolutional stage, older or younger it may be, in the life history of bodies of which the vitality is heat. Further, elements which play a chief rôle in terrestrial physics, as iron, hydrogen, sodium, magnesium, calcium, were found to be the first and the most easily recognised of the earthly substances in the stars.

Soon after the completion of the joint work of Dr. Miller and myself, and then working alone, I was fortunate in the early autumn of the same year, 1864, to begin some observations in a region hitherto unexplored; and which, to this day, remain associated in my memory with the profound awe which I felt on looking for the first time at that which no eye of man had seen, and which even the scientific imagination could not foreshow.

The attempt seemed almost hopeless. For not only are the nebulæ very faintly luminous—as Marius put it, " like a rush-light shining through a horn "—but their feeble shining can not be increased in brightness, as can be that of the stars, neither to the eye nor in the spectroscope, by any optic tube, however great.

Shortly after making the observations of which I am about to speak, I dined at Greenwich, Otto Struve being also a guest, when, on telling of my recent work on the nebulæ, Sir George Airy said, " It seems to me a case of ' Eyes and No Eyes.' " Such work, indeed, it was, as we shall see, on certain of the nebulæ.

The nature of these mysterious bodies was still an unread riddle. Toward the end of the last century the elder Herschel, from his observations at Slough, came very near suggesting what is doubtless the true nature, and place in the Cosmos, of the nebulæ. I will let him speak in his own words:

" A shining fluid of a nature unknown to us.

" What a field of novelty is here opened to our conceptions! . . . We may now explain that very extensive nebulosity, expanded over more than sixty degrees of the heavens, about the constellation of Orion; a luminous matter accounting much better for it than clustering stars at a distance. . . .

" If this matter is self-luminous, it seems more fit to produce a star by its condensation than to depend on the star for its existence."

This view of the nebulæ as parts of a fiery mist out of which the heavens had been slowly fashioned, began, a little before the middle of the present century, at least in many minds, to give way before the revelations of the giant telescopes which had come into use, and especially of the telescope, six feet in diameter, constructed by the late Earl of Rosse at a cost of not less than £12,000.

Nebula after nebula yielded, being resolved apparently into innumerable stars, as the optical power was increased; and so the opinion began to gain ground that all nebulæ may be capable of resolution into stars. According to this view, nebulæ would have to be regarded, not as early stages of an evolutional progress, but rather as stellar galaxies already formed, external to our system—cosmical "sand-heaps" too remote to be separated into their component stars. Lord Rosse himself was careful to point out that it would be unsafe from his observations to conclude that all nebulosity is but the glare of stars too remote to be resolved by our instruments. In 1858 Herbert Spencer showed clearly that, notwithstanding the Parsonstown revelations, the evidence from the observation of nebulæ up to that time was really in favour of their being early stages of an evolutional progression.

On the evening of the 29th of August, 1864, I directed the telescope for the first time to a planetary nebula in Draco. The reader may now be able to picture to himself to some extent the feeling of excited suspense, mingled with a degree of awe, with which, after a few moments of hesitation, I put my eye to the spectroscope. Was I not about to look into a secret place of creation?

I looked into the spectroscope. No spectrum such as I expected! A single bright line only! At first I suspected some displacement of the prism, and that I was looking at a reflection of the illuminated slit from one of its faces. This thought was scarcely more than momentary; then the true interpretation flashed upon me. The light of the nebula was monochromatic, and so, unlike any other light I had as yet subjected to prismatic examination, could not be extended out to form a complete spectrum. After passing through the two prisms it remained concentrated into a single bright line, having a width corresponding to the width of the slit, and occupying in the instrument a position at that part of the spectrum to which its light belongs in refrangibility. A little closer looking showed two other bright lines on the side toward the blue, all the three lines being separated by intervals relatively dark.

The riddle of the nebulæ was solved. The answer, which had come to us in the light itself, read: Not an aggregation of stars, but a luminous gas. Stars after the order of our own sun, and of the brighter stars, would give a different spectrum; the light of this nebula had clearly been emitted by a luminous gas. With an excess of caution, at the moment I did not venture to go further than to point out that we had here to do with bodies of an order quite different from that of the stars. Further observations soon convinced me that, though the short span of human life is far too minute relatively to cosmical events for us to expect to see in succession any distinct steps in so august a process, the probability is indeed overwhelming in favour of an evolution in the past, and still going on, of the heavenly hosts. A time surely existed when the matter now condensed into the sun and planets filled the whole space occupied by the solar system, in the condition of gas, which then appeared as a glowing nebula, after the order, it may be, of some now existing in the heavens. There remained no room for doubt that the nebulæ, which our telescopes reveal to us, are the early stages of long processions of cosmical events, which correspond broadly to those required by the nebular hypothesis in one or other of its forms.

Not, indeed, that the philosophical astronomer would venture to dogmatize in matters of detail, or profess to be able to tell you pat off by heart exactly how everything has taken place in the universe, with the flippant tongue of a Lady Constance after reading "The Revelations of Chaos":

"It shows you exactly how a star is formed; nothing could be so pretty. A cluster of vapour—the cream of the Milky Way; a sort of celestial cheese churned into light."

It is necessary to bear distinctly in mind that the old view which made the matter of the nebulæ to consist of an original fiery mist—in the words of the poet:

> " . . . a tumultuous cloud
> Instinct with fire and nitre"—

could no longer hold its place after Helmholtz had shown, in 1854, that such an originally fiery condition of the nebulous stuff was quite unnecessary, since in the mutual gravitation of widely separated matter we have a store of potential energy sufficient to generate the high temperature of the sun and stars.

The solution of the primary riddle of the nebulæ left pending some secondary questions. What chemical substances are represented by the newly found bright lines? Is solar matter common to the nebulæ as well as to the stars? What are the physical conditions of the nebulous matter?

Further observations showed two lines of hydrogen; and recent observations have shown associated with it the new element recently discovered by Professor Ramsay, occluded in certain minerals, and of which a brilliant yellow line in the sun had long been looked upon as the badge of an element as yet unknown. The principal line of these nebulæ suggests probably another substance which has not yet been unearthed from its hiding place in terrestrial rocks by the cunning of the chemist.

Are the nebulæ very hot, or comparatively cool? The spectroscope indicates a high temperature—that is to say, that the individual molecules or atoms, which by their encounters are luminous, have motions corresponding to a very high temperature, and in this sense are very hot. On account of the great extent of the nebulæ, however, a comparatively small number of luminous molecules might be sufficient to make them as bright as they appear to us; taking this view, their mean temperature, if they can be said to have one, might be low, and so correspond with what we might expect to find in gaseous masses at an early stage of condensation.

In the nebulæ I had as yet examined the condensation of nearly all the light into a few bright lines made the observations of their spectra less difficult than I feared would be the case. It became, indeed, a case of "Eyes and No Eyes" when a few days later I turned the telescope to the great nebula in Andromeda. Its light was distributed throughout the spectrum, and consequently extremely faint. The brighter middle part only could be seen, though I have since proved, as I at first suggested might be the case, that the blue and the red ends are really not absent, but are not seen on account of their feebler effect upon the eye. Though continuous, the spectrum did not look uniform in brightness, but its extreme feebleness made it uncertain whether the irregularities were due to certain parts being enhanced by bright lines, or the other parts enfeebled by dark lines.

Out of sixty of the brighter nebulæ and clusters, I found about one third, including the planetary nebulæ and that of Orion, to give the bright-line spectrum. It would be altogether out of place here to follow the results of my further observations along the same lines of research, which occupied the two years immediately succeeding.

I pass at once to a primary spectroscopic observation of one of those rare and strange sights of the heavens, of which only about nineteen have been recorded in as many centuries:

> ". . . those far stars that come in sight
> Once in a century."

On the 18th of May, 1866, at 5 P. M., a letter came with the address "Tuam, from an unknown correspondent, one John Birmingham." Mr. Birmingham afterward became well known by his observations of variable stars, and especially by his valuable catalogue of Red Stars in 1877. The letter ran:

"I beg to direct your attention to a new star which I observed last Saturday night, and which must be a most interesting object for spectrum analysis. It is situated in Cor. Bor., and is very brilliant, of about the second magnitude. I sent an account of it to the 'Times' yesterday, but as that journal is not likely to publish communications from this part of the world, I scarcely think that it will find a place for mine."

Fortunately the evening was fine, and as soon as it was dusk I looked, with not a little scepticism, I freely confess, at the place of the sky named in the letter. To my great joy, there shone a bright new star, giving a new aspect to the Northern Crown; of the order doubtless of the splendid temporary star of 1572, which Tycho supposed to be generated from the ethereal substance of the Milky Way, and afterward dissipated by the sun, or dissolved from some internal cause.

I sent a messenger for my friend Dr. Miller, and an hour later we directed the telescope, with spectroscope attached, to the blazing star. Later in the evening a letter arrived from Mr. Baxendale, who had independently discovered the star on the 15th.

By this evening, the 18th, the star had already fallen in brightness below the third magnitude. The view in the spectroscope was strange, and up to that time unprecedented. Upon a spectrum of the solar order, with its numberless dark lines, shone out brilliantly a few very bright lines. There was little doubt that at least two of these lines belonged to hydrogen. The great brilliancy of these lines as compared with the parts of the continuous spectrum upon which they fell suggested a temperature for the gas emitting them higher than that of the star's photosphere.

Few of days, as indeed had been its forbears appearing at long intervals, the new star waned with a rapidity little less remarkable than was the suddenness of its outburst, without visible descent, all armed in a full panoply of light from the moment of its birth. A few hours only before Birmingham saw it blazing with second-magnitude splendour, Schmidt, observing at Athens, could testify that no outburst had taken place. Rapid was the decline of its light, falling in twelve days from the second down to the eighth magnitude.

It was obvious to us that no very considerable mass of matter could cool down from the high temperature indicated by the bright lines in so short a time. At the same time it was not less clear that the extent of the mass of the fervid gas must be on a very grand scale indeed, for a star at its undoubted distance from us, to take on so great a splendour. These considerations led us to suggest some sudden and vast convulsion, which had taken place in a star so far cooled down as to give but little light, or even to be partially crusted over, by volcanic forces, or by the disturbing approach or partial collision of another dark star. The essential character of the explanation lay in the suggestion of a possible chemical combination of some of the escaping highly heated gases from within, when cooled by the sudden expansion, which might give rise to an outburst of flame at once very brilliant and of very short duration.

The more precise statement of what occurred during our observations, as made afterward from the pulpit of one of our cathedrals—" that from afar astronomers had seen a world on fire go out in smoke and ashes "—must be put down to an excess of the theological imagination.

From the beginning of our work upon the spectra of the stars I saw in vision the application of the new knowledge to the creation of a great method of astronomical observation which could not fail in future to have a powerful influence on the progress of astronomy; indeed, in some respects greater than the more direct one of the investigation of the chemical nature and the relative physical conditions of the stars.

It was the opprobrium of the older astronomy—though, indeed, one which involved no disgrace, for à l'impossible nul n'est tenu—that only that part of the motions of the stars which is across the line of sight could be seen and directly measured. The direct observation of the other component in the line of sight, since it caused no change of place and, from the great distance of the stars, no appreciable change of size or of brightness within an observer's lifetime, seemed to lie hopelessly quite outside the limits of man's powers. Still, it was only too clear that, so long as we were unable to ascertain directly those components of the star's motions which lie in the line of sight, the speed and direction of the solar motion in space, and many of the great problems of the constitution of the heavens, must remain more or less imperfectly known.

Now, as the colour of a given kind of light, and the exact position it would take up in a spectrum, depends directly upon the length of the waves, or, to put it differently, upon the number of waves which would pass into the eye in a second of time, it seemed more than probable that motion between the source of the light and the observer must change the apparent length of the waves to him, and the number reaching his eye in a second. To a swimmer striking out from the shore each wave is shorter, and the number he goes through in a given time is greater than would be the case if he had stood still in the water. Such a change of wave-length would transform any given kind of light, so that it would take a new place in the spectrum, and from the amount of this change to a higher or to a lower place, we could determine the velocity per second of the relative motion between the star and the earth.

The notion that the propagation of light is not instantaneous, though rapid far beyond the appreciation of our senses, is due, not, as is sometimes stated, to Francis, but to Roger Bacon. "Relinquitur ergo," he says, in his "Opus Majus," "quod lux multiplicatur in tempore . . . sed tamen non in tempore sensibili et perceptibili a visu, sed insensibili. . . ." The discovery of its actual velocity was made by Roemer, in 1675, from observations of the satellites of Jupiter. Now, though the effect of motion in the line of sight upon the apparent velocity of light underlies Roemer's determinations, the idea of a change of colour in light from motion between the source of light and the observer was announced for the first time by Doppler in 1841. Later, various experiments were made in connection with this view by Ballot, Sestini, Klinkerfues, Clerk Maxwell, and Fizeau. But no attempts had been made, nor were indeed possible, to discover by this principle the motions of the heavenly bodies in the line of sight. For, to learn whether any change in the light had taken place from motion in the line of sight, it was clearly necessary to know the original wave-length of the light before it left the star.

As soon as our observations had shown that certain earthly substances were present in the stars, the original wave-lengths of their lines became known, and any small want of coincidence of the stellar lines with the same lines produced upon the earth might safely be interpreted as revealing the velocity of approach or of recession between the star and the earth.

These considerations were present to my mind from the first, and helped me to bear up under many toilsome disappointments: "Studio fallente laborem." It was not until 1866 that I found time to construct a spectroscope of greater power for this research. It would be scarcely possible, even with greater space, to convey to the reader any true conception of the difficulties which presented themselves in this work, from various instrumental causes, and of the extreme care and caution which were needful to distinguish spurious instrumental shifts of a line from a true shift due to the star's motion.

