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To the Readers of SCIENCE.

THE ENLARGEMENT OF "SCIENCE," BY DOUBLING THE NUMBER OF PAGES, WILL HAVE TO BE INDEFINITELY POSTPONED, AS THE RESULT OF OUR EFFORTS, FOR THREE MONTHS, TO GET EIGHT HUNDRED NEW SUBSCRIBERS, AT THE PROPOSED INCREASED PRICE, HAS BEEN TOO SMALL TO JUSTIFY OUR GOING TO FURTHER EXPENSE.

N. D. C. HODGES, 874 *Broadway, New York.*

SCIENCE

NEW YORK, JANUARY 6, 1893.

THE MICROSCOPE AND THE STUDY OF THE CRYSTALLINE SCHISTS.

BY GEORGE H. WILLIAMS, JOHNS HOPKINS UNIVERSITY, BALTIMORE, MD.

IN some preliminary pages from the Twentieth Annual Report of the Geological Survey of Minnesota, Professor N. H. Winchell has recently circulated some considerations on the structures and origin of the crystalline rocks.[1] In so far as these are the expression of a sincere desire to advance this difficult line of inquiry by summarizing results secured and by striving toward a more precise definition of terms to be employed in descriptions of crystalline terranes, they are worthy of appreciative consideration by all geologists. Certain of Professor Winchell's statements relative to the comparative value of microscopical and field evidence seem, however, liable to cause misapprehension, and it therefore appears to the writer worth while to call attention to these, at least in so far as they involve his own work on the so-called "greenstones" and "greenstone-schists" of the Lake Superior region.

No problems of geology are more intricate and at the same time more attractive than those presented by the pre-Cambrian formations. The stratigraphy, correllation, and genesis of these vast rock masses must be deciphered mostly without the aid of fossils; hence any kind of evidence, however slight, which throws real light on the questions at issue must be welcomed by the geologist and must be so thoroughly studied by him that it can be accorded its full significance.

The sub-division of the pre–Cambrian rocks into distinct formations has long been recognized as a desideratum in geology but one unattainable without minute and detailed work. General theories have proved futile for its accomplishment. Only now has the problem begun to be attacked by methods which are a stimulus for the present and a promise for the future. In Great Britain, Germany, Norway, Russia, Canada, and the United States facts are being rapidly gathered whose ultimate correlation will surely bring order out of chaos. Field study, areal mapping on a large scale, and the detailed study of stratigraphy must always be the first and most important means of deciphering a crystalline terrane. But the structure planes of the rocks are so often secondary and their original character so obscured by alteration, that stratigraphy, and indeed all field evidence, may prove inadequate to the task set for it. Then it is that help from other sources is required, and none has thus far shown itself more efficient than that furnished by the microscope.

In the history, which in the future will be written of the pre-Cambrian formations, the work already accomplished in the Lake Superior region must occupy a most honorable place. Many pioneers have there pointed out methods and secured results which the world will recognize as fundamental. There the large number of workers have stimulated discussion and has led to a constant re-examination of the same points in the light of accumulating evidence; there repeated surveys have carried on detailed mapping and the field study of stratigraphy; and there, if anywhere, the value of uniting out-of-door and laboratory methods has found demonstration.

In his present communication, Professor Winchell first summarizes the results reached by the Geological Survey of Minnesota in regard to the classification of various pre-Cambrian formations distinguishable within that State. Upon this subject the writer wishes to express no opinion. In the second section of the paper the use of terms is dealt with. A generally accepted distinction is made between constructive (metamorphic) and destructive (weathering) processes of rock alteration, and a plea is entered for some "middle ground" between the interpretations given to the various parallel structures in crystalline schists by those who hold too exclusively to either a sedimentary or a dynamic theory of their origin.

In the third division of his paper Professor Winchell discusses the comparative value of microscopic and field evidence, and it is here that the writer would take issue with his conclusions. He says: "It is in the nature of the problem involved in the study of the complicated structures and relations of some of the Archæan rocks, that the differences between the microscopic evidence and that derived from their macro-structure shall gradually fade out and that one or the other shall usurp the whole field." Later he does indeed allow that "this is not intended to shut out any individual geologist from exercising the right to employ any and all lines of research for the solution of all the problems that he has to solve," (!) but in spite of this generous permission the implication is that, after all, the ordinary mortal must be satisfied to be *either* a field, or a microscopical geologist.

Now, the writer is not aware that the most ardent advocate of the study of petrography (microscopical or otherwise) considers this branch as more than an aid to geological research. Divorced from field observation it becomes unreliable and trivial. As a supplement to field-work it is most serviceable, as the beautiful results of Iddings, Cross, Van Hise, and many others in this country (not to mention European investigators) fully show. The microscopical study of isolated hand-specimens as mere mineral aggregates once served a useful purpose, but this stage in petrography has now passed.

If, then, it be the acknowledged duty of every petrologist to be at the same time a field geologist, and to study his material in the laboratory in the light of his own observations in the field, is it at the same time too much to expect that the field geologists at work on the crystalline rocks will thoroughly inform themselves of the methods, progress, and aims of petrographical research, at least before they complain of their tendency to mislead? The microscope is now but one of the elements in modern petrographical investigation. Progress made by many workers is constantly advancing the point of view, as well as multiplying methods. Is it fair that the field geologist should remain more one-sided than the petrologist would allow himself to be? Between results obtained in the field and laboratory there is no discrepancy, except to one who incompletely comprehends one or the other method of work.

Professor Winchell says that "the sedimentary structure in a rock is one of those characters which the field geologist only can be allowed to pronounce upon with authority." If this be so, it does not follow that he who is *only* a field geologist possesses in such cases the greatest authority. If he has microscopical and other petrographical methods to aid him, it stands to reason that his opinion will be worth more. If he is certain in the field, he may, it is true, be brought to doubt by laboratory study, but this doubt is itself a gain, since there are some crystalline rocks whose origin can perhaps never be put beyond doubt.

Professor Winchell then proceeds to discuss what he calls a concrete case from the greenstones of the Lake Superior region and gives what he thinks would be the conflicting conclusions obtained by a microscopical and field study. To illustrate this case, he reproduces two figures taken from the writer's Bulletin (U. S. Geological Survey, No. 62) on the Lake Superior greenstone schists, and says: "These figures could be repeated many times in the course of a brief examination in the field. These cases present the issues fairly. It remains to be decided whether the

[1] The Crystalline Rocks, some preliminary considerations as to their structures and origin.—N. H. Winchell, Twentieth Ann. Report Geol. Survey of Minnesota, 1891.

testimony of the student who relies on his microscope and starts out with the idea of subordinating his facts to the answers it may give, or that of the field-observer, who only studies the grander structures and has a predisposition to explain such as the foregoing by referring them to sedimentation, shall here be received with the greater credence."

The Bulletin here quoted embodies the results of portions of two seasons' field-work, as well as a large amount of laboratory study of the greenstone schists. However fairly the figures may "present the issues," it is unfortunate for Professor Winchell's argument that he did not select some of the many similar examples with which his field experience has made him personally acquainted. The fact is that the two occurrences selected by Professor Winchell from Bulletin No. 62 demonstrated in the field the dynamic origin of their structures so convincingly, that no microscopical examination was ever made of them. It would never have occurred to Professor Winchell or to any other "field geologist" to explain the particular features which, in the Bulletin, these two figures represent, by sedimentation, if they had observed the natural exposures. A single narrow shear zone, crossing a great wall of massive diabase 60 feet in height, makes it certain, without help from the microscope, that the chlorite schist which borders the zone is the result of the fraying out of the rock by the motion. Nor is there less certainty that the wide gaping gashes in the basic eruptives are due to some mechanical strain. There are cases without number, as every one who has worked in the crystalline schists well knows, where their is doubt as to whether a parallel structure is due to sedimentation or to dynamic metamorphism; but why Professor Winchell should select two cases as clear as these, it is difficult to understand. In the text descriptive of the original figures, it is plainly stated that the first is unsatisfactory because it represents only a *hand-specimen*, whereas the structure, to be appreciated, must be seen on the face of a high rock-wall. In regard to the second figure, it is also stated that it is only a diagramatic representation of an *area* on the rock-wall about three feet square. If there is difficulty in arriving at correct conclusions from the study of natural exposures, all the more caution is necessary in interpreting another author's figures, especially when these are distinctly described as inadequate.

In reality, what are known in the Lake Superior region as "greenstones" and "greenstone-schists" are not one thing but a great variety of different things. Some of them are massive lavas, others accumulations of ash material stratified by gravity or water. They possess structures of diverse origin, which may to the field geologist appear very much alike. These must be studied first and foremost in the field, but to avoid confusion and misinterpretation we need all the help available, even from the microscope. Here we may see plainly that what macroscopically looks alike is in reality different. In fine, there is no discrepancy between the results of field and laboratory work, and if he who is *only* a field geologist find his conclusions at variance with those of a field geologist who is also a student of the microscope, it behooves him to revise these conclusions before he casts aside the results of modern petrographic research.

WORCESTER SCHOOL CHILDREN. — THE GROWTH OF THE BODY, HEAD, AND FACE

BY GERALD M. WEST, CAMBRIDGE, MASS

AN investigation into the laws governing the growth of various parts of the body was instituted in the Worcester schools in the spring of 1891, and a short notice of the growth in width of the faces of girls was published in *Science* (July 3, 1891). I now propose to give a summary of some of the other results obtained.

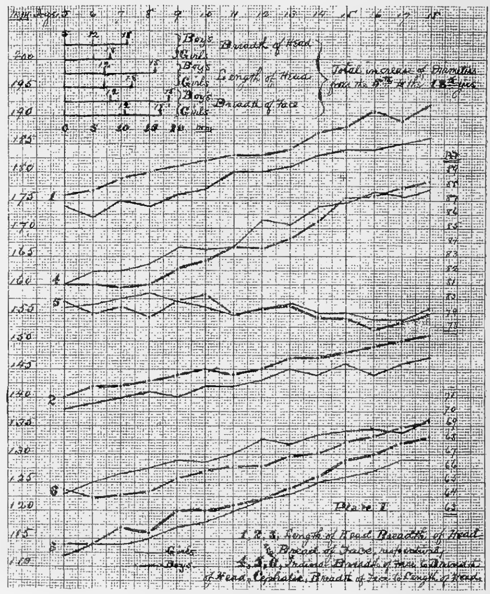
The observations were made in the primary, high and normal schools, and in two of the private schools in the city of Worcester. The number of individuals examined was 3,250, the ages ranging from 5 to 21 years. The nationalities were numerous, but about 66 per cent were of American parentage, 20 per cent of Irish, 7 per cent of English and Scotch, and 6 per cent scattering.

Plate I. contains the curves of growth of the diameters of head and face, with their indices.

Absolute measurements.
1. The maximum length measured from the glabella. } Head.
2. The maximum breadth. } Head.
3. The " " of the face.

Indices.
4. The proportion of the breadth of the face to the breadth of the head.
5. The proportion of the breadth of the head to the length of the head.
6. The proportion of the breadth of the face to the length of the head.

Length of Head (1). — In absolute length we see that the girls' length of head is less than that of the boys throughout its whole period of growth, and consequently throughout life. We find, however, that this difference in length does not remain the same year by year, but varies considerably, being, for example, 3 millimeters at the ages of 11, 12, and 13, and rising as high as 6 millimeters before, and 7 millimeters after, that age. We find also



that the annual increment is very irregular in both sexes. We have periods of growth alternating with a cessation of growth.

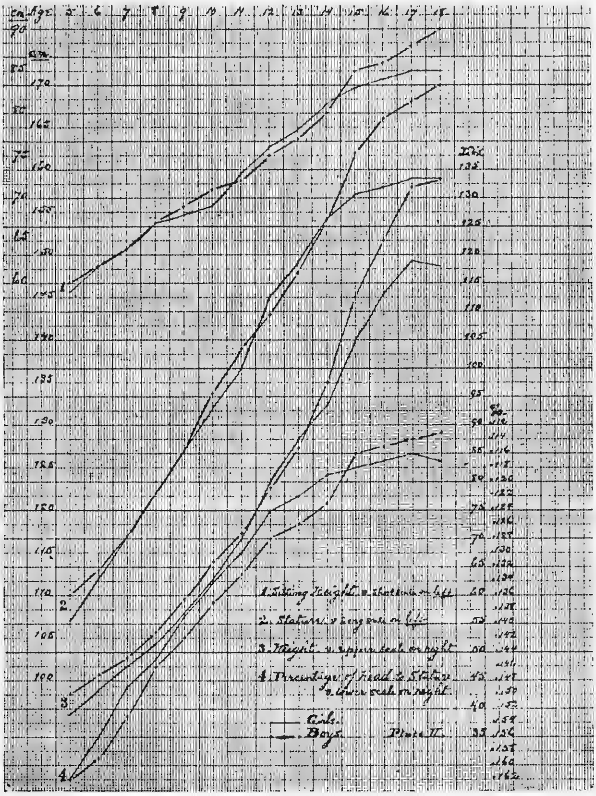
In girls the greatest length of head is reached at about the beginning of the eighteenth year. In boys the head continues to grow until at least the age of twenty-one. The period of greatest irregularity in the annual increment seems in the case of girls to be before, in the case of boys after, the eleventh and twelfth years.

Breadth of Head (2). — The breadth of head presents phenomena very similar to those of the length of head, i.e., periods of alternate growth and cessation of growth. The girls' width of head is less than that of the boys, but the difference diminishes markedly about the eleventh year, from this age until the fourteenth year the curves are parallel, then this again becomes more widely separated. The age of maximum width in girls is about seventeen, in boys the maximum is not yet reached at the age of twenty-one.

Breadth of Face (3). — Here again we meet with similar phenomena; the breadth of face of the girls increasing rapidly with irregular annual increments until the seventeenth year, when the maximum growth is reached. The faces of the boys continue to grow until the eighteenth year and probably beyond.

As in the case of the two preceding diameters, the breadth of face of the boys exceeds that of the girls. But there is this slight difference here; the diameters of the girls' heads approached more nearly those of the boys' heads during a certain period, approximately, from the eleventh to the thirteenth years; but in the diameter of the face the girls not only approach, but at the twelfth year seem quite to reach that of the boys.

These three curves evidence four things: first, that the time of growth in the diameters of the heads and faces of girls is shorter than in the case of boys; second, that up to about the twelfth year these diameters grow more rapidly in girls than in boys, while after that age the contrary is the case; third, that by an apparently sudden rise in the annual rate of growth in the girls their diameters approach much more nearly that of the boys during the period of the eleventh, twelfth, and thirteenth years. Finally, the average annual rate of growth in the diameters of the girls heads and faces is nearly uniform during the two periods before and after the eleventh-thirteenth years. While in the case of boys it is considerably greater, actually and relatively, after than before. Between the fifth and the eighteenth years the length of head of boys increases 16 millimeters, in the same period the breadth of head increases 11 millimeters, and the width of face 18.5 millimeters. The corresponding measurements in the case of girls increase 12 millimeters, 8 millimeters, and 17 millimeters, respectively, for the same period of time. The horizontal lines on the upper left hand of the diagram indicate the entire altitude of the curves, the cross-bar indicating the altitude at twelve years. A comparison of the annual increments of the length of head and of the breadth of head seem almost to suggest an alternation in growth between the two diameters, as we shall see, the alternate rising and falling of the curves of the cephalic index would seem to strengthen this suggestion.



Let us now turn to the three indices, numbered on the plate 4, 5, and 6. and taking up as first in order the cephalic index.

The Cephalic Index (5).— The curve of the cephalic index shows, as would be expected from an examination of the component curves of length of head (1) and breadth of head (2), a considerable degree of irregularity in its annual stages. There is, nevertheless, a certain general regularity displayed, taking the curves as a whole; both displaying three periods, composed each of a decided maximum and minimum. These periods are from about the fifth to the eleventh, the eleventh to the sixteenth, and the sixteenth on in girls; from the fifth to the tenth, from the tenth to the thirteenth, and from the thirteenth to the eighteenth in boys. The whole range of the two curves is very small, scarcely two and a half per cent; the final index being, for boys, about one and one-half per cent below that of the index at five years of age; the final index of the girls being very nearly the same as at five years of age. The greatest altitude of the curve is, for boys, at ten years, and for girls at eight years. The greatest depression is at about sixteen years of age for both sexes. The cephalic index of girls is for the period of growth higher than that of boys, except at about the ages of nine and ten.

Breadth of Face to Breadth of Head (4).— In comparing the growth of the breadth of the face to the breadth of the head, we find that the breadth of face grows much more rapidly proportionately than the breadth of head. This is shown by the rapid rise of the curve of the index. That the increase is actually greater than the width of face we have already seen. The breadth of face as compared with the breadth of head is greater in the case of girls than in the case of boys until the fifteenth year, at which time the boys' curve becomes the higher, falling again the next year, and rising finally in the seventeenth year.

Breadth of Face to Length of Head (6). — As in the index just discussed, the breadth of face increases more rapidly proportionately than does the length of head. We have the index of the girls higher than that of the boys until about the sixteenth year, when the two curves intersect, that of the boys becoming the higher for one year, and again failing below in the eighteenth year.

We see, therefore, that in proportion to the length of head, the width of head and the width of face of girls are generally greater than those of boys, and that in proportion to the width of head the width of face also is greater in girls than in boys.

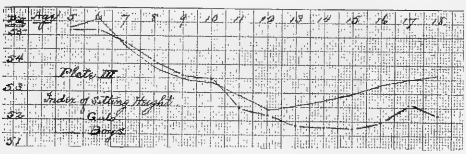
Body measurements (Plate II.):—

1. Sitting height. — Vertex to oleacronon, approximately.
2. Stature. — Standing erect without shoes.
3. Weight. —In in-door clothing.
4. Comparison of length of head to stature, expressed in per cents of stature.

Plate III.:—

Index of sitting height. — Comparison of sitting height to stature, expressed in per cents of stature.

The Stature (2). — Taking the stature as properly first in order, we find the boys starting out at five years of age apparently taller than the girls, but the girls appear to catch them in the seventh year and continue at an equal stature up to and including the



ninth year, after which the boys again rise above the girls for two years. At about the twelfth year the girls suddenly become taller than the boys, continuing taller until the fifteenth year, when the boys again and finally regain their superiority in stature. After the age of seventeen, there seems to be very little if any increase in the stature of girls while the boys are still growing vigorously at eighteen, and probably continue to grow for several years after that age.

The intersection of the two curves at the ages of twelve and fourteen is a more accentuated instance of the phenomenon which we have already met with in the curves of the diameters of the head and face. We shall see it again in the curves of sitting height and of weight.

The Sitting Height (1). —The curves of the sitting height present the same characteristics, somewhat more accentuated, as the curves of stature. The boys start out at five the taller, but by the next year the girls are of equal stature and continue equal until and including eight years of age. From eight until eleven the boys are again the taller. In the eleventh year, nearly a year earlier than in the case of stature, the girls shoot ahead of the boys, the latter not regaining their superiority until the fifteenth year, about half a year later than in the case of stature. Again, we find the girls' curve stopping abruptly at seventeen, while the boys continue to grow for some years longer.

The Weight (3). — The curves of weight, while preserving the general characteristics of the curves of stature and sitting height, show minor differences. The boys are in all years from five to eleven inclusive heavier than the girls. From the twelfth to the fourteenth year the girls are the heavier. From fourteen on the boys are again superior in weight. The superiority of the girls in respect to weight is for a much shorter period than in respect to total height or sitting height.

In weight, also, the girls seem to reach their maximum average at seventeen, the boys continuing to increase in average weight until a much later period in life.

Comparison of Length of Head to Stature (4). — The curves of this index bear a strong resemblance to those of stature. From this comparison it seems that until the fifteenth year the length of head of girls is less in proportion to their stature than is that of boys to their stature. At fifteen the ratio of the boy's length of head to their stature suddenly drops, while that of the girls gradually rises, indicating that in the adult the heads of women are proportionately longer than those of men. This is also true of the width of head and the width of face.

The Index of Sitting Height (Plates III.). — These curves, starting at a high per cent at five years of age, drop rapidly until the twelfth year in the case of girls and the fifteenth in the case of boys. From the twelfth year on the girls' curve rises; from the fifteenth to the seventeenth years, inclusive, the boys' curve also rises, but drops again during the next year. These movements of the curves seem to indicate that the greater part of the growth in stature, up to the twelfth year in the case of girls and until the fifteenth year in case of boys, is made in the lower limbs, while after these respective ages it is made in the trunk. Except for about two years, throughout the period from five to eighteen, the limbs grow more rapidly than the trunk in boys, while in the case of the girls the period of greater comparative growth is divided nearly equally between the extremities and the trunk. Except from about the seventh to the tenth year, the trunk is proportionately longer in girls than in boys, after the thirteenth year the difference is much more marked.

As we found in the case of the diameters of the head and face, girls grow more rapidly than boys up to twelve years of age, less rapidly after that age. Comparing the two periods, we find that in the case of stature and sitting height the annual rate of increase for girls is considerably less after twelve than it was before it. The boys maintain the same rate throughout. Although both sexes make greater annual rates after than before twelve, yet the girls make their greatest absolute increase before, the boys theirs after, that period.

These results seem conclusive evidence that women reach maturity several years before men. There seems little doubt that for all the measurements of the body, except the weight, girls have completed their growth by the eighteenth year.

BIRD-MUSIC IN AUGUST.

BY MARY HYATT, STANFORDVILLE, N.Y.

MUCH has been written about the songsters of spring and early summer, but there is something of a lack of information concerning the birds that sing in August. It would be interesting to compare notes from different localities on this subject.

Bird-music in this month of oppressive heat is doubly welcome, and the few singers that help to enliven the sultry days should receive their share of attention and praise.

Burroughs says that there are but four songsters that he hears "with any regularity after the meridian of summer is past, namely, the indigo bird, the wood or bush sparrow, the scarlet tanager, and the red-eyed vireo." He further observes that "birds sing as long as nidification goes on. . . . Hence our wood-thrush will continue in song into August if, as frequently happens, its June nest has been broken up by the crows or squirrels." The wood or bush sparrow mentioned is, we think, Spizella pusilla, a faithful little minstrel of morn and eve all through the heated term. The goldfinch, whose lively notes as he dips and rises through the air are so prominent in mid-summer, and whose canary-like song is occasionally heard, should certainly be included among August songsters. With us the yellow throated vireo is as regularly tuneful in August as the red-eyed, while the white-eyed vireo is heard now and then.

In a note-book kept through August of 1889, we have an account of such birds as were in song for many days during the month in our vicinity. Beginning Aug. 3, we have on record: Indigo bird, chewink, Baltimore oriole, wood pewee, red-eyed vireo, phebe bird, song, field, and chipping sparrows. When out riding on Aug. 4 we heard the strain of a meadow lark, and on the 6th the noisy tirade of a white-eyed vireo.

On Aug 8 the note-book tells of a fine concert, when a goldfinch, an indigo bird, field, song, and chipping sparrow sang, an oriole whistled a few times, and a yellow-throated vireo was tuneful by spells for a long while.

Aug. 15. Red-eyed vireo, chewink, and field sparrow; 16th, yellow throated vireo, phebe, goldfinch; 17th, oriole, chewink; 18th, red-eyed vireo; 19th, yellow-throated vireo, and "orioles make themselves heard nearly every morning now." Aug. 21, field sparrow, wood pewee, and black and white warbler.

Aug 29. "The yellow-throated vireo sings nearly every day — almost the only bird we hear nowadays. Yesterday we noticed the songs of a goldfinch and a song-sparrow; chickadees also were musical." This closes the month's record, but it is noted down as something unusual, that the yellow-throated vireo continued to sing during every forenoon for the first six days of September.

There are usually a few fiery days in mid-summer when nearly every bird is silenced, but rarely an August morning passes without a salute to the dawn from sparrow or goldfinch.

A RARE FORM OF POLISHED STONE IMPLEMENTS AND THEIR PROBABLE USE.

BY WALTER HOUGH, WASHINGTON, D.C.

AMONG the collections from Mexico, Central and South America, exhibited in the Columbian Historical exposition at Madrid, the writer noticed a number of oblong polished blocks of hard stone of unknown use, averaging 3¼ inches in length, 2¾ inches in width, and 1¼ inches in thickness. The broad surfaces of these stones are plane, bearing a number of grooves parallel to the length, froming ridges like those seen on Polynesian tapa mallets.

The edges, as a rule, are hollowed out by pecking, seemingly for convenience in grasping the block, so that the section is that of the modern eraser for the blackboard. Often these blocks are only nicked at the corners, and usually two sides and one end only are hollowed out, which seems to indicate that they were mounted in a handle, perhaps by means of a wythe going around the hollowed edge.

In most cases both sides are ridged, one side coarse and the other much finer; a peculiarity noticed in the Polynesian mallet of square section, which often bears four grades of ridges, which are used successively in reducing the bark to thinner texture.

Only one of the blocks seen is round in outline; a few others have rounded corners; the ridges are parallel and the ridged surfaces perfectly flat. An aberrant block of this type, which is probably a stamp, has a convex surface, with sawed diagonal grooves crossing (hatchwork) at either end bounding a band of horizontal lines enclosing shallow bored pits and a central series of shallow bored circles with cores.

The material is usually hard basalt or porphyritic rock, and the channels bounding the ridges are fine examples of sawed work.

The resemblance of these objects to those used by so many different peoples, in beating out fibrous bark for clothing, paper, etc., is very striking. May it not be said that this is a pre-historic implement for the same purpose, and that they give an insight into the manufacture of the paper upon which the Mexican codices are painted? In Costa Rica, Nicaragua, and certain countries of South America, the present aborigines use ridged wooden mallets resembling the Polynesian for making bark clothing.

It may also be affirmed that there is no other form of implement than the one having the combination of ridges and grooves, that is useful in expanding and separating the fibres of bark evenly without rupture, which is evident from the effect produced by the blow.

The distribution of the 31 bark-beaters measured and described by the writer is as follows: Mexico, 25; divided among the Nahuas, (12); Totonacs, (1); Tarascos, (6); and the Miztecs-Zapotics, (6). One of these in the Mexican collection has been channeled, probably by the Tarahumares, and adapted for one side of an arrow-smoother, the other side is a smaller block of freestone of reddish color. This was taken from a cave anciently inhabited by the Cromachi. Two bark-beaters are from Nicaragua; one in the collection of Dr. Carlos Bovallins of Upsala, Sweden, and the other from the exhibit of the government of Nicaragua. One specimen is from Columbia in the collection of the Archæological Museum of Madrid and three from the exhibit of Costa Rica.

After examining the paper upon which the Mexican codices are written, the opinion is expressed that it is not made from the magney, but is from a tree furnishing bark available for paper, probably of the family to which the mulberry belongs.

ETHNOGRAPHICAL SURVEY OF THE UNITED KINGDOM.

BY E. W. BRABROOK.

IN the early part of 1892, on the suggestion of Professor Haddon of Dublin, the Society of Antiquaries of London, the Anthropological Institute, and the Folk-Lore Society appointed delegates to discuss the means of combined action for obtaining simultaneous observations on the monuments of antiquity, the physical characters of the people, and their customs, traditions, and beliefs in various parts of the United Kingdom. They agreed to seek the co-operation of the British Association, which has local corresponding societies in connection with it, and received authority to act as a committee of that association, with the additions of a delegate from the Dialect Society, and of others specially representing Wales, Scotland, and Ireland. It was generally admitted that the success of the work depended upon its being taken in hand at once, since the forces impelling country folk towards the great towns, and the rapid means of transit from place to place now available to the very poorest, are fast effacing all special local peculiarities, and mixing up inextricably the races of which the population is composed.

The first step of the committee has been to issue a circular to persons known to be well acquainted with the rural districts, requesting them to indicate such villages and places as appear especially to deserve ethnographic study, so that a list might be formed, ont of which a selection might afterwards be made for the survey. The villages or districts suitable for entry on the list are defined to be such as contain in general not less than a hundred adults, the large majority of whose forefathers have lived there so far back as can be traced, and of whom the desired physical measurements, with photographs, might be obtained. For such typical villages and the neighboring districts the committee propose to record (1) physical types of the inhabitants, (2) current traditions and beliefs, (3) peculiarities of dialect, (4) monuments and other remains of ancient culture, and (5) historical evidence as to continuity of race. In each such place they will endeavor to obtain the assistance of observers resident in the locality.

The response which the committee have obtained to this preliminary inquiry has been more general and encouraging than they had expected. In some places they have been met with the lament, — this ought to have been done fifty years ago, and it is now too late; but from numerous others, in all quarters of the three kingdoms, they have received information of places where the people are still primitive in their ideas and customs, unaffected by intercourse with strangers, and bear a marked strain of one or other of the races by which this country has been peopled. For the use of these informants, a brief code of directions is being prepared.

This endeavor to record the natural history of the elements which go to make up the population, so far as they can be traced in the localities where its race-elements have remained undisturbed, will, I have do doubt, interest many of those whose ancestors have carried to the United States some recollection of the peculiarities and customs of the people of that part of the United Kingdom from which they sprung.

The Journal of Hygiene will be the name of the *Herald of Health* on the 1st of January, 1893. The *Herald of Health* is now in its 43d year and has been edited since 1866 by Dr. M. L. Holbrook. The journal is published in New York, at $1 a year.

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HOW MANY ARCHÆAN ROCK-GROUPS HAVE WE IN GREAT BRITAIN?

BY CH. CALLAWAY, D.SC, M.A., F.G.S., WELLINGTON, SHROPSHIRE, ENGLAND.

RECENT geological research amongst the pre-Cambrian rocks of North America, while it has settled some points, has unsettled others. A generation ago the terms "Laurentian" and "Huronian" were thought to have a clear and definite application. At that time, we in Great Britain knew of only one Archæan group, called Hebridean or Lewisian, and supposed to be the equivalent in time of the Laurentian. Later on, British geologists discovered a second pre-Cambrian formation, the "Pebidian" of Dr. Hicks, or "Uriconian" of the writer. This great volcanic system bore many resemblances to the published descriptions of the Huronian, and it was referred with more or less hesitation to that group. Meanwhile, Dr. Sterry Hunt was creating more systems in America. We heard of his "Norian," "Moutalbian," "Taconian," and "Keweenian," and every year we looked for new worlds from his prolific brain. Unfortunately, subsequent research in the United States and Canada has but very partially confirmed Dr. Hunt's results, and even our faith in "Laurentian" and "Huronian" has been somewhat confused. "Huronian" appears to be several things, and "Laurentian" in some localities is said to be an intrusive granite. Nevertheless, it appears to be generally admitted that in North America there are gneisses and granites which are older than any other rock-masses, and that in the same region there are volcanic formations which are younger than these crystallines, and more ancient than the Cambrian; so that the old notions on "Laurentian" and "Huronian" remain true in a general way. It would also seem that North America contains sedimentary rocks which are newer than the Huronian, and are yet pre-Cambrian. Thus it would hardly be rash to conclude that, on the western side of the Atlantic, there exist at least three Archæan roch-groups, a gneissic, a volcanic, and a sedimentary, and that they succeed each other in the order here given. Now it is interesting to remark that this description agrees with the latest results of research in Great Britain. We have first of all the gneisses and schists, which in Scotland are called "Hebridean," and "Malvernian" in England. We cannot say that these formations are the exact equivalents of each other, and it would certainly be rash to assert that they, or either of them, can be correlated with any rock-masses the other side of the Atlantic. Nevertheless, they are admitted to be the oldest rocks in Britain, and, in the opinion of the writer, they are separated by a considerable interval from the formation which comes next. This great volcanic system holds the place originally assigned to it in the Archæan series by Dr. Hicks and the writer. Its pre-Cambrian age has been admitted by Sir A. Geikie, director-general of the Geological Survey of Great Britain and Ireland, so far as the Uriconian rocks of Shropshire are concerned; but he assigns the Pebidian of St. Davids to the base of the Cambrian. In the opinion of the writer, the volcanic rocks of St. David's are truly pre-Cambrian; so that the name "Pebidian," originally given to them by Dr. Hicks, has priority over the more modern term "Uriconian." These rocks are of wide distribution, being found in North and South Wales, at Charnwood, near Leicester, in many parts of Shropshire, in the Malvern Hills, and probably at Howth, near Dublin. Evidence has recently been collected of a third pre-Cambrian system. Near Church Stretton, in Shropshire, is a chain of hills, forming Longmynd, built up of conglomerates, sandstones, and slates. Murchison called these sediments "Bottom Rocks," and he referred them to the Lower Cambrian. This view has been adopted by the English Geological Survey, and generally accepted. Recently, however, evidence has been collected which makes it almost certain that this formation is of pre-Cambrian age, and the present writer has given it the name "Longmyndian." The true basal Cambrian, a band of quartzite, occurs in close proximity to the Longmynd rocks, though not in absolute contact; and it is incredible that the Longmyndian, which is some miles in vertical thickness, should be a mere subdivision of the Cambrian, which is found in three of its four members within a few miles to the east. It would seem, then, that on both sides of the Atlantic, the Archæan (or pre-Cambrian) series consists of (at least) three members, gneissic, volcanic, and sedimentary, which follow each other in the same order, suggesting a similarity of conditions in both areas in the successive epochs of Archæan time.

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.

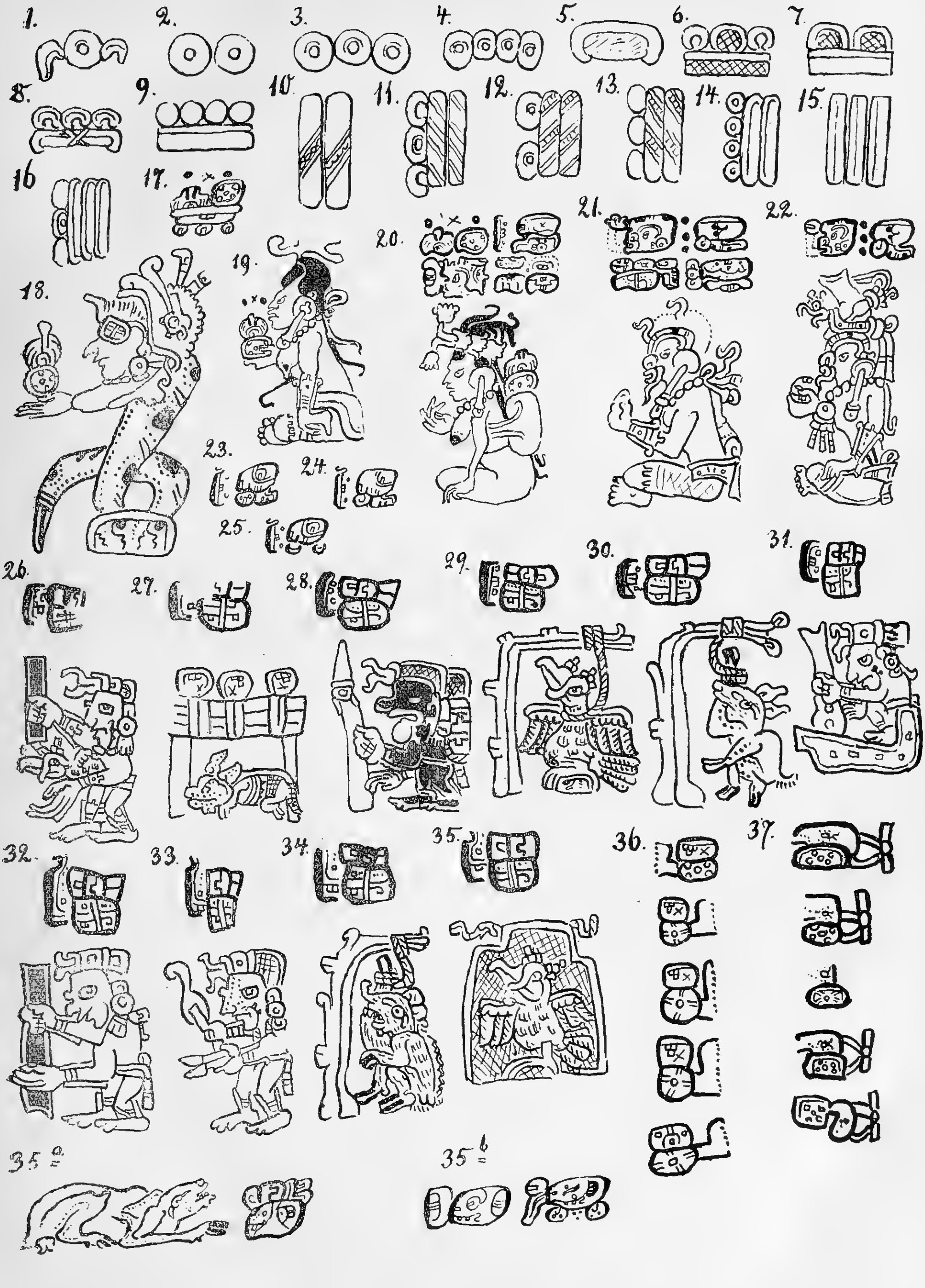
The editor will be glad to publish any queries consonant with the character of the journal.

Is the Maya Hieroglyphic Writing Phonetic?

In No. 505 of the *Science*, Professor Cyrus Thomas devotes a few more pages to the problem of the Maya hieroglyphic writing. "These," he says, "may perhaps be profitable to the subject, if confined to an earnest endeavor to arrive at the truth." The "additional evidence," introduced in this manner by Professor Cyrus Thomas, he has seen fit to precede by some remarks intended to invalidate the criticism I offered in this paper some months ago (*Science*, Aug. 26). My answer to these remarks is presented in the following lines, which, I trust, will also be profitable to the subject, although I do not claim to be the only scientific man that "earnestly endeavors to arrive at the truth."

Professor Thomas is correct in stating that "a dot and two crosses with a month-symbol form a date in the bottom line of Plate 49, Dresden Codex." Nevertheless, I firmly believe I can maintain that "there does not exist a numeral designation with crosses *between* the dots." I have never seen it in the Codices. On the other hand, I found, for instance, on the sides of the Stela J of Copan (Maadsley, "Biologia Centrali Americani," Pl. 69–70) that the *one* dot of the numerals 1, 6, 11, and 16 always is framed by two ornamental signs, but there is never an ornamental sign between the *two* dots of the numbers 2, 7, and 12. Compare the Figs. 1–16 of the adjoined table. Moreover, I think, the analogy between the two hieroglyphs, Figs. 29 and 30 (of my former paper), is obvious. Since in the one case the two dots and the cross are a part of the hieroglyph and not a numeral, I hope, it will not be a fault of veracity to believe the same in the other.

Professor Thomas says I am not correct in stating that Fig. 30 (of my former paper) is the glyph he interpreted "moisture." "True, the parts are similar," he says, "but the details and surroundings are different." In the adjoined table I reproduce the Fig. 30 of my former paper by Fig. 17, and Professor Thomas's moisture symbol by the Fig. 18. Certainly, the surroundings are different. In Fig. 17 the hieroglyph is placed on a dish, in Fig. 18 on the hand. And there are wanting in Fig. 18 the two dots and the cross that are seen in Fig. 17. But the parts are not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"similar,' but essentially the same. And that the whole hieroglyph is really the same, is proved by comparing Figs. 19 and 20 of the adjoined table. taken from the Dresden Codex, 18a and 19c. In Fig. 20 the hieroglyph of Fig. 17 is the first hieroglyph of the text. Its representative is shown in the hieroglyph carried on the back of the woman figured below. This representative of the text-hieroglyph exhibits the same elements in the same order as Professor Thomas's moisture-symbol held on the hand of Fig. 18.

Professor Thomas asserts that my statement that the first glyph shown in his Fig. 2, p. 46 (*Science*, July 22), is the same as that in certain groups mentioned by me, and Figs. 31–33 (of my former paper) are incorrect, as I had failed to include the prefix. The character of my first figure, he says, is the same, but the characters of my two other figures are different and give a different word. The first character Professor Thomas had interpreted *u-zabal*, "set the snare." Respecting the latter, he says, it is possible that the signification is suggested by *haɔab*, "a sword, weapon to wound with, a whip." This agrees, Professor Thomas asserts, "very well with what we see in the hands of the figures below, and also with the general tenor of the series." True, instead of naming one character and one series, I ought to have spoken of two allied characters and two allied series. But my objections to Professor Thomas's interpretation were chiefly based on the fact that each one of the two hieroglyphs is the leading character in a *series* of representations, embracing different actions, and not only the "setting of the snare." The first character is the leading hieroglyph in the series Figs. 26–31 of the adjoined table; the second one in the series Figs 32–35. It is obvious that — although there are represented different persons and animals — the general tenor of the two series is essentially the same. Both, undoubtedly, refer to capturing animals, showing the deity armed for hunting and different captured animals. Now, it can be proved that the leading character of the hieroglyphic groups of a series suggests the action in which the persons figured below are represented (compare, for instance, Codex Dresden 4c and 7c and the two leading hieroglyphs in Codex Dresden 12c, Codex Troano 19c, etc.). As, in our case, the general tenor of the two series is the same, the first of our characters (Figs. 26–31) will be intended to indicate the same action as the second one (Figs. 32–35). We must conclude, therefore, that the second part, which is common to the two hieroglyphs, is the essential one; and that the other, the so-called "prefix," is subordinate, referring to circumstances of minor importance, perhaps interchangeable. This conclusion will be proved once more by the fact that the second part occurs alone, and apparently with the same general signification (see Fig. 35a, taken from Dresden Codex 60a).

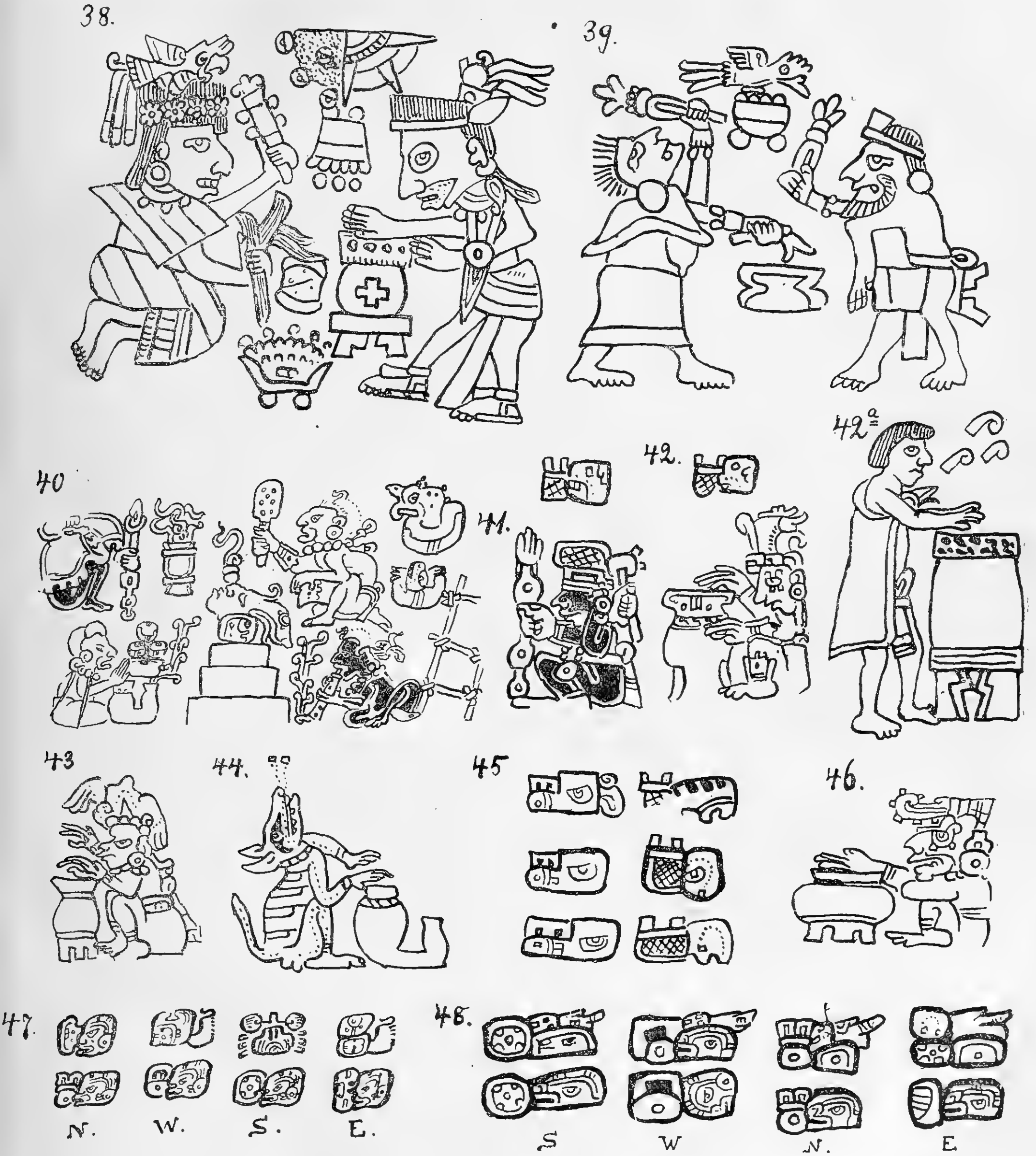
As to Professor Thomas's interpretation, the name *haɔab* he gives does not agree with his own alphabet. For the element in question, the knot or loop, seen on the top of the second part of the hieroglyph, according to Professor Thomas's alphabet, does not express the sound of the "letra herida" ɔ, that is to say, *ts*', but that of *z*. or *s*. The word itself is not *ha-ɔab*, as Professor Thomas reads. but *haɔ-ab*, an instrumental noun derived from the verb *haɔ*, "to whip, to wound." Finally, it is obvious that the rendering, "sword, a weapon to wound with, a whip," does not more agree "with what we see in the hands of the figures below, and also with the general tenor" of the second series (Figs. 32–35), as it would agree with that of the first one (Figs. 26–31). I may safely abandon to the reader's judgment to decide whose interpretation in this case is the more based on "mere assumptions," Professor Thomas's or mine, and who has more earnestly endeavored to arrive at the truth.

Professor Thomas acknowledges the correctness of my statement that the sign of aspiration found in Brasseur's "Landa" is not in the original text. "Nevertheless," he says, "we have to thank the Abbé for a happy suggestion. . . . I may add that Dr. Seler has gone farther than Brasseur, as he has given us in his 17a a character which appears to be new, — at any rate, I have been unable by a careful search to find it in any of the codices." I refer Professor Thomas to the Figs. 23–25 of the adjoined table. These, and some other variants, act as leading hieroglyphs in a series of twenty-nine hieroglyphic groups, accompanying as many figures of the rain-god. My Fig. 23 contains the element in question, with exactly the same characters as I rendered them in Fig. 17a of my former paper. This Fig. 23 occurs three times in the series, in Dresden Codex 30c, 31c, and 39c. Professor Thomas, therefore, has not carefully searched. To call a notorious falsification "a happy suggestion," and to stigmatize a correct statement as a conscious falsification (I say it with due regard to courtesy), we are not wont to consider as an earnest attempt to arrive at the truth.

Professor Thomas argues that I had criticised his article without having thoroughly read it, because, in the fourth character of his Fig. 4, I overlooked, he says, the little item on the front of the face. Had I but looked to his Fig. 3, I would not have fallen into the error of considering the two as the same. I regret to say that the writer of the Dresden Codex has fallen into the same error, since he mentions the deity, seen in the Figs. 21, 22, of the adjoined table, in Dresden Codex 5a by the first hieroglyph, Fig. 21, in Dresden Codex 13b by the first hieroglyph, Fig. 22, both differing from another in "the little item on the front of the face," nearly in the same way as the characters of Professor Thomas's Figs. 3 and 4 (*Science*, p. 45) differ from another.

Professor Thomas himself, in most cases, has overlooked the notorious existence of variants of writing and the replacement of one element by another. He says, "To assume that the Fig. 29 (of my former paper) is a variant of Fig. 30, is certainly straining a point to the utmost tension." I could show to my opponent more curious variants. As to the mutual replacement of the element *Kin* and Professor Thomas's "letter-glyph" *b* — that, in my view, renders the sound *Kan* "yellow" — I refer him to Figs. 36, 37, of the adjoined table, the first showing the leading hieroglyphs of Cort. 21, Tro. 35d, the latter those of Codex Tro. 24* 23*a.

Professor Thomas concludes his objections against my criticism with the following phrase: "I must confess that his (Dr. Seler's) eyes are sharper than mine, if he can find any figures in either of the Codices representing a god or any one else beating a drum. This, like other of his assertions in regard to the significance of other figures, appears to be 'merely hypothetical.'" My reply to this apostrophe is the Fig. 40, taken from Dresden Codex 34a, which, for the benefit of the reader, I have contrasted with two Mexican paintings, Figs. 38 and 39, taken from Codex Borgia 55, and Codex Land. 39. In the two Mexican paintings, a goddess is seen and a god, the latter beating a drum, in Fig. 39, curiously held between the legs. No scholar versed in Mexican prolographic style, will deny that the instrument seen in those paintings is really the drum, the *tlalpan-neuetl*, made of wood and covered with a tiger-skin. Compare Fig. 42a, the well-known musician of the Mendoza Codex. Now the god of Fig. 39 has his exact counterpart in one of the persons of Fig. 40. Here, in the very middle of the scenery, we have the head of the sacrificed (or the dead deity) exposed on the top of the altar-pyramid. On the left side a fire is burning, and below it an offering of maize is placed on a dish. To the right hand other offerings are seen, consisting of a meal of maize and turkey, and of a meal of maize and certain other game. Four persons sit around, playing different instruments. On the upper part of the left side, a black-colored person holds the *chicauaztli*, the well-known rattling staff of the Mexican paintings (see "Compte Rende, VII. Sess. Congr. International Américanistes," Berlin, 1888, p. 661–664, and "Veröffentlichungen aus dem Königlichen Museum für Völkerkunde," I., p. 147, 152). Below him a woman beats a drum of curious form. The music is seen rising from the end of the instrument. To the right hand of the altar, in the lower part, a man is playing a flute. Here, also, the music is seen rising from the lower end of the flute. The upper figure, on the right side, with one hand shakes the rattle and with the other beats the drum, held between the legs exactly in the same manner as with the god of the Codex Land. (Fig. 39). Another series of musicians occurs in Codex Tro. 24* 23*d. Here a person, exhibiting a black-colored skin, like that of Fig. 40, is seen with the *Chicauaztli* in the one hand, and a rattling-ring (?) in the other (Fig. 41), while another deity (Fig. 42) is beating a drum. On the top of the figures I reproduce the leading hieroglyphic that accompanies the figures and undoubtedly refers to the general tenor of the series. The curious form of the instrument of Dresden Codex (Fig. 40) occurs also on Plate 24 of the Codex Tro., together with another more regular form (see Figs. 43 and 44 of the adjoined table). And considering the former (Fig. 43) and the other figures of the instrument represented above, I think, nobody will doubt that also in the figures of Codex Cortes 21ª and Codex Tro. 35ᵇ (Fig. 46 of the adjoined table) the writer intended to represent a drum. We shall the less doubt of it, as this action here is accompanied by hieroglyphs (Fig. 45), the one of them exhibiting the same characteristics as those accompanying the musicians in Figs. 41 and 42. We have, thus, in the known Maya Codices at least five well characterized representations of persons or gods beating a drum. My mentioning, therefore, of a god beating a drum was not "merely hypothetical," not a "mere assumption," but based wholly on proofs.



I shall not go into further details; nor shall I attempt to criticise the "additional evidence" brought forward by Professor Thomas in his last article, or to discuss the probability of that curious enumeration of historical facts occurring every 177 days, for the space is limited. Only, by the way, I note that Professor Thomas interprets phonetically *Xaman* as "north," the character that, in reality, designates *nohol* "south" (see the evidence adduced by me in "Zeitschrift für Ethnologie," XXIII., p. 104). His third sample of the use of his "letterglyph" *b* is one of those interesting hieroglyphs that change the so-called "prefix" according to the four cardinal points. Compare Figs. 47, 48 of the adjoined table, the former taken from Codex Dresden 29, 30c, the latter from Codex Tro. 31, 30d. These varying elements undoubtedly are indicating the names of colors, as each of the four cardinal points was distinguished by a special color. And the so-called letterglyph *b*, with all probability, has to be considered as expressing the element *Kan* "yellow" (see "Zeitscrift für Ethnologie," XXIII., p. 108, 109). The explanation Professor Thomas gives of the five dots, seen under certain hieroglyphs, as rendering the sound *ho* "five," will receive a curious illustration by the varied form these dots exhibit, for instance, in the Fig. 35b, taken from the Dresden Codex. It does not appear, with all, that the samples of interpretation presented by Professor Thomas in his last paper are more satisfactory than those of his former one. It will be seen, indeed, that there is no reliance in the simple fact that, applying a certain key, the parts give apparently appropriate results. In a similar way there could be proved and has been proved that the Mexican and Peruvian languages are derived from Sanscrit, and that the descendants of the lost tribes of Israel survive in the Southern Sea. The right, Professor Thomas claims, to apply such a key has to be proved in the first place. I am awaiting if, in the paper he is preparing for publication by the Bureau of Ethnology, he will be able to do so.

Dr. Seler.

Steglitz, Germany, Dec 18.

Irrigation Surveys.

I have just had the pleasure of perusing your issue of the 16th, with its review of Irrigation Work by the General Government. Allow me, in returning my thanks for the comprehensive references made, to make some brief corrections:—

In the first place, then, the expenditures of the Geological Survey as to "irrigation" work, have been that of two appropriations — in all $350,000. This is wholly outside of printing, which is paid for under other appropriations. The cost thereof will not be less than $15,000. Besides these two direct sums of $100,000 and $250,000, with the printing of Part II. in Annual Reports 10 and 11, the Survey for work in the Arid Region, topographic and hydrographic, has had two more annual appropriations of not less than $100,000 in all. The terms of the appropriations were designed unquestionably to continue indirectly irrigation work which Congress had declared should not be continued by the Geological Survey. Its irrigation work, then, has cost much nearer $500,000 than it has $235,000. Its results are two finely printed volumes — one of 123 pages and the other of 395. In the latter are 80 or 90 pages of matter previously printed — the larger part of it, indeed, having been twice printed by committees of the Senate and House. The reprint in Report Eleven is of Major Powell's testimony and argument before the House Select Committee on Irrigation, 51st Congress, which in substance and effect is the same that Director Powell made to the Senate Committee at the same session. So, in effect, it has cost nearly half a million dollars to publish 419 pages of "original" reports. There are no topographical maps of significance as yet issued.

Now, the Department of Agriculture, under its office of Artesian and Underflow Investigation, and of Irrigation Inquiry, received and expended between April 15, 1890, and May 1, 1892, just two years, the munificent sum of $70,000. During that time it made and has reported on two engineering, geological, and economic examinations of the Great Plains region, between 97° and 105° of longitude, and two reports besides on Irrigation proper. It prepared and issued six volumes in all, — a report on Artesian Wells, and the three parts you have noticed of the closing report on Artesian and Underflow Waters, also Progress Irrigation Report for 1891, and the volume referred to as "miscellaneous" by the review. As the work is in part only my own, though I edited all of it, I can justly challenge the value of it all in quality, as much as I may claim it exceeds the report in quantity, as compared with the Geological Survey. The three reports (six volumes or parts) embrace in all 1,694 pages, and some 58 valuable profiles, maps and geologic sections, besides more than 100 other special illustrations. The report (four parts) you reviewed has been printed to the number of but 1,733 copies for the use of Congress, and it has cost something less than $4,000. The other reports cost in all about $2,500 — a total estimate of $6,500. Since that publication, Congress has appropriated $6,000 more for Irrigation Inquiry. How much of this has been used I do not know; some of it I am aware has been wasted and I make the remark advisedly, as much as I regret to say anything except in approval of the Department of Agriculture.

The account stands then:—

A. Ten thousand copies (5,000 each volume under a general provision of law) of two reports, and some other reprinting by the U. S. Geological Survey, with a number of reservoir sites reserved on the public lands, most of which have been restored under later law by the Land Office to the Public Domain; the cost of all, at least, $465,000.

B. Eight reports in all by the Office of Irrigation Inquiry, Department of Agriculture, — three of the Engineers, three of the Geologists, and the same number of the Agent in charge (myself) — in all seven parts or volumes, containing the matter in brief, already stated, all this, too, in cost has been less than $80,000.

The Weather Service volume (chiefly Mr. Glassford's work) is above criticism and that of the U. S. Census Office in its "Irrigation Division" work is only an adjunct to the U. S. Geological Survey, unduly fostered by the Secretary of the Interior and the Superintendent of the Census to enable Director Powell to do that which the 51st Congress by withdrawal of a specific appropriation had forbidden him doing, viz., continue the work of irrigation survey and inquiry. The agent in charge was formerly an hydrographer in the Survey and was transferred to the Census. He has done better than it could have been anticipated he would from his first bulletins, but the work has cost far more than it is worth. That, too, from the value of the conditions and not the ability of the agent himself. Of course, it will be noticed most because it has the benefit of the expensive printing and publishing of the Census Office.

This whole irrigation inquiry has been characterized by a wasteful scramble to get in or on it. The State Department has published a volume thereon; the Treasury's Bureau of Statistics has dabbled therein in its volumes on "Internal Commerce"; the General Land Office has had its shy; the Weather Service is discussing "Earth Moisture," etc., and the Army Engineer Office got in a little one on Egypt. The Department of Agriculture only did what it was ordered and of late months not all of that.

Richard J. Hinton.
Member Am. So. of Irrigation Engrs.

Washington, Dec. 26.

Geographical Variation in Birds.

In ornithology geography is the father of trinomial nomenclature. Climate is one great factor in variation, and topography has not a little to do with making the climate; but geography is unquestionably *the* cause of variable climate, else would the polar regions be tropical instead of frigid. Topography is at best local.

The variations of a species of birds, which make of it several sub-species, are due to its geographical distribution. These varying individuals do not take the name of "forms," as in entomology, but are set apart as true sub-species, each with a more or less well defined habitat of its own. But there is a serious difficulty in ascribing any sharp line of difference between the forms which intergrade on the outskirts of the geographical range, and a corresponding difficulty in ascribing any definite geographical limit. It is not seldom that individuals of one sub-species are found far within the range of another sub-species.

It is a little singular that certain species do not vary, species which are not only found from ocean to ocean in North America, but which are nearly or quite cosmopolitan. Why this should be true of some species and not of others is still an open question. If the scorching sun of the desert regions will bleach out one species why will it not do the same for another? The plea of adaptation of coloration for protection cannot be urged here.

Not only are colors affected, but size as well, by geographical position. This is probably more marked north and south than east and west. And yet the variation in size alone is not sufficient for a subspecific division. It is not at all strange that those individuals of a migratory species which push farthest north should possess stronger bones and muscles, and so be larger than those which were not able to fly so far. It would seem natural that the constant recurrence of such a difference would tend, in time, to form a race peculiar enough to be recognized as a sub species. But it has not proven true thus far in the history of the world, and why should there be any change under the same conditions?

LYNDS JONES.

Oberlin, Ohio, Dec. 26.

On the Use of the Compound Eyes of Insects.

IN an interesting note on the above subject by Mr. E. T. Lewis in *Science* of Dec. 2, there is a reference to my note on Professor Exner's beautiful researches on the question of how the compound eyes of insects see, in my recent edition of "The Microscope and Its Revelations." Mr. Lewis says (p. 315), "but it may be as well to note that the figure on page 908 of 'The Microscope and Its Revelations' appears to have been laterally inverted by the engraver," his observations enabling him to say "that in the original photograph the letter R was not reversed as shown in the wood-cut, and the church faced the other way."

This is entirely fallacious; the wood-cut in the current edition of the "Revelations of the Microscope" is in every sense correct. It has been seen by Exner, and was copied from the original photograph, which now lies before me as sent me by Professor Exner himself; and a study of "Die Physiologie der Facettirten Augen von Krebsen und Insecten" will make this clear.

W. H. DOLLINGER.

Lee, London, S.E., England.

Discovery of Mexican Feather-Work in Madrid.

THERE are not many well-preserved specimens of native Mexicae feather-work in existence, and every addition to their number is of interest and importance. During a recent visit to the land which gave birth to the conquerors of Mexico, Mrs. Zelia Nuttall — whose researches on Mexican antiquities are well known — was so fortunate as to discover a fine example of Mexican feather-mosaic in the shape of a valuable shield, with an authentic history, preserved in the Royal Armory of Madrid. It is known as the shield of Philip the Second, for whom it was indubitably made in Spain of cane and leather in the oval shape of the Moorish *adarga*. It was then sent out to Mexico with four beautiful Spanish designs of historical scenes and a central device. These were executed in Mexico entirely of feather-mosaic, which covers the whole surface of the shield and makes it one of the most surprising and superb examples of this curious lost art of miniature painting with feathers. Mrs. Nuttall has paid considerable attention to the subject of ancient Mexican feather-work, and has already accumulated novel data which promise to throw light on the methods of its manufacture. We may look for an interesting paper on this subject from her pen before long.

Soon after the shield in question was identified by her as of Mexican workmanship — an unrecognized fact which was not recorded in the oldest Inventories — it was removed from the Royal Armory and placed on exhibition in the interesting Hispano-American Historical Exposition in Madrid. In the Spanish section of the same building may now be found also the elaborate tables, fourteen metres long, originally dasigned to illustrate Mrs. Nuttall's "Preliminary Note on the Ancient Calendar of the Axtecs," which formed the most original and valuable communication presented to the recent Americanist Congress at Huelva. It was then generally admitted that Mrs. Nuttall had fairly solved the great problem which has long puzzled Mexicanist students in general. Guided by a luminous passage occurring in an unpublished Hispano-Mexican MS. which she had previously discovered in a Florentine library and intends to reproduce in *fac-simile*, Mrs Nuttall may be said to have furnished the key to the hitherto unknown calendar system of the ancient Aztecs. It now seems to be of a very simple and harmonious character and to have been employed by them, judging from astronomical calculations, for a period of at least 4,000 years.

The Mexican cycle, it appears from these researches, was one of 13,515 days. It comprised 52 *ritual* years, less five days at the end of the cycle, each year of 260 days, or 51 *lunar* years of 265 days, based on nine moons in each year, or 37 *solar* years of 365 days in each year. At the end of the fifty-first *lunar* year ten intercalated days made the *lunar* year equal to the *solar* year, in such a manner that the new cycle began in the same lunar and solar positions as the preceding cycle of 13,515 days. Each period began with a day bearing one of four names, *acatl*, *tecpatl*, *calle'*, or *tochtle'*.

This is the most important discovery hitherto made known by the indefatigable Nahuatl scholar. Full details will eventually be published in one of the Peabody Museum papers of the American Museum of Archæology, on which Mrs. Zelia Nuttall, special assistant in Mexican archæology, and director of the same department in the Columbian Exhibition, has already reflected much honor.

AGNES CRANE.

Brighton, England, Dec. 20, 1892.

Is it Instinct or Intelligence?

I HAVE a nearly pure-blooded water spaniel. Though a great pet and most valuable watch-dog, in my busy life I have devoted little time to training him, — rather have watched carefully the development and application of his own powers, under a uniformly kind treatment. When only five weeks old, he made his first debut into the open world, — following mother and myself to church. Crossing the street, we heard the patter of little feet, and, looking around, I saw his nose close to the ground as the little ball trudged along. I took him home and started again, only to have the performance repeated, but this time I shut him in the house. Just as church service opened, mother thought she felt something strangely warm at her feet. And lo! there was Master Carlo. He had escaped, perseveringly followed our track around two blocks, and discovered mother in the congregation. From that time a remarkably keen scent has been a prominent quality. Early he manifested a love for watching and chasing chickens, — a pastime not to be neglected with the small opportunities of the city. We soon, by kindness and firmness and much talking, broke him of disturbing our own chickens. We often took a little chick in our hands, and said to him "pretty chickey, Carlo's chickey!" and allowed him to lick it gently. Soon it was not only safe, but safer to have him in the pen with the chicks than otherwise, as then no rat or mouse dared venture there. From the first, Carlo has deemed these marauders worthy of death whenever and wherever seen, and acts out his convictions. As the chickens grew, and Thanksgiving approached, their number was reduced to twelve, and these were transferred to the barn. Every night for two years Carlo made a detour of the perches, giving each fowl a good lick, — they were so acquainted it did not alarm them at all, — and if one or more of the number was absent, he would immediately scour the premises till it was found then gave a peculiar bark indicating the discovery; nor would he give it up till the number was complete. Could he count? How did he know there should be just twelve — no more, no less? Occasionally a stray fowl would come to our yard. This he tormented by keeping it constantly "on the move," not by making [illegible] run, but simply kept it walking about, persistently unless it [illegible] into the street, when he considered it game, and pursued it thoroughly. The following spring and summer, as the chic[illegible]gan to lay, he took it upon himself, without any teac[illegible] to find and bring in the eggs, never sucking any, and [illegible]aking them; if broken, it was because he laid them do[illegible]ly upon the veranda floor. When a then stole her nest, he was sure to miss her and search her out, then get her eggs, if he could reach them; if unable to do so, he would stand and whine till aid came. In our daily drives, Carlo was accustomed to go with mother and me, so when left at home he was very sad. To deceive him as to our going, we came to spell the words *go*, *barn* and *ride*. For a few days the plan succeeded well, but, regardless of special tone or other (to us) apparent association, he soon pricked up his ears at the sound "g-o," and that mystery was solved, then followed "r-i-d-e" and "b a-r-n," till those combinations were nearly as significant of a pleasure-hour to him as to us.

