found only $\frac{2}{1000}$ of 1 per cent of oxide of copper, an amount so small that one would need to eat from one-half to one ton of these grapes, stems, skins, and all, to obtain the least injurious effect, and that, notwithstanding the fact that the bunches were selected from those having the largest amount of the copper mixture adhering to them.

In sample No. 2 not a trace of copper could be found. It would seem from the above that, even under the most careless use of the copper solutions, no injurious effects need be feared, and that when properly applied there will not be a trace of copper left upon the fruit at harvesting.

Apples.

Early in December, the *Pall Mall Gazette* of London, England, published an article headed "American Apples. Alarming Allegations — Are They Doctored with Arsenic?" Then the statement is made "that American orchardists use arsenic in such large quantities to protect their fruit from insects as to completely saturate it, and that the bloom or white powder found on American apples is arsenic, brought to the surface by evaporation, and, if the fruit is eaten, this should be wiped off to avoid injurious effects. That the delicate, unnatural (?) bloom of the American apples is due to arsenic, a drug that is largely used by people, especially the fair sex in America, to make the complexion fair," and other statements equally absurd and without a shadow of foundation. These statements were undoubtedly made in the interest of speculators for the purpose of injuring the sale of American apples in the English market.

To determine the amount of copper and arsenic adhering to the surface of apples (for it could not have been absorbed into the substance of the fruit) which had been sprayed three times with the Bordeaux mixture and Paris-green, twenty apples, measuring one peck, were taken to the State Experiment Station for analysis. The amount of copper oxide found on these apples was twenty-two thousandths (.022) of one grain. This equals about five ten-thousandths (.0005) of one ounce to the barrel, or requiring two thousand barrels to yield one ounce of copper oxide. The specimens selected for this analysis were those with the roughest surface, to which would adhere more of the copper solution of Paris-green than to the average apples.

Not a trace of arsenic could be detected in this analysis, as Paris-green (average samples of Paris-green contain about thirty-three parts of oxide of copper and sixty-one parts of arsenious oxide) was not used after July 1, but it was probably all washed off during the three months following, before the apples were gathered, which was Oct. 1.

When we consider the fact that probably not one fruit-grower in one hundred throughout the country used Paris-green at all, and that not one barrel in thousands came from sprayed trees, the absurdity of the "scare" becomes still more apparent.

LETTERS TO THE EDITOR.

*** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

The Ancestry of Chalicotherium.

CHALICOTHERIUM is a genus which appears in the lower Miocene simultaneously in Europe and America, where it has been very recently discovered. It extends into the Pliocene and then disappears. It has attracted unusual attention of late, owing to the discovery by Filhol and independently by Forsyth Major that the foot-bones of *Macrotherium*, which has been considered an Edentate, really belong to *Chalicotherium*. As the teeth are wholly different from those of the Edentates, and similar to those of the Ungulates, this genus represents a very aberrant and unique family.

The only known Ungulates which present a dentition at all similar are *Palæosyops* and *Meniscotherium*. The latter is from near the base of the Eocene, and last year in analyzing its dentition I found so many very striking resemblances to that of *Chalicotherium* that I was led to suggest that *Meniscotherium* might be the long-sought ancestral form, reserving final judgment until the feet were discovered. Marsh has very recently figured the feet of *Meniscotherium* (*Hyracops*), and, upon the whole, I think they sustain the supposition that the *Chalicotheriidæ* were derived from the *Meniscotheriidæ*. There are some profound differences, but these are mainly such as separate primitive from highly modified forms. The resemblances consist in the tridactylism of both genera and the marked similarity in tooth structure. I will discuss these points in more detail in the *American Naturalist* for June.

HENRY F. OSBORN.

New York, May 5.

Detection of Artificial Gems.