At last, in 1868, I felt able to announce in a paper printed in the "Transactions" of the Royal Society for that year the foundation of this new method of research, which, transcending the wildest dreams of an earlier time, enables the astronomer to measure off directly in terrestrial units the invisible motions in the line of sight of the heavenly bodies.

To pure astronomers the method came before its time, since they were then unfamiliar with Spectrum Analysis, which lay completely outside the routine work of an observatory. It would be easy to mention the names of men well known, to whom I was "as a very lovely song of one that hath a pleasant voice." They heard my words, but for a time were very slow to avail themselves of this new power of research. My observations were, however, shortly afterward confirmed by Vogel in Germany, and by others the principle was soon applied to solar phenomena. By making use of improved methods of photography, Vogel has recently determined the motions of approach and of recession of some fifty stars, with an accuracy of about an English mile a second. In the hands of Young, Dunèr, Keeler, and others, the method has been successfully applied to a determination of the rotation of the sun, of Saturn and his rings, and of Jupiter.

It has become fruitful in another direction, for it puts into our hands the power of separating double stars which are beyond the resolving power of any telescope that can ever be constructed. Pickering and Vogel have independently discovered by this method an entirely new class of double stars.

Double stars too close to be separately visible unite in giving a compound spectrum. Now, if the stars are in motion about a common centre of gravity, the lines of one star will shift periodically relatively to similar lines of the other star, in the spectrum common to both; and such lines will consequently, at those times, appear double. Even if one of the stars is too dark to give a spectrum which can be seen upon that of the other star, as is actually the case with Algol and Spica, the whirling of the stars about each other may be discovered from the periodical shifting of the lines of the brighter star relatively to terrestrial lines of the same substance. It is clear that as the stars revolve about their common centre of gravity, the bright star would be sometimes advancing, and at others receding, relatively to an observer on the earth, except it should so happen that the stars' orbit were perpendicular to the line of sight.

It would be scarcely possible, without the appearance of great exaggeration, to attempt to sketch out even in broad outline the many glorious achievements which doubtless lie before this method of research in the immediate future.

Comets in the olden time were looked upon as the portents of all kinds of woe:

> "There with long bloody haire, a blazing star
> Threatens the World with Famin, Plague, and War."

Though they were no longer, at the time of which I am speaking, a terror to mankind, they were a great mystery. Perhaps of no other phenomenon of Nature had so many guesses at truth been made on different, and even on opposing principles of explanation. It was about this time that a beam of light was thrown in, for the first time, upon the night of mystery in which they moved and had their being, by the researches of Newton of Yale College, by Adams, and by Schiaparelli. The unexpected fact came out of the close relationship of the orbits of certain comets with those of periodic meteor-swarms. Only a year before the observations of which I am about to speak were made, Odling had lighted up the theatre of the Royal Institution with gas brought by a meteorite from celestial space. Two years earlier, Donati showed the light of a small comet to be in part self-emitted, and so not wholly reflected sunshine.

I had myself, in the case of three faint comets, in 1866, in 1867, and January, 1868, discovered that part of their light was peculiar to them, and that the light of the last one consisted mainly of three bright flutings. Intense, therefore, was the great expectancy with which I directed the telescope with its attached spectroscope to the much brighter comet which appeared in June, 1868.

The comet's light was resolved into a spectrum of three bright bands or flutings, each alike falling off in brightness on the more refrangible side. On the evening of the 22d I measured the positions in the spectrum of the brighter beginnings of the flutings on the red side. I was not a little surprised the next morning to find that the three cometary flutings agreed in position with three similar flutings in the brightest part of the spectrum of carbon. Some time before, I had mapped down the spectrum of carbon, from different sources, chiefly from different hydrocarbons. In some of these spectra, the separate lines of which the flutings are built up are individually more distinct than in others. The comet bands, as I had seen them on the previous evening, appeared to be identical in character in this respect, as well as in position in the spectrum, with the flutings as they appeared when I took the spark in a current of olefiant gas. I immediately filled a small holder with this gas, arranged an apparatus in such a manner that the gas could be attached to the end of the telescope, and its spectrum, when a spark was taken in it, seen side by side with that of the comet.

Fortunately the evening was fine; and on account of the exceptional interest of confronting for the first time the spectrum of an earthly gas with that of a comet's light, I invited Dr. Miller to come and make the crucial observation with me. The expectation which I had formed from my measures was fully confirmed. The comet's spectrum when seen together with that from the gas agreed in all respects precisely with it. The comet, though "subtle as Sphinx," had at last yielded up its secret. The principal part of its light was emitted by luminous vapour of carbon.

This result was in harmony with the nature of the gas found occluded in meteorites. Odling had found carbonic oxide as well as hydrogen in his meteorite. Wright, experimenting with another type of meteorite, found that carbon dioxide was chiefly given off. Many meteorites contain a large percentage of hydrocarbons; from one of such sky-stones a little later I observed a spectrum similar to that of the comet. The three bands may be seen in the base of a candle flame.

Since these early observations the spectra of many comets have been examined by many observers. The close general agreement as to the three bright flutings which form the main feature of the cometary spectrum confirms beyond doubt the view that the greater part of the light of comets is due to the fluted spectrum of carbon. Some additional knowledge of the spectra of comets, obtained by means of photography, will have its proper place later on.

About this time I devoted some attention to spectroscopic observations of the sun, and especially to the modifications of the spectrum which take place under the influence of the solar spots.

The aërial ocean around and above us, in which finely divided matter is always more or less floating, becomes itself illuminated, and a source of light, when the sun shines upon it, and so conceals, like a luminous veil, any object less brilliant than itself in the heavens beyond. From this cause the stars are invisible at midday. This curtain of light above us at all ordinary times shuts out from our view the magnificent spectacle of red flames flashing upon a coronal glory of bright beams and streamers, which suddenly bursts upon the sight, for a few minutes only, when at rare intervals the light curtain is lifted by the screening of the sun's light by the moon, at a total eclipse.

As yet the spectrum of the red flames had not been seen. If, as seemed probable, it should be found to be that of a gas, consisting of bright lines only, it was conceivable that the spectroscope might enable us so to weaken by dispersion the air-glare, relatively to the bright lines which would remain undispersed, that the bright lines of the flames might become visible through the atmospheric glare.

The historic sequence of events is as follows. In November, 1866, Mr. Lockyer asked the question: "May not the spectroscope afford us evidence of the existence of the red flames, which total eclipses have revealed to us in the sun's atmosphere; though they escape all other methods of observation at other times?"

In the "Report of the Council of the Royal Astronomical Society," read in February, 1868, occurs the following statement, furnished by me, in which the explanation is fully given of the principle on which I had been working to obtain the spectrum of the red flames without an eclipse:

"During the last two years Mr. Huggins has made numerous observations for the purpose of obtaining a view, if possible, of the red prominences seen during an eclipse. The invisibility of these objects at ordinary times is supposed to arise from the illumination of our atmosphere. If these bodies are gaseous, their spectra would consist of bright lines. With a powerful spectroscope the light reflected from our atmosphere near the sun's limb edge would be greatly reduced in intensity by the dispersion of the prisms, while the bright lines of the prominences, if such be present, would remain but little diminished in brilliancy. This principle has been carried out by various forms of prismatic apparatus, and also by other contrivances, but hitherto without success."

At the total eclipse of the sun, August 18, 1868, several observers saw the light of the red flames to be resolved in their spectroscopes into bright lines, among which lines of hydrogen were recognised. The distinguished astronomer, Janssen, one of the observers in India, saw some of the bright lines again the next day, by means of the principle described above, when there was no eclipse.

On October 29th Mr. Lockyer sent a note to the Royal Society to say that on that day he had succeeded in observing three bright lines, of a fine prominence.

About the time that the news of the discovery of the bright lines at the eclipse reached this country, in September, I was altogether incapacitated for work for some little time through the death of my beloved mother. We had been all in all to each other for many years. The first day I was sufficiently recovered to resume work, December 19th, on looking at the sun's limb with the same spectroscope I had often used before, now that I knew exactly at what part of the spectrum to search for the lines, I saw them at the first moment of putting my eye to the instrument.

As yet, by all observers the lines only of the prominences had been seen, and therefore to learn their forms it was necessary to combine in one design the lengths of the lines as they varied, when the slit was made to pass over a prominence. In February of the following year it occurred to me that by widening the opening of the slit the form of a prominence, and not its lines only, might be directly observed. This method of using a wide slit has been since universally employed.

It does not fall within the scope of this article to describe an ingenious photographic method by which Hale has been able to take daily records of the constantly varying phenomena of the red flames and the bright faculæ, upon and around the solar disk.

The purpose of this article is to sketch, in very broad outline only, the principal events, in the order of their succession in time, quorum pars magna fui, which contributed in an important degree to the rise of the new astronomy. As a science advances it follows naturally that its further progress will consist more and more in matters of detail, and in points which are of technical rather than of general interest.

It would, therefore, be altogether out of place here to carry on in detail the narrative of the work of my observatory, when, as was inevitable, it began to take on the character of a development only, along lines of which I have already spoken—namely, the observation of more stars, and of other nebulæ, and other comets. I pass on at once, therefore, to the year 1876, in which by the aid of the new dry plates, with gelatine films, introduced by Mr. Kennett, I was able to take up again, and this time with success, the photography of the spectra of the stars, of my early attempts at which I have already spoken.

I was now better prepared for work. My observatory had been enlarged from a dome of twelve feet in diameter to a drum having a diameter of eighteen feet. This alteration had been made for the reception of a larger telescope made by Sir Howard Grubb, at the expense of a legacy to the Royal Society, and which was placed in my hands on loan by that society. This instrument was furnished with two telescopes: an achromatic of fifteen inches aperture, and a Cassegrain of eighteen inches aperture, with mirrors of speculum metal. At this time, one only of these telescopes could be in use at a time. Later on, in 1882, by a device which occurred to me of giving each telescope an independent polar axis, the one working within the other, both telescopes could remain together on the equatorial mounting, and be equally ready for use.

By this time I had the great happiness of having secured an able and enthusiastic assistant by my marriage in 1875.

The great and notable advances in astronomical methods and discoveries by means of photography since 1875 are due almost entirely to the great advantages which the gelatine dry plate possesses for use in the observatory, over the process of Daguerre, and even over that of wet collodion. The silver-bromide gelatine plate, which I was the first, I believe, to use for photographing the spectra of stars, except for its grained texture, meets the need of the astronomer at all points. This plate possesses extreme sensitiveness; it is always ready for use; it can be placed in any position; it can be exposed for hours; lastly, immediate development is not necessary, and for this reason, as I soon found to be necessary in this climate, it can be exposed again to the same object on succeeding nights; and so make up by successive instalments, as the weather may permit, the total long exposure which may be needful.

The power of the eye falls off as the spectrum extends beyond the blue, and soon fails altogether. There is therefore no drawback to the use of glass for the prisms and lenses of a visual spectroscope. But while the sensitiveness of a photographic plate is not similarly limited, glass, like the eye, is imperfectly transparent, and soon becomes opaque, to the parts of the spectrum at a short distance beyond the limit of the visible spectrum. To obtain, therefore, upon the plate a spectrum complete at the blue end of stellar light, it was necessary to avoid glass, and to employ instead Iceland spar and rock crystal, which are transparent up to the limit of the ultra-violet light which can reach us through our atmosphere. Such a spectroscope was constructed and fixed with its slit at the focus of the great speculum of the Cassegrain telescope.

How was the image of a star to be easily brought, and then kept, for an hour or even for many hours, precisely at one place on a slit so narrow as about the $\frac{1}{200}$ of an inch? For this purpose the very convenient device was adopted of making the slit-plates of highly polished metal, so as to form a divided mirror, in which the reflected image of a star could be observed from the eye end of the telescope by means of a small telescope fixed within the central hole of the great mirror. A photograph of the spectrum of *α* Lyræ, taken with this instrument, was shown at the Royal Society in 1876.

In the spectra of such stars as Sirius and Vega there came out in the ultra-violet region, which up to that time had remained unexplored, the completion of a grand rhythmical group of strong dark lines, of which the well-known hydrogen lines in the visible region form the lower members. Terrestrial chemistry became enriched with a more complete knowledge of the spectrum of hydrogen from the stars. Shortly afterward Cornu succeeded in photographing a similar spectrum in his laboratory from earthly hydrogen.

I presented in 1879 a paper, with maps, to the Royal Society, on the photographic spectra of the stars, which was printed in their "Transactions" for 1880. In this paper, besides descriptions of the photographs, and tables of the measures of the positions of the lines, I made a first attempt to arrange the stars in a possible evolutional series from the relative behaviour of the hydrogen and the metallic lines. In this series Sirius and Vega are placed at the hotter and earlier end; Capella and the sun at about the same evolutional stage, somewhere in the middle of the series; while at the most advanced and oldest stage of the stars which I had then photographed came Betelgeux, in the spectrum of which the ultra-violet region, though not wanting, is very greatly enfeebled.

Shortly afterward I directed the photographic arrangement of combined spectroscope and telescope to the nebula in Orion, and obtained for the first time information of the nature of its spectrum beyond the visible region. One line a little distance on in the ultra-violet region came out very strongly on the plate. If this kind of light came within the range of our vision it would no doubt give the dominant colour to the nebula, in place of its present blue-greenish hue. Other lines of the hydrogen series, as might be expected, were seen in the photograph, together with a number of other bright lines.

In 1881, for the first time since the spectroscope and also suitable photographic plates had been in the hands of astronomers, the coming of a bright comet made it possible to extend the examination of its light into the invisible region of the spectrum at the blue end. On the 22d of June, by leaving very early a banquet at the Mansion House, I was able, after my return home, to obtain, with an exposure of one hour, a good photograph of the head of the comet. It was under a great tension of expectancy that the plate was developed, so that I might be able to look for the first time into a virgin region of Nature, as yet unexplored by the eye of man.

The plate contained an extension and confirmation of my earlier observations by eye. There were the combined spectra of two kinds of light—a faint continuous spectrum, crossed by Fraunhofer lines which showed it to be reflected solar light. Upon this was seen a second spectrum of the original light emitted by the comet itself. This spectrum consisted mainly of two groups of bright lines, characteristic of the spectra of certain compounds of carbon. It will be remembered that my earlier observations revealed the three principal flutings of carbon as the main feature of a comet's spectrum in the visible region. The photograph brought a new fact to light. Liveing and Dewar had shown that one of these bands consisted of lines belonging to a nitrogen compound of carbon. We gained the new knowledge that nitrogen, as well as carbon and hydrogen, exists in comets. Now, nitrogen is present in the gas found occluded in some meteorites. At a later date, Dr. Flight showed that nitrogen formed as much as seventeen per cent of the occluded gas from the meteorite of Cranbourne, Australia.

I have now advanced to the extreme limit of time within which the rise of the New Astronomy can be regarded as taking place. At this time, in respect of the broad lines of its methods, and the wide scope of the directions in which it was already applied, it had become well established. Already it possessed a literature of its own, and many observatories were becoming, in part at least, devoted to its methods.

In my own observatory work has gone on whenever our unfavourable climate has permitted observations to be made. At the present moment more than one research is in progress. It would be altogether beyond the intention and limited scope of the present article to follow this later work.

We found the New Astronomy newly born in a laboratory at Heidelberg; to astronomers she was

> ". . . a stranger,
> Born out of their dominions."

We take leave of her in the full beauty of a vigorous youth, receiving homage in nearly all the observatories of the world, some of which, indeed, are devoted wholly to her cult. So powerful is the magic of her charms that gifts have poured in from all sides to do her honour. It has been by such free gifts that Pickering, at Cambridge, United States, and in the southern hemisphere, has been able to give her so devoted a service. In this country, where from almost the hour of her birth she won hearts, enthusiastic worshippers have not been wanting. By the liberality of the late Mr. Newall, and the disinterested devotion of his son, a well-equipped observatory is now wholly given up to her worship at Cambridge. This Jubilee year is red-lettered at Greenwich by the inauguration of a magnificent double telescope, laid at her feet by Sir Henry Thompson. Next year the Royal Observatory at the Cape will be able to add to its devotion to the old astronomy a homage not less sincere and enthusiastic to the New Astronomy, by means of the splendid instruments which Mr. McClean, who personally serves under her colours, has presented to that observatory. In Germany, the first National Observatory dedicated to the New Astronomy in 1874, under the direction of the distinguished astrophysicist, Professor Vogel, is about to be furnished by the Government with new and larger instruments in her honour.

In America many have done liberally, but Mr. Yerkes has excelled them all. This summer will be celebrated the opening of a palatial institution on the shore of Lake Geneva founded by Mr. Yerkes, and dedicated to our fair lady, the New Astronomy. This observatory, in respect of the great size of its telescope, of forty inches in aperture, the largest yet constructed, its armoury of instruments for spectroscopic attack upon the heavens, and the completeness of its laboratories and its workshops, will represent the most advanced state of instrument-making; and at the same time render possible, under the most favourable conditions, the latest and the most perfect methods of research of the New Astronomy. Above all, the needful men will not be wanting. A knightly band, who have shown their knighthood by prowess in discovery, led by Professor Hale in chivalrous quest of Truth, will surely make this palace of the New Astronomy worthy to be regarded as the Uraniborg of the end of the nineteenth century, as the Danish Observatory, under Tycho and his astronomers, represented the highest development of astronomy at the close of the sixteenth.

AN ASTRONOMER'S WORK IN A MODERN OBSERVATORY

BY

DAVID GILL

AN ASTRONOMER'S WORK IN A MODERN OBSERVATORY[1]

THE work of astronomical observatories has been divided into two classes—viz., astrometry and astrophysics. The first of these relates to astronomy of precision—that is, to the determination of the position of celestial objects; the second relates to the study of their physical features and chemical construction.

Some years ago the aims and objects of these two classes of observatories might have been considered perfectly distinct, and in fact were so considered. But I hope to show that in more recent years their objects and their processes have become so interlaced that they can not with advantage be divided, and a fully equipped modern observatory must be understood to include the work both of astrometry and astrophysics.

In any such observatory the principal and the fundamental instrument is the transit circle. It is upon the position in the heavens of celestial objects, as determined with this instrument or with kindred instruments, that the whole fair superstructure of exact astronomy rests—that is to say, all that we find of information and prediction in our nautical almanacs, all that we know of the past and can predict for the future motions of the celestial bodies.

Here is a very small and imperfect model, but it will serve to render intelligible the photographs of the actual instrument which will be subsequently projected on the screen. (Here the lecturer described the adjustments and mode of using a transit circle.)

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[1] From the "Proceedings of the Royal Institution of Great Britain," 1890–'92.

We are now in a position to understand photographs of the instrument itself. But first of all as to the house in which it dwells. Here, now on the screen, is the outside of the main building of the Royal Observatory, Cape of Good Hope. I select it simply because, being the observatory which it is my privilege to direct, it is the one of which I can most easily procure a series of photographs. It was built during the years 1824–'28, and like all the observatories built about that time, and like too many built since, it is a very fair type of most of the things which an observatory should not be. It is, as you see, an admirably solid and substantial structure, innocent of any architectural charm, and so far as it affords an excellent dwelling-place, good library accommodations, and good rooms for computers, no fault can be found with it. But these very qualities render it undesirable as an observatory. An essential matter for a perfect observatory should be the possibility to equalize the internal and external temperature. The site of an instrument should also be free from the immediate surroundings of chimneys or other origin of ascending currents of heated air. Both these conditions are incompatible with thick walls of masonry and the chimneys of attached dwelling-houses, and therefore, as far as possible, I have removed the instruments to small detached houses of their own. But the transit circle still remains in the main building, for, as will be evident to you, it is no easy matter to transport such an instrument.

The first two photographs show the instrument in one case pointed nearly horizontally to the north, the other pointed nearly vertical. Neither can show all parts of the instrument, but you can see the massive stone piers, weighing many tons each, which, resting on the solid blocks ten feet below, support the pivots. Here are the counterweights, which remove a great part of the weight of the instrument from the pivots, leaving only a residual pressure sufficient to enable the pivots to preserve the motion of the instrument in its proper plane. Here are the microscopes by which the circle is read; here the openings through which the instrument views the meridian sky. The observer's chair is shown in this diagram. His work appears to be very simple, and so it is, but it requires special natural gifts—patience and devotion, and a high sense of the importance of his work—to make a first-class meridian observer. Nothing apparently more monotonous can be well imagined if a man is not " to the manner born."

Having directed this instrument by means of the setting circle to the required altitude, he clamps it there and waits for the star which he is about to observe to enter the field.

As the star enters the field it passes wire after wire, and as it passes each wire he presses the key of his chronograph and records the instant automatically. As the star passes the middle wire he bisects it with the horizontal web, and again similarly records on his chronograph the transit of the star over the remaining webs. Then he reads off the microscopes by which the circle is read, and also the barometer and thermometer, in order afterward to be able to calculate accurately the effect of atmospheric refraction on the observed altitude of the star; and then his observation is finished. Thus the work of the meridian observer goes on, star after star, hour after hour, and night after night; and, as you see, it differs very widely from the popular notion of an astronomer's occupation. It presents no dreamy contemplation, no watching for new stars, no unexpected or startling phenomena. On the contrary, there is beside him the carefully prepared observing-list for the night, the previously calculated circle setting for each star, allowing just sufficient time for the new setting for the next star after the readings of the circle for the previous observation.

After four or five hours of this work the observers have had enough of it; they have, perhaps, observed fifty or sixty stars, they determine certain instrumental errors, and betake themselves to bed, tired but (if they are of the right stuff) happy and contented men. At the Cape we employ two observers—the one to read the circle and the other to record the transit. Four observers are employed, and they are thus on duty each alternate night. Such is the work that an outsider would see were he to enter a working meridian observatory at night; but he would find out if he came next morning that the work was by no means over. By far the largest part has yet to follow. An observation that requires only two or three minutes to make at night requires at least half an hour for its reduction by day. Each observation is affected by a number of errors, and these have to be determined and allowed for. Although solidly founded on massive piers resting on solid rock, the constancy of the instrument's position can not be relied upon. It goes through small periodic changes in level, in collimation, and in azimuth, which have to be determined by proper means, and the corresponding corrections have to be computed and applied; and also there are other corrections for refraction, etc., which involve computation and have to be applied. But these matters would fall more properly under the head of a special lecture upon the transit instrument. I mention them now merely to explain why so great a part of an astronomer's work comes in the daytime, and to dispel the notion that his work belongs only to the night.

One might very well occupy a special lecture in an account of the peculiarities of what is called personal equation—that is to say, the different time which elapses for different observers between the time when the observer believes the star to be upon the wire and the time when the finger responds to the message which the eye has conveyed to the brain. Some observers always press the key too soon, some always too late. Some years ago I discovered, from the observations to which I will subsequently refer, that all observers press the chronograph key either too soon for bright stars or too late for faint ones.

Other errors may, and I am sure do, arise both at Greenwich and the Cape from the impossibility of securing uniformity of outside and inside temperature in a building of strong masonry. The ideal observatory should be as solid as possible as to its foundations, but as light as possible as to its roof and walls—say, a light framework of iron covered with canvas. But it would be undesirable to cover a valuable and permanent instrument in this way.

But here is a form of observatory which realizes all that is required, and which is eminently suited for permanent use. The walls are of sheet iron, which readily acquire the temperature of the outer air. The iron walls are protected from direct sunshine by wooden louvres, and small doors in the iron walls admit a free circulation of air. The revolving roof is a light framework of iron covered with well-painted papier-maché.

The photograph now on the screen shows the interior of the observatory, and this brings me to the description of observations of an entirely different class. In this observatory the roof turns round on wheels, so that any part of the sky can be viewed from the telescope. This is so because the instrument in this observatory is intended for purposes which are entirely different from those of a transit circle. The transit circle, as we have seen, is used to determine the absolute positions of the heavenly bodies; the heliometer to determine with greater precision than is possible by the absolute method the relative positions of celestial objects.

To explain my meaning as to absolute and relative positions: It would, for example, be a matter of very little importance if the absolute latitude of a point on the Royal Exchange or the Bank of England were one tenth of a second of arc (or ten feet) wrong in the maps of the Ordnance Survey of England—that would constitute a small absolute error common to all buildings on the same map of a part of the city, and common to all the adjoining maps also. Such an error, regarded as an absolute error, would evidently be of no importance if every point on the map had the same absolute error. There is no one who can say at the present moment whether the absolute latitude of the Royal Exchange—nay, even of the Royal Observatory, Greenwich—is known to ten feet. But it would be a very serious thing indeed if the relative positions on the same map were ten feet wrong here and there. For example, if of two points marking a frontage boundary on Cornhill one were correct, the other ten feet in error—what a nice fuss there would be! what food for lawyers! what a bad time for the Ordnance-Survey Office! Well, it is just the same in astronomy.

We do not know, we probably never shall know with certainty, the absolute places of even the principal stars to one tenth of a second of arc. But one tenth of a second of arc in the measure of some relative position would be fatal. For example, in the measurement of the sun's parallax an error of one tenth of a second of arc means an error of one million miles, in round numbers, in the sun's distance; and it is only when we can be quite certain of our measures of much smaller quantities than one tenth of a second of arc that we are in a position to begin seriously the determination of such a problem as that of the distance of the fixed stars. For these problems we must use differential measures—that is, measures for the relative positions of two objects. The most perfect instrument for such purposes is the heliometer.

Lord McLaren has kindly sent from Edinburgh, for the purposes of this lecture, the parts of his heliometer which are necessary to illustrate the principles of the instrument.

This instrument is the same which I used on Lord Crawford's expedition to Mauritius in 1874. It was also kindly loaned me by Lord Crawford for an expedition to the Island of Ascension to observe the opposition of Mars in 1877. In 1879, when I went to the Cape, I acquired the instrument from Lord Crawford, and carried out certain researches with it on the distances of the fixed stars.

In 1887, when the Admiralty provided the new heliometer for the Cape Observatory, this instrument again changed hands. It became the property of Lord McLaren. I felt rather disloyal in parting with so old a friend. We had spent so many happy hours together, we had shared a good many anxieties together, and we knew each other's weaknesses so well! But my old friend has fallen into good hands, and has found another sphere of work.

There is now on the screen a picture of the new heliometer of the Cape Observatory, which was mounted in 1887, and has been in constant use ever since. It is an instrument of the most refined modern construction, and is probably the finest apparatus for refined measurements of celestial angles in the world.

(Here were explained the various parts of the instrument in relation to the model, and the actual processes of observation were illustrated by the images of artificial stars projected on a screen.)

Here, again, there is little that conforms to the popular idea of an astronomer's work; there is no searching for objects, no contemplative watching, nothing sensational of any kind. On the contrary, every detail of his work has been previously arranged and calculated beforehand, and the prospect that lies before him in his night's work is simply more or less of a struggle with the difficulties which are created by the agitation of the star images, caused by irregularities in the atmospheric refraction. It is not upon one night in a hundred that the images of stars are perfectly tranquil. You have the same effect in an exaggerated way when looking across a bog on a hot day. Thus, generally, as the images are approached, they appear to cross and recross each other, and the observer must either seize a moment of comparative tranquility to make his definitive bisection, or he may arrive at it by gradual approximations till he finds that the vibrating images of the two stars seem to pass each other as often to one side as to the other. So soon as such a bisection has been made the time is recorded on the chronograph, then the scales are pointed on and printed off, and so the work goes on, varied only by reversals of the segments and of the position circle. Generally I now arrange for thirty-two such bisections, and these occupy about an hour and a half. By that time one has had about enough of it; the nerves are somewhat tired, so are the muscles of the back of the neck, and, if the observer is wise and wishes to do his best work, he goes to bed early and gets up again at two or three o'clock in the morning and goes through a similar piece of work. In fact, this must be his regular routine night after night, whenever the weather is clear, if he is engaged, as I have been, on a large programme of work on the parallaxes of the fixed stars, or on observations to determine the distance of the sun by observations of minor planets.

I will not speak now of these researches, because they are still in progress of execution or of reduction. I would rather, in the first place, endeavour to complete the picture of a night's work in a modern observatory.

We pass on to celestial photography, where astrometry and astrophysics join hands. The observer's work during the exposure is simply to direct the telescope to the required part of the sky, and then the clockwork nearly does the rest—but not quite so. The observer holds in his hand a little electrical switch with two keys; by pressing one key he can accelerate the velocity of the driving screw by about one per cent, and by pressing the other he can retard it one per cent. In this way he keeps one of the stars in the field always perfectly bisected by the cross-wires of his guiding telescope, and thus corrects the small errors produced partly by changes of refraction, partly by small unavoidable errors in cutting the teeth of the arc into which the screw of the driving shaft of the clockwork gears.

The work is monotonous rather than fatiguing, and the companionship of a pipe or cigar is very helpful during long exposures. A man can go on for a watch of four or five hours very well, taking plate after plate, exposing each, it may be, forty minutes or an hour. If the night is fine a second observer follows the first, and so the work goes on the greater part of the night. Next day he develops his plate.

Working just in this way, but with the more humble apparatus which you see imperfectly in the picture now on the screen, we have, with a rapid rectilinear lens by Dallmeyer of six inches aperture, photographed at the Cape during the past six years the whole of the southern hemisphere from 20° of south declination to the south pole.

The plates are being measured by Professor Kapteyn, of Groningen, and I expect that in the course of a year the whole work containing all the stars to 9½ magnitude (between 200,000 and 300,000 stars) in that region will be ready for publication. This work is essential as a preliminary step for the execution in the southern hemisphere of the great work inaugurated by the Astrophotographic Congress at Paris in 1887, the last details of which were settled at our meeting at Paris in April last. What we shall do with the new apparatus perhaps I may have the honour to describe to you some years hence, after the work has been done.

We now come to an important class of astronomical work more purely astrophysical, for the illustration of which I can no longer appeal to the Cape, because I regret to say that we are not yet provided with the means for its prosecution. I refer to the use of the spectroscope in astronomy, and especially to the latest developments of its use for the accurate measurement of the velocity of the motions of stars in the line of sight.[2]

It is beyond the province of this lecture to enter into history, but it is impossible not to refer to the fact that the chief impulse to astronomical work in this direction was given by Dr. Huggins, our chairman to-night; nay, more, except for the early contributions of Fraunhofer to the subject, Dr. Huggins certainly is the father of sidereal spectroscopy, and that not in one but in every branch of it. He has devised the means, pointed the way, and, while in many branches of the work he still continues to lead the way, he has of necessity left the development of other branches to other hands.

From an astrometer's point of view the most important

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[2] The older methods enabled us to measure motions at right angles to the line of sight, but till the spectroscope came we could not measure motions in the line of sight.

advance that has been made in spectroscopy of recent years is the sudden development of precision in the measures of star motion in the line of sight. The method remained for fifteen or sixteen years quite undeveloped from the condition in which it left the hands of Dr. Huggins, and certainly no progress in the accuracy attained by Dr. Huggins was made till the matter was taken up by Dr. Vogel at Potsdam. At a single step Dr. Vogel has raised the precision of the work from that of observations in the days of Ptolemy to that of the days of Bradley—from the days of the old sights and pinnules to the days of telescopes. Therefore I take Potsdam observation as the best type of a modern spectroscopic observation for description, especially as I have recently visited Dr. Vogel at Potsdam, and he has kindly given me a photograph of his spectroscope, as well as of some of the work done with it.

The method of observation consists simply in inserting a small photographic plate in the dark slide, directing the telescope to the star, and keeping the image of the star continuously on the slit during an exposure of about an hour; and this is what is obtained on development of the picture.

If the star remained perfectly at rest between the jaws of the slit the spectrum would be represented by a single thread of light, and of course no lines would be visible upon such a thread; but the observer intentionally causes the star image to travel a little along the slit during the time of exposure, and so a spectrum of sensible width is obtained.

You will remark how beautifully sharp are the faint lines in this spectrum. Those who have tried to observe the spectrum of Sirius in the ordinary way know that many of these fine lines can not be seen or measured with certainty. The reason is that on account of irregularities in atmospheric refraction, the image of a star in the telescope is rarely tranquil; sometimes it shines brightly in the centre of the slit, sometimes barely in the slit at all, and the eye becomes puzzled and confused. But the photographic eye is not in the least disturbed; when the star image is in the slit, the plate goes on recording what it sees, and when the star is not in the slit the plate does nothing, and it is of no consequence whatever how rapidly these alternate appearances and disappearances recur. The only difference is that when the air is very steady and the star's image, therefore, always in the slit, the exposure takes less time than when the star is unsteady.

That is one reason why the Potsdam results are accurate; and there are many other reasons besides, into which I can not now enter. What, however, it is very important to note is this: that we have here a method which is to a great extent independent of the atmospheric disturbances which in all other departments of astronomical observation have imposed a limit to their precision. Accurate astrospectroscopy, therefore, may be pushed to a degree of perfection which is limited only by the optical aid at our disposal and by the sensibility of our photographic plates.

And now I think we have sufficiently considered the ordinary processes of astronomical observation to illustrate the character of the work of an astronomer at night; the picture should be completed by an account of his work by day. But to go into that matter in detail would certainly not be within the limits of this lecture. It is better that I should in conclusion touch upon some recent remarkable results of these day and night labours. It is these, after all, that most appeal to you; it is for these that the astronomer labours; it is the prospect of them that lightens the long watches of the night and gives life to the otherwise dead bones of mechanical routine.

Let us take first some spectroscopic results. To explain their meaning let me remind you for a moment of the familiar analogy between light and sound.

The pitch of a musical note depends on the rapidity of the vibrations communicated to the air by the reed or string of the musical instrument that produces the note, a low note being given by slow vibrations and a high one by quick vibrations.

Just in the same way red light depends on relatively slow vibrations of ether, and blue or violet light on relatively quick vibrations. Well, if there is a railway train rapidly approaching one, and the engine sounds its whistle, more waves of sound from that whistle will reach the ear in a second of time than would reach the ear were the train at rest. On the other hand, if the train is travelling at the same rate away from the observer, fewer waves of sound will reach his ears in a second of time. Therefore an observer beside the line should observe a distinct change of pitch in the note of the engine whistle as the train passes him, and as a matter of fact such a change of pitch can be and has been observed.

Just in the same way, if a source of light could be moved rapidly enough toward an observer it would become bluer, or if away from him it would become red in colour; only it would require a change of velocity in the moving light of some thousands of miles per second in order to render the difference of colour sensible to the eye. The experiment is, therefore, not likely to be frequently shown at this lecture table!

But the spectroscope enables such changes of colour to be measured with extreme precision. Here on the screen is the most splendid illustration of this that exists at present—viz., copies of three negatives of the spectrum of α Aurigæ, taken at Potsdam in October and December of 1888, and in March, 1899.

The white line (the picture being a positive) represents the bright line Hγ given by the artificial light of hydrogen; the strong black line in the picture of the star spectrum corresponds to the black absorption line which is due to hydrogen in the atmosphere of the star.

Why is it that the artificial hydrogen line does not correspond with the stellar line in these three pictures? The answer is, either the star is moving toward or from the earth in the line of sight, or the earth is moving from or toward the star. But in December the earth in its motion round the sun is moving at right angles to the direction of *a* Aurigæ; why, then, does the stellar hydrogen line not agree in position with the terrestrial hydrogen line? The simple explanation is that *a* Aurigæ is moving with respect to the sun.

In what way is it moving? Well, that is also clear; the stellar line is displaced toward the red end of the spectrum —that is to say, the star light is redder than it should be in consequence of a motion of recession; this proves that the star is moving away from us, and measures of the photograph show the rate of this motion to be fifteen and a half miles per second. We also know that in October the earth in its motion round the sun is moving toward *a* Aurigæ nearly at the same rate as we have just seen that *a* Aurigæ is running away from the sun. Consequently, at that time, their relative motions are nearly insensible, because both are going at the same rate in the same direction, and we find accordingly, in October, that the positions of the stellar and artificial hydrogen lines perfectly correspond. Finally, in March, the earth in its motion round the sun is moving away from *a* Aurigæ, and as *a* Aurigæ is also running away from the sun, the starlight becomes so much redder than normal that the stellar hydrogen line is shifted completely to one side of the hydrogen and artificial line.

The accuracy of these results may be proved as follows:

If we measure all the photographs of *a* Aurigæ which Dr. Vogel has obtained, we can derive from each a determination of the relative velocity of the motion of the star with respect to our earth.

Of course these velocities are made up of the velocity of motion of *a* Aurigæ with respect to the sun (which we may reasonably assume to be a uniform velocity) and the velocity of the earth due to its motion round the sun. But the velocity of the earth's motion in its orbit is known with an accuracy of about one five-hundredth part of its amount, and therefore, within that accuracy, we can allow precisely for its effect on the relative velocity of the earth and *a* Aurigæ. When we have done so we get the annexed results for the velocity of the motion of *a* Aurigæ with respect to the sun. You see by the annexed table how beautifully they agree in the Potsdam results, and how comparatively rough and unreliable are the results obtained by the older method of Greenwich.[3]

I believe that in a few years—at least in a period of time that one may hope to see—we shall not be content merely to correct our results for the motion of the earth in its orbit only, and so test our observations of motion

[3] a *Aurigæ—Potsdam*

Date.	Observed relative motion of earth and star. Miles per second.	Motion of earth.	Concluded motion. Star relative to sun.
1888.			
October 22d	+2.5	−13.0	+15.5
" 24th	+3.1	−12.4	+15.5
" 25th	+3.1	−12.4	+15.5
" 28th	+2.5	−11.8	+14.3
November 9th	+6.8	−8.7	+15.5
December 1st	+11.8	−3.1	+14.9
" 13th	+14.9	+0.6	+14.3
1889.			
January 2d	+20.5	+0.8	+13.7
February 5th	+32.9	+14.3	+18.6
March 6th	+34.2	+16.8	+17.4

a *Aurigæ—Greenwich*

Date.	Observed relative motion of earth and star. Miles per second.	Motion of earth.	Concluded motion. Star relative to sun.
1887.			
January 26th	+16.4	+12.6	+3.8
February 16th	+34.4	+15.9	+18.5
October 22d	+39.8	−13.5	+52.3
" 25th	+25.4	−13.0	+38.4
" 29th	+40.6	−12.1	+52.7
1888.			
December 7th	+29.0	−1.2	+36.2
1889.			
February 15th	+23.8	+16.0	+7.8
March 5th	+20.3	+17.1	+3.2
September 17th	+18.6	−13.3	+33.3
" 19th	+21.8	−16 7	+38.5
" 25th	+24.8	−16.5	+41.3
November 25th	+24.5	−4.9	+29.4

in the line of sight, but that we shall have arrived at a certainty and precision of working which will permit the process to be reversed, and that we shall be employing the spectroscope to determine the velocity of the earth's motion in its orbit, or, in other words, to determine the fundamental unit of astronomy, the distance of the sun from the earth.

I will take as another example one recent remarkable spectroscopic discovery. Miss Murray, in examining a number of photographs of stellar spectra taken at Harvard University, discovered that in every spectrum of β Aurigæ certain lines doubled themselves every two days, becoming single in the intermediate days. Accurate Potsdam observations confirmed the conclusion.

The picture on the screen shows the spectrum of β Aurigæ photographed on November 22d and 25th of last year. In the first the lines are single, in the other every line is doubled. Measures and discussions of a number of these photographs have shown that the doubling of the lines is perfectly accounted for by the supposition of two suns revolving round each other in a period of four days, each moving at a velocity of about seventy miles a second in its orbit.

When one star is approaching us and the other receding, the lines in the spectrum formed by the light of the first star will be moved toward the blue end of the spectrum, those in the spectrum of the second star toward the red end of the spectrum; then, as the two stars come into the same line with us, their motions become at right angles to the line of sight, and their two spectra, not being affected by motion, will perfectly coincide; but then, after the stars cross, their spectra again separate in the opposite direction, and so they go on.

Thus by means of their spectra we are in a position to watch and to measure the relative motions of two objects that we can never see apart; nay, more, we can determine not only their period of revolution, but also the velocity of their motions in their orbits. Now, if we know the time that a body takes to complete its revolution, and the velocity at which it moves, clearly we know the dimensions of the orbit; and if we know the dimensions of an orbit we know what attractive force is necessary to compel the body to keep in that orbit, and thus we are able to weigh these bodies. The components of β Aurigæ are two suns, which revolve about each other in four days; they are only between 7,000,000 and 8,000,000 miles (or one twelfth of our distance from the sun) apart, and if they are of equal weight they each weigh rather over double the weight of the sun.

I have little doubt that these facts do not represent a permanent condition, but simply a stage of evolution in the life history of the system, an earlier stage of which may have been a nebular one.

Other similar double stars have been discovered, both at Potsdam and at Cambridge, United States—stars that we shall never see separately with the eye aided by the most powerful telescope; but time does not permit me to enter into any account of them.

I pass now to another recent result that is of great cosmical interest. The Cape photographic star charting of the southern hemisphere has been already referred to. In comparing existing eye estimates of magnitude by Dr. Gould with the photographic determinations of these magnitudes, both Professor Kapteyn and myself have been greatly struck with a very considerable systematic discordance between the two. In the rich parts of the sky—that is, in the Milky Way—the stars are systematically photographically brighter by comparison with the eye observations than they are in the poorer part of the sky, and that not by any doubtful amount but by half or three fourths of a magnitude. One of two things was certain: either that the eye observations were wrong, or that the stars of the Milky Way are bluer or whiter than other stars. But Professor Pickering, of Cambridge, America, has lately been making a complete photographic review of the heavens, and by placing a prism in front of the telescope he has made pictures of the whole sky like this. (Here two examples of plates of Pickering's spectroscopic "Durchmusterung" were exhibited on the screen.) He has discussed the various types of the spectra of the brighter stars as thus revealed, according to their distribution in the sky. He finds thus that the stars of the Sirius type occur chiefly in the Milky Way, while stars of other types are fairly divided over the sky.

Now, stars of the Sirius type are very white stars, very rich relative to other stars in the rays which act most strongly on a photographic plate. Here, then, is the explanation of the results of our photographic star-charting, and of the discordance between the photographic and visual magnitudes in the Milky Way.

The results of the Cape charting further show that it is not alone to the brighter stars that this discordance extends, but it extends also, though in a rather less degree, to the fainter stars of the Milky Way. Therefore we may come to the very remarkable conclusion that the Milky Way is a thing apart, and that it has been developed perhaps in a different manner, or more probably at a different and probably later epoch from the rest of the sidereal universe.

Here is another interesting cosmical revelation which we owe to photography. You all know the beautiful constellation Orion, and many in this theatre have before seen the photograph of the nebula which is now on the screen, taken by Mr. Roberts. Here is another photograph of the same object taken with a much longer exposure. You see how over-exposed—in fact, burned out—the brightest part of the picture is, and yet what a wonderful development of faint additional nebulous matter is revealed. But I do not think that many persons in this room have seen this picture, and probably very few have any idea what it represents. It is from the original negative taken by Professor Pickering, with a small photographic lens of short focus, after six hours' exposure in the clear air of the Andes, 10,000 feet above sea level. The field embraces the three well-known stars in the belt of Orion on the one hand,

and β Orionis (Rigel) on the other. You can hardly recognise these great white patches as stars; their ill-defined character is simply the result of excessive over-exposure. But mark the wonders which this long exposure with a lens of high intrinsic brilliancy of image has revealed. Here is the great nebula, of course terribly over-exposed, but note its wonderful fainter ramifications. See how the whole area is more or less nebulous, and surrounded, as it were, with a ring fence of nebulous matter. This nebulosity shows a special concentration about β Orionis.

Well, when Professor Pickering got this wonderful picture, knowing that I was occupied with investigations on the distance of the fixed stars, he wrote to ask whether I had made any observations to determine the distance of β Orionis, as it would be of great interest to know from independent evidence whether this very bright star was really near to us or not. It so happens that the observations were made, and their definitive reduction has shown that β Orionis is really at the same distance from us as are the faint comparison stars. β Orionis is, therefore, probably part and parcel of an enormous system in an advanced but incomplete state of stellar evolution, and that what we have seen in this wonderful picture is all a part of that system.

I should explain what I mean by an elementary or by an advanced state of stellar evolution. There is but one theory of celestial evolution which has so far survived the test of time and comparison with observed facts—viz., the nebular hypothesis of Laplace. Laplace supposed that the sun was originally a huge gaseous or nebulous mass of a diameter far greater than the orbit of Neptune. I say originally; do not misunderstand me. We have finite minds; we can imagine a condition of things which might be supposed to occur at any particular instant of time, however remote, and at any particular distance of space, however great; and we may frame a theory beginning at another time still more remote, and so on. But we can never imagine a theory beginning at an infinite distance of time or at an infinitely distant point in space. Thus, in any theory which man with his finite mind can devise, when we talk of " originally " we simply mean at or during the time considered in our theory.

Now, Laplace's theory begins at a time, millions on millions of years ago, when the sun had so far disentangled itself from chaos, and its component gaseous particles had by mutual attraction so far coalesced as to form an enormous gaseous ball, far greater in diameter than the orbit of the remotest planet of our present system. The central part of this ball revolved. There is nothing improbable in this hypothesis. If gaseous matter came together from different parts of space such coalition would unquestionably occur, and as in the meeting of opposite streams of water or of opposite currents of wind, vortices would be created and revolution about an axis set up, such as we are familiar with in the case of whirlpools or cyclones. The resultant would be rotation of the whole globular gaseous mass about an axis.

Now, this gaseous globe begins to cool, and as it cools it necessarily contracts. Then follows a necessary result of contraction—viz., the rotation becomes more rapid. This is a well-known fact in dynamics, about which there is no doubt. Thus, the cooling and the contracting go on, and simultaneously the velocity of rotation becomes greater and greater. At last the time arrives when, for the outside particles, the velocity of rotation becomes such that the centrifugal force is greater than the attractive force, and so the outside particles break off and form a ring. Then, as the process of cooling and contraction proceed still further, another ring is formed, and so on until we have finally a succession of rings and a condensed central ball. If from any cause the cooling of any of these rings does not go on uniformly, or if some of the gaseous matter of the ring is more easily liquefied than others, then probably a simple nucleus of liquid matter will be formed in that ring, and this nucleus will finally by attraction absorb the whole of the matter of which the ring is composed—at first as a gaseous ball with a condensed nucleus, and this will finally solidify into a planet. Or, meanwhile, this yet unformed planet may repeat the history of its parent sun. By contraction, and consequent acceleration of its rotation, it may throw off one or more rings, which in like manner condense into satellites like our moon, or those of Jupiter, Saturn, Uranus, or Neptune. Such, very briefly outlined, is the celebrated nebular hypothesis of Laplace. No one can positively say that the hypothesis is true; still less can any one say that it is untrue. Time does not permit me to enter into the very strong proofs which Laplace urged in favour of its acceptance.

But I beg you for one moment to cast your imagination back to a period of time long antecedent to that when our sun had begun to disentangle itself from chaos, and when the fleecy clouds of cosmic stuff had but begun to rush together. What should we see in such a case were there a true basis for the theory of Laplace? Certainly, in the first place, we should have a huge whirlpool or cyclone of cosmic gaseous stuff, the formation of rings, and the condensation of these rings into gaseous globes.

Remembering this, look now on this wonderful photograph of the nebula in Andromeda, made by Mr. Roberts. In the largest telescopes this nebula appears simply as an oval patch of nearly uniform light, with a few dark canals through it, but no idea of its true form can be obtained, no trace can be found of the significant story which this photograph tells. It is a picture that no human eye unaided by photography has ever seen. It is a true picture, drawn without the intervention of the hand of infallible man, and uninfluenced by his bias or imagination. Have we not here, so at least it seems to me, a picture of a very early stage in the evolution of a star cluster or sun-system—a phase in the history of another star-system similar to that which once occurred in our own—millions and millions of years ago—when our earth—nay, even our sun itself—"was without form and void," and "darkness was on the face of the deep"?

During this lecture I have been able to trace but very imperfectly the bare outlines of an astronomer's work in a modern observatory, and to give you a very few of its latest results—results which do not come by chance, but by hard labour, and to men who have patience to face dull, daily routine for the love of science—to men who realize the imperfections of their methods, and are constantly on the alert to improve them.

The mills of the astronomer grind slowly, and he must be infinitely careful and watchful if he would have them, like the mills of God, to grind exceedingly small. I think he may well take for his motto these beautiful lines:

> "Like the star
> Which shines afar,
> Without haste,
> Without rest,
> Let each man wheel
> With steady sway
> Round the task
> Which rules the day,
> And do his best."

THE SYSTEM OF THE WORLD—THE NEBULAR HYPOTHESIS

BY

PIERRE SIMON, MARQUIS DE LAPLACE

(Born 1749; died 1827)

THE SYSTEM OF THE WORLD[1]

WE have five phenomena, as follows, from which to ascend to the cause of the primitive motions of the planetary system: The motions of the planets are in the same direction and almost in the same plane; the motions of the satellites are in the same direction as the planets; the motions of rotation of these several bodies and of the sun are in the same direction as their motions of translation, and in planes only slightly inclined to each other; the eccentricity of the orbits of the planets and of the satellites is very small; and, lastly, the eccentricity of the orbits of the comets is large, although the inclinations of these bodies may have been the result of chance.

Buffon is the only philosopher known to me who, since the discovery of the true system of the world,[2] has attempted to explain the origin of the planets and of the satellites. He supposes that a comet, falling upon the sun, has expelled from it a torrent of matter which has coalesced once more, at a distance, into various globes, larger or smaller, and nearer or farther from that heavenly body; and that these globes, having become opaque and solid as they cooled, are the planets and their satellites.

This hypothesis explains the first of the five phenomena

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[1] "Œuvres Complétes de Laplace" (reprint of 1884, from fifth edition, 1835), publiées sous les auspices de l'Académie des Sciences par MM. les Secrétaires perpetuels. Paris, 1884, vol. vi. "Système du Monde," note vii, pp. 498–509. Translated by Sara Carr Upton.

[2] By Copernicus, Kepler, and Newton.—Editor.

previously cited, for it is clear that all the bodies thus formed must move very nearly in the plane which passes through the centre of the sun, and in the direction of the torrent of matter which has produced them; the other four phenomena seem to me inexplicable by his method. In truth, the absolute motions of the molecules of a planet would then be in the same direction as the motion of its centre of gravity. But it does not at all follow that the rotation-motion of the planet would have the same direction; thus the earth might turn from east to west, and yet the absolute motion of each of its molecules might be directed from west to east. This would also apply to the motion of revolution of the satellites, the direction of which, under the hypothesis in question, is not necessarily the same as the direction of the motion of translation of the planets in their orbits.

A phenomenon not only very difficult to explain under this hypothesis, but one even contrary to it, is the small eccentricity of the planetary orbits. It is known from the theory of central forces that if a body moving in an orbit revolving around the sun touches the surface of that heavenly body, it will constantly return to the same point there at each of its revolutions; whence it follows that, had the planets been detached from the sun at their origin, they would touch it upon each of their returns toward that body, and that their orbits, far from being circular, would be very eccentric. It is true that a torrent of matter expelled from the sun can not be exactly compared to a globe which grazes its surface; the impulsion which the parts of such a torrent would receive from each other, and the reciprocal attraction they would exert among themselves as they changed their direction of motions, might force their perihelions farther from the sun. But their orbits would always be very eccentric, or at least they could not all of them have small eccentricities, except by the most extraordinary chance. Lastly, under Buffon's hypothesis it can not be explained why the orbits of more than one hundred comets already observed are all much elongated. This hypothesis, therefore, is very far from explaining the preceding phenomena. Let us see if it is possible to rise to their veritable cause.

Whatever may be the nature of this cause, since it has produced or directed the motions of the planets it must have comprehended all such bodies, and in view of the prodigious distances which separate them one from another, it can only have been a fluid of immense extension. To have given the man almost circular motion in the same direction around the sun, this fluid must have surrounded that body like an atmosphere. The consideration of the planetary motions thus leads us to think that by reason of excessive heat the atmosphere of the sun originally extended beyond the orbits of all the planets, and that it has successively shrunk to its present limits.

In the primitive condition that we have ascribed to the sun it resembled the nebulæ, which the telescope shows to be composed of a nucleus more or less brilliant, surrounded by nebulosity which, as it condenses on the surface of the nucleus, transforms it into a star. If, by analogy, we conceive all stars to be formed in this manner, we may imagine their former state of nebulosity which was itself preceded by other states in which the nebulous matter was still more diffused, the nucleus still less luminous. We thus reach, going backward as far as possible, a nebulosity so diffused that its existence is barely manifest.

For a long time the special configuration of certain stars visible to the naked eye has struck philosophical observers. Mitchel has already remarked how improbable it is that the stars of the Pleiades, for example, should have been clustered together in the small region containing them as the result of mere chance, and he concludes therefrom that this group of stars and other similar groups displayed before us in the sky are the effects of some primitive cause, or of a general law of Nature. Such groups are a necessary result of the condensation of nebulæ with several nuclei, for it is plain that the nebulous matter, being constantly attracted by these different nuclei, must finally form a group of stars like the Pleiades. The condensation of nebulæ with two nuclei similarly will form stars in very close proximity, which will turn one around the other, similar to those double stars whose relative motions have already been determined.

But how did the solar atmosphere determine the motions of rotation and revolution of the planets and of the satellites? Had those bodies penetrated far into that atmosphere its resistance would have caused them to fall upon the sun. It may therefore be conjectured that the planets were formed at the limits of the solar atmosphere by the condensation of the zones of vapours, which, as it cooled, the atmosphere successively abandoned in the plane of its equator.

The atmosphere of the sun can not extend outward indefinitely; its limit is the point where the centrifugal force due to its motion of rotation balances its weight. Now, as the cooling contracts the atmosphere by degrees, and condenses the neighbouring molecules on the surface of the body, the motion of rotation augments; for, according to the principle of "conservation of areas," the sum of the areas described by the radius-vector of each molecule of the sun and of its atmosphere, projected upon the plane of its equator, being always the same, the rotation must be more rapid when such molecules are near the sun's centre. The centrifugal force due to this motion thus becoming greater, the point where the weight is equal to it is nearer to that centre. Supposing, therefore, which it is natural to admit, that the atmosphere reached at some epoch to its extreme limit, it must, in cooling, have abandoned the molecules situated at that limit and at the successive limits due to the increase in the rotation of the sun. These abandoned molecules have continued to circulate around that body, since their centrifugal force was balanced by their weight. But this equality not existing for the atmospheric molecules situated upon parallels of the solar equator, the latter molecules have been drawn by their weight nearer to the solar atmosphere as it progressively condensed, and they have not ceased to belong to it, until by such motion they have drawn close to that equator.

Let us consider now the zones of vapours that have been successively abandoned. Such zones must, in all probability, have formed, by their condensation and the mutual attraction of their molecules, various concentric rings of vapours revolving around the sun. The mutual friction of the molecules of each ring must have accelerated some and retarded others until all acquired a like angular motion. Thus the real velocities of the molecules farthest from the centre of the sun were the greatest. The following cause must also have contributed to this difference of velocity: The molecules farthest from the sun, and those which from the effects of cooling and condensation have drawn near the sun to form the upper part[3] of the ring, constantly described areas proportional to the time, since the central force which animated them was constantly directed toward the sun. Now, this conservation of areas necessitates an increase of velocity in proportion to their proximity to the sun. We see that the same cause must have diminished the velocity of the molecules which have moved to form its lower part of the ring.[4]

If all the molecules of a ring of vapours continued to condense without disuniting, they would finally form a liquid or solid ring. But the regularity in all the portions of the ring and in their cooling, demanded by such a formation, must have rendered such a phenomenon extremely rare. Thus the solar system presents but a single example of it—the rings of Saturn. Almost always each ring of vapours must have broken into several masses which, moving by very slightly differing velocities, continued to revolve at the same distance around the sun. Such masses would have assumed a spheroidal form, with a motion of rotation in the same direction as that of their revolution,

[3] The part most distant from the sun.

[4] That is, the part nearest to the sun.

since their lower[5] molecules had less real velocity than their upper[6] ones; they have found thus so many planets in a vaporous state. But if one has been powerful enough to reunite successively by its attraction all the others around its own centre, the vaporous ring would have been thus transformed into a single spheroidal mass of vapours, revolving around the sun with a rotation in the direction of its revolution. The latter case has been the most common; nevertheless, the solar system presents the first case in the four small planets which move between Jupiter and Mars,[7] unless we suppose with M. Olbers that they originally formed a single planet that has been divided by some great explosion into several portions having different velocities.

Now, if we follow the changes that subsequent cooling would have produced in the planets in a vaporous state, the formation of which we have been conceiving, we shall see produced in the centre of each of them a nucleus that will constantly increase by the condensation of the atmosphere surrounding it. In this state the planet would exactly resemble the sun in the nebulous state in which we have just been considering it: the process of cooling would therefore have produced at the various limits of its atmosphere phenomena similar to those which we have described —that is to say, rings and satellites revolving around its centre in the direction of its motion of rotation and turning in the same direction upon themselves. The regular distribution of the mass of the rings of Saturn around its centre, and in the plane of its equator, follows naturally from this hypothesis and without it is inexplicable; these rings seem to me to be ever-existing proofs of the original extension of the atmosphere of Saturn and of its successive recessions. Thus the singular phenomena of the slight eccentricity of the orbits of the planets and of the satellites, of the slight inclination of those orbits to the solar equator, and of the identity of direction of the motions of rotation and of revolution of all those bodies with that of

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[5] Nearer the sun. [6] Farther from the sun. [7] The asteroids.

the rotation of the sun, flow from the hypothesis which we propose, and give it a great probability, which may be still more augmented by the following consideration:

All the satellites that revolve around a planet having been formed, according to this hypothesis, by the zones successively abandoned by the planet's atmosphere, and its motion of rotation having become more and more rapid, the duration of its motion of rotation must be less than the durations of the revolutions of these various bodies. The case is the same for the sun as compared with the planets.[8] All this is confirmed by observations. The duration of the revolution of Saturn's nearest ring is, according to Herschel's observations, $\frac{438}{1000}$ of a day, and that of the rotation of Saturn is but $\frac{427}{1000}$. The difference, $\frac{11}{1000}$, is very slight, as it should be, because the portion of Saturn's atmosphere which the diminution of heat has deposited on its surface since the formation of the ring having been small, and coming from a small distance above the planet, it could have only augmented the rotation of the planet in a small degree.

Had the solar system been formed with perfect regularity, the orbits of the bodies which compose it would have been circles whose planes, as well as the planes of the several equators and rings, would have coincided with the plane of the solar equator. But it is readily conceivable that the numberless varieties of temperature and of density of the various parts, which must have existed in these great masses, have produced the eccentricities in their orbits and the deviations in their motions from the plane of that equator.

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[8] Kepler, in his work, "De motibus stellæ Martis," has explained the motion of all the planets in a like direction, as caused by immaterial substances emanating from the surface of the sun, which, preserving the rotation motion that they had at the surface, gave like motions to the planets. He concludes therefrom that the sun turns upon itself in a period less than that of the revolution of Mercury, which Galileo determined to be the case by observation soon afterward. Kepler's hypothesis is doubtless inadmissible, but it is noteworthy that he should have made the identity of direction of the planetary motions depend on this rotation of the sun, so natural would such tendency appear.

Under our hypothesis, comets are foreign to the planetary system. In considering them, as we have done, as small nebulæ wandering about from solar system to solar system, and formed by the condensation of the nebulous matter scattered with so much profusion throughout the universe, we see that when they arrive in that part of space where the attraction of the sun predominates, it forces them to describe elliptical or hyperbolical orbits. But their velocities being equally possible in all directions, they must move indifferently in all directions and with every degree of inclination to the ecliptic, which conforms with what is actually observed. Thus the condensation of the nebulous matter by which we have just explained the motions of rotation and of revolution of the planets and satellites in the same direction and in slightly differing planes, explains equally why the motions of the comets depart from the general law.

The great eccentricity of the cometary orbits is also a result of our hypothesis. If these orbits are elliptical they are very elongated, since their major axes are at least equal to the radius of the sphere of activity of the sun. But these orbits may be hyperbolic, and if the axes of these hyperbolas are not very large in relation to the mean distance of the sun from the earth, the motion of the comets which describe them will appear sensibly hyperbolic. However, out of at least a hundred comets whose elements we know, none has appeared to move in a hyperbola; it must be then that the circumstances which produce a sensible hyperbola are extremely rare in relation to the contrary circumstances. Comets are so small that they do not become visible until their perihelion distance is inconsiderable. Up to the present time this distance for any comet has only twice surpassed the diameter of the terrestrial orbit, and oftenest it has been less than the radius of that orbit. It is obvious that to approach so near to the sun their velocity at the moment of their entrance into its sphere of activity must have an amount and a direction comprised within narrow limits. In determining by analysis of the probabilities the ratio of the chances which, in such limits, give a sensible hyperbola to the chances which give an orbit likely to be confounded with a parabola, I have found that there are at least six thousand chances to one that a nebula which penetrates into the sphere of activity of the sun, so as to be observed, will describe either a very elongated ellipse or a hyperbola that from the size of its axis will be sensibly confounded with a parabola in the portion of the orbit that is observed. It is not surprising, therefore, that so far hyperbolic motions have not been recognised.

The attraction of the planets, and perhaps also the resistance of the ethereal media, must have changed many cometary orbits into ellipses whose major axes are much smaller than the radius of the sphere of activity of the sun. This change may also result from the meeting of these heavenly bodies; for it follows, from our hypothesis of their formation, that there must have been a prodigious number of them in the solar system, it having been possible to observe only those which approach near enough to the sun. It is to be believed that such a change took place for the orbit of the comet of 1759, whose major axis only exceeded thirty-five times the distance from the sun to the earth. A still greater change happened to the orbits of the comets of 1770 and 1805.

If a certain number of comets penetrated the atmospheres of the sun and of the planets at the epoch of their formation, they must have fallen upon those bodies in spiral orbits, and by their fall have removed the planes of the orbits and of the equators of the planets from the plane of the solar equator.

If in the zones abandoned by the atmosphere of the sun there have been molecules too volatile to unite among themselves or with the planets, they must, while they continue to revolve around the sun, offer every appearance of the zodiacal light, without opposing any sensible resistance to the various bodies of the planetary system, because of their extreme rarity, or because their motion is very nearly the same as that of the planets which they meet.

The closer examination of all the circumstances of this system still further increases the probability of our hypothesis. The primitive fluidity of the planets is clearly indicated by the flattening of their figures, in conformity with the laws of the mutual attraction of their molecules. It is, moreover, proved in regard to the earth, by the regular diminution of gravity from the equator to the poles. This state of original fluidity to which we are led by astronomical phenomena ought to be manifested in the phenomena which natural history presents to us. But to find it there it is necessary to take into consideration the immense variety of combinations formed by all the terrestrial substances mingled in a vaporous state, when the lowering of the temperature has allowed their elements to unite. We must next consider the prodigious changes which such lowering of temperature must have brought about successively in the interior and at the surface of the earth, in all its products, in the constitution and pressure of the atmosphere, in the ocean and in the bodies which it has held in solution. Lastly, the abrupt changes, such as the great volcanic eruptions which must have disturbed at various times the regularity of the changes, must be noted. Geology, from this point of view, which links it with astronomy, might, with respect to many objects, acquire the precise and certain knowledge of the latter science.

One of the most singular phenomena of the solar system is the strict equality which has been observed between the angular motions of rotation and of revolution of each satellite. It is a wager of infinity against one that this is not the result of chance. The theory of universal gravitation causes the infinity to disappear from this improbability, and shows us that for the phenomenon to exist it suffices that at the beginning the two motions shall differ but slightly. Then the attraction of the planet establishes a perfect equality between them, but at the same time it gives rise to a periodic oscillation in that axis of the satellite which is directed toward the planet, an oscillation the extent of which depends on the original difference of the two motions. The observations of Mayer upon the libration of the moon and those which Messrs. Bouvard and Nicollet have just made upon the subject, at my request, not having shown this oscillation, the difference on which it depends must be very small. This points with extreme probability to a special cause, which at first has confined that difference within very narrow limits, where the attraction of the planet has been able to establish a strict equality between the mean motions of rotation and revolution, and which afterward ended by destroying the oscillation which was produced by such an equality. Both of these effects are results of our hypothesis, for it is obvious that the moon in a vaporous state formed, under the powerful attraction of the earth, an elongated spheroid, whose major axis must have been constantly directed toward that planet, owing to the readiness with which vapours yield to the slightest forces acting upon them. The terrestrial attraction continuing to act in the same manner so long as the moon was in a fluid state must at last, by constantly causing the two motions of that satellite to approximate, have made their difference fall within the limits where their strict equality begins to establish itself. Afterward this attraction must have, little by little, destroyed the oscillation which that equality produced in the major axis of the spheroid directed toward the earth. It is thus that the fluids which cover this planet have destroyed by their friction and by their resistance the original oscillations of its axis of rotation which now is subject only to the nutation resulting from the actions of the sun and of the moon. It is easy to convince one's self that the equality of the motions of rotation and of revolution of the satellites must have placed an obstacle to the formation of rings and of secondary satellites by the atmospheres of those bodies. Thus, so far, observation has indicated nothing of the sort.

The motions of the first three satellites of Jupiter present a phenomenon still more extraordinary than the preceding. It consists in the fact that the mean longitude of the first, less three times that of the second, plus twice that of the third, is constantly equal at two right angles. There is infinity against one to be wagered that this equality is not due to chance.

But we have just seen that, in order to produce it, it was sufficient that the mean motions of these three bodies should approximately satisfy the ratio which makes the mean motion of the first, less three times that of the second, plus twice that of the third, equal to zero. Their mutual attraction has subsequently established this relation rigorously, and furthermore has made the mean longitude of the first satellite less three times that of the second plus twice that of the third equal to a semi-circumference. At the same time a periodic inequality has arisen which depended upon the small quantity by which the mean motions originally deviated from the relation we have just stated. However much care Delambre has taken to prove this inequality by observations, he has not succeeded, which proves its extreme smallness, and which consequently shows with great probability a cause which has made it disappear. According to our hypothesis the satellites of Jupiter, immediately after their formation, did not move in a perfect vacuum; the least condensable molecules of the original atmospheres of the sun and of the planet formed then a rare medium whose resistance, differing for each of the heavenly bodies, caused their mean motions of the relation in question to approximate little by little. When these motions have thus attained the conditions required in order that the mutual attraction of the three satellites should establish this relation rigorously, the same resistance constantly diminished the inequality which the relation caused at first, and finally rendered it insensible. We can not better compare these effects than to the motion of a pendulum moving by great velocity in a medium of small resistance. At first it will describe a great number of circumferences, but at last its motion of revolution, constantly decreasing, will be changed into a motion of oscillation, which, itself diminishing more and more by the resistance of the medium, will finally be destroyed; then the pendulum, having come to rest, will forever remain in that condition.

ARE THE PLANETS HABITABLE

BY

GEORGE M. SEARLE

ARE THE PLANETS HABITABLE[1]

HAVING completed our survey of the planetary system in which we live, a question naturally occurs to us, which has occurred to every inquiring mind since the real dimensions of the orbs belonging to it were known. To the great majority of mankind it is, and is rightly, a question of greater interest than any one with which mathematics or physics has to deal; of greater interest, since life is a much higher and nobler thing than machinery, and the spiritual far above the material. This question is, "Are these planets which, like our earth, move in their appointed paths around the sun, and on which there is certainly ample room for a population far greater than what our globe could support, actually inhabited by beings in any way like ourselves?"

Almost every astronomer has probably been asked what his views are on this question, and whether his science has anything to tell us about it. At each successive increase in the size of telescopes, men vaguely hope that with the new optical power it may be possible to discover some signs of sentient, and perhaps even of intelligent, life in the celestial worlds. "How much does this telescope magnify?" is always the interesting question to the popular mind. The professional astronomer perhaps is not looking so much for that. He wants to get more light; to see and to delineate faint nebulæ, to follow a comet as far as he can into the darkness of space, in order to determine its orbit as well as possible; but the world in general has comparatively little sympathy with him in this. The discovery

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[1] **A lecture delivered before the Catholic University of America.**

of one intelligent being outside this planet of ours would be more interesting to most men here than all the comets which ever have been or ever will be seen.

Is it then possible that the power of telescopes will at any time be so increased that any discovery of this kind can be made? That is what people would like to know. Let us answer this question in the first place.

The moon is our nearest neighbour. If we can magnify enough to see an object the size of a man on any of the planetary orbs, we must first be able to see such an object on the moon. Is it possible to obtain a magnifying power sufficient for this?

It is possible, we answer, to have such a magnifying power; but the difficulty is to avail ourselves of such a power when we have got it. The great and turbulent sea of atmosphere which lies above us is a seemingly insuperable difficulty. To some extent, of course, we can get free from this by placing our telescope on some high mountain; but there is no mountain high enough to place us altogether out of the atmosphere, and if there were one, we could not live or carry a telescope there. At the highest point at which observations would be possible, which probably would be a good deal below the summit of the Himalayas, enough air still would remain above us to prevent our using a power high enough to discern men like ourselves on the face of our satellite. The tremulousness and waviness produced in the telescopic image by the air, which is, of course, increased the more we magnify, would hopelessly obscure outlines so delicate as those here concerned, and make of such small points a simple invisible blur.

Even for the moon, then, the direct discovery of animal life by increased optical power would seem to be a dream which will never be realized. The difficulty, of course, is immensely increased for any other celestial object. No other planet comes nearer to us than about one hundred times the moon's distance; and, moreover, in examining them, we should have to contend with the confusion of outlines coming from their atmospheres as well as from our own.

We may then as well give up hope of trying to answer the question, " Are the planets inhabited? " as one which never will be solved for us in this world by any natural means; and fall back on another, on which science, certainly, can give us some light—namely, " Are they habitable; are the physical conditions such in them, so far as we can ascertain, that the life of man or of any highly organized animal could there subsist? "

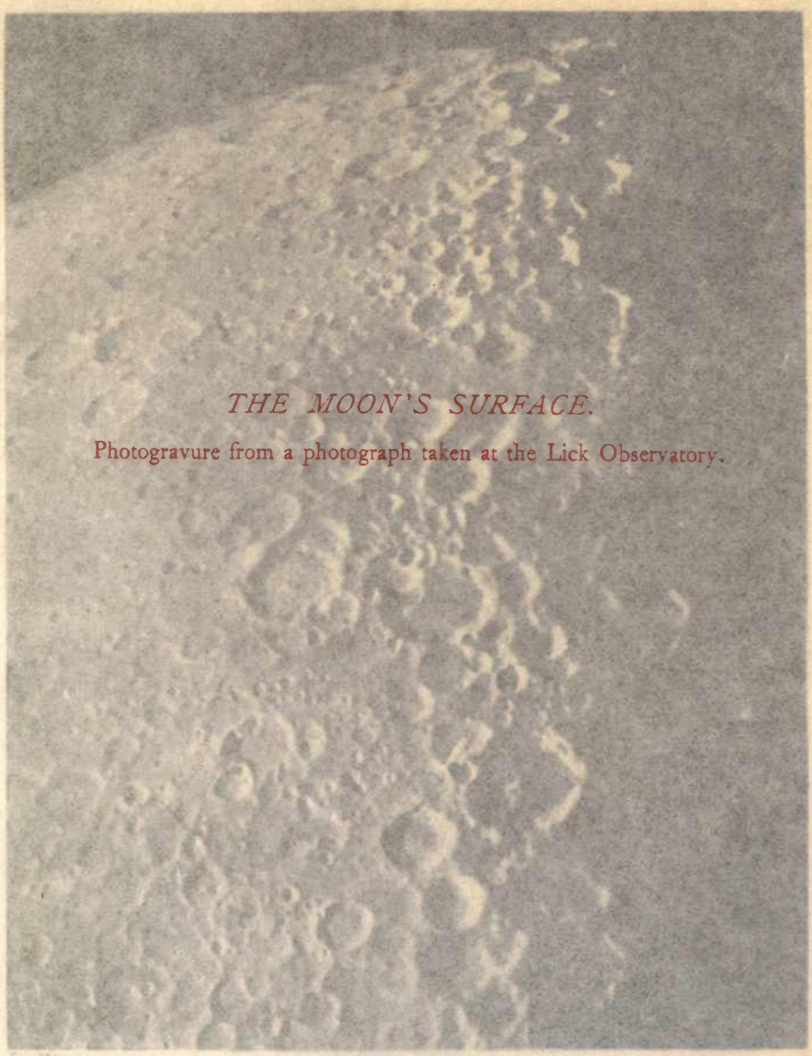
Now, I say the " planets "; for it seems to me that we may as well put the great central body of our system, the sun itself, out of the question. I think it is pretty clear that the surface at least of this enormous globe is in such a state as to make it absolutely impossible for us to conceive of any organized life existing there. It is true that we do not know exactly how much complexity of structure is required in matter as a basis of life; but we can hardly consider life in the proper sense as belonging to a chemical molecule, and everything would indicate that on the surface of the sun matter is reduced to its simply chemical or molecular state. Any structures or organisms which we call alive would instantly be destroyed in that intense flame; even inanimate shapes, like those of crystals, would not survive its action for a moment.

But may there not be a cooler region below the sun's surface, protected in some way from the intense heat of the exterior? Such a theory was entertained in the last century and even in this; but it is pretty safe to say that no one now would hold it. That it should have held its ground so long is due perhaps, in great measure, to the authority of Sir William Herschel. I do not think it was ever satisfactorily explained just how the interior was protected from the immense radiation of its envelope; certainly it is hard for us to see nowadays, knowing as we do the radiating power of the surface (10,000 horse power per square foot, as we find it to be) how such a blaze as this could even be supposed to be cut off from any point within. To suggest a cool place in the interior of the sun is much as if one should advise a person suffering from the heat of a furnace to wrap himself up well and take a seat inside. Moreover, we know from spectroscopic indications now, particularly from those of oxygen in the sun, that the farther in we go, the hotter it gets; and this also would follow from the only theory which can reasonably account for the formation of the sun, and the maintenance of its heat.

We may pretty certainly say, then, that in any common-sense way of using the word, the sun is not habitable. Absolutely speaking, of course, all space is habitable; there is no conclusive reason why an organized being should require nutriment or air, and hence an animal might be conceived as being launched into space as a planet on his own account. But what we mean by a place being habitable is, that it should furnish the requisites and conveniences belonging to a life similar in its principal features to that with which we are acquainted. It is not a thing which can be strictly defined; nevertheless, we know well enough for practical purposes what we are talking about, and we know that such a place as this empty space is not "habitable."

From the consideration of the sun we will pass to that of the next most conspicuous object to us in the planetary system—that is to say, the moon. I have already expressed in a previous lecture the views generally entertained by astronomers about the moon. It is pretty certain that the side of it which we see offers nothing in the way of a convenience of life except mere standing-room. There is hardly a doubt that its surface consists simply of bare rock, unvaried by water, soil, or any kind of vegetation; that if there be any atmosphere upon it, it is so excessively rarefied as to be, for purposes of life, practically equivalent to none.

As to the other side, of course, we can say nothing positively. It may perhaps in some way be different from this. But taking the ordinary and (to say the least) very



THE MOON'S SURFACE.

Photogravure from a photograph taken at the Lick Observatory.

Goupilgravure

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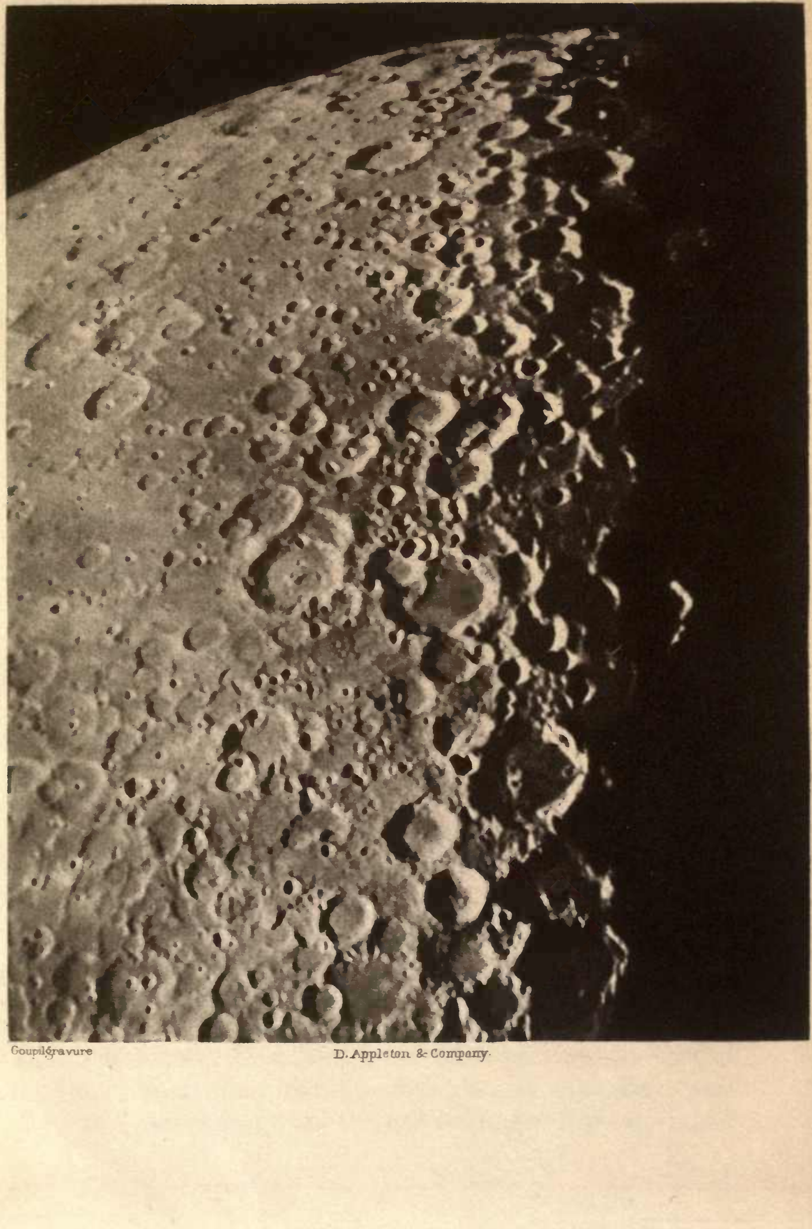

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probable view as to the method of formation of the planetary masses, by cooling from a liquid condition, it is hard to see how there could possibly be any considerable difference of shape or of density between the half of the lunar sphere which is turned toward us and that which is turned away. And unless there be such a difference, the other side must be as destitute of atmosphere as this; and if of atmosphere, of water as well; for the water or other fluid, if existing in any quantity, would form an atmosphere, if none previously existed.

The moon then hardly seems to present the condition required for what we should call a habitable planet; though it fails in a very different way from the sun. The moon is dead; the sun is too much alive. The moon may have been habitable and inhabited once; the sun may be in the future.

So far, our survey has not been very encouraging. But we have not yet considered the planets properly so called.

In considering them from this point of view, let us proceed in the contrary order to that which we followed in describing them in detail. Let us start at the outer limit, with the great twin planets, as we may call them, on account of their great similarity, widely separated in space as they are—namely, Uranus and Neptune.

These would perhaps generally be imagined as very cheerless habitations for intelligent beings, on account of their distance from the sun, and the comparatively small amount of light and heat which that great central fire sends to them, if that which the earth receives be taken as the standard. Particularly would this impress us in the case of Neptune. Its distance from the sun is about thirty times ours, and, according to the oft-repeated law of the inverse squares of the distances, the light and heat which it gets from the sun is only one nine-hundredth part of that which we receive. But let us not give up the matter as hopeless on this account. One nine-hundredth part of sunlight is not such a faint illumination, after all. It is nearly seven hundred times the light of the full moon, and indeed equal to that given by a large electric arc-lamp at a distance of a few feet. There would be no difficulty about reading by means of it; it would be quite sufficient for all the ordinary practical purposes for which sunlight is used here. And then there is another consideration which is of very great weight.

It is this: You know that, as I have said, what astronomers increase the size of telescopes for is to gather more light, rather than to get greater magnifying power. A telescope of two inches diameter, or aperture, as it is technically called, will give four times as much light as one of only one inch; one of ten inches will give twenty-five times as much as the two-inch, or a hundred times as much as the one-inch. The great Lick telescope, of three feet aperture, makes a star look about thirteen hundred times as bright as a one-inch spy-glass, and enables us to see stars about twenty thousand times fainter than any which can be seen with the naked eye. And the same rule would hold for the eye itself. If we should increase the size of the pupil of the eye, we should see fainter objects than we do now; and we indeed actually do this when we go from bright light into a dark room. We can easily see how the pupil dilates to accommodate itself to reduced light, by simply examining another person's eye in these changed conditions, or our own before a looking-glass. The eye of a cat changes much more. If the retina of the cat's eye is as sensitive as our own, she must habitually see stars five or six times fainter than any which we can discern without a glass, and the heavens must present to her a magnificent appearance, if she cares to look at them. Probably she actually uses this increased light rather to discover mice than stars; but her astronomical opportunities are there all the same, though she may not avail herself of them.

It is true that this increased light is obtained in the eye at some sacrifice of definition, or sharpness of vision in detail; but still an inhabitant of Neptune might have a good deal larger pupil than ours in proportion to the size of his eye than ours. And then, again, there is no reason why the retina itself should not be made much more sensitive to light than ours; and here we have an increase which has no limit, so far as we can tell. It would be an injury to us to have our optic nerve more sensitive; the strong sunlight to which we are exposed would hurt us. But there is no reason why the Neptunians should not have what would be a benefit to them.

The whole question, then, of light in the solar system is one of little consequence; eyes could easily in any planet be such as to suit the exigencies of the case.

With regard to heat, the question is a little more difficult, but not very much. If we should assume that the 500° Fahrenheit by which our temperature here is raised above that of space are simply due to our distance from the sun, and that Neptune could only have one nine-hundredth part of that, of course the temperature there would practically be that of space itself, or 460° below the Fahrenheit zero. But we know that, in fact, the genial warmth of the earth is in a great measure due to its atmospheric garment or blanket; and we can not be at all sure that an atmosphere may not exist on Neptune which may make the absorption so much greater than the radiation that an equality between the two would not be reached before the planet had accumulated from its scanty solar supply enough to make its temperature equal to ours.

And, besides, there is no certainty that these great outer planets may not still retain a great deal of their own intrinsic heat; that they may yet be warm enough, even on the surface, to act as a source of heat to their inhabitants. Indeed, the danger here is rather that they are too hot than too cold. Yes, that is the trouble with all the great outer planets, with Jupiter and Saturn, as well as Uranus and Neptune, as we shall shortly see. As far as atmosphere is concerned, the spectroscope would indicate rather a dense one on both Uranus and Neptune, and of the same character on each. Uranus shows belts on its surface similar to those seen on Jupiter; but we can not be sure that this indicates a similar constitution in the two planets. On the whole, we may say that there is quite what we may call a probability that Uranus and Neptune are in a habitable condition; the probability is, however, as we may say, rather negative than positive; we can not give any certain reason why they should not be; but there are really no positive indications to show that they are fit to be the abode of life. The arguments against habitability become much stronger in the case of the two giants of the planetary system, Saturn and Jupiter, which come next in order as we proceed toward the sun. The brilliancy of Jupiter's surface, and the rapidity of the changes which we see there, exceeding what the moderate light and heat which it receives from the sun would be likely to produce, seem to be quite strong arguments that it is still in a condition to emit light and heat to a considerable extent on its own account; and, indeed, that its temperature is still sufficient to keep it in a fluid state. If its surface be indeed in the condition of molten metal, it certainly becomes uninhabitable in the common-sense view of the subject; for in melted metal no organism composed of ordinary chemical elements could possibly subsist.

These arguments apply with somewhat diminished force to Saturn. Another, however, which may perhaps be derived from the lightness or small density of all the four great exterior planets of which we have been speaking, is strongest in the case of this one. This lightness may indicate that they have not yet shrunk to their proper dimensions; for it seems reasonable enough to suppose that the chemical constituents throughout the solar system are the same; that all the planets are chips out of the same block; and that when all are reduced to the physical state of the earth they would have about the same density. But this does not seem to amount to much; for though it holds well enough in the cases of Mars and Venus, it notably fails in that of Mercury, if the determinations of the mass of that planet can be considered as trustworthy. The density of Mercury would appear, it will be remembered, to be twice that of the earth; which would prove most undoubtedly that it was made of decidedly heavier materials, unless we maintain that it is very much more solidified than the earth, which would seem to be improbable. When a planet has once become, like the earth, solid on the surface, no further perceptible shrinkage is possible except by a complete breaking up of the crust, which could hardly result except from a collision.

But to return to the great planets of which we have been speaking. I think few, if any, astronomers believe them to be habitable in their present condition; for, though the case is more doubtful for Uranus and Neptune, still they have, in their general features, so much resemblance to Jupiter and Saturn, that it is usually presumed that they are in the same state. But no one could pretend to be certain with regard to the matter.

Before we leave this portion of our system, however, we must not omit a part of it which is eminently worth considering with reference to the present question. I mean the numerous satellites, which are such a striking feature in it.

Let us consider specially those of Jupiter, about which we know the most. The four moons of Jupiter are all quite considerable bodies, ranging in size from that of our moon to that of the planet Mars. There is plenty of room on them for a very large population; the surface of the largest does not fall far short of that of the land part of our own globe. There is no reason why they should not be in the same general physical state as the earth is; we have already seen that, as far as light and heat are concerned, they may be considered as amply provided; perhaps, indeed, even better than we; for the great planet itself, round which they circulate, would probably serve as a much better luminary by night than our own moon, and may very probably contribute not a little to keeping them comfortably warm, if it is indeed still in a melted and glowing condition. We may well believe that it is indeed a second sun to them, and if the satellites of Jupiter keep, like our own moon, the same side always turned toward the primary planet, that favoured side would enjoy a continual warmth, which might indeed be excessive.

Similar remarks may, of course, be made of all the other satellites which we find in this great region, revolving round Saturn, Uranus, and Neptune. Much has been said of the splendour of the Saturnian sky as seen from the planet itself, with the great ring arching over the heavens and the satellites circling along it. It is far more likely that, if this splendour is seen at all, it is from the satellites, from which, especially from Japetus, the most remote, whose orbit lies outside of the plane of the ring, a most magnificent view of the noble planet, with its rings and the other satellites, could be had. Saturn from Japetus would look as it does to us with a magnifying power of about three hundred and fifty diameters; or, to use another illustration, the ball of the planet would look about three and a half times the diameter of the moon, and the rings nearly nine times that diameter.

We come next, in our inward course, to the planet Mars. Here, for the first time, we begin to see positive signs, instead of mere negative possibilities, of what we have been looking for.

We have noticed, as we passed this planet on our way outward from the sun, the similarity of its surface to that of the earth, the permanent configurations on it of what we have a good right to assume to be land and water. We have seen its polar ice-caps, its green seas, and red earth; and we know that it has an atmosphere which, though not as dense as our own, is still enough, as it would seem, for life. We know that it has a day almost exactly the same as ours, and not only this, but seasons substantially like our own, as far as the varying angle is concerned at which the sun's rays strike its surface, though it is true that these are a good deal interfered with by the considerable variation in the sun's heat, depending on the eccentricity of its orbit; still this would not amount to so very much. In this latitude, for instance, on the earth, we receive more than three times the heat from the sun in one day in the middle of

June than we get in the middle of December, on any given area, say a square mile or a square yard, owing to the combined influence of the greater height of the sun above the horizon and the greater length of the daylight. About the same would be the case in the same latitude on Mars. The effect of the eccentricity would be quite considerable, making the sun's heat once and a half as great at the nearest point as at the farthest; still, if we can sustain the threefold multiplication, a half as much again might be added, without the variation becoming intolerable. Moreover, this great variation would only occur when the summer solstice of one of the hemispheres coincided with the point of nearest approach to the sun. During half the time, the eccentricity would tend to moderate, instead of to accentuate, the seasons, as it does with us here in the northern hemisphere now.

Mars is certainly the most favourable case for those who would believe the planets to be habitable. It really seems that it might be inhabited by men like ourselves. As remarked on a previous occasion, its climate seems, from the small size of the polar ice-caps, to be warmer than that of the earth, in spite of its greater distance from the sun.

As to Venus and Mercury, we can hardly form any decided opinion. They seem to be surrounded by dense, cloudy atmospheres, which may tend, in a great measure, to keep off the intense heat of the sun. A rather singular thing has lately been observed, or at least thought to be observed, by Schiaparelli, with regard to Mercury—that is, that some markings on it seem to indicate that its period of rotation round its axis is the same as that of its revolution round the sun; or, in other words, that it acts as our moon does, keeping always the same face toward the centre round which it revolves. This would seem to be borne out by the white spot on the black disk of the planet, which has been reported by various observers as regularly visible at the time of its transits across the sun's face. If this white spot is a real object, it would seem that it is always turned away from the sun. If this can be accepted, it would be, of course, to some extent, an argument against the habitability of Mercury, as its inhabitants would be deprived of the vicissitude of day and night, and the side turned constantly toward the sun would probably, in spite of everything, become uncomfortably warm.

Now that we have—though quite hurriedly—completed our consideration of the planets as to their suitableness for habitation, what answer shall we give to the question with which we started? Before giving it, another reflection must be made, which will brighten the prospect a good deal for those who would fain believe all these magnificent orbs to be the abode of life like ours.

It is this: Will it not suffice to satisfy the minds of those who can not believe that these great globes, similar in so many respects to ours, can be tenantless, to hold that they are habited for a portion, though not for the whole, of their history? For myself, I do not feel the craving for the plurality of worlds, as it is called, which seems to be general. I must confess that I have never been able, personally, to feel the force of the argument which strikes most minds so powerfully, that these habitations could not have been made by their Creator except to be actually inhabited. The mere size and mass of an object seem to me to amount to little. Jupiter itself, or Saturn, with its beautiful ring and satellite system, simply as a mass of matter or a mechanical construction, is a far less noble creation of God than a single human soul; nor does it seem to me that the mere size of these planets makes them much more remarkable, or requires more reason for their formation, than if they were only a few feet in diameter. The technical study of astronomy, no doubt, has the effect of reducing the impression made by mere magnitude on the mind; whether this is a delusion or the removal of a delusion, of course I can not say. That the mere size of a body itself does not require inhabitants for it, seems plain from the generally confessed impossibility of inhabiting the sun, the surface of which far exceeds that of all the planets put together—that is to say, that it does not require them at every moment; but it may be, if you will, that it does require that at some time or other it should be used for such a purpose. The general belief is, we may say, an argument for the fact.

And, of course, the argument for the plurality of worlds is strengthened if, besides size or standing-room, as we may say, we see some other conditions indicating conveniences for life, though they be imperfect or incomplete. If we see a house with only its framework up, we say, " Nobody lives there now, but it is being built for some one "; and if we see a house in ruins, we say, " Somebody lived there once."

Now, this is certainly very plausible; and I think that the history of our own earth, so far as it can be learned from science, increases the probability of the opinion that the planets, and perhaps even the sun itself, were made to be inhabited at some time or other. The teaching of geology is that our own earth was for a long time uninhabitable; that it subsequently became fitted to be the abode of the inferior and simpler forms of life, and finally became ready for the reception of man; and we can hardly shut our eyes, either, to the scientific conclusion that, from the operation of natural causes alone, it would at some time in the distant future become uninhabitable again, though in a different way; that it would become, simply from the changes which must come from the gradual progress of cooling necessarily going on in the solar system, no longer a building which its Creator is forming, but a cold and desolate ruin like the moon.

The history of this earth is probably the history of the other planets, if they are to be allowed to develop in a natural way. Some, like the moon, seem to have passed farther along the road than our own planet. This is probably the case with Mars, the most habitable in appearance of them all. As a rule, of course, the smaller a planet is, other things being equal, the more rapidly it will cool from its originally incandescent state; Mars then should be older—that is, have passed through more of its successive changes—than we. It looks so, besides. The seas seem to be drying up, the air thinning away. On the other hand, the great superior planets, Jupiter, Saturn, Uranus, and Neptune are young, and have the best part of their life before them.

What portion of the total life of a planet is that in which it becomes habitable by beings like ourselves we can not very well determine. If we accept the estimates of geology, the time that the human race has been here is a very small part of our world's history. But how much longer this earth would naturally remain a possible residence for us we can not say with accuracy. It would seem probable, however, that the period in which all the necessary conditions of life would simultaneously exist can hardly be a very considerable part of the whole. The inhabitants of a planet in the stage of decadence from its most perfect state could, no doubt, on the principle of the "survival of the fittest," accommodate themselves to their more unfavourable circumstances for a good while; but the time would come when the struggle would have to be abandoned.

If it is true that the period of habitability by the high organisms is a small part of a planet's life, obviously the chance is small for any planet in particular of its being in that period now or at any particular time. We must say that it probably is not, unless we have, as in the case of Mars, some positive indications that it is. So far as we can trust such positive indications, Venus and Mercury are approaching that part of their life that the earth is in at present; the earth seems at one time to have had the very dense and vaporous atmosphere that apparently surrounds them now.

To sum up now, briefly, the results to which our examination has led us: In the first place, our observations should probably be modified by the very plausible theory, now generally adopted, that all the bodies of our system, sun and planets, have passed and are passing through a series of changes, beginning with a state of great heat and expansion, in which and for a long time no life is possible on their surfaces, and in a great part of which indeed, as in the case of the sun at present, they can hardly be said to have a surface at all. As the changes due to the gradual cooling and contraction proceed, life in its simpler forms becomes possible, and in course of time a state is reached like that of this globe at present, in which the conditions for highly organized life are at their best.

Assuming this, the question of fact becomes, Is there any other planet or satellite in the system in which this state of maximum habitability, if we may so call it, now exists? We can say with great confidence that it does not on Jupiter and Saturn; that the chances are much against it on Uranus and Neptune; that Venus and Mercury are probably still too young for it; but that there is a reasonable probability for it on Mars, though this planet seems to be passing into the decline, the steps of which we do not clearly understand, but of which we see perhaps the final result in the torn, scarred, and desolate surface of our own satellite. With regard to the satellites of the great planets, we have absolutely to suspend judgment. As the period of habitability is probably less than that of development, though of this we are far from certain, the chances are perhaps against any particular one of them being in that state just now; but as they number at least seventeen altogether, the probability that some one of them may be habitable is not so inconsiderable. As to the satellites of Mars, and the swarm of asteroids, they seem to be too small to retain an atmosphere sufficient for the support of beings like ourselves. If they had a course to run, it has probably been concluded long ago.

In speaking of the natural life and development of the planets, we are, of course, looking at the matter merely from a scientific point of view. Of course, most Christians believe that long before the natural life of this earth would be concluded, it will suffer a final catastrophe which will at least close the history of the human race on it as it exists now. Such catastrophes may, of course, occur to any planet by natural as well as supernatural causes; by collision with some other body, for instance; or to the whole planetary system, by some large body striking on the sun. One thing which we may perhaps look forward to is a time when, after the death or destruction of all the planets, the sun itself ceasing to be a luminary and furnace for bodies circulating round it, may itself become the great seat and home of life. In theorizing on this point we have no past experience or history to guide us. We shall see as we go on to discuss the stellar systems that we have at least one case, perhaps more than one, of a body sunlike in dimensions, which has either ceased to give light or never gave it. It is only in exceptional cases that we have any means of recognising the existence of such bodies; they may be very numerous. Neither can we tell whether the other innumerable brilliant suns scattered through space have attendant planets like our own. But it would be strange if they had not. If any considerable proportion of them have, evidently the chance that there are other habitable worlds in the universe becomes very great.

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THE END