During mother's long and severe illness, he took great interest in all that pertained to her, watching the doctor very closely, and sitting, by the half-hour, with his chin on the bed by her side. We bought our bread, and, knowing Carlo's fondness for warm biscuit, the baker often gave him one which he quickly despatched. Once, during a very severe attack of mother's, when we were doing our utmost to tempt her appetite, Carlo came in early one morning, bringing his warm biscuit untouched, and laid it on the floor by mother's side. Too sick to notice this act of his, but not to be disappointed in his own plan, he came forward and lifted the biscuit to her pillow, and retired again to his corner to wait some look of thanks from her. It came, and such a happy dog! He had brought his choicest offering — a warm biscuit — and it had been recognized. Was there a loving plan and careful observation in this act?

One day while busy writing, I heard him in the dining-room asking to go out. The outside doors were open, and I said "Yes, Carlo may go!" and returned to my desk. Soon he repeated his request, and I rose saying "Now you must go, and not bother me so!" but he lay quietly, though anxiously, in the middle of the floor. Going to him, I found he had my canary between his front feet, not a feather injured, but waiting for me to release it in safety. The cage had accidently been left open, and finding the bird free, with these outside doors of the room open, he had gently caught and held my pet. Why should he catch it when the doors were open, when if closed he made no such effort? Who will say this was mere instinct? MARY E. HOLMES.

Rockford, Ill.

BOOK-REVIEWS.

Deep-Sea Sounding. By CAPT. A. S. BARKER, U.S.N. New York, J. Wiley & Sons, 1892. 133 p. Maps. 8°.

CAPTAIN BARKER, in this very interesting work, gives an account of the results of the explorations of deep-sea bottoms by the officers of the U.S.S. "Enterprise" in the years 1883–1886. The casts of the lead were made by Messrs. Norris and Marix, lieutenants attached to the "Enterprise," the one on the outward, the other on the homeward voyage. The ship sailed and steamed across the Atlantic and the Indian Oceans, and returned by way of the Pacific, sounding out different routes. Soundings were made daily, often for many days together; steam being raised for the purpose each time, and the fires allowed to go out again immediately after the cast. With characteristic naval spirit, the author assumes all responsibility for even the minutest detail, as where he says "*my* usual custom, during the cruise, was to use only two boilers when steaming," and where similar assumptions of credit in regard to details for which other officers were responsible, and which a commanding officer in the merchant service would have given credit for, and left absolutely, to the person best prepared by experience and judgment to perform. The two lieutenants who did the work, and the chief engineer, are, however, complimented as officers "whose intelligence, zeal, and devotion to duty could not be surpassed." This innocent and unconscious self-assertion runs through the book.

The volume is very interesting, however, and contains much new and valuable information and data. New submarine mountain ranges were discovered, and previously unknown obstructions to navigation. The voyage terminated at the further side of the Indian Ocean, immediately after the great eruption of Krakatoa;

CALENDAR OF SOCIETIES.

Society of Natural History, Boston.

Jan. 4.—W. G. Farlow, Account of Some of the Botanical Establishments of Europe; J. Eliot Wolff, Application of the Microscope to the Study of Rocks.

Entomological Society, Washington.

Dec. 31.—The eighth annual and eighty-fifth regular meeting of the society was held at the residence of the president, Dr. C. V. Riley. The following officers were elected: President, C. V. Riley; vice-presidents, W. H. Ashmead and C. W. Stiles; recording secretary, C. L. Marlatt; corresponding secretary, L. O. Howard; treasurer, E. A. Schwarz; executive committee, the officers and Dr. W. H. Fox, Dr. Geo. Marx, and Mr. B. E. Fernow. Mr. Frank Benton was elected an active member. The retiring president, Dr. C. V. Riley, then delivered his annual address on the subject of "Parasitism in Insects." The address began with a definition of the term and a classification of the subject, and then treated in detail the following subdivisions: (1) The parasites among insects proper, by orders; (2) origin of insect parasitism; (3) effects of the parasitic life; (4) economic bearings of the subject. At the conclusion of the address, on motion of Dr. Gill, the thanks of the society were voted to the president.

and the exploration of the desolated islands of that neighborhood formed one of the most interesting parts of the work. Depths were reached in the Atlantic exceeding 4.500 fathoms, and a line across the South Pacific was the first ever made in deep-sea sounding. The visit to Australia was especially interesting. The opinion of the author is highly favorable to that growing empire of the distant seas. He thinks the "working people" of Australia have more influence than elsewhere, and that their average intelligence exceeds that of our own people even. The four millions are "a whole-souled and gallant race," and the visitors "left their country with a genuine love for the people and a firm belief in their future greatness." The "Enterprise" sailed on Jan. 3, 1883, and went out of commission, after having thus in three years circumnavigated the globe, March 31, 1886. The log of the soundings, and the roll of the officers and men, are appended to the book, which is continuously of interest from its first page to the last.

Elements of Graphical Statics. By L. M. HOSKINS. New York and London, Macmillan & Co., 1892. 8vo. pp. viii., 191. Pl. v. $2.25.

THIS work is an elementary text-book for use of students in engineering. Fundamental principles and simple methods of treatment are illustrated, and illustrated well. The funicular and other polygons, and figures related to them, are deduced from statical principles. The theory of elasticity is omitted. Bow's notation has been adopted and extended, and the lettering of both the force and the space diagrams is thus made at once convenient and intelligible. The whole constitutes an excellent graphical discussion of the general and fundamental principles of mechanics, and in such form as to be especially useful in applications by the engineer in design and construction. The tracing of the forces involved in the framing of structures and the relations of efforts and resistances in such constructions is, by these methods — now becoming well known and extensively applied — made both easy and simple. The work is likely to prove valuable both in instruction and in office work.

AMONG THE PUBLISHERS.

THE ninth volume in the series of technological handbooks issued by George Bell & Sons, London (Macmillan, New York), is devoted to "Silk Dyeing, Printing, and Finishing," by George H. Hurst, F.C.S. The substance of the book consists of a series of articles contributed to a technical journal, though they have been revised and to some extent rewritten. In addition to these there are added chapters on silk printing and finishing, and on the testing of dyed silks. The methods of using all the new coal-tar colors, which have of late years led to new developments in silk dyeing, have been included. The book is a thoroughly practical one, not a mere collection of recipes — though recipes are not lacking. The appendix contains a series of patterns illustrative of the tints and shades produced in the dyeing of silks. (237 pages. 12°. $2.)

— Macmillan & Co. have just issued a revised and enlarged second edition of "Blowpipe Analysis," by J. Landauer, member of the Imperial German Academy of Naturalists (authorized English edition by James Taylor, B.Sc., Wh. Sc., A.R.S.M. The soundness of the principles on which the work is based is attested by the favorable reception accorded to it in the various languages into which it has been translated, as well as by the fact that new editions have been found necessary. In the present edition not only has the text been completely revised, but new methods of approved value have been incorporated, so as to bring the work up to the present time. Some additional details of manipulation will be found of value by readers who are working up the subject without a teacher. A handsome plate of the spectra of the metals of the alkalies and alkali groups, from the drawings of Bunsen and Kirchhoff forms the frontispiece. (173 pages. 12°. $1.10.)

TO THOSE INTERESTED IN SCIENCE.

[Science (weekly) established in 1883, N. D. C. HODGES, 874 Broadway, New York City.]

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To the Readers of SCIENCE.

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N. D. C. HODGES, 874 *Broadway, New York.*

// SCIENCE

NEW YORK, JANUARY 13, 1893.

THE WORK OF THE U. S. GEOLOGICAL SURVEY.[1]

BY J. W. POWELL, DIRECTOR.

Organization.

UNDER statutory provisions, it is the function of the Geological Survey to classify the public lands and examine the geologic structure and the mineral resources and products of the national domain, and to prepare a geologic map of the United States.

When the bureau was instituted in 1879, it was organized into a number of geologic divisions by the first director, Mr. Clarence King. Work was at once commenced in western States and Territories in several localities selected by reason of great mineral wealth or special scientific interest.

It was found at the outset that there were no adequate maps of the regions selected for survey; and it soon became evident that the geologic work could not be carried on without maps showing the relief of the land as well as the hydrography and culture. Accordingly, topographic surveys were instituted in each of the regions selected for examination. At first these surveys were planned to meet immediate needs, and the methods of mapping were not systemized or unified; the scales were diverse and the methods various, the areas were selected by geologic needs and were not fitted to a general scheme for the geologic map of the country, and the resulting maps were discordant in their conventions. At this stage the topographic surveys were executed under the direction of the chiefs of the geologic divisions. After two or three years of trial this form of organization was found unsatisfactory, and the topographic surveys were separated from the geologic work and assigned to a geographic division, which has ever since been maintained.

When the bureau was created, the science of geology was less specialized than to-day, and the geologists assigned to the different divisions were individually charged with the duties of identifying fossils, making analyses and assays, recording mineral statistics, and other collateral work, in addition to their areal and structural surveys; but, with the expansion of the several divisions, the different lines of work were gradually differentiated in each, so that each chief geologist employed assistants charged with special work; and still later it was found more economical to separate the collateral work for the entire survey, and to assign it to special divisions. In this way a division of chemistry and physics, a division of mining statistics and technology, and several divisions of paleontology were created and have since been maintained.

In the beginning the geologists commonly made their own drawings and constructed their own geologic maps; but with the extension of the work it was found better to assign all such mechanical work to skilled draftsmen; and still later it was found more economical to concentrate the work of this character in a division of illustrations.

Under the comprehensive plan for the construction of a geologic map of the United States, the topographic surveys were planned to yield atlas-sheets of uniform character, so related as together to make up a great map of the national domain. At first these atlas-sheets were engraved under contract through the Public Printer; but it was subsequently ascertained by experiment that the engraving could be executed at much less cost in the office of the Geological Survey, and an engraving division was instituted and is still kept up.

Thus the principal branches of work in the bureau are (1) the geologic survey proper, including the examination of the public lands and the study of mineral deposits as well as the preparation of the geologic map; (2) a topographic survey, designed as a basis for the geologic map of the United States; (3) paleontologic researches, designed primarily to aid geologists in the identification and classification of rock formations and mineral deposits, and incidentally to increase knowledge of past life upon the earth; (4) the collection and publication of mineral statistics; (5) the chemical and physical examination of ores, rocks, and other mineral substances; (6) the preparation of special charts and other illustrations for reports; and (7) the engraving of topographic and geologic maps. Clerical, editorial, and other lines of work are also carried on.

Current Work.

Three principal lines of work are carried on, to which the other lines are collateral. Foremost among these is the geologic survey proper, which is made with a view to the preparation of the geologic map of the United States; but this work can be carried forward only in those areas in which the second principal line of work, i.e., the topographic survey, is completed. The third principal line is the collection and publication of information concerning the mineral resources and mining products of the country.

In describing the work of a scientific institution it is necessary to distinguish two stages in the development of scientific work, viz., the preliminary, or experimental, or preparatory stage, and the final or effective stage. During the first stage methods are devised, experiments are conducted, scientific apparatus is invented and subjected to trial, and the plan for the work is formulated; during the second stage the methods and apparatus are practically employed and the plans are carried out. Thus the first stage is that of research, more or less recondite according to the character of the work, the second stage is that of applied science; and since it is the highest function of systemized knowledge to promote human welfare, the first stage represents the seed-time, the second the harvest-time of science.

Now, in classing the work of scientific institutions by these stages, it is to be observed that the stages are unequal in all cases and dissimilar when different cases are compared: Thus, topographic surveying may be considered as an art and the methods and apparatus already known may be employed without research into principles or the development of new methods, or the art may be considered incomplete and new principles and methods may be developed from research and experiment; while geodesy always involves research concerning principles, which, in turn, affects methods. So, too, geologic surveys might be taken to represent applied science, and geologic tryos might be sent over the land to plat dips, strikes and outcrops, and to construct simple and primitive geologic maps adapted to the needs of the preceding generation; but it is the honor of geology that geologic surveys have commonly begun their work by researches relating to their special fields, and have thus advanced the science and subserved the needs of their own contemporaries and the coming generations. Again, simple land surveys represent art or applied science alone, while the natural history surveys sometimes conducted by States represent nearly pure research. In brief, it may be said that the proportion of research to effective work increases with the complexity of the branch of knowledge to which it pertains. So in the three principal lines of work in the Geological Survey, the collection of information concerning mineral resources and mining products is a statistical work involving no research and little experiment; the topographic survey might have been conducted by old methods and apparatus without research and experiment, but since these were expensive and dilatory, considerable preparatory work became desirable; while the geologic survey required a vast amount of research and experiment for the purpose of developing a sat-

[1] Read before the Geological Society of America at Ottowa, Dec. 30, 1892.

isfactory classification of rocks and a satisfactory system of mapping. These conditions, in connection with the conditions growing out of the changes in the organic law of the bureau, have determined the character and progress of work in the Geological Survey.

The work of collecting mineral resources has been steadily carried forward, and it has been found thereby that the mineral production of the country has more than doubled within the thirteen years since the institution of the Survey, and that many new resources have been discovered and utilized. The statistics are collected with care by means of an elaborate system of correspondence and collaboration, and are published in a series of annual volumes. The annual mining product of the country has now reached the value of six hundred and fifty million dollars. The products form the basis for many of our industries and a large proportion of our commodities, and give employment to a great part of our population. Thus a principal source of our national prosperity is made public and rendered available for further development through this part of the work of the Geological Survey.

At first the art of topographic surveying was imperfect and the work was slow and expensive. Accordingly, experiments were conducted in different types of country, with different kinds of apparatus, and with different men and methods; and, after determining the best methods and apparatus for each part of the country, a corps of topographers was trained. This experimental stage of the work lasted four or five years, though the experiments were so conducted as to yield useful results which are incorporated in the atlas-sheets representing the general topographic survey. Some of the earlier sheets were, however, found defective, and in these cases the experimental surveys were repeated and the sheets revised. The topographic surveys have how been completed over an area of about six hundred thousand square miles, or about one-fifth of the national domain, exclusive of Alaska. The total cost, including experimental work and an extensive plant, has ranged from one dollar to fourteen dollars per square mile, averaging about four dollars. Thus it is believed that the surveys of the bureau have been more expeditious and less expensive than any other topographic surveys of equal accuracy thus far made in any country. The work is not geodetic, nor is it cadastral; yet, while it is primarily designed only as a basis for the geologic surveys and thus for the geologic map contemplated in the statute, the atlas-sheets have been found useful for many other purposes. They are in constant demand by engineers, road commissioners, miners, and prospectors, and are widely accepted as the most useful bases for mining and commercial maps and school and general atlases.

When the geologic studies were commenced much of the national domain had never been examined by geologists, and thus the rock formations and mineral deposits of the country were not classified; moreover, there was no comprehensive plan for geologic mapping. Accordingly, in geology as in topography, the initial stage of the work was preparatory and was designed to develop, first, a system of classification of rocks, and, second, a system of mapping them. But, while the work was experimental, it was conducted in accordance with the best systems of classification and mapping already in vogue in this and other countries, and was thus made to yield useful results which are published in preliminary maps and reports. These preliminary results of the work are incorporated in thirteen royal octavo annual reports (of which the last three are about to leave the press), twenty quarto monographs, and one hundred octavo bulletins, in addition to seven octavo volumes of the reports of mineral resources.

By reason of the immaturity of the science, and by reason of the vast extent and complexity of the rocks of the country, the preliminary stage in this work was longer than in topography, lasting indeed ten or twelve years. Within the last two years the classification of rocks, mineral deposits, and superficial formations has been so far elaborated as to warrant use as a basis for the geologic map of the United States; and, at the same time, a system of mapping has been developed. Under this system provision is made for representing the sedimentary, igneous, and ancient crystalline formations, as well as the mineral deposits associated with each, by distinct conventions; and provision is made also for mapping the superficial formations on separate sheets in those regions in which they are well developed and of economic or scientific importance. This system of mapping has been under actual trial for two years, and is now practically applied. Over a dozen sheets have been engraved in the office of the Survey within the past year, and several others have been published or are in press, appended to reports on special regions or topics; and a still larger number are completed in manuscript. One hundred atlas-sheets representing rock formations and mineral deposits, each constituting a section of the final map, are engraved or ready for engraving; and these sheets cover an area of about 120,000 square miles, or four per cent of the national domain, exclusive of Alaska. Moreover, sixty atlas-sheets showing superficial formations have been completed in regions in which the underlying rocks are generally inaccessible and of little economic value, and these sheets cover an additional area of about 60,000 square miles. Thus the aggregate area now mapped geologically reaches 180,000 square miles, or six per cent of the national domain.

In addition to the areal surveys, important results have flowed from the researches conducted by the Geological Survey. These results are not easily stated, partly because science is not quantitative and cannot be weighed and measured in any standard units, partly because science is common property and some portion of each great result is to be credited to scientific investigators not connected with the bureau. Nevertheless, a number of valuable additions to the science of geology have been made during the past decade, largely through the labors of the able corps of experts, to whose skill, zeal, and industry the bureau owes much of its success. Among these may be mentioned, the recognition and founding of a great rock system, the Algonkian; the discrimination of glacial deposits throughout northern United States and the interpretation of the complex and wonderfully interesting history of which they are records; the discovery of the rate of seismic transmission and of other laws of earthquakes; a classification of the igneous rocks and a tentative grouping of the ancient crystallines; the development of a new division of geologic science — Geomorphy, or the New Geology — in which the past history of the earth is read from topographic forms, as formerly from formations and their fossils; and a general physical classification of the rocks of a considerable portion of the country.

The cost of the areal geologic work has ranged from less than a dollar per square mile in provinces of simple structure to fifty or sixty dollars per square mile in certain mining regions of exceptionally complex structure. The average cost, making reasonable allowance for reconnaissance, and reckoned on the basis of aggregate appropriations, is eight or nine dollars per square mile. It is to be noted that this figure includes all collateral work in paleontology, chemistry and physics, mineral resources, engraving, and miscellaneous work of all kinds, as well as the acquisition of a large library, the publication of one hundred and forty reports, the training of experts, and the purchase and maintenance of an extensive plant, together with a general reconnaissance of the country. The actual cost of the geologic surveys in two representative provinces, including field and office work as well as supervision and revision, ranges from two to three dollars per square mile. Accordingly, although the geologic work is barely past the experimental stage, the cost compares favorably with that of similar work executed in foreign countries and in our own States.

Future Work.

It is believed that the organization of the work of collecting and publishing mineral statistics is now so complete and the corps of correspondents and other collaborators so expert and zealous that this branch of the work may be carried forward more expeditiously and economically than ever before. It is the design not only to continue but gradually to expand this branch of the work, in order that it may keep pace with the increasing development of mining production, the discovery of new mineral resources, and the invention of new applications for resources already known.

While some mistakes have been made, it is believed that the topographic methods and the apparatus employed are now thoroughly effective; and that for this reason, and for the further reason that a corps of expert topographers has grown up, this branch of the work can also be carried forward more expeditiously and economically than ever before. At first most of the work was executed on a scale of four miles to the inch, another part on the scale of two miles to the inch, and only a small part on larger scales; but the improvement in methods, apparatus, and skill has been such that the surveys can be made on a scale of a mile to the inch at slightly greater cost than the original surveys on a quarter of that scale; and, accordingly, all the surveys of the bureau are now made on the two-mile scale and the one-mile scale, and the four-mile scale has been abandoned. It is proposed to continue the work on these scales, and to give such attention to minor topographic details as to yield a good topographic map of the entire country, which, while neither geodetic nor cadastral, will serve as a satisfactory basis for geologic surveys and for a wide variety of industrial purposes.

In geologic surveying, and thus in the preparation of the geologic map of the United States, the work is rapidly passing from the preliminary stage of research to the effective stage of applied science; and it is believed that the methods developed are so far satisfactory as to warrant a definite working plan for the future. This plan includes a system of rock classification and a system of map conventions based thereon by which widely applicable and useful distinctions may be made. It includes also a system of arranging the atlas-sheets constituting sections of the geologic map of the United States provided for by the statute, and the accompanying descriptive text in atlas folios designed for convenient distribution and use. Each atlas folio is inclosed in a cover bearing a suitable title and a key-map locating the atlas-sheet, and each contains a copy of the topographic sheet without geologic colors; a second copy colored by formations; a third copy colored by groups with structure sections introduced; a fourth copy colored by formations of economic value and showing also the locations of mines and industrial establishments depending on mineral resources; in the glaciated regions a fifth copy showing superficial formations and their resources; and sometimes additional sheets giving columnar sections and other illustrations of the region. The accompanying text includes an elementary explanation of the atlas, a general sketch of the geologic province, and a special description of the area covered by the sheet. Furthermore, the plan contemplates the extension of the geologic surveys from the regions of complex structure, in which the classifications were developed, into regions of simpler structure, in which more rapid progress may be anticipated. Moreover, since a corps of experts has now been trained in the methods and the classification developed in the bureau, and since these experts are now ready to extend operations into the rich mining regions and other important fields in which premature work would have been unwise, it is planned to strengthen this technical work and thus to enhance the economic value of the geologic map without detracting from its scientific character.

The purpose of our statesmen in instituting the Geological Survey was to enable those engaged in mining and related industries to exploit our mineral resources safely and economically. It has always been recognized that mineral resources depend on rock structure, and that the structure and relations of rocks cannot be made intelligible to practical men without classification; moreover, it was understood that the structure and relations of rocks cannot be described, and in some cases cannot even be ascertained, without maps. It was for these reasons that statutory provision was made for the construction of a geologic map of the United States in connection with the examination of the mineral resources and mining products; it was for the same reasons that the topographic survey was undertaken as a basis for the final geologic map. The work of the topographic branch of the bureau has passed the experimental stage and entered upon the effective stage, while the work of the geologic branch is now passing from the stage of elaborate and often recondite research to the effective stage; and it is designed to carry forward the work of both branches with energy and to proceed with the preparation of the geologic map on a basis at once thoroughly scientific and economically useful. In fact, during the past two or three years the transformation in geologic work has been in progress and is now practically accomplished. Thirteen atlas folios are now engraved, and the field-work for about 160 atlas folios is completed, while the field-work required for a still larger number is in progress. In addition to these completed surveys, a general reconnaissance has been extended over about four-fifths of the entire area of the United States, and a reconnaissance map representing the results of this work is now in the hands of the engraver.

THE GEOLOGICAL SOCIETY OF AMERICA.

THE fifth annual and winter meeting of the Geological Society of America was held in Ottawa, Canada, beginning Wednesday, Dec. 28, 1892.

Through the kindness of Dr. J. G. Bourinot, C.M.G. of the Royal Society of Canada and clerk of the House of Commons the ample and commodious room of the Railway Committee of the House of Commons was placed at the disposal of the society. There were about forty Fellows present, sixteen of whom came from various portions of the United States. The meeting was under the presidency of Mr. G. K. Gilbert, Chief Geologist of the United States Geological Survey, Washington, whilst Prof. H. L. Fairchild of the University of Rochester was secretary.

If we are to judge by the attendance and interest manifested at the meeting, as well as by the grade of papers presented, there is no doubt that it was a decided success. A local committee composed of Fellows of the Royal Society, members of the Logan Club, which comprises the scientific staff of the Geological Survey of Canada, etc., had made all necessary arrangements for the comfort and lodging of the members during the meeting. Dr. Selwyn, as chairman of the committee, and Mr. Smith as secretary, spared no pains to give the visiting Fellows of the Society a good reception. Much praise is also due His Excellency the Governor General for the exceedingly kind and generous manner in which he devoted so much time and attention to the society besides furnishing the Fellows from a distance with an excellent opportunity of having a glimpse of social life at the Canadian capital by giving an "at home" at Rideau on Friday afternoon. To Dr. Ells, Mr. J B. Tyrrell and others, much credit is due for their exertions in preparing matters.

Shortly after ten o'clock on Wednesday, the 28th, President Gilbert took the chair and called upon His Excellency the Governor General to give the address of welcome. His Excellency made a very neat address which was received enthusiastically. To this the president replied, and referred to the proverbial hospitality for which Canadians were noted. The report of the Council was then presented by the secretary and the result of the vote announced so far as conclusions were arrived at. The following leading officers were then declared elected: President, Sir J. William Dawson; secretary, Prof. H. L. Fairchild; treasurer, Dr. T. C. White. The Secretary's report, as well as that of the treasurer, showed the society to be in a flourishing condition. Then followed obituary notices of three deceased Fellows: T. Sterry Hunt, J. S. Newberry, and J. H. Chapin. Prof. Raphael Pumpelly's notice of Dr. Hunt was read by Mr. Van Hise; that of Prof. Newberry, prepared by Dr. Kemp, was read by Prof. H. L. Fairchild; and Prof. Hitchcock read W. M. Davis's memorial of J. H. Chapin.

The reading of papers or work proper of the society began Wednesday afternoon at 2 P.M. Below is a list of the papers, in the order in which they were taken up at the meetings. The whole time of the society was taken up reading and discussing papers until a late hour Friday, the 30th of December. Time and space do not allow us here to do justice to the interesting discussions on the papers presented. Both Glacial and Archæan geology received a goodly share of animated discussion, whilst a few papers on palæontology also stimulated further inquiry. Dr. Willard Hayes's paper on "The new geology" was a splendid contribution to the geomorphology of the district examined by that author and described by him.

List of Papers.

A. R. C. Selwyn, On the coals and petroleum of the Crow's Nest Pass, Rocky Mountains; H. P. Brumell, On the geology of natural gas and petroleum in Ontario; H. P. Brumell, Note on the occurrence of petroleum in Gaspé, Quebec; Elfric Drew Ingall, Some features of the phosphate-bearing rocks of Ottawa (read by title); Sir J. William Dawson, Note on sponges found in the Cambro-Silurian at Little Metis, Canada (read in the absence of the author by Mr. F. D. Adams); J. F. Whiteaves, Notes on the Devonian formation of Manitoba and the N. W. Territories; Henry M. Ami, Notes on Cambrian fossils from the Selkirks and Rocky Mountain Region of Canada; Henry M. Ami, On the Potsdam and Calciferous terranes of the Ottawa Palæozoic basin; R D. Salisbury, Distinct glacial epochs, and the criteria for their recognition; J. B. Tyrrell, Pleistocene phenomena in the region southeast and east of Lake Athabasca, Canada; A. P. Low, Notes on the glacial geology of the Northeast Territories; Robert Chalmers, The height of the Bay of Fundy coast in the glacial period relative to sea-level, as evidenced by marine fossils in the boulder clay at Saint John, New Brunswick; W. J. McGee, The Pleistocene history of Northeastern Iowa; Warren Upham, Eskers near Rochester, N. Y.; Warren Upham, Comparison of Pleistocene and present ice-sheets; G. Frederick Wright, The post-glacial outlet of the Great Lakes through Lake Nipissing and the Mattawa River; N. H. Darton, On certain features in the distribution of the Columbia formation on the Middle Atlantic slope; George M. Dawson, Note on the geology of Middleton Island, Alaska (read by R. W. Ells); Waldemar Lindgren, Two Neocene Rivers of California; Robert W. Ells, On the Laurentian of the Ottawa district; Robert Bell, The contact of the Laurentian and Huronion north of Lake Huron; W. H. C. Smith, The Archæan Rocks west of Lake Superior; Alfred E. Barlow, On the Archæan of Sudbury mining district; C. R. Van Hise, The volcanics of the Huronian south of Lake Superior; Charles Rollin Keyes, Some Maryland granites and their origin (read by Mr. U. S. Grant); Charles Rollin Keyes, Epidote as a primary component in granites (read by Mr. U. S. Grant); James McEvoy, Notes on the gold range in British Columbia; Israel C. Russell, A geological reconnoissance in the central part of the State of Washington; R. W. Ells, The importance of photography in illustrating geological structure; J. W. Powell, The work of the United States Geological Survey (read by W. J. McGee); J. S. Diller, Cretaceous and Tertiary rocks of the Pacific States; T. W. Stanton, On the faunas of the Shasta and Chico formations; C. Willard Hayes and M. R. Campbell, Geomorphology of the southern Appalachians; N. H. Darton, Overthrust faults in eastern New York (read by W. J. McGee).

The president's address, on the "Problems of the Continents," was an admirable paper which brings up and introduces a subject of paramount importance. It serves as a preliminary basis for work at the coming meeting of geologists at the International Congress to be held in Chicago this summer.

Mr. W. J. McGee's public lecture was given in the new auditorium of the Normal School, on the subject "A fossil earthquake;" seldom has an Ottawa audience listened to a clearer and more striking bit of inductive reasoning than this lecture. About 300 persons were present, and the lecture was illustrated by stereopticon views. Mr. H. N. Topley kindly assisted the lecturer in this matter.

After the reading of the last papers on the list and programme on Friday evening, three votes of thanks were unanimously passed by the society. The first to the President and Fellows of the Royal Society, for their invitation and attention during the session of the Geological Society. The second to the Governor General for his hospitality and the generous as well as gracious interest he had taken in the meetings. The third to the Logan Club of Ottawa for its exertions in making the meeting a success.

One interesting feature of these meetings was the presence of the Premier of Canada, the Hon. Sir John Thompson, K.C.M.G., and of the Hon. T. M. Daly, Minister of the Interior and Geological Survey Department, when Dr. McGee read the paper prepared by Major J. W. Powell, director of the United States Geological Survey on the work of that survey. At the conclusion of the paper Sir John Thompson, Mr. Daly, M P., and Dr. Selwyn took part in the discussion. The comparative work and usefulness of the Geological Surveys of Canada and the United States was an interesting as well as practical question.

Altogether the meetings were most successful and teeming with interest, and closed with hopes of having another similar one at no distant date.

SEVENTH ANNUAL MEETING OF THE IOWA ACADEMY OF SCIENCE.

The seventh annual meeting of the Iowa Academy of Science convened in the High School Building in Cedar Rapids. Several enthusiastic sessions were held during day and evening of the 27th and 28th. The following papers were read: —

Professor S. Calvin presented a paper "On the Relation of the Woodbury Sandstones and Shales and the Inoceramus Beds of White to the Subdivisions of the Cretaceous proposed by Meek and Hayden," in which he gave a thorough review of the subject and illustrations of various sections bearing upon it. Perhaps one of the most important points of the paper was in regard to the identity of strata differing lithologically at different points, but proven to be the same, and in the view of the author the difference due simply to difference in distance from the shore line of the water in which they were deposited.

In a paper on "The Structure and Probable Affinities of Cerionites dactyloides Owen," Professor Calvin discussed the former views regarding this problematic fossil and showed some very fine specimens and drawings illustrating his view that this is a gigantic Prototozoan or colony of protozoans, a view which, with the evidence presented, seems more reasonable than any hitherto offered.

Dr. C. R. Keyes read a paper "On Natural Gas and Oil in Iowa," in which he maintained that the failure to find these materials in paying quantities so far in this State is not to be taken as proof that they will not be found. He also presented by title two papers, one entitled "Some Mineralogical Notes," and the other "Surface Disintegration of Granitic Masses and Some American Eruptive Granites."

Professor J. L. Tilton, in a paper "From Ford to Winterset," gave a number of carefully determined sections of the various exposures between these towns, and illustrated by a large chart in which they were shown drawn to scale for the entire distance.

Professor C. O. Bates discussed the "Analysis of Water for Railway Engines," giving details of his work in this line and suggestions as to the methods to be used and the results desired in such work.

Professor F. M. Witter, in "Some Observations on Helix cooperi," gave an interesting account of his observations on this mollusk in Colorado and exhibited a number of specimens of different ages.

His paper on the "Absence of Ferns between Fort Collins and Meeker, Colorado," contained a statement of his efforts to secure these plants in that region and discussed the causes for paucity of such material.

Professor Witter also presented a paper entitled "Notice of Stone Implements from Mercer County, Illinois, and Louisa County, Iowa," and accompanied it with exhibition of two very interesting stone implements.

Mr. Gilman Drew discussed "The Frogs' Lease of Life," giving a graphic account of the ability of frogs to survive under adverse circumstances, and showing that it has a very strong vitality. Details of a number of experiments in subjecting frogs to temperatures at varying points below freezing were given, also observations on the vitality of frogs' eggs.

Mr. Drew also remarked upon the inheritance of acquired characters as illustrated in the Honey Bee.

Professor C. C. Nutting, the retiring president of the Academy, took as his subject for presidential address "What We Have Been Doing," and showed in a very exhaustive and pleasing article what the members of the Academy had been engaged in scientifically during the year past. His paper will be an interesting bibliography of the scientific papers published by Iowa men.

Professor Nutting's "Report as Chairman of the Committee on State Fauna" contained a number of additions to the known fauna of the State and notes on varied faunal relations among a number of species heretofore recognized. The additions, which embrace only Vertebrates, include two mammals, nineteen birds, five reptiles, one batrachian, and five fishes.

The "Significance of the Concealed Crests of the Tyrannidae" was the title of another paper by the same author and discussed very elaborately the origin and use of the bright colored crests of different members of the Flycatcher family. Considerable evidence was produced to show that they assist the birds in securing food by alluring insects within easy reach.

Professor L. H. Pammel presented papers on "Phænological Notes for 1892," "Relation of Frost to Certain Plants," "Notes on the Flora of Arkansas and Texas," and "Pollination of Cucurbits."

The second paper contained records of numerous observations on temperatures and effect on different kinds of vegetation. The third contained notes collected by the author during two trips in the region mentioned, and the third, which was accompanied by a number of very fine drawings illustrating the anatomy of the flowers of cucurbits, a number of observations with regard to the pollination of these plants.

Mr. F. C. Stewart presented a paper on "Palisade Cells and Stomata of Leaves," giving record of numerous examinations of leaves of various plants, and especially of different varieties of apple and presenting the conclusion that these factors have little relation to the resistance of the plants to climatic conditions. Mr. Stewart also presented a "Key to the Identification of Weed Seeds."

Mr. H. A. Gossard presented "A List of Insects that have been taken in Clover, in Iowa," with observations on a number of the different species. It includes a large proportion of the species that have been accredited to this plant heretofore and a number of species not hitherto accredited with feeding upon it.

Dr. W. B. Niles presented "Preliminary Observations on a Cattle Disease of Frequent Occurrence in Some Parts of Iowa." In this paper the symptoms of the disease were described, and a statement of efforts to secure cultures of organisms occurring in the diseased animals. Inoculations direct from diseased animals had produced similar symptoms and disease, but inoculations with pure cultures of any of the organisms isolated had so far given negative results.

Mr. F. Reppert presented some "Notes on the Flora of Muscatine," containing record of some plants which appear to be quite out of their normal range. He described the peculiar conditions of the locality where most of these exceptional plants have occurred and suggested that such plants had probably been introduced there by the agency of such birds as ducks or geese.

Mr. F. W. Mally presented a "List of the Tenthredinidæ of Iowa," preliminary to a more exhaustive study of this group in the State.

Professor Herbert Osborn and F. A. Sirrine in "Notes on Aphididæ" presented a list of about forty species that had not hitherto been recognized in the State and notes of the habits of a number of species, also a description of a new species.

Professor Osborn also read a paper "On the Life Histories of Certain Jassidae," giving in detail the life histories of *Deltocephalus inimicus, Deltocephalus debilis*, and some others, and mentioned their relation to economic treatment of these species.

He also presented some notes on the "Catalogue of Iowa Hemiptera," making some additions and corrections to preceding lists.

His talk on a collecting trip to southern Mexico contained observations on various points visited as far south as Isthmus of Tehuantepec and references to the native people and animals observed. The talk was illustrated with lantern views of scenery in the localities visited and views showing costumes of the natives, animals of the region, etc.

The Proceedings of the Academy are now published by the State, a bill for that purpose having passed the last Legislature, and the papers presented at this meeting will be printed as soon as possible.

The officers for the current year are: President, L. H. Pammel, Ames; first vice-president, C. O. Bates, Cedar Rapids; second vice-president, A. A Veblen, Iowa City; secretary-treasurer, Herbert Osborn, Ames; Executive Committee, the officers and S. Calvin, Iowa City; F. M. Witter, Muscatine, and H. W. Norris, Grinnell.

UNUSUAL ABUNDANCE OF THE GROSBEAK IN EASTERN MASSACHUSETTS.

BY J. H. BOWLES, PONKAPOAG, MASS.

ALTHOUGH considerable of a rambler, I have never until this year noticed the Pine Grosbeak (*Pinicola canadensis*) in this vicinity. The unusually cold weather that we have had this winter seems to have thoroughly disturbed them in their northern homes, as, for the last two weeks, they have been around here in great numbers. The first that I noticed was a flock of six, on Dec. 19, which were feeding on cone-seeds in the top of a hemlock tree. Since then I have noticed flocks, almost every day, ranging in numbers from three to seventeen, although small flocks of six or eight are most commonly seen. Only a very few were in the full red plumage, most of them showing it on the head and rump only. Their flight is exceedingly graceful, consisting of dips toward the ground, in the manner of a woodpecker, only not so much exaggerated, in which they utter from time to time a short, mellow whistle. They are seen principally feeding on the buds of maple, walnut, ash, and evergreen trees, and seem to be always hungry, which, I think, in a measure accounts for their extreme tameness, as they will allow a person to approach within a few feet of them without taking any notice. When feeding in the road, which they sometimes do, they will allow a team to come almost on top of them before flying to the side of the road, only to come back again as soon as the team has passed. I cannot help mentioning here that the trait of coming one winter and being absent the next is very common with some birds. The Snow Bunting (*Plectrophanes nivalis*), for instance, although seen in large numbers last winter, has not made its appearance once this year as far as I have heard. Certainly it is not so plentiful, as last winter I saw a large number of flocks of from six to fifty and one flock of about one hundred and fifty. The red-bellied Nuthatch (*Sitta Canadensis*) and the Yellow Red-Poll Warbler (*Dendroica palmarum*) also, which were very common several winters ago, have been completely missing since that time.

NOTES AND NEWS.

FOUR courses of lectures are being given by the Department of Biology of Columbia College, in Room 11, Library Building, on successive Thursday evenings, at eight o'clock, beginning Nov. 10, 1892. They are designed for those who desire to keep abreast of the later advances in biology without entering any of the technical courses. A limited number of tickets for the entire course will be issued to persons not students on payment of a small fee. Application should be made to the Secretary of the President, Columbia College. The course on the History of the Theory of Evolution, by Henry F. Osborn, Sc.D., Da Costa professor of biology, was finished Dec. 15. A course on The Cellular Basis of Heredity and Development, by Edmund B. Wilson, Ph.D., adjunct professor of biology, beginning Thursday, Jan. 12, will consist of: Introduction: Cellular Basis of the Living Body. The Germ-Cells: Sex and Fertilization. Cell Genesis and Division. Egg and Spermatozoön: The Preparation for Development. Physiology of the Individual Cell. Inter-Cellular Dynamics: Theories of Heredity. This course will be of the greatest interest, as the progress during the last two years in our knowledge of the cell is simply marvellous. Courses will follow on The Origin and Evolution of the Fishes, by Bashford Dean, Ph.D., instructor in Biology, and Amphioxus and Other Ancestors of the Vertebrates, by Arthur Willey, B.Sc., tutor in Biology.

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Attention is called to the "Wants" column. It is invaluable to those who use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The "Exchange" column is likewise open.

RECENT PROGRESS IN AMERICAN HORTICULTURE.[1]

BY L. H. BAILEY, ITHACA, N.Y.

YOU have asked me to say something about recent progress in horticulture. I am at a loss to know how you want the subject treated. The subject is a large one, and can be approached in many ways. It is by no means admitted that there is any recent progress. There is a large class of our horticultural public which disparages these modern times as in no way so good as those of several or many years ago. These men are mostly gardeners who were apprenticed in their youth. There is another class which decries the introduction of new varieties of plants, thinking these novelties to be unreliable and deceitful. There are others who are content with the older things and who have never had occasion to ask if there has been any progress in recent years. Others have looked for progress, but have not found it. A professor of horticulture told me a few days ago that nothing new nor interesting seems to be transpiring in the horticultural world. Some people even deny outright that any progress is making at the present time. On the other hand, there are some, perhaps the minority, who contend that they see great advancement. Perhaps these are mostly young men. Then there are the catalogues with their fascinating impossibilities, pregnant with the glory that is to come. Between all these diversities, where is the young man to stand who loves plants and sunshine and is yet ambitious? Is there any progress in horticulture? If not, it is dead, uninspiring. We cannot live on the past, good as it is; we must draw our inspiration from the future. This subject is of vital personal interest to me; it must be so to you.

I cannot forego the satisfaction of saying at the outset, that some of this supposed stagnation must be due to blindness on the part of the observer. The apprenticed gardener underwent in his youth the stupendous misfortune of having learned the art and science of horticulture. The apprentice system, in itself, does not often educate a man; that is, it does not make him a student. It teaches him to base the whole art upon rule, personal experience and "authority;" it is apt to make him a narrow man, and he may not readily assimilate novel methods. Those who have looked for progress and have not found it, may have looked in the wrong place. It is possible that they do not understand very clearly just what progress is. Those who are simply indifferent exert little influence upon our inquiry and may be omitted. Those who see progress upon all sides may be over-sanguine. Perhaps they project something of their own passion into their statements. And the catalogues, being for the most part editorial rather than horticultural productions, may be liberally discounted as evidence. It is apparent, therefore, that we must make an independent inquiry if we are to answer our own question. Several considerations incline me to believe that progress is not only making, but that it is making very rapidly. And I may say here that I care little for any facts or illustrations of progress merely as facts. There must be some law, some tendency, some profound movement underlying it all, and this we must discover. I shall not attempt, therefore, to indicate how great the progress has been in any definite time, but endeavor to ascertain if there is progression which gains impetus with the years.

1. *There is a progressive variation in plants.* Horticulture is the science of cultivation of plants. The plant is the beginning and the end. For the plant we till the soil, build green-houses, and transact the business of the garden. All progress, therefore, rests upon the possibility of securing better varieties, those possessing greater intrinsic merit in themselves or better adaptations to certain purposes or regions. In other words, all progress rests upon the fact that evolution is still operative, that garden plants, like wild animals and plants, are more or less constantly undergoing modification. American horticulture may be said to have begun with the opening of the century. It was in 1806 that Bernard M'Mahon wrote his "American Gardener's Calendar." This work contains a catalogue of 3,700 "species and varieties of the most valuable and curious plants hitherto discovered." Among the cultivated varieties of fruits and vegetables, the present reader will see few familiar names. He will observe among the fruits, however, some American types, showing that even at that date American pomology had begun to diverge from the English and French which gave it birth. This is especially true of the apples, for of the fifty-nine kinds in the catalogue about 66 per cent are of American origin. Several nurseries were established in the next thirty years and fresh importations of European varieties were made, so that when Downing, in 1845, described the 190 apples known to be growing in this country, American varieties had fallen to 52 per cent. In 1872, however, when almost 2,000 varieties were described in Downing's second revision, the American kinds had risen to 65 or more per cent, or to about the proportion which they occupied at the opening of the century. At the present time, the per cent of varieties of American origin is much higher, and if we omit from our calculations the obsolete varieties, we find that over 80 per cent of the apples actually cultivated in the older apple regions at the present time are of American origin. The percentage of native varieties, in other words, has risen from nothing to 80 per cent since the apple settlement of the country, and at least once during this time the native productions have recovered from an overwhelming onslaught of foreigners. Except in the cold north and north-west where the apple industry is now experiencing an immigration not unlike that which befell the older States early in the century, few people would think of importing varieties of apples with the expectation that they would prove to be a commercial success in America. Other plants have shown most astounding development. In 1889, 39 varieties of chrysanthemums were introduced in North America; in 1890, 57 varieties; and in 1891, 121 varieties. The chrysanthemum is now the princess of flowers, yet in 1806 M'Mahon barely mentioned it, and there were no named varieties. All this is evidence of the greatest and most substantial progress, and much of it is recent; and there is every reason to believe that this rapid adaptation of plants to new conditions is still in progress in all cultivated species. In fact, the initial and conspicuous stage of such adaptation is just now taking place in the Russian apples in America, in which the American seedlings are even now gaining a greater prominence than some of their parents. Both the parent stock and the seedling brood are radical and progressive departures of recent date. The same modification to suit American environments is seen in every plant which has been cultivated here for a score or more of, years. The mulberries are striking examples, for our fruit-bearing varieties are not only different from those of Europe, whence they came, but many of them belong to a species which in Europe is not esteemed for fruit. The European varieties of almonds are now being superceded in California by native seedlings which are said to be much better adapted to our Pacific climate than their recent progenitors. These facts of rapid adaptation are everywhere so patent upon reflection that I need not consider them further at this time. They

[1] Read before the Agricultural and Experimental Union of Ontario, at the Ontario Agricultural College, Guelph, Dec. 23, 1892.

are indisputable evidence that there is permanent contemporaneous progress, and upon them alone I am willing to rest my whole argument.

There is another feature of this contemporaneous variation which must be considered at this point,—the great increase in numbers of varieties. This increase is in part simply an accumulation of the varieties of many years, so that our manuals are apt to contain descriptions of more varieties than are actually cultivated at the time. But much of this increase is an actual multiplication of varieties. That is, there are more varieties of all plants in cultivation now than at any previous time. M'Mahon mentions six beets as grown at his time; in 1889, there were 42 kinds. Then there were 14 cabbages, now there are over 100. Then there were 16 lettuces against about 120 now. He mentions 59 apples, now there are about 2,500 described in this country. He mentions 40 pears, against 1,000 now. There were something over 450 species of plants native to the United States mentioned by M'Mahon, now there are over 2,000 in cultivation. These figures are average examples of the marvellous increase in varieties during the century. I may be met here with the technical objection that M'Mahon did not make a complete catalogue of the plants of his time. This may be true, but it was meant to be practically complete, and it is much the fullest of any early list. Gardening occupied such a limited area a century ago that it could not have been a burdensome task to collect very nearly all the varieties in existence; and any omissions are undoubtedly much overbalanced by the shortcomings of the contemporaneous figures which I have given you. It is certainly true that during the nineteenth century varieties of all the leading species of cultivated plants have multiplied in this country from 200 per cent to 1,000 per cent. This variation still continues, and the sum of novelties of any year probably exceeds that of the preceding year. Every generation sees, for the most part, a new type of plants.

But I suppose that these statements as to the increase of varieties will be accepted without further proof. The question which you all desire to ask me is whether all this increase represents progress. Many poor varieties have been introduced, beyond a doubt, but I am convinced that the general tendency is decidedly progressive. You may cite me the fact that we have not improved upon the Rhode Island Greening and Fall Pippin apples, the Montmorenci cherry, the Green Gage plum, and other varieties which were in cultivation at the opening of the century, as proof of a contrary conviction; but I shall answer that we now have a score of apples as good as the Greening, although we may have none better. This habit of saying that we have not improved upon certain old plants is really a fallacy, for the reference is always made to quality of fruit alone; and, furthermore, the test of progress is not the supplanting of a good variety, but the origination of varieties which shall meet new demands. The more numerous and diverse the varieties of any plant, the more successful will be its cultivation over a wide area, because the greatest number of different conditions — as soils, climates, and uses — will be satisfactorily met. If we had at present only the apples which were grown in M'Mahon's time, apple culture in the prairie States, in our bleak North-West, and even in some of the apple sections of Ontario, would be impossible. We are constantly extending the borders of the cultivation of all fruits by means of these new varieties. The horticultural settlement of our great West and of the cold North is one of the wonders of the time. We should not ask ourselves of a new variety if it is better in all respects than other varieties, but if it will fill some specific need more satisfactorily. If a variety does better than all other varieties in one locality alone, for one specific purpose, it is not a failure, and it represents progress. Every peculiar or isolated region tends to develop a horticulture of its own, but this is possible only with a corresponding initial variation in plants. No doubt many of our discarded varieties failed to find the place or conditions in which they would have succeeded. We should not look upon adverse reports upon the novelties as necessarily denunciatory; they may only indicate that in some places or for some purposes the variety in question is unsatisfactory. I must also call your attention to the fact that while the areas of cultivation have greatly widened in recent years because of the evolution of adaptive varieties, the economic uses of the plants have increased in like ratio. We now have varieties of fruits which are specifically adapted to the making of dried fruit, to canning, to enduring long journeys, and the like; and flowers which meet specific demands in decoration or other uses. The period of maturation of varieties has extended greatly in both directions, so that fruits and flowers are now in season much longer than formerly. The gist of the whole matter is simply this, that our horticultural limits and products have greatly broadened in very recent times by reason of the great increase in number and diversity of varieties; and this leads us to expect that still other wants will be met in like manner, and that the uttermost habitable parts of the country will develop a special horticulture

2. *There is a constant augmentation in new specific types of plants, both from our native flora and by importation from without.* I suppose that there is no parallel to the marvellous evolution of native fruits in America. Within a century we have procured the grapes, cranberries, the most popular gooseberries, some of the mulberries, the raspberries and blackberries, the pecans and some of the chestnuts, from our wild species. Perhaps the strawberries can be traced to the same source. There are many men still living who remember when there was no commercial cultivation of these fruits. Here is progress enough for one century; yet an overwhelming host of new types is coming upon us. I sometimes think that the improved native plants are coming forward so rapidly that we do not properly appreciate them. Witness the perplexing horde of native plums, the varieties even now reaching nearly 200, which are destined to occupy a much larger area of North America than the European plum now occupies. New species of grapes are now coming into cultivation. The dewberries, juneberry, Crandall currant type, buffalo berry, wild apples, and more than a score of lesser worthies, are now spreading into our gardens. Many of these things will be among the staples a hundred years to come. One hundred and eighty-five species of native plants, some for fruit but mostly for ornament, were introduced into commerce last year; and the number of plants native to North America north of Mexico which have come into cultivation is 2,416. Under the stimulus of new conditions, some of these species will vary into hundreds, perhaps thousands, of new forms, and our horticulture will become the richest in the world. It is a privilege to live when great movements are conceived and new agencies first lend themselves to the dominion of man.

Many species have come to us from many parts of the world throughout the century, but the immigration still continues, and perhaps is greater now than at any previous time. It is well nigh impossible to chronicle the new types of ornamental plants which have come to America during the last two decades. Consider the overwhelming introduction of species of orchids alone. Even the wholly new types of fruits are many. Over twenty-five species of edible plants have come to America comparatively recently from Japan alone, and some of these species are already very important. Two of them, the Japanese persimmons and the Japanese plums, are most signal additions, probably exceeding in value any other introductions of species not heretofore in the country, made during the last quarter-century. During the years 1889, 1890, and 1891, some 380 species of plants not in commercial cultivation here were introduced into North America, partly from abroad and partly from our own flora. In the year 1891 alone 219 distinct species were introduced.

Valuable as these new types are in themselves, all experience teaches that we are to expect better things from their cultivated and variable progeny. We can, therefore, scarcely conceive what riches the future will bring.

3. *There is great progress in methods of caring for plants.* The manner of cultivating and caring for plants has changed much during recent years. It is doubtful if all this change represents actual progress in methods, but it indicates inquiry and growth, and it must eventually bring us to the ideal treatment of plants. Some of the change is simply a see-saw from one method to another, according as our knowledge seems to point more strongly in one direction than another. In one decade we may think lime to be an indispensable fertilizer, and in the next it may be discarded; yet we may eventually find that both positions are un-

tenable. Yet there has been a decided uplift in methods of simple tillage and preparation of land and the science of fertilizing the soil; and, moreover, the application of this knowledge is widespread where it was once local or rare. And the application of machinery and mechanical devices to almost every horticultural labor cannot have escaped the attention of the most careless observer.

Among specific horticultural industries, the recent evolution of the glass-house has been remarkable. In 1806 the green-house was still a place in which to keep plants green, and M'Mahon felt obliged to disapprove of living rooms over it to keep the roof from freezing, because they are "not only an additional and unnecessary expense, but they give the building a heavy appearance." The first American green-house, with a wooden roof and heavy sides, was built in 1764. Glass-houses increased in numbers very slowly until the middle of this century, and they can only now be said to be popular. Twenty years ago a glass-house was a luxury or an enterprise suited only to large concerns, and the management of it was to most intelligent people an impenetrable mystery. At the present time, even the humblest gardener. if he is thrifty, can afford a green-house. In fact, the glass-house is rapidly coming to be an indispensable adjunct to nearly all kinds of progressive gardening. The secret of this increasing popularity of the glass-house is the simplicity of construction of the modern building. Large glass, low, straight roofs, light frames, simple foundations, small wrought-iron pipes, portable automatic heaters — these are the innovations which have given the green-house a greater popularity and practicability in America than anywhere else in the world. Yet many of these features would have been heresies when Leuchars wrote his excellent book in 1850.

The simplification and popularization of the glass-house has simplified the management of plants in them. Even laymen are now taking to green-house plant growing, and many of them achieve most gratifying results. The first days of the commercial forcing of plants are still within the memory of many of this audience; and it is only within the present decade that great attention has been given in this country to the forcing of tomatoes, cucumbers, carnations, and many other plants. The business is yet in its infancy. The green-house has also exerted a marked influence upon the plants which are grown in them. There has now appeared a list of varieties of various plants which are especially adapted to the purposes of forcing; and this phenomenon is probably the most important and cogent known proof of contemporaneous evolution.

If one were asked off-hand what is the most conspicuous recent advancement in horticulture, he would undoubtedly cite the advent of the sprays for destroying insects and fungi. These are not only eminently effective, but they were perfected at a time when dismay had overtaken very many of our horticulturists, and they have inspired new hope everywhere, and have stimulated the planting of fruit and ornamentals. I fancy that the future historian will find that the advent of the spray in the latter part of this century marked an important epoch in agricultural pursuits. Yet this epoch is not disconnected from the era before it. It is but a natural outcome or consequence of the rapid increase of insect and fungous enemies, which increase, in turn, is induced by the many disturbing influences of cultivation itself. When we devise effective means of checking the incursions of our foes, therefore, we are only keeping pace with the initial progress fostered by the origination of new varieties and the quickening commercial life of our time. Yet the era of spraying is none the less a mark of great achievement, and we have not yet seen the good of which it will ultimately prove to be capable. But a greater achievement than this must be made before we shall have reached the ideal and inevitable method of combatting external pests: we must learn to so control natural agencies that one will counteract another. Nature keeps all her forces and agencies in comparative equilibrium by pitting one against another in the remorseless struggle for existence. The introduction of insect parasites and predaceans, entomogenous fungi, colonization of insectivorous birds, and the use of strategy in cultivation and in the selection of immune species and varieties and the planning of rotations and companionships of plants, will eventually be so skilfully managed that most of our enemies will be kept under measurable control. A short rotation is now known to be the best means of combatting wire-worms and several other pests. The first great success in this direction in America is the introduction of the Australian vedalia, or lady-bug, to devour the most pestiferous of the orange-tree scales on the Pacific coast. This experiment is pregnant of greater and more abiding results than all the achievements of the sprays. But in your generation and mine, men must shoulder their squirt-guns as our ancestors shouldered their muskets, and see only the promise of the time when they shall be beaten into pruning-hooks and plough shares and there shall come the place of a silent warfare!

4. *There is great progress in the methods of handling and preserving horticultural products.* I need not tell the older men in this audience that there has been progress in the methods of handling fruits. When they were boys, apples and even peaches were taken to market loose in a wagon-box. We have all seen the development of the special-package industry, beginning first with rough bushel baskets or rude crates, then a better made and smaller package which was to be returned to the consignor, and finally the trim and tasty gift packages of the present day. I am sorry to say that some regions have not yet reached this latter stage of development, but their failure to do so only makes the contrast stronger of those who have reached it. Quick transportation and methods of refrigeration have tied the ends of the earth together. Apples in quantity are carried 14,000 miles from Tasmania to England, and in 1890 they reached the San Francisco markets to compete with the fruits of the Pacific coast. From a small beginning in 1845, the exportation of American apples to England and Scotland began to assume commercial importance from 1875 to 1880, until nearly a million and a half barrels have been exported in a single season. It is said that the first bananas were brought to the United States in 1804, and the first full cargo in 1830. Now from eight to ten million bunches arrive annually. The Canary Islands are now shipping tomatoes to London, and the United States will soon be doing the same. Watermelons will follow. California now unloads her green produce in the same market. Even pears are exported from America to Belgium, disputing the old saw that it is unwise to carry coals to Newcastle. The world is our market. But this result may have been achieved with some detriment to home markets and transportation, which have been in some measure overlooked and neglected; but this evil must correct itself in the long run.

Perhaps we owe to a Frenchman the first distinct exposition, some eighty years ago, of a process of preserving perishable articles in hermetically sealed cans; but the process first gained prominence in the United States, and it became known as canning. In 1825, James Monroe signed patents to Thomas Kensett and Ezra Daggett to cover an improvement in the art of preserving, although Kensett appears to have practised his method somewhat extensively as early as 1819. Isaac Winslow of Maine is supposed to have been the pioneer in canning sweet-corn, in 1842. About 1847 the canning industry began to attract general attention, and in that year the tomato was first canned. The exodus to California in 1849 stimulated the industry by creating a demand for unperishable eatables in compact compass. North America now leads the world in the extent, variety, and excellence of its canned products, and much of the material is the product of orchards and gardens. In 1891, the sweet-corn pack of the United States and Canada was 2,799,453 24-can cases, and the tomato pack was 3,405,365 cases! Over 20,000 canning factories give employment, it is said, to about one million persons during the canning season. The rise of the evaporated fruit industry is not less remarkable in its way than that of the canning industry.

There are other marvels of progress in methods of caring for horticultural products, but these examples sufficiently illustrate my position. I am aware that all these things are features of commerce and manufacture rather than of horticulture, but they are responsible for much of the phenomenal extension of horticultural interests in recent years. They have also exerted a powerful influence upon the plants which we cultivate, and varieties have appeared which are particularly adapted to long carriage and to canning and evaporating. The vegetable kingdom is everywhere responsive to the needs of man.

5. *There is a corresponding evolution in the horticulturist.* The rapidity with which education and general intelligence have spread in recent years is patent to every one. The rural classes have risen with the rest, but among the agricultural pursuits horticulture has probably shown the greatest advance in this respect. The horticulturist grows a great variety of products, many of which are perishable, and all of which demand expedition, neatness, and care in marketing. And these many and various crops bring in a multitude of perplexities which not only demand a ready knowledge for their control, but which are important educators in themselves. The horticulturist lives nearer the markets and the villages than the general farmer, as a rule, and he is more in touch with the world. Downing rejoiced in 1852 that there were "at least a dozen societies in different parts of the Union devoted to the improvement of gardening, and to the dissemination of information on the subject." Since that time a dozen national horticultural societies of various kinds have come into prosperous existence, and there are over fifty societies representing States, provinces, or important geographical districts, while the number of minor societies runs into the hundreds. Over fifty States, Territories, and Provinces have established agricultural schools and experiment stations, all supported by popular sentiment. The derision of "book farming" is well nigh forgotten. Subjects which a few years ago were thought to be "theoretical" and irrelevant are now matters of common conversation. In short, a new type of man is coming onto the farms. This uplift in the common understanding of the science of cultivation, and of the methods of crossing and of skilful selection, is exerting a powerful accelerating influence upon the variation of cultivated plants. But the most important and abiding evolution is that of the man himself; and I expect that the rising intellectual status will ultimately lead people to the farm rather than away from it. We are just now living in a time of conspicuous artificialism; but the farm must be tilled and it must be inviting. When agriculture cannot pay, something is wrong with the times.

These, then, are the chief lines of progress in horticulture, and they are all still operative and capable of indefinite growth. The achievement of a generation has been phenomenal. The prospect is inspiring to both the cultivator and the student.

THE IMPORTANCE OF "NEXT-TO-NOTHING" IN CHEMISTRY.

BY W. H PENDLEBURY, M.A. (OXON), SCIENCE LECTURER OF DOVER COLLEGE, ENGLAND.

In the year 1888 the President of the British Association for the Advancement of Science took for the subject of his inaugural address "The Importance of 'Next-to Nothing.'" As a matter of course, Sir Frederick Bramwell treated his subject with his usual wit and ability, and pointed out the influence of small things on the advancement of his particular branch of science—engineering. It might, however, be well to carry the idea still further and to collect together, as far as is possible in a short paper, the facts that have come to light showing the influence of traces of a foreign substance upon chemical change. Some of the facts are almost paradoxical. Take the case of an ordinary coal fire, which was probably one of the first objects which aroused the interest and curiosity of mankind and awakened the instinct of scientific investigation. It is needless to refer to the erroneous views held on the subject of combustion, but it may just be mentioned that the discovery of oxygen seemed to settle the matter and to establish on a firm basis the whole theory of combustion. In the years 1887 and 1888[1] the experiments of Mr. H B. Baker made it quite clear, however, that the presence of aqueous vapor had a great deal more to do with combustion and hence the burning of an ordinary coal fire than we were aware of. He showed that if oxygen be rendered perfectly dry, by leaving it for some time in contact with phosphorus pentoxide, combustion is rendered impossible in such gas. Carbon, sulphur, or phosphorus may be strongly heated in an atmosphere of perfectly dry oxygen without taking fire, and, in fact, the sulphur and phosphorus may be distilled in it. The presence of a trace of moisture at once brings about the combustion. The writer has seen Mr. Baker distil phosphorus in an atmosphere of oxygen and then, whilst the phosphorus was still melted, admit a bubble of oxygen which has been standing over water and at once the phosphorus burst into flame. Hence it is highly probable that the ordinary phenomena of combustion could not take place in our atmosphere if there was not aqueous vapor also present. This would furnish another reason against the probability of the moon's being inhabited, as owing to the absence of aqueous vapor fire would not be possible there.

The great influence of a trace of moisture in bringing about chemical changes in which of itself it is not directly concerned, if we may so express it, is evident from many other observations. Wanklyn discovered that dry chlorine will not combine with dry metallic sodium, but that a trace of moisture will start the reaction. Dixon found that a mixture of carbon monoxide and dry oxygen will not be exploded by the electric spark, but that the presence of a trace of moisture will bring about a silent combination under the influence of the spark, whilst if the gases are moist, the explosion will take place readily.

Again, it has been recently observed that ethylene and oxygen, when perfectly dry, do not explode when acted upon by the electric spark, but the presence of moisture acts in this case as in the former.

Again, carbon dioxide is not absorbed by dry lime. Sulphuretted hydrogen in the dry condition does not tarnish dry silver. Dry iodine does not decompose dry sulphuretted hydrogen.

We may take another example of the influence of next-to-nothing of an impurity in bringing about a change in which its influence had been till lately little regarded. The experiments of Mr. V. H. Veley[2], of Oxford University Museum, on the action of nitric acid on various metals has conclusively shown that the violent action which nitric acid has upon many metals is due to the presence of a trace of nitrous acid in the nitric. He has kept spheres of copper in the strongest nitric acid (freed from the presence of nitrous acid) for some time without any reaction occurring, but when once a trace of nitrous acid or of any nitrite was added, the copper was at once dissolved. The same kind of result was observed when mercury, silver, or bismuth were exchanged for the copper. It was found that from 1 to 2 parts of nitrous acid in 10,000 of the nitric were sufficient to set up the reaction.

Mr. Cross found that jute fibre, when treated with sulphuric acid, is simply hydrolysed. If, however, ordinary nitric acid, containing a trace of nitrous acid, be allowed to act on the jute, a considerable amount of chemical action takes place, and amongst other substances, like urea, which either prevents the formation of nitrous acid or decomposes it as quickly as it is formed, the action of nitric acid on jute is strictly comparable with that of sulphuric acid, simple hydrolysis taking place.

It is highly probable that many of the changes in organic chemistry, generally ascribed to the action of nitric acid alone, are due to the presence of traces of nitrous acid.

It is well known that pure zinc will not dissolve in pure hydrochloric acid or pure sulphuric acid, but the presence of a trace of a metallic salt sets up the reaction very readily.

If we take another branch of chemistry—metallurgical chemistry—the immense importance of the presence or absence of a trace of a foreign substance in a metal is readily seen, since it produces an immediate effect on the hardness or tenacity of the metal, and so may destroy its usefulness in commerce. Take the case of copper. Professor Roberts-Austen states in his Cantor lectures that a cable made of the pure copper of to-day will carry twice as many messages as a similar cable made of the less pure copper of 35 years ago, when the importance of the purity of copper was not so well understood, and he quotes a saying of Sir Wm. Thomson's that the presence of $\frac{1}{10}$ per cent of bismuth in the copper of a cable would entirely destroy its commercial success by reducing its conductivity. Sir Hussey Vivian has

[1] Preceedings of the Royal Society, vol 45, and Phil. Trans., 1889

[2] Philosophical Transactions, 1891.

stated that $\frac{1}{1000}$ part of antimony will convert the best select copper into the worst conceivable. Another instance occurs in the case of iron. By the addition of $\frac{2}{10}$ per cent of carbon steel is produced of such a kind as would make an excellent bridge, or boiler plate, but if fashioned into a weapon would be absolutely untrustworthy. If, on the other hand, $\frac{8}{10}$ per cent of carbon were introduced, a material is obtained from which a good razor might be made, but it would be useless for a rail or the construction of a bridge. A trace of manganese in steel renders it impossible to make a magnet out of such a specimen. It also prevents the hardening of such steel by rapid cooling after heating to redness.