I WAS much interested in reading an article by Mr. W. G. Miller on the "Detection of Artificial (Imitation) Gems," that appeared in your issue of April 29. The writer states that, 1, hardness is no test for cut stones, because cutting softens the surface; 2, that specific gravity is no test in polished stones, because polishing affects the specific gravity, and because imitation-gem manufacturers made them with a specific gravity as near that of the real gem as possible; 3, that the examination of the optical properties of cut stones is difficult (and therefore presumably impracticable) because of the many facets; 4, that fusibility is the only reliable test. I desire to advert briefly; but first let me say that the title of the article, "The Detection of Artificial (Imitation) Gems," is misleading, and confounds two totally distinct things. *Artificial* gems, such as the rubies of Fremy or the emeralds of Hautefeille, are constitutionally identical with real gems, but are the product of a chemical process, and not the work of nature; whereas imitation gems, such as paste or glass or the so-called doublets, are gems only in appearance, consisting of two or three layers of quartz or garnet and one or more layers of glass of such intensity of color as to tone down or change the quartz or garnet to the red color of the ruby or the green color of the emerald or the blue of the sapphire, according as it is intended to counterfeit one or the other of these. The same confusion is also apparent in the statement that "the ancient Egyptians and Greeks were well versed in the manufacture of artificial stones." That they produced remarkable glass *imitations* is indisputable, — witness the marvellous collections of antique pastes in the museums of Europe, — but it is safe to say that the ancients never produced an *artificial* precious stone of any kind. So much for the title.

Now, second, as to hardness as a test, let me say that I differ entirely from Mr. Miller when he states that the hardness of a precious stone is reduced by cutting or polishing. The hardness is not affected in any way, and so far from cutting impairing the test for hardness it can in point of fact be more delicately given if made on cut and polished stones with properly prepared points made of the various gem minerals than when made on the rough uneven surfaces of uncut and natural minerals. That polishing reduces the hardness by *one-tenth* is ambiguous. Though in the Mohs scale of hardness the sapphire is placed at 9 and the diamond at 10, it would be more in keeping with fact when the abrasive quality or hardness of a diamond is considered to rate the diamond at 100 or even 1,000, so great is the difference between the two. Surely the writer does not mean to imply that, simply by polishing, the hardness of the diamond is reduced to 9 (the hardness of the sapphire), or that the sapphire is reduced to 8 (the hardness of topaz), or that topaz is reduced to 7 (the hardness of quartz). It is well known that *imitation* (not artificial) gems will scratch glass, and there is no reason why they should not. Their hardness is

not even as great as that of feldspar, never that of quartz. Popular beliefs are not scientific facts, and it is a scientific fact that nothing but the natural edge of a diamond crystal will cut glass (frequently with very little visible scratching), but everything having the hardness of feldspar will scratch it, as well as glass itself. Popular errors are numerous, and these errors are frequently extensively copied. For instance, a statement appeared some years ago in one of our large magazines that if a precious stone could not be scratched by quartz it would surely be a diamond, and that any jeweller who would object to having a diamond tried with a file should be condemned as a fraud.

Polishing the surface of a precious stone can in no way affect its specific gravity if the stone is properly cleaned, and if the operator has a delicate balance and sufficient experience. In these circumstances it is surprising what exact results the various colors of the various precious stones give us. Further, I may say that, after visiting nearly all the known gem-cutting centres and the chief seats of the manufacture of imitation gems, I have never yet known of an instance where the manufacturer cared the slightest what the specific gravity of his product was, providing it had the desired color, or, if it were to imitate a diamond, it had a greater amount of brilliancy than the material made by one of his most successful competitors. The majority neither know nor care what the specific gravity of the gems is any more than does the regular jeweller.

As regards the optical properties of gems no mention is made of the dichroscope, with the use of which the facetting in no way interferes. The polariscope is also of considerable value. In fact, in the determination of rubies, sapphires, and emeralds, their pronounced optical properties, as shown by the dichroscope, or the polariscope and the spectroscope, together with their specific gravity and their hardness, which is so much greater than that of quartz, will readily distinguish them from everything "imitation." By means of the spectroscope we obtain the red band for the ruby, the absorption bands for the garnet, at D, E, and F in the spectrum, or the series of black absorption bands for the zircon. To distinguish glass from a real ruby requires but a glance; to detect the difference between rubies, spinels, garnets, and rubellite is not so easy, and in these cases fusibility is of no value.

I think the experience of those who have given attention to this matter is, first, that the specific gravity of the various precious stones is remarkably constant according to their color, seldom varying more than one in the second place of decimals, and, second, that the hardness of the gem is also remarkably constant, and that lines can be more clearly drawn in cut than in natural crystals, which are frequently not transparent, owing to impurities; namely, placing the sapphire at 9, the ruby at 8.8, the aquamarine at 8, and the emerald at 7.8.