The metal, however, which shows the most remarkable change in its physical properties when contaminated with next-to-nothing of a foreign substance is gold. The addition of $\frac{2}{10}$ per cent of bismuth would render a specimen of gold useless for coinage purposes, as it would crumble to powder under the pressure of the die. Lead acts in a similar way. One part of lead added to two thousand parts of gold reduces its tenacity from 18 tons per square inch to only 5 tons. A bar of such gold can be readily broken by a tap from a hammer. The color of the gold is changed from yellow to orange brown. Such a remarkable change in the appearance and properties of gold on the addition of small quantities of other substances was known in the seventh century and helped to confirm the belief of the alchemists that they had only to find some substance which would alter the properties and appearance of any given metal so that it would change into and acquire the properties of gold. Hence the search for the philosopher's stone.

This paper might be indefinitely extended, but enough has probably been said to show that even in chemistry the day of small things is not to be despised, and that a thorough investigation of some of the commonest and best-known chemical changes would doubtless bring to light many facts at present overlooked, and would tend to a better understanding of the workings of nature.

BREAD-FRUIT TREES IN NORTH AMERICA.

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THE living species of the genus Artocarpus are exclusively Old World, being confined in their distribution to tropical Asia and the Malay Archipelago. About forty species have been described, of which number two or three are now widely cultivated throughout the tropics, the most important of these being *A. incisa*, the true bread-fruit tree. They are small or medium-sized trees with a milky juice, and large, leathery, entire, or pinnately lobed, or rarely pinnately compound leaves. The flowers are monœcious with the staminate ones borne in long club-shaped spikes, and the pistillate in rounded heads. The female flowers soon grow together and form one large, fleshy mass, or the so-called bread-fruit. When mature, the fruit is marked on the exterior with hexagonal knobs, and in the interior consists of a whitish pulp, having the consistence of new bread, whence its name.

Although not at present an element in the flora of the New World, there is now abundant evidence to show that the genus Artocarpus was, during late Cretaceous and earlier Tertiary times, an inhabitant of North America. The best known species, called *Artocarpus lessigiana* (Lx.), was discovered in 1874 in the Lower Laramie on Coal Creek, in Boulder County, Colorado. It was first described by the late Professor Leo Lesquereux, under the name of *Myrica ? lessigiana*, on the supposition that it was a gigantic representative of the genus Myrica. Specimens, now known to represent the upper portions of large leaves, were later obtained from the andisitic deposits forming the recently differentiated Denver formation of South Table Mountain, near Golden, Colorado. These leaves were called *Aralia pungens* by Professor Lesquereux, who naturally confounded the imperfect examples at his disposal with well known fossil forms of this genus, which they much resemble. Since that time several additional specimens have been obtained, which not only prove that *Myrica ? lessigiana* and *Aralia pungens* are identical, but also that they should be referred to Artocarpus.

The leaves of *Artocarpus lessigiana* were very large, measuring 30 centimeters in length and 18 or 20 centimeters in width. They are thick, probably coriacious in texture, broadly oblong in general outline, and deeply, pinnately 4–6-lobed. The lobes are oblong, lanceolate, taper-pointed, and separated at the base by broad, rounded sinuses, the lobation being most extensive at the base of the leaf, where the sinus almost reaches the midrib, and the two lower lobes are connected by a narrow ring only. The nervation of the leaf is very strong, and precisely like that of the living *A. incisa*, which differs from the fossil in having the deepest lobation in the upper part of the leaf.

Closely allied to this species, and possibly identical with it, is what I propose to call *Artocarpus californica*, which is founded upon specimens obtained by Dr. Cooper Curtice, then of the U. S. Geological Survey, from the auriferous gravels at Independence Hill, Placer County, California. This species differs from the former by its smaller size, thinner texture, and shorter, more acute, lobes. It is not sufficiently well preserved to show the finer nervation, but, as far as can be made out, it is very similar to *A. lessigiana*, and additional material may show them to be the same.

Specimens, probably belonging to this species (*A. californica*), were obtained some years ago from the John Day Valley in Oregon, the age of which is either Upper Miocene or Lower Pliocene. They were identified by Professor Lesquereux both with his *Myrica ? lessigiana* and *Aralia pungens*; but, as they are somewhat fragmentary, it is not possible to be positive as to their correct determination.

The most northern point at which the genus Artocarpus has been found fossil is northern Greenland, in latitude 70°. Dr. A. S. Nathorst obtained a large leaf, which he named *A. dicksoni*, in the Cenomanian near Waigatt. This species is also closely related to *A. incisa*, and was associated with a fruit which is unquestionably that of a bread-fruit tree. Nathorst, who was the first to point out the true relationship of Lesquereux's *Myrica ? lessigiana* and *Aralia pungens*, suggests the possibility of their being the descendants of the Greenland species, which may have deen dispersed over the North American continent by the ice-sheet. The material at present available is hardly sufficient to establish unquestioned relationship between them, for the nervation of *A. dicksoni* is not to be made out, but, as all are undoubtedly related to the living bread-fruit (*A. incisa*), they may be more closely related among themselves than now seems apparent.

From the above account, it appears that the bread-fruit trees existed in North America as far north (in Oregon) as 46°, and as late as early Pliocene or late Miocene time. The reason for their complete disappearance from the American flora, and that within such a comparatively short space of time, is difficult to supply. If they had been pushed southward, and now inhabited the tropics, it would be readily explainable, and quite in accord with other well-known instances, but they have totally disappeared from the New World, notwithstanding the fact that they grow when transplanted as freely in tropical America as in their native country. It is probable that the advance of the refrigeration was so rapid that they were unable to escape in the New World, and perished to the last one, while in the Old World some avenue permitted their perpetuation. The genus Eucalyptus is another example of the same condition. During Cretaceous and Tertiary times it was an inhabitant of North America and Greenland, but is now entirely confined to Australia.

The deductions to be drawn, as to the climate that prevailed at the time when these trees existed in North America, are to be made with caution. The fact that all the living species of a genus are tropical does not necessarily prove that it has always been so. Again, a genus that is essentially tropical may have species extending into sub-tropical or even temperate regions. The genus Dicksonia is a marked example of this kind. It is principally an inhabitant of tropical America and Polynesia, but one species reaches as far north as Canada, and several are scattered throughout the southern part of the temperate zone.

Taken by itself, Artocarpus would indicate a tropical climate, but the plants with which it is associated have also great weight in confirming or modifying this view. In Greenland it is associated with ferns of the order Gleicheniale̱æ and at least four species of Cycas, all of which goes to prove that the climate at the time they grew was probably tropical, or at least very warm. In North America the Laramie bread-fruit tree was associated with an abundance of palms, which also argue a warm climate, but in the same beds are found a host of genera (Salix, Populus, Quercus, Juglans, Carga, Magnolia, Ginkgo, Taxodium, Sequoia, etc.), which point with stronger force to a probably temperate climate. The Pacific coast species was found with genera usually relied upon to prove a temperate climate, and while it was undoubtedly warmer than now, for the present forest vegetation is mainly or largely coniferous, there is little beside this to show that it was actually tropical.

NOTES ON MARS AND METEORS.

BY E. MILLER, LAWRENCE, KANSAS.

THE recent opposition of Mars, the appearance of Holmes's comet, and the meteoric display of the night of Nov. 23, 1892, were events that concentrated the attention not only of the general scientific world, but of specialists also, more largely than such events ever did before. It was thought that some of the celestial riddles were about to be solved, that some positive addition, neither nebulous nor fragmentary in its character, was to be made. Now, that they have all become things of the past, and it becomes possible to sum up the results of all the labor performed, theories propounded and exploded, and computations made, it is no wonder that the "οἱ πολλοι" ever impatient to see tangible results, and always clamorous in demanding large returns for even the smallest expenditures of time, labor, and money, are shouting "imposture." But science is not to be balked in this way; there is no release from this war.

The position of Mars relatively to the earth was such during the recent opposition that the best instruments and the best observers were at a great disadvantage. The results were not altogether satisfactory and in many cases were at variance with old theories and with each other. The observations made in this country, east of the Rocky Mountains, were scarcely of any value at all in the most of them, owing to the hazy condition of the atmosphere, as well as the low altitude of Mars. But west of the Rocky Mountains, especially along the Pacific coast, notably at Lick Observatory and the mountain observatory, near Arequipa, Peru, the conditions were the best attainable. At Guaymas, Mexico, on the coast of the Gulf of California, in latitude 27° 30′ N., the writer, about the middle of August, 1892, was impressed with the splendid appearance of Mars. The planet shone with a brilliancy that was almost, if not altogether, as great as it was at the opposition of 1877. Venus and Jupiter, also, seemed to have received extra touches of brilliancy that generally are not so pronounced in latitude 39° N.

Guaymas, located as it is on the shore of the Gulf of California, and surrounded by mountains ranging from a thousand to two thousand feet in height, with a sky that is always of the deepest blue, possesses advantages of a very superior kind, for an astronomical observatory. The great objection to such a location, to a northerner, would be the intense heat of the summer. In addition to the advantages for astronomical work, the harbor of Guaymas, as well as the Gulf itself, offers facilities and material for the study of marine life, that are beyond a doubt unsurpassed. A well-equipped biological station and some good biologists would soon furnish to the scientific world splendid results.

At midnight of August 18, 1892, as the writer was entering the open court of a large adobe house in Guaymas, in company with two or three friends, one of the most beautiful of celestial sights greeted their astonished vision. Suddenly from blue concave of the heavens, about midway between the zenith and the pole star, a meteor of the largest size shot out with a splendor of color such as is not often seen. The orange, red, violet, and other colors, were deep and most handsome to behold. Apparently, the meteor seemed to be moving from its initial point in a southerly direction, and had a disc, so to speak, almost equal to that of the full moon, and a train following that was remarkable for its width as well as its length. The train was broken into blocks of color that made this celestial visitant in all its outline, size, color, and general appearance, an intensely interesting object.

The stream of meteors, called the Andromedes, which our planet encountered on the 23d of November, made a very good display here in Kansas. Although no attempt was made to count the number or estimate the total fall of meteors during the night, except at intervals of five or ten minutes, yet judging from what was done in this discontinuous manner, there must have been an average of from sixty to one hundred meteors per minute from 9 to 11 P.M. The "radiant point" was in Andromeda, from which by far the greater number seemed to start. Many others, apparently, had no connection with the "radiant," for they shot out from other points of the sky and at every moment. Generally, the meteors were small, but at times one more brilliant than the others appeared, adding very much to the interest of the observer. During the next four nights following the night of the 23d, it was hoped that a finer display would make its appearance, but two of the nights were overcast with clouds, and the other two, although clear, offered no show.

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.

Breathing Wells.

REFERRING to the article of Mr. J. T. Willard in *Science* for Dec. 16, with reference to a "breathing well" in Kansas, I would say that such wells are also common in Nebraska. I have compared their behavior with the fluctuations of the barometer, and my observations agree with those of the writer in showing the entire dependence of the air movements in the well upon the changes in the atmospheric pressure outside. The cessation of an outward current from the well always corresponds with a curve of barometric depression, but always occurs some hours later than the minimum of pressure, and the amount of retardation depends upon the slowness with which the barometer rises.

These wells have often given considerable trouble in cold weather as the influx of cold air is liable to freeze the water in the pump at a considerable depth below the surface of the ground. GOODWIN D. SWEZEY.

Crete, Neb., Dec. 23.

Hybridism in Genus Colaptes.

ACCORDING to the *résumé* of hybridism in the genus Colaptes by Professor Rhoads in *Science* for Dec. 9, it would appear that King's River was out of the limit of variation. Still I found one adult male of *C. cafer* at Dunlap, Cal., in the Sierra Nevadas, about 3,500 feet elevation, a perfect representative in every way save the occipital mark of *C. auratus*.

I also found an adult male in Cantua Creek, in the coast range, with the same marking. The former was in January, 1891, the latter in April, 1892. As both of these were found dead, I thought possibly the marking might have been caused by old age; but more probably they were stragglers from the north.

ALVAH A. EATON.

Riverdale, Cal., Dec. 26.

How Shall I Study Ants?

CAN some naturalist refer me to some article or book, or tell me himself how I can best keep a colony of ants, for inspection.

DWIGHT GODDARD.

Hosmer Hall, Hartford, Conn., Jan. 6.

BOOK-REVIEWS.

Waterdale Researches; or, Fresh Light on Dynamic Action. By "WATERDALE." London, Chapman & Hall, 1892. 12mo. pp. vi., 293.

Cosmical Evolution: A New Theory of the Mechanism of Nature. By E. McLENNAN. Chicago, Donahue, Hennenberry, & Co., 1890. 12mo. 399 p. $2.

In these volumes we have interesting illustrations of those methods of thought, and their results, which are characteristic of the attempts of amateurs in science to bring contributions of new thought and novel theories to the attention of scientific men. In the first-named, the anonymous author, writing under the *nom de plume* "Waterdale," presents his "discovery of a cause for gravity other than the hypothesis of attraction," and "other theorems as important." That an amateur should, especially in physical science, have the courage to propose to lead the connoisseur in the serious consideration of presumably crude notions—in these days of higher research, when even the professional expert finds himself entirely at a loss to find a way, even in following the specialist in other lines than his own, and entirely unable to propose original theories — speaks well for the confidence, if not for the discretion, of the ingenuous adventurer. We regret to say that we must coincide with the reviewer in *Nature* and the critic in *Science and Art*, who are apparently unable to find anything novel in what is right in the book, or anything right in what is novel. The idea that some other explanation of the action of forces on matter than that provisionally held, that of an inherent attractive "action at a distance," is as old as Greek philosophy, and remains, no doubt, an admitted probability among the best thinkers and most expert physicists and chemists of the time; but our author and Sir Isaac Newton are alike in the dark as to the real nature of the action noted. The proposed substitution of another term for the well-understood and precisely-defined word *mass*, certainly affords no aid to either imagination or experience.

The author introduces his book into the United States "in the hope that there is there less clique prejudice among scientists than in England;" but we fear that, here as in Europe, the prejudice that the man who has made a life-work of the study of a subject and has acquired reputation through actual investigation and systematic research, through exact and productive measurement, is competent to act as the adviser of the laymen, and that the amateur with an unscientific imagination, unfamiliar even with the precision of scientific definition, can claim little consideration when thus out of his element, will be found unconquerable. This book is written in such vague and ildefined language that its assertion that it presents "substantial evidence that energy pervades the ethereal fluid with which every sphere is surrounded" will hardly be taken as substantiated, however well established the fact may be; and its "law of induction" that "every substance, by exchange during pulsation of fine matter internally from one atom to another, sets up increased hydraulic force with fine matter, which force decreases inversely as the square of the distance through which the force has at any point reached" will hardly displace Newton's laws. Its author is not yet a sufficiently advanced student to be prepared to teach.

Of Mr. McLennan's book, it may at least be said that, although the author is an amateur in that lofty region of scientific philosophy into which he endeavors to find entrance, and has as yet never earned that right of prophecy which only comes to the man who becomes known as thoroughly familiar with existing human knowledge and the grandest of modern achievements, and who himself has done his part in promoting positive learning, he has certainly collated numerous facts of real interest and of possible, if not probable, importance in the relations to which he seeks to attach them. But his supposed original matter seems based upon imagination rather than ascertained fact; and we can find little

CALENDAR OF SOCIETIES.

Anthropological Society, Washington.

Dec. 20.—Symposium, Is Simplified Spelling Feasible? Discussion by F. A. March, A. R. Spofford, Wm. T. Harris, and Edwin Willits.

Dec. 27.—Continuation of Symposium, Is Simplified Spelling Feasible? Discussion by Alexander Melville Bell, E. M. Gallaudet, John M. Gregory, Benj. E. Smith, Charles R. G. Scott, and W. B. Owen.

Jan. 3.—Close of the Symposium, Is Simplified Spelling Feasible as Proposed by the English and American Philological Societies? Discussion by Lester F. Ward, Wm. B. Powell, Benj. E. Smith, Charles R. G. Scott, E. T. Peters, John W. Powell, and Weston Flint. The discussion will be closed by A. R. Spofford and Wm. T. Harris.

Philosophical Society, Washington.

Jan. 7.—G. K. Gilbert, Illustrations of the Physical History of the Moon (lantern slides); T. C. Mendenhall, The Use of Planes and Knife-Edges in Pendulums.

Appalachian Mountain Club, Boston.

Jan. 11.—Warren Upham, Drumlins Near Boston.

connection between the undeniably interesting facts related and that "material connection" between the bodies of our universe, which he claims to have discovered. Whatever may be the real nature of that connection — and we doubt if our author has hit upon it — these facts will, unquestionably, be found perfectly consistent with it, and a part of it; but a thousand other schemes than this may be produced by the poetic imagination of the amateur in science into which these facts may be also worked, and it remains, most likely, for direct investigation, with all the aid of the most perfect modern apparatus and methods, to finally determine solutions of the still numerous problems of contemporary science. The Greek methods of speculation and non-scientific imagination are not of much promise where a "material connection" between the bodies of the solar and other systems of the universe is the subject-matter of investigation. The machinery of the universe must probably be ultimately revealed by expert and practised mechanicians.

Dynamics of Rotation. By A. M. WORTHINGTON. London, Longmans & Co. 1892. 155 p. 12°.

A LITTLE book on a very elementary portion of the science of mechanics, as here treated, but an excellent treatise for beginners. Professor Worthington has made his process of instruction a most practical and sensible one — giving first a statement of the facts and data as developed by experiment and then deducing the laws of mechanics applying to the case and finally applying those laws and the equations expressing them to the solution of problems. Such applications are well illustrated by considerable numbers of well-chosen examples. This method of treatment is certainly well suited to the instruction of young students, and we are not sure that it is not the best for older ones in many cases in which the opposite course of enunciating the law and later illustrating it and deducing constants by experiment. We observe that the new term, "torque," is accepted by the author and that he also adopts the "poundal" and the conventional distinction pound for force and lb. for mass. We are not sure that either is needed or desirable; but fashion and convention have almost as much influence in science as in *modes de Paris*. They have probably come to stay, like the barbarous nomenclature of the electricians; but, in this book, the frequent use of the "engineers, or gravitation" units, as its author calls them, will go far toward relieving the mind of its readers of those misapprehensions and confusions which so constantly arise in the study of the older text-books.

Mechanical Drawing. By C. W. MCCORD, A.M., Sc.D. New York, J. Wiley & Sons. 245 p. 4°.

THIS large and handsomely made book contains the line of work proposed for use in the elementary instruction of the technical schools, especially those of engineering. The exercises given are those which have proved successful, during twenty years of work, by its author. They are intended to train eye, hand, and judgment as well. "The artificial and often useless stage machinery of descriptive geometry" is kept out of sight as far as possible, although they are not considered entirely useless, nevertheless. Maxims, bits of condensed wisdom, are sprinkled throughout the work, as "Pencil lightly," "Pencil clearly," "Make haste slowly," and are clearly themselves the result of long experience and a fruitful observation. The methods are excellent, the manner of doing the work no less satisfactory; and the whole constitutes one of those rare treatises on a technical subject which can only be produced by an author who is wise in the principles of his craft and experienced, practically, in their application to the actual, live problems of the profession in which he is an expert. The principles of projection, the laying-out of curves, and the construction of problems in connection with the design and adaptation of gearing to its work, illustrate especially this advantage possessed by the author in the present case. This is an admirable work, and author and publishers are alike entitled to great credit.

Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

The undersigned has the following specimens to exchange for crystals of any eastern or foreign localities or Indian relics: tin ore, metacinnabarite, stibnite, garnierite, calenanite, hanksite, ulexite, rubellite, lepidolite, blue and green onyx, Cal. pineite, aragonite on chalcedony, cinnabar, double refracting spar, clear and clouded, and others. J. R. Bush, care of General Delivery, Los Angeles, Cal.

For sale or exchange.—A private cabinet of about 200 species of fossils, well distributed geologically and geographically. Silurian, about 40; Devonian, about 50; Carboniferous, about 80; others, about 30. Frank S. Aby, State University, Iowa City, Ia.

For exchange.—Minerals, fossils, F. W. shells, land shells, native woods, Indian relics, two vols. of Smithsonian reports, odd numbers of scientific magazines, copper cents, etc., for good minerals not in my collection, good arrow- and spear-heads and natural history specimens of all kinds. Correspondence solicited with list of duplicates. G. E. Wells, Manhattan, Kan.

For sale or suitable exchange.—A spectrometer made by Fauth & Co., Washington, D. C., according to the plan of Prof. C. A. Young. This instrument is suitable for the most advanced investigations and determinations. Cost originally $700 and has been used but little. Will be disposed of at a considerable reduction. Address Department of Physics, Ohio University, Athens, O.

I will send British land and fresh-water shells in return for those of America, any part, sent to me. I have at present about fifty or sixty species, with many varieties. W. A. Gain, Tuxford, Newark, England.

The Biological Department of Hamline University desires to offer microscopic slides of animal tissues, or whole animals, in exchange for first-class fossils. Address correspondence to Henry L. Osborne, Hamline University, Hamline, Minn.

Wants.

WANTED.—American Journal of Conchology, seven volumes. Parties having these for sale will please address the undersigned, stating condition and price. R. Ellsworth Call, Louisville, Ky.

A GRADUATE ENGINEER will give instruction evenings in geometry, trigonometry and surveying, mechanics, physics, mechanical drawing and general engineering construction. Five years' experience in field and editorial work on engineering journal. References furnished. C. S. H., 102 Tribune Building, New York.

A POSITION is desired in the South, preferably the Gulf States, where I can teach the sciences Can also instruct in other branches. Salary only nominal, as I am simply desirous of employment while spending the winter in the South. A private family preferred, but will accept regular school work if not too confining. MORRIS GIBBS, M.D., Kalamazoo, Mich.

WANTED.—By well-qualified and experienced science master and associate of the Royal School of Mines, London, aged 26 (at present in England), a mastership in technical college or university for any of the following subjects: Engineering sciences, geology and mineralogy, physics, chemistry and metallurgy, etc., etc. Can provide excellent references and credentials. Apply, J. G., 17 Sussex St., Rochdale, England.

A GRADUATE of the University of Pennsylvania and a practical mineralogist of twenty years' experience desires to give his services and a cabinet of 25,000 specimens, all named, with about the same number of duplicates, in minerals, crystals, rocks, gems, fossils, shells, archæological and ethnological specimens and woods to any institution desiring a fine outfit for study. The owner will increase the cabinet to 50,000 specimens in two years and will act as curator. Correspondence solicited from any scientific institution. J. W. Hortter, M.D., Ph.D., San Francisco, Cal., General P. O. Delivery.

SCIENCE

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SCIENCE

NEW YORK, JANUARY 20, 1893.

GRAVEL MAN AND PALÆOLITHIC CULTURE; A PRELIMINARY WORD.

BY W. H. HOLMES, SMITHSONIAN INSTITUTION, WASHINGTON, D.C.

THE theory of a palæolithic man in eastern America has been before the archæological world for a number of years and much has been said *pro* and *con*. A large body of evidence, believed by advocates of the theory to be satisfactory and conclusive, has been collected and published, and theories, borrowed and evolved, have been promulgated, discussed, and modified until the literature of the subject has grown to imposing volume.

It should be observed, however, that the term "palæolithic" does not fully cover the ground. The subject is not a simple one. Two important questions are involved, and these should, for the sake of clearness, be treated separately. These questions are, first, Is there evidence of a glacial man in eastern America? and second, Is there evidence of a palæolithic or primal stage or period of culture? Although closely related in some respects, these questions are, in the main, independent of one another. The existence of an ice-age man may be proved without securing the least evidence of the existence of a palæolithic period, the latter expression implying a primal and well differentiated stage or period of art in stone. It is possible to collect a large body of objects of art from a given formation without being able to make any deductions whatsoever as to the particular stage of culture represented, since certain types of artificial products necessarily appear at all periods from the beginning to the end of the stone age, making hasty conclusions unsafe. On many sites representing middle neolithic culture of modern date countless numbers of flaked stones may be collected without the discovery of a single specimen that the advocates of a palæolithic man would not, three years ago, have called palæolithic. Practically the same conditions will no doubt be found to prevail on Aztec and Maya quarry sites, representing the most advanced stone-age culture.

On the other hand, the existence of a palæolithic, or primal stage of culture, if such there was in this country, may be proved independently of glacial gravel finds, for it is possible that such a stage of art may have existed before, during, or after the gravel-forming epochs. The proofs would be found in pre-glacial or post-glacial formations or upon inhabited sites of any period furnishing the necessary data; but demonstration is not easy in any case, as it is necessary in each instance to show that the art recovered is actually palæolithic art and not merely a partial representation of neolithic art — of the ruder tools or the rejects of an advanced people. The burden of proof rests with advocates of the theory, since they assume to introduce to the world cultures, peoples, and conditions not within the limits of ordinary experience.

It will be seen that in the discussion of these questions two distinct classes of testimony are involved, one dealing with the phenomena of human handicraft, and the other with the phenomena of geologic formations. These phenomena are complex and their relations obscure and subtle in a high degree, and it would appear that until students of the great questions of chronology and culture acquire a thorough scientific knowledge of geology as well as of all early phases of human art the discussions in which they indulge can be of little real value.

The fact is that the field has, up to this time, been occupied mainly by amateurs who have not mastered the necessary fundamental branches of science. The work done is mainly their work, the literature produced is mainly their literature, and the world has received its impressions from this source. This no doubt is an unavoidable condition of the evolution of archæologic science. It is necessary that all departments of investigation should pass through this novitiate or formative stage and the world of science must look with lenience upon the mistakes of the period, for that which is to-day or may be to-morrow is in great part the outcome of that which was yesterday. But the time has now come for a change — for the opening of an era when scientific acquirements of the highest possible order shall be brought to bear upon these questions. Anthropologists are now to unite with geologists in investigating the early history of man and his culture, just as the geologist has been for years assisting the biologist in unfolding the history of living things.

The requirements of the investigation may be briefly outlined and the present status of the evidence characterized. In the first place, the discussion of the early history of man requires a scientific knowledge of certain phases of art, including especially all flaked-stone art. Until very recently the origin, genesis, and history of artificially flaked stones have been but imperfectly understood. Those forms not properly designated implements were not separated from those properly so called, although it is found that the former probably greatly outnumber the latter, and as long as all were indiscriminately treated as implements their discussion was little more than a farce. The discussion of flaked-stone art in America has consisted mainly in describing and illustrating unfinished forms and rejects of manufacture as implements and in speculating on their possible age, functions and ethnic bearings. This fundamental misconception as to the nature of a large portion of flaked stones has led to most deplorable mistakes in interpretation, and erroneous theories of age and culture have been hatched and fed and still feed upon these primary blunders. The whole discussion of early man has been so surcharged with misconceptions of fact and errors of interpretation that all is vitiated as a stream with impurities about its source. Until an exhaustive scientific study of the origin, form, genesis, and meaning of all the handiwork of man made use of in the discussion is completed, the discussion of man and culture is worse than useless, and speculation can lead but to embarrassment and disaster.

The geologic aspects of the case are hardly more satisfactory than are the anthropologic. In the discussion of the early history and chronology of man and his arts geology must play a prominent part. Two questions, for whose answers we must appeal to this science, are constantly arising, first, What is the age, or relative age, of the formations concerned in human chronology? and, second, What is the exact nature of the association of works of art with these formations. It is readily seen from the nature of these questions that they require expert answers, but persons unskilled in geologic science cannot be expected to give expert answers. Those persons who have turned their attention to these studies have not, as a rule, been competent to determine the age or relative age of the sedimentary deposits, and they were equally incompetent to determine, in obscure cases, the exact relations of associated works of art with these formations, although constantly essaying to do so. I have for many years been engaged more or less fully in geologic work, but so obscure are the phenomena of the glacial and post-glacial formations, that I do not permit myself to make and use any observation in which these phenomena are seriously involved without consulting one or more geologists of the highest standing in that particular field. There are so many chances for error in observation and so many pit-falls for the unwary theorist, that it may well be questioned whether or not the student of archæology not highly skilled in geologic science can be justified in seeking unaided to enlighten the world upon these questions.

The fact is that a large part of the literature relating to the palæolithic and ice-age questions is so hopelessly embarrassed with the blunders and misconceptions belonging naturally to the initial stages of the investigation that it is but little more than a stumbling-block to science, notwithstanding the possibility that there may be many hints of truth in what has been written. It would appear to be a more collossal task, however, to discover these hints of truth in the literature of to-day than to wrench them afresh from the rocky tomes of nature.

In conclusion, I would add that if there was, as is claimed, an ice-age man or at any time a palæolithic man in eastern America, the evidence so far collected in support of these propositions is so unsatisfactory and in such a state of utter chaos that the investigation must practically begin anew. That it will begin anew is rendered practically certain by the facts that geologists are now showing a decided disposition to take up that part of the work naturally belonging to them; and that primitive forms of art in stone are now for the first time receiving the critical attention necessary to make them available in a scientific discussion. Thus it appears that the suggestion embodied in the title of this communication may not be wholly unwarranted and inappropriate.

THE NEST OF THE TRAP-DOOR SPIDER.

BY D. CLEVELAND, SAN DIEGO, CALIFORNIA.

The trap-door spider (*Mygale henzii* Girard) is widely diffused in California. While wandering over the Mesa (table lands), just back of this city a few months ago I was struck by the great number of their nests in favorable localities. In the adobe land hillocks are numerous; in fact, in many places, they are as thick as the ground will permit. They are about a foot in height, and some three or four feet in diameter. These hillocks, which are an interesting formation in themselves, are selected by the spiders, apparently, because they afford excellent drainage, and cannot be washed away by the winter rains. Their stony summits are often as full of spiders' nests as they well can hold. These subterranean dwellings are shafts sunk vertically in the earth, except where some stony obstruction compels the miner to deflect from a downward course. These shafts are from five to twelve inches in depth, and from one-half to one and a half inches in diameter, depending largely upon the age and size of the spider.

When the spider has decided upon a location, which is always in clay, adobe or stiff soil, he excavates the shaft by means of the sharp horns at the end of his mandibles, which are his pick and shovel and mining tools. The earth is held between the mandibles and carried to the surface. When the shaft is of the required size, the spider smooths and glazes the wall with a fluid which is secreted by herself. Then the whole shaft is covered with a silken paper lining, spun from the animal's spinnarets.

The door at the top of the shaft is made of several alternate layers of silk and earth, and is supplied with an elastic and ingenious hinge, and fits closely in a groove around the rim of the tube. This door simulates the surface on which it lies, and is distinguishable from it only by a careful scrutiny. The clever spider even glues earth and bits of small plants on the upper side of his trap-door, thus making it closely resemble the surrounding surface.

The spider generally stations itself at the bottom of the tube. When, by tapping on the door, or by other means, a gentle vibration is caused, the spider runs to the top of his nest, raises the lid, and looks out and reconnoitres. If a small creature is seen, it is seized and devoured. If the invader is more formidable, the door is quickly closed, seized and held down by the spider, so that much force is required to pry it open. Then, with the intruder looking down upon him, the spider drops to the bottom of his shaft.

A young friend of mine has spent much time lately watching and investigating the operations of this spider. He found by many experiments—all with the same result—that when the door of his nest is removed, the spider can renew it five times—never more than that. Within these limitations, the door torn off in the evening was found replaced by a new one in the morning. Each successive renewal showed, however, a greater proportion of earth, and a smaller proportion of silk, until, finally, the fifth door had barely enough silk to hold the earth together. The sixth attempt, if made, was a failure, because the spinnarets had exhausted their supply of the web fluid. When the poor persecuted spider finds his domicil thus open and defenceless, he is compelled to leave it, and wait until his stock of web fluid is renewed.

From forty to fifty cream-colored spiderlings are hatched from the yellow eggs at the bottom of the nest. When these have attained only a fraction of their full size—before they are half grown—their affectionate mother drives them out into the world to shift for themselves. After a brief period of uncertainty, they begin active life by making nests, each for itself, generally close to "the old homestead," sometimes within a few inches of it. These nests are always shallow and slender, and are soon outgrown. When the spider attains its full size it constructs a larger nest.

The spider is seldom seen outside of its nest, which it rarely leaves—during the day, at least, and then only for a few minutes, and for a short distance. Upon any alarm, it hastens to its nest, lifts the door, which quickly springs back into its place, and is held down by the householder until the alarm has subsided.

I now have a large nest, containing a mother and her yellow spiderlings, which I am carefully watching and studying.

BALANCES OF THE PERUVIANS AND MEXICANS.

BY WALTER HOUGH, WASHINGTON, D.C.

THE employment of weights and measures among the existing uncultivated peoples is a subject upon which but little information has been gathered. The following instances of the use of balances and weights in pre-Columbian America are interesting from an archæological point of view.

In the Archæological Museum of Madrid there are two pairs of balances and four beams, from sepulchres of the Yncas at Pachacamac, Peru. The possession of this probably oldest weighing appliance by the ancient Peruvians is very curious. A flat strip of bone suspended edgewise by a cord midway forms the beam. To the ends of the beam are hung, by short cords, slings of net-work made of fine thread, the free edges being strengthened by cord.

One of these balances is plain, while the beam of the other is elaborately fretted and engraved with circles-and-dots, and curves outlining the fretted spaces. Red paint has been rubbed in these incisions. The long suspending cord is strung alternately with a row of small beads of turquoise and red and white shell and a large, flat, oblong piece of shell pierced through the axis. The string is terminated by the figure of a bird and a fret ornament of shell representing a seated human figure with head-dress. Three small pendants of beads and shell hang below this and the whole forms an ornate and striking specimen.

One of the beams exhibited is of bone, ornamented with circles-and-dots, so regular, that they would appear to indicate the use of another instrument of precision, the compass.

Dr. Brinton has ascertained that the weights were small stones.[1] It would seem that, for the purpose of equalization of weights, the equilibrium of the beam being gauged by the eye, these balances are quite accurate. They are in perfect order at the present time.

In the Mexican collection at the Columbian Historical Exposition in Madrid there are two spherical objects of basalt, from the ancient Tarascos of Michoacan, which Dr. Troncoso, director of the Mexican National Museum, believes are weights. He supports this view by stating that at present the Indians use similar stone weights on their imperfect balances, which are formed of two small trays of wood, each suspended by three strings from the end of a wooden beam, which is balanced by a cord fastened at the middle.

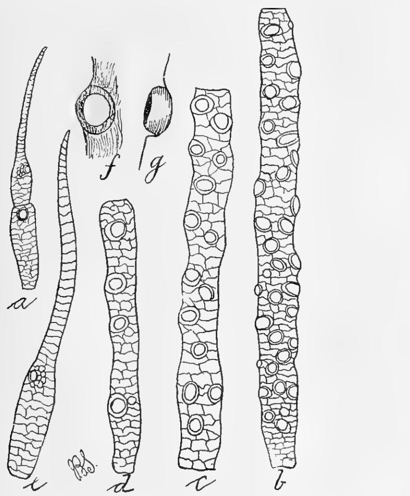
It is possible that the use of the balance will be found to have been more extensive in America than is suspected.

[1] Proceedings Numismatic and Antiquarian Society, Philadelphia, 1891.

AN INTERESTING SENSORY ORGAN IN CERTAIN PLANT LICE.

BY JOHN B. SMITH, SC D., RUTGERS COLLEGE.

DURING the season of 1890 plant-lice were unusually abundant and destructive on a number of cultivated crops in New Jersey, and I devoted some little time to the study of the more troublesome species, presenting the results, in popular form, in Bulletin No. 72 of the New Jersey Agricultural College Experiment Station. One of the matters that attracted my attention more particularly was the peculiar pitting of the antennæ. These pits and tubercles, as they have been indifferently called, are well known; but they have been often treated as merely sculptural features, and no special importance has been attributed to them. In my examinations of the structures I recognized them as sensory in character; but was not then and am not now able to specify their exact function, since they differ from what are usually described as the tactile and olfactory organs. The appended figure, showing the pittings of the antenna of the peach-louse, will serve to illustrate the appearance of the structures.



PEACH LOUSE:—*a*, Antenna of young louse; *b*, First long joint of winged form; *c*, Second long joint; *d*, Third long joint; *e*, Whip-joint; *f*, Sensory pit of antenna, from top; *g*, Same, from side.

I found that in all the wingless forms of all the species examined by me one type only was present. There is a single large pit, surrounded by a little group of small pits, on the last or whip-joint and, usually, a single large pit near the tip of the penultimate segment. This structure never changes in character while the insect remains wingless, whether it is newly-born or has reached a point where it reproduces its kind agamically. It continues also throughout the pupa state; but as soon as the winged form is assumed a very decided change appears, and every species shows a pitting peculiar to it. It may involve all the joints or only one may be modified; but, whatever the type, it is invariable within specific limits, and I have not found thus far any two species in which the pitting is identical. It may be that where a species is dimorphic, or where there are migratory and sedentary forms, that each form may have peculiarities of structure; but this I do not know.

At the time I made the studies above mentioned, I had neither males nor oviparous females of any species before me, and not until the fall of 1892 was I in position to examine sexed individuals carefully. I was curious to find whether any further modifications appeared in the true sexes, and whether the wingless, oviparous females shared in the larval type of structure. I obtained males and females of *Aphis brassicæ*, *Siphonophora cucurbitæ*, *S. rosæ*, *Myzus cerasi*, and *Phorodon humuli*. In the males of all, as I expected, I found the antennal pittings present, and was not unprepared to find that they differed from the viviparous winged forms in their somewhat greater number and distinctness. I was disappointed to find in the oviparous female no modification of the simple larval type; but, as I was in search of some character that would always distinguish this particular form without recourse to the primary sexual structures, I examined all parts of the insects minutely, and was rewarded by finding on the posterior tibiæ a series of sensory pittings exactly similar in structure to those of the antennæ in the male. I found that these structures differed in each of the species examined, in size, arrangement, and number, and the character is probably as little variable here as it is in the antenna. *Myzus cerasi* was the only species in which I had any number of specimens for examination, and in this I found that the tibial pitting does not appear until the insect becomes sexually mature.

To ascertain whether other species showed the same structure, I wrote Dr. C. V. Riley, asking whether he had observed it or could inform me as to its presence or absence in other species. Recently he very kindly replied as follows: "I have not yet been able to examine all the material at hand, but I can say that I have verified your interesting discovery in the following species: The pits are present in *Aphis mali*, *A. pruni*, *Myzus mahaleb*, *Siphonophora rosæ*, *Siphonophora sp.* on rose, *Callipterus sp.?* on oak, in *Phyllaphis fagi*, and in *Melanoxanthus salicti*. I do not find them present in the following genera: *Schizoneura*, *Glyphina*, *Pemphigus*, and *Phylloxera*, while in *Lachnus* they are not at all well developed or distinctly observable. This list, so far as it goes, would, therefore, show that they occur in what may be looked upon as the higher forms, and are absent in the *Pemphiginæ* and *Phylloxerinæ*."

I have not seen any mention of the structures above described, and am less than ever able to attribute a function to them. Finally, I desire to express my obligation to Dr. Riley, who not only examined the species mentioned in his letter, but also sent me the sexed specimens on which my studies were first made.

THE INDIANA ACADEMY OF SCIENCE.

THE eighth annual meeting of the Indiana Academy of Science convened in the rooms of the State Board of Agriculture, Capitol Building, Indianapolis, Ind., Dec 28, 1892, and continued through the 29th. The president was Professor J. L. Campbell of Wabash College, Crawfordsville, Ind. The meeting was one of universal interest. The attendance was large; the list of papers showed 92 titles, almost all of which were read.

The officers chosen for the year were: President, J C. Arthur, Purdue University, Lafayette, Ind.; vice-president, W. A. Noyes, Rose Polytechnic Institute, Terre Haute, Ind.; secretary, Amos W. Butler, Brookville, Ind.; assistant secretary, Stanley Coulter, Purdue University, Lafayette, Ind.; treasurer, C. A. Waldo, DePauw University, Greencastle, Ind.; auditors, Thomas Gray, Rose Polytechnic Institute, Terre Haute, Ind.; W. S. Blatchley, High School, Terre Haute, Ind.; programme committee, L. M. Underwood, DePauw University, Greencastle, Ind.; W. A. Noyes, Rose Polytechnic Institute, Terre Haute, Ind.

The arrangements for the spring meeting the third week in May contemplate a two days' session in the picturesque and interesting region in Park County, closing with a session Friday evening at Terre Haute.

The editors presented their report and also the first volume of the Academy's Proceedings ready for distribution. The volume contains the papers of the last preceding meeting together with an account of the field meetings, a bibliography of all papers read before the Academy since its organization in 1885, together with reference to the place of publication of each.

The following papers were presented: —

Notes on the Reproduction and Development of *Grinnellia Americana* Harv., M. A. Brannon; Evidences of Man's Early Existence in Indiana, from the Oldest River Gravels along the White Water River, A. W. Butler; On the Construction of a Sensitive Galvanometer, Benj. W. Snow; Some Facts as to the Varying Conditions of Rock Deposits as Observed in the Hudson River

Beds of Indiana and an Inquiry as to the Cause of the Same, Joseph Moore; A Simple Air Thermometer for the Determination of High Temperatures, W. A. Noyes; Test of the Torsional Strength of a Steel Shaft, Thomas Gray; An Extreme Case of Parasitism, Robert Hessler; Exhibition and Explanation of a Geological Chart, Elwood P. Cubberly; Local Variations, C. H. Eigenmann; Botanical Field-Work in Western Idaho, D. T. MacDougal.

When this stage on the programme was reached, the hour for noon adjournment arrived. It was then decided to meet in three sections in the afternoon, in order to accommodate members who were present with papers. The next morning, it was understood, the general sessions would be resumed. The three sections organized were, A, mathematics, physics, chemistry, and geology; B, botany; C, zoölogy and anthropology. In them the following papers were presented: —

The Quaternion Treatment of the Motion of Two or More Bodies under the Law of Gravitation, A. S. Hathaway; The Electrical Oxidation of Glycerin, W. E. Stone and H. N. McCoy; Notes Concerning Tests of the Purdue Experimental Locomotive, W. F. M. Goss; The Electrostatic Theory of Cohesion and Van der Waal's Equation, Reginald A. Fessenden; On Sulphon-Pthaleins, Walter Jones; Quartz Suspensions, Benj. W. Snow; Observations on Glacial and Pre-Glacial Erosion at Richmond, Indiana, Joseph Moore; A Modification of Grandeau's Method for Determination of Humus in Soils, H. A. Huston and F. W. McBride; Experiments with and Phenomena of Vacuum Tubes, R. A. Fessenden; The Extraction of Xylan from Straw in the Manufacture of Paper, W. E. Stone and W. H. Test; The Electro-Magnetic Inertia of a Large Magnet, Thomas Gray; The Determination of Chlorine in Natural Waters, W. A. Noyes; Some New Electrical Apparatus, R. A. Fessenden; Thiofurfurol and its Condensation Products, W. E. Stone and Clinton Dickson; On the Construction and Use of a Bolometer, B. W. Snow; On the Determination of Valence, P. S. Baker; An Application of Mathematics in Botany, Katharine E. Golden; On the Fertilization and Development of the Embryo in *Senecio aureus*, D. M. Mottier; Distribution of North American Cactaceæ (by title), John M. Coulter; *Marchantia polymorpha*, not a Typical or Representative Liverwort, L. M. Underwood; Notes Concerning Certain Plants of the South-Western Counties of Indiana, John S. Wright; Spines and Epidermis of the Cactaceæ (by title), E. B. Uline; Preliminary Notes on the Genus Cactus, E. M. Fisher; An Auxanometer for the Registration of Growth of Stems in Thickness, Katharine E. Golden; The Apical Growth of the Thallus of *Fucus vesiculosus*, D. M. Mottier; Symbiosis in Orchidaceæ, M. B. Thomas; Notes on Pediastrum, W. L. Bray; The Genus Corallorhiza, M. B. Thomas; Notes on Root Tubercles of Indigenous and Exogenous Legumes in Virgin Soil of the North-West (by title), H. L. Bolley; Notes on Archæology in Mexico, J. T. Scovell; Notes on the Loss of the Vomerine Teeth with Age in the Males of the Salamander, *Desmognathus fusca* (by title), F. C. Test; Modern Geographical Distribution of Insects in Indiana (by title), F. M. Webster; New Species of Indiana Hymenoptera, reared at LaFayette, Indiana (by title), F. M. Webster; Description and Elevation of Mount Orizaba, J. T. Scovell; The Climate and Glaciers of Mounts Orizaba and Popocatepetl, J. T. Scovell; A Mite, probably *Hypoderas columbæ*, Parasitic in the Pigeon, W. W. Norman; The Locustidæ of Indiana with Description of New Species, W. S. Blatchley; Early Stages in the Development of Cymatogaster, C. H. Eigenmann; Some Remarks Regarding the Embryology of Amphiuma, O. P. Hay; Some Structural Peculiarities of Pacific Slope Fishes (by title), A. B. Ulrey; Peculiar Death of an Oriole (by title), T. B. Redding; The Range of the American Crossbill (*Loxia curvirostra minor*) in the Ohio Valley, with Notes on its Unusual Occurrence in Summer, A. W. Butler; A Note on Loxia curvirostra, W. S. Blatchley; Notice of a Terrapin to be Restored to the Fauna of Indiana, O. P. Hay; A Migration of Birds and One of Insects, T. B. Redding; The South American Catfishes Belonging to Cornell University (by title), E. M. Kindle; Notes on the Genus Lytta, W. P. Shannon; The Icthyologic Features of the Black Hills Region, B. W. Evermann; Explorations in Western Canada, C. H. Eigenmann.

In the evening the Academy convened to listen to the address of President Campbell on "The Inter-Dependence of Liberal Pursuits."

At the general Session of the second day the following papers were presented: —

Ancient Earthworks near Anderson, Indiana, Francis A. Walker; The Work of the U. S. Fish Commission Steamer Albatross, in the North Pacific and Behring Sea in 1892, B. W. Evermann; A Thermo-Regulator for Rooms Heated by Steam, J. C. Arthur; Archæology of Tippecanoe County, O. J. Craig; Some Indian Camping Sites near Brookville, A. W. Butler; Relation of Kings County Traps to Those of Cumberland County, N.S., V. F. Marsters; The Traps of Red Head, N.B., V. F. Marsters; On Birds in Western Texas and Southern New Mexico (by title), A. W. Butler; An Account of Vegetable and Mineral Substances that Fell in a Snow-Storm in LaPorte County, Jan. 8–9, 1892 (by title), A. N. Somers; How a Tendril Coils, D. T. MacDougal; Remarkable Pre-Historic Relic, E. Pleas; The Bruns' Group of Mounds, H. M. Stoops; Some Points in the Geology of Mount Orizaba (by title), J. T. Scovell; Two-Ocean Pass (by title), B. W. Evermann; The Blattidæ and Phasmidæ of Indiana, W. S. Blatchley; Forestry Exhibit of Indiana at the Columbian Exposition, Stanley Coulter; The Yolk Nucleus, J. W. Hubbard; Some Causes Acting Physiologically toward the Destruction of Trees in Cities, J. C. Arthur; British Columbia Glaciers, C. H. Eigenmann; A State Biological Survey — a Suggestion for Our Spring Meeting, L. M. Underwood; The Mounds of Brookville Township, Franklin County, Ind., H. M. Stoops; How the Colleges Could Aid the Public Schools in Teaching Biological Subjects, W. W. Norman; Notes on the Flora of the Chilhowee and Great Smoky Mountains, Stanley Coulter; The Need of a Large Library of Reference in Cryptogamic Botany in Indiana, What the Colleges Are Doing to Supply the Deficiency, L. M. Underwood; Exhibition of a Series of Grouse and Ptarmigan from Alaska, B. W. Evermann; Botanical Assemblies in the United States Announced for the Year 1893, J. C. Arthur; Development of Ovule in Aster and Solidago (by title), G. W. Martin; Remarks on Archæological Map-Making (by title), A. W. Butler; The "Lilly Herbarium" and Its Work, John S. Wright; Additional Facts Regarding Forest Distribution in Indiana, Stanley Coulter; Rotary Blowers, John T. Wilkin; Some Effects of Mutilation on the Forms of Leaf and Sex of *Morus alba* and *Morus nigra* (by title), A. N. Somers; The Crawford Mound (by title), H. M. Stoops.

LIFE-SAVING.

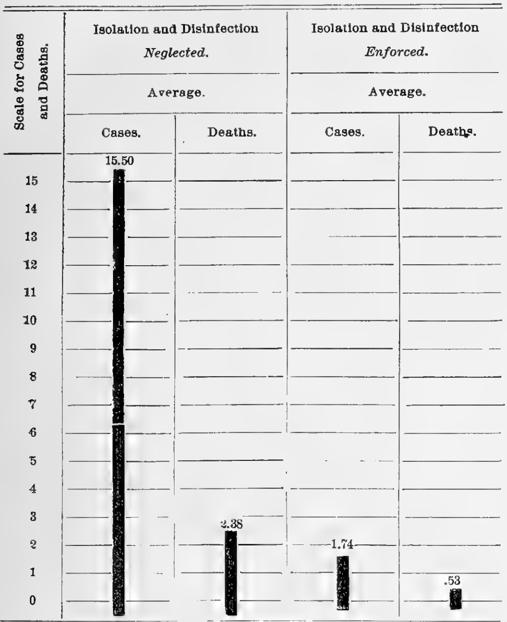
BY DELOS FALL, ALBION, MICH.

SANITARIANS are in the habit frequently of advancing claims in regard to the practical value of their work, resulting, they say, in a great lessening of sickness and the actual saving of many lives. For example, in a carefully prepared paper, read before the Sanitary Convention at Vicksburg, Dr. Baker, secretary of the Michigan State Board of Health, gave official statistics and evidence, which he summarized as follows: —

"The record of the great saving of human life and health in Michigan in recent years is one to which, it seems to me, the State and local boards of health in Michigan can justly 'point with pride.' It is a record of the saving of over one hundred lives per year from small-pox, four hundred lives per year saved from death by scarlet-fever, and nearly six hundred lives per year saved from death by diphtheria — an aggregate of eleven hundred lives per year, or three lives per day saved from these three diseases! This is a record which we ask to have examined, and which we are willing to have compared with that of the man who 'made two blades of grass grow where only one grew before.'"

It has occurred to the writer that even scientific workers look upon such statements with a large degree of allowance. They can be demonstrated, however, as the following will illustrate: The table below is compiled from reports of local health officers to the secretary of the State Board of Health relative to the cases of sickness and deaths from diphtheria in Michigan during the year 1888. The reports taken for this study deal with 92 outbreaks. In 34 of these the sanitary precautions of isolation and disinfection were neglected; in 58 outbreaks these preventative measures were enforced.

Diphtheria in Mighigan in 1888: Exhibiting the average numbers of cases and deaths *per outbreak*: in those outbreaks in which isolation and disinfection were both *neglected*; and in those outbreaks in which both were *enforced*. Compiled in the office of the Secretary of the State Board of Health, from reports made by local health officers.



It will be seen that if in all the 92 cases isolation and disinfection had been neglected, the total number of cases of sickness would have been 92 × 15.5 = 1426; and the number of deaths would have been 92 × 2.38 = 219.

On the other hand, if all had been done that could have been done, if all possible means had been employed, there would have been 92 × 1.74 = 160 cases of sickness, and 92 × .53 = 49 deaths. The saving in sickness would have been 1426 — 160 cases, and the saving of life would have been 219 — 49 = 170.

But the whole number of outbreaks of this disease in Michigan during the year was 311. Applying the same reasoning as before, first, if isolation and disinfection were neglected in every case, there would have been 311 × 15.5 = 4820 cases of sickness and 311 × 2.38 = 740 deaths. On the other hand, if all had been done that ought to have been done, there would have resulted 311 × 1.74 = 541 cases of sickness and 311 × .53 = 171 deaths. The total saving in sickness would have been 4820 — 541 = 4279, and the saving of life would have been 740 — 171 = 569.

These figures are at the same time a justification of the claims which health officers make, suggested above, and a demonstration of the efficacy of the means employed, isolation and disinfection, in producing these results.

NOTES AND NEWS.

THE partnership heretofore existing between Geo. L. English, E. C. Atkinson, and Wm. Niven, as Geo. L. English & Co., has been dissolved by mutual consent. Geo. L. English, having purchased all the stock, good-will, and fixtures, will continue the business under the same firm name. The firm has removed to a new ground-floor store, No. 64 East 12th Street, five doors east of Broadway, three doors west of Fourth Avenue. They have purchased the business of Mr. Philip Fuchs, who for ten years was in the employ of Tiffany & Co., and more recently has been in business for himself. Mr. Fuchs has entered their employ, and a complete lapidary equipment, including five lathes, has been put in the new store. Very much more attention will be given to gems, especially rare stones, and they propose materially enlarging this department and carrying a good stock. Among gems now on hand are willemite, oligoclase, beryllonite, diopside, sphene, obsidian, phenacite, demantoid, peridote, prehnite, hiddenite, garnet, amethyst, cat's-eye, aquamarine, golden beryl, emerald, chrysoberyl, moonstone, rubellite, turquois, zircon, opal, sapphire. Any other gem will be secured. Mounting done to order. Microscopical sections of rocks and minerals will be manufactured on the premises and a good stock kept on hand. Their enlarging stock of meteorites will soon be worthy of prominence.

— During the past summer courses of instruction were offered by professors and instructors of Cornell University in Greek, Latin, French, German, English, philosophy, mathematics, physics, chemistry, botany, drawing, and physical training. In all there were a hundred and fifteen in attendance, representing twenty-two States and Territories, Canada, and Japan; and of these far the greater part were teachers and advanced students. The private venture, begun so auspiciously, has now taken a more permanent form, and the school has been made an integral part of the university. The list of courses offered for the summer of 1893 is greatly increased, and among the additions to the corps of instruction of last summer are Professors Wheeler and Bristol and Dr. Laird in Greek, Professor Bennet in Latin, Professor Smith in elocution and oratory, Professor Tichener in psychology, Professor Williams in pedagogy, Professor Wilcox and Dr. Hull in social and economic science, Professor McMahon in mathematics, and Professor Hitchcock in physical training. Summer courses in the school of law will also be offered this year for the first time, instruction being given by the entire faculty of the school.

— The third annual meeting of the American Morphological Society was held at Princeton College, Dec. 27 and 28, under the presidency of Dr. C. O. Whitman of the University of Chicago. The meeting was well attended, and several additions were made to the list of members, which includes the majority of the active workers in the department of animal morphology in this country. The following is a list of the papers presented at the meeting: Dr. E. B. Wilson, Columbia College, The Cleavage of the Ovum and the Teloblasts of Amphioxus; Dr. C. W. Stiles, the Agricultural Department, Washington, The Topographical Anatomy in the Family Tæniadæ; Dr. E. O. Jordan, University of Chicago, The Maturation and Fertilization of the Egg of the Newt; Professor E. D. Cope, Philadelphia, False Elbow-Joints in Man and the Horse; Mr. Arthur Willey, Columbia College, On Acinetæ Parasitic in the Buccal Tube of Diplosoma; Dr. C. B. Davenport, Harvard College, On the Development of the Cerata of Eolis; Dr. H. B. Ward, University of Michigan, On the Host of Nectonema; Dr. C. O. Whitman, University of Chicago, The Metamerism of Clepsine; Dr. W. B. Scott, Princeton College, The Evolution of the Premolars; Dr. H. Ayers, the Lake Laboratory, Milwaukee, The Ending of the Auditory Nerves in the Hair-Cells; Dr. E. A. Andrews, Johns Hopkins University, Notice of a New Sort of Amphioxus; Professor A. E. Verrill, Yale College, Some New Forms of Menerteans; Dr. T. H. Morgan, Bryn Mawr College, Preliminary Note on Balanoglossus; Professor B. Sharp, Academy of Natural Sciences, Philadelphia, Joint-Formation among the Invertebrata; Professor W. A. Locy, Lake Forest University, The Formation of the Medullary Groove and Some Other Features of Embryonic Development in the Elasmobranchs. The officers of the Society for the current year are: President, Dr. C. O. Whitman, University of Chicago; vice-president, Dr. E. B. Wilson, Columbia College; secretary and treasurer, Dr. J. Playfair McMurrich, University of Cincinnati; members of the executive committee elected from the society at large, Dr. T. H. Morgan, Bryn Mawr College, and Dr. C. B. Davenport, Harvard College.

SCIENCE:

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THE AMERICAN PSYCHOLOGICAL ASSOCIATION.

One of the most significant meetings that occurred during the Christmas holidays was that of the American Psychological Association in Philadelphia on Dec. 27 and 28. This was the first regular meeting of this body, a temporary organization having been effected in June last. The coming together of psychologists is significant not alone of the rapid strides which this science has recently been making, but particularly of the unity of method and subject matter which the introduction of scientific modes of observation into this controverted field has made possible. This association of psychologists is composed almost exclusively of specialists who are studying the nature of mental processes by the help of ingeniously devised apparatus, are propounding new problems and adopting new methods to their solution, and, in brief, are proceeding with that painstaking vigor and caution characteristic of all phases of modern science. For these reasons an account of their proceedings necessarily becomes somewhat technical; but this moderate element of technicality is itself a welcome relief from that over-popularization and almost sensational publicity in which a line of activity too often confused with psychology has indulged.

The meeting was called to order by the president of the association, President G. Stanley Hall, of Clark University, and the reading of papers began with a paper by Professor Catell, of Columbia College, on "Errors of Observation in Physics and in Psychology." Professor Catell criticised that line of psycho-physical observation in which the liability to degree of error was taken as a standard of the sensibility for differences. He considered that the entire subject needed re-investigation, with a complete separation of these two points. He also regarded that recent experiments of his own and Professor Fullerton tended to show that the errors of observation do not fall under the law as usually stated (Weber's law) but approximate the law which the distribution of errors demanded.

A very interesting problem was presented by Dr. Witmer, of the University of Pennsylvania, in an account of a research upon the æsthetics of visual form. Dr. Witmer attempted to determine, by a large number of experiments, such questions as, What are the most pleasing forms? What proportions of the division of a line, and what proportions of the relations of the height to the breadth of a rectangle are the most pleasing? and the like. The results of these preferences were interpreted by reference to the general outline of the field of vision, of which the figures form a part. He showed conclusively that the former views of the conditions of such æsthetic judgments were inadequate, and that this neglected factor of the position of the figure with reference to the edges of the field of vision was a most important one. The experiments will be continued and give promise of contributing some measure of system and order to a field usually regarded as determined by caprice.

President Hall presented a brief outline of the history and prospects of experimental psychology in America, tracing the beginnings of this study from the first laboratory founded at Johns Hopkins University some eight years ago, up to the present time, when there are as many or more psychological laboratories established in this country than in all Europe. The rapid dissemination of interest in psychological studies and the material provisions for its future development were ably presented, and various measures of credit judiciously assigned. The publication of such a review of the purposes, methods and results of the new psychology, as President Hall outlined, should certainly contribute much towards a more general understanding of what psychology and psychologists are doing and—equally important—not doing.

Professor Jastrow, of the University of Wisconsin, gave an account of the exhibit of experimental psychology, which is to be made at the World's Fair. Here, for the first time, the attempt will be made to gather together various types of apparatus which are used in psychological research, to maintain in running order a working laboratory, in which simple tests of the senses, powers of judgment, the times of mental processes, the peculiarities of association, the limits of memory, of fatigue, and the like, may be made and recorded; and to exhibit in some measure the results of statistical and other forms of research. Considerable expenditure, the co-operation of colleges, of individual psychologists and of makers of apparatus have been secured for the successful completion of this large task. It is hoped that this somewhat comprehensive exhibit of the method and aims of this new science may aid in disseminating a truer and more appreciative view of the theoretical and practical value of this line of research than has yet been accomplished.

Professor Münsterberg, of Harvard University, upon the request of the president, addressed the association, speaking of the problems that were engaging his attention at his laboratory at Cambridge. No less than fifteen subjects of investigation are here in progress, and the nature of some of these Professor Münsterberg described in a very interesting manner. The impetus to work in this direction, which his acceptance of the chair at Harvard has given, has already made itself evident, and, before the year is over, many important results will undoubtedly be issued from his laboratory. The subjects under investigation covered a wide range, from the determination of the methods of localizing sounds in space, and a new method of determining when differences of sensation may be regarded as equal, to complicated experiments upon the nature of association, of changes in mental condition, of complex forms of reaction, and the like.

Dr. Sanford reported some of the minor studies which are in part completed and in part in progress at the laboratory of Clark University. One of these studies gave an account of the fluctuations in mental power at different portions of the day, as determined by the capacity to remember a series of arbitrary impressions. Another dealt with the frequency and character of dreams of subjects who every night at once recorded their dreams upon awakening from them. The great frequency of dreams, the fact that they are concentrated in the early hours of the morning, that they are so largely based upon actual experiences, and that recent events contribute much to their content,—these and other points clearly appeared in the analysis which this material furnished.

Professor Bryan, of the University of Indiana, presented two papers, in one of which he gave an account of experiments establishing the effect of the intensity of the stimulus upon the reaction time; and, in the other, described some tests which had been made in schools of Springfield, Mass. These tests show the development of motor power in children at different ages, and brought out many unexpected and significant relations.

Papers were also read by Dr. Nichols, of Harvard College, presenting some novel experiments upon illusions of rotation and upon the sense of pain; by Professor Pace of the Catholic University of Washington, describing some observations upon the power of judging the thickness of surfaces held between the thumb and forefinger; by Dr. Witmer, describing the results of a few simple reaction times, taken upon a great variety of unpractised subjects; other papers of a somewhat philosophical nature were presented by Dr. Chamberlain, on the "Relation of Psychology to Anthropology," and Dr. Aikens on an "Analysis of Cause."

The meeting adjourned, to meet next December, at Columbia, N. Y. The officers of the association are: G. Stanley Hall, president; Professor Ladd of Yale University, vice-president; and Professor Jastrow of the University of Wisconsin, secretary.

ASSOCIATION OF AMERICAN ANATOMISTS.[1]

The following persons were elected to membership: —Herbert S. Birkett, M.D., Montreal, Canada, Demonstrator of Surgery, McGill University; Tracy Earl Clark, B.S., Clinton Liberal Institute, Ft. Plain, N. Y.; J. Milton Greenman, Assistant Director Wistar Institute of Anatomy, University of Pennsylvania; James W. Hartigan, M.D., Morgantown, W. Va., Professor of Biology, University of West Virginia; Geo. S. Huntington, M.D., New York City, Professor of Anatomy, College of Physicians and Surgeons; Peter J. McCourt, M.D., New York City; Middleton Michel, M D., Charleston, S. C., Professor of Physiology, Medical College of South Carolina; Wm. B. Scott, Princeton, N. J., Professor of Geology and Paleontology; Wm. Anderson, F.R.C.S., etc., London, England, Demonstrator of Anatomy, St. Thomas's Hospital College (honorary); C. S. Minot, S.D., Harvard Medical School, Professor of Histology and Embryology; C. A. Hamann, M.D., Assistant Demonstrator of Anatomy, University of Pennsylvania.

The executive committee, through the secretary, reported that the circular in regard to information concerning the Negro race was nearly ready.

The following papers were then read: 1. Crania of the Cetacea. 2. The human lower jaw, Dr. Harrison Allen, University of Pennsylvania. These two papers were illustrated by specimens and discussed by Professor Herrick and by Professor Geo. Macloskie of Princeton University. 3. History of the development of bone-tissue. Illustrated by microscopic slides. Dr. Carl Heitzmann, New York City. Discussed by Professors Macloskie and William Libbey, Jr., of Princeton University.

The following quotation is an extract from Dr. Allen's presidential address: "It is now four years since the Association of American Anatomists was founded, with a list of fifteen members. Many were the objections raised when it was proposed to organized a new society. Eminent professors declared that it was not needed; others, while sympathizing with its objects, were convinced that the list of members would be so small that it would be a difficult matter to fill the necessary offices. The fact that an active membership exists of ninety-four persons, representing twenty two States, the District of Columbia and the United States army, sufficiently meets both the above-mentioned objections. It tells us unmistakably that the society is needed, and that not only are the offices filled, but that the association is recognized as a devoted band of students whose activity compares favorably with that of other scientific organizations.

"But the work thus far accomplished is but the harbinger of what it is hoped may be undertaken. An attempt at co-operation between the American Anatomists is to be brought before you at this meeting. The executive committee will present a plan by which observations on the anatomy of the Negro shall be entered upon. It is earnestly hoped that every teacher and demonstrator of anatomy in the country, whether a member of this society or not, will assist its committee in accumulating observations on this class of subjects."

4. An anomalous development of the human sternum. Specimen and remarks by Dr. D. S Lamb, Army Medical Museum, Washington, D. C. Discussed by Dr. Dwight. 5. Discovery of an ossified thyroid cartilage and a supposed rudimentary clavicle in an Artiodactyl. Professor Wm. B. Scott, Princeton University. Specimen exhibited. Discussed by Professor Cope and Dr. Allen. 6. Observations on the psoas parvus and pyramidalis. A study of variations. Dr Thomas Dwight, Harvard Medical School. 7. Significance of percentages in reversions in human anatomy. Professor H. F. Osborn, Columbia College, New York City. Discussed by Professor Cope and Drs. Dwight and Lamb.

3 P.M. session resumed. The following papers were read: 8. Histogenesis in the brain, and its bearings on development and decline. Professor C. L. Herrick, Dennison College, Granville, Ohio. Discussed by Drs. Heitzmann and Piersol. 9. The metapore or foramen of Magendie, with photographs. Professor B. G. Wilder, Cornell University. In the absence of Professor Wilder, the paper was read by Mr. Clark. Discussed by Professor Herrick. 10. Neuromerism and the cranial nerves of Ophidia. Professor Herrick. 11. The insula of the pig. With specimens. Discussed by Drs. Allen and Dwight. 12. Note on diagrams of the spinal cord. Dr. J. T. Duncan, Toronto, Canada. Read by the secretary, and referred to the Committee on Nomenclature. 13. Duration of motion of human spermatozoa. Professor Geo. Piersol, University of Pennsylvania. Discussed by Drs. Spitzka and Heitzmann.

Thursday, Dec. 29, 1892. The report of the Committee on Nomenclature, Dr. Wilder, secretary, was presented. The reading of the report was dispensed with, copies having been placed in the hands of the members present.

The following papers were read: 14. The innervation of the organ of Corti. Howard Ayers, Ph.D., Curator of the Lake Laboratory, Milwaukee, Wis. Microscopical slides with remarks. 15. The posterior surface of the liver, as described by Vesalius. Dr. F. H. Gerrish, Bowdoin College, Maine. Discussed by Drs. Dwight, Allen, and Heitzmann. 16. Embryos of bats. With specimen and plates. Dr. Allen. Discussed by Professors Cope and C. S. Minot. 17. Meckel's diverticulum. Dr. D. S. Lamb, Army Medical Museum, Washington. Discussed by Drs. Dwight and Minot. 18. Delimitation of abdominal regions. Dr. E. A. Balloch, Howard University, Washington. Read by the secretary. 19. The need of agreement in the limits of the adbominal regions. Dr. Gerrish. The last two papers were discussed together by Drs. Dwight, Piersol, Kemp, Heitzmann, and Lamb. It was decided that, with the consent of the authors, copies of these papers be sent to the committee on this subject appointed by the Anatomical Society of Great Britain and Ireland; and also to the committee of the German Anatomical Society. 20. Physical characteristics of the Kootenay Indians of South Eastern British Columbia. Professor Alex. F. Chamberlain, Clark University, Worcester, Mass. Read by title. 21. Series of thirty-five natural-size photographs of sections of human brain, with brief remarks. Dr. I. S. Haynes, University of New York.

[1] Fifth annual session, at Princeton, N. J., December 27, 1892. Dr. Harrison Allen, of Philadelphia, president; Dr. D. S. Lamb, U. S. A., secretary.

NOTES ON THE OCCURRENGE OF RUBELLITE AND LEPIDOLITE IN SOUTHERN CALIFORNIA.

BY HAROLD W. FAIRBANKS, BERKELEY, CAL.

THE work of the California State Mining Bureau has recently brought into notice a very interesting association of minerals in San Diego County, California. The most important of these are lepidolite and rubellite. The former remarkable for the great quantity and purity in which it occurs, and the latter for its exquisitely radiated crystal aggregates. The ruby-tinted tourmaline imbedded in the pale lilac-colored mica presents a picture of beauty rarely equaled in the mineral kingdom. Before giving a detailed description of the occurrence of these minerals, a few words on the general geology of the district may not be out of place.

San Diego, the southern county of the State, is dominated by one main system of mountains known as the Peninsula Range. This consists of a confused mass of mountains and valleys rising gradually from the coast to the summit, forty miles inland, from which the descent is quite abrupt to the Colorado Desert. The average height of the watershed is about four thousand feet, but toward the northern boundary of the county, Mount San Jacinto reaches an altitude of about ten thousand feet. This Peninsula Range consists chiefly of granite which often takes on a dioritic facies. Dark basic diorite and rocks of the norite type occur as intrusions of considerable magnitude. Quartzite, mica schist, and thin bedded gneisses form long, narrow areas extending a little west of north and east of south. They represent extremely metamorphosed remnants of the original sedimentary formation.

Lying on the west of the summit of the range and extending parallel with it is a strip of granitic country filled with irregular dikes or veins of coarsely crystallized quartz, feldspar and muscovite ; or frequently of feldspar and quartz only, in the latter case taking on a pegmatitic structure. Black tourmaline in irregular crystals is generally characteristic of these dikes.

The rubellite and lepidolite are found associated with an immense dike of this character near Pala, a short distance west of the foot of Smith's Mountain. The dike occurs in one of the norite bosses which forms a high hill over half a mile across. Similar bodies of pegmatitic rock are found in the granite in the vicinity but contain no rubellite. The outcrop is tracible along the eastern slope of the hill for nearly three thousand feet, in places forming a precipitous ledge. It gradually increases in width toward the southern end, where it is three hundred feet across.

It is near one edge of this great mass of pegmatite, and inclosed in it, that the minerals in question occur.

The northern portion of the dike contains no tourmaline ; the dominant character being that of a very coarse muscovite granite, with a sprinkling of minute garnets. Both large and small bodies of finely formed pegmatite lie apparently wholly isolated in the coarse granite.

As the dike is followed southward to a point about midway in its course, crystals of black tourmaline begin to appear in abundance. One crystal ten inches long appeared broken into a dozen pieces, which had been moved a slight distance apart but were perfectly angular. The quartz-feldspar matrix showed no signs of crushing, and it is difficult to understand how the appearance could have been produced unless the crystal existed prior to the consolidation of the yielding magma.

Parallelism of the smaller and more slender crystals is often to be observed as taking place about the larger ones. Green tourmaline is present in small amount. It does not generally show any crystalline form, but is disseminated in small granular particles irregularly or aggregated about the black tourmaline.

The lepidolite appears here first in small irregular patches. A few yards to the south it forms a well-defined vein, and is filled with minute needle-like crystals of rubellite. Quartz crystals with fairly well defined boundaries are scattered through it.

At the point where the lepidolite reaches its greatest width, about sixty feet, it contains very little rubellite and is quite massive and pure save for granular aggregates of an acid plagioclase feldspar, probably oligoclase. It is near the southern end of this great body of lithia mica that the rubellite appears in the large radiated aggregates. Fan-shaped clusters of rubellite also occur in the quartz and feldspar adjoining the lepidolite. Single crystals in these groups are often fifteen inches long and one-half inch in diameter. One cavity containing good quartz crystals has been found, and it is possible that with farther exploration gem tourmalines may be found. Many of the smaller crystals in the lepidolite are clear and of good color, but are full of checks.

The rubellite crystals are generally gathered in radial aggregates six inches to a foot in diameter, but sometimes occur singly. Single crystals appear with smaller ones branching from one end presenting a tree-like form, or two or more intersect each other so as to form a cross. The aggregates are sometimes slender, with a slightly wavy course. The crystals either branch outward without any order or they all incline one way, giving the appearance of a fern. In other specimens lines of crystallization radiate from a common centre ; curved or club-shaped crystals branching from each line. Hematite is sometimes found coating the tourmaline crystals.

Nine minerals are thus found associated together here—quartz, feldspar, muscovite, garnet, hematite, oligoclase, green, red, and black tourmaline.

A somewhat similar occurrence of minerals is reported from the mountains of Lower California, but nothing is known about it.

The granitic portions of the Sierra Nevada and Peninsula Ranges contain but few rare or beautiful minerals, and on that account the deposit at Pala is all the more remarkable.

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.

Snake Story.

APPROPOS of the interesting notes on snakes, lately published in your columns, I would like to relate the following : —

I think it was about the middle of last June that our little boy, who is interested in collecting various natural history objects, brought home a full-grown water-snake. He procured a box of generous dimensions, one whole side of which he covered with wire-screen, such as is used in windows. A small slide was made in the top of the box, so that the porcelain tray (such as photographers use for developing trays), which he placed within, could be kept filled with water, and also for the introduction of food.

This box was thenceforward "the snake den," and here the snake passed the remainder of its existence. A small frog, several grasshoppers, and various insects were dropped through the opening in the top of the box from time to time, but we are not sure whether the snake ever deigned to taste a morsel during her entire captivity; certain it is, however, that if she did finally taste the frog, she did not find it a very appetizing meal, for the little frog hopped about in the box for days and days without any food itself. It was just as apt to rest upon the body of the snake as anywhere else, each seemingly indifferent to the presence of the other. The grasshoppers also were entirely ignored. The snake was left in the box, in the back-yard, during the months of July and August, with no care whatever, we being absent during that time, and the little boy who had agreed to look after it having deserted it.

What was our surprise, after our return early in September, to find one day that Mrs. Snake had giving birth to thirteen little ones. Such a little, writhing, squirming, snaky mass! The little snakes were about five inches long, and soon became quite active. In the course of a few weeks they were much more ready to take their own part than their mother seemed to be. She had probably learned by experience that it was of little or no avail to "fight back," and contented herself with running out her forked tongue when irritated, and then trying to creep out of harm's way. The little ones, on the contrary, would crawl up the screen as far as possible, and when pushed off, with a straw or wire introduced through the screen, they would at once crawl up again, run out their little tongues, and show all the rashness of youth.

Wondering how far the maternal instincts were developed in the mother snake, whether she would try to defend or protect them, the young snakes were frequently irritated, in order to arouse, if possible, her defensive propensities; it was all to no purpose; she seemed a heartless mother, ignoring wholly that her offspring were in danger. A long wire was often thrust into the box, and under a little snake, which could thus be dangled before the old snake in a most irritating manner. But, whether from fear on her own part, or utter indifference to the welfare of her young, she paid no attention whatever to the provocation.

The mother snake lived until the middle of October, when she succumbed to the white frosts of autumn.

A few weeks later two of the young snakes fell asleep; one of them was given to a little schoolmate, who put it in his pocket and took it to school, when, lo! and behold! the warmth from his body resuscitated it, and the "bad boy" played with it in school. To the teacher's question as to what he had, he replied, "A shoestring!"

Learning thus that possibly the remaining little snakes might sleep (hibernate) through the winter, soil, small stones, dead leaves, etc., were placed in the box, and they crept away out of sight. Whether they are dead, or only sleeping, we do not know. They lived, however, some seven or eight weeks, were active, seemed well and happy (?) and, as far as we know, never ate a mouthful of anything during the entire time. I neglected to mention that the old snake shed her skin once during her captivity, unfortunately, it was during our absence, and we did not witness the operation.

It certainly seems strange that, with so much fasting, they none of them should look thin and poor, but should apparently grow and increase when having consumed nothing.

MRS. W. A. KELLERMAN.

Columbus, O., Jan. 9.

Geographical Variation in Birds.

IN your issue of Jan. 6 there appears a communication entitled "Geographical Variation in Birds," containing several remarkable statements, two of which I would like to correct. In speaking of "desert coloration," this writer says: "If the scorching sun of the desert regions will bleach out one species, why will it not do the same for another? The plea of adaptation of coloration for protection cannot be urged here." Such a conclusion does not follow, and the plea of protective coloration might reasonably be made, because the "bleaching" of which he speaks did not take place during one summer, but is the result of natural selection for an unknown number of generations, and, while in some species this protective coloration has proved beneficial, it does not follow that in other species with different habits natural selection would work along the same lines.

But this is only a slight error compared to the following astonishing paragraph, which I quote in full, the italics being my own: —

"Not only are colors affected, but size as well, by geographical position. This is probably more marked north and south than east and west. *And yet the variation in size alone is not sufficient for a sub-specific division* It is not at all strange that those individuals of a migratory species which push farthest north should possess stronger bones and muscles and so be larger than those which were not able to fly so far. It would seem natural that the constant recurrence of such a difference would tend in time to form a race peculiar enough to be recognized as a sub-species. *But it has not proven true thus far in the history of the world,* and why should there be any change under the same conditions?"

If the above quotation means anything, it is that the author believes increase in size to be more or less general and due to the longer migrations of originally stronger individuals, and yet that this process of selection has not up to this time produced even a tenable sub-species! Considering these two unique ideas in reverse order, let us see whether there are not some species, or at least sub-species, based solely on an increase or decrease in size. Hastily running over the list of North American birds, we find the following interesting facts: *Troglodytes alascensis* is accepted as a different species from *T. hiemalis*, but the variation is only in the size. *Accipiter velox* differs from *A. cooperi* practically in size only. *Totanus melanolensus* and *T. flavipes* are described by Coues as "precisely the same" except for size. And, lastly, *Rallus virginianus* is "a perfect miniature" of *R. elegans*, being about forty per cent smaller.

In addition to these species, we find there are at least sixteen sub-species which differ from the original stock only in size. About half of these vary from east to west, the others north and south. Furthermore, as Dr. Coues so truly says, many American representatives of European species are "larger and better birds" than their foreign relatives, but we will not include them here, because there are generally some slight differences in coloration as well. So much for the existence of sub-specific variation in size; now, as to the idea that this increase is due to migration. If it is so, how will we account for the cases already given in the genera *Accipiter* and *Totanus*, where the differing species have practically a co-extensive range; or for the even more difficult case of *Rallus*, in which the smaller species is a much more northern bird? But the best illustration to show the fallacy in both ideas is *Dryobates villosus*. Here we have a widely distributed bird, a resident, not a migratory species, which has two accepted sub-species based solely on variation in size: the northern form, *D. v. leucomelas*, larger than normal, and the southern form, *D. v. audubonii*, smaller. How can this be accounted for on the proposed "migration" theory? To sum up the whole matter, it is probable that northern birds will average larger as a rule, especially in resident species, as they are thus better fitted to stand the severity of the climate and the other difficulties of boreal existence. Furthermore, it can hardly be denied that variation in size is in a number of cases not only sufficient to denote a sub-species, but, occasionally, where the connecting links have disappeared, to form what is universally considered a distinct species.

HUBERT LYMAN CLARK.

Pittsburgh, Pa., Jan. 13.

Pseudoaurora Not Shadows.

THE explanation of the phenomena reported by me in *Science*, issue of the 16th of December, is altogether too common an observation to leave any doubt of its failure to clear up the mystery. My calling has made me very familiar with all of the "shadows cast upon the fog by projecting arms or objects in the beam from the light," as "seen at any time when there is smoke, light fog, or mist."

The phenomena which I described was entirely new to me, and apparently distinct from *shadows* of any kind, consisting of pencils of light radiating upward from a dark arc, the centre of which was somewhat east of north, the pencils constantly changing in length, and having an apparent movement laterally precisely like those of the ordinary northern lights while I remained standing still. The characteristic coloration of the pencils was unmistakable, but not as distinct as I have sometimes seen it. I have seen the "shadows" so often under similar circumstances of smoke, fog, and mist, that I should scarcely have noticed the matter but for the dark arc with its superimposed luminous arch and the radiations described. But I spent considerable time in making the different observations mentioned before, and took in the familiar shadows that impress the mind of Professor Hazen so strongly.

P. L. HATCH.

Anacortes, Washington.

Natural Selection at Fault.

WE are generally told by orthodox Darwinians that both the structure and the actions of animals are to a great extent dependent upon natural selection. Any organ, or any habit which is not advantageous to its species will be, it is said, promptly suppressed as a possible danger, or at least, an unremunerative demand.

Yet there are a few cases of habits which seem to have been acquired or maintained in flat contradiction to this doctrine. Every one knows that the *Felidæ*, from the Bengal tiger down to our domestic mouser, when they have seized a prey do not at once kill and devour it, but either torment it or at least sit and watch it for some time before administering the fatal bite or blow. The consequence is that the victim sometimes escapes, as we all have witnessed, when pussy is playing with a mouse. One instance at least is on record where a man, struck down by a tiger, quietly drew a dagger and stabbed the assailant to the heart. This could not have been done with a beast of prey of the canine or ursine family, as they do not allow time for devising and executing such a manœuvre. Hence we see that the peculiar conduct of the cats is disadvantageous to themselves, and we ask why it has not been abandoned. Certainly any cat which should at once devour any mouse or bird which it had caught would, in times of scarcity, have a decided advantage over its fellow-cats.

Similarly injudicious is the conduct of the domestic hen. As soon as she has laid an egg she at once announces the fact to all whom it may concern by her well-known cackling. What benefit is this outcry to herself or to her species? On the contrary, the outcry is heard by animals which are given to stealing eggs and is understood by monkeys, if we may accept the evidence of La Vaillant. There again, therefore, we have a line of conduct quite contrary to what natural selection would determine.

In man, there is not, indeed, a habit, but an organ which has lost its uses, yet is still developed in every child brought into the world. The outer ear was formerly provided with muscles by which it could be turned towards any sound for its better recognition. These muscles have become obsolete by heriditary disuse, so that in all normal subjects the ear is motionless. That it could formerly be directed so as the better to receive a sound will the less be denied as it survives in certain exceptional individuals. But as it is absent in the great bulk of our species, the question arises, Why does the external ear not gradually cease to be developed? No one can now contend that it is useful.

J. W. SLATER

London, England.

Speed of Flight of Birds.

I HAVE always been more or less of a sceptic in regard to the high rate of speed in the flight of certain birds, but I have only just obtained a bit of satisfactory evidence from my own observations. Our wild ducks are admitted to be among our strongest flyers, but I am satisfied that the buffle-head (*Charitonetta albeola*) does not attain a speed of forty miles per hour. While travelling on the Baltimore and Ohio Railway, up the valley of the Potomac, on Jan. 3, I saw a great many ducks, nearly all of which were buffle-heads. Those who are familiar with the road will recall how closely it follows the windings of the river, so that a bird flying up mid-stream would travel just the same distance as the train on the bank. It so happened that, on rounding a sharp curve, my train flushed a pair of buffle-heads, which started up stream at full speed. On watching them I found that, instead of leaving us behind, we were actually beating them, and I am confident that their rate of speed was not equal to that of the train. We kept alongside of them for nearly a minute before they turned back down-stream. Careful calculation showed that the train was running at about thirty-seven miles per hour, so that the rate of speed for those wild ducks would be about thirty-six. I hope that others may have some evidence on this question of speed in flight which will throw more light on the subject.

HUBERT LYMAN CLARK.

Pittsburgh, Pa.

Bowser's Trigonometry.

As I have learned to admire the mathematical text books of Professor Bowser from the excellent results I have had from their class room use for several years, I was surprised to see the somewhat adverse criticism of his Trigonometry in *Science* of Nov. 25. I disagree with your critic's assertion that the best way to study trigonometry is along the line of its historical development. I believe that such a course of study would be objectionable, because of the long time it would require, and because the student would be compelled to unlearn, if I may so phrase it, many things he would necessarily be called upon to learn if he followed the historical method. It is a recognized pedagogical fact that it is easier to teach correct methods to a student who has never used incorrect methods, than to one who has. To acquire a complete knowledge of trigonometry would undoubtedly require a study of its development, to acquire the knowledge required for its proper and facile use in its many applications, does not require a study of its history.

And accordingly I believe his plan of giving the best results and methods of the best students and workers in trigonometry is to be preferred to a method which requires a student to test and reject what has long before been tested and rejected. I admire Professor Bowser's plan of giving such definitions of the functions as apply to all angles, acute, obtuse or reflex. I think some of the writers on the subject have fallen into a grave error when they give definitions of the functions of acute angles, and afterward modify the definitions to suit obtuse angles.

In Professor Bowser's development of the theoretical part of the subject, he is especially clear. His book is a readable one. He is precise in his statements, and his demonstrations are such as the average student can readily follow—which cannot be said of every book on the subject.

The collection of exercises and examples is an unusually large one, suited to every requirement, while the model solutions are truly model in their methods and arrangement. His chapter on De Moivre's Theorem is more complete than is usually given in text-books, while his final chapter on the application of spherical trigonometry serves at once to show the student its use, and to give him a glimpse of several fascinating branches of mathematics.

Your critic is hardly justified in his claim that Professor Bowser has made several historical mistakes. It is unfortunate that Professor Bowser should imply that Napier was the inventor of what are now called Napierian logarithms; but surely he is right in saying that Briggs introduced the common system in 1615, since it is generally admitted that Briggs lectured on them in that year, though his tables were not published until two years later. And why your critic should object because Professor Bowser, in speaking of addition and subtraction logarithms, refers to Zech's tables, I fail to understand, since Zech's tables are equal if not superior to any others published.

Of course, only a class-room test can determine the merits of a text-book, but this latest book of Professor Bowser is so filled with the many qualities which have made his previous books so successful that I cannot see any reason why it should not meet with a like success. H. L. HODGKINS, Professor of Mathematics.

Columbian University, Washington, D. C., Jan. 5.

Humming-Bird's Food.

IN several recent numbers of *Science* there have been notices of the habit of *Trochilus colubres* feeding on the sap of different trees. I have also noticed the fact, and was interested on becoming acquainted with *T. anna* to find that it also made this a staple article of food during the summer and fall. In this part of California there are few trees yielding a sap save the cottonwood and willow.

During a mountain trip in August, 1890, I found the humming-bird very common in the willows along the creeks, at about 5,000 feet elevation; and was pleased to find that the red-breasted sap-sucker (*Sphyrapicus rufer*) filled the office of *S. varius* to the ruby-throat. The willow thickets were very dense and composed mostly of dwarfish shrubs of *Salix lariolepis*. I forced my way into the interior, and watched the birds; sap-suckers, humming-birds, and warblers (*Dendroica auduboni*), often waiting turns at a favorite drinking-spot; though possibly the latter were more interested in the insects attracted by the honey than by the honey itself. There were often three, and even four or five, humming-birds in sight at a time. They were very tame, and very curious; coming within three or four feet of me, poising themselves on their wings and looking me over. I noticed most of these were young, and that the adult males were quite shy.

Subsequently, while teaching at Dunlap, at about 3,500 feet elevation, I found the birds as late as December feeding in the same manner.

Irrigation seems to have an important influence on the habitat of this bird.

For three years I have lived most of the time in the southern half of Fresno County, in an open plain. For the first two years I saw but very few humming-birds, and never saw them feeding on the native flowers, no matter how showy they were.

Meanwhile, the water had formed a pond by sub-irrigation on the ranch, and the same variety of willow (*Salix lariolepis*), which in the valley forms a tree 40–60 feet high and 3–5 feet in diameter, had come in thickly and grown to about 15 feet in height. This fall I noticed many humming-birds about the place, and traced them to this pond.

I have never seen but one or two sap-suckers here, but I found the birds in great numbers feeding on the sap exuding from the wounds caused by a large borer, the moth of which, about two inches across the wings, colored black and white, was flying about in abundance.

I have not as yet found them feeding upon any tree save this willow. Maples are very scarce in the Sierras of this county, and the sap-suckers prefer willows to any other tree. I have not observed that the squirrels score the bark of trees here as in the east, so the seeming preference for the willow may be owing to lack of drinking-places elsewhere.

It would be interesting to know if the other species of this genus were addicted to the same habit. Who knows?

ALVAH A. EATON.

Riverdale, Fresno County, Cal, Dec. 26.

A Peculiar Fire.

In *The Ladies' Home Journal* for January is an account of a fire from gasoline that originated in a rather peculiar manner. A lady was cleaning a Brussels carpet with gasoline. She had cleaned about one-third of the carpet when she noticed one spot that looked a little dull and which must have a little more rubbing. She says, "I gave one quick, hard rub, the cloth in my hand ignited. There was a sort of a puff, and the flames went creeping all over the carpet I had cleaned." The explanation suggested was that the friction ignited the gasoline, but no suggestion is made as to whether that was caused by raising the temperature to a high degree as might ordinarily happen by friction or whether it was otherwise.

Some of my experience in the cold, dry climate of Minnesota has suggested a very plausible explanation for this accident, which seems surprising that such accidents are not more frequent. Our sleeping-room has an ingrain carpet from which we get marked electrical experiences. On a cold morning one can hardly take a step without being strongly electrified. By shuffling across the carpet, taking only two steps, I have many times drawn a spark one-eighth of an inch long. By taking a dozen shuffling steps and touching the water faucet I have several times drawn a spark nearly one-half of an inch long. Indeed, it is so common and so excessive that it is quite uncomfortable. I have several times thought seriously of getting up some arrangement for gradually dissipating the charge on one's body, so that we can avoid the unpleasant shock when using the water. It should be stated that this high degree of electrification is not an every-day experience, but it is very common when the thermometer in the room goes below 50° or 40° Fahr.

A similar experience is very common here when one is putting on a fur overcoat or one simply with a fur collar. The simple rubbing of the fur in putting on the coat will so electrify it that one gets a prickly sensation from the charge from the collar when it is turned up against one's neck. Quite frequently simply picking up a flannel undergarment will so electrify it that one hears a decided crackling. These experiences are very common here in Minnesota with the dry atmosphere, and are quite surprising to one accustomed to the more moist climate of New York of the sea-coast.

This experience suggests at once that the gasoline in the case above noted was ignited by an electric spark caused by rubbing the carpet. G. D. SHEPARDSON.

University of Minn., Minneapolis, Minn.

Electrical Phenomena on Mountains.

THE experience of Mr. Chariton and the relation of Mr. Stone, as given in *Science* Sept. 23 and Dec. 2, have a parallel in the account of a traveller in Italy in 1814, who is quoted in the volume of Inne's Telescope for 1827, under the heading of "Curious Effects of Electricity upon Mount Ætna," and from which I extract as under.

"June 2, 1814. Before midday two travellers were returning from the mountain, guided by Vicenza Carbonaro, one of the guides from Nicolesi. They had arrived in the Piano del Huga, when, expecting a hail-storm, they quickened their pace. Walking on frozen snow, Carbonaro was the most advanced of the party, he felt his hair stand on end, his forehead and the skin of his face felt benumbed, and he heard a hissing noise. He took off his cap and his hair became more bristled, and the whistling noise more powerful The traveller nearest to Carbonaro also heard a humming sound, and asked the guide what it was; he could not give any reason for it, and he stopped, supposing he was dizzy. In the meantime they approached each other and were pleased with the magic sound. The traveller turned to call his companion, who was at a little distance, and made a sign to him with his hand, the hand when raised produced a much stronger sound, so much so, that moving the fingers singularly modulated it. The traveller approached and heard the sound produced by the head and body of his companion, but, not having entered the current of electric air, his repeated attempts produced no sound. Finally, the three persons having joined, they experienced great pleasure, as with moving their fingers they produced the above extraordinary effect. In the meantime the hail-storm fell on them, and, being rather curious than erudite, they resolved to prosecute their journey downwards, without caring to make further investigation. Scarcely had they gone a few paces, advancing beyond the electric air, than the sounds ceased." GEO. CLULOW.

51 Belsize Avenue, Hampstead, N.W., London, Jan 2.

Maya Codices.

AS the controversy between Dr. Seler and myself has drifted into mere criticisms of each others' statements, and no serious attempt to test my interpretations or to show that they are incorrect has been made, I think a continuance on this line would be unprofitable. I therefore close it, on my part, by suggesting to students of the Maya Codices that it might be worth the trouble to test my interpretations by an attempt to apply them in deciphering other combinations. I also call Dr. Seler's attention to the fact, that, notwithstanding his firm belief to the contrary, there is a numeral designation with a cross *between* the dots in the bottom line of Dres. 46, — 2 *Kayab*. Moreover, it is precisely of the form shown in his Figs. 17, 19, and 20, *Science*, Jan. 6, 1893.

CYRUS THOMAS.

Washington, D.C., Jan, 16.

BOOK-REVIEWS.

Experimental Evolution. By HENRY de VARIGNY. London and New York, Macmillan & Co. $1.50.

THROUGHOUT the whole line of biological research the progress of advance has been from statical to dynamical science. The first study is always a study of facts of nature as they exist, of their relations to each other and of their history. Later follows the study of nature in motion accompanied by experimental work and an endeavor to modify the activities of nature. Already biologists have inaugurated the science of experimental evolution, and this book by De Varigny is designed to start biologists to the study of a new science which the author calls experimental evolution. This work consists of a series of lectures originally delivered by the author before the Summer School of Art and Science at Edinburgh. The author points out that while the various lines of biological research, embryological, paleontological and morphological, all point in the direction of evolutionary theory, they fail to be conclusive demonstrations of evolution, because no one of them shows us the process of evolution in action. Evolution is an inference from the facts, but not a demonstrated truth. There is needed as a final test experimental study in regard to the production of new species by process of nature. To the discussion of the possibility of this branch of experimentation, these lectures are devoted. The author first summarizes, in an extremely interesting fashion, the chief lines of fact which have been collected in connection with variations of animals in nature. Second, in a similar way, he summarizes and discusses variations which are known in animals under domestication. Third, he endeavors to show how these variations are under the influence of conditions; conditions of environment, conditions of heredity, conditions of interbreeding, etc.; and, last, he tries to point out how it may be possible in the future for the experimenter so to regulate these conditions of environment as to cause at will actual changes to take place in the structure and characteristics of animals and plants which may result in the not too greatly distant future in the production of new species and hence in the final demonstration of a doctrine of evolution. Although largely a compilation the work is withal interspersed with many new and interesting observations made by the author in connection with the subjects discussed, the changes in the structure and characteristics of animals brought about by the changes in conditions surrounding them. The series of lectures is extremely interesting and suggestive. It will be found to contain a most excellent summary of the important facts known in regard to variations and the conditions regulating variations in animals and plants, and it will also be found to be full of suggestions to guide further experiments in the future. The work perhaps shows some trace of lack of sufficient care and occasionally carelessness in quotations from the authors cited, but on the whole we must regard these lectures as an extremely valuable addition to our knowledge of the doctrine of evolution and possibly as a stepping-stone into a new department of investigation upon the doctrine of evolution. Especially important are they as opening a new field of research, which is so broad and yet so close at hand that there is opportunity for all to work therein with strong confidence in being able to obtain valuable results.

Text-Book of Elementary Biology. By H. J. CAMPBELL, M.D. London and New York, Macmillan & Co. $1.60.

THE last few years have seen the publication of several books on elementary biology, and those already published very satisfactorily fill the need felt by schools for such works. One can but wonder at the appearance of this new book by Dr. Campbell, especially when we see that it covers practically the same ground as some of the others and in no more satisfactory a manner. The book is entitled Introduction to the Study of Elementary Biology, but it certainly could never be used as such unless it were accompanied by a long course of lectures or by considerable assistance in practical work. The text is too condensed, the subject too crowded and everything is treated in too concise a manner, to be intelligible to a student who is beginning to study elementary biology. In some places the text is scarcely more than a catalogue of anatomical details perfectly unintelligible without a large amount of outside assistance. The book is divided into two parts, the first giving general biological truths and the second giving more detailed descriptions of a few types. The author advises the student to read the two parts together and not consecutively, a precedure which most students would be sure not to follow. The author also strongly advises a student to do a considerable amount of practical work in connection with the reading, but nowhere in the book does he give any directions for such practical laboratory work, any directions for obtaining material or for using it, so that a student would be utterly unable to work in the laboratory by the use of this book alone. In short, the book as an introduction is impracticable unless it is accompanied by considerable personal direction on the part of instructors. Seemingly this book is designed chiefly for medical students, or at least so one would judge from the apportionment of space allowed to types. Of 160 pages which are devoted to types, over 55 are taken by the study of parasitic worms including leeches, 31 more with the unicellular organisms, leaving less than 30 pages for all the rest of the animal kingdom, including invertebrates; perhaps the most curious apportionment of space to be found in any text-book. While for an elementary text-book it seems to be not usable, the work does contain an interesting summary of biological principles and facts which would be instructive and pleasant reading to a person already acquainted with elementary biology and wanting an outline summary of leading biological principles. For such a purpose the book many be recommended, and will be found readable and instructive.

Physics. Advanced Course. By GEORGE F. BARKER, Professor of Physics in the University of Pennsylvania. American Science Series. New York, Henry Holt & Co. 902 p. 8°.

THIS addition to the excellent series of scientific text-books published by Messrs. Henry Holt & Co. will be welcomed by teachers of physics both on account of Professor Barker's reputation as a teacher and as an investigator.

In the preface the author states that the progress which has been made in physical science within the past decade has com-

CALENDAR OF SOCIETIES.

Chemical Society, Washington.

Dec. 8.—Subject of Discussion, National Chemical Society Plans.

Jan. 12.—Ninth Annual Meeting. Officers elected: President, Dr. F. P. Dewey; vice-presidents, Mr. Cabell Whitehead, Mr. K. P. McElroy; treasurer, Dr. E. A. de Schweinitz; secretary, Dr. A. C. Peale; additional members of executive committee, Professor H. W. Wiley, Professor F. W. Clarke, Dr. Thomas Chatard, and Professor R. H. Warder. Papers were read as follows: On Some Old Vegetable and Animal Oils, by K. P. McElroy and W. D. Bigelow. An examination had been made of thirteen oils that had formed part of the exhibit at the Centennial Exposition of 1876, with the view of determining the effect of age. The conclusion reached was that age diminishes the iodine number of oils and fats but increases the ether and free acid members. On Some Problems of Physical Chemistry, by Robert B. Warder, who submitted the following as some of the open problems. 1. What is the real nature of matter in atoms and in molecules, in elements and compounds, and in the several states of aggregation? 2. How far can the properties of each kind of matter be exposed as a function of the atoms (or other constituents) of which it is composed? 3. What are the mechanical possibilities and limitations of chemical change? Subject discussed, What May We Hope to Gain from the Congress of Chemistry at Chicago Next August.

New York Academy of Sciences, Biological Section.

Jan. 9.—A. A. Julien, Suggestions in Microscopical Technique, including (*a*) a carrier of cover impressions (mycoderm blood), utilizing as clamps a coil of brass wire moulded in a phial. The same device with a platinum coil serves as a convenient staining phial for cover-glass preparations. (*b*) A suggested medium for mounting delicately intractile protoplasmic objects. (*c*) Devices for avoiding inclusion of air-bubbles in mounts. (*d*) Balsam-paraffine as a ring varnish. O. S. Strong, On the Components of Cranial Nerves of Amphibia. In the seventh a dorsal root was shown to pass off into brain, representing Ophthalmicus, Superficialis, Facialis, and Buccalis of fishes, and innervating the lateral sense-organs of the head. In vagus a root of similar internal origin passes into the R. laterales, innervating the lateral sense-organs of the body. Another component of the facialis is the fascicalus communies of Osborn, which was believed to represent the lobus vagi of fishes. This passes off into the palatinus and mandibulaies internus, innervating the mucous epithelium of the oral cavity; while in the glosso-phangugrus and vagus similar components derived from this fasciculus innervate in like manner portions of the alimentary canal and its appendages. The relation of the results to segmentation of head was discussed. N. L. Britton, A Review of the N. A. Species of Lespedeza, With Comments on the Eleven Native Species, Shown to be Divisible into Two Groups, (*a*) those producing both petalous and apetalous flowers, and (*b*) those in which the petalous flowers are developed. Of the two naturalized species, one, in the south-eastern part the United States, L. striata (shrug) H. and A., is a native of eastern Asia, appearing (about 1848) in Georgia.

Society of Natural History, Boston.

Jan. 18—W. M. Davis and students in geological field-work in Harvard University, Report on a Study of Glacial Sand-Plains in Eastern Massachusetts (illustrated by lantern slides).

Society for the Advancement of Science, Las Cruces, N.M.

Jan. 12.—C. H. Tyler Townsend, President's Annual Address: The Present Status of Science in New Mexico.

pletely changed its aspect, the physics of to-day being distinctly the science of energy. It is from this point of view that the text-book has been written, the classification which has been adopted being based on the most recent views of energy, considered as being ultimately a phenomenon of the æther. The fact is significant that more than half of the entire work has been devoted to æther-physics.

The introductory portion of the book considers, first, physical relations in general, and second, the laws of motion; the latter being discussed, first in the abstract, and second with reference to the action of force upon matter. Under mass-physics energy is first treated of as a mass-condition, and then work, as being done whenever energy is transferred or transformed; the subject of potential being developed as a consequence of mass-attraction. The properties of matter are next considered, including the modern views of its structure; and then follows the subject of sound considered as a mass-vibration. Under molecular physics the phenomena of heat alone are treated; the term heat being restricted, in accordance with modern usage, to molecular kinetic energy. Under the head of æther-physics are grouped: (1) æther-vibration or radiation, (2) æther-stress or electrostatics, (3) æther-vortices or magnetism, and (4) æther-flow or electrokinetics; following the classification so well set forth by Lodge. Radiation is considered broadly without any special reference to those wave-frequencies which excite vision and are ordinarily called light.

The ground covered is that which is usually traversed by students in the more extended courses in physics in our leading universities, colleges, and technological institutes.

The book is well printed and well illustrated, a colored magnetic map of the United States being given as a frontispiece. The text is interspersed with examples, and descriptions of illustrative experiments in smaller type. The metric system has been used throughout, and all the units employed are those of the C. G. S. system.

Although the preface only bears date of October 1, 1892, a second edition has already been issued.

AMONG THE PUBLISHERS.

A NEW edition of Haeckel's "The History of Creation," has been issued in Germany, and a translation, revised by Professor E. Ray Lankester, is just published in this country by D. Appleton & Co. The book is a popular exposition of the doctrine of evolution in general, and of that of Darwin, Goethe, and Lamarck in particular. The reviser calls it "a statement of the views of one of the most learned, experienced, and honored naturalists of modern times." It is issued in two volumes, with numerous illustrations.

— D. Appleton & Co. publish a "Dictionary of Every-Day German and English," by Martin Krummacher, Ph.D. In addition to the dictionaries proper, it contains lists of the most important technical terms, proper names spelled differently in the two languages, a sound-notation, an outline of grammar, and several pages of "travel talk" in parallel columns.

— Ginn & Company announce "A Students' Manual of a Laboratory Course in Physical Measurements," by W. C. Sabine, Instructor in Harvard University, to be published in February. The manual will contain an outline of seventy experiments in mechanics, sound, heat, light, magnetism, and electricity, arranged with special regard to a systematic and progressive development of the subject. The description of each experiment will be accompanied by a brief statement of the physical principles and definitions involved, and a proof of necessary formulae. That the manual may be of more ready and general service a set of apparatus has been designed which is especially adapted to the course and can be found complete on the market. The book is intended for use in supplementing college courses in physics.

Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

The undersigned has the following specimens to exchange for crystals of any eastern or foreign localities or Indian relics: tin ore, metacinnabarite, stibnite, garnierite, calenanite, hanksite, ulexite, rubellite, lepidolite, blue and green onyx, Cal. pineite, aragonite on chalcedony, cinnabar, double refracting spar, clear and clouded, and others. J. R. Bush, care of General Delivery, Los Angeles, Cal.

For sale or exchange.—A private cabinet of about 200 species of fossils, well distributed geologically and geographically. Silurian, about 40; Devonian, about 50; Carboniferous, about 80; others, about 30. Frank S. Aby, State University, Iowa City, Ia.

For exchange —Minerals, fossils, F. W. shells, land shells, native woods, Indian relics, two vols. of Smithsonian reports, odd numbers of scientific magazines, copper cents, etc., for good minerals not in my collection, good arrow- and spear-heads and natural history specimens of all kinds. Correspondence solicited with list of duplicates. G. E. Wells, Manhattan, Kan.

For sale or suitable exchange.—A spectrometer made by Fauth & Co., Washington, D. C., according to the plan of Prof. C. A. Young. This instrument is suitable for the most advanced investigations and determinations. Cost originally $700 and has been used but little. Will be disposed of at a considerable reduction. Address Department of Physics, Ohio University, Athens, O.

I will send British land and fresh-water shells in return for those of America, any part, sent to me. I have at present about fifty or sixty species, with many varieties. W. A. Gain, Tuxford, Newark, England.

The Biological Department of Hamline University desires to offer microscopic slides of animal tissues, or whole animals, in exchange for first-class fossils. Address correspondence to Henry L. Osborne, Hamline University, Hamline, Minn.

Wants.

WANTED.—American Journal of Conchology, seven volumes. Parties having these for sale will please address the undersigned, stating condition and price. R. Ellsworth Call, Louisville, Ky.

A GRADUATE ENGINEER will give instruction evenings in geometry, trigonometry and surveying, mechanics, physics, mechanical drawing and general engineering construction. Five years' experience in field and editorial work on engineering journal. References furnished. C. S. H., 102 Tribune Building, New York.

A POSITION is desired in the South, preferably the Gulf States, where I can teach the sciences Can also instruct in other branches. Salary only nominal, as I am simply desirous of employment while spending the winter in the South. A private family preferred, but will accept regular school work if not too confining. MORRIS GIBBS, M.D., Kalamazoo, Mich.

WANTED.—By well-qualified and experienced science master and associate of the Royal School of Mines, London, aged 26 (at present in England), a mastership in technical college or university for any of the following subjects: Engineering sciences, geology and mineralogy, physics, chemistry and metallurgy, etc., etc. Can provide excellent references and credentials. Apply, J. G., 17 Sussex St., Rochdale, England.

A GRADUATE of the University of Pennsylvania and a practical mineralogist of twenty years' experience desires to give his services and a cabinet of 25,000 specimens, all named, with about the same number of duplicates, in minerals, crystals, rocks, gems, fossils, shells, archæological and ethnological specimens and woods to any institution desiring a fine outfit for study. The owner will increase the cabinet to 50,000 specimens in two years and will act as curator. Correspondence solicited from any scientific institution. J. W. Hortter, M.D., Ph.D., San Francisco, Cal., General P. O. Delivery.

SCIENCE

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SCIENCE

NEW YORK, JANUARY 27, 1893.

THE NEW BOTANY.

BY LESTER F. WARD, WASHINGTON, D.C.

THIS is an age of new sciences; at least we have a new chemistry, a new astronomy, and a new geology. May we not have a new botany? The real science of botany is what we know of the origin and nature of plants. All other knowledge about plants is preparatory to this. Not only is this true of descriptive botany, which is merely taking off the slabs, as it were, but it is true of structural botany, even where this becomes histological. What has been the object of all the thorough and profound investigations of the German botanists? To show how the existing vegetation has become what it is and how the various kinds of plants are related to one another from the standpoint of real kinship. In a word, it is the development of plant life that constitutes the true science of botany. And think of the enormous labor and research that it has required to arrive at this through the study of the existing plants alone! Whether we consider the working-out of the anatomical structure of all the various types of vegetation in order to conclude from the different grades of tissue along what lines development has taken place, or whether it be the reproductive organs that engage attention, from the relationships of which the course of botanical evolution may be inferred, the task in either and in any case is immense and has properly engrossed the attention and absorbed the energies of the foremost students of that noble science. And it is proper that the great universities should have prominent chairs of botany to push on the solution of the still unsolved problems of the vegetable world.

But there are two routes that lead to these important results. There are two methods by which the development of plant life may be studied. The one I have outlined is what Huxley has so happily called "the method of Zadig."[1] The past is seen through the present and ancestral forms are inferred from the marks they have stamped upon their posterity. It is a true scientific method, usually the best that nature affords, and it has led us to the greater part of the knowledge we possess with respect to the evolution of world systems, of our own planet's history, and of the development of organic beings.

But far better than this method of "retrospective prophecy" or rational inference, wherever it can be applied, is the method of direct comparison. No one claims that the nature of a form can be reasoned out from no matter how complete a series of facts with the same certainty that it can be learned if it can be actually brought forward for direct observation. Yet this latter is the method of paleontology in all the departments of life to which it can be applied. In the animal kingdom this great resource is freely drawn upon, but in the study of plants it is almost entirely neglected. In all Europe I can only name one chair of botany, that of the University of Strasburg, which is occupied by one who has paid special attention to the paleontological side. In America there is none, and yet we have several able students of botanical evolution from the morphological side, who are doing excellent work. I will not be deemed invidious if I mention the thorough and successful researches of Professor Douglas H. Campbell of the Leland Stanford, Jr., University.

Why have we not equally competent men at work upon the ancient forms? It can no longer be said that the material is wanting. It exists in vast quantities and excellent quality. There have been already collected and not yet at all studied fossil plants enough to furnish employment for a corps of investigators during the balance of the present century. But what exists is nothing to what may be easily obtained. I could direct any one to hundreds of localities where a little labor would certainly be rewarded by abundant results. In nearly all the geological formations of the United States, from the Devonian to the Pleistocene, there exist rich beds of vegetable remains, as yet only slightly explored, which, if thoroughly developed and studied, would, with scarcely any doubt, throw more light on the evolution of our American floras than any amount of histological investigation of those floras themselves as we now find them could be expected to do.

Without going into details, and omitting entirely the Paleozoic floras, which, as every one knows, are very rich in America and have been chiefly studied, a glance at the Mesozoic and Cenozoic series may be of interest. It begins, so far as we now know the plant-bearing horizons, with the Upper Trias, but this, as I have shown,[2] is found in nine of the States and Territories of the Union, and has already yielded 119 species of fossil plants, sufficient to fix with great accuracy the geological position of the beds and show the general character of the vegetation that flourished on this continent at that remote period. We also know that extensive Permian deposits occur in the West, and there is hope that the interval between these and the plant-bearing Trias may yet be bridged over by the discovery of Lower Triassic forms.

We as yet know nothing of the Jurassic flora of America, unless the Trinity beds of Texas, the supposed Kootanie deposits of Montana, and the lowest Potomac strata of Virginia prove to reach downward into that system. But in these and the great series of clays that overlie them and seem to occupy the entire interval to the Laminated Sands of New Jersey, placed in the Upper Cretaceous, we have an immense period represented by successive plant-bearing horizons, and by scarcely any other remains of life, from which, at this writing, nearly a thousand different plant forms are known, with large collections still awaiting study. If to this we add the great Dakota formation of Kansas and Nebraska, we nearly double these figures, and have a Lower and Middle Cretaceous flora that compares favorably in its number and extent with that of the same areas at the present day.

Between this and the rich Laramie flora of the extreme Upper Cretaceous there is a newly-discovered plant-bearing horizon in the Montana formation, probably the equivalent of the Belly River series of the Canadian geologists, the flora of which is as yet very little known.[3] Of the Laramie flora I need scarcely speak[4] further than to say that all that has thus far been done is merely preliminary to the elaboration of the extensive collections that I have myself made in this vast store-house of facts bearing upon the history and nature of plant life on this continent.

Overlying the Laramie, or perhaps forming an upper member of it, and occupying wide areas west of the great plains, are other plant-bearing deposits, some of them now known as the Denver formation, others of more doubtful age embracing the Carbon and Evanston coal-fields of Wyoming, others farther north long known as the Fort Union group, and all taken together nearly or quite filling the interval from the recognized Laramie to the Green River group, about whose Tertiary age there has never been any question; and this last itself has entombed along with its beautiful fishes and with insects a great number of vegetable remains in an admirable state of preservation.

In Montana, about the sources of both the Upper Missouri and the Yellowstone Rivers, especially in the Bozeman coal mines

[1] Nineteenth Century for June, 1880, vol. vii., p. 929.

[2] Bulletin of the Geological Society of America, Proceedings, vol. iii., 1891, pp. 23-31.

[3] See the American Journal of Science for April, 1884, 3d Series, vol. xxvii., pp. 292-303.

[4] See my Synopsis of the Flora of the Laramie Group. Sixth Annual Report of the U. S. Geological Survey, 1884-85, pp. 399-557, pl. xxxi.-lxv.; also, Types of the Laramie Flora. Bulletin of the U. S. Geological Survey, No. 37.

and on the flanks of the Amethyst Mountain in the National Yellowstone Park, the series, probably beginning as early as Laramie age, is represented by an almost unbroken succession of plant-yielding deposits, extending upward into the Volcanic Tertiary, where the ruins of vast Sequoian forests mantle the slopes with their erect and prostrate trunks, among whose still persisting roots of stone lie buried in great profusion the more delicate parts, branches, leaves, fruits, and even flowers, of a rich and varied flora. Thousands of beautifully preserved impressions of these have been collected by Professor Knowlton and myself in two field seasons' operations, besides a most extensive series of the silicified wood, showing its internal structure as perfectly as if it were still living.

On the other side of the great continental divide, in California, Oregon, and Washington, there are Miocene and still later deposits, in which have been found the later floras of the continent, but whose extent can as yet only be conjectured. Even in Alaska there are great areas which have only to be scratched to make them tell of oaks and willows and a great number of vegetable forms that flourished there in late Tertiary time, the analogues of which are now only found in the latitude of the States and along the Atlantic border.

Is it possible that botanists care nothing for all this? Do they prefer to drudge upon the tissues of living plants to learn what may be known by actually confronting the witnesses themselves of the real character of the ancient vegetation of the earth and the true lines along which it has developed? It cannot be. And yet such would be the logic of their action. The truth is that institutions of learning, much like the masses of mankind, are the votaries of fashion. It is fashionable to found chairs of structural and physiological botany, and it is fashionable to occupy them and work out refined problems in the niceties of the science. Would there were no worse fashions! "These ought ye to have done and not to leave the other undone." The government has led the way, through its several geological surveys, in establishing the existence of these inexhaustible sources of botanical knowledge, but it cannot, and probably should not, sustain the careful and prolonged researches necessary to the solution of the many and important scientific problems that naturally grow out of such a mass of information. It can only use the data thus accumulated in the settlement of the geological questions involved, and in the development of the economic resources of the country to which they serve as aids. The purely scientific results belong to the higher institutions of learning to work out. It is true that only the great and well-endowed ones can conveniently undertake this work, but these are in condition to do so, and there is nothing that could reflect greater credit upon an American university. Such institutions make themselves a history by the original research they foster and not by their pedagogic achievements. A proper amount of teaching in the form of lectures growing out of laboratory work is useful to give precision to such work as well as to instruct, but it should never engross the energy of the teacher to the exclusion of the chief object, the advancement of science. In this case the materials are bulky and their collection and transportation expensive, yet several leading American colleges have frequently indulged in this part of the expense, and then, strangely enough, stopped there, and stored their cellars with undetermined material; or, if they have gone further, as at Princeton, and been to the expense of installing the specimens in their museums and employing a curator to take charge of them, they only cumber their shelves with unnamed and unknown objects, to be looked at as mere curiosities.

To set forth any detailed plan for putting these suggestions into practice would unduly prolong this article, but surely no one will claim that the prosecution of paleobotanical research is impracticable in a country that boasts of such universities as those at Chicago and at Palo Alto. All that is needed is that its importance be recognized; the task of reducing it to practice is only a matter of administration. The difficulty is to persuade educators to look to value instead of custom in the encouragement of research. The great energy that is devoted to small things is only less strange than the little energy that is devoted to great things, and a new and advanced spirit needs to be breathed into our higher education.

The new botany is not merely the study of plants from the paleontological side; it is their study from all sides and from all points of view, and a school of botany in a great modern university should no more limit itself to the facts that living plants present than a school of history should be narrowed down to the old method of recounting the deeds of kings, dynasties, and warriors, as constituting all of human history. The mere "determination" of fossil plants, although of course the most laborious part, is a comparatively unimportant part from the botanical standpoint. The great work is their affiliation. As I have shown, we have in America a succession of plant-bearing horizons not so widely separated in time but that the later forms may be in large degree affiliated upon the next earlier ones, so that, in the right hands, there is hope that something like a complete history of plant development may be ultimately worked out. No grander theme presents itself to the scientific world, and the time is ripe for its inauguration. Hitherto the study of fossil plants has been conducted wholly from the geological standpoint, and, as I have been obliged to insist,[1] this does not necessarily involve the correct systematic determination of fossil forms, provided their identity can be surely recognized wherever found. A new method is therefore loudly called for, by which far greater certainty than heretofore can be reached in establishing the real nature and affinities of extinct floras. In other words, they must be studied from the botanical standpoint and all the light brought to bear upon them that the known flora of the whole globe is able to shed. This is no simple task, it is one that demands the highest ability and the widest facilities. But thus pursued, with sufficient time, patience, and labor, its success is certain, and its value beyond calculation.

THE STRUCTURE OF INSECT TRACHEÆ, WITH SPECIAL REFERENCE TO THOSE OF ZAITHA FLUMINEA.

BY DR. ALFRED C. STOKES, TRENTON, N.J.

THE following paper has a threefold purpose. First, to confirm an important discovery made in this country, but, so far as I have been able to learn, never corroborated in any American publication. It was Professor George Macloskie of Princeton College who announced in *The American Naturalist* for 1884, page 567, that the so-called spiral threads of insect tracheæ are in reality chitinous folds of the membrane, and consequently tubules, which are longitudinally fissured. Professor A. S. Packard, in the same magazine for 1886, page 438, in a paper "On the Nature and Origin of the So-Called Spiral Thread of Tracheæ," says, "All the figures of the spiral thread hitherto published I believe to be incorrect," adding in a foot-note that "Thus far I find myself unable to agree with Professor G. Macloskie that the 'spirals of the proper tracheæ' are 'crenulated thickenings of the intima,' or that the tænidia are really tubular." Unless I have overlooked some more recent American contribution to the literature of the subject, this is the latest statement, with the single exception of a short note from Professor Macloskie himself published in a recent number of *Science*, in which communication his former conclusions are re-affirmed, as the result of another examination of the so-called spirals. But, although Dr. Packard does not accept these conclusions, he suggests the word "tænidium" as a name descriptive of the solid thread, as it is generally considered to be, a name which it may be well to adopt, but with a meaning somewhat different from that attached to it by its learned inventor, who considers the objects which the word describes "to be separate, independent, solid rings, more or less parallel and independent of each other, . . . usually thin, flat, but often concavo-convex, the hollow looking toward the centre of the tracheæ."

Some months ago my correspondent, Mr. Fr. Dienelt of Loda, Illinois, sent me a microscope slide of the tracheæ of the not uncommon aquatic bug *Zaitha fluminea*, for a purpose to be specially referred to hereafter, but one that had no connection with the structure of the tænidia; and, still more recently, at my request, Mr. B. F. Quimby of Chicago collected in Jackson Park, in that city, several specimens of the same insect and kindly sent them

[1] American Geologist, vol. ix., January, 1892, pp. 39-40.

to me, as I had observed on Mr. Dienelt's preparation certain structural points which together form the subject of this paper.

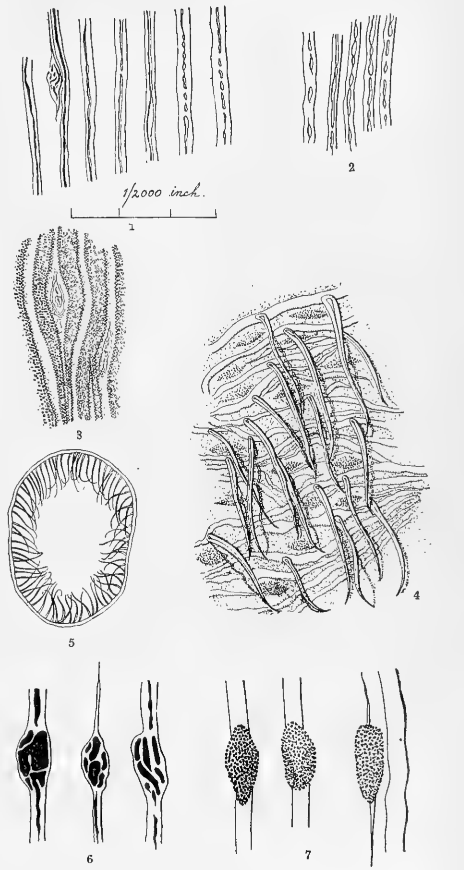
The tracheæ of the insect are large, and, as the tænidia are also comparatively broad, the entire collection of tracheal tubes, especially in the principal trunks, readily lend themselves to investigation. It is here an easy task to demonstrate that the tænidia are fissured tubules formed within and from chitinized folds of the intima, the convexity of the folds looking toward the lumen of the tracheæ, the fissure, as Professor Macloskie has observed, being directed away from that lumen. In balsam mounts, and perhaps somewhat more distinctly in glycerine preparations, under a wide angled, homogenous immersion $\frac{1}{12}$-inch objective, the irregular edges of the longitudinal fissure in each tænidium of the larger tracheæ can be seen and studied at the microscopist's pleasure; indeed, so well marked are they that they may easily be seen with Zeiss's apochromatic 6-millimeter objective, 0.95 N. A., and, in favorable circumstances, with Gundlach's dry $\frac{1}{8}$, N. A. 0.92. The appearances are in no way those of the diffraction phenomena produced by solid fibres, but rather an aspect which suggests the illumination of a hollow tube by reflection from its walls. The method of focussing which gives this bright band yields a picture of the edges of the fissures, with a more or less brilliant space between them. But by using the method employed by every well-informed microscopist when studying the secondary structure of the diatoms, a different appearance is obtained. The method is nothing more than a certain manner of focussing the objective, but one which produces the "black dot resolution" which has revealed so much of importance in reference to the intimate structure of those silicious plants. The "black dot" focus is as correct when applied to these minute tænidia as when obtained over the secondary membranes of the diatoms. With it the margins of the tænidial fissures are separated by a black space that defines them and every irregularity of the edges with convincing distinctness.

These margins are not parallel, a fact which alone would preclude the possibility that the appearances are diffraction phenomena. Neither is the fissure that separates them even in width. It is narrowed in indescribable ways by the approach of one margin toward the other, by a retreat from each other, and by wavy and more or less crenulated outlines. In some tænidia these margins have come in contact and have apparently been united, showing, in many instances, the point of union as an exceedingly slender line, whilst in others the juncture has been obliterated, and in still others the union has been accomplished in such a way that minute and irregular spaces have been left in the course of the original fissure, like little islands of darkness, or of brightness, according as the microscopist uses the black dot resolution, or focusses for the illuminated band.

In Fig. 1 are shown portions of several tænidia exhibiting the fissure, and, although the drawings were originally made with the camera lucida, they are necessarily somewhat diagrammatic. The one two-thousandth inch scale appended, is magnified to the same degree as are the tænidia, but is applicable to these particular drawings only, and not to the spaces between them. In the first two bands on the left-hand side the fissure is shown black, as I think it should be, whilst in the others it is left white, to exhibit rather more distinctly for this purpose the irregular margins and the irregular widths of the fissure. In the last two portions on the right-hand side, and in the five of Fig. 2, are delineated some of the various aspects assumed by the union of the edges of the fissures, and the formation of what may be called apertures in a chitinous bridge.

The fissures are so distinctly defined in ordinary preparations that sections of the tænidia are not necessary to show them. Such sections, however, are desirable, but to make them is an impossibility for me. The microscopist that nowadays tries to work alone and without a laboratory at his disposal, and without the refinements of microtomy and of photo-micrography, can do but little at a disadvantage. Yet when a tænidium is traced to the folded and flattened margin of a large trachea, in some instances the narrow, externally concave fissure can be plainly seen, although confusing diffraction effects must there be contended with. A longitudinal section of the tube, that is, a transverse section of these tænidia, would present a superb and convincing picture.

Not only do the tænidia of this special insect (*Zaitha flumineu*) clearly reveal the fact that they are longitudinally fissured tubules, but these tracheæ as clearly show that the so-called spirals are inwardly directed folds of the membrane. This is especially conspicuous near the spiracles, where the tracheæ, both on their internal and external surfaces, exhibit evidences of the fact. Near the spiracles the tracheal membrane is externally studded with minute papillæ, which become fewer and smaller as the distance from the spiracle increases. Here the intima is thrown into folds so shallow and so broad that they are often mere grooves, and with no specially



conspicuous deposit of chitin. Here too the margins of these shallow and groove-like infoldings are crenulated by the papillæ, which become more conspicuous as they are presented in profile on the edge of the furrow. An attempt has been made to show this appearance in Fig. 3, where are delineated three broad and incomplete tænidia, with the tapering termination, or the beginning, of another. Here they are only broad grooves, with no appearance of the narrow fissure of the completed tænidium, as it is now all fissure; in many instances these shallow depressions are even more irregular than shown in the figure. Near the spiracles they are sometimes hardly more than a collection of wrinkles in the crumpled membrane, as is delineated in Fig. 4, which is an attempt to exhibit a portion of the internal surface of a large trachea near the external orifice, the membrane being hardly more, so far as the tænidia are concerned, than a mass of wrinkles whose folds project into the lumen of the trachea and are scarcely chitinous, when in that respect they are compared with what, for the want of a better name, I have called the completed tænidia.

The second object of my paper is to call attention to certain tracheal appendages which were discovered by Dujardin as long ago as 1849, and by him referred to in a brief note published on page 674 of *Comptes Rendus* for that year. Since then they seem to have been almost forgotten. These are internal, chitinous, hair-like bodies arising from the fold of the tænidia and projecting into the lumen of the tubes. Dujardin gives a list of the insects in whose tracheæ he has seen these hairs, and remarks upon the evidence which they afford as to the external origin of the tracheal intima. A few scattered references to the observation may be found in the European literature of the subject, with absolutely none in this country, with the exception of one contribution by Dr. Henry Shimer of Mount Carroll, Ill., in an elementary microscopical magazine, with one or two made by me in the same journal, my hope being thereby to interest amateur microscopists in the matter, and one or two additional notes by Mr. Fr. Dienelt in a similar magazine (*The Observer*, Portland, Conn.), intended to accomplish the same purpose. This occurred within the last two years, and with these unimportant exceptions, internal tracheal hairs seem never to have been noticed by any American microscopist, although insects possessing them are not uncommon. My own first acquaintance with them was brought about through the courtesy of my correspondent, Mr. Fr. Dienelt of Loda, Illinois, who sent me a slide of the tracheæ from the common Colorado potato beetle (*Doryphora decemlineata*), calling my attention to certain appearances within them which he was at a loss to interpret. These proved to be produced by tracheal hairs similar to those discovered by Dujardin, and since examining that preparation I have seen the appendages in the tracheæ from the ovipositor of the common house fly, whilst Mr. Dienelt has observed them in several other insects; indeed, it was he who called my attention to their abundance and to their great size in the tracheæ of *Zaitha fluminea*.

Whether they are of any importance in the economy of the insect possessing them, it is of course impossible to do more than to conjecture. Dujardin has called attention to their use as evidence in regard to the external origin of the tracheal membrane, referring to them as epidermal appendages, analogous to those of the wings or of the tegument.

In the larger tubes of *Zaitha* these hairs are so abundant that the surface is villous with them. They gradually become fewer as the tracheæ ramify and grow smaller, until they entirely disappear from the finer divisions. They arise from the chitinous folds of the membrane, rarely from the intima itself between the tænidia, and extend obliquely into the lumen, their free extremities usually being directed toward the spiracle. They are hollow, their minute lumen communicating distinctly with that of the tænidium, to which they are attached, or from which they arise by an enlarged base. Their length averages about $\frac{1}{450}$ of an inch, although it is difficult to measure them with any accuracy, as they are rarely straight. The free extremity of each tapers to an exceedingly fine point, which is sometimes bifid, occasionally trifid. In Fig. 4 several are shown attached to the wrinkles of the tracheæ near a spiracle, and in Fig. 5 is exhibited a transverse section of a tube with the hairs projecting into its lumen.

The third and last of the points to which this paper is devoted is one which, so far as I have been able to ascertain, has not been previously observed as a part of the structure of any insect's tracheæ. These are certain minute, elliptical bodies in the tænidia, each with an internal, presumably glandular, appendage, to all appearance forming part of the tænidium from which it springs. Whilst these are numerous in the main trunks and in the larger branches where the hairs are abundant, they are more conspicuous and seem also to be more numerous in those that bear but few of these internal filamentous appendages.

The external bodies were at first supposed to be the remains of hairs which had been broken away in the preparation of the tracheæ for microscopical study, but further examination soon dispelled that illusion, as the objects differ widely from the bases of the hairs, which are only thick-walled circular openings. The enigmatical bodies are more or less elliptical or elongate-ovate in contour, no two being of precisely the same shape nor of the same size, although in size they are rather more constant, the diameter varying from $\frac{1}{5000}$ to $\frac{1}{8000}$ of an inch, the length externally being about $\frac{1}{3000}$ of an inch, or but little longer than the diameter of a human red blood-corpuscle. They are commonly in the tænidia, the lateral margins of the fissure within the latter separating to give them space, and they are perforated in the most irregular way, the small apertures varying in number and in form as the bodies themselves vary in shape, the openings occasionally being reduced to a single circular one. These objects are shown in Fig. 1, within the short tænidium beside the second on the left-hand side; in Fig. 3, where there are two in the broad, shallow folds of the membrane, and more in detail by Fig. 6.

Here again enters another application of the diatomist's black dot resolution which has made plain the structure of the secondary membrane of so many of those plants. In Fig. 6 the black dot resolution shows the perforations, which are always irregular in number and in form, with the space between the uneven edges of the tænidium, and, in the sketch on the right-hand side, the continuation of an aperture with the tænidial fissure.

These elliptical, cribriform bodies seldom occur on the tracheal membrane between the tubules. Occasionally they are seen to form the principal portion of a short, otherwise solid, tænidium, which to all appearance has been produced only to accommodate that special object. In such cases there is but one; usually a single tubule possesses several.

The perforations pass through the substance of the tænidium and are received, usually by means of a short pedicle, in a cushion-like, apparently glandular, body attached to the inner surface and projecting into the lumen of the trachea. In Fig. 7 are shown three of these glandular bodies, if they are glandular; and it is equally difficult to suppose that they are and that they are not. They appear under the microscope as collections of exceedingly minute, rounded apertures, which, in certain positions, may be seen to be continuous with narrow passages directed toward the pedicle, when that exists, and toward the external cribriform plate. Their structure in minuteness is comparable with the secondary structure of the diatoms, which I have so often mentioned, being as exquisite and as difficult to resolve, in this taxing the good qualities of the microscopist's best objectives. The thickness of the cushion-like objects is about $\frac{1}{8000}$ of an inch, a space capable of being occupied by much microscopic structure.

Although they do not commonly occur on the tracheal membrane between the tænidia, they may be found there, as shown in Fig. 4, where is delineated a portion of a crumpled region of the membrane near a spiracle, with a few hairs and with several of these problematical, presumably glandular, bodies scattered about ike so many islands in a sea of wrinkles.

What their function may be it is difficult to conjecture. Their position within the lumen of the tracheæ, and their connection with the external cribriform spaces, in no way simplify the problem.

Their presence, however, seems to add a unique scientific value to the tracheal tubes of *Zaitha fluminea*, to say nothing of microscopical interest. A microscopist, with a well-trained and intelligent microtomist to do his bidding, might be able to add much to our knowledge of the structure, not only of these apparently glandular organs of the pedicle and the perforated, elliptical objects, but of certain other regions of these remarkable tracheæ.

CURRENT NOTES ON ANTHROPOLOGY.—XXI.

[*Edited by D. G. Brinton, M.D., LL.D.*]

The So-Called Caucasian Race.

IN a paper which he presented to the Moscow Congress last summer, M. Ernest Chantre, well known for his profound studies in the ethnology of north-western Asia, enters a remonstrance against the erroneous use of the term "Caucasian Race," as synonymous with "White Race." I take the greater pleasure in seconding his protest, as in my "Races and Peoples" I discarded the term, and gave similar reasons as his own for denying its right to exist in ethnographic classification.

M. Chantre points out that it is demonstrable that none of the so-called Caucasian peoples ever lived in the Caucasus or can be traced to the Ponto-Caspian area. The study of local archæology proves that this tract was comparatively lately inhabited; that its occupants in early times, as to-day, had no ethnic unity, but were the *disjecta membra* of various stocks, who fled to these mountain fastnesses as asylums; that they are without linguistic or somatologic connection; and that the only proper use of the term is to apply it solely to the tribes occupying the main chain of the Caucasus, tribes who have no historic or ethnic identity with any others outside this area.

Yet so slowly does a correction of this kind penetrate popular science, which is nearly always made up at second or third hand, that the term "Caucasian race" will probably survive in school geographies and encyclopædias for a generation to come.

The Unity of Religious Conceptions.

The curious similarity between the myths and other religious conceptions of nations far asunder in space and kinship has often impressed students, and has been explained in a variety of ways. An instructive comparison of the early Semites and the Indo-Germanic nations in this respect is given by Dr. W. Schwartz in the *Zeitschrift für Ethnologie*, 1892, heft. III. He shows that there is "a whole cycle of mythical conceptions and narrations which are common to Indo-Germanic and Semitic peoples." The books of the Old Testament are a rich mine of such. Some of these can hardly be explained otherwise than as direct borrowing, or, as our author prefers, slightly varied versions from some original stock of conceptions belonging to a common ancestry. He is inclined to consider that the conservation of myths and religious notions is stronger than that of language even. He scarcely seems to allow enough latitude to the fact that certain impressions, which are the same everywhere, are likely to evoke similar expressions of the religious sentiment.

The article is an interesting contribution to the science of religion, and shows a proper understanding of its meaning; being, in this, singularly in contrast with the printed circular issued by those Chicago luminaries who represent the "Department of Religion" in "The World's Congress Auxiliary of the World's Columbian Exposition." This astonishing body has summoned a congress of teachers and members of all faiths, "to indicate the impregnable foundations of theism, and the reasons for man's faith in immortality;" blandly and densely unaware, it would appear, that one or both these dogmas are absent as religious elements in many highly-developed religions! What a spectacle for the world of science!

On Demographic Neurology.

In Dr. Rockwell's letter about the relation of nervous diseases and civilization (*Science*, Dec. 30), he advances several very judicious observations on their prevalence in the United States, though disagreeing with me entirely on the general thesis. As Dr. Rockwell is aware, this is by no means the first time that I have joined issue with him and his friend the late Dr. Beard, on this subject. I shall not renew this discussion, which was carried on in various medical journals, but would ask the attention of readers who would like recent information on the subject to an article by Dr. Irving C. Rosse, professor of nervous diseases in the Georgetown Medical College, which appeared in the *Journal of Nervous and Mental Disease* for July, 1891.

It is entitled "The Neuroses from a Demographic Point of View," and, apart from its medical value, is interesting to the ethnologist as a contribution to comparative nosology. From quite an extended collation of authorities, he shows that there is as much, if not more, nervous disease in low stages of civilization and inferior races than in those which are higher. In the District of Columbia, for example, the decedents among the colored people from nervous diseases often exceed those of the white population thirty-three per cent. Dr. Rosse is inclined to believe that a sudden change in the social habits and condition of any race, at any stage of advancement, will result in a prompt development of neurotic disease. A high civilization, which is stable, excites such a condition less than instability in lower grades. This seems very reasonable.

Ethnography of the Picts.

It used to be taught that the Picts, who once inhabited portions of northern Great Britain, were so called from the Latin *pictus*, painted, because they colored themselves with woad and other paints. They were believed to have been Celts, and linguistically allied to the Welsh.

Both these opinions have been challenged. Their name is a Latinized form of Gaelic *peht* or *peght;* and from the sparse fragments of their tongue preserved, scarcely anything more than lists of kings and names of places, it is quite possible that it belongs to an allophyllic stock.

Their material remains are believed to be the numerous earth-houses or *weems,* found in the Orkney and Shetland Isles, and in many parts of Scotland near the seashore. An excellent description of these has recently been privately printed at Edinburgh by David MacRitchie, under the title "The Underground Life." Many of these subterranean dwellings have been carefully explored by archæologists; but the results it must be said are disappointing. Few objects referable to the culture of the Picts proper can be discerned. The ancient notion that they were an undersized people seems borne out by the narrowness of some of the passages. They are not over four and a half feet high, and two or two and a half feet wide. The walls are of stone and sometimes also the roof. The *weem* is sometimes below the level of the soil, sometimes above it, and is then covered with a mound. Mr. MacRitchie gives a number of plans and illustrations. In the Hebrides these *weems* were inhabited as late as the close of the last century by a class of predial slaves of debased condition, called *sgalag*. Perhaps in this word is to be found the much-sought-for original of our colloquial term *scalawag*.

The Craniology of Spain.

Two meritorious authors, Luis de Hoyos Sainz and Telesforo de Aranzadi, published last year an excellent survey of Spanish craniology under the title "Un Avance à la Antropologia de España." In text, maps, and tables, it displays the results of the examination of a number of series of skulls obtained from most of the provinces of Spain. The conclusions are drawn with calmness and under the proper reserves on account of the material from various areas being incomplete.

These conclusions point to the presence in prehistoric times of an "indigenous primitive race," characterized by dolichocephalic, leptorhinic skulls. These became modified by a series of invasions; first, of a brachycephalic people in the north, whom our authors identify with the Celts; then certain sub-dolichocephalic, leptorhinic peoples, supposed to be Visigoths, Suevi, and "Blond Tamau from Africa"; finally certain later Berber and Moorish hordes, which are described as dolichocephalic and platyrrhinic; though the Berbers in the latter respect have the same index as the average Londoner and Parisian to-day, that is, between 46 and 47.

The most interesting point of the discussion, that which is peculiarly the duty of Spanish craniologists to decide, namely, as to whether the primitive stock was identical in osteology with the Basques of the Pyrenees, is left unclear. The fact is, he would be a daring anthropologist who would positively say what the Basque type of skull is. The assertion of Quatrefages, that it is the *tête de lièvre* shape, has now no supporters in Spain. The evidence has proved inconclusive, and with it falls the theory that the Portuguese kitchen-middens are of Basque origin, as it was on such skulls that the theory was based.

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NOTES FROM THE CORNELL INSECTARY.

BY M. V. SLINGERLAND, CORNELL UNIVERSITY, ITHACA, N. Y.

II. — Some Observations Upon Plant-Lice

More than three-quarters of a century ago Kyber, a German observer, published under a similar title the results of experiments which not only confirmed the earlier observations of Bonnet and De Geer, but which also threw increased light on the generation and development of Aphids. Kyber found, for instance, that by keeping the insects in a warm room a series of agamic generations was produced which extended through four years without the intervention of the sexual forms. However, no record was kept of the number of generations produced during the time. In 1779, thirty-six years before Kyber's experiment, Bonnet had carried *Aphis sambuci* through nineteen generations without commerce with the male insect. From these and similar experiments a law has been educed, which we dare not deny, that "under certain circumstances a female Aphis may, without coupling, continue propagating to infinity, provided that the necessary conditions for the development of young — food and heat — are not wanting."

April 2, 1890, nearly two years and ten months ago, the writer isolated a nymph recently born of a wingless agamic female plant-louse, and since that time the experiment has been continued and is now in progress in the green-house at the Insectary of the Cornell Agricultural Experiment Station. The insect is probably *Myzus achyrantes* Monell, one of the "green-flies" of florists, which is always to be found in considerable numbers upon the varieties of *Achyrantes* grown in the Insectary; at one time it also attacked buckwheat and radishes growing in the green-house.

The experiment has been conducted in the following manner: The first nymph was isolated by placing her on a small plant known to be free from Aphids, and this plant was kept in a cage made by placing the flower-pot in a large saucer partly filled with sand, and then placing over the plant and pot a glass cylinder, which sank for a slight distance into the sand, and was covered with Swiss muslin at the top. The nymph was examined daily, and when she became a mother two or three of her young would be isolated in similar cages. When these daughters of the original mother became mothers themselves, their young would also be isolated, and so on. Not all of the young of each parent were allowed to live, and sometimes, to save cages, a mother would be removed to give place to one of her daughters; usually, however, a mother would not be destroyed until some of her daughters became mothers, for it frequently happened that some of the nymphs first isolated died before becoming mothers. In some cases the mother was left undisturbed, the young being removed and counted every day or two, to see how long she would live, how many nymphs might be born of a single mother, and whether there was any diminution in the reproductive power as generation after generation passed by without the male element again entering into the case. Great care has been taken to insure the isolation of the nymphs; when there was any doubt as to the pedigree of the nymph, she was immediately replaced by one of known pedigree.

Nothing but wingless agamic females have thus far been produced in the cages. Winged forms are sometimes seen, in April especially, on the other *Achyrantes* plants in the Insectary. The following table has been prepared to show the number of the generations, the rapidity with which they have been produced, and many other interesting points which have been brought out during the two years and ten months that the writer has cared for these little creatures — my pets, as I call them.

Generations.	When Nymphs were Isolated.	When Motherhood Began.	Age When Reproduction Began.
1	2 Apr. 1890	14 Apr. 1890	12 days
2	18 " "	30 " "	12 "
3	6, 9 May "	18, 28 May "	12 and 19 "
4	26, 28 " "	6, 11 June "	11 " 14 "
5	25 June, 9 July "	12, 20 July "	17 " 11 "
6	12, 13 " "	26, 31 " "	14 " 18 "
7	29 " "	8 Aug. "	10 "
8	8, 9 Aug. "	18 " "	10 " 11 "
9	20, 21 " "	3, 1 Sept. "	14 " 11 "
10	1, 5 Sept. "	13, 15 " "	12 " 10 "
11	18 " "	17 Oct. "	29 "
12	17, 27 " "	15, 10 Nov. "	29 " 14 "
13	10, 13 Nov. "	11, 8 Dec. "	31 " 25 "
14	8, 11 Dec. "	12, 5 Jan. 1891	35 " 25 "
15	5, 12 Jan. 1891	26, 28 " "	21 " 16 "
16	26 Jan. 2 Feb. "	14, 16 Feb. "	19 " 14 "
17	14 " "	27 Feb., 4 Mar. "	13 " 18 "
18	27 Feb. 4 Mar. "	20, 23 " "	21 " 19 "
19	20, 30 " "	10 Apr. "	21 " 11 "
20	6, 8 Apr. "	16, 20 " "	10 " 12 "
21	16, 21 " "	27 Apr., 5 May "	11 " 14 "
22	27 " "	10 " "	13 "
23	10 May "	26 " "	16 "
24	27 " "	10 June "	14 "
25	10 June "	22 " "	12 "
26	24 " "	10 July "	16 "
27	10 July "	21 " "	11 "
28	21 " "	30 " "	9 "
29	31 July, 14 Aug. "	11, 24 Aug. "	11 " 10 "
30	24 " "	5 Sept. "	12 "
31	5 Sept. "	18 " "	13 "
32	23 " "	5 Oct. "	12 "
33	5 Oct. "	27 " "	12 "
34	27 " "	11 Nov. "	15 "
35	14 Nov. "	30 " "	16 "
36	3 Dec. "	14 Dec. "	11 "
37	22 " "	4 Jan. 1892	13 "
38	4 Jan. 1892	23 " "	19 "
39	23 " "	10 Feb. "	18 "
40	10 Feb. "	24 " "	14 "
41	24 " "	10 Mar. "	15 "
42	10 Mar. "	25 " "	15 "
43	25 " "	5 Apr. "	11 "
44	5, 12 Apr. "	20, 25 " "	15 " 13 "
45	20 " "	5 May "	15 "
46	5 May "	13 " "	8 "
47	13, 16 " "	25 " "	12 " 9 "
48	25 " "	4, 7 June "	10 " 13 "
49	5 June "	22 " "	17 "
50	22 " "	4 July "	12 "
51	4 July "	13 " "	9 "
52	13 " "	22 " "	9 "
53	18 Aug. "	1 Sept. "	14 "
54	1, 6 Sept. "	13 " "	12 " 7 "
55	16 " "	26 " "	10 "
56	26 " "	7 Oct. "	11 "
57	7 Oct. "	22 " "	15 "
58	22 " "	7 Nov. "	16 "
59	7 Nov. "	21 " "	14 "
60	21 " "	6 Dec. "	15 "
61	9 Dec. "	27 " "	18 "
62	27 " "	17 Jan. 1893	21 "

Let me point out a few of the most interesting facts to be gleaned from an examination of the above table. Sixty-two generations have been produced thus far, or nearly two (1.8) generations a month on an average. The extremes were in the 13th and 14th generations, when it took a month for a generation to develop; and in the 46th, 47th, and 48th generations, which were all produced within a month.

This difference was due to the fact that the plants had become old, stunted, and pot-bound when the 13th and 14th generations of the Aphids were produced, while in the other case the plants were young and vigorous. It was also found that this difference in the plants produced not only the retardation of development of the Aphids, but there was also a very marked difference in their size and reproductive power. On these stunted plants it takes from two to three times as long for the nymph to develop, it does not attain more than one-third the size, and less than one-third as many young are produced as there would be if the nymph were reared upon a vigorous young plant. I have seen mother Aphids, on old plants, which were not larger than nymphs after their second moult on young plants. In the 12th generation this point was well illustrated. Here we have the record of two nymphs, the first, isolated Sept. 17 on an old pot-bound plant, did not become a mother until Nov. 15, or 29 days after; while a nymph, isolated 10 days later on a young, vigorous plant, attained motherhood in 14 days. The 19th generation presents a similar case. Nymphs born of these dwarfed and retarded mothers and placed on young plants have become normal-sized mothers in from 15 to 18 days in my cages.

In one instance (54th generation) a nymph became a mother in 7 days, while one of the 14th generation was 35 days in attaining the adult stage. I believe, that by carefully watching the Aphids and always isolating the first nymphs born upon young, vigorous plants, that at least thirty generations of this *Achyrantes* Aphis could be produced in a year. In 1890, Mr. W. J. MacNeil, while studying a black chrysanthemum Aphis, at the Insectary, reared, in 13 months and 5 days, thirty-two generations of the insect, all agamic wingless females. As the table shows, I have reared from twenty to twenty-five generations of the *Achyrantes* Aphis in a year.

As the experiment progressed, many other interesting facts were learned which could not be included in the table. I will now briefly discuss some of them.

The mother Aphids were often caught in the act of giving birth to a daughter. The operation required about five minutes, and in every instance the nymph was born tail end first in a thin transparent sheath or pellicle. Before being entirely delivered from the mother, however, the nymph begins to work the pellicle off; the antennæ and first pair of legs are freed about the same time, then follow the remaining legs and the honey-tubes, and the pellicle appears as a minute whitish mass about the tail of the nymph. The nymph remains attached to the mother until its appendages are free and the little creature is able to stand alone.

There seems to be no published record of the young of wingless agamic female Aphids being born in a pellicle as just described. Buckton gives five or six instances in as many genera where the young of winged agamic females are born thus. I believe, however, that this manner of giving birth to their young is as common among the wingless as among the winged agamic forms of Aphids. I have observed it many times in the case of *Myzus achyrantes*, and several times in the field among other common species. Mr. MacNeil showed it to be true of the black chrysanthemum Aphis; and Mr. W. E. Rumsey, while studying the woolly apple-louse, *Schizoneura lanigera*, here at the Insectary, watched under a compound microscope one of the wingless agamic females giving birth to a daughter, and there was no doubt that the nymph was born in a pellicle. This last case is contrary to the observations of Mr. L. O. Howard as published in Comstock's Report as U. S. Entomologist for 1879, p. 259; but the fact remains that a wingless agamic female of *Schizoneura lanigera* here at the Insectary has been clearly seen in the act of giving birth to several nymphs, each enveloped in a pellicle.

The nymphs begin to suck the sap of the plant very soon after birth, and as they increase in size moults occur. The minuteness of the insects and the delicacy of their cast skins renders the observation of the numbers of the moults very difficult. I worked nearly five months before I satisfactorily settled the fact that the *Achyrantes* Aphis moults four times during its lifetime. My method was to use a small plant with a few leaves and place a piece of stiff black paper close around the plant on the surface of the soil. This was necessary, as the delicate white cast skins frequently fell from the plant and would have been easily lost unless this smooth black surface had caught them.

The records of four nymphs of the 7th, 9th, and 15th generations show that the first moult occurs from 3 to 4 days after birth; the second from 2 to 5 days after the first; the third from 1 to 3 days after the second; and the fourth from 3 to 5 days after the third. In one instance, when the growth of the nymph was retarded by a stunted plant, its moults occurred about one week apart. It requires from 15 to 20 minutes to complete a moult.

The nymphs of a black chrysanthemum Aphis and of *Schizoneura lanigera* also moulted four times, as recorded in the theses of Messrs. McNeil and Rumsey. *Pemphigus filaginis* and *Tetraneura ulmi* are also recorded as moulting four times; thus four seems to be the normal number of moults among plant-lice.

Under the more even temperature during all the seasons in a green-house, plant-lice there do not show such a wonderful fecundity and rapidity of production as has been recorded from field observations. The table above shows that the seasons have no material effect upon the rapidity with which the generations are produced in a green house.

To ascertain whether the fecundity of the Aphids diminished through the successive generations of the agamic females, I counted the number of nymphs born of a single mother in several instances. During the 1st and 2d generations, 37 nymphs were born of a single mother. In the 3d generation, 3 to 4 nymphs were born each day of a single mother for 14 days in succession. A mother of the 18th generation lived 63 days and gave birth to 59 young. In the 20th generation a mother gave birth to 62 daughters in 19 days, or at the rate of three a day. Sixty-one nymphs were born of a mother of the 35th generation in one month. A mother of the 46th generation gave birth to 15 young in 3 days. Fifty-four daughters were born to a single mother of the 41st generation. And in the 54th generation a mother gave birth to 55 young. It is thus seen that the reproductive power of the agamic females has not decreased through nearly 60 generations.

Mr. MacNeil had one wingless agamic female of the black chrysanthemum Aphis which gave birth to 70 young in 34 days; at one time 7 were born in 27 hours. Mr. Rumsey reared in one instance 68 nymphs in 65 days from a wingless agamic female of *Schizoneura lanigera*; this female lived 12 days after the birth of the last nymph, and was nearly three months old when she died. From another female Mr. Rumsey reared 86 young in 55 days. Several of the agamic females of the *Achyrantes* Aphis have lived two months after becoming mothers.

To learn whether winged females might not be produced if the plants became overcrowded with the Aphids, I allowed, in several instances, reproduction to go on undisturbed in the cages. Several hundred wingless females would accumulate on a small plant, and possibly winged forms might have been forced in time if in each instance the overcrowding had not been checked by a fungous growth, which set in and destroyed a majority of the insects.

Many volumes have been written upon the habits and life histories of plant-lice; enough has been written upon the grape Phylloxera alone to fill a small library. And yet we have much to learn about plant-lice. I believe they present as varied, peculiar, interesting, and wonderful phases in their habits and life histories as do any other insects.

THE EXTREME HEAT AND COLD ENDURED BY MAN.

BY THE MARQUIS DE NADAILLAC, PARIS, FRANCE.

THE exceptional faculties of Man enable him, alone of all the mammals, to battle with extreme cold as with extreme heat, and it is with real astonishment that we ascertain what men of our race can endure. In the earliest times of which we have any knowledge, we have strong evidence that our species lived, both in America and in Europe, when large extents of both continents were covered with ice and when his companions were the elephant and the woolly rhinoceros. Later, the Aryan race, whatever may have been its birthplace, reached step by step in the south the Gangetic Peninsula, 8° only removed from the equator, and, in the north, Iceland and Greenland, which seem the extreme points attained by our most prolific race in those days so distant from ours.

A few years ago the English and Russian officials assembled at Maruchak for the delimitation of Afghanistan suffered a mean temperature of $-20°$ C., which was considered moderate in those regions. In his eventful journey across the mountains of Central Asia, utterly unknown to us, Prince Henry of Orleans had to support a cold of — 40° C. (mercury is congealed at — 29° C.; alcohol alone, highly rectified, can mark the low temperatures we give here), with piercing northern winds. The horses and camels died; man resisted.

The northern parts of America have known still more severe colds. Captain Back reported at Fort Reliance—56.74° C., and Captain Dawson, at Fort Rae, in 62° 30′ north latitude, —67° C., in April, 1882. Other explorers have never observed such low temperature. The Abbe Petitot gives us — 40° C., as the mean temperature of January at Fort Good Hope, and — 35° C. for January, and — 42° C. for February, at Yukon, Alaska.

In Siberia we find the coldest points inhabited by comparatively civilized men. In the government of Yenissei, the winter time is double the summer time. Autumn sets in in August, and the Yenissei River is completely frozen by the month of October. Yakoutsk was long considered the coldest town of the world. During the winter months the thermometer is as low as — 45° C. But Yakoutsk must yield to Verkhoyansk, a small Siberian town at the mouth of the Lena, where we find — 55° C. in January. And yet this cold is far from being the most severe suffered in those dreary regions. A Frenchman, Mr. Martin, recently dead, travelling in Eastern Siberia, wrote to the Society of Geography, of Paris, that he experienced in 59° north latitude and 132° east longitude a cold of — 63° C.

Physical phenomena, the differences in the relation of the continents and the oceans, have a greater importance than was suspected some years ago. Yakoutsk, which I have just mentioned, is only 6° nearer the pole than Edinburgh, and numerous arctic islands are on the same latitude. Yet Edinburgh and these islands enjoy a much warmer climate, thanks to the Gulf Stream, so well studied by Lieutenant Maury, one of the glorious scientists of our day.

This is probably the cause that some of the polar lands do not always experience the extreme cold we find in some parts of Siberia. Captain Nare's careful observations in Grinnell Land, in 1875–6, only give for January — 36° C., for February — 38° C., for March—39.90° C., for November—27.12° C., for December—36.6° C. Nordenskjöld, in one of his latest voyages, speaks of — 47.7° C. We have still higher records. Lieutenant Greely, in his illfated expedition, tells us that during his long stay at Discovery Bay the temperature maxima never exceeded + 50° (Fahrenheit) and was at one time as low as — 66° F. This difference of temperature, supported in a few months time by the same men, is most remarkable. Hunger, dearth of provisions, incredible hardships broke down those who had so bravely suffered extreme cold.

Nothing daunted by the cruel fate of Lieutenant Greely's companions, Lieutenant Peary tried, in his turn, to attain the solution of the northern problem, and, with a courage which does infinite honor to her sex, Mrs. Peary elected to accompany her husband. They wintered, in 1891, in MacCormick Bay, about a hundred miles distant from the great Humboldt Iceberg, and lived for three months under a temperature varying from —30° C. to—50° C. without experiencing any very great inconvenience. It is Lieutenant Peary, if I make no mistake, who approached the nearest to the Pole. He got farther than Frederick William's Land and Cape Bismarck, the extreme northern points reached before him.

In one of the last polar expeditions attempted by the English, in the month of November the thermometer marked — 60° C., and on the 25th of January it went down to — 63° C. on board the "Varna" and the "Dymphna," blockaded in the ice.

But probably the highest amount of cold ever suffered by white man is the one recorded by Mr. Gilder, a reporter of the New York *Herald* attached to the expedition which, under command of Lieutenant Schwatka, went in search of Franklin. In the letters sent home during the winter of 1879–80, so severe in all parts of the world,[1] he speaks of the thermometer lower than — 71° C. Here again we find men of our race supporting an almost incredible amount of cold from November, 1879, to March, 1880. Their power of endurance may be attributed to their stay at Camp Daly from August, 1878, to March, 1879. They experienced there a range of temperature from +14° C. to —51° C. The members of the expedition had adopted the way of living of the Innuits. Like them, they fed on the raw flesh of the seals and the walrus and absorbed large quantities of oily and fatty matters which prevented the spread of scorbutic diseases, so fatal to many of their predecessors. The tents were rapidly discarded and replaced by *iglous*, the native winter houses of hard frozen ice, which, curious enough, offer a considerable amount of heat. Their clothes were made of reindeer skin without any linen underclothing, so as not to put a stop to perspiration.

Another day I will compile the highest amount of heat supported by men of the white race. I will only mention here that in Algeria, by no means the hottest point of the globe, our soldiers have often seen the thermometer as high as + 51° C., and Mr. Buveyrier, in his travels amongst the Touaregs, noted + 67.7° C. If we compare this extreme heat (and we will certainly find higher points) the difference between — 71° C., recorded in the Schwatka expedition, and + 67.7° C. reach nearly 138° C., and testify, as I said in the beginning, to the remarkable power of endurance of the white race.

[1] As a comparison, I give the lowest temperature experienced in Paris during the last century. January 20, 1788, — 21.5° C.; January 25, 1795, — 23.5° C.; December 9, 1871, — 21.3° C.; December 10, 1879, — 23 9° C.

BEZOARS.

BY ELIZA BRIGHTWEN, GREAT STANMORE, ENGLAND.

THE almost fabulous value set upon Bezoars in olden days, and the medical virtues often attributed to them, invest these concretions, which are found in the alimentary canal of animals, both wild and domestic, with a certain amount of interest; and, although belief in their curative power has long since passed away, it may be deemed worth while to try and put together a few items about their history and uses.

The name of Bezoar appears to be derived from the Persian *pād* (expelling) and *zahr* (poison), in allusion to the supposed virtues of the stone as a remedy for snake-bites and other wounds. Others again derive it from the name of the goat in which one variety is found.

These stones were introduced as medicines in the East by the Arabian physicians in the tenth century, there seems to be no mention of them in Greek or Latin authors, but from the East their use gradually spread into Europe. They are referred to by Frampton as far back as 1580, and as late as 1746 these stones were in use in England, being found in the London Pharmacopœia of that date. A severe blow to their reputation was administered by Ambrose Paré, who gave a dose of Bezoar to a criminal condemned to death and to whom arsenic had been given, death, however, was the result.

In the Royal College of Surgeons' Museum in London cases may be seen filled with all the various kinds of concretions which have been found in the intestines of different animals, including some very fine bezoars.

They may be roughly divided into six classes:—

1. Balls composed of animal hairs.
2. Those composed of vegetable hairs.
3. The Oriental Bezoars, composed of ellagic acid.
4. The Occidental Bezoars, formed of resin or bezoardic acid.
5. Concretions of phosphate of magnesia, ammonia, and earthy calculi.
6. Ambergris, found in the intestines of the whale.

We will briefly notice facts relating to each of these classes.

I. Animals, especially horses and oxen, are much given to licking each other and themselves, and the loose hairs being swallowed become felted into spherical balls of various sizes, generally black in color, with a hard, shiny surface, which often consists of phosphate of magnesia.

In the College of Surgeons' Museum there is one such hair-ball, taken out of an ox at Buenos Ayres, which measures forty inches in circumference, and one of oval shape, found in a peccary, measures six inches by four in diameter.

II. Vegetable hair concretions are usually formed round some nucleus, such as a horse-nail, plumstone, or a piece of flint.

The setæ of the oat seem to have a constant tendency to form into spherical balls, and when felted they sometimes alternate with layers of phosphates, so that when divided the transverse sections of these are found to be marked by concentric lines.

III. The true Oriental Bezoar is found in the wild goat of Persia (*Capra œgagrus*), and is brought to India from the Persian Gulf. In appearance it is black and hard, oval in shape, with a smooth surface, which has a peculiar shiny lustre.

This stone consists entirely of ellagic acid, which is an insoluble organic acid derived from certain constituents of the diet of the Persian goat. This acid can also be extracted from an infusion of gall-nuts when exposed to the air.

Bezoars were frequently set in hoops of gold or silver, having a chain of some metal by which they were suspended in the liquid to which it was desired they should impart their curative virtues.

Koemfer says: "In Persia all people of consequence possess one or more of these stones preserved with great care as valued treasures." A proof of their value is found in the fact that amongst the treasures sent to the Emperor Napoleon the First, by the Shah of Persia, were three Bezoars valued at nearly two hundred pounds.

Five hundred crowns (£125) have been given for one such stone, and Tavernier mentions one, weighing four ounces, which was sold for one hundred and fifty pounds.

The diseases supposed to be cured by Bezoars were of varied character, such as epilepsy, palpitation, vertigo, contagious fevers, etc. It is said to have been a custom in Persia to take a dose of powdered Bezoar at the beginning of the year to protect the body from poison for the succeeding year.

They may have been useful perhaps in some cases, owing to the amount of bile contained in them, and also because they were sometimes steeped in infusions of active medicinal plants.

IV. The Occidental Bezoar.

This is found in the goat of Peru and India, and, as a rule, it is larger, lighter in color, and for the most part without the peculiar black metallic lustre of the true Oriental stone, and is of much less value. The chamois yields what is known as German Bezoar, and another similar stone is found in the llamas of Peru.

The high price of the Oriental Bezoar led to numerous imitations, for the most part made of chalk and pipe clay, frequently gilded to give the high polish of the Eastern stone.

By putting butter of antimony under the action of nitric acid an artificial Bezoar can be made, and other imitations were made of vegetable resin identical with the litho-fellic acid of M. Goebel, which he found in a calculus examined by him. These stones are sometimes called resino-bezoardic concretions.

The snake stones of the Portuguese were probably made by the Brahmins, who pretended that they were taken from behind the head of the Cobra da Capello. They were called Pedra di Cobra, and were made of calcined bone-earth finely powdered and mixed with musk and aromatic gums. They were probably of use when applied to wounds, although not quite in the way imagined, for, being highly porous and absorbent, when applied in quick succession to a recent snake-bite, these stones would naturally draw out the poison by capillary action; when one stone fell off another would be supplied until the wound was sucked dry. Koemfer says 28 stones were needed to be applied to effect a cure.

Fossil Bezoars are found in Sicily in sand and clay-pits. They are concretions of a purple color, around some usually organic body, and are of the size of a walnut.

V. Concretions of phosphates of magnesia and ammonia. The consideration of these calculi would hardly come within the limits of this paper.

VI. Ambergris.

Concretions found in the Spermaceti whale. This substance is found also floating on the sea upon the coasts of Japan, Coromandel, and Madagascar. It is of very light specific gravity, ash-colored, with black veins and spots. It is supposed to be a product of disease, as it is only found in dead or sickly whales.

One more so-called Bezoar may be mentioned, and then, as far as is known, all the various kinds will have been touched upon.

In the Malay Peninsular there is sometimes found in the cocoanut a stony concretion, properly called *Callapitte*, which is worn by the Malays as an amulet of great value. This is so like Bezoar that it is sometimes mistaken for it, although a purely vegetable product.

THE STUDY OF MOULTING IN BIRDS.

BY WITMER STONE, M.A., CONSERVATOR, ORNITHOLOGICAL SECTION, ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA.

THE question of change of plumage in birds, even in our commonest species, has never received the attention that it deserves, and, considering the number of ornithologists which we now have in the United States it seems strange that we know so little of the matter.

Perhaps now that the field for the discovery of new species or races of North American birds is narrowing so rapidly, attention will be turned to the study of moulting and other none the less interesting phases of bird life. Comparatively little material seems to have been gathered as yet for the proper discussion of plumage changes, for in almost all the private collections of bird-skins that I have examined I have been struck with the lack of specimens illustrating seasonal changes of plumage, the bulk of the material being either adult spring birds or fall birds which have completed the moult.

The reason for this is easily seen, as in August, the season when most birds experience their complete moult, collecting is by no means easy work. The birds themselves are quiet and inactive, which renders them inconspicuous and hard to find; then, also, the specimens secured during the moulting season are difficult to prepare satisfactorily, while the heat of mid-summer renders immediate preparation necessary. Such obstacles should not, however, stand in the way of the collector and those making local collections of birds should aim to have a sufficient series of each species to show all its seasonal changes of plumage.

Having been recently engaged in examining some interesting series of moulting birds, a few words on these and the question of moults in our passerine birds in general may not be out of place.

Change of plumage in birds, as is well known, takes place in two ways (1) by the acquisition of an entirely new set of feathers and (2) by an abrasion or wearing away of portions of the old feathers.

As a matter of fact both of these methods are employed by all our birds though the amount of change and the number of changes during the year vary in different species.

In all our birds there is a moult of all the feathers late in the summer or early in September, when the breeding season is over, and the feathers are in the poorest condition. The moult at this season is an obvious necessity, as without it the birds would be unable to accomplish their autumnal migration and would be but ill prepared to withstand the cold of winter. Specimens secured just before this moult takes place are in a wretched condition, many of the tail feathers are reduced to mere spines and the wing feathers are often more or less broken while the body plumage is very much worn and some patches are often entirely lacking.

In effecting the complete moult the feathers are renewed a few at a time in a regular sequence, and the utility of this can easily be seen for if the old plumage was all lost at once the bird would be unable to fly for some days and would in all probability perish. On the wings the moult begins with the middle feathers and extends outward and inward, corresponding feathers being lost from each wing simultaneously. At the same time the feathers on the sides of the breast, centre of the back, and the wing coverts are renewed. Male bobolinks taken in this state show the process very clearly, and the bright bands of buff forming an inverted V on the breast stand out in relief against the dull black of the old summer plumage. The change of plumage on the other parts of the body follows rapidly, and the new dress is donned in a remarkably short time, with the exception of the last wing and tail quills.

The second method of changing plumage — by abrasion — is best seen in birds having parti-colored plumage where the centre of the feather is of one hue and the margin of another. Of course, abrasion occurs in all birds, but when the feathers are uniform in color no marked difference is produced by the process. The abrasion begins soon after the autumal moult and continues throughout the year, being effected by the general wear and tear on the plumage and by the action of the bird itself in cleaning its feathers by drawing them through its bill. The margin of the feather—that is the terminal portion of the barbs—seems to become brittle and break off at a slight touch, the point at which the fracture occurs being on the line where the color changes, in parti-colored feathers. In this way the color of a bird may be entirely changed without the loss or gain of a feather, for owing to the shingled arrangement of the plumage only the terminal portion of the feather is seen, and when this is worn off the central and basal portion, which is frequently differently colored, comes into view.

The series of snow-buntings collected in Greenland by the late Peary expedition, which have passed through my hands, taken in connection with winter specimens from Pennsylvania, show this method of plumage change very well. Taking the feathers of the back, for instance; in the winter birds we find them so broadly tipped with white and brown as to give a light-colored appearance to the bird; in the summer specimens, however, the light tips have been entirely lost and the back becomes solid black. The actual shape of these feathers has changed too, for while those of the winter birds were oval, those of the summer specimens are found to be pointed, with the sides somewhat concave. This was the shape of the black central portion of the feathers in the winter, and when the light margin has been worn off the black portion is, of course, all that remains. This change of shape in the feathers, due to abrasion, is best seen, however, in the curlews and other birds in which the back and rump feathers have peculiarly lobed black centres with light-re-colored margins. The breeding birds of these species will be found to have these feathers deeply sinuated along the margins due to the loss of the light portions, between the black lobes, in striking contrast to the oblong oval feathers of the fall plumage.

In birds which experience a loss of the tips of the feathers by abrasion, but which, owing to the manner of coloration of the feathers do not show any marked change in the general coloration of their plumage, as in the common Song Sparrow, the fall specimens can still be distinguished from spring ones at a glance; as the plumage in the former is long and blended while in the latter the feathers have the appearance of having been clipped with shears.

As has already been said, these two methods of changing plumage (1) by a complete moult, and (2) by abrasion, take place in all birds, but the time and extent of the changes differ in different species.

Our passerine birds may be grouped in three classes according to the changes which take place in their plumage during the year. The most usual system is (1) a complete moult in the autumn or late summer and (2) an abrasion of the tips and margins of the feathers in the spring accompanied by a more or less extensive renewal of the smaller body of feathers.

In some species the acquisition of new feathers in the spring is so slight that it is scarcely apparent and can only be detected by careful scrutiny while in other cases considerable patches of feathers are renewed.

In the Sharp-tailed Finch, of which I have examined a series of eighty specimens taken during every month of the year on the New Jersey salt marshes by Mr. I. N. DeHaven and myself, I find a considerable acquisition of new feathers taking place in April; in some individuals even the tail feathers are being renewed, which is not surprising as owing to the habits of the bird the plumage must become very much worn. Many male birds which require several years to attain their full adult plumage acquire some of the feathers characteristic of the adult plumage at this spring moult. The White-throated Sparrow, for instance, acquires additional white feathers on the throat and head, and yellow ones in front of the eye, and the Myrtle Warbler experiences an increase in the yellow feathers on the sides of the breast.

The second system of moulting consists of (1) a complete moult in the autumn or late summer and another moult in the spring, which is either complete or excludes the remiges and rectrices. Such birds as the Scarlet Tanager, Indigo bird, etc., are examples of this class. Owing to the fact that many of them winter in the tropics it is difficult to obtain specimens showing the progress of the spring moult, and we are forced to a comparison of fall and spring birds. The Goldfinch, however, which can be obtained throughout the year in this latitude, shows the double moult very nicely, and specimens taken in April and September will be found respectively to be acquiring and losing the familiar bright yellow plumage, the gray feathers of winter appearing in the fall birds and disappearing in the spring ones.

The third system of moults seems the most complicated of the three, and was first pointed out by Mr. Frank M. Chapman in the case of the bobolink.

This bird has a complete moult in the late summer, then another complete moult in the early spring before it starts north from the tropics, and between that time and the breeding season an extensive abrasion, which again completely alters the appearance of the plumage. (See *The Auk*, 1890, p. 120.)

Specimens of the Rose-breasted Grosbeak which I have recently examined, taken in South America, seem to indicate that this species has a similar system of moulting to that of the Bobolink.

So far as I can ascertain, the adult male in fall assumes the striped brown dress of the female, but differs from it in having bright pink under wing-coverts and a marked pink suffusion on the breast. Opposed to this is the well-known black and white plumage of the breeding bird with its brilliant pink breast. Now the South American birds above alluded to are different from either of these. They possess the full plumage of the breeding bird, but every feather has a light brown or buff edging which gives the bird a "veiled" appearance and conceals to a certain extent the striking markings of the nuptial dress.

These specimens indicate pretty clearly that in addition to the annual fall moult the male Rose breast has a complete moult during the winter or early spring, assuming at this time a dress which differs decidedly from the breeding plumage, but which changes into it by means of extensive abrasion.

The lower orders of birds have as a general thing still more complicated moults than are found in the Passeres, and of most of them comparatively few of the details are known.

In consideration of these facts as well as the great interest that this study possesses, I cannot but recommend to all collectors to have this matter in view in making future additions to their collections and to look over the material which they already possess with an eye to the moult, feeling sure that they will be well repaid for their pains.

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.

Comparative Longevity.

Among mammals the epoch of maturity is usually stated as reached in one-fifth of the animal's life. Thus the ages of maturity and periods of life of several forms are about as follows: horse, bull, four to twenty; sheep, two to ten; rabbit, one to five; but there are exceptions, such as the cat, which matures in one year and may live twenty.

It is assumed that man matures at twenty, and hence, by the rule, should live to be one hundred, as does the elephant, which matures in its twentieth year. But, I think that writers upon longevity, such as Hufeland, Flourens, Quatrefages, Thoms, etc., when commenting upon these relationships, have overlooked the fact that the general rule holds better for the lower races of men who mature sooner than the civilized, to whom the retardation of early development is an advantage, as it prolongs the plastic, receptive period.

The helplessness of the human infant at birth, and the length of time it needs parental care, in these respects differing from other mammals, enables training of a progressive nature in succeeding generations, and whether this lengthened immaturity is a result, or cause, or both, it is a great advantage. We may be justified in regarding the immaturity as prolonged beyond that of other mammals rather than that man's longevity is proportionately less. S. V. CLEVENGER.

Chicago, Ill.

BOOK-REVIEWS.

Handbook of Australian Fungi. By M. C. COOKE. London, 1892. 458 p. 36 pl.

Select Extra-Tropical Plants, Readily Eligible for Industrial Culture or Naturalization. By BARON FERD. VON MUELLER. 8th Edition. Melbourne, 1891. 595 p.

HE who nowadays would keep posted in regard to the progress of science must frequently turn to the southern hemisphere. In South America, in South Africa, and in Australia the devotees of science have been and are working. The recent organization of an Australasian Association for the Advancement of Science is an effort toward a union of scientific men such as already exists in North America, England, France, and Germany; and it will do much toward unifying the work of the numerous scientific bodies that have long existed in the various colonies. The vast extent of territory and the distances between the capitals of the several colonies is paralled only by our own country, but here we have the advantage of a greater net-work of railways and more rapid means of communication. From Hobart, the capital of Tasmania, to Christchurch, New Zealand, where the meeting of the Association was held in 1891, the distance is about 1,000 miles. From Sydney, in New South Wales, it is over 1,200 miles; from Adelaide, in South Australia, the distance is over 2,000 miles; while it is even further than this from Brisbane, in Queensland. All of these places are included in the comprehensive Australasian Association.

To enumerate the scientific societies in Australia would require considerable space. We cannot, however, forbear alluding to some of the more important, as shown by their publications. There is, for example, the Royal Society of New South Wales, that has issued 24 volumes of proceedings; the Royal Society of Victoria, and the New Zealand Institute, also each with 24 volumes; the Linnæan Society of New South Wales, with 6 volumes; the Australasian Association for the Advancement of Science, 3 volumes, and the Royal Society of Tasmania, that has been publishing since 1863. Besides these there are innumerable irrigation, engineering, mining, and geological reports published by the governments of the several colonies. The agricultural side is represented by reports of the secretaries for agriculture of Queensland, New South Wales, etc., and by the grand publications of Mueller on the Eucalypts, and the well-edited agricultural journal of New South Wales. To mention all the official publications would be a task too great to be undertaken here. But from what has already been said, it must be manifest that the Australian colonies are not one whit behind the rest of the civilized world in their contributions to scientific and practical literature.

There is, of course, a reason for this activity. The country is new, and is full of wonderful birds and animals and plants; and the men who left behind them in Europe an exhausted field, as far as novelties in science go, find in the colonies a virgin field. The vegetation, the animal life is so different from that of the northern hemisphere that we may look forward for years to come for additions to our knowledge of the productions of the wonderful island.

Two books that have lately been added to botanical literature from Australia are those mentioned at the head of this article. Both are from veterans in their respective fields, one a cryptogamist, the other a phanerogamist. Both have a world-wide reputation, and both have exceeded the three-score and ten years of allotted human life and are yet active workers. Although here brought into conjunction, the men themselves are residents of opposite sides of the globe. The names of Dr. M. C. Cooke and Baron Ferd. von Mueller must live as long as the science of botany exists. Students of science are grateful that they have been spared long enough to give them two such valuable works.

The "Handbook of Australian Fungi" is a compilation of the descriptions of these plants that have at various times been published in widely-scattered volumes. The work was undertaken at the request of the Australian colonies, and is published under their authority. A limited edition only has been printed, some 500 copies, and the major part of it has gone to Australia. The total number of species given in the volume is 2,087, exclusive of varieties. This, in comparison with the total number of species recorded by Saccardo, some 36,000, seems small when the extent of country covered is considered. But it is of course very improbable that all the Australasian forms have been described. Indeed, scarcely a month passes but some new forms are recorded, and it is probable that they will continue to be sent in for many years to come.

The largest order represented is Hymenomycetes, with 1,178 species, more than half the total number recorded. This is probably due to the fact that the species are large, or at least conspicuous, and are therefore collected. Another order, however, also with conspicuous members, the Gastromycetes, is exceptionally well represented, as there are 174 species out of a total known from the whole world of 650 species. Among the interesting species of this family we note *Podaxis indica*, a plant bearing a surprising outward resemblance to *Coprinus cornatus*, but of course with a very distinct internal structure. There is also *Xylopodium ochroleucum*, with a long stalk and a peridium marked with numerous angular projections.

The occurrence of a number of species in the two islands of Ceylon and Australia is noted as a curious fact in geographical distribution. The flora in general and the fauna as a whole is so distinct in these two countries that it is difficult to account for this fact. It is true that plants in many cases seem to overstep the bounds that have been assigned to them by botanists, and do not appear to follow the ordinary laws of distribution. Especially is this true of ferns and fungi, two classes having spores so minute as to be capable of transportation long distances through the air by winds. Some species thus become cosmopolitan, but at present we cannot account for finding some species of such conspicuous genera as *Lepiota*, *Hymenochæte*, *Stereum aseröe*, etc., *only* in Ceylon and Australia. It is of course possible that when the intervening islands of New Guinea, Java, Borneo, Celebes, Sumatra, and other smaller ones of the Malay archipelago are explored, that the same species may be found there. That would do away with the anomaly. Comparing the flora with that of Europe, Dr. Cooke finds that 332 of the Hymenomycetes are exclusively Australian, 472 are also found in Europe, and 370 are common to Australia and some other country. Of the Gastromycetes only 31 out of 173 species are European.

In the introduction Dr. Cooke gives condensed accounts of the principal groups, with tables of the genera. This, while not claimed to be complete in any sense, cannot but be of assistance to the student. The species will have to be identified from the descriptions. This is to some extent facilitated by the plates. Of the 36, 20 are colored, and on them are given 377 figures. A list of the authorities cited, and a full index are valuable portions of the book. The descriptions of the plates would have been more convenient for reference had the page where each species is described been given.

The second one of our titles is a new edition of an old book, but it is such a valuable book that it deserves to have general attention called to it. The early editions being exhausted, and there being much new matter in hand, the government of Victoria publishes this volume. To give an adequate idea of its contents would be to index it. We can only refer in a general way to its contents and perhaps mention a few of the more important and interesting facts presented. We have also been struck with Baron Mueller's remarks in both preface and postscript. In the former, after reviewing in a general way the contents of the volume, and mentioning the various editions of it that have appeared from time to time, he says: —

"The fact that this work through successive editions and extensive issues came into use over a large portion of the world, whether for educational, or rural, or journalistic, or touristic wants, has been most gratifying to the author; but this brightness is dimmed by the circumstance that the book has not unfrequently been used even in public departments with perhaps unintentional evasion of all literary or any other acknowledgment. Nor did hardly ever words of appreciation reach the author from wherever rural successes were gained from even practical exertions of the author."

This is too often the experience of the literary and scientific man. His ideas, his knowledge, are seized upon, or his books and papers are received and no hint of the benefits he has conferred ever reaches his ears; no indication is ever apparent that the seed has fallen upon fertile ground. In his postscript the author requests persons using the book to send him suggestions or additions, concluding with the following words:—

"While approaching the eighth decade of his life, the author cannot hope to see many more editions of this work, brought up to the newest standard, through the press himself; but, as he may perhaps still be able to publish one more edition before passing away, he is now particularly eager that the next issue should by some special efforts be rendered as complete as this, within the knowledge of the present days, can be accomplished. Such help, furthermore, would really be a recompense only from those who in using this book derived some practical benefit or instructive advantages from its pages."

The number of practical suggestions is endless. For example, in speaking of the "Black Wattle" of Australia, mention is made of the great value of the bark for tanning purposes. One and one-half pounds of this will do as much as five pounds of English oak bark. The tree is easily grown, and the seeds may be sown broadcast or in drills. It grows on the poorest and dryest soil, and a return may be expected in from five to ten years. Full-grown trees yield about 100 pounds of bark. It grows about an inch in diameter annually, and is hardier than *Eucalyptus globulus* (the gum tree). On this account it would be valuable to introduce into our Southern States and Southern California. The seeds retain their vitality for several years, and can be obtained in Melbourne for 5 shillings per pound, each pound containing from 30,000 to 50,000 grains. They germinate best after being soaked in warm water.

The "rain-tree" is described as reaching a height of 70 feet, with branches extending 150 feet away from the trunk. It grows rapidly and makes an admirable shade-tree in countries where there is no frost, and where the rainfall fluctuates between thirty and sixty inches annually. The leaves shut up at night and allow rain and dew to reach the ground beneath, so that grass will grow. The pods are produced in great abundance, and are fattening to cattle, which feed upon them greedily.

The tea-plant is stated to be hardy near Melbourne, enduring light frosts and scorching hot summer winds. It thrives best, however, in humid valleys with rich alluvial soil, where there are springs for irrigation. The greater the rainfall the larger the yield of tea. In Japan the plant is cultivated as far north as 43° latitude, where the thermometer occasionally falls to 16° F., and the ground remains frozen several inches deep for weeks. In 1840 India sent her first sample of tea to European markets, and in 1864 exported 7,800,000 pounds. In 1889 the amount had risen to 101,000,000 pounds. Three hundred pounds to the acre is the average yield in India. The author believes that for many years to come it will be a profitable business to raise tea-plants for the seeds alone.

Some twenty-five pages are devoted to the *Eucalyptus*, full accounts being given of several of the species. The "giant gum tree" (*Eucalyptus amygdalina*) reaches a height of 415 feet. The tree sometimes measures 69 feet in circumference at the ground, and one has been recorded as 33 feet in diameter at 4 feet from the ground. One 78 feet from the ground was 9 feet in diameter.

CALENDAR OF SOCIETIES.

Philosophical Society, Washington.

Jan. 21.—T. C. Mendenhall, The Use of Planes and Knife-Edges in Pendulums; R. S. Woodward, The Use of Long Steel Tapes for Measuring Base Lines. A report will be presented from the committee appointed to consider suitable commemoration exercises at the 400th meeting of the society.

Agassiz Scientific Society, Corvallis, Ore.

Jan. 11.—G. W. Shaw, Gravitation a Form of Energy.

Publications Received at Editor's Office.

A Free Land. The Cry of the Children. London, Williams & Norgate. 123 p. 12°.

Becker, G. F. Finite Homogeneous Strain, Flow and Rupture of Roc s. Rochester, N. Y., Geol. Soc. Amer. 8°.

Boyd, R. Nelson. Coal Pits and Pitmen. New York, Macmillan & Co. 256 p. 12°. $1.

Carus, P. Truth in Fiction. Chicago, OpenCourt Pub. Co. 111 p. 8°. $1.

Congres International des Americanistes. Compte-rendu de la Nuitième Session. Paris, Ernest Leroux. 704 p, pl. 8°.

Dumble, E. T. Report on Brown Coal and Lignite of Texas. Austin, Tex., Geolog. Survey. 243 p., pl. 8°.

Foster, L. S. The Published Writings of George Newbold Lawrence, 1844–1891. Washington, Smithsonian Inst. 124 p. 8°

Lodge, Oliver. Pioneers of Science. London and New York, Macmillan. 404 p. 12°. $2.50.

MacDonald, A. Criminology. Introduction by Dr. C. Lombroso. New York, Funk & Wagnalls Co. 416 p. 12°

Simmons, H. M. The Unending Genesis. Chicago, C. H. Kerr & Co. 111 p. 24°. 25 cents

Talmage, J. E. Domestic Science. Second Edition. Salt Lake City, Utah, G. Q. Cannon & Sons Co. 389 p. 12°.

The Journal of Political Economy. Vol. I., No. 1. Dec., 1892. Chicago, The University Press. 161 p. 8°. $3 per year.

World's Fair Electrical Engineering. An Illustrated Monthly Magazine. Chicago, Elec. Eng. Pub. Co. 56 p. 8°. $3 per year.

It grows with great rapidity, in France reaching a height of 50 feet in 8 years. It is hardy in the milder parts of England. It yields a valuable volatile oil, and it is said when combined with thymol and other antiseptics to be efficacious in treatment of scarlet fever and other infectious diseases, especially as an external application. Fresh branches placed under the bed of phthisic patients act as an antiseptic, a sedative, and a hypnotic. The plant is also useful in subduing malarial effluvia in fever districts.

We cannot go further into details, but commend the volume to all botanists and horticulturists. Various indexes, of trees best adapted for certain purposes, of genera referred to, of synonyms, of vernacular names and of species best adapted to various regions of the globe, add greatly to the value. The price of the book is only five shillings. JOSEPH F. JAMES.

Washington, D.C., Jan. 12.

A Text-Book of Least Squares. By MANSFIELD MERRIMAN. 6th Ed. New York, J. Wiley & Sons. 1892. 198 p. 8°.

Theory of Errors and Method of Least Squares. By W. W. JOHNSON. New York, J. Wiley & Sons. 1892. 174 p. 12°.

WE have here two excellent works, written by two able men, and illustrating in an interesting manner those different views of identical principles and methods which independent thinkers are always able to exhibit, however old and well-worked the subject. Professor Merriman wrote his first edition of this treatise in 1877, with the purpose of presenting the facts and principles of this somewhat abstruse subject in such form as to make them easily comprehended by students and by engineers, in practice often less familiar than the student with work underlying the higher mathematics. That treatise, while successful, served nevertheless, to indicate where still further improvement might be effected, and the present is a re-written treatise, of which the major portion was prepared and printed in 1884, as a second edition. The sixth edition, now before us, contains the same matter in substance, but with the usual and unavoidable printers' and other errors, always found in first issues, removed, and some improvements introduced in the treatment of adjustments of two related quantities, and with notes of interest appended. The book has become a standard work of reference, as well as a text-book, and needs no special commendation from us, other than the expression of full agreement with the verdict of the purchasers and users of five issues, who have made necessary this sixth edition.

Professor Johnson has condensed his work into a smaller compass than the preceding; but it is all the more rich and "meaty." The author follows Gauss in the methods laid down in "Theoria Motus Corporum Cœlestium" (Werke, VII.). and treats the "reduced observation equations" by the more explicit methods introduced by Jordan ("Handbuch der Vermessungskunde," 1888) and later writers, including Oppolzer, to whom he goes for some of the more important forms adopted in computations. The book is systematic, logical in its sequence, and well illustrated by carefully chosen examples in application. Appended are tables of values of the probability-integral and of powers and roots.

When a mathematician of eminence undertakes thus to provide a treatise upon a subject of importance, and compiles a text-book, for young students, not only the youth who is thus provided with a text-book but the whole world of observers employing such methods become more indebted to him than to the less distinguished and less talented man doing similar work; we have the assurance, not only that the book will serve its purpose, but that it represents the latest and best thought and labor of the time. This assurance is worth much to teacher and pupil; and it can hardly be doubted that the use of this little treatise will extend beyond the limits of the United States Naval Academy, where it was originally intended by its author to be used, in his own classes.

Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

For sale or exchange.—I have a few copies of my translation of "Strasburger's Manual of Vegetable Histology, 1887," now out of print, which I will send post-paid for $3 or for one dozen good slides illustrating plant or animal structure. Address A. B. Hervey, St. Lawrence University, Canton, N. Y.

The undersigned has the following specimens to exchange for crystals of any eastern or foreign localities or Indian relics: tin ore, metacinnabarite, stibnite, garnierite, calenanite, hanksite, ulexite, rubellite, lepidolite, blue and green onyx, Cal. pineite, aragonite on chalcedony, cinnabar, double refracting spar, clear and clouded, and others. J. R. Bush, care of General Delivery, Los Angeles, Cal.

For sale or exchange.—A private cabinet of about 200 species of fossils, well distributed geologically and geographically. Silurian, about 40; Devonian, about 50; Carboniferous, about 80; others, about 30. Frank S. Aby, State University, Iowa City, Ia.

For exchange.—Minerals, fossils, F. W. shells, land shells, native woods, Indian relics, two vols. of Smithsonian reports, odd numbers of scientific magazines, copper cents, etc., for good minerals not in my collection, good arrow- and spear-heads and natural history specimens of all kinds. Correspondence solicited with list of duplicates. G. E. Wells, Manhattan, Kan.

For sale or suitable exchange.—A spectrometer made by Fauth & Co., Washington, D. C., according to the plan of Prof. C. A. Young. This instrument is suitable for the most advanced investigations and determinations. Cost originally $700 and has been used but little. Will be disposed of at a considerable reduction. Address Department of Physics, Ohio University, Athens, O.

I will send British land and fresh-water shells in return for those of America, any part, sent to me. I have at present about fifty or sixty species, with many varieties. W. A. Gain, Tuxford, Newark, England.

The Biological Department of Hamline University desires to offer microscopic slides of animal tissues, or whole animals, in exchange for first-class fossils. Address correspondence to Henry L. Osborne, Hamline University, Hamline, Minn.

Kindly mention "Science" in writing to Advertisers.

Wants.

WANTED.—American Journal of Conchology, seven volumes. Parties having these for sale will please address the undersigned, stating condition and price. R. Ellsworth Call, Louisville, Ky.

A GRADUATE ENGINEER will give instruction evenings in geometry, trigonometry and surveying, mechanics, physics, mechanical drawing and general engineering construction. Five years' experience in field and editorial work on engineering journal. References furnished. C. S. H., 102 Tribune Building, New York.

WANTED.—By well-qualified and experienced science master and associate of the Royal School of Mines, London, aged 26 (at present in England), a mastership in technical college or university for any of the following subjects: Engineering sciences, geology and mineralogy, physics, chemistry and metallurgy, etc., etc. Can provide excellent references and credentials. Apply, J. G., 17 Sussex St., Rochdale, England.

A GRADUATE of the University of Pennsylvania and a practical mineralogist of twenty years' experience desires to give his services and a cabinet of 25,000 specimens, all named, with about the same number of duplicates, in minerals, crystals, rocks, gems, fossils, shells, archæological and ethnological specimens and woods to any institution desiring a fine outfit for study. The owner will increase the cabinet to 50,000 specimens in two years and will act as curator. Correspondence solicited from any scientific institution. J. W. Hortter, M.D., Ph.D., San Francisco, Cal., General P. O. Delivery.

SCIENCE

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SCIENCE

NEW YORK, FEBRUARY 3, 1893.

SOME INSECT IMMIGRANTS IN OHIO.[1]

BY F. M. WEBSTER, OHIO AGRICULTURAL EXPERIMENT STATION, WOOSTER, OHIO.

In the following paper the term immigrant is to be understood as given in our lexicons, viz: a species that has come to this State from elsewhere and taken up its permanent abode in our midst. While such species are largely of foreign origin, yet this is not true in all cases, and the term foreign is hereafter intended to apply to territory outside of the State of Ohio. Nor do I intend to enumerate all of the foreign species that have gained a residence within the boundaries of the State, but to give some facts relative to the time, place and method of introduction of a number of them. Without wishing to present a paper on Economic Entomology, it will be necessary to use, as illustrations, injurious or beneficial species, from the fact that these are more closely watched and their movements best understood; but among the earlier known species we find that even these are often difficult to follow in their advance across the country. There are, seemingly, two what we may term gateways through which the majority of species that have come to us from the east, have made entrance into the State, and, later, spread out over the northwest. The first, and apparently the most important one of these, being at the extreme northeastern part, adjoining Lake Erie, and which we might term the north gate, and, second, the valley of the Ohio River, from a point where it begins to form the eastern boundary of the State, southward—perhaps to Wheeling, W.Va. Now, there also appear to be two great national avenues or highways which insect migrations follow; progressing more rapidly along either one or the other, but not equally so along both, and often following only one; the more sub-tropical species, whether American or introduced, taking the southern or what I would call the Great Southwestern route, while the sub-arctic, including, besides American, such species as have come to us from England or Europe north of latitude 45° north, take what I would term the Great Northwestern route. The division between these two great thoroughfares will be indicated, approximately, by a line drawn from New York City, latitude 40° 43′ north, to St. Louis, Missouri, latitude 38° 38′ north, thence to Pueblo, Colorado, latitude 38° 17′ north (about), the line of separation trending northward, east of St. Louis, under the influence of the Gulf Stream and the Great Lakes, chiefly the former. Of course it is not to be understood that this line is direct, as it is doubtless more or less irregular, and, from its very nature, to some extent unstable, nor is it to be supposed to form a radical boundary, as some northern forms gradually work their way south of it, and *vice versa.* Yet it will, I think, be found approximately correct.

One of the first species to push its way across our country was the Angoumoise Grain Moth, *Sitotroga cerealella* Oliver. From the best information we can obtain, it seems to have been introduced into this country from southern France, as early as 1728, occurring at that time in North Carolina. This is a southern species, and it is no way likely that it entered from the north, but found its way into Ohio, where it appeared, probably about 1840, from Kentucky. It has not, so far as I am aware, been observed in any considerable numbers north of the line indicated, but has pushed its way to the southern part of Texas. The wheat midge, *Diplosis tritici* Kirby, which probably came to us from England, via Quebec, Canada, entered the United States through northern Vermont in 1828–29, pushing southward and westward, but seemingly making more rapid progress to the west. This certainly entered Ohio through the northern gateway, over-running the State, as also Indiana. Though reported, first in 1843, and again in 1847, in central Ohio, it was in 1849 reported in destructive numbers along the northern part of the State, while the eastern and southern portions seemed exempt. Therefore, I conclude that it came to the State through the north gate. It is one of the species that has followed both the northwestern and southwestern routes, but has probably made more rapid progress and advanced farther along the former. Of the early movement of the Hessian Fly, *Cecidomyia destructor* Say, in Ohio, I have no exact data. It might have come up from the South, or entered by either of the two eastern gateways. Like the wheat midge, however, it appears to have made more rapid progress north of the line than south of it. The Imported Cabbage butterfly, *Pieris rapæ* Linn., a native of England, but first appearing in this country in the vicinity of Quebec, Canada, in 1860, pushed its way southward, and in ten years had reached southern New York. From here it gradually moved to the west and south, being first observed in Ohio, about Cleveland, in 1873, a year earlier than elsewhere in the State. From this we infer another entrance through the north gate. Though spreading southward, so that the line given does not at present mark the boundaries of its habitat, yet it flourishes best near or to the north of it, and is not nearly so abundant in the Gulf States, though reintroduced into South Carolina in 1873 and in Florida in 1874. It has mainly followed the northwestern route, but, like the wheat midge, its southern boundary lays far south of the line. The three clover insects, *Cecidomyia leguminicola* Lint., *Hylesinus trifolii* Muel., *Phytonomus punctatus* Fab., without exception, I believe, first came to us from the north-east; though the last two are now known to occur in extreme south-western Ohio and south-eastern Indiana. They probably entered the State from the south east by way of the Ohio River, at a later date, there being none continuous of the northern colonies to the southward so far as I have been able to observe or learn. The Phytonomus, two specimens of which were, last spring, found by Mr. Dury near Cincinnati, I feel confident was carried into the Ohio River by some of its smaller tributaries, one of which, Beaver River, rises in north-eastern Ohio, by the exceedingly high waters of last spring, and conveyed down by the current and left along the shores.

Hylesinus may have been introduced in the same manner, but probably several years earlier, as it has already become abundant enough to prove destructive in Dearborn and Franklin Counties in Indiana.

The Horn Fly, *Hæmatobia serrata* Rob-Desv., probably came first from the north-east, followed almost immediately by an independent introduction by way of the south-east gateway. Coming originally from France, this species, in spreading over our country, does so entirely regardless of the lines we have drawn. Still, its more rapid progress along the southern route, where the facilities for its diffusion are much inferior to those along the northern route, where it has made even less rapid progress, shows that it is swayed by the same influences that have directed the course of other species. So far, we have been dealing largely with species of trans-Atlantic origin. Now we will take an American species—the Locust Borer, *Cyllene robinæ* Forst. This species had for upwards of a century been known in New York, as an enemy of the Black Locust, *Robinia pseudacacia* L. Some time about the year 1850 it began its invasion westward across northern Ohio, Indiana and Illinois, reaching the Mississippi River about 1865, carrying death and destruction to the Black Locust along its path, but not at once extending its ravages, to a serious degree, in the southern portion of these States. Again, reversing the order of migration that we have been following, we will take another American species, *Doryphora 10-lineata* Say.

[1] Read before the Ohio Academy of Science, Dec. 29, 1892.

Starting in Colorado, it pushed its way rapidly eastward to the Atlantic coast, and, though not confined to our north-western route, as we have termed it, nevertheless, its most rapid progress and greatest destruction was executed north of our imaginary line. Even yet it has not spread southward to the Gulf of Mexico.

Another species of whose advance I am not so certain, is *Diabrotia longicornis* Say, described many years ago from examples collected near the foot of the Rocky Mountains, but which I know to now occur in Arizona and Central America. This has become a terrible pest in fields of Indian corn all over the west; and Professor Forbes of Illinois, some years since, expressed the opinion that it was moving eastward. While mentally differing from this opinion myself, yet the fact that it has, this year, been reported for the first time in Ohio, along the western border, has led me to feel that Professor Forbes' opinion may yet prove to be a correct one. Certainly we shall watch and mark its progress carefully. Its congenor, *Diabrotica 12-punctata* Oliv., though we do not know it to be of southern origin, yet it is very destructive to the same cereal in the south, but this injury, so far as known, is confined, largely at least, to the territory south of the dividing line, unless it be in Ohio, where, I strongly suspect, it is more destructive than we are aware. Of the south-west route, we have already observed much in relation to such species as have pushed their way over it from the east toward the south-west. Therefore, I shall speak only of such species, with two exceptions American, I believe, as have passed over the ground from south-west to north-east. One of these exceptions is the larger corn-stalk-borer, *Diatræa saccharalis* F.

According to Mr. L. O. Howard, who has studied the species quite thoroughly, it may be a native of the West Indies, or it may have originated in South America and made its way to the United States by way of these islands. Be this as it may, it occurs along the Gulf and Atlantic coasts, and, in the light of recent observations, it seems to be pushing its way northward along the Atlantic, having now reached the vicinity of Washington, D. C. Though for years known to infest both sugar-cane and maize, in Louisiana, yet we have no information of a corresponding advance northward. This, in some respects, appears to be the case with another insect, *Cylas formicarius* Fab., which breeds in the sweet potato, a native of Cochin China, India and Madagascar, but introduced into the United States, probably, by way of Cuba. This may have been first introduced either into Florida or Louisiana, as it occurs in both States, and is now pushing its way west across Texas. Whether it will follow in the path of the Diatræa, along the Atlantic, remains yet to be seen. They are both slowly making their way along our great south-western highway, and if either reach Ohio it will most likely be the Diatræa and along our southern border. The Harlequin Cabbage Bug, *Murgantia histrionica* Hahan, is known to occur as far south as Guatemala, through Mexico, and first came to notice in destructive numbers in Texas about 1866. Four years later, it had pushed north to Missouri, and in 1875 it had made its way to Delaware, and on the west occupied wholly or in part Arizona, Nevada, California, Indian Territory and Colorado. It is now found in extreme southern Illinois, Indiana and Ohio, in all cases, I believe, near the dividing line between the two routes, also in New Jersey, thus covering almost exactly the south-western highway, but, excepting, perhaps, in the far west and near the Atlantic, not extending far beyond it. Although an older established species, *Dynastes tityus* Linn. occupies almost exactly the same area except in the extreme east, where Dr. Lintner has recorded it at Kingston, some seventy miles north of New York City. To my personal knowledge it breeds in southern Illinois, and also at Bloomington, Indiana. I have found it at Columbus, Indiana, and have good evidence of its occurrence in the vicinity of Columbus, Ohio. It has been known from southern Pennsylvania, and, later, from New York. Of the Bag or Basket-worm, *Thyridopteryx ephemeræformis* Haw., also a southern species, I only know that it breeds in southern Illinois, Indiana, and in Ohio, a short distance north of Hamilton, Butler County, while under Atlantic influences, it is sometimes abundant as far north as New York and found also in Massachusetts. The Praying Mantis, *Stagmomantis carolina* Linn., breeds in extreme southern Illinois, and also in extreme southern Indiana, but is said not to do so in Ohio. I have a male, given me some years since by Professor S. S. Gorby, State Geologist of Indiana, that was captured in a railway coach, running between Cincinnati, Ohio, and Indianapolis, and was captured between the latter city and Dayton, Ohio. I also learned that this summer a female was captured in Indianapolis, by which I judge that these two southern species are hovering in the vicinity of our boundary line.

Map indicating, approximately, the natural divide between the northern and southern insect faunas, east of the Rocky Mountains.

As before stated, we have used as examples species of economic interest, only for the reason that their movements are better understood. A careful study of the geographical distribution of other species would, doubtless, throw more light upon the problem. Our dividing line is supposed to be correct only in a general way, as, of course, there can be no such thing as an exact or continuous line of demarkation. This will of necessity be more or less irregular. Again, a species spreads over an area particularly adapted for its occupancy. But, no sooner is this done than the individuals along the frontier begin to adapt themselves to an environment but slightly unfavorable, and, as their adaptation changes, so do they slowly advance outward from the territory originally occupied. A series of unfavorable seasons might occasion the occupation of a wide margin of adjoining country, while a series of unfavorable seasons might sweep this tide of advance back to the place of its origin. But, as the receding tide of the ocean leaves many pools of water in the depressions of rock, so will there be left, in especially favorable nooks, a few of the insects which will retain their hold and form small, local colonies, of perhaps not more than a few individuals, and the offspring of these will meet the investigator long distances from the real habitat of the species. There is scarcely a collector who does not know of one or more small, secluded areas, in his neighborhood, that are rich in varieties, and which he seldom visits without satisfaction, and frequently he is astonished at his success. How long this ebb and flow has been going on, and how many species have been brought to us in this way, are problems we are yet unable to solve. Therefore these facts have been brought together, and are here presented, not as a finished, nor, indeed, as an advanced study, but rather as a primary outline, to be revised and modified as our knowledge of the geographical distribution of our species shall be enlarged by additional study and research.

A SKELETON OF STELLER'S SEA-COW.

BY BARTON W. EVERMANN, PH.D., ASSISTANT, DIVISION OF SCIENTIFIC INQUIRY, U. S. FISH COMMISSION.

DURING the time from March to September of last year the U. S. Fish Commission steamer "Albatross" was engaged, under the direction of the State and Treasury Departments, in making investigations regarding the habits, distribution, and abundance of the fur seal in Bering Sea and the North Pacific Ocean; and it was my good fortune to accompany the vessel as senior naturalist.

While carrying on these investigations, we had occasion to visit the Commander Islands, situated in Bering Sea, off the coast of Kamchatka about 80 miles. We spent the first week of June on or about these islands, and in this article I wish to call attention to one of the most interesting and valuable results of our visit to Bering Island, the more important one of the group. This was no less than the discovery of a nearly perfect skeleton of the now extinct Steller's sea-cow, *Rytina gigas*.

This remarkable animal was first discovered in the fall of 1741 by Captain Vitus Bering when his ship was wrecked upon the island now bearing his name. Geo. W. Steller was the surgeon and naturalist of Bering's party, and it is to him that we owe about all that we know about the sea cow in life.

At the time of its discovery this large marine mammal was quite abundant about Bering Island, as Steller reports that he saw them in great herds feeding upon the kelp and other sea-weeds that grow in abundance in the shallow water about the island. It was soon discovered that the flesh of the sea-cow was good eating, and the men killed many of them for food.

According to Steller, the sea-cow when fully grown was 24 to 30 feet in length, 20 feet in girth, and weighed 6,000 to 8,000 pounds. It was of a nut-brown color and covered with hair, matted like the outer bark of a tree. The skin was exceedingly thick, and so tough that the hunters had to cut it with an ax. The head was very small when compared with the great size of the body, the jaws were toothless, but were furnished with a thick, horny pad. The anterior limbs were modified into flippers, while the hind limbs were entirely absent, and the tail was widely forked, as in the sperm whale.

This animal was gregarious, stupid, sluggish, and comparatively helpless, being unable to protect itself by diving, and was occasionally washed ashore by breakers.

When, in 1743, the news of the discovery of Bering Island reached Kamchatka, several expeditions were fitted out for the purpose of hunting the sea-cow and the various fur-bearing animals, such as the sea-otter, fur seal, and blue fox, which are found there; and very soon many whaling vessels began to stop there to lay in a supply of sea-cow meat for food. So great was the destruction wrought by these whalers and fur-hunters that by 1754, only 13 years after its discovery, the sea-cow had become practically exterminated. In 1768, according to the investigations of Dr. L. Stejneger of the National Museum, who has made a most careful study of the question, this large and important marine mammal became wholly extinct, the last individual ever seen alive having been killed in that year; and the fate which overtook Rytina so speedily has almost become that of the buffalo, and will as certainly become that of the fur seal unless it be protected.

Mr. Frederic A. Lucas of the National Museum has recently published a most interesting and valuable paper on "Animals Recently Extinct or Threatened with Extermination," in which he gives in readable form about all that is known of the sea-cow. In this paper, of which I have made free use in the present article, Mr. Lucas states that, up to 1883, but two skeletons of the sea-cow were known. One of these is in the Imperial Museum at St. Petersburg, and the other is in the Imperial Academy of Helsingfors. There are two ribs in the British Museum. During Dr. Stejneger's stay of about two years (1882–1883) upon Bering Island, he succeeded in finding a number of skulls, ribs, vertebræ, and other bones. One complete skeleton was found buried in the sand, but the bones were too far decayed to permit handling. From the various individual bones found by Dr. Stejneger a fairly good skeleton was "made up," which is now in the National Museum. This, together with the two skeletons at St. Petersburg and Helsingfors, and the two ribs in the British Museum, constituted the total amount of material pertaining to Rytina found in the museums of the world at the time of my visit to Bering Island.

Being conversant with these facts, imagine my surprise and delight upon learning, soon after landing, that a native had recently found a nearly perfect skeleton in a good state of preservation, and that he would sell it. I took the first opportunity to examine the skeleton, and was not slow in deciding that it should be purchased for our National Museum. This skeleton was found in 1891 by the same native who found the one which was sent to the Czar. It was embedded in the sand to a depth of a few inches, and lay several rods from the present water-line. It is in a good state of preservation and proves to be very nearly complete. The cervical vertebræ are complete and show that the number is seven instead of six a point that was in dispute until settled by the study of this skeleton made by Mr. Lucas of the National Museum.

Unfortunately the anterior limbs are incomplete, and whether Steller's sea-cow had any hand or finger bones must still remain an unsettled question.

PLANT DISEASES, CAUSED BY NEMATOID WORMS OF THE GENUS APHELENCHUS BAST. I.

BY DR. J. RITZEMA BOS, MEMBER OF THE INTERNATIONAL PHYTOPATHOLOGICAL COMMISSION, PROFESSOR OF ZOOLOGY AND ANIMAL PHYSIOLOGY, AGRICULTURAL COLLEGE, WAGENINGEN, NETHERLANDS.

BIS vor kurzer Zeit waren blos aus den Nematoden Gattungen *Heterodera Gruff* und *Tylenchus Bastian* in Pflanzen schmarotzende Arten bekannt; in den letzten drei Jahren gelang es mir drei neue, bisher unbeschriebene Species aus der Gattung *Aphelenchus Bastian* als die Ursache von Pflanzenkrankheiten zu entdecken.

Bekanntlich sind die *Aphelenchen* den *Tylenchen* nächst-verwandt; es sind beide aalförmige Anquilluliden mit schwach geringelter Cuticula und mit einem Mundstachel hinter der Mundöffnung zum Durchbohren von Zellwänden. Während aber bei *Tylenchus* der Darm in der halben Länge des Oesophagus eine kugelförmige oder ovale muskulöse Auschwellung (den "Muskelmagen") besitzt, und nachher am Hinterende des Oesophagus eine nochmalige Auschwellung (den "Magen"), findet sich bei *Aphelenchus* wohl das erst genannte, nicht das zweite Organ, sodass der eigentliche Darm unmittelbar hinter den Muskelmagen anfängt. Es haben weiter die Männchen der *Tylenchus*-

arten eine Bursa, welche den *Aphelenchus*-arten fehlt (Vgl. 2B. Bastian, "Monograph of the Anquillulidæ," in Transactions of the Linnean Society, xxv., S. 122–124; auch de Man, "Die frei in der reinen Erde und im süssen Wasser lebenden Nematoden," S. 137).

Aus dem Genus *Aphelenchus Bastian* waren bisher 10 Arten bekannt; es wird aber wahrscheinlich herausstellen, dass mehrere dieser Arten unter sich identisch sind. Es wurde von diesen 10 *Aphelenchus*-arten *Aphelenchus Avenæ Bastian* zwischen den Blattscheiden und dem Halme von Haferpflanzen, *A. villosus Bast.* in Moosrasen, *A. parielinus Bast.* in Flechtenkrusten gefunden; ich finde aber keine Mitteilung darüber ob die beiden letztgenannten Arten als Schmarotzer in den Moosen resp. Lichenen, oder vielmehr *zwischen* den Blättern resp. den Teilchen des Thallus, lebten. Auch über irgend welche von *A. Avenæ* verursachte Pflanzenkrankheit findet sich bei Bastian gar keine Mitteilung, sodass bis jetzt unentschieden bleibt, ob diese *Aphelenchus*-species als wirklicher Pflanzenschmarotzer angesehen worden muss oder vielleicht blos zufällig in einer Haferpflanze gefünden wurde.

Ich habe bis jetzt drei *Aphelenchus*-species entdeckt, die zweifelsahm wahre Pflanzenschmarotzer sind, und von denen die eigentümliche von ihnen hervorgerufene Pflanzenkrankheit von mir studiert wurde. Es sind die folgenden Arten: *Aphelenchus Fragariæ nov. spec.*, *A. Ormerodis nov. spec.* und *A. olesistus nov. spec.* Eine Beschreibung dieser drei Arten will ich hier nicht geben; die ersten zwei Arten wurden schon von mir in Sorasier's "Zeitschrift für Pflanzenkrankheiten" (Bd. I., S. 7, 1891) beschrieben und abgebildet; die letztgenannte Art wird bald im dritten Bande derselben Zeitschrift ihre Beschreibung finden.

Aphelenchus Fragariæ fand ich in eigentümlich erkrankten, aus St. Paul's Cray, Kent (England), herkömmlichen Erdbeerpflanzen; es erkrankten auf einem Felde von 14 Acres etwa die Hälfte der dort wachsenden Pflanzen. *Aphelenchus ormerodis* wurde von mir in kranken Erdbeerpflanzen gefunden, die mir aus Erith (Kent, England) zugingen. Für diese beiden Arten verdanke ich das Untersuchungsmaterial der freundlichen Bemittlung Miss Ormerod's aus St. Albans, England, der zu ehren ich die eine Species benannt habe. *Aphelenchus olesistus* erkannte ich als die Ursache einen sehr typischen Erkrankung von Begonien-blättern, die mir Herr Dr. Masters (London) zugehen liess, sowie einer dergleichen Erkrankung von *Asplenium bulbiferum* und *diversifolium*, welche mir Herr Dr. Klebahn in Bremen zusandte.

A. Die von Aphelenchus Fragariæ verursachte Erdbeerpflanzenkrankheit. Dieser Nematode lässt im allgemeinen dieselben Abnormitäten bei der Erdbeerpflanze auftreten, welche die anderen Nematodenarten verursachen, wenn sie in Pflanzengeweben schmarotzen, nämlich eine Einschränkung resp. ein Stillstehen der Längenwachstums der Gefässbündel, gewöhnlich eine ungemein starke Verästelung derselben,—Hypertraphie der Parenchymzellen der Stengel, Aeste und Blätter,—starke Teilung, zuletzt Absterben dieser Zellen. Es versteht sich, dass dem Habitus nach sehr verschiedene Missbildungen entstehen, je nachdem eine Pflanze oder irgend welcher Pflanzenteil früher oder später von Parasiten heimgesucht wird; und je nachdem sich in demselben eine grössere oder geringere Anzahl von Anquilluliden befindet.

Bei den von vielen *Aphelenchen* bewohnten Erdbeerpflanzen findet sich eine starke Verdirbung aller Stengelteile und eine starke Verästelung sowie die Bildung einer grossen Anzahl neuer Knospen. In den Achseln der niedern, normal entwickelten Blätter finden sich zahlreiche, sehr dickschuppige Knospen, welche eine grosse Uebereinstimmung haben mit den kleinen Brutzwiebeln, welche sich innerhalb der ausgewachsenen Zwiebeln bilden; diese abnorm dicken Knospen bilden niemals Stolonen. Der Hauptstengel ist bei einigen Exemplaren anfänglich ziemlich regelmässig ausgewachsen (wahrscheinlich weil die Pflanze nicht sogleich von einer grossen Anzahl von *Aphelenchen* bewohnt wurde); aber in einer gewissen Höhe verästelt er sich stark; die Aeste find nicht nur dick und breit, sondern bleiben während ihres weiteren Wachstums auf eine grosse Strecke ihrer Oberfläche hin vereinigt, sodass wahre *Verbänderungen* ("*Fasciationen*") entstehen. Es bilder sich aber gewöhnlich keine bandförmige Stengelform, sondern eine Verdickung, welche sich am besten mit einem Stücke Blumenkohl vergleichen lässt, weshalb ich—in Uebereinstimmung mit Miss Ormerod—die von *Aphelenchus Fragariæ* verursachte Krankheit "die *Blumenkohlkrankheit* der Erdbeerpflanze" ("*Cauliflower disease*") genannt habe. In einigen Fällen aber bildet sich eine einfache, bandförmige Verbreiterung, also eine wahre Verbänderung des Stengels resp. des Astes, während die an derselben befindlichen, immer sehr zahlreichen Blumen, oder Blattknospen, mehr oder weniger normal zür Entwickelung kommen.

Bisweilen auch ist das Wachstum auf der einen Seite des Stengels oder des Astes, welcher eine Verbänderung bildet, kräftiger als auf der anderen Seite; es entsteht infolge dessen eine Biegung des betreffenden Teiles, welche sich so sehr steigern kann, dass letzterer sich ganz zusammenkrümmt. Oft teilt der Gipfel des Fasciation sich wieder in eine grosse Anzahl verschiedener Aeste, welche mehr oder weniger normal entwickelte Blüten und Blätter tragen.

Allein am meisten kommt es vor, dass der Stengel oder der Ast sich nicht nur in die Breite sondern auch in die Dicke vergrössert; die Seitenäste verwachsen entweder zum grössten Teile oder gänzlich mit einander, und die Knospen kommen nur ausnahmsweise zu vollkommener Entwickelung. In diesem Falle ähnelt ein grosser Teil der kranken Pflanze sehr dem Blumenkohle oder dem Broccoli, je nachdem die Knospen entweder gar nicht oder doch noch teilweise zur Entwickelung gelangen und normale oder abnorme Blüten entstehen lassen.

Bisweilen bleibt der Stengel sehr verbreitert und kurz, und sind die Knospen an seinem Gipfel, oder vielmehr an seinem Kamme, zusammengedrängt, wie beim Hahnenkamme (*Celosia cristata*); bisweilen zeigen letztere sich auch an den Seiten des Achsenteiles, und zwar infolge des unregelmässigen Wachstums, sehr unregelmässig verbreitet, oft in grosser Anzahl dicht zusammengedrängt, eine bedeutende Oberfläche einnehmend. Gewöhnlich aber finden sich die Knospen, ganz wie beim Blumenkohl, auf dem grössten Teile der Oberfläche der zu einer dichtgedrängten Masse veränderten Achsenteile. Die Aehnlichkeit mit Blumenkohl kann wirklich eine sehr grosse sein.

Von den Blättern der von *Aphelenchus Fragariæ* heimgesuchten Pflanzen sind gewöhnlich zwar einige normal; viele aber bleiben immer klein, wobei die Blattfläche verhältnismässig kleiner bleibt als der Stiel; bisweilen ist die Blattfläche nicht mehr dreizählig sondern aus einen Stücke bestehend; auch ist sie oftmals gefaltet.

Die Blütendeckblätter sind gewöhnlich klein, oder sie sind zwar kürz, aber dick und unregelmässig gefaltet.

In Betreff der Blütenknospen bemerke ich folgendes: Bisweilen ist der Achsenteil sehr dick und bleiben die Blatteile sehr-dünn, schuppenförmig. Bisweilen werden die Blatteile zwar dicker, bleiben aber nichtsdestoweniger kurz und behalten den Habitus von Schuppen. Oft sind dann die beiden Blätterreihen des Kelches ("Calyx duplex") vollkommener als die anderen Reihen von Blütenblättern entwickelt. Oefter bleibt die Knospe ganz oder fast ganz geschlossen; in anderen Fällen aber öffnet die Blütenknospe sich.

Die äusserste Blätterreihe des Kelches besteht oft aus dünnen, schmalen, sogar nadelförmigen Blättchen. Die Blätter der inneren Reihe kommen gewöhnlich weit mehr zur Entwickelung; sie sind oft mehr oder weniger gefaltet und bisweilen an der Unterseite blasenförmig angeschwollen. Oft sind sie mehr oder weniger gelappt, gespalten oder eingeschnitten; sie können auch dreizählig sein und also die Form der gewöhnlichen grünen Blätter nachahmen.

Die Kronenblätter bleiben oft ganz rudimentär. Bisweilen kommen sie zwar zu weiterer Entwickelung, bleiben aber kleiner als die Kelchblätter, biegen sich hin und her und falten sich; sie sind dann aber nicht weiss, wie die gesunden Kronenblätter, sondern grünlich weiss bis hellgrün, allein dünn und zart wie die gewöhnlichen Kronenblätter.

In vielen Blüten fehlen die Staubblätter oder sie sind rudimentär; bei anderen Staubblättern ist der Staubbeutel normal, der Faden aber dick und kurz.

Der Blütenboden, d. h. der Achsenteil der Blüte, mit den auf demselben eingepflanzten Pistillen, bleibt in vielen Fällen sehr klein; letztere können auch gänzlich fehlen. Bisweilen entsteht eine anilläre Prolification der Blüten, und zwar immer in der Weise, dass in den Achseln von zwei bis drei Kelchblättern sich neue Knospen bilden, aus denen aber wohl niemals normale Blüten entstehen.

Uebrigens versteht es sich von selbst, dass an den weniger heimgesuchten Pflanzen auch ziemlich normal entwickelte Aeste, Blätter und Blüten vorkommen.

Die von *Aphelenchus ormerodis nov. spec.* und *A. olesistus nov. spec.* verursachten Pflanzenkrankheiten werden in der zweiten Abteilung dieses Aufsatzes beschrieben werden.

BURIED ALIVE, — ONE'S SENSATIONS AND THOUGHTS.

BY WARREN K. MOOREHEAD, 5215 WASHINGTON AVE., HYDE PARK, CHICAGO.

THE title of a paper written for *Science* — "Buried Alive" — seems rather sensational, and, so far as the title goes, the article might be more properly published in one of the daily newspapers. I have made bold to write upon an unpleasant experience of the year 1888 at the suggestion of several friends interested in studying suspended respiration. They have told me that cases of complete burial in earth (the subject being conscious meanwhile) where the person "interred" escaped with his life and was able to give a satisfactory or intelligent account of his feelings, are extremely rare. They suggested that, as my accident would furnish material for consideration among medical men interested in kindred studies with themselves, it had better be described.

A mound was being excavated near Frankfort, Ross County, Ohio, in August. At the centre the wall (from the base-line upwards) was fifteen feet high. It was undermined by the workmen, and, as I bent down to examine a small bone uncovered in the process of undermining, a mass of earth equal to several cartloads suddenly dropped from above.

There was no one in the excavation, the men having gone on top preparatory to cutting down the undermined wall. As the earth cracked loudly, I looked up and started to rise. The falling mass knocked me back about five feet, so that I fell with my head and shoulders resting upon a heap of loose earth. The falling wall was, of course, seen only for an instant. It looked black, and the rush of wind it caused I well remember. My head and shoulders were somewhat higher than my legs, possibly a foot. The feet were spread apart. There was little pain, only pressure, *intense pressure*. It forced the buttons of my light field costume partly inside the flesh; my watch-chain left a bright-red mark along my left side. I could feel the watch strongly pressed against two ribs (these were broken). The skin over my forehead seemed being cut, but it was the pressure of my hat forcing the flesh between the laced straws. A knife in my pocket seemed burningly hot. Just under the small of my back lay a large clod. The pain at the point of contact was considerable at times, and my spinal column seemed slowly breaking. Then the pain stopped and I could feel nothing.

Thoughts coursed like lightning, — past life, future, and home. I did not think much of the situation, except to wonder if I could breathe when I got out. One singular thought occurred. I remembered reading of women who, in war times, buried their husbands in ash-piles or sand-heaps to prevent their being drafted into the army. I had often wondered if it were possible for one so placed to breathe through a tube, as described in the stories. I remember trying to move a hand, even a finger. One could not have been more firmly held in a mould. My arms and hands were perfectly motionless. The chest could not be inflated or moved the slightest distance. On the contrary, the downward pressure forced all the air out of my lungs. I remember how hot the earth against my face became as the last breath was forced from me. Just in front of my mouth and chin was a slight hollow, formed by the arching of two good-sized lumps of clay. I could move my chin and open and shut my mouth. That was the only part of my entire body that could be moved. I remember trying to keep my mouth shut to keep out the dirt. But after a few seconds my mouth instinctively opened, and, the arch having broken down, earth filled it. I remember the horrible sensation of trying to dislodge the earth and the fear of strangling that suddenly seized upon me. I then felt that I was doomed to perish, but had no fear and did not particularly care.

It was sixty seconds, so the surveyor says, when the men reached my head. The laborers think it was over a minute, but I am inclined to believe the surveyor. I felt the earth move slightly above my head. That gave me hope. I had not thought much of rescue, but I gathered my remaining strength. A shovel passed across the top of my head, cutting the scalp; I remember feeling it as if a hot iron had struck me. Then they uncovered my head and removed the earth from my mouth and eyes. For some unaccountable reason they stopped for an instant. The surveyor says the pressure was so great upon the imprisoned portions of my body that the blood was forced to the head, and the veins stood out so strongly he feared they would burst. Even with the head uncovered I could not breathe. They soon had me laid outside upon some wheat sheaves. I remember, just as they carried me out, seeing a little yellow "wild canary" perch upon a tall thistle near at hand. I heard it sing a sweet song. As the bird flew away, I seemed to follow it, dancing about the fields, perching upon this and that shrub, just as it did. The sky seemed to have a different color from that usually noticed, I was impressed with its grandeur, — the scenery of the surrounding country was remarkably beautiful, and as I observed all these things they affected me, and I cried.

They rubbed my limbs, I could see the men at work, but could feel nothing. The partial paralysis of my limbs continued for some days. To some extent the accident has affected my mind. I cannot now enter an underground cave, or mine, or stand under an overhanging bank without an effort; it requires all my will to go in them. I also often dream of caving banks and experience precisely the same feelings as I did in reality. I neglected to state that the earth above my head was about three feet thick, that over my legs was much deeper. Many persons buried in gravel pits and in earth not nearly so deep have been taken out dead.

NOTES AND NEWS.

IN a letter to Dr. Charles S. Minot the method used by Dr. M. von Lenhossék to obtain his remarkable results on the nerves of earth-worm, is described as follows: The method cited by me corresponds to Golgi processo rapido: Pieces of an earth-worm, each three-quarters millimeters long are placed for three to five days in about ten cubic centimeters of the following mixture: Bichromate of potasssium, 3.5 per cent, four parts; perosmic acid, 1 per cent, one part. The pieces are then dried off with filter paper, and placed for about forty-eight hours in the second solution of 0.75 per cent nitrate of silver, to every two hundred cubic centimeters of which one drop of formic acid is added. As soon as the pieces are placed in the second solution a reddish brown silver precipitate is thrown down upon their surfaces; the success of the method depends upon this precipitate being formed in the interior of the tissues also. The pieces after this treatment must be hardened rapidly in absolute alcohol (probably a large quantity of 96 per cent alcohol will act equally well), and are then imbedded in elder pith and cut with the microtome. If the reaction has been successful, the nerve-fibres and the cells from which they spring will show the well-known and characteristic Golgi coloration (almost black owing to the silver deposit). If the first attempt at the reaction fails, the coloration may be often obtained by repeating the sojourn in the two liquids as above directed. But even after double treatment the reaction is often not accomplished, but when it succeeds it amply repays all the trouble and vexation it causes. The sections must be mounted at once in Canada Balsam dissolved in xylol (or benzole), and left without a cover-glass. (In the second volume of the "Anatomische Hefte" a method is described by which Golgi preparations may be made so permanent that they may be mounted with a cover glass.) It is by means of this method that Lenhossék made the discovery that the sensory nerve-fibres arise from the sensory cells of the epidermis and branch in the same manner as in vertebrates, forming within the central nervous system a branch running tailward and another running headward.

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Attention is called to the "Wants" column. It is invaluable to those who use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The "Exchange" column is likewise open.

THE RELATION OF ALIMENTATION TO SOME DISEASES.[1]

BY JAMES WOOD, M.D., BROOKLYN, N.Y.

THE general statement that one has partaken for a considerable time of an incorrect diet gives the impression, and true, that the body generally is affected, and nowhere is this more strikingly seen than when acute disease attacks an organism whose habit of feeding has been faulty. We have therefore the salient thoughts that improper food induces abnormal functional performance, which by continuance becomes organic; that an improper diet lessens the chances of recovery from acute and chronic conditions, and that improper alimentation prolongs convalescence from disease processes and predisposes to a diminished vitality.

The thought of first importance, however, is, To what extent does disease depend on alimentation? This is answered by considering the subject of the relation of health to alimentation. As a usual condition we each had given to us at birth a body very well suited to continue to exist if properly nourished. Any hereditary influence, with the exception of a few instances, is merely a decrease in the complement of vitality. What is meant by that is best illustrated in the case of consumption. It is a very common error to hear both the profession and laity remarking that consumption is hereditary. This we dispute, and on the best of grounds consider the one factor of the three, a lowered vital tone only as being transmitted. This lowered vitality being so often dependent on transmission makes the consideration of what food should be partaken of by progenitors of the race their most important thought if we desire to give to our offspring a constitution capable of withstanding the adverse influences met with in life. This can be done only by using a diet whose quantity and quality bear a proper relation to each other. Why you ask? Because the single cells and their sum the body does not remain in an unchanged condition: there are two great phenomena constantly taking place in each individual cell. Nature calls for such a quantity of proteid matter as, when appropriated by the organism, will meet the daily nitrogenous expenditure as shown by the excretion of the normal amount of urea. The intake of oxygen and food meets the demand on the part of the various cells for nutritive pabulum to carry on the anabolic or constructive processes of the body.

The second phenomenon commences with combustion or oxidation, and passes through a long series of destructive or katabolic phases to the formation of nitrogenous metabolites, "and this process is carried on in an organism with an activity which is dependent on the activity of the living substance itself, and on the quantity of material supplied to it."

The discharge of these products of katabolic metabolism is termed excretion. From a study of these we are enabled, as it were, to glance back over the whole series of vital processes and ascertain in which one there exists an abnormality.

To continue in perfect health, therefore, such food products must be partaken of which shall insure the perfect functional workings of the body, supply elements to carry on vital action and give material to build up degenerated tissue, or, to be more general, we must supply each day the needs of the body which have been brought about by its activity. More than this quantity or such as is improper in quality will act as a deleterious agent and destroy to that extent vitality. The subject of the use and misuse of vitality is very large, and we must, to be brief, consider it as an element whose quantity is limited, depending largely on the physical condition of our ancestors. We have such a proportion given us as will, with proper care, last us for the natural allotment of years. To misuse it means succumbing to disease before our time, just as the athlete by the expenditure of such a large amount of vitality each day in the perfect training of his muscular organs uses more than can be formed for any length of time by the transforming powers of the organs of digestion. When these become used up then he, of necessity, must die. Had these organs been the study of successive generations, the standard of their power to produce vitality could have been raised and physical and mental vigor prolonged and increased.

As we have before stated, there must exist an equilibrium between production and destruction if we will have perfect health. The condition of production is dependent solely on the quantity and quality of the food; and when we consider that the whole process of animal life is a constant metamorphotic progression, only limited by the varied isomeric forms which the nutritive elements are capable of assuming under the pressure of organic influences, we are capable to some extent to appreciate what a great influence the nature of the nourishing bodies must have on a continued normality.

If we use up a large part of the oxygen of the body by oxidizing a diet composed largely of the starches, sugars, and fats, we will have but an insufficient amount left for the complete transformation of the food ingested into its kenitic or final products. It was shown in an article written for Merck's Bulletin of last year that these products of suboxidation of the proteids belonged to the most poisonous agents of which we have any knowledge, i. e., ptomaïns, leucamaïns, etc. The absorption of these products of but partial oxidation, leads to a profound state of malnutrition, with all its accompanying symptoms and sequelæ.

Jaksch, in his investigations, found that in anæmia the precursors of uric acid in the blood united in that fluid instead of the renal epithelium, so greatly were the functions of the body at fault.

Is it not evident in this condition that we have a frequent source for the derangements of bodily action and disease?

By the suboxidation of the proteid food-stuffs from the ingestion of large quantities of the corbohydrates, which is the general evil, we have another cause or predisposing factor besides the ptomaïnic poisoning to a certain distinct line of abnormal conditions.

If the quantity of food ingested is too large, we have, from the inability of the system to transpose such a large bulk completely, the same conditions as above, or a quantity beyond what nature demands exhausts the limited oxygenating capacity of the blood and causes the appropriation of that oxygen which should go for the complete transformation of the more difficult nitrogenous compounds. Thus, from an incomplete oxidation of these latter compounds, we get but partial metabolic changes; derangement of the organs of secretion and excretion rapidly follows, which in turn gives products antecedent to perfect metamorphosis, and the final result is a systemic poisoning.

Thus we see if a larger quantity of food is eaten than can be perfectly oxidized in the body, and especially if the starches, sugars, and fats be in preponderance, imperfect results of general bodily oxidation must take place. If this supra-feeding should continue for a certain time, with its resultant incomplete products, a devitalization of the protoplasmic elements of the hepatic cells occurs, with serious deterioration of the most important func-

[1] A portion of a paper read before the regular meeting of the New York Academy of Anthropology, Jan. 17, 1893.

tions of the liver and kidneys. In consequence of these abnormal changes in such important organs and the decrease in the oxygenating capacity of the body, a host of incomplete katabolins is developed and retained to a large extent within the organism.

The fact becomes very prominent, therefore, that much of the ill-health and almost all of the cases of uricæmia can be traced to the universal habit of over-eating. A strong healthy individual whose life is spent entirely in the open air and at vigorous work can ingest greater quantities and varieties of food than is necessary to supply to the system the requisite amount of nutrition and energy, without suffering much from the indulgence. If the stomach does not reject this burden at once it is largely taken care of by the system. When the varied capacity in different persons for storage is exceeded, the organism balances the accounts by a period of vomiting and misery incident to a bilious attack. After middle life these attacks may become less frequent and the excessive amount of food is changed into fat and the individual becomes more corpulent, providing a facility for converting elements into fat is consistent with the constitution of the organism. Some people seem incapable of storing fat however rich the diet or sedentary their habits, and as this over-supply of nutritive pabulum must go somewhere, we find that it is but partially used by the system and the larger part exists in the organism as irritating elements, becoming a most potent factor in inducing functional derangement of the liver and other organs or manifesting itself as gout, rheumatism, diabetes, etc. In persons whose occupation keeps them in illy-ventilated rooms for long periods and whose general system is consequently in a devitalized condition, nature is not so kind, but jumps at once to the more serious complaints.

The perfect performance of no functions is probably so important as those of the liver and intestines. They both lie at the gateway to the system, and if by improper food they are deranged damage must follow.

One of the most important functions of the liver is to prevent the entrance into the general system of those poisonous agents above referred to

When we recognize the fact that all forms of extraneous and poisonous substances which are introduced into or are developed within the system "are carried to the liver, and there acted upon by the chemico-physiological transforming power of the protoplasmic masses composing the cells of that organ, it is quite easy to see and comprehend how derangement, faulty action, or even an absolutely pathological condition, of these cells is developed." Any one of these three conditions will disturb normal action on the part of the liver, and will tend to derange one of its most important functions, that of bile formation — a function upon which the whole matter of digestion and assimilation are dependent. This is very evident when we consider that the greater part of the digestion of the food-stuffs takes place in the intestinal canal. On imperfectly formed biliary fluid on reaching the alimentary canal will be unable to perform those functions delegated to it by nature, such as the emulsification of neutral fats, conversion of starch and glycogen into sugar, exciting contractions of the muscular coats of the intestines and assuring an evacuation of its contents, stimulation of the muscles of the villi, which empty the nutrient sacs into the lacteals, exciting the vital activity of the intestinal epethelium and thus determining the absorption of digested material, moistening the intestinal wall, lubricating the colon, and last, and most important, preventing decomposition, and thereby preventing auto-intoxication. As a consequence of the bile not fulfiling its many offices, fermentation is rapidly excited within the bowel, producing many delederious products, which pass through the the entero-hepatic circulation and reach the liver, thus increasing the work of an already overtaxed organ. "This, together with the damaging effects of the original poison and imperfect oxidation, explains the chief factors in the development of increased bodily heat in connection with all diseased conditions. In this manner the process continues repeating its injurious effects upon the system, until nature, by her inherent power, grants relief by destroying or eliminating the poisons, or is aided in so doing by the skilful administration of some medical agent." In a certain percentage of cases, however, both the inherent power of nature and the skill of the physician are held in abeyance.

"By converting all the food-stuffs into a thoroughly and easily diffusible fluid, by decreasing to the minimum the products of intestinal fermentation, and by stimulating the activity of the gut, we have produced a condition which favors the most rapid absorption attainable, with the least expenditure of force, and, as a result, there is carried to the liver and system at large a completely digested intestinal product — a nutritive pabulum which contains the smallest possible amount of effete or deleterious matter."

This is of vital moment: for there exists a certain relation between the imperfect workings of the vital forces, and disease. If the organism ingest food ill-suited for its needs, functional derangement will soon occur. Thus, it has come to be recognized by most physiologists and pathologists, that many of our chronic diseases which occur largely in late life are the result of a state of malnutrition and a consequent long-continued physiological derangement — rather than of an inherited vice. This functional perversion is largely due to the habit of feeding the growing child on a diet composed mainly of the starches and sugars. It is a most common sight to see our children eating freely of the confectioner's goods, and as they grow older desiring the rich pastries, marmalades, etc., until an almost passionate and insatiate fondness for this non-nutritious and highly stimulating diet is induced and they soon prefer this kind of food to the plain, nutritious nitrogenous compounds.

This stimulation soon fails to uphold the buoyancy of spirit and apparent good health, and a condition is left which becomes a great deteriorating factor.

When these individuals attempt to accomplish great physical or mental tasks, there is noticed a greater and more rapid expenditure of vital force than nature intended. The demand on the part of the system for a strong stimulant is not fully met by the sugars ($C_6H_{12}O_6$); and the more active C_2H_6O (alcohol) is called into requisition. Thus future habits of intemperance often have their origin in this simple cause.

You may ask, Is not a liking for strong drink inherited? We answer, No! But a weakened system craving for something to stimulate it is, and if not corrected by a non-stimulating nitrogenous diet in youth, those who fail through ignorance or otherwise must be held responsible.

All of these factors leave the system in a much lowered condition and offer an organism which is a good pabulum for disease-germs and poisons of all kinds, and incapable of withstanding their inroads. One has but to call to mind the number of cases of tuberculosis following a continued state of malnutrition; indeed, this lowered bodily tone is one of its chief etiological factors.

There are two great collections of forces, therefore, which attempt to keep the body in a healthy condition. If you have a derangement of either intestines or liver you will certainly have disease following its continuance. If you have a condition of suboxidation of the proteid food-principles you may expect certain diseases which are caused by the retention in the body of the products of such an abnormal condition.

Let us glance very briefly at some of the diseases depending upon the antagonizing power of the intestines and liver being below normal. We place cholera among the first. It was found during the recent plague of this disease in St. Petersburg and Hamburg that the cholera attacked those who had acute intestinal complaints and chronic gastric or intestinal indigestion, who ate food poor in quality, i.e., coarse, badly cooked, or partially decomposed meats, fruits over or under ripe, or who were addicted to the use of alcohol. The greatest mortality was among those whose habits of life in eating or otherwise produced or had a tendency to produce a diminished vitality. Cholera, to the one whose liver and intestines are in a normal condition and who eats wholesome and proper food, is not a disease to be dreaded.

Typhus fever but rarely attacks the healthy subject, but is very fatal to those who, by reason of fatigue, starvation, or other conditions, are below the requisite standard of health.

Typhoid fever is the same. In the healthy body the chances of the peculiar germ living and producing harmful effects is very small. This broad statement is applicable to all kinds of infectious intestinal disease.

Very interesting is the study of tuberculosis. According to the latest authorities, consumption is dependent upon three factors: First, decreased vitality, antagonizing powers, or what you will. This we have spoken of, and in what manner it is induced or transmitted. Second, an active inflamatory condition. This may be pneumonia, bronchitis, laryngitis, or the like. Third, the presence of the bacillus tuberculosis. Without these three there can be no consumption. It is the usual thing, in the examination in the dead-house, to find evidences in the lungs that the subject had at some time a commencing consumption, but the vitality had been so great that nature had encapsulated the infected part with tissue of high vitality and the condition became inocuous. The fatality from tuberculosis, then, is dependent on a decreased vitality, and we must look to a proper kind of diet and a consequent increase in the general antagonizing power of the body for the remedy.

Those diseases dependent on or induced by suboxidation are very many. A few only will be mentioned by way of illustration. If we take a proteid molecule ($C_{72}H_{112}N_{18}O_{22}S$) and attack it by 139 molecules of oxygen we will have the normal oxidation and the usual excretory products given off, namely, urea, uric acid, kreatinine, carbon dioxide, water, and a molecule of sulphuric acid appearing as a sulphate. If attacked by 136 oxygens only we have the same but with an increase in the amount of uric acid. At this stage we have a condition present which is at the bottom, probably, of more diseases than any other. So that we consider the presence of an abnormal quantity of this acid in the renal excretion to show a condition of suboxidation of the nitrogenous elements of the food-stuffs. Had the nutritive compounds been completely transformed within the system they would have been eliminated as urea — a compound very soluble and easily handled by the organism in proper amounts. But such not being the case we find that the failure in its elimination gives us many diseases. People who have this condition are greatly disposed, by the antecedents or isomers of this acid in the tissues, to congestive conditions of all the structures where such compounds exist, but more especially the naso-pharyngeal mucous membrane and the intramuscular planes. They suffer from dyspepsia, functional disturbance of the liver, palpitation of and peculiar feelings about the heart, bronchial affections, often iritis, eczema, and a number of peculiar symptoms generally known by the obscure term, neurasthenia. They are most sensitive to changes in temperature and atmospheric density, declaring they cannot live in certain localities, and, in fact, suffer from general bodily derangement. We look at uricæmia in this wider and more general way and recognize its influence in connection with many of the vague abnormalities of childhood. Some observers have found as high as 30 per cent of the children — especially those confined at school — troubled with neurasthenic and other incomplete expressions of defective metabolic action. A very large percentage of the nervousness and ill-health of young women has this condition as one of their chief ætiological factors.

In an article on the "Pathology and Rational Treatment of the Uric Acid Condition," in Merck's Bulletin of last year, it was shown that the prevalence of uricæmia was very great and because of the almost universal habit of partaking of food whose nature as a nutrient compound is bad and whose quantity far exceeds the physiological demands, especially during early youth, some degree of uricæmia under twenty years is almost the general rule. Let us look at the frequency of uric-acid calculi and the age when they most frequently occur. From a large number of cases of calculi found in the bladder by English observers, 83 per cent were of uric acid. In America the percentage is about 78. If we take the cases of Civiale, Coulson, and Thompson, numbering in all 10,467, we find that 6,524 or 62⅓ per cent were under twenty years of age. In the statistics of 3,574 cases in this country, 4,986 or 58 per cent were under twenty.

Returning to the same method of investigation, we find that if only 129 oxygen elements are used we have the condition known as oxaluria. If we fall still further below the normal and have but 94 elements of oxygen to attack the proteid we have lactic acid formed, and rheumatism, neuralgia and the like as the result. If we have but 76 elements to attack the proteid molecules, we have as one of the products of incomplete metamorphosis glucose, and thus either temporary or permanent diabetes.

Has not enough been said to show that suboxidation is a dangerous if not fatal condition? Why should we multiply difficult chemical explanations for known clinical and every-day facts?

Bright's diseases are probably more often caused by the same condition than any other. You really call upon the kidneys to do more work than normally in taking care of increased quantities of refuse matter because of the large quantity of food ingested and at the same time place it upon decreased nutrition. The result is, you have first a functional derangement which it is possible to disperse, and then an organic condition which it is impossible to remedy. Those who understand the science of proper feeding and apply that which they know to their cases of Bright's get results satisfactory both to their patients and themselves, otherwise their patients inevitably get worse and die early.

The accumulation of fat in the tissues, or obesity, is a pathological or diseased condition. All the fat that is added to the body above five per cent of the total bodily weight is usually the result of an abnormal physiological condition of the nutritive system. Obesity is the result of an incomplete oxidation of the proteids with the formation of fat as one of the by-products resulting from an imperfect metabolism of such bodies. This is substantiated by fully recognized chemical laws. It is Professor W. H. Porter who says that "while this abnormal amount of adipose tissue may perhaps to the ordinary eye beautify the macroscopic appearance of the individual, it is no guarantee of a sounder constitution or a higher vitality in the microscopic and chemical construction of the bodily tissues, generally it indicates the reverse or that a pathological condition is hidden beneath this superficial beauty."

We have not spoken of a large number of diseases in which an incorrect diet is an important factor in their continuance, space will not allow; enough has been said, however, to call attention to certain facts of great importance. First, we should understand which kind of diet is the best suited to furnish to the body the elements which it requires daily; second, the constituents of the diet should be such as will give nourishment to the body and use but a minimum of vital force in its preparation; third, the quantity ingested daily should be such as will maintain an equilibrium between production and destruction, this is determined by a study of the renal excretion; fourth, auto-intoxication by products of decomposition and fermentation in the intestines is prevented by the application of the above facts; fifth, entrance of deleterious agents into the entero-hepatic circulation is prevented by preventing hepatic derangement; sixth, suboxidation is a dangerous condition, and has as its sequelæ a definite line of disease processes.

In conclusion, we desire to impress upon the mind that there exists a very intimate relation between imperfect alimentation and organic or functional derangement, and that as we are understanding more about diet and the proper food principles forming it each year, we appreciate its enormous importance.

We are rewarded when supplying a scientific course of diet and regulating the same by a conscientious study of the renal excretion, by seeing abnormal processes of the body give place to normal, acute diseases decrease in mortality, convalescence speedy and complete, and chronic conditions ameliorated, and comfort replacing pain and annoyance.

SNOW-ROLLERS.

BY DR. E. W. CLAYPOLE, AKRON, OHIO.

In the early part of last year I received from a former student a letter telling me that in the place where he is now residing (Milledgeville, Fayette County, O.) a very curious phenomenon had been observed. After a light fall of snow the ground was strewn with small balls, light and fragile, the like of which no one could recollect having previously seen.

Recognizing from his letter that he was writing of a case of the formation of the rather rare "snow-rollers," I wrote immediately requesting details, and received in reply the following letter:—

MILLEDGEVILLE, O., Feb. 7, 1892.

PROFESSOR E. W. CLAYPOLE.

Dear Sir: On the morning of Jan. 30, 1892, a curious phenomenon was witnessed here — snow-rollers — of which I send account. I found it difficult to obtain trustworthy information as regards extent of area. None of the city papers spoke of that, and only quoted from local county papers, the correspondents of which furnished all that I saw regarding the occurrence. The rollers may have extended over a very large area in southern Ohio. They were formed in the streets of Wilmington, in Clinton County, though they are not mentioned in the adjoining country. Wilmington is twenty miles from here. No one residing a mile from here, in any direction, with whom I have spoken, witnessed the phenomenon.

People here are divided in opinion whether they fell or were formed by rolling. Our local editors alluded to their correspondents who spoke of the balls as bearers of "fish-stories," refusing to believe them.

The morning of Jan. 30, 1892, presented in this vicinity a phenomenon of nature as striking as it is rare. The surrounding clean, level fields were covered with balls of snow, varying in size from three to five inches long and from one to two inches wide. Wheat-fields and meadows abounded with these balls, and suggested, at first sight, that a troop of school-boys had been having a battle with the snow.

Two fields, of thirty acres each, that came under my observation (one a new-sown wheat-field and the other a meadow) were literally covered with these "snow-rollers," there being at least 500 on the acre. Roadsides and lots contained a few, and, what is remarkable in this connection, I noticed them on housetops and straw-ricks.

On close investigation, I found the balls to be uniformly light and fragile, so that to lift one and preserve its form was impossible. Some were oblong, some almost spherical, while others resembled a tea-cup or small bowl.

There were no tracks behind them, or, if these had been made, the falling snow had obliterated them.

The accompanying weather conditions were as follows: The ground had been covered with snow for three weeks. A crust had formed on the top, thick and firm enough in places to bear up a person. This thawed a little during the afternoon of the 29th. The ensuing night was warm, the mercury registering 40° F. By ten o'clock a brisk wind was blowing, which increased in velocity, and soon the snow began to fall in large, moist flakes. The morning showed that about a half-inch had fallen on the crust, and on this lay the balls.

The phenomenon was reported from several places in this vicinity, chiefly in Fayette County, and from Clinton County, which adjoins it on the west, but nowhere did the rollers extend uninterruptedly over any great area. W. S. FORD.

In reply to a later enquiry, Mr. Ford informed me that no one took a photograph of this interesting occurrence. This is much to be regretted, as I am not aware that a view of any kind is in existence showing these snow-rollers. The chance of obtaining a unique negative was lost.

I cannot say if the phenomenon here described is really so rare as the scarcity of published accounts would lead us to infer. Perhaps the publication of this note may lead to clearer knowledge on this point.

Not having access to the earlier scientific literature on the subject, I am able to quote only a few instances of snow-rollers. Several years ago there was a short correspondence in the columns of *Nature*, from which I condense the following statements.

In the issue of March 29, 1883, Mr. G. J. Symons wrote that he believed that the first recorded account of the phenomenon appeared in an early number of the Philosophical Transactions, from the pen of Dr. Clouston of Sandwich Manse, in the Orkney Islands.

Mr. Symons adds that he has heard of but one case in England. This was reported by Admiral W. F. Grey in the *Meteorological Magazine* for May, 1876. It occurred on his lawn in the south-east of England, and the balls or rollers varied from the size of an ordinary snow-ball to that of a cubic foot, and each one left a decided track to the leeward. In this case they were seen to form in the evening.

The correspondence above quoted was called out by a letter from Mr. S. Hart of Trinity College, Hartford, Conn., to the editor of *Nature*, mentioning that on Tuesday evening, Feb. 22, 1883, a light, damp snow fell on the crust that had formed over the earlier snowfall, and that a brisk wind sprang up after midnight. Next morning on the college campus and in the adjoining park and empty lots numbers of natural snow-balls, spherical and cylindrical, from twelve to eighteen inches in diameter, and hollow at both ends, were strewn over the ground. Behind them were visible to leeward tracks 25 or 30 feet long in the new-fallen snow. The rollers were so light and fragile that handling was impossible. A few of them could be traced 60 feet and some had even been rolled up hill. They were especially fine on the frozen Connecticut River.

The same writer also reports a similar occurrence in New Jersey in 1808, when the rollers were seen to form in the day-time, and extended over at least 400 square miles of country.

In a subsequent letter, printed in the number of *Nature* for March 6, 1884, Mr. Hart returns to the subject and reports a repetition of the phenomenon in Oneida and Herkimer Counties, in New York, on Tuesday, Jan. 22, 1884, adding that the rollers were of the same size as in the former case, but were in some instances firm enough to be picked up and handled without crumbling. This is, so far as I know, the only occurrence in which this has been reported possible.

The wide experience of Mr. Symons is sufficient ground for assuming that the formation of snow-rollers is not frequent in Great Britain, and the scarcity of records here leads to a similar conclusion for this country. Further reports on the subject are desirable.

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.

Some Detailed Evidence of an Ice-Age Man in Eastern America.

MR. HOLMES'S statement in his communication to *Science* for Jan. 20, that "If there was, as is claimed, an ice-age man or at any rate a palæolithic man in eastern America, the evidence so far collected in support of these propositions is so unsatisfactory and in such a state of utter chaos that the investigation must practically begin anew," should not be allowed to go unchallenged. I will content myself, however, by giving the details in a single case, namely, those concerning the implement which was found in 1889 by Mr. W. C. Mills at Newcomerstown, Ohio, and which is now in the collection of the Western Reserve Historical Society in Cleveland.

Though the discovery was made in October, 1889, it was not brought to public notice until the next spring, when I chanced to meet Mr. Mills and learned about it. He then forwarded it to me, when its exact resemblance in form and finishing to an implement which I have in my own collection, that was obtained by Dr. Evans of London at Amiens, France, greatly impressed me. I forwarded it immediately to Professor H. W. Haynes of Boston, whose expert judgment is second to that of no other person in America, or indeed of the world. Professor Haynes exhibited it at the meeting of the Boston Society of Natural History on May 7, 1890, and his account was published in the Proceedings of that evening. In conclusion, after having enumerated its distinctive characteristics, he said, "I desire to express most emphatically my belief in the genuineness and age of this Newcomerstown implement, as well as to call attention to the close resemblance in all particulars which it bears to those unquestioned palæolithic implements [which he exhibited beside it] of the Old World." This implement is not a "reject," but is a finished implement, with the secondary chippings all around the edge. The cuts, reproduced from photographs, on pages 252 and 253 of my volume on "Man and the Glacial Period," perfect as they are, by no means do the implement justice.

I promptly gave an account of this discovery in *The Nation* in its issue for April, 24, 1890, and repeated it in substance with some additional particulars on page 620 of the third edition of my volume on "The Ice Age in North America." The account in my later volume is still more condensed. The detailed evidence is published in Tract No. 75 of the Western Reserve Historical Society, Cleveland, Ohio, which contains the report of the meeting when Mr. Mills was present and gave his own testimony. This was held Dec. 12, 1890.

The facts are these: There is a glacial gravel terrace in Newcomerstown at the mouth of Buckhorn Creek, where it enters the larger valley of the Tuscarawas River. There can be no question about the glacial age of this terrace. It is continuous up the river to the terminal moraine. Its surface is about 35 feet above the flood-plain of the Tuscarawas; it consists of stratified material, containing many granitic pebbles and much granitic gravel. The deposit at Newcomerstown extends over many acres, having been protected from erosion in the recess at the mouth of Buckhorn Creek. Through the middle of this deposit the railroad has cut its road-bed, and for years had been appropriating the gravel for ballast.

Mr. Mills is an educated business man, who had been a pupil in geology of Professor Orton of the State University, and had with him done considerable field-work in geology. Mr. Mills's character and reputation are entirely above suspicion. In addition to his business he took a laudable interest in the collection of Indian relics, and had in his office thousands of flint implements, collected by him and his associates in the vicinity, who had been organized into an archæological society. His office was but a few yards' distant from the gravel pit from which I have said the railroad had been for so many years obtaining ballast. The perpendicular face of this bank of gravel as it was exposed from time to time by the excavations of the railroad men was frequently examined by Mr. Mills, not with special reference to finding implements, for that thought had not entered his mind, but for the sake of obtaining specimens of coral, which occasionally occurred in the gravel. While engaged in one of these rounds on the 27th of October, 1889, he found this specimen projecting from a fresh exposure of the perpendicular bank, 15 feet below the surface, and, according to his custom, recorded the facts at the time in his note-book. There was no lack of discrimination in his observations, or of distinctness in his memory. There is no possibility of any doubt about the undisturbed character of the gravel from which Mr. Mills took the implement with his own hands. The photograph of the bank, to which I refer in my volume, is not, as I say, of the same one from which this implement was taken, but it is so like it that it illustrates the character of the problem just as well. I will, however, speedily prepare an illustration from photographs of the terrace at Newcomerstown.

These facts, submitted at the meeting of the Western Reserve Historical Society referred to, were fully detailed upon the spot to myself and a party of gentlemen, consisting of Judge C. C. Baldwin, E. A. Angell, Esq., Wm. Cushing, Esq., all lawyers of eminence, and Mr. David Baldwin, who accompanied me in a visit to the place on the 11th of April, 1890. We had all the opportunity to question and cross-question that could be desired. Now this is only one case, but it comes in as cumulative evidence with other cases; that of Dr. Metz of Madisonville being almost equally good. I will only make a further passing reference to the evidence at Trenton. Dr. Abbott is not the only competent person who has discovered implements at Trenton in undisturbed gravel. In addition to those mentioned in my communication for Nov. 11, Mr. Lucien Carr has specifically stated in two different meetings of the Boston Society of Natural History (see their Proceedings for Jan. 19, 1881) that he, in company with Professor J. D. Whitney, found several implements at Trenton, one of which was in place "under such circumstances that it must have been deposited at the time the containing bed was laid down."

I submit that this evidence is neither "chaotic" or "unsatisfactory," but is as specific and definite and as worthy to be believed as almost anything any expert in this country, or any country, can be expected to produce. If the public cannot be convinced by such evidence, it is doubtful if any expert will be able to convince them. "If they believe not Moses and the prophets, neither will they believe, though one rise from the dead."

No one will have any objections to Mr. Holmes beginning the investigations anew, but many will object if, when he makes discoveries of relics of man in glacial deposits, he shall claim that they are the first discoveries of the kind which have been made in America. G. FREDERICK WRIGHT.

Oberlin, O., Jan. 27.

Palæolithic Man in North America.

IF the weight of opinion may be considered as having settled any question, the fact that in some part of the world man once existed in so low a stage of culture as to have possessed only implements rudely chipped out of stone may be regarded as established. If this so-called "palæolithic man" existed anywhere else, why may we not suppose that he has lived on this continent also? To hold the contrary is to imply that this part of the world was not peopled until mankind had developed into the neolithic stage of culture. With such an *a priori* probability, therefore, of finding proofs of his existence here as well as elsewhere archæologists have applied themselves to the task of searching for such evidence in this country. But when archæologists make use of the term "implements rudely chipped out of stone," they have in mind certain well-known and perfectly defined objects. They do not mean pebbles showing the marks where certain portions have been casually detached by blows. By the term "palæolithic implement" the instructed archæologist intends certain definite and fixed types of chopping or cutting utensils, which have been found in large quantities, more especially in western Europe, both in gravel beds of ancient quaternary rivers and sealed up in caverns by overlying layers of stalagmite. These chipped implements have a *facies*, or family likeness, that is unmistakable, and they are accompanied by the remains of certain extinct animals, which furnish a guarantee of their great antiquity. They are implements perfect, complete, and finished in themselves, and not merely objects rudely blocked out to a general outline of the shape intended to be given to them by subsequent toil. They are entirely unlike those rude beginnings of implements which were intended to be perfected by being ground down to a polished surface. Such unfinished articles are quite as common as the polished stone axes themselves, both in Europe and in this country, but no competent archæologist would ever confound one with the other. The general appearance of a series of palæolithic implements and of a set of unfinished, chipped, neolithic implements is entirely different. Thus the term "palæolithic implement" has become a perfectly established technical term, and archæologists, understanding well its full meaning, have accordingly sought for examples of it in the river-gravels of North America. They have confidently asserted that they have found such, not in large quantities, it is true, but sufficiently to establish the fact that palæolithic man lived here also, as well as in Europe, Asia, and Africa.

But quite recently there has been put forth by a little knot of men, principally connected with the U. S. Geological Survey, the claim that this conclusion is entirely wrong; that no palæolithic implement has ever been discovered in this country, and that those objects which are claimed to be such are merely "rejects," or imperfect or unfinished articles left behind by the natives who were found in possession of this continent, and who were then living in "the age of polished stone."

"With that half-wisdom half-experience gives" these geologists, whose archæological studies have been limited to our native Indian tribes and their remains, have had the assurance to maintain that the so-called "palæolithics" of this country are nothing more or less than what are sometimes styled "turtle-backs," or those unfinished polished celts, one of whose sides has had less material detached from it than the other. This is the whole question in a nut shell; certain Washington geologists claim to know everything about palæolithic man, and that those who disagree with them are utterly ignorant of the subject. But they have put forward this preposterous claim in the most offensive and contemptuous manner possible, using language in regard to those who differ from them such as no gentleman would employ, and wrapping up their conceited ignorance in a cloud of fustian, which appears to pass for philosophical writing in the atmosphere which surrounds them. That this style of "argument" is confined to a very limited circle would seem to show either that the word of command has been given out from some autocratic source, which they dare not disobey, or that they are actuated by jealousy at the success that has crowned the labors of those who maintain the existence of palæolithic man in North America.

Only a jury of the acknowledged pre-historic archæologists of the world is competent to pronounce judgment upon this question.

HENRY W. HAYNES.

Boston, Mass., Jan. 24.

Criticism of the U. S. Geological Survey.

THE frequent complimentary notices and encomiums upon the U. S. Geological Survey that have appeared in *Science* without any adverse criticisms, might lead one not conversant with the subject to suppose that the Survey reflects the geological learning of this country, or that it is rapidly discovering the resources, or in some other way is giving *quid pro quo* for the money expended.

Looking upon the Survey as a public matter, it is a proper subject of criticism, by any citizen, and among those who have given it any attention, with whom I converse or correspond, not one expresses satisfaction, and generally they have only words of severe condemnation.

The Director has called special attention to it by his article in *Science* of Jan. 13, and stated his claims for the work accomplished. He says:

"When the bureau was instituted, in 1879, it was found at the outset that there were no adequate maps of the regions selected for survey; and it soon became evident that the geologic work could not be carried on without maps showing the relief of the land as well as the hydrography and culture. Accordingly, topographic surveys were instituted in each of the regions selected for examination. At first these surveys were planned to meet immediate needs, and the methods of mapping were not systemized or unified; the scales were diverse and the methods various; the areas were selected by geologic needs and were not fitted to a general scheme for the geologic map of the country, and the resulting maps were discordant in their conventions. At this stage the topographic surveys were executed under the direction of the chiefs of the geologic divisions. After two or three years of trial this form of organization was found unsatisfactory, and the topographic surveys were separated from the geologic work and assigned to a geographic division, which has ever since been maintained."

In short, he says, at the outset, it soon became evident that the geological work could not be carried on without maps made by a topographical survey and accordingly the topographical surveys were instituted, but after two or three years of trial this form of organization was found unsatisfactory, and the topographical surveys were separated from the geological work. I will agree with him that, for the first two or three years, "the methods of mapping were not systemized or unified," and I am willing to believe they were of little or no geological value, and I am willing to agree that after two or three years of experience and study he ascertained that a topographical survey belongs to geographical work; but there are two matters arising from his statement that are not exactly clear, viz.:

1. If it was evident, at the outset, that geological work could not be carried on without a topographical survey, why was it necessary, within two or three years, to separate the topographical surveys from the geological work?

2. Was there, at the outset, any intelligent geologist or geographer, in the United States, not connected with the U. S. Survey, who did not know that topographical surveys belong to geographical work?

We do not desire any play on words and, therefore, come at once to the question, What geological work has been done by the Survey that is of any general benefit to the science, or that is of any economical value, or that is of any general application to the stratified rocks of the continent? For my part, having examined nine of the Annual Reports, and observed nothing of general scientific value or utility (excluding a few definitions of fossils), I would answer this question negatively. And if there is work that might possess some geological value as a preliminary reconnoissance, such work is more than destroyed by inexcusable provisional names for the groups, without characterizing them or stating the fossils by which alone their places in the geological column are to be determined. (I do not use the word "group" in the sense in which it is used, generally, in the survey, but I use it in its established geological sense.)

A lawyer in any State can go into any court in any other State or into any of the courts of the United States or into those of Canada or England and hear and understand the technical words of the science. No word will be used by any judge or attorney with which he is not familiar and it will be used in the exact legal sense in which he learned it and used it at home. More law books have been published than belong to all the sciences of natural history combined, but no one in centuries has proposed a substitute or provisional word for any technical one in use, though it cannot be denied that more expressive or euphonious words might, in some instances, be proposed. Blackstone made his fame by abstracting the technical definitions from the opinions of the courts, as written in the books, with full references and citations to his authorities, and it is for that reason alone that the use of his commentaries can be justified in any law school in this country. The whole value of precedents and court reports is in the fixity of the technical words used and their established definitions. What the science of geology demands is fixity in the names of the subdivisions of the stratified rocks, and the accurate determination of the fossils that characterize each subdivision, for by the fossils alone can the subdivisions be determined. And these demands have been wholly disregarded and set aside by the U. S. Survey since 1879, and we have synonym after synonym for equivalent rocks, vague and worthless definitions, and what seems to me the culmination of absurdity if not crime against the progress of geological knowledge, the pretension that they are developing a "New Geology."

This matter of nomenclature alone, in my opinion, will everlastingly condemn the Survey, so far as it deals with stratigraphical geology, and make students of the science wish there had been some power to suppress the publication even if it was necessary to expend the appropriations. It would have been better to have given the money to the printer and consigned the stratigraphical manuscript to the flames.

But, aside from the questions of nomenclature, that are so intimately connected with learning, and so vital to the understanding of any subject, there are numerous fundamental errors. If any one will turn to page 372 of the Seventh Annual Report, under the head of "Paleontological Characters as a Basis for Classification," he may read pages in consecutive connection where every idea expressed will be recognized as absolutely erroneous by any competent paleontologist. I will quote only a single sentence. He says:

"We have now constantly to remember that paleontology is based wholly upon stratigraphy, and consequently that the conclusions that we would draw from our fossils must constantly be checked by stratigraphical observations."

This statement is made, in the face of the fact, that no species in the great Subkingdom Echinodermata is known to have a vertical range of 500 feet, in the palæozoic rocks of North America; that not one is known to cross the line subdividing the groups of rocks recognized in the Geological Surveys of New York, Pennsylvania, Illinois, Indiana, or Canada; and in the face of the fact, that science has not recognized a group of rocks within the past twenty years, in America or elsewhere, except the subdivision was based on the fossil contents.

There is not space in a scientific journal to review the ponderous volumes of the Survey, but I do not discover any attempt to make a geological survey of the United States or of the Territories; but instead thereof, the volumes contain theoretical discussions about the glacial period, that have no economical value, and which period, I think, is fiction, and they contain a vast amount of extremely localized and temporary matter of no general utility. This is well illustrated in the Seventh Report now before me. One of the principle articles is entitled "The Geology of the Head of Chesapeake Bay." It covers more than one hundred pages, has sixteen plates and six additional illustrations. The author says:

"The investigation here recorded was made under the joint auspices of the U. S. Geological Survey and the U. S. Fish Commission, for the purpose of determining the probable success of an artesian boring at Fishing Battery station, off Spesutie Island, five miles south of Havre de Grace, Md., and near the head of Chesapeake Bay. The field-study occupied a portion of July, 1886."

The article is so free from geology and so extremely localized that I have been unable to discover the object in publishing it in the U. S. Geological Survey. The author, however, says, on page 564, under the head of "The Geologic Exposures":

"So variable are the different formations of the region in the several exposures that the differences exceed the resemblances, and, since the local diversities are due to local causes the characteristics of the formations cannot be elucidated by generalized description with sufficient minuteness for the purposes of the local student."

Another one of the principal articles, hugely illustrated, in the Seventh Report is entitled "Report on the Geology of Martha's Vineyard." I have lookod through it, in vain, to find an item of geological information. It would certainly take the cake in any walk where pretension and nothingness were to be the winners.

In conclusion, I am opposed to the continuation of the U. S. Geological Survey, under the present management, because, I think, it is not prosecuted in the interest of science but quite the contrary, and because the publications now hang, like a millstone, around the neck of progress, in the dissemination of geological information among the people. S. A. MILLER.

Cincinnati, Ohio, Jan. 23.

Monument to Hirn.

IN a letter, just received from Mon. G. Kern, President of the Commission established for the purpose of securing the erection of a monument to his late distinguished friend and colleague, Mon. G. A. Hirn, the great engineer-physicist and investigator, on account of which subscriptions have been received in considerable amounts, both in Europe and America, he writes as follows:—

"The monument proposed for Hirn, and of which the plans were made by Mon. Bartholdi, will consist of a bronze figure, seated, with pedestal, and will cost about 30,000 M. To complete the subscription, there still remains a balance of 10,000 M., and I have knocked at the doors of many friends and acquaintances of Hirn, finding welcome, in Paris and in Bordeaux; I anticipate full success."

It has been the hope of the gentlemen engaged in this enterprise that a fair proportion of the subscription might come from citizens of the United States of North America, among whom Mon. Hirn counted some personal friends, and many warm admirers. He was always peculiarly appreciative of such good will and such praise of his work as came to him from this side of the Atlantic. Those who desire the privilege of contributing may send their drafts on Paris to the "Comité-Hirn," I Obstmarkt, Marché aux Fruits, I, Colmar, Alsace.

Very respectfully yours,
R. H. THURSTON.

Ithica, N. Y., Jan. 25, 1893.

CALENDAR OF SOCIETIES.

Society of Natural History, Boston.

Feb. 1.—H. L. Harris, A New Instance of the Capture of Streams; W. T. Sedgwick, The Natural History of Disease.

Publications Received at Editor's Office.

ANDERSON, W. Mineral Springs and Health Resorts of California. San Francisco, The Bancroft Co. 384 p. 8°.
BEECHER, H. W. Bible Studies. Edited by J. R. Howard. New York, Fords, Howard & Hurlbert. 438 p. 12°. $1.50.
DANA, EDWARD SALISBURY. Catalogue of American Localities of Minerals. New York, Wiley. 51 p. 8°. $1.
DE MOTTE, J. B The Secret of Character Building. Chicago, S. C. Griggs & Co. 130 p. 12°. $1.
DREYSPRING, A. French Reader on the Cumulative Method. New York, Amer. Book Co. 171 p. 12°. 75 cents.
HOLMAN, SILAS W. Discussion of the Precision of Measurements. New York, Wiley. 176 p. 8°. $2.
HOREY, HENRY T. Theory of Structures and Strength of Materials. New York, Wiley. 817 p. 8°. $7.50.
HUTCHINSON, H. N. Extinct Monsters. New York, Appleton. 254 p, 8°.
PARSHALL, N. C. Proofs of Evolution. 5th 1000. Chicago, Chas. H. Kerr & Co. 70. p. 12°.
PEET, S. D. The Mound Builders: Their Works and Relics. Chicago, The American Antiquarian. 376 p. 8°.
SHALER, N. S. The Interpretation of Nature. Boston, Houghton, Mifflin & Co. 305 p. 12°. $1.25.
STYX. Hermetic Philosophy. Vol. III. Can Virtue and Science be taught? Philadelphia, Lippincott. 221 p. 12°. $1.25.
SYKES, JOHN F. J. Public Health Problems. New York, Scribner. 370 p. 12°. $1.25.
THE SONG BUDGET, The Song Century, The Song Patriot. Syracuse, C. W. Bardeen. 12°. 50 cents.
WEYL, THEO. The Coal-Tar Colors. A Sanitary and Medico-Legal Investigation. Preface by Professor Sell. Tr. by H. Leffmann. Philadelphia, Blakiston. 154 p. 8°. $1.50.
WHITBY, BEATRICE. In the Suntime of Her Youth. New York, Appleton. 365 p. 12°. 50 cents. Paper.

"Breathing Wells" in Missouri.

I WOULD like to refer those interested in the subject recently presented in your columns (Dec. 16 and Jan. 13) to notes on such wells in Nebraska, published in the *American Naturalist*, April and May, 1883. The conclusions there presented harmonize with those demonstrated by Mr. Willard and Professor Sweezy.

The tendency of such wells to freeze was first brought to my knowledge in connection with some that occur near Mt. Leonard and Marshall, in Saline County, Missouri. They are frequent in what I take to be an ancient channel of the Missouri River, which has become filled, largely with sand. I was assured by several persons directly acquainted with the facts, one of them experienced in putting in and repairing pumps, that in such "blowing wells" pumps not infrequently froze to the depth of 70 or 80 feet below the surface, and in one case ice had been found in a pump cylinder 100 feet down, which was about 10 feet above the water. In all these cases the reservoir of confined air is in an extensive deposit of dry sand connecting with the outer air through the mouth of the well. J. E. TODD.

Vermillion, S.D., Jan. 24.

BOOK-REVIEWS.

People of Finland in Archaic Times. Compiled by J. C. BROWN, LL.D. London, Kegan Paul, Trench, Trubner, & Co. 290 p. 8°.

BR. BROWN has in view the compilation of a series of volumes on the ethnography of northern Europe, prepared for popular study, of which this is the first. It is principally composed of extracts and abstracts from the Finnish national epic, the Kalevala, and some additional material is obtained from other sources. The whole forms an excellent popular introduction to the study of this ancient and remarkable poem, and enables the reader to understand the cultural condition of the people among whom it originated.

Dr. Brown is inclined to assign the date of the construction of the poem in its present form to about 1100 A.D. Doubtless, however, a great part of it is many centuries older, and the myths and legends which it embodies are referable to an ancient, prehistoric period, before the separation of the western Ugrian stock into its various existing branches.

AMONG THE PUBLISHERS.

"HIGHWAYS and Byways of Europe" is the title of a volume translated from the French of M. Jules Michelet by Mrs. Mary J. Serrano and published by the Cassell Publishing Company.

—Miss Kate Marsden's book, "On Sledge and Horseback to the Outcast Siberian Lepers," the Cassell Publishing Company will soon have ready. Ever since Miss Marsden began her work among the outcast lepers of Siberia the world has been ready to read her own story of her adventures and experiences.

—"The Principles of Rank among Animals," by Professor Henry Webster Parker, is the title of a paper read before the Victoria Institute, London, Dec. 5, 1892. It is a condensed digest, under eighteen heads of remark, of the recognized principles that determine grade, and with incidental reference under each to man's zoölogical position, but without touching the question of his origin. A distinction is emphasized by the author between anatomical and zoölogical position, as illustrated, for example, in the three sub-classes of birds, which are based far more upon mode of life than upon any morphological differences; also by the rank given to singing-birds as justified by the function far more than the anatomy of the syrinx. The ideal is still recognized in zoölogy, as in bird, fish, and insect; the ideal of man is the antithesis of that of anthropoids; and by nearly every principle of zoölogical rank he is shown to have a place quite apart, and in same respects less near to anthropoids than to animals lower in grade.

Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

For sale or exchange.—I have a few copies of my translation of "Strasburger's Manual of Vegetable Histology, 1887," now out of print, which I will send post-paid for $3 or for one dozen good slides illustrating plant or animal structure. Address A. B. Hervey, St. Lawrence University, Canton, N. Y.

The undersigned has the following specimens to exchange for crystals of any eastern or foreign localities or Indian relics: tin ore, metacinnabarite, stibnite, garnierite, calenanite, hanksite, ulexite, rubellite, lepidolite, blue and green onyx, Cal. pineite, aragonite on chalcedony, cinnabar, double refracting spar, clear and clouded, and others. J. R. Bush, care of General Delivery, Los Angeles, Cal.

For sale or exchange.—A private cabinet of about 200 species of fossils, well distributed geologically and geographically. Silurian, about 40; Devonian, about 50; Carboniferous, about 80; others, about 30. Frank S. Aby, State University, Iowa City, Ia.

For exchange.—Minerals, fossils, F. W. shells, land shells, native woods, Indian relics, two vols. of Smithsonian reports, odd numbers of scientific magazines, copper cents, etc., for good minerals not in my collection, good arrow- and spear-heads and natural history specimens of all kinds. Correspondence solicited with list of duplicates. G. E. Wells, Manhattan, Kan.

For sale or suitable exchange.—A spectrometer made by Fauth & Co., Washington, D. C., according to the plan of Prof. C. A. Young. This instrument is suitable for the most advanced investigations and determinations. Cost originally $700 and has been used but little. Will be disposed of at a considerable reduction. Address Department of Physics, Ohio University, Athens, O.

I will send British land and fresh-water shells in return for those of America, any part, sent to me. I have at present about fifty or sixty species, with many varieties. W. A. Gain, Tuxford, Newark, England.

The Biological Department of Hamline University desires to offer microscopic slides of animal tissues, or whole animals, in exchange for first-class fossils. Address correspondence to Henry L. Osborne, Hamline University, Hamline, Minn.

Wants.

WANTED.—American Journal of Conchology, seven volumes. Parties having these for sale will please address the undersigned, stating condition and price. R. Ellsworth Call, Louisville, Ky.

A GRADUATE ENGINEER will give instruction evenings in geometry, trigonometry and surveying, mechanics, physics, mechanical drawing and general engineering construction. Five years' experience in field and editorial work on engineering journal. References furnished. C. S. H., 102 Tribune Building, New York.

WANTED.—By well-qualified and experienced science master and associate of the Royal School of Mines, London, aged 26 (at present in England), a mastership in technical college or university for any of the following subjects: Engineering sciences, geology and mineralogy, physics, chemistry and metallurgy, etc., etc. Can provide excellent references and credentials. Apply, J. G., 17 Sussex St., Rochdale, England.

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N. D. C. HODGES,

874 Broadway, New York.

SCIENCE

NEW YORK, FEBRUARY 10, 1893.

THE ICE-WALL ON THE BEACH AT HULL, MASSACHUSETTS, JANUARY, 1893.

BY J. B. WOODWORTH, SOMERVILLE, MASS.

THE exceptionally long-continued cold of the early part of January, this year, favored the development of a considerable wall of coast-ice on the long barrier beach connecting the rocky headland of Nantasket with Strawberry Hill and the neighboring drumlin at Point Allerton at the entrance to Boston Harbor. At the same time, the embayed waters of Boston Harbor froze over. I visited the beach at Hull on the 24th of the month, at a time the temperature had risen above the freezing point, and when the sheet-ice had left the shore and was only visible as cakes floating near the horizon.

At Nantasket, from the vicinity of the cafes northward to near Point Allerton, the ice-wall formed a rampart near high-tide mark of triangular cross-section, having an average elevation of about 8 feet and a breadth of base of 20 feet. The seaward slope of this wall was shorter and steeper than the landward, and was also much more irregular, owing to the action of the waves and some melting. The landward slope merged into the sheet of snow back of the beach. The accompanying diagram will make clearer this description.

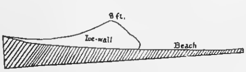

FIG. 1. — General cross-section of the ice-wall at Hull.

The wall was composed in part of cakes, but in larger measure of granular ice, making the whole a compact mass, whose front was broken at frequent intervals by recesses swept by the waves at high tide. The beginnings of these recesses were seen in numerous caverns at the bottom of the ice, some of which were large enough to permit a man to crawl under the arch, and in one case a breach had been made through to the beach in the rear of the wall. In another instance, where the crest of the wall was low, the arch was fissured, as shown in Fig. 2, apparently by the pressure of waves in passing through the tunnel.

FIG. 2. — Ice-arch fissured by wave-action.

From many of the small caves little streams were trickling out over the sand beach in front. These streams were busily employed at low tide in building and re-arranging small deltas of fine sand, a long stretch of which lay between high and low tide-marks.

The drainage of water produced by the superficial melting of the ice at mid-day was mainly in the form of drops from the protuberant masses one or two feet above the base, which was slightly receding, a feature determined by the water at high tide. These drops of water fell upon the wet sand of the beach and made well-marked pits, the cross-sections of some of which are shown in the adjoined Fig. 3. These pits were distributed along the front of the ice-wall just under the high-tide limit.

Some of these depressions resemble the so-called rain-drop imprints on the older strata, and serve to make us cautious in the interpretation of such markings. I have also seen the spray from surf, as on the beach at Gay Head, Mass., make similar impressions. The larger impressions at Hull were as much as three inches in diameter, but correspondingly shallow, while those which were in process of formation were not over half an inch across and half an inch deep. Around each pit, into which water was dropping, a rim of sand was raised. The larger pits, just described, were, except for what I am about to describe, without any signs of the cause of their formation. In several instances, however, I observed that water was dropping in a narrow, deep pit, formed exactly in the centre of one of the large shallow ones.

FIG. 3. — Pits made on the beach by water dripping from coast-ice. *a.* Deep and narrow pit. *b.* Broad, shallow pit. *c.* Renovated pit.

The explanation of these pits seems to be this, that, after the dropping has ceased for a time, as by the freezing of the surface of the ice-wall at night, the sands about the deep pits cave in, being highly mobile by reason of the water they contain. If now, on the next period of melting, drops of water drip from the same icicle-like projection of the ice-wall, a new, deep, but narrow pit will form in the place held by the old one. The geological interest of these pits is evident when we compare them with some of the pit-like depressions found in the Cambrian and other deposits of beach origin. The surface of the arenaceous slates of presumably Lower Cambrian age in Somerville, Mass., are marked with pits closely resembling many of these made by water falling from coast-ice. In fact, it would be difficult to distinguish them from the so-called genus of worm-burrows, Monocraterion, where the long tube penetrating the sand is obscure or wanting.

The strength of the waves applied to the face of this wall of ice can be estimated from the fact that a whale, about 40 feet long (*Physeter macrocephalus*), had been washed ashore abreast of Strawberry Hill, and lay with his head to the north, close up to the foot of the wall of ice, apparently in a position determined by the run of the shore-current during a "north-easter." The depth of water necessary to float this body in was in part obtained through the backing-up of the waves against the wall of ice. The effect of this action on the regimen of the beach was better shown on the bouldery pavement near Point Allerton.

Under the ordinary summer conditions of this beach, the swash of the surf advances up it as a thin sheet of foaming water, halts for an instant, and then recedes, to be overtaken by another wave. The ice-wall, however, at high-tide mark, or just below it, interferes with the action of the swash. The result is that the water is held up against the ice-wall, and when it recedes goes out as a deeper sheet than when the wave has a chance to run up the beach and spread out as a thin layer of water. This thin sheet of water cannot move the larger boulders except by removing the finer materials from their bases, but the thick sheet in front of the ice-wall acts more potently on the larger cobbles and boulders, dragging them up and down the beach, so that its aspect is for the time quite altered. To this action must be added the effect of cakes of ice, with inter-stratified layers of sand and gravel and occasionally included cobbles, which are left pell-mell on the beach with the receding tide.

The larger beach pebbles, which have been reduced to the form of wear characteristic of their class, exhibit an interesting fact which should be noticed here. During the season of minimum wave-action, the pebbles are smoothed by attrition with the finer gravels and sands, which are alone in movement; but in the winter, during heavy storms, the pebbles and cobbles are dashed together, and their smooth surfaces scarred with dents. In the case of an elongate cylindrical pebble, it was very apparent from the grayish pulverulent appearance of the extremities that the wear was greater on the ends than on the sides, though it should be remarked that this pebble was probably thrown sideways quite as frequently, if not more frequently, than endwise against its neighbors.

THE GENERIC EVOLUTION OF THE PALÆOZOIC BRACHIOPODA.

BY AGNES CRANE, BRIGHTON, ENGLAND.

IT is a time-honored saying that "a prophet is not without honor save in his own country," but the name and fame of Professor James Hall, LL.D., director of the State Museum of Natural History of New York, and its veteran State geologist, are well known in Canada and the United States and have long been recognized and appreciated among the geologists and invertebrate palæontologists of Europe. The highest recognition in geological circles was accorded him nearly a quarter of a century ago, when he was awarded the Wollaston Medal of the Geological Society of London, the year after Barrande, and a year before Charles Darwin received it. His arduous life-long researches have resulted in the production of the fine series of monographs of "The Palæontology of New York," of which Vol. VIII., Part I., Brachiopoda,[1] by James Hall, assisted by John M. Clarke, has recently made its appearance, with an unusually interesting text and the well-executed plates for which the series has been remarkable. As a fossil brachiopodist Professor Hall ranks with his eminent contemporaries, the late Dr. Thomas Davidson, F.R.S., and Joachim Barrande of Prague. In one respect he may be said to take higher position as a philosophical investigator, inasmuch that he kept free from prejudice with regard to the theory of evolution as applied to the class Brachiopoda at a time when, owing to the condition of our knowledge of the group, it was not possible to adduce actual proofs of the logical postulate in that direction.

Times and methods have changed indeed since the celebrated Bohemian palæontologist definitely proclaimed that the evidence of the Cephalopoda[2] and of the Brachiopoda[3] was opposed to the truth of the theory of evolution, and Dr. Davidson, in answer to a personal appeal from Darwin, replied that he was unable to detect direct evidence of the passage of one genus into another.[4]

There has been a marked advance in the philosophical treatment of this important group of ancient and persistent organisms during the last decade, and to this progress American scientists have contributed largely. Mr. W. H. Dall has differentiated and described some new genera and species of the recent forms of interest and value. Professors Morse, Brooks, and Beyer, and of late Dr. Beecher and Mr. Clarke, have revealed suggestive phases in the developmental history of typical genera and well-known species. Now Professor James Hall and Mr. J. M. Clarke have sifted and compared the vast accumulations of data recorded by earlier writers by the older methods of descriptive palæontology, and, combining the results thus gained with the best features of the new school of investigators, have effected a revolution in the general treatment of the entire class of Brachiopoda. They trace important stages in the phylogeny of the fossil forms and various links connecting them through their immediate successors with the surviving members of the group.

Much of this work could not possibly have been accomplished had it not been for the mass of descriptions and figures of the vast number of species recorded in the works of Barrande, Davidson, De Koninck, D'Orbigny, Defrance, Deslongchamps, Suess, Lindstrom, Pander, Quenstedt, Geinitz, Littell, Oppel, Oehlert, Waagen, and Neumayr, in Europe, and Billings, Hall, Clarke, Meek, Shumard, Worthen, Walcott, White, Whitfield, and others on the continent of America.

[1] Natural History of New York. Palæontology, vol. viii. (Geological Survey of the State of New York), "An Introduction to the Study of the Genera of Palæozoic Brachiopoda." Part. I. By James Hall, State Geologist and Palæontologist, assisted by John M. Clarke. Albany, 1892.

[2] Cephalopodes, Etudes Générales par Joachim Barrande, Prague, 1877, p. 224.

[3] Brachiopodes, Etudes Locales, *Ibid*, 1879, p. 206.

[4] "What is a Brachiopod?" by Thomas Davidson, F.R.S., Geological Magazine, Decade II., vol. iv., 1877.

The warm and discriminating recognition of the valued labors of his European fellow-workers is one of the most agreeable features of Professor Hall's new volume. It is pleasant to read "of the greatest of all works on the Brachiopoda by Thomas Davidson," of the just appreciation of Barrande's herculean efforts in the Silurian field, of the excellence of William King's anatomical investigations, to find Pander's early work valued and his names restored. These are just and generous tributes to the memory of comrades who have gone before most welcome in these latter days of that strident "individualism" which is often mere egotism in disguise.

The New York palæontologist's recent work is not only a critical *résumé* with descriptions and figures of the Brachiopoda of New York, but a careful analysis of the results of the labors of his predecessors and contemporaries in the same extended palæozoic field of research in the United States, Canada, Russia, Sweden, and Great Britain. This gives it a cosmopolitan value, and affords opportunity, by means of critical comparisons of genera, species, and varieties from the geological horizons of both hemispheres, to recognize the identity of species, to define synonyms, to collate genera and sub-genera, to indicate their inter-relationships, and to illustrate the passage-forms linking one group, or assemblage of allied genera, to another. To this branch of the subject we must now restrict our observations.

With singular modesty the authors refrain, for the present, from proposing any new scheme of classification. The primary division of the class into two orders comprising the non-articulated and articulated genera is adopted. We fail to see why Owen's names of *Lyopomata*, or "loose valves," and *Arthropomata*, or "jointed valves," should have been discarded, for they define the same limits and distinctions as Huxley's simpler, but later, names, *Articulata* and *Inarticulata*, the first of which was employed by Deshayes to designate certain forms of Brachiopoda before the publicatian of Huxley's "Introduction to the Classification of Animals." In England it is generally conceded that the priority and scope of Owen's orders were clearly established by the American systematist, Dr. Theodore Gill. The matter, however, is of less moment now that a general tendency to admit greater ordinal sub-division has arisen. Waagen has proposed six orders, Neumayr eight, and Beecher four, based on the peduncular opening and associated characters.

The names *Inarticulata* and *Articulata* express certain general distinctions. Nevertheless, it is a matter of fact that forms have often appeared which cannot be separated thus, for tendencies to transgress these artificial limits become apparent in various directions. For instance, the species of the Silurian genus *Trimerella* was shown by Davidson and King to be but feebly articulated, and now *Neobolus*, *Spondylobolus*, and Hall's new linguloid genus, *Barroisella*, are shown to exhibit the same propensity. We are glad to note that, although fifteen years have elapsed since the publication of the Memoir on the *Trimerellidæ*, by Thomas Davidson and William King,[5] it is frankly admitted that later observations have hitherto added comparatively little to the results achieved by those eminent investigators and have taken away nothing from their value.

In the present publication the semi-artificial, but convenient, family designations are not adopted, but the genera discussed fall into groups of associated genera, often exhibiting intermediate characters, which link one genus naturally with another. More has been accomplished in this direction than could possibly have been anticipated, and the eighth volume of the Geological Survey of the State of New York (Palæontology) would have made glad the heart of Darwin, for its dominant note is the evolution of genera.

Hitherto *Lingula* has always been regarded as taxonomically at the base of the Brachiopoda in spite of the acknowledged complexity of its muscular system and the date of its appearance in the geological series. It is now shown conclusively to be developed from an obolelloid type which culminated in a faunal epoch anterior to the appearance of *Lingula*, and Brook's history of the development of the living species is cited as confirmatory proof

[5] Quarterly Journal of the Geological Society of London, vol. xxx., p. 124, 1874.

of the direct obolelloid derivation of the palæozoic *Lingulæ* from *Obolella*. *Lingulella* and *Lingulepis*, forerunners of *Lingula*, may be found to be important connecting links, having the outward form of linguloids with the muscular arrangements and narrow pedicle slit of the obolelloids. "The development on the linguloid line has continued, as we believe, from early Silurian to the present time with frequent modifications. From *Lingula* we may depart in many directions. In *Lingulops* and *Lingulasma* we get indications of physiological influences on the origin of genera."

It appears that "augmented muscular energy and concomitant increased secretion of muscular fulcra" with the large size and consequent displacement of the liver induced the thickening of the entire area of muscular implantation. Gradual excavation of this solid plate ensued, and the formation of a more or less vaulted platform, extremely developed, in the feebly articulated Trimerellids of those Silurian seas, which favored the rapid development of the platform-bearing Brachiopoda, a race which was abruptly exterminated at the close of the Niagara and Wenlock period. Hall's new genus, *Barroisella*, is a divergent so marked by the development of deltidial callosities as to indicate their approximating specialization for articulating and interlocking purposes. Thus we get most striking evidence of a tendency to span the interval between the so-called edentulous *Inarticulata* and the articulated genera in the Linguloid and Trimerelloid groups.

The genus *Obolus* is shown to be more specialized than *Obolella*, less so than *Lingula*, *Neobolus* being an intermediate form with cardinal processes, also indicative of progress in this direction towards the *Articulata*. In *Obolus*, however, the muscular scars are excavated as in *Lingula*, not elevated as in the forms tending to *Trimerella*. Thus we get indications in the history of the ancestral Trimerellids of the attainment of a like remarkable resultant along distinct lines of development, of which another instance has been furnished by Messrs. Fischer and Oehlert's recent studies of the development of the living *Magellanæ* of the boreal and austral oceans, to which we had elsewhere occasion to refer.[1] As Hall and Clarke's generalizations are formulated with a due regard to geological sequence, they possess more validity than the phylogenetic deductions enunciated by a Teutonic palæontologist, in which that important factor was somewhat neglected.[2] "We have yet to seek," the American brachiopodists conclude, "the source whence these numerous closely allied primoidial groups are derived, in some earlier comprehensive stock of which we have yet no knowledge. The ages preceding the Silurian afforded abundant time for a tendency to variability to express itself" (p. 168).

From this satisfactory discussion of the origin and development of the palæozoic unarticulated genera and species, Hall and Clarke proceed to consider the structure and relations of the far more numerous and more complicated order of the articulated species, and commence with the Orthoids, the lowest forms of the Articulata, as, by common consent, they are now regarded. The allied Strophomenoid, Streptorhynchoid, and Leptaenoids, as defined by Dalman, are then treated of and the first part terminates with a discussion of some carboniferous Productoids. The spire-bearers, Rhynchonelloids and Terebratuloids, of the Palæozoic seas are thus left for the concluding volume, when we may look for a valuable general summary of results and for that systematic classification, based on their completed investigations, which the authors are bound to propose in the interest of students for the root, stem, branches, and twigs of the genealogical tree of the Brachiopoda, as they have definitely abandoned the family names hitherto in vogue. It must certainly be admitted that brachiopodists have often found it difficult, and sometimes impossible, to determine to which of two well characterized families certain annectent forms should be definitely referred.

In Europe, however, the retention of family designations is not always considered incompatible with the modern philosophical and evolutional methods of class treatment. They have been preserved with advantage; for instance, in Mr. A. Smith Woodward's[3] masterly systematic classification of the fossil fishes in the British Museum, and also in Professor W. A. Herdman's[4] exhaustive report on the Tunicata dredged by the "Challenger" expedition, associated in this case with evolutional data and the presentation of numerous phylums showing the inter-relations of genera, somewhat after the same plan as that adopted in the "Introduction to the Study of the Palæozoic Genera of Brachiopoda." With all due respect to the veteran of the old school and the disciple of the new, we venture to submit the impossibility of impressing on the mental retina a permanent photograph of the innumerable and fascinating phylums which they have provided with such industrious research. But we are not all endowed with so much insight, knowledge, and experience.

The most revolutionary feature in the present instalment of their researches on the *Articulata* is the extreme sub-division to which the great group of Orthoids has been subjected. The genus *Orthis* is absolutely restricted to eight species (instead of two hundred), with *O. callactis* of Dalman as the type, and his early figures and original descriptions are judiciously reproduced for the benefit of American students. The remainder of the large number of species are placed under various new genera and sub-genera, or restored to their former appellations. For instance, Pander's name, *Clitambonites*, is once more applied to species unjustly usurped by D'Orbigny's *Orthisina*, and *Plectambonites* of the same Russian palæontologist is restored for the Palæozoic species grouped by the French conchologists and those who followed them under the genus *Leptaena* of authors not of Dalman. The *Leptaena rugosa* of this author is taken as the type of his genus, the scope of which is thus much restricted, and new generic names are proposed for several of the species indifferently described as *Strophomenas* or *Leptaenas* by various authors. Linné's sub-genus *Bilobites* is revived for those abnormal bilobed species of *Orthis*, which, according to Dr. Beecher's investigations, originated from a normal form at the adolescent and mature stages of growth in both direct and indirect lines of development. In view of the extensive breaking-up of the Orthoids, here proposed, into several genera and sub-genera, we are willing to confess that to object to the revival of *Bilobites* would be but straining at a gnat and swallowing the camel. We, however, admit a preference for those among the proposed new or restored designations which give some indications of the former position of the species among genera. Such are *Protorthis*, *Plectorthis*, *Heterorthis*, *Orthostrophia*, *Platystrophia*, and so on. *Orthidium* for the generic divergent nearest allied to *Strophomena* seems a less happy selection. Tabular views, both instructive and suggestive, are given to show the approximate range in the geological horizons from the calciferous shales of the Lower Silurian to the Upper Coal Measures which indicate the appearance, persistence, and extinction of the various genera into which, under new, old, or restored appellations, the Orthoids, Strophomenoids, and Leptaenoids are sub-divided — a sub-division which, with its associated shifting of types, will not escape criticism.

There will always be differences of opinion respecting generic values. Here, as Heckel long ago pointed out, the personal equation becomes prominent. We believe Professor Cope was the first to advance the then heterodox view that species could be transferred from one genus to another without affecting their specific characters. Many so termed genera represent what have now become abbreviated transitional phases in the development of the race which, of old time, became stereotyped for periods of longer or shorter geological duration. The researches of Friele and Oehlert on the recent Magellanæ (*Waldhumia*) the ultimate phase of development of the long-looped branch of the Terebratuloids, illustrate this point most clearly. If the inter-relationships and passages of these generic phases are carefully noted, they become so many illustrations of one method of the evolution of genera, which, sometimes, it is evident, originated from causes incidental to individual development, accelerated growth, and the circumstances of the environment.

[1] On the Distribution and Generic Evolution of Some Recent Brachiopoda, By Agnes Crane, Natural Science, January, 1893.

[2] Neumayr, "Die Stämme des Thierreichs Brachiopoda," 1890.

[3] A Catalogue of the Fossil Fishes in the British Museum, Part I., 1889; Part II., 1890.

[4] Reports of the "Challenger" Expedition: Tunicata, vols. vi., xiv., and xxvi.

Professor Hall evidently considers it better to deal with a small number of well-characterized species instead of a large number of ill-defined forms, and that such minor structural internal modifications as can be shown to be constant in a recognized geological horizon should be raised to generic or sub-generic rank. The description and portrayal of such generic divergencies afford the best means for general comparison and thus tend to promote a clearer comprehension of the manifold phases of the evolution of genera. The fact that specific characters sometimes make their appearance in individual development before generic features is most suggestive. For the laws of "science and growth,"[1] first made known by Heckel, and since extended by Hyatt to the *Cephalopoda*, Jackson to the *Pelycypoda*, and Beecher and Clarke to the Brachiopoda, the term auxology[2] has been lately proposed by English systematists, with some elucidative and etymological modifications in Hyatt's terminology. These principles govern individual and specific development of genera. For genera are stages in the life history of the race as distinguished from the genealogical records of the individual. It would seem, however, that just as the co-existence of a large number of individuals tends to perpetuate specific variation, so the simultaneous occurrence of abundance of species in one horizon and area is productive of the divergence of genera.

We cannot enter further into details; enough has been written to show beyond contradiction the value and interest of his "Introduction to the Study of the Genera of Palæozoic Brachiopoda," with its concise descriptions of genera and passage-forms, their inter-relations, and affiliated species. It is rendered complete by excellent specific bibliographies, well considered genealogical trees, showing the common ancestry, diverging lines of descent, and affinities of genera with their geological range, a register of genera and of species, authors' and general index. The work is most creditable to Professor James Hall and his assistant, Mr. J. M. Clarke, and reflects honor on America in general and the State of New York in particular. It deserves to be carefully studied by invertebrate biologists in both hemispheres. We trust the publication of the second part will be proceeded with, and that by its rapid completion, on similar lines of thought, science may be enriched by a general view of the evolution of the Brachiopoda. It is much to be desired that the relations of the secondary and tertiary species should be discussed in a like thorough, philosophical, and generally satisfactory manner.

We have become so convinced of the advantages of this method of treatment, that we have begun to form the nucleus of a collection in the Brighton Museum, destined to illustrate the evolution of genera among the Brachiopoda.

ON THE SO-CALLED INCAS EYES.

BY W. S. MILLER, UNIVERSITY OF WISCONSIN, MADISON, WIS.

AT the time of the earthquake and accompanying tidal-wave which swept over Arica, Peru, August 13, 1868, causing so much destruction of life, property, and shipping, the U. S. man-of-war "Kearsarge" was lying some two hundred miles down the coast. The shock there was comparatively slight to what it was at Arica. Word was received the following morning of the disaster up the coast, and the vessel left immediately to render such assistance as lay in its power. The history of that earthquake is well known. I will refer any who may wish to read an account of the occurrence to an article in *Harper's Monthly* for April, 1869. The late Lieutenant Gardner, U. S, N., was at that time stationed on the "Kearsarge," and it is to him that I am indebted for the material which forms the subject of this article.

After the officers of the "Kearsarge" had rendered what assistance they could towards alleviating the distress caused by the earthquake, they turned their attention to the havoc wrought by the shock and tidal-wave. Prominent amidst the debris, and about a quarter of a mile from the shore, they found a number of so-called "mummies," which had been exposed by the receding tidal-wave. These Peruvian mummies are not mummies in the same sense that we speak of those of Egypt. The Egyptian mummies were preserved artificially from putrefaction by being embalmed, an art peculiar to the people of that country; but the Peruvian bodies are simply desiccated, the conditions of the atmosphere and soil being conducive to their preservation.

The mummies are usually found in vaults or chambers of adobe, roofed with sticks or canes and a layer of rushes; these usually contain several bodies, which are placed in a sitting posture, the chin resting on the knees, the hands being clasped around the knees. Sometimes the face rested on the hands, with the elbows crowded down between the thighs and abdomen. The bodies are wrapped in native cloths and bound with cords. A small thin piece of copper was usually placed in the mouth; this corresponded to the *óbolos* which the ancient Greeks put into the mouths of their dead as a fee for Charon. They were accustomed to bury with them such utensils as they were supposed to need in the country to which they journeyed. The farmer had seeds of various kinds and agricultural implements placed about him; the fisherman had his net wrapped about him, and nearby fish-hooks were placed with barbs wonderfully like those in use at the present time. The wealthy had costly articles in pottery and precious metals buried with them, and it is on account of this custom that many graves have been opened with the expectation of finding valuables. The women had their spindles for spinning, and in some instances the last thing they did before leaving their work forever, as shown by the unfinished web of cloth placed about them. Flowers were found by Lieutenant Gardner as fresh to the eye as if plucked only a short time previous, but of course in a dried state.

Articles of the toilet were also found, such as mirrors, combs made of fish bones set in wood, and hollow bones of birds carefully plugged with cotton and filled with pigments of various colors, while close at hand was the swab used in applying them to the face. Rings were in some instances of the precious metals, but all those seen by Lieutenant Gardner were made of copper; he also found implements for sewing. The children were surrounded by toys of native make.

On account of their nearness to the shore and their surroundings, it is highly probable that the mummies seen by Lieutenant Gardner were those of fishermen and their families.

The most interesting thing about these mummies is the finding of the so-called "Incas eyes." These were of various sizes, corresponding to the age of the individual.

These eyes are of an oval outline, flattened at one end and made up of concentric layers deposited about a central point. They are brittle and quite iridescent. They were found in the orbit, being held in place by the cloth which was bound about the head. Lieutenant Gardner was not certain whether they were placed under the eyelid — the eye being removed — or were outside the lid. His impression was, that they were outside, as they fell out as soon as the cloths which bound the head were removed. I cannot find any reason why they were used.

At first, I thought the eyes were composed of some resinous substance, but as soon as I began to examine them critically, I found that my first impression was erroneous. After examining sections and fragments, softened by long immersion in glycerine, I came to the conclusion they were the crystalline lens of some animal.

The next point to decide was from what animal they were taken. A clue was given by the fact that fragments left in distilled water for a day or two under a dust-shade, developed an odor which I could compare to nothing but that of old bilge water. Although this was a very questionable clue, yet it led to the successful solution of the question.

If the eye of a cephalopod be removed and carefully opened, it will be found that the "anterior of the retinal chamber is occupied by a bi-convex lens divisible into a smaller outer and a larger semi-globular internal part, the two being separated by a membrane." The principle of the well-known Coddington lens is the same as that which enters into the formation of this eye. The posterior portion of this eye is the one made use of by the

[1] αὔξη, growth, and λόγος, science.

[2] See a paper entitled "The Terms of Auxology," by S. S. Buckman, F.G.S., and F. A. Bather, M.A., F.G. S., London, in the Zoölogischer Anzeiger, No. 405 and 406, p. 42), Nov. 14 and 28, 1892.

ancient Peruvians. The source of supply was doubtless from the squid or octopus, which are still found in abundance along the coast.

CURRENT NOTES ON ANTHROPOLOGY.—XXII.

[*Edited by D. G. Brinton, M.D., LL.D.*]

The Canstatt and Neanderthal Skulls.

EVEN in some very late treatises on archæology and ethnology I observe that there is still talk of the "race of Canstatt" and the "race of Neanderthal," these imaginary races of ancient Europe being supposed to be represented by the skulls found in those respective localities. The late M. de Quatrefages was, I believe, responsible for the erection of these skulls into "types," and for the theories of prehistoric ethnography based upon them.

It should be recognized, once for all, that there is no sort of foundation for these scientific dreams. In neither instance did the locality in which these skulls were found guarantee them any high antiquity. The Canstatt skull was unearthed along with Roman pottery, and in all probability belonged to the fourth or fifth century, A. D. The Neanderthal skull, on which still greater stress has been laid, and casts of which are to be seen in most archæological museums, was not dug up at all, but was picked up in a gully which had been washed in the mountain side, and came from dear knows where. Probably there had been an old graveyard further up the hill, but by no means one in quaternary times. The fragment, moreover, is so fragmentary, and presents such evident signs of pathologic processes, that it is more than daring to assume it as the typical cranium of any race.

These and many allied facts in the same direction were admirably brought out in a discussion last August at the meeting of the German Anthropological Association by such speakers as Von Holder, Virchow, Kollmann, and Fraas. Their arguments leave no room to doubt the unimportance of these remains.

Time-Reckoning of the Mayas.

A short but carefully studied article in a recent number of the *Globus* (Bd. 63, No. 2), by Dr. Förstemann, presents some striking facts showing the accuracy attained by the ancient Mayas of Yucatan in the calculation of time. His sources are the Dresden and other ancient codices, to the interpretation of which he has devoted fruitfully much study. The contents of the Dresden Codex is largely astronomical or astrological, several of its pages being comparisons of the relative times and positions of the heavenly bodies. It is clear that these sky-readers had ascertained that the mean synodical revolution of Venus is 584 days, which is correct to a very small fraction. They had fixed the revolution of Mercury at 115 days, and it is probable but not quite certain that they had rightly estimated the revolution of Mars at 780 days. Jupiter and Saturn they did not study, or, at least, take into these calculations.

Not less surprising was the accuracy they reached in measuring the lunar month. They had by their observations reduced it to 29.526 days. This is about five minutes in the month too short, as the true synodical revolution is 29.53 days. For this differencs, intercalary days would be required at certain epochs.

It is probable from this that the Mayas were ahead of any other American stock in the measurement of time, exceeding even the Mexicans; though these also appear to have discovered the length of the year of Venus. Dr. Förstemann's discussion of the subject amounts to a demonstration, and merits the close attention of students of Maya civilization.

The Co-Existence of the Mammoth and Man.

Not long since, the distinguished and venerable archæologist, J. Steenstrup, of Copenhagen, published a paper examining the discoveries in Europe which are supposed to prove the contemporaneity of man with the mammoth; and reached the conclusion that not only is the evidence inadequate, but for climatic and geologic reasons no such co-existence was possible.

At the last meeting of the German Anthropological Association Professor Virchow quoted Steenstrup's conclusion and endorsed it, as did also others present. The "reindeer period" was the remotest to which they were willing to assign the appearance of man in Europe on existing evidence. The artefacts of mammoth teeth and bones found in the caves were asserted to be from fossil remains picked up by the cave men. Where such artefacts are found in gravels along with mammoth bones, they would say that these gravels are themselves posterior to the reindeer period, and hence contain objects of various preceding periods.

There remains for consideration the delineation of a mammoth on a bone from the Lena cave in the south of France. This was but discussed, being probably considered of questionable origin. In the United States two such delineations have been brought forward. They are both strikingly similar to this French original, which has long been made familiar to American readers through various publications. Both proceed from the valley of the Delaware River. One is on shell and one on stone. I have examined both originals very carefully, and apart from the vagueness which surrounds the finding of both, for purely technical reasons I believe both to be recent. There still lacks conclusive evidence that man and the mammoth were contemporaneous in the area of the United States.

Proposition for an Ethnographic Study of the White Race in the United States.

In preparing some lectures last winter on the ethnography of the United States, I was struck with the deficiency of trustworthy material on this subject. The Indians and the Negroes have received far more attention at the hands of ethnologists than the whites. It is high time that a systematic study be made of the latter, with a view to discover what influences the New World and its conditions have exerted on this race wholly foreign to its soil.

I would propose that a plan be adopted similar to that which has recently been outlined in Great Britain for an ethnographic survey of that kingdom. A joint committee has been appointed by the leading anthropological, antiquarian, and folk-lore societies to raise means and carry out details. A list of certain typical villages will be made in which there are at least a hundred adults whose ancestors are believed to have lived a number of generations in the district, and to have been subjected to a minimum outside influence. From this list the committee will select the most promising places, and will send a properly equipped student to record the following points:—

1. Physical type of the inhabitants by measurements, photographs, etc.
2. Peculiarities in dialect, local pronunciations, expressions, etc.
3. Local traditions and superstitions.
4. Old buildings, relics, and other antiquities.
5. Historical evidence and genealogies showing purity of race.

Such a plan could be most advantageously carried out in the United States. Suppose thirty students were selected, trained, and sent to pass their summer vacation in as many secluded villages in New England, the Middle States, and the oldest settled portions of the South, all pursuing their investigations on the same lines. We should receive a mass of the most valuable information by which to solve many most interesting and instructive ethnographic problems. One pleasant feature would be the very moderate expense for which this could be accomplished; for these secluded villages are precisely where one can live the cheapest in the whole country.

We could then compare the descendants of the middle class English who settled New England with those of the Scotch-Irish and Palatine Germans of Pennsylvania, with the French of South Carolina and Louisiana, the settlers of the mountains of Virginia and East Tennesse, the "crackers" of Georgia, and so on. Will not the active societies in the United States interested in these lines of research unite their efforts to realize some such project?

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RECENT OBSERVATIONS AT KILAUEA.

BY JOSIAH KEEP, MILLS COLLEGE, ALAMEDA CO., CAL.

The great volcano of Kilauea, on the island of Hawaii, like all other live things, is constantly changing, and any report of its condition is liable to need important corrections on the advent of the next steamer. The last great explosion, however, took place in 1790, more than a century ago, and since that time the huge pit has been filling up with black lava and the area of activity has been narrowing. During the month of July last I had an opportunity to observe the igneous action under exceptionally favorable circumstances, and a record of its condition at that time can hardly fail to be valuable for comparison with past and future reports.

The crater is a huge depression or pit, about three miles long and two miles broad. The walls are mostly precipitous, though quite irregular, and the floor is some three hundred feet below the surface of the island at that point. Forty years ago it was several hundred feet lower. Standing on the brink of the crater and looking down, one is reminded of a great cellar after a fire. Every thing is black or rusty, and the smoke and steam coming up from dark clefts put you in mind of the charred and smoking timbers to be seen after a conflagration. A zigzag path, a mile long, leads down through ferns and bushes to the black lava, and then you step out on a sea of absolute desolation. The lava is cold now, but there are the most abundant evidences of its recent fusion. The surface is greatly varied; here being nearly smooth, and there swelling up into steep hillocks, perhaps with caves beneath them, into which you can creep or perhaps walk upright. Cracks abound, and out of some of them the hot slag has oozed, and flowed, and cooled, and hardened.

After walking over two miles of this rough floor I came suddenly to the brink of a second pit in the floor of the greater one. This second pit, the "Halem'oum'ou" of the natives, is about half a mile in diameter, and at the time of my visit its floor was some two hundred and fifty feet below the point where I was standing. Some adventurous climbers descended the precipitous sides and actually stood on the freshly-cooled lava, but I did not accompany them. In the centre of this lower floor was the lake of moulten lava, nearly circular in outline, and about one thousand feet across. Its level surface was largely covered by a thin, gray crust, portions of which would often sink and reveal the glowing liquid beneath.

The fiery lake was never free from agitations, particularly around its edges, but the extent and violence of the activity were constantly changing. Occasionally a liquid hillock would rise like an enormous bubble, then sink back again, while a puff of thin, blue smoke would slowly rise and float off from the spot, showing that in a condensed state it had doubtless been the lifting agent. But most of the agitation resembled the lively boiling of a kettle of water over a brisk fire. The glowing fountains would jump and dance in the wildest manner, often throwing up the fiery drops to a height of fifty feet, while waves of lava would surge against the curb of the lake with a sound like that of ocean breakers In the night time, seen through an opera glass, the display was beautiful and grand beyond description.

The continual falling of half-cooled drops of lava around the edge of the lake, combined with the wash of the fire-waves, serves to build up a curb, which grows in proportion to the activity of the lake. On one side of the pool of melted rock its top was some thirty feet higher than the floor which joined the base of the curb to the walls of the pit. One night the lava rose in the lake and poured over the curb on that side in a magnificent cascade of fire. It was not possible to get in front of the overflow, but it was estimated that the stream was fifty feet wide. The motion of the current was like that of a water cascade, but when the flood reached the floor of the pit it quickly began to congeal on the top, while the under part ran on till it reached the confining walls. Another overflow, where the curb was not so high, came directly towards my point of observation, and I could clearly see that the central point of the stream moved swiftest, causing the hardening waves to assume the well-known crescent forms.

By such overflows from the moulten lake the inner pit is being gradually filled up; in fact, its floor has risen several hundred feet the past few years. The lake rises *pari passu*, the curb never rising very high above the floor. What the result will be is uncertain. Should the lava continue to rise, the pit will soon be filled and will overflow into the basin of Kilauea itself. But instead of this the bottom of the pit may drop out, so to speak, as it did very suddenly before this last rise, and instead of gazing into a lake of fire the tourist may be compelled to look into a huge smoking hole, some five or six hundred feet deep. Doubtless the whole floor of Kilauea rests on a very hot foundation, as the steam which ascends from many cracks indicates, but at the time of my visit there was no melted lava visible except in the lake which I have described.

The questions presented by these phenomena are intensely interesting; but the more I observed the boiling of the lava, the more I became convinced that aqueous vapor is not the chief agent which does the work, though it may be concerned in starting the tremendous chemical action, perhaps a decomposition of sulphides, which I think is the source both of the heat and of the commotion.

EXTREMES IN THE PLANT WORLD.

BY PROF. J. I. D. HINDS, LEBANON, TENN

Of living organisms, the largest, as well as the smallest, are found in the vegetable kingdom. In point of bulk, even the elephant compares unfavorably with the largest trees, and the smallest living objects, seen by the help of the microscope, are undoubtedly plants.

The largest plants known are what are popularly called "the big trees of California." They are conifers, belonging to the genus Sequoia, which is intermediate between the firs and cypresses. There are two species, *S. sempervirens* and *S. gigantea.* The former is the common redwood and abounds on the Coast Range from the southern part of California northward into Oregon. The latter is not so common, but grows to a larger size. "It is confined to the western portion of the great California range, occurring chiefly in detached groups, locally called 'groves,' at an altitude of from 4,000 to 5,000 feet above the sea." It grows to enormous size, varying in height from 200 to nearly 400 feet and in diameter from 20 to 30 feet. One tree in Calaveras County is 325 feet high and 45 feet in circumference six feet from the ground. Another measured 90 feet in girth and 321 in height. Some of these trees are supposed to be 3,000 years old. They were then in their vigor when the Roman Empire was at the height of its glory and hoary with age when Columbus landed on the American shore.

Let us now turn from these giants of the forest to those plants which can only be seen with the higher powers of the microscope. The smallest of these and at the same time the smallest of living things are the plants known as Bacteria. They have an average diameter of one twenty-five thousandth of an inch and a length one to ten times as great. Many of them have a diameter of less than one fifty-thousandth of an inch and it is probable that there are multitudes of them so small that the highest powers of the microscope do not render them visible. Two thousand of them could swim side by side through the eye of a needle and one could hold in his single hand fifty millions of millions of them. Of the smaller ones it would take 15,625,000,000,000 to fill one cubic inch.

Now compare these with our mammoth Sequoias. The trunk of one of these trees, to say nothing about its roots and branches, contains at least 200,000,000 cubic inches. It is, therefore, 3,125,000,000,000,000,000,000 times as large as a single bacterium. This number is, of course, inconceivable. It may be read 3 125 millions of millions of millions. The proportion is about the same as that of an ordinary football to the earth itself.

Again, the duration of the life of many of the bacteria is only an hour. There are 8,760 hours in a year, and in 3,000 years there are 26,280,000 hours. Thus the tree has lived on while more than twenty-six millions of generations of its invisible kindred may have lived and died in the stream at its base. From the bacterium to the sequoia, what a span! Yet the rolling globe on which they live is but a speck in the universe, its diameter too small to be used as a measuring unit for interstellar spaces. As many bacteria could be laid side by side on a linear inch as earths upon the diameter of its orbit around the sun. Compared with the tree, the bacterium is almost infinitessimal; by the side of the earth, the tree is insignificant; in the solar system, the earth is but a small factor; and if the solar system were annihilated, it would be millions of years before its loss would be felt on distant stars. Magnitudes are, therefore, relative, and things are great or small according to the standpoint from which we view them.

Cumberland University.

DESTRUCTION OF CROWS DURING THE RECENT COLD SPELL.

BY DR. ROBERT RIDGWAY, SMITHSONIAN INSTITUTION, WASHINGTON, D.C.

WHETHER it be the result of disease or exposure, the suffering inflicted on the crows in the vicinity of Washington during the recent severe weather is of great extent, and of such a character as to excite the sympathy of any one familiar with the facts. On the 20th of January my son went rabbit hunting, and on his return told me he had found many dead crows in the pine woods, and others that were totally blind. The following day I accompanied him to the place where he had found them, and was really astonished at the sight presented. Very few crows were seen flying about, but upon entering the thick woods of scrub-pines, which was evidently the roosting-place of large numbers of these birds, they were met with on every hand. Some were lying on the snow, dead and frozen stiff; many more were perched in the trees, at various heights, in all stages of helplessness. The majority of them could fly, and on our near approach would do so; but in a moment it became apparent that they could not see, for the first thing in their line of flight, as, for example, a branch, would stop them, when they would either flutter to the ground or, changing their course, would continue their flight, to be again checked by a branch, or if they happened to miss any obstruction until clear of the woods (which rarely occurred) they continued, slowly feeling their way, over the open fields, often dropping to the snow-covered ground after flying a few hundred yards. Those which did not fly at our approach were too much weakened from starvation to do so. They were easily caught, and in every instance were found to be absolutely blind, except one individual, which had one eye but little affected. In many the eyes were closed and much swollen; in some one or both eyes had burst and frozen, this having possibly been caused by violent contact with the sharp ends of broken twigs. In all cases in which the eyes were not closed or inflamed the pupil was milky white and the iris bluish. Inability to find food on account of their blindness was evidently the immediate cause of starvation; for it was found that the dead birds were, as a rule, very much emaciated, while many of the living ones, particularly those which were most active, and consequently difficult to capture, were in fairly good condition. It was pitiful to behold their suffering, both from the pangs of hunger as well as from the pain of their wounded eyes. Sometimes the snow beneath the trees was nearly covered by pine needles and small twigs which they had plucked off and tried to eat (they were seen doing this), while several of those which had fallen to the ground were eating snow.

The extent to which this epidemic, or whatever it may be, has affected the crow population of this locality is not easy to estimate. My first impression was that the species was nearly exterminated there, since certainly 95 out of every 100 crows seen during the day were perfectly "stone-blind," and 10 per cent of them dead. That this impression was incorrect was, however, proven by the next day's observation, the locality being visited much later in the day, when large numbers were seen coming in from the surrounding country to roost, — all these "able-bodied" crows having been abroad after food at the time of our previous visit. There seemed to be about as many of these as there were of the disabled ones, so the reduction in their numbers will probably not exceed one-half, and may not be so great.

A third visit, several days later, showed no increase among the afflicted birds. There were, however, as might have been expected, a much larger number of dead ones, while those still living were found more scattered, being encountered nearly everywhere in the open fields, where they had fallen, exhausted, during their flight from the woods.

So far as I was able to discover, after very careful examination of all specimens within reach, during both visits, only the common species, *Corvus americanus*, was affected by the malady. At any rate, neither my companions nor myself could discover a single fish crow (*C. ossifragus*), though the latter was well represented among those which were flying about.

I am at a loss to account for this scourge. Several causes have been suggested, the most plausible of which, it seems to me, is that in returning to their roosting-place one excessively cold evening they were compelled to face a freezing wind, perhaps bearing minute ice-particles, which actually froze their eyes. It may be, however, that a better explanation can be given.

REMARKS ON AMERICAN LICHENOLOGY. — III.

BY W. W. CALKINS, CHICAGO, ILL.

THE explorers for lichens in a locality so favorable as Florida will not fail to notice the abundance of brilliantly colored fungi, and, if interested, will be tempted to collect them. On some of these will perhaps occur parasitic lichens of rarity, as *Colnogonium* and *Opegrapha*. But beneath a bed of *Agaraci*, on the sandy soil of an old plantation, a close search will show another interesting lichen, known as *Heppia despreauxii* Tuck. Its character was long disputed, owing to a close resemblance to an allied genus of lichens, *Solorina*. The small cup shaped apothecia, growing single or in clusters, immersed in a green thallus, have deceived good lichenists. We owe to Dr. Tuckerman the elucidation of this elegent species. Only two were described by him in the "Synopsis." Last winter I had the good fortune to find another in the mountains of Tennessee, which, having been sent in vain around our own country, a puzzle to all, was promptly determined by Dr. Nylander of Paris to be the *Heppia virescens*, *Ach. variety rugosa* Nyl. I may remark that it is astonishing how soon afterwards we all saw the point.

In the old field as well, with a mixed second growth of *Pinus taeda*, *Ilex opaca*, *Ilex Cassine*, *Myrica cerifera*, *Olea americana*, etc., will be found on their foliage numerous small fungi, such as *Sphaeria* and *Cercospora*, many of which have been illustrated by Professor Ellis in his "Exsiccati" from my collections of fungi.

In close contact, lichens and fresh-water algæ and *Hepaticæ* also hold equal sway. But, towering over all, the stately *Magnolia* and the *Gordonia* (red or bull bay), with their glossy evergreen foliage, afford us the tropical lichen, *Strigula complanata* Fee., and, rarer still, *Heterothecium augustini* Tuck., though, indeed, the *Sabal serrulata*, common everywhere, abounds in elegant specimens in some localities. There are also on this prostrate palm most remarkable fungi, for which see Ellis. By the slow-running stream occur *Biatora hynophila*, on mossy substrates. Many terricoline lichens of rarity will reward a patient collector. I have often visited one locality, leaving it at last in the belief that nothing more could be found. However, still unsatisfied and impelled by something, I would return and find new prizes, as I soon learned from my teachers. I mention this to show that no researches in the field of nature can be wholly completed. I also offer it as an incentive to thorough work. Whilst lichens thrive almost everywhere in Florida, sometimes in very novel situations, the vicinity of the ocean is prolific of them. Even an old *Ostrea* shell has its peculiar *Verrucaria;* on old timber, *Xylographa;* while just inland, among dense thickets of *Ilex cassine*, revel *Arthonias* and *Graphis*. Here also the beautiful rosettes of the *Cladonia rangiferina* L., *variety alpestris* L. (which is *F. minor* of Michaux), cover the earth and are known to the uninitiated as mosses,— price to the winter tourist who searches for nature's gems in hotels twenty-five cents.

In open places the eye will often rest upon a carpet of the crimson-fruited *Cladonia leporina* Fr. and *C. pulchella* Schw. There are also other species of this genus, but less conspicuous on account of having brown fruit. On shrubs near the sea occur in abundance very fine specimens of *Ramalina rigida*, *variety montagnaei* Tuck. But we tire of conspicuous forms at last, and seeking the most difficult and least known, find them in *Arthonia* and *Graphis*. The following species are sufficient to show what may be expected in a field where investigations have been merely begun.

Arthonia albovirescens Nyl. A new species on *Ilex cassine* at Fort George, and on shrubs in tropical Florida. A good species (Nyl. Lich. N. G.) (*Bot. Bull.*, 1889). (*Syn. Arthonia* Willey.) Abundant.

Arthonia floridana Willey. A new species collected by me at Jacksonville on *Ilex* (*Syn. Arthonia* Willey). Rare.

Arthonia ochrospila Nyl. On *Myrica cerifera*, at Jacksonville. Also Cuba. Rare.

Arthonia gregarina Willey. On *Myrica* sparingly at Jacksonville and south. (*Syn. Arth.*)

Arthonia taedescens Nyl. A very fine and rare species on *Ilex cassine*, at Jacksonville and south. (*Syn. Arth.*)

Arthonia ochrodiscodes Nyl. A new species, *Ilicicola*. Fort George and southward. Described by Nylander in "Lichenes Japoniæ," page 107. Quite distinct. Abundant.

Arthonia platygraphidea Nyl. An elegant species I collected from Fort George south. Also Mexico.

Graphis adscribens Nyl (*Lich. N. Caled.*). Found by me on *Gordonia* and other trees, Jacksonville to tropical latitudes. Also in Mexico. Very fine.

Graphis nitidescens Nyl. Very minute, white, and hard to find. I have had several so named, all differing from the true one identified for me by Nylander. On *Liriodendron*, at Jacksonville and southward to Cape Sable (Tuckerman, Syn. Pt. II., page 123).

Among the new *Graphis* of Florida described by Nylander, I will only mention now *Gr. abaphoides*, *Gr. subparilis*, *Gr. subvirginalis*, *Gr. turbulenta*, all tropical or sub-tropical.

Platygrapha subattingens Nyl. A new species, *Supercorticem*, *Liriodendri*, at Jacksonville; southwards to Cuba. Described by Nylander in "Lichens N. G.," page 51. A very fine lichen (*Bot. Bull.*, 1889).

OSTEOLOGICAL NOTES.

BY DANIEL DENISON SLADE, MUSEUM OF COMPARATIVE ZOÖLOGY, CAMBRIDGE, MASS.

THE order of the Ungulata may at the present time be divided into the Ungulata vera, including therein the two sub-orders, Perissodactyla and Artiodactyla, and the Ungulata polydactyla, or Subungulata of Cope, which also comprises two sub-orders, Hyracoidea and Proboscidea.

In its morphology, the jugal arch of the Ungulata presents various modifications. With few exceptions, two bones only enter into its composition, the squamosal and jugal, which are connected by a suture, the general direction of which is horizontal. Both the horizontal and vertical curvatures of the arch present considerable variations, as does also its relation to the neighboring parts.

In the group Perissodactyla, the family Equidæ exhibits an arch, which, although relatively slender, is quite exceptional in its arrangement. The large and lengthened process of the squamosal not only joins the greatly developed post-orbital process of the frontal, but, passing beyond, forms a portion of the inferior and posterior boundary of the orbit. The malar, spreading largely upon the cheek, sends back a nearly horizontal process to join the under surface of the squamosal process above described, thus completing the arch, while the orbit is entirely surrounded by a conspicuous ring of bone, thereby clearly determining the bounds between it and the temporal fossa, which last is remarkably small. Moulded into this fossa, which is bounded above and posteriorly by more or less well-developed crests or ridges, is the temporal muscle. The pterygoids are slender and delicate, without the presence of any fossa. The glenoid surface is much extended transversely, concave from side to side, and bounded posteriorly by a prominent post-glenoid process. The angle of the jaw is much expanded. The condyle is much elevated above the molar series, while the coronoid process is long, narrow, and slightly recurved.

In the Rhinoceridæ and Tapiridæ the arch is strongly developed, and composed of the squamosal and jugal processes, which are joined at about its centre by an oblique suture from above downwards, backwards, and upwards. There is a small post-orbital process, largest in the tapir, but the orbital and temporal fossæ are continuous. The surface for the temporal muscle is extensive. The glenoid fossa presents a transverse, convex surface to articulate with the corresponding one of the mandible, which is not much elevated above the dental series. The coronoid process is slender and recurved, while the angle is broad, compressed, somewhat rounded, and incurved.

In the Artiodactyla, the arch is slender, and is composed of the process from the jugal, which passes backwards beneath the corresponding forward projecting process of the squamosal, the juncture being by a suture nearly horizontal in direction, and longest in the Cervidæ. The jugal also sends up a postorbital process to meet the corresponding descending one of the frontal, the suture which unites them, being about midway. Thus the bony orbit is complete, while the jugal is forked posteriorly. The temporal region is relatively small. The horizontal curvature of the arch is very slight. The glenoid fossa is extensive and slightly convex, with a well-developed post-glenoid process. The pterygoids present a large surface and are situated nearer the middle line than is the case in the Perissodactyla. The condyle is broad and flat, and the coronoid process is long, compressed, and slightly recurved. The angle is rounded and much expanded.

The Tylopoda alone among the Ruminantia have large surfaces and accompanying crests and ridges for the increased development of the temporal muscles. The horizontal curvature of the arch is greater than in the true Ruminants, consequently the temporal fossa is wider and deeper — all in correlation with the powerful canine teeth. The forked articulation between the molar and the squamosal is also more strongly marked.

Among the non-Ruminantia, the family Suidæ, or true pigs, exhibit an arch in which the process of the jugal underlying the squamosal extends back to the glenoid fossa — the two bones being connected by a suture, which is vertical anteriorly for the depth of half the bone, and then horizontal. The post-orbital process does not meet the frontal; in fact, all traces of this are lost in Sus serofa. In the Peccary and Barbaroussa it is quite prominent. The arch is short, and the vertical as well as horizontal curvatures are considerable. The narrow, transverse, condylar surface of the mandible, and the small coronoid process, with its rounded superior surface, are but slightly raised above the level of the alveolar surface. The pterygoid surface is extensive and the fossa deep.

In the Hippopotamidæ, the arch is broad and strong. Its superior border presents a marked sigmoid curvature, and the convexity, which is always posterior, is in this case much shorter in proportion. The temporal fossa, as also the surface for the muscular insertions, are extensive. The pterygoid surface is not so large as in the Suidæ. The glenoid fossa is slightly concave, but not bounded externally by a continuation of the jugal. The condyles of the mandible are nearly on a level with the molars, and the coronoid process is small and recurved. The angle is greatly modified for muscular attachment.

In the Hyracoidea, the arch is composed of three bones, of which the jugal is the most important. Resting anteriorly upon the maxilla, the jugal sends backwards a process to form the external boundary of the glenoid fossa. It also sends upwards a post-orbital process to meet a corresponding one from the parietal alone or from the parietal and frontal combined, thus completing the bony orbit. Both horizontal and vertical curvatures are slight. The surface for the temporal muscle is largely developed, while the pterygoid fossæ are well marked. The ascending ramus of the mandible is high, and the angle is rounded and projects very much behind the condyle, which last is wide transversely, and rounded on its external border. The coronoid process is small, slightly recurved, and not on a level with the condylar surface.

In the Proboscidea, the arch is straight and slender and composed of three bones. The maxilla forms the interior portion, while the jugal, supported upon the process of the maxilla, meets that of the squamosal in the middle of the arch, and is continued under this as far as the posterior root. This modification is unlike that of any other ungulate. There is a small post-orbital process from the frontal. The temporal surface is extensive, and that of the pterygoid considerable. The ascending ramus of the mandible is high, and the condyle small and round. The coronoid process is compressed, and but little elevated above the molar series. The angle is thickened and rounded posteriorly.

As has previously been remarked in regard to other orders of the Mammalia, the modifications undergone by the jugal arch in the Ungulata are determined by the development of the masticatory muscles. In the Perissodactyla, for example, the sagittal crest, ridges, and extensive parietal surface are correlated with increased insertions of the temporal muscle, while the large, strong, and complicated arch have equal reference to a powerful masseter. So in the Artiodactyla, especially in the Ruminantia, the diminished surface for the temporal, and the smaller, weaker arch, both denote diminished energy in the above muscles, while the enlarged pterygoid muscular insertions show that the required action has been provided in another direction. As Professor Cope has shown, "Forms which move the lower jaw transversely have the temporal muscles inversely as the extent of the lateral excursions of the jaw. Hence these muscles have a diminished size in such forms as the Ruminants, and are widely separated."

The singular fact that the Tylopoda alone of the selenodont Artiodactyla possess the sagittal crest is explained by Professor Cope, by the presence of canine teeth, which are used as weapons of offence and defence, and which demand large development of the temporal muscles. Moreover, this group retains the primitive form of the molar series, which is below and posterior to the vertical line of the orbit, while in the Bovidæ it is anterior.

EARLY METHODS OF BORING.

BY JOSEPH D. MCGUIRE, SMITHSONIAN INSTITUTION, WASHINGTON, D. C.

IN the process of recent investigations at the National Museum into early methods of boring as practised by different races, the writer thought that the similarity existing between the Esquimaux toggle or two-handed strap-drill, and practically the same implement used by the ancient Greeks and Hindus, and also the resemblance between the bow-drill used by the early Egyptians and the same tool used by American Indians could not fail to interest those concerned in early methods of boring.

There is an Egyptian fresco in the Royal Museum of Berlin representing a workman with a bow-drill boring a hole in the bottom of a chair, and the only difference between the drill he is using and those in the National Museum collection, especially from the Eskimoan area, is that the Egyptian bow appears much longer than the same tool used by our Indians.[1]

There is much in a comparison of these drills that is of interest regarding the evolution of the implement and the possibility of independent invention. The toggle or two-handed drill consists of a shaft a foot or more in length, a head-piece or bearing of wood or ivory, with often a stone socket for the drill-shaft to revolve in at the top. This socket-piece is held by the one working it between his teeth, and thus kept in position. The shaft is revolved by means of a narrow strap of leather wrapped once around it. On the ends of the thong are tied pieces of wood or bone by which the operator pulls the strap alternately to the right and to the left, thereby revolving the drill, which by downward pressure on the socket-piece is prevented from slipping aside.

In the ninth book of the Odessey, Ulysses describes how he and his companions, imprisoned in a cave, bored out the eye of Polyphemus (Cowper's translation.)

> "They grasping the sharp stake of olive wood,
> Infixed it in his eye, myself advanced
> To a superior stand, twirled it about.
> As a shipwright with his wimble bores
> Tough oaken timber, placed on either side
> Below, his fellow artists strain the thong
> Alternate, and the restless iron spins,
> So, grasping hard the fire-pointed stake,
> We twirl'd it in his eye; the bubbling blood
> Boil'd round about the brand."

The bow-drill used by the Zuni and other American tribes is an immense improvement on the above, for the thong is attached to a bow worked with the right hand, and the head-piece is held by the left, thus saving the jar to the head and teeth, which with the toggle drill was considerable.

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.

Confusion in Weights and Measures.

THE remarks of Professor W. P. Mason on "Confusion in Weights and Measures" in *Science* for Dec. 23, 1892, are interesting and timely. A few erroneous statements which they contain serve only to emphasize the fact that the system of weights and measures in customary use is so confusing, so unscientific, and, in some instances, apparently so contradictory that it is difficult to write of it, even briefly, without falling into error. It may be useful to the readers of *Science* to have some of these errors corrected and also to be furnished with a brief statement of the existing condition of the question of standards in the United States.

Professor Mason's difficulty in ascertaining the number of grains in a gallon of water at 60° F. is a very natural one, and one not likely to disappear in the near future. The United States gallon is a measure of capacity and not of mass. It contains 231 cubic inches. The mass of this volume of water at any given temperature can only be determined by experiment, and an accurate determination is exceedingly difficult. All results must be regarded as approximations, and variation among them means no more than variation among published values of other physical constants, which are determined by experiment, but can never be fixed by legislation. It has always been customary in the United States Office of Weights and Measures, as indeed it may be regarded as almost necessary, to adjust the volume of a capacity standard by ascertaining the mass of water which it will hold under certain conditions of temperature and pressure. But this is merely a matter of convenience; the gallon is by definition 231 cubic inches, and the bushel is 2150.42 cubic inches, and when it is desired to ascertain the mass of a gallon of water one must select that value of the density of water which one thinks the

[1] Lepsius, Kong'l. Museum, Abtheil. der Aegypt. Alterthümer, Berlin, 1855, tafel x.

most accurate. The latest determination of the mass of a cubic inch of water is that of Mr. H. J. Chaney, superintendent of weights and measures in London, which was communicated to the Royal Society on Feb. 4, 1890. Mr. Chaney ascertained the weight of water displaced by three bodies, which he designated respectively by the letters C, Q, and S They were:—

C, a platinized hollow bronze circular cylinder, 9 inches in diameter and height.

Q, a quartz cylinder, 3 inches in diameter and height.

S, a hollow 6-inch brass sphere.

With these he found as follows: —

In normal air a cubic inch of distilled water, freed from air, at the temperature of 62° F., was found to weigh —

C 252 267
S 252.301
Q 252.261

By normal air is meant "Air at $t = 62°$ F.; $p = 30$ inches, containing four volumes of carbonic-anhydride in every 10000 volumes of air, and also containing two-thirds of the amount of aqueous vapor contained in saturated air, weighed at Westminster, latitude 51° 29′ 53″— at 16 feet above sea-level. A cubic-inch of such air weighs 0.3077 of a grain "

The International Bureau of Weights and Measures is engaged in the investigation of this constant, and when its conclusions are published the question will probably be definitely disposed of for a long time to come.

The Troughton 82-inch scale was formerly accepted as a standard of length, but for many years it has not been actually so regarded. By reason of its faulty construction it is entirely unsuitable for a standard, and for a long time it has been of historic interest only. Since its rejection as a standard the United States yard has been considered as identical with the imperial yard of Great Britain, the material representations of which are two accurate copies, made and presented to the United States at the time of the adoption of the imperial yard.

The standard of mass has been the avoirdupois pound, identical with the imperial pound of Great Britain, except for purposes of coinage, for which the standard is the Mint Troy pound, brought from London in 1827, and which was legalized for this purpose by Act of May 19, 1828, and re-enacted in the year 1873.

As, with a single notable exception to be referred to later, this is the only legislation by Congress upon the subject of standards, it is important to inquire by what authority the standards above mentioned exist as such. Professor Mason has indirectly answered this. Congress having failed to take advantage of its constitutional privilege of establishing a uniform system of weights and measures. it became necessary to provide standards for the executive departments, by means of which taxes and revenues could be determined and collected. As the Treasury Department was mostly concerned in these matters, the question of standards was left to it. To the first superintendent of the Coast Survey, Mr. Ferdinand Hassler, was committed the task of constructing standards having the necessary degree of precision, and he was made superintendent of the Office of Weights and Measures. The Troughton scale was brought to this country by him early in this century. A part of it was selected as the standard yard. In the absence of legislation, it will be seen that the standards of the United States Government were those approved as such by the secretary of the Treasury, on the recommendation of the superintendent of Weights and Measures. In the mean time, it was known that there was great lack of uniformity among the various States. To encourage such uniformity Congress, in 1836, authorized the construction of copies of the various standards used in the Treasury Department, to be distributed to the governors of the several States. This action was taken by the Office of Weights and Measures, and did much to bring about uniformity. At once many, and finally nearly all, of the States made these copies their standards, and thus practical uniformity was secured. Theoretically or rigorously, however, there are about as many systems of weights and measures in use to-day as there are States in the Union. There are cases, indeed, in which no legislation whatever has taken place, and, while there are severe penalties for the use of false measures, there is nothing to fix what measures are true, except, of course, as custom or common law controls.

The additional national legislation referred to above is the Act of 1866, by which the metric system was legalized over the whole country. This is interesting and important as being the one single bit of general statute upon the subject of weights and measures.

In 1875 the International Metric Bureau was organized. To it practically all civilized nations are now contributors. Its object was to construct and distribute prototype standards of the metre and kilogramme to the various contributing nations. These standards were completed and distributed about three years ago. The seals upon the standards for the United States, metre No. 27 and kilogramme No. 20, were broken by Benjamin Harrison, president of the United States, on Jan. 2, 1890, in the presence of James G Blaine, the Secretary of State, William Windom, the Secretary of the Treasury, and a number of gentlemen distinguished in the various professions in which precision in measurement is highly regarded.

They have thus been accepted as standards of the first authority in this country, second only to the International prototype metre and kilogramme of the International Bureau at Paris.

The metric system having thus received the recognition of the only general legislation by Congress and of executive approval, it has been determined that both the necessities of practical operations in weighing and measuring and the demands of precise metrology will be best met by referring the units of the customary system to those of the infinitely more perfect and rapidly becoming universal system based on the metre and the kilogramme. The relations of the respective units are now so accurately known that this may be done with an approximation entirely satisfactory.

Fortunately the law of 1866, in its table of equivalents, is based on these relations as then known, and later investigations have only tended to confirm the value of the yard in metres as there defined. Thus the wisest course is also the easiest, and the yard and pound, as known in the Office of Weights and Measures, are now defined as a certain part of a metre and a kilogramme, respectively.

These definitions are as follows: —

1 yard $= \frac{3600}{3937}$ metre.

1 pound = 0.453597 kilogramme, according to the statute of 1866.

Or more accurately —

1 English pound = .4535924277 kilogramme.

These two values differ by approximately one part in one hundred thousand. T. C. MENDENHALL.

Office of Weights and Measures, Washington, D.C.

Easy Method of Calculating Complex Surveys.

A METHOD of calculation employed by Mr. L. M. Graham, manager of the McLean Co. Coal Co., of this place. is new to me, and may be useful, or at least interesting, to some of your readers. In the payment of royalties on coal mined, many exceedingly complicated underground surveys must be made, the computations of which are very difficult. Having made on a piece of tracing paper a plat of the survey, in all its windings, he transfers this plat to a piece of cardboard; and then cuts away the cardboard, making an opening the exact form of the plat. The cardboard containing this opening is then attached to a smooth surface as a back. As a measure, he has made in cardboard an opening one inch wide and several inches long; and down the edge of this has marked a scale; one square inch representing one hundred square feet. Taking very fine shot, he fills with this the opening in the cardboard representing the plat, taking pains to see that the shot lie but one deep; then pours these out into the measure; and readily makes his estimate. The manager says the plan was thought out by himself; and if a similar plan has been used elsewhere, he has not known of it. It strikes me as being ingenious, and widely applicable to complicated surveys, whether below or above ground. R. O. GRAHAM.

Bloomington, Ills., Jan. 25.

Notes on Several Special Transformations.

WHILE reciprocating and subtracting from unity both belong to the periodic transformations whose period is two, yet the two combined lead to a transformation whose period is six. It is of special interest to observe that the six values thus obtained are the six related values of an anharmonic ratio. They are the following: —

$$a, \frac{1}{a}, \frac{a-1}{a}, \frac{a}{a-1}, \frac{1}{1-a}, 1-a.$$

This furnishes a convenient means of remembering these important values.

In the special case of homographic transformations, when

$$x = \frac{ay - a_2}{a_3 y - a}$$

we easily see that is expressed in the same form with respect to x as x is with respect to y. That is

$$y = \frac{ax - a_2}{a_3 x - a}$$

When x and y are reals the locus of this equation is symmetrical with respect to the bisector of the angle between the x and y axes. GEO. A. MILLER.

Eureka College, Jan. 26.

Skeletons of Steller's Sea-Cow Preserved in the Various Museums.

IN the last number of *Science* (Feb. 3, 1893, p. 56) Dr. Barton W. Evermann has an interesting note on the "Skeleton of Steller's Sea-Cow," which he was fortunate enough to purchase for the National Museum during his stay at Bering Island, 1892. The article is slightly erroneous where he enumerates the material in the museums previous to his visit to the island, as many more skeletons and parts of skeletons are preserved than he thinks.

He says: "This [i.e., the skeleton in the U. S. National Museum, made up from bones brought home by me], together with the two skeletons at St. Petersburg and Helsingfors, and the two ribs in the British Museum, constitute the total amount of material pertaining to Rytina found in the museums of the world at the time of my visit to Bering Island."

Let me add to this that there is a fairly good skeleton in the museum of the Swedish Academy of Sciences at Stockholm, brought home by Nordenskiöld, and figured by him in his famous account of the "Vega" expedition. Another "nearly perfect" skeleton is in the British Museum, described and figured by Henry Woodward in the Quarterly Journal of the Geological Society (London, August, 1885, pp. 457–472). A third skeleton of *Rytina gigas*, and, in some respects at least, the best one, is in the museum of the Academy of Sciences in San Francisco, where it was mounted during the early part of 1892. This skeleton was formerly part of the museum belonging to the Alaska Commercial Company, but was afterwards presented to the Academy. As I said, this skeleton is in some respects superior to any one thus far found, although the cranium mounted with it belongs to another specimen. It was found on Bering Island during the winter of 1881–82, and as the cranium was not in as good condition as the rest of the skeleton a better one was substituted. I acquired the original, which is among the many crania which I collected for the National Museum.

These are the three entire skeletons of which I have any record, but there are undoubtedly several others in various museums. If I am not mistaken, St. Petersburg has acquired additional material (recently the Museum there offered a skull in exchange), and so have the museums in Moskva, Odessa, and, above others, Warshaw, to which city Dybowski sent most of the material collected by him. It is also reasonable to suppose that he reserved some for the universsty in Lemberg.

I myself collected about 20 crania for the National Museum besides quite a number of isolated bones in addition to those which were used in the "made-up" skeleton. Some of this material has been distributed to the various museums, if I am not mistaken.

It will thus be seen that "the total amount of material pertaining to Rytina found in the museums of the world" is considerably larger than the three skeletons and two ribs mentioned by Dr. Evermann. LEONHARD STEJNEGER,

U. S. National Museum, Smithsonian Institution, Washington, D.C., Feb. 7.

"Unconscious Cerebration."

SOME very puzzling psychological phenomena may be explained in simple ways by happening upon the correct point of view.

Numerous theories have been afloat to account for recollections of what had apparently never been seen before. For example, a friend of mine came across a scene in the Yellowstone, on his first visit to that region, and was astounded at the familiarity of every detail upon that occasion.

Knowing that he was addicted to fits of abstraction, I suggested that while preoccupied he had unconsciously mentally registered his surroundings and soon thereafter, without being aware of so doing, compared a conscious impression with an unconscious one.

A convincing illustration in common experience is afforded all of us when we are carefully reading a book and suddenly become aware of having turned a page or even several pages while thinking of something else all the time, and when we turn back and begin again are surprised to find that every word is familiar to us, though the reading over again was necessary to supply what otherwise might have been a gap in memory.

There may be other causes for similar instances, but the above will satisfactorily explain some cases, and simple explanations are preferable to far-fetched ones. S. V. CLEVENGER.

Chicago, Ill.

BOOK-REVIEWS.

Hereditary Genius: An Inquiry into Its Laws and Consequences. By FRANCIS GALTON, F.R.S., etc. London and New York, Macmillan & Co. 379 p. 8°. $2.50.

Finger Prints. By FRANCIS GALTON, F.R.S. London and New York, Macmillan & Co. 216 p. 8°. $2.

THE first edition of Galton's "Hereditary Genius" appeared as long ago as 1869, and that before us is the second. His observations excited considerable attention, for, although he dealt with familiar facts for the most part, his methods of analyzing and stating them were new, and the results which he arrived at were not merely unexpected, to an English public they were startling.

These results are by no means modified to a feebler expression in the present edition. A few examples will illustrate this. On page 132 he says, "I look upon the peerage as a disastrous institution, owing to its destructive effects on our valuable races." Of the Christian Church in earlier centuries he writes: "She brutalized human nature by her system of celibacy, and demoralized it by her system of persecution of the intelligent, the sincere, and the free." Nor does he allow that she is much better to-day. She keeps us "in antagonism with the essential requirements of advancing civilization," and "leads us to a dual life of barren religious sentimentalism and gross materialistic habitudes."

These severe arraignments are not the hasty attacks of a polemist, but the calm reflections of a mature student of social statistics and historic data. If they shock any one by their force, he should study the volume, and ask himself whether they are not amply justified by the array of evidence it contains. The title, "Hereditary Genius," falls singularly short of the real scope of the work. It is, in fact, a comprehensive study of the means of improving the human race through wiser arrangements for reproduction. The precepts it inculcates will convince as well as surprise the reader, and many an ancient saw is pricked and disappears like a bubble by the keen points of the author's reasoning.

Mr. Galton's "Finger Prints" is a volume made up of various essays and observations, which have engaged him for several years, on the external anatomy of the papillary ridges on the extremities of the thumb and fingers. He has found that they remain singularly individual in character through long periods of life, and thus may serve for purposes of identification. They are slightly hereditary and have little or no ethnic value. They do not appear to be correlated to mental ability, temperament, or character. The volume as a whole presents an admirable model of a closely scientific investigation of a somatologic point; and perhaps is as valuable in this respect as for any definite results reached.

The Foot-Path Way. By BRADFORD TORREY. Boston, Houghton, Mifflin, & Co.

STUDENTS of living things have not inaptly been divided into two general classes, naturalists and biologists; the former including Englishmen like Gilbert White, Thomas Edward, and Richard Jefferies, and Americans like Thoreau, Burroughs, and Bradford Torrey, who delight in studying the actions of living beings on their native heath, in the coppice beside the brook, or amid the silence of the forest. Among the biologists are found the great majority of modern students whose days are spent in the laboratory, and who care little for a living organism until it has been killed, dissected, frozen, and cut into infinitesimal slices by the microtome. Without attempting to discuss the relative merits of these two methods, it will readily be admitted that the naturalists can put into their writings much more of that humanitarian interest which gives the charm to literature. Readers of Mr. Bradford Torrey's "Birds in the Bush" and "A Rambler's Lease" will know what to expect in wandering with him along the present "Foot-Path Way." They are not likely to be disappointed. Besides glimpses of rare warblers and individual peculiarities of common birds, they will now and then see a beautiful landscape, or hear the murmur of a mountain brook, while mingled with all they will find much delightful philosophy. They will go to beautiful Franconia in June to learn

"How good life is at its best! And in such
'charmed days,
When the genius of God doth flow,'

what care we for science or the objects of science,— for grosbeak or crossbill (may the birds forgive me!) or the latest novelties in willows? I am often where fine music is played, and never without being interested; as men say, I am pleased. But at the twentieth time, it may be, something touches my ears, and I hear the music within the music; and, for the hour, I am at heaven's gate. So it is with our appreciation of natural beauty. We are always in its presence, but only on rare occasions are our eyes annointed to see it."

Besides June in Franconia, there are papers on December Out-of-Doors, Dyer's Hollow, Five Days on Mount Mansfield, A Widow and Twins, A Male Ruby-Throat, Robin Roosts, The Passing of the Birds, A Great Blue Heron, Flowers and Folks and the Weymouth Pine. The humming-bird sketches (A Widow and Twins and A Male Ruby-Throat) are peculiarly interesting, while those on The Robin Roosts and The Passing of the Birds are full of fascinating bird news.

The Testimony of Tradition. By DAVID MACRITCHIE. London, Kegan Paul, Trench, Trübner, & Co. 204 p. Illustrated.

THE writer of this volume attempts to show that the ancient Picts of Scotland were of Mongolian descent, and had come across the sea from Norway. That, so far as we know, there never were any Finns in Norway about Bergen, whence the "Finmen" are said to have come, does not trouble Mr. MacRitchie. He merely remarks that "it may be assumed" that there were (p. 35). He lays much stress on the skin boats which these early seafarers used. But the Welsh used also just such, as well as many other nations. He makes no attempt to trace any of the ancient Pictish names to Finnish radicals, though he hints that it could be done.

CALENDAR OF SOCIETIES.

Philosophical Society, Washington.

Feb. 4. — R. S. Woodward, Abstract and Discussion of Paper Read at Last Meeting; F. L. O Wadsworth, Method of Determination of the Metre in Terms of a Wave-Length of Light; Waldeman Lindgren, Two Neocene Rivers of California; H. W. Turner, Remarks on the Geology of Calaveras County, California.

The Finns, Lapps, and Eskimos, he teaches, belong to the same race — a surprising piece of information, which can scarcely also be "assumed." Still more extraordinary is the discovery, which is wholly new and wholly his own, that the colony of Swedes who settled on the Delaware River in the seventeenth century were not Swedes at all, but "Swedish Finns," and that they introduced among the Pennsylvanian colonists "plainly an infusion of unadulterated Eskimo blood!" (p. 36). This will be a startling bit of news to those worthy Philadelphians who take so much pride in their genealogies reaching back before the landing of Penn.

Seriously, the very slender basis for the whole theory is the syllable *Fin*, the same that occurs in "Fenian," "Fingal," etc., and which has evidently started the author in pursuit of this Mongolian *ignis fatuus*.

Criminology. By ARTHUR MACDONALD. With an Introduction by Dr. Cesare Lombroso. New York, Funk & Wagnalls Company. 416 p. 8°.

THE brief introduction by Dr. Lombroso (only three pages) is a defence of his favorite theory of the criminal "type," by which he means "the organicity of crime, its anatomical nature, and degenerative source." This notion was distinctly rejected by the criminal anthropologists assembled last summer in Brussels, and it is encouraging to note that this fact was not lost on Mr. MacDonald, for he tells us in his preface that "the 'type' has been considered from the psychological rather than the physical side." This is virtually giving up the position of Lombroso, which, in fact, is no longer defensible. There is absolutely no fixed correlation between anatomical structure and crime, so far as has yet been shown.

In his text, the author draws largely from well-known writers, as Lombroso, Ferri, and Corre, though he is also by no means deficient in facts from his own observation. He begins with a study of the evolution of crime, proceeds to discuss the physical and psychical sides of the criminal, his intelligence, and his associations. Criminal contagion, hypnotism, and relapse furnish topics for other chapters. Special studies of murder, theft, and meanness follow, and the volume closes with a copious and excellent "Bibliography of Crime," and a satisfactory index. The work may be recommended to all who would take up the study of this attractive and practical branch of anthropology.

Bible Studies. By HENRY WARD BEECHER. Edited by John R. Howard. New York, Fords, Howard, and Hulbert. 438 p. 8°. $1 50.

The Evolution of Christianity. By M. J. SAVAGE. Boston, G. H. Ellis. 178 p. 8°.

THESE volumes may appropriately be placed together. Both acknowledge as their main aim the widening of the religious concepts of modern Christianity, the teaching a broader, a more liberal, and more charitable construction of the tenets and the dogmas of protestant theology.

The "Bible Studies" begins with a chapter on the right understanding of the inspiration of the Bible, and follows with a series of readings and familiar comments upon them, extending from Genesis to Ruth. Beecher's admirable command of the English language needs no praise, and is well illustrated in these talks; and his position as a theologian is familiar to all American readers. Many of the passages in this book, however, sounded better than they read; they are in such colloquial style that they look frivolous.

Mr. Savage's notion of the evolution of Christianity is that it may finally evolve out of Christianity. He betrays some doubt whether it will even be called Christianity. But he is convinced that all that is best and truest in it, the love of neighbor and the faith in God, will be preserved; and that the conflict of religion with science, with free investigation and free speech, will cease. We can only say, "Soon be that day and quickly come that hour."

Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

For sale or exchange. — I have a few copies of my translation of "Strasburger's Manual of Vegetable Histology, 1887," now out of print, which I will send post-paid for $3 or for one dozen good slides illustrating plant or animal structure. Address A. B. Hervey, St. Lawrence University, Canton, N. Y.

The undersigned has the following specimens to exchange for crystals of any eastern or foreign localities or Indian relics: tin ore, metacinnabarite, stibnite, garnierite, calenanite, hanksite, ulexite, rubellite, lepidolite, blue and green onyx, Cal. pineite, aragonite on chalcedony, cinnabar, double refracting spar, clear and clouded, and others. J. R. Bush, care of General Delivery, Los Angeles, Cal.

For sale or exchange.—A private cabinet of about 200 species of fossils, well distributed geologically and geographically. Silurian, about 40; Devonian, about 50; Carboniferous, about 80; others, about 30. Frank S. Aby, State University, Iowa City, Ia.

For exchange.—Minerals, fossils, F. W. shells, land shells, native woods, Indian relics, two vols. of Smithsonian reports, odd numbers of scientific magazines, copper cents, etc., for good minerals not in my collection, good arrow- and spear-heads and natural history specimens of all kinds. Correspondence solicited with list of duplicates. G. E. Wells, Manhattan, Kan.

For sale or suitable exchange.—A spectrometer made by Fauth & Co., Washington, D. C., according to the plan of Prof. C. A. Young. This instrument is suitable for the most advanced investigations and determinations. Cost originally $700 and has been used but little. Will be disposed of at a considerable reduction. Address Department of Physics, Ohio University, Athens, O.

I will send British land and fresh-water shells in return for those of America, any part, sent to me. I have at present about fifty or sixty species, with many varieties. W. A. Gain, Tuxford, Newark, England.

The Biological Department of Hamline University desires to offer microscopic slides of animal tissues, or whole animals, in exchange for first-class fossils. Address correspondence to Henry L. Osborne, Hamline University, Hamline, Minn.

Wants.

WANTED — Second-hand. Foster's Physiology, Balfour's Comparative Embryology, Claus & Sedgwick's Zoology, Flower's Osteology of Mammalia, Vine's Physiology of Plants. Please state editions and prices asked and address Richard Lees Brampton, Ontario, Canada.

WANTED.—American Journal of Conchology, seven volumes. Parties having these for sale will please address the undersigned, stating condition and price. R. Ellsworth Call, Louisville, Ky.

A GRADUATE ENGINEER will give instruction evenings in geometry, trigonometry and surveying, mechanics, physics, mechanical drawing and general engineering construction. Five years' experience in field and editorial work on engineering journal. References furnished. C. S. H., 102 Tribune Building, New York.

WANTED.—By well-qualified and experienced science master and associate of the Royal School of Mines, London, aged 26 (at present in England), a mastership in technical college or university for any of the following subjects: Engineering sciences, geology and mineralogy, physics, chemistry and metallurgy, etc., etc. Can provide excellent references and credentials. Apply, J. G., 17 Sussex St., Rochdale, England.

A GRADUATE of the University of Pennsylvania and a practical mineralogist of twenty years' experience desires to give his services and a cabinet of 25,000 specimens, all named, with about the same number of duplicates, in minerals, crystals, rocks, gems, fossils, shells, archæological and ethnological specimens and woods to any institution desiring a fine outfit for study. The owner will increase the cabinet to 50,000 specimens in two years and will act as curator. Correspondence solicited from any scientific institution. J. W. Hortter, M.D., Ph.D., San Francisco, Cal., General P. O. Delivery.

TO THOSE INTERESTED IN SCIENCE.

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QUERY.

Can any reader of *Science* cite a case of lightning stroke in which the dissipation of a small conductor (one-sixteenth of an inch in diameter, say,) has failed to protect between two horizontal planes passing through its upper and lower ends respectively? Plenty of cases have been found which show that when the conductor is dissipated the building is not injured to the extent explained (for many of these see volumes of Philosophical Transactions at the time when lightning was attracting the attention of the Royal Society), but not an exception is yet known, although this query has been published far and wide among electricians.

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N. D. C. HODGES, 874 BROADWAY, N. Y.

SCIENCE

NEW YORK, FEBRUARY 17, 1893.

A REMARKABLE OCCURRENCE OF SELENITE.

BY DR. J. E. TALMAGE, SALT LAKE CITY, UTAH.

THE writer is pleased to report a deposit of selenite in southern Utah, which is remarkable for the size, perfection, and variety of the crystals there to be found. It is situated in the newly-created

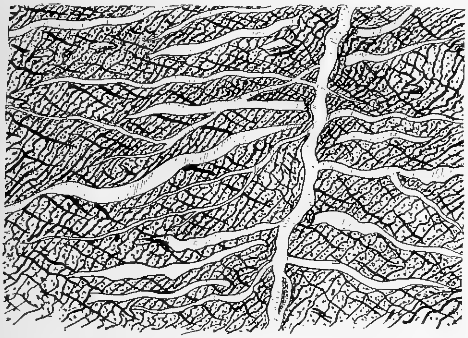

FIG. 1.

county of Wayne, in what is locally known as the South Wash, which is connected with the canyon of the Fremont River, and this in turn is tributary to the Colorado.

The formation in the neighborhood of the deposit in question is mostly sandstone and argillite, with a top dressing of erratic boulders of lava. Innumerable fantastic forms in stone declare the cutting power of water and wind; indeed, the entire region has been the site of wonderful eroding action. Ripple marks in great distinctness are frequent in the sandstone of this region and other evidences of lake formation are common.

The most convenient way to reach the deposit from the north is by way of either the Grand Wash or the Capitol Wash, spurs of the Fremont Canyon, both of which abound in scenes which are terribly grand. As one leaves the deep canyons, however, and enters the side washes, the scenery assumes a milder, though a scarcely less diversified, character.

Here and there along the gorges are outcroppings of gypsum, varying in degrees of purity; and seams of this material cut through the country rock in all directions. In places, veins of satin spar, as thin as a sheet of note-paper, or even an inch in thickness, can be traced for many hundreds of yards upon the surface of the ground in uninterrupted course, except for intersecting planes of the same material. On the walls of the ravines and canyons places are seen where spar veins cross and recross each other with bewildering profusion. Here (Fig. 1) is a sketch of such seams in an exposed face eight by twelve feet on the steep side of a ravine.

Gypsum in all varieties may be found within a short radius, fibrous and scaly laminæ, plaster-stone or rock-gypsum in masses, lumps of pure alabaster, and fragments of selenite crystals are scattered along the washes and strewn upon the bench-lands, as they have been left by the fierce floods which tore them loose from the place of formation. These occurrences form an encouraging introduction to the superb deposit of crystals already mentioned.

The crystals occur in a cave, and this is inclosed by a thick shell forming a mound which stands in relief on the side of a hill bounding the Wash. Of this formation, a good idea may be gained from Fig. 2, which is reproduced from a photograph. The mound is somewhat of an egg-shape, 35 feet in length east and west, 10 feet in breadth, and of an average height of 20 feet from the ground on the lower side; all outside measurements. This selenite mass seems to have been left exposed by the weathering of the loosened friable sand and clay, of which the hill whereon the mound is situated is composed. The mound consists entirely of selenite, the outside having a somewhat battered and roughened appearance from the action of the wind-driven sand; yet the whole exterior is made up of the exposed ends and sides of crystals, and in the sunlight the formation glistens with indescribable beauty. The outer walls are generally regular, though there are a few depressions and sheltered niches, within which small prisms of selenite nestle snugly, in groups.

The entrance to the cavern faces the east, and when first observed by the writer it was about six feet in height, and three and a half in width. The cave can be travered to a depth of 26 feet. Generally the crystals project from either side toward the central line of the cavern, approaching each other within about three feet, though some of the largest crystals extend entirely across the cavern like huge beams.

Fig. 3 is from a photograph of the interior of the cave, one massive crystal having been sawn off to afford a better view. The floor of the cavern consists mostly of sand, probably deposited by water in flood times, and carried in at all seasons by winds. Projecting out of the sandy floor are the terminations of many superb crystals. Inside the cavern, a yard from the entrance, the crystals descend within three feet of the bottom, so that one has to

FIG. 2.

stoop to pass; but farther in there is room to stand erect, and near the back wall a person may clamber up to a height of fifteen feet. Looking upward from the bottom of the cavern, one sees a mass of mammoth prisms, suggesting, but for their singular beauty, the heavy timbers of a deep mine. The entire deposit is a colossal group of crystals, the like of which is seldom to be seen.

The writer's attention was first attracted to the place through receiving several small specimens of the selenite from sheep-herders, who had discovered the deposit while searching for feeding-places, and who claimed to have found a mine of mica, which they called "isinglass." Their disgust was great when assured, by the conclusive experiment of holding a bit of the material in the flame of a candle, that the stuff was not what it seemed. I first visited the place in April last, and my rapture at the superb display of crystal beauty was checked by the evidences of vandalism on every hand. Some of the finest crystals had been hacked and carved, and cow-boys' initials were scratched and cut on almost every prismatic face which the light could reach. Visiting the place again six months later, I found that still greater destruction had been waged, and, becoming convinced that good crystals would soon be difficult to obtain, I took steps to secure legal claim to the land, and proceeded to remove the remaining crystals of greatest value to a place of safety. Under the auspices of the Deseret Museum of Salt Lake City, the work of removal is still in progress. Already over twenty tons of most beautiful crystals have been taken out and shipped to this city.

Fig. 3.

Prisms of perfect form and varying in length from one to five feet, and in weight from ten to one hundred pounds, are of frequent occurrence. One of the most regular yet taken out is four feet long, and the widest faces are six inches across. Cleaved slabs are obtainable six feet in length, and two and a half feet in breadth. One of the longest perfect prisms yet obtained extends fifty-one inches, and from one of its faces nineteen smaller crystals sprout. Twins are common, as are also compound terminations of very complicated structure. A magnificent group, weighing over six hundred pounds, was removed from the floor of the cavern; it was set up on the outside and photographed (see Fig. 4).

As to the habit of the crystals, in the midst of such variety it is difficult to specify. Prisms short and stout, also long and comparatively slender, are numerous; and of twins, the "swallow-tail" vie with the cruciform and penetration varieties in points of abundance and perfection. Some of the crystals are of perfect transparency, and cleaved slabs of this quality are common. Sometimes the prisms inclose sand and clay, which is so distributed as really to add to the beauty of the crystals in the eyes of all save the mineralogist. When fracture planes are made visible by striking a crystal containing such impurities, the particles appear on the internal planes as on shelves of glass.

Some of the finest specimens will probably be on exhibition in Chicago next summer.

THE FUTURE OHM, AMPERE, AND VOLT.

BY HENRY S. CARHART, ANN ARBOR MICH

Since the International Congress of Electricians in Paris in 1881, the most eminent physicists have been agreed as to the theoretical values to be assigned to the three fundamental units of electrical measurement; but it has been a matter of ten years' labor on the part of many distinguished investigators to embody these theoretical definitions in practical units for universal use.

Up to the date mentioned the two units of resistance in use were the British Association (B.A.) unit and the Siemens unit. Only the former represented an attempt to construct an ohm corresponding to the theoretical definition. The B.A. unit has served a useful purpose, but it is now known to be 1.34 per cent too small.

The "legal ohm" was provisionally adopted in 1883 by an international committee to which the Congress of 1881 had committed the subject. It was in the nature of a compromise, and fixed the practical ohm as the resistance at 0° C. of a column of mercury one square millimeter in cross-section and 106 centimeters long. Competent investigators, like Lord Rayleigh and Professor Mascart, contended that a column 106 3 centimeters in length was nearer the true value; but a few smaller values obtained by some well-known physicists decided the adoption of the mean value 106 centimeters. This conclusion satisfied no one, and the "legal ohm" was never legally or officially adopted by any European or American government.

Fig. 4.

Subsequently, Professor Rowland came forward with his determination of 106.32, and errors were found in the data of some who had contended for the lower values. Hence the number 106.3 has been tacitly accepted for two or three years already, and it is now believed that this does not differ from the true value by more that two units in the fifth figure; that is, the length of the mercurial column representing the true ohm is not less than 106.28 and not more than 106.32 centimeters.

Somewhat over two years ago a commission was appointed by the British Board of Trade to draft an "Order in Council" as a legal settlement of the units to be employed by the Board of Trade Electrical Bureau, and hence as the legal electrical units for Great

Britain. After this committee had made its report, but before the "Order in Council" had been signed by the Queen, an intimation was received from Professor von Helmholtz that something might be done toward international agreement if the order were delayed till he could communicate in person the results of the most recent determinations in Berlin. Accordingly von Helmholtz and some others were invited to be present at the British Association last August, and to sit with the famous B.A. "committee appointed for the purpose of constructing and issuing practical standards for use in electrical measurements." The report of the committee, recently published, says: "During the Edinburgh meeting the committee were honored with the presence of Dr. von Helmholtz, M. Guillaume of Paris, Professor Carhart of the United States Dr. Lindeck and Dr. Kahle of the Berlin Reichsanstalt. These gentlemen came by invitation to consider the question of establishing identical electrical standards in various countries." The committee had two long sessions, and there were present Professor Carey Foster, chairman, Lord Kelvin, Professors Ayrton, Perry, and Sylvanus Thompson, Dr. Oliver Lodge, Mr. Glazebrook, secretary, Mr. Preece of the Post-Office, Major Cardew of the Board of Trade Bureau, and others.

The most important results of these conferences were the abandonment of the time-honored B.A. unit, the disregard of the "legal" ohm, and the adoption of the mercury standard of 106.3 centimeters. The reports from Berlin and Paris showed most conclusively that mercurial standards, set up with the precautions recently adopted, agree with surprising accuracy. The uncertainty of the relation between the centimeter and the gramme was avoided by defining the mass of the mercury column of 106.3 centimeters in length, which has a resistance of one ohm. It is 14.4521 grammes. This corresponds to a specific gravity for mercury of 13.5956. "In reality the square-millimeter cross-section remains the elementary definition, but with the specification that this is arrived at by mercurial weighings."

Standards of resistance for industrial purposes in solid metal will still be made as heretofore. But it must be borne in mind that such resistances, especially when made of alloys, should be kept at a temperature near the one at which they have been standardized; otherwise small changes take place in the resistance, due perhaps to a kind of annealing and recrystallizing process.

It was further agreed with regard to the unit of current that the number 0.001118 should be adopted as the number of grammes of silver deposited per second from a neutral solution of nitrate of silver by a current of one ampere. This corresponds to 4.025 grammes per hour. The silver voltameter, with the proper manipulation, becomes, therefore, a secondary standard for the determination of the unit current.

The electromotive force of the Clark standard cell has been re-determined both in Berlin and Cambridge, England, within a year or two; and the results are in rather surprising agreement. A comparison of these determinations led the B.A committee to decide upon 1.434 as the number of volts representing the electromotive force of the old form of Clark cell at 15° C. containing a saturated solution of zinc sulphate and crystals in excess. This is .001 volt lower than the value heretofore assigned to this cell. It was not determined to adopt this form of cell as the standard, but only to decide upon its voltage when set up by competent persons in accordance with certain specific directions. My own form of Clark cell is perfectly portable, has an electromotive force of 1.44 volts at 15° C., and its change of electromotive force with temperature is only half as great as that of the old Clark cell containing crystals.

We have as yet in this country no bureau where concrete standards of resistance and standard instruments for other electrical units are preserved. Such a bureau, under federal control, is greatly to be desired. Germany has its Reichsanstalt, under the direction of von Helmholtz, in Berlin; England has not only the standards of the British Association in the keeping of Mr. Glazebrook at Cambridge, but also the Board of Trade Bureau in London, under the directorship of Major Cardew. Mr. Glazebrook undertakes the comparison and certification of standard coils for the English-speaking world while the bureau in London issues certificates of instruments for commercial purposes in Great Britain.

Government bureaus mean certified standards and legally adopted units. The decisions of the B.A. committee last August were with the full concurrence of Professor von Helmholtz, and it is understood that the German government will adopt the B.A. proposals. The committee appointed by the Board of Trade in London has already made its supplementary report in accordance with the conclusions of the B.A. committee, and these units will doubtless very soon acquire a legal character in England. The coming electrical congress in Chicago will afford an opportunity for official delegates to adopt these same units for their respective countries, and official ratification will then naturally follow. Lord Kelvin (Sir Wm. Thomson) predicted at the close of the Edinburgh meeting that the system of units adopted by the B.A. committee will become thoroughly international. It should be the duty and pleasure of all electricians to contribute toward this result.

THE CLASSIFICATION AND NAMING OF IGNEOUS ROCKS.

BY W. S. BAILEY, WATERVILLE, ME.

THE discussions of Mr. Iddings[1] relating to the crystallization of lava have led him to conclusions that will undoubtedly prove of vast significance in the attempt to ground the study of rocks in a firm and sure foundation. Heretofore, most petrographers have busied themselves with descriptions of rock-types, confining their discussions principally to the mineralogical composition and the structure of the specimens studied, and to their similarity to other specimens assumed as types. Such work as this is of course absolutely necessary to the right treatment of rock classification. It is evident that we must first know the characteristics of bodies to be classified before we can hope to separate them into genetic groups. But the time has now come when students of rocks must seek for a generalization that will do for their science what the atomic theory has done for chemistry or the theory of evolution for the biological sciences, viz., elevate petrography from the position of a descriptive science to that of a philosophical one. Mr. Iddings's recent studies in the causes producing the differences noted in different lavas emanating from the same volcanic centre, and the generalizations drawn from them, will go far toward affording a philosophical basis for rock classification, and, consequently, toward the inception of a broader study of rocks in their relationships to each other than has heretofore been possible.

The rocks on the surface of the earth all tend toward the production of a few simple types, in which tendency may be traced the action of chemical laws, under the definite conditions existing at the surface, producing from unstable compounds those that are most stable under these conditions.

Mr. Iddings believes that the relation existing between chemical action and the conditions under which it occurs is discoverable not only in the breaking down (degradation) of rocks, but also in their construction. He believes that the intimate gradations in composition and structure that are known to exist between the types of eruptive rocks are due to the action of chemical laws under changing but definite conditions — conditions that are determined largely by the position of the magmas from which the rocks are derived. If this be true, petrographers have at last a thread to which they can tie the results of their investigations: they have offered them a conception as to the *cause* of the existence of eruptive rock-types, whose discussion *pro* and *con* will compel them to study not simply rock-specimens, but rather rock-masses, in the attempt to learn just what relations exist between their various parts, with respect to composition and structure, and to discover the conditions under which these parts were formed. In other words, *petrography*, as the result of this discussion, will become *petrology*, just as "natural history" has become "biology."

[1] J. P. Iddings, The Origin of Igneous Rocks, Bull. Philos. Soc., Washington, vol xii, 1892, p. 89.

It is not the theory of a science which urges the progress of that science, but it is the attempt to discover whether or not the suggested theory will explain the facts of the science, that leads to the latter's rapid development. The suggestion of the atomic theory demanded its discussion, and it was this discussion that advanced chemistry to the position it now occupies among the exact sciences. The theory of evolution did not by any means explain away the difficulty of accounting for the existence of many species of living things, but it was the attempt to discover whether the theory is founded on a secure basis or not, that has led to the wonderful progress of biology within the past quarter of a century. So the mere suggestion of Mr. Iddings's theory as to the origin of eruptive rocks, because of its comprehensiveness, is bound to lead to discussion that will in the end give us a conception of the cause of the almost infinite variety among these bodies more simple than any other conception that has thus far been held.

Mr. Iddings was highly favored in the beginning[2] of his studies by the opportunity afforded him of comparing the deep-seated portions of a series of rocks with their surface equivalents. Electric Peak and Sepulchre Mountain, in the Yellowstone National Park, are separated from each other by a great fault, in consequence of which the intrusive stock and its apophyses of Electric Peak are brought to the same horizon with the dykes and surface-flows of Sepulchre Mountain.

Upon comparing the Electric Peak intrusives with the Sepulchre Mountain effusives, it was found that, although each group comprehends a complex series of rock-types, the two groups have, on the whole, a striking similarity in composition. Certain characteristic minerals found in the intrusives are also common in the effusives. Moreover, the transition between the members of each series is so very gradual that it is impossible to draw any sharp line between the different types. These facts indicate the existence of a close relationship between the typical intrusives of Electric Peak and the typical effusives of Sepulchre Mountain, and a unity of origin for the members of each series, with a gradual change in the conditions under which the different members were formed. Though the individual members of the effusives differ markedly in structure from the members of the intrusive group, the two groups are regarded as having resulted from the cooling of what was originally one mass of magma, but which, in consequence of a differentiation of its parts, became separated into various magmas differing in composition. The differentiated magmas, upon their extrusion from the depths, consolidated as widely differing rocks, either of the intrusive type, or of the effusive type, according as the magmas cooled beneath the surface or upon it.

Examination of other regions of eruptive rocks reveals the same relationship existing between the various rock-types occurring in them. There is a more or less striking similarity in some respects between all types occurring within a region covered by rocks extruded from a single centre, and a marked difference between these and the series of rocks of other regions. Thus the rocks of a single eruptive centre are more closely related to each other than to similar mineralogical aggregates originating at a different centre, or, as Mr. Iddings expresses it, the rocks of a single centre are consanguinous.

No matter how different in mineralogical composition and in structure, all the products of a given centre — consanguinous products — should be grouped together in a classification of rocks, rather than rocks of similar mineralogical composition and similar structure from widely separated regions of volcanic activity. The differences in structure and mineralogical composition of consanguinous rocks are the result of the differentiation of the magma from which they were derived, together with differences in the conditions under which the differentiated parts of this magma were cooled. Their chemical peculiarities are the direct result of the chemical nature of the homogeneous magma before its differentiation into parts. If this notion is correct, the succession of products originating during the course of a volcanic extrusion should be "from a rock of average composition through siliceous and more siliceous ones to rocks extremely low in silica and others extremely high in silica, that is, the series commences with a mean and ends with extremes."

It will be the endeavor to discover whether this law of succession expresses the facts in the case or not, that will advance the science of petrography to that of petrology. If Mr. Innings's law of succession is found to hold, the future classification of rocks will be based upon the principle of consanguinity; there will be grouped in the same great division types of different mineralogical composition and of different structure, while the different great divisions will be based primarily upon chemical considerations. What these chemical considerations are to be it is difficult at present to foresee.

Whatever may be the future classification of rocks, however, it is quite certain that petrographers are in the main right in distinguishing between rocks of different structures and different mineralogical composition by different names. There is a fashionable tendency apparent among English and American petrographers to decry the habit of naming these slight differences, not because the number of rock-types in nature is in reality small, but simply because the terminology of petrography by the addition of these names becomes large — as if we could increase the simplicity of the science by refusing names to the objects of whose study it consists. The same tendency has been observed also in the history of chemistry. Some inorganic chemists have objected seriously to the introduction of the many new terms into organic chemistry, and yet nothing has done more to advance this particular phase of the science than its system of nomenclature. It is easily understood why *geologists* should object to the increase in rock names, since this increase necessitates a greater amount of labor upon their part in becoming acquainted with the terms. But why *petrographers* should object to a more accurate designation of the objects of their study is not understandable. It would seem to the writer that for petrographical purposes every rock-type that differs in some one essential feature from all other rock-types should receive a distinctive name, in order that its differences might be emphasized. If all the types with major characteristics in common should be grouped under the same name, we should lose sight entirely of their minor characteristics that may be exceedingly important as throwing light on the relation of composition and structure to the conditions under which the rocks were formed. Again, it is much more convenient to speak of a keratophyre than of a "granophyric granite differing from ordinary granophyre in the possession of anorthoclase instead of orthoclase." This difference between keratophyre and granophyre, though of insignificant importance from the point of view of the geologist, ought to be of considerable importance to the specialist in rocks. It may express simply a difference in the original constitution of the magma from which the rock was formed, or it may be the expression of peculiar conditions under which solidification took place. In either case the difference is of importance and should be emphasized.

It would appear that the difficulty to the geologist of acquainting himself with the complete terminology of petrography might be avoided by grouping rocks in accordance with their chemical composition and structural *similarities*, and by dividing the groups according to the *differences* prevailing among their members. Geologists need take account merely of the great groups, while petrographers would require to become acquainted with their subdivisions.

In denying the necessity of expressing in their names the comparatively slight differences noted between many rocks, it will not do to say that petrography is simply a branch of geology and that there is no room for the study apart from geology. The methods of petrography are entirely different from those of geology; in many cases they are as different from those of the last-named science as are those of paleontology. Petrography is the special science dealing with some of the materials of geology. Unless it is recognized as distinct from geology it will never become of the importance that it will otherwise assume, and cannot aid geology as it should do. If it be regarded as something worthy of study for its own sake, then it is necessary to label the objects of its study so that they may be handled conveniently,

[2] The Eruptive Rocks of Electric Peak and Sepulchre Mountain, Yellowstone National Park. 12th Ann. Rep. Director U. S. Geol. Survey, p. 569.

and it is advisable to express as much in the labels as may suffice for a pretty complete knowledge of the objects labeled. If this notion is a correct one, let us welcome the designation of *differences* in rocks by their names, and not seek to lose sight of these differences in contemplating simply likenesses. On the other hand, it is well to exercise care in the selection of types to be named, so as to avoid as far as possible the lumbering of the terminology with needless expressions. Discrimination must, of course, be exercised in the naming of types, and experience must decide as to the value of any proposed name. The writer would prefer that the *varietal* names should be based upon mineralogical composition, and that adjectives should express the structural *differences*, where the structure of the variety departs from the characteristic structure of the group.

CLOUD CLASSIFICATION.

BY CAPT. DAVID WILSON-BARKER, R N., H.M.S. "WORCESTER," ENGLAND.

FOR some years meteorologists have been in doubt as to the nomenclature of clouds, greatly to the retardment of this important and practical branch of the science. The nomenclature of Luke Howard answered very well for a time, but with our advanced knowledge it scarcely answers at all. It is not simple enough for beginners, nor elaborate enough for those well advanced. Many of the systems proposed lately are simply modifications of this old nomenclature, and retain its faults. Unfortunately, in cloud classification we are met with many difficulties at the outset, we cannot collect and label clouds in a cabinet for reference, but here photography may aid us much. From personal experience it has been found quite possible to portray even the most delicate and fleecy clouds with sufficient accuracy to leave no doubt as to their type. It is proposed in this article to lay before the readers of *Science* a simple scheme of cloud nomenclature suitable for beginners and those unable to devote much time to the study. On this simple scheme can be founded a more elaborate system for skilled nephologists.

It will soon strike any one who notices weather phenomena ever so casually, that clouds have a tendency to assume one of two well-known forms or shapes, either a heapy or globular form, or that of thin sheets or layers. Clouds in the first form are known as cumulus (cumulus, a heap) clouds. In the second as stratus (stratus, a layer) clouds. Once it is clearly understood that all clouds be divided into these two types as a starting-point, and belong to one or other of these types, the question of a minute sub-division becomes, comparatively speaking, easy.

It may be well to give here a cloud definition. *A cloud is vapor, which has ascended or descended in the atmosphere from a position having a temperature or density greater than the portion of the atmosphere it ascends or descends to, which is then unable to retain it in its invisible form. According to the physical state of the position it is attracted to, so will be the form it will assume on becoming condensed.* It will be seen from this that the shape of a cloud is more or less determined by its physical surroundings, and consequently it affords a valuable index, not only to the state of the immediately surrounding atmosphere, but also to the weather we may expect, and this frequently some time before any instrumental warnings are indicated.

Cumulus is essentially the cloud of the lower atmosphere, as, although it sometimes tops to great altitudes, yet its formation commences at a, comparatively speaking, low level. Cumulus clouds assume varied and fantastic shapes, and vary very often from clouds of enormous extent to small nubecules, still there is in them a distinct and marked similarity, which must be easily recognized. There are three forms of cumulus clouds from which rain falls, viz.: 1. Bold, massive cumulus with feathery tops, which appear to be composed of ice crystals, and are like the high variety of stratus known as cirrus; 2. bold, massive cumulus with all clearly defined borders, only seen in the tropics; 3. fleecy, ill-defined cumulus. The first may be accompanied by either snow, hail, or rain, with a decided increase of wind, and, in fact, is a squall, which often gives warning hours before it reaches the observer. In the second is heavy rain with little increase of wind-force, and at sea is the kind of cloud which sometimes accompanies waterspouts; and the last has only drizzling rain and no increase in wind-force.

Stratus is formed in all layers of the atmosphere. On the ground it is fog, in the lower atmosphere as covering the sky oftentimes for days in anticyclone areas; in the middle layers in broken-up or more or less circular patches constantly, though erroneously, called cirro-cumulus or cumulo-cirrus, and in the highest layers as the well-known cirrus or curl-cloud. It is the cloud of the finest settled weather, and also of the front of cyclonic disturbances, but there can be no mistaking these two conditions. In the former case, it forms a pall over the whole sky, perhaps broken here and there by a rift, through which a blue sky, quite free from other clouds, may be seen, and appearing in all directions in lines parallel to the horizon. The first sign of any change is preceded by the disappearance of this cloud, and the formation of fine threads of cirrus over the sky; these threads gradually grow closer and closer together until the sun or moon shines through surrounded by a halo. As the cloud gets thicker (seems to grow in the air) this too disappears, rain begins to fall, and a cyclonic disturbance is well under way. In the first case the stratus was in the form of a cloud of great superficial extent and small depth, in the second it has great depth and uniformity of texture.

Cloud observing is a difficult branch of meteorology, yet no great advances can be made in the physics of the atmosphere until we have a better knowledge of its movements, and this article is written in the hope that those interested in the subject may not be appalled by the apparently hopeless condition of cloud nomenclature. For if we could have a series of observations taken carefully on even this simple basis, they would be of more value than the majority of observations taken now; and this especially applies to observations at sea, as it is to the sea we must look for the most valuable meteorological observations. Personal experience has shown that observers, while finding it comparatively easy to distinguish between cumuliform clouds and stratiform clouds and the different altitudes at which they float, yet often make great mistakes when they have to deal with the subdivisions as they are at present determined.

NOTES AND NEWS.

FIVE lectures on anthropology are to be given on Monday afternoons by Daniel G. Brinton, M.D., LL.D., at the Philadelphia Academy of Natural Sciences, admission free. Tickets can be obtained at the Academy from Dr. E. J. Nolan, secretary. Feb. 13, The Bonds of Social Life; Feb. 20, The Growth of the Arts; Feb. 27, The Progress of Religions; Mar. 6, Language and Literature; Mar. 13, Folk-Lore, or the Past in the Present.

— The Royal Academy of Sciences of Turin announces that the ninth Bressa Prize, consisting of 10,416 francs, will be awarded to any scientific author or discoverer who, during the years 1891–94, shall, in the judgment of the Academy, have made the most important or useful discovery or published the most valuable work on physical and experimental science, natural history, mathematics, chemistry, physiology, and pathology, as well as geology, history, geography, and statistics.

— From the American Book Company we have received the four latest volumes of their English Classics for Schools. They are: "Ivanhoe," by Sir Walter Scott (484 pages, 50 cents); "Julius Cæsar," by Shakespeare (114 pages, 20 cents); "Ten Selections from the Sketch-Book," from Washington Irving (149 pages, 20 cents); and "The Sir Roger de Coverley Papers," from the *Spectator*, by Addison, Steele, and Budgell (148 pages, 20 cents). The first-named volume is provided with a serviceable glossary, and all are well printed, on good paper, and are neatly bound.

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Attention is called to the "Wants" column. It is invaluable to those who use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The "Exchange" column is likewise open.

THE MIOCENE GROUP OF ALABAMA.[1]

BY LAWRENCE C. JOHNSON.

FIVE or six years ago it was doubtful if the Gulf States had any well-defined Miocene. The brilliant discoveries of Mr. D. W. Langdon, at Chattahoochee and at Alum Bluff on the Apalachicola, set that question at rest for Florida; still there was general doubt of any extension of the same into Alabama.

Dr. Eug. W. Hilgard had defined and located the Grand-Gulf Group in Mississippi, and conjectured it might be Miocene. But he was not perfectly satisfied, and for want of fossils no paleontologist would undertake a decision. This also it would have been bold a few years ago to say had any continuation into Alabama.

Whilst at work for the U. S. Geol. Survey (1889), the Grand Gulf was explored, to a considerable extent, in Louisiana, Mississippi, and Alabama.

Dr. Hilgard, for Mississippi, divided it lithologically into two phases: the first well seen at Grand Gulf, in which quartzites prevail; the second, most abundant, has the peculiar, characteristic silicious clay-stones, found in such masses nowhere outside of this group.

It was the fortune of the writer to observe two other phases in Mississippi:

(1) The quartzitic phase — being only a phase of the next — roughly estimated, extends from the north-west corner of the formation on Big Black River, to a curved line drawn across from Rodney to Pelahatchie. It is most largely developed on Bayou Pierre and Cole's Creek. For convenience it may be called the "Bayou Pierre Phase."

(2) The second, having very irregular boundaries, may have its southern line drawn from Tunica, in Louisiana, by Columbia, Miss., by the mouth of Okatoma Creek, by the Falls on Leaf River near Estabuchie, passing to the southward of Ellisville and crossing Chickasawhay River between Winchester and Waynesboro. For convenience this division will be called the Fort Adams, or the Ellisville phase.

(3) More remote from the Great River, and southing farther, the less silicious the formation becomes, at Hattiesburg, and on that part of Leaf River from Okatoma to Rogers' Creek, and on Chickasawhay above Leakesville, a third phase is exhibited, abounding in phytogene remains — almost lignitic. This is the Hattiesburg phase or formation.

(4) A fourth manifests itself below Leakesville on the Chickasawhay, on the Lower Leaf River and Pascagoula — being clays of a more tenaceous quality — abounding in specks and nodules of calcareous material, and in a few places holding shells of mollusks. One locality of the last, where first discovered, is the Shell Landing below Roberts' Bluff, four miles south-west of Vernal postoffice. This is the Pascagoula phase or formation.

The last three extend into Alabama, though the fourth lies so deep under the great ridge of sands of Mobile County that no overland outcrop has yet been discovered. In the deep boring at Mobile it was reached at about 600 feet. The shells of the boring have been pronounced by Dr. Dall identical with those of Pascagoula, and a list of them furnished by him in this journal, Sept. 16, 1892.

The second and third phases have been traced across Alabama. The second is finely developed at Healing Springs, in the northern part of Washington County. Briar Creek is about the boundary between it and the Eocene, and southward of that it is frequently exposed by the washing away of the surface sands, as far as the head-waters of Bilbo Creek.

The southern part of Washington is underlaid by the Hattiesburg — the third phase. Many of the shallow ponds and cold clay flats are to be accounted for by this fact; and so are the ridges of better soil. Lignitic spots, coming to the surface where drainage is sufficient, have weathered into limited areas of better soil. Such a spot is on the southern branches of Basset Creek, on the St. Stephens and State Line road.

Baldwin and Escambia counties afford a continuation of these parallel lines of silicious clay-stones, of ponds, and of cold clay flats — not without places of better soil. This is the true origin of the well-known strip of red lands on the high ridges northward of Williams and Canoe stations — up West Escambia.

Finally, in the vicinity of Brewton, Burnt Corn, Murder Creek, and Conecuh River, expose sufficient of these three older phases of the Grand Gulf as to leave no doubt with regard to the horizon to which it properly belongs.

Chalk Hill, at N. B. Dixon's (Sec. 1, T. 2, N. of R., 13 E), is a repetition of Chalk Hill at Healing Springs, while the hills near Castleberry on Murder Creek (Sec. 1, T. 2, N. of R., 10 E.), and the exposures on Conecuh at Silas Bluff (Sec. 6 and 7, T. 1, N. R., 12 E.), at Coal Bluff (Sec. 7, T. 1, N. R., 11 E.), and at Roberts, Silas Creek (Sec. 5, T. 1, R. 12 E.) display the Hattiesburg phytogene phase as fairly as Augusta on Leaf River,— with the addition of molluscan fossils not found in Mississippi. These are only in casts, true: because the clay-sandy material, without lime, was too porous to retain calcareous shells.

Of the precise type and horizon of these fossils we are not left in doubt; but to marshal our testimony on the subject, it will be necessary to step across the line, and to bring forward by continuity the Miocene Marls of Florida, to wed these Grand-Gulf clays of Escambia.

II. Wakulla Springs, in the county of the same name, has a peculiar limestone, which is found from St. Mark's Bay into Georgia. Generally fossils are few. The great coral, *Astræa Floridana* or *belea*, and *Orbitolites Floridana*, ever present in these warm sea-formations, are among them. In the deep excavation at Weelaunee, Jefferson County, and the Bloxham well near Tallahassee, fossils are more numerous and of greater variety.

On the Chattahoochee River, at Rock Island, between Port Jackson and the mouth of Flint River, the Wakulla rocks again manifest themselves, and upon the Eocene Vicksburg limestones of Jackson County. Below the mouth of Flint, at the village Chattahoochee, and forming the left bank of the Apalachicola to Aspalaga landing — ten miles — is another phase of the older Miocene.

On Chipola River, at and above Chipola postoffice, is another phase of the older Miocene — the Chipola beds. These lie in and upon the Chattahoochee more silicious form, in which the fossils are very obscure. In the more calcareous ferruginous deposits of Chipola, Farley Creek, and Ten-mile Creek, they are the best preserved in the world. Even the Orbitolites are perfect, instead of a mere impression. Neither of these formations can be said to cross the Choctawhatchee River, westward. The obscurely fossiliferous sandy rocks of Econfina, and at Douglas Ferry on the Choctawhatchee, may be assigned to the Chattahoochee. But west of that great river the territory which should be occu-

[1] Recent investigations made by Lawrence C. Johnson, for the Geological Survey of Alabama, published in advance of the General Report, by permission of Dr. Eug. A. Smith, the Director.

pied by the older Miocene, is usurped by a long synclinal valley, which is overwhelmed by deep sands. Upon the maps this syncline is easily followed by Sandy Creek and the upper waters of Shoal River. From the bend of the latter the syncline crosses to the Yellow River and takes the whole valley of Black-water north of Otabeite.

The great roll, or anticline, forming the southern and western limits of this syncline, may be easily followed westward from Alum Bluff, by Abes Spring — crossing Choctawhatchee at Knox Hill — and forms the high ridge upon which Defuniak is located. It is cut through by Shoal River at the high hills or bluffs between the Wise Bridge and Christmas Bluff, and by Yellow River at Hickman Bluff, above the railroad, and subsiding below the same southward to Daw's Bluff and northward to Oak Grove, whence fifteen or twenty miles of the river is in the trough of the syncline, the very region where the older Miocene should be found, if continued west of Econfina River.

Walton County, West Florida, is traversed by some considerable streams, which have removed in places the superficial Orange Sands, and cut deep into fossiliferous beds, on Yellow River, Shoal River (lower part), Alaqua, and Blairs Creek or Euchee.

Christmas Bluff — an almost inaccessible locality in the vicinity of Taylor's Mill, to the north of Mossy Head, is perhaps the best for the amount of the exposure — there being sixty feet from the water to the Orange Sand of the ridge above — twenty feet of the top being calcareous and full of the finest shells. The lower forty feet of the bluff seem to be without fossils, at least none were discovered at the brief visit of 1889. The compact sands of this lower part of the bluff strongly resembled those at the base of Silas Bluff on Conecuh River, and still more the sands at the head of Ten-mile Creek, and on Econfina in Calhoun County, Fla. In the last, however, were impressions of fossils, which connected it with the Chattahoochee Formation.

At no one of the many localities exposing fossils could all the phases of these formations be seen. Alum Bluff comes the nearest to this requirement; yet