I should not want to be responsible for the consequences if, at a jeweller's, anyone tried heating a gem in the flame of a spirit lamp or in the flame of a Bunsen burner, any more than I should if a buyer started to try a diamond with a file. Nor should I care to be responsible for the heating in a Bunsen burner of a fine ruby or sapphire, which frequently contains fluid-cavities, or of an emerald, which, if of a fine color, is seldom perfect, owing to internal striæ and fluid-cavities, or the topaz, which is affected by heat, and nearly always contains many minute fluid-cavities. The fusibility of the edges of the gems would not distinguish the artificial rubies of Fremy from those of the true ruby, as both are infusible. Nor would the test of heating in a Bunsen burner be practicable if Mr. Miller were called upon to examine in a few hours from one thousand to fifty thousand gems, and at the same time be perfectly sure that there were no imitation gems in the lot. Such testing needs the experience of the expert, who, before he opens a paper marked "blue or green aquamarine," can tell simply by the weight that the stone in the paper is a blue or green topaz, or who, if the stone is labelled "yellow topaz," can, without looking at it, but simply by the facility with which it slips through the fingers, determine that it is citrine (decolored smoky quartz) or the true mineral topaz; or who, if one hundred stones mounted as rings were placed before him in a tray, without supposing the presence of an imitation stone, could at once detect the single imitation present. Nor would fusibility be of any value in the examination of that class of imitations which are made by dipping heated quartz in green, red, or blue solutions, a common variety of which is known as Mount Blanc, or Alpine ruby.

Finally, few mountings which secure gems are improved by heating them to any extent, and generally the owners do not wish the settings disturbed. As to imitation diamonds there is surely not a jeweller worthy the name who cannot tell a true diamond from a paste one at the first glance, by its adamantine lustre. If it scratches sapphire he may be sure it is a diamond, whereas putting the gem into the flame would not distinguish the diamond from the white topaz or the white zircon or the white sapphire or the white tourmaline or any other white stone that is not fusible.

In conclusion, let me suggest to Mr. Miller the simple test for diamonds, of drawing the stone sharply over a piece of unpainted board in a dark room. Every diamond phosphoresces by friction.

GEORGE F. KUNZ.

New York, May 11.

Artificial Production of Variation in Types.

IN reply to your request for a few words on the question of artificial production of variations, as presented by Mr. West in *Science* of April 22, I may say that I quite agree with Mr. West in thinking that all attempts to produce new species by mutilations of the parents are foredoomed to failure. The idea that the embryo is in any sense a *reflected* image of the parent, and consequently that any particular loss or modification of an organ in the parent during adult life must impress itself upon the embryo, has not a shadow of a basis in embryology.

Mr. West asks, "Would it not seem the proper and only method to study the laws governing the modifications of the embryo?" If we substitute germ-cells for "embryo," the question may be answered affirmatively. If the question, as it stands, implies that modifications received during *embryonic* life, as the result of external influences, would be any more likely to repeat themselves in the next generation than if acquired during adult life, I should say that the assumption is entirely unwarranted.

The form and features of the adult are predetermined in the constitution of the germ-cell. No one denies that external conditions and influences may affect more or less the course of development; but the specific form of the adult is already settled in the germ before development begins. These are mere truisms in embryology.

C. O. WHITMAN.

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The "Hongote" Language.

IN a series of ten studies of South American Languages, principally from MS. sources, which I published in the last number of the Proceedings of the American Philosophical Society, one was partly devoted to the "Hongote" language, a vocabulary of which I found in a mass of documents in the British Museum stated to relate to Patagonia. I spoke of it as an independent stock, not related to other languages of that locality. In a letter just received from Dr. Franz Boas, he points out to me that this "Hongote" is certainly Salish, and must have been collected in the Straits of Fuca, on the north-west coast. How it came to be in the MSS. referred to, I cannot imagine, but I hasten to announce the correction as promptly as possible.

D. G. BRINTON, M.D.

Philadelphia, May 4.

AMONG THE PUBLISHERS.

THE number of the *American Journal of Psychology* which is about to appear will contain an article on the variations of the knee-jerk by Dr. Noyes, which contains the results of experiments on a case of dementia. Mr. Bolton contributes a digest of the experiments on memory made by Dr. Boas in the Worcester schools. Mr. Fraser shows the psychological origin of the *naive* realism of the unthinking man and of the philosophic realism of the Scottish school; both are due to a postulate of the sensations of touch as the ultimate realities. The old philosophers have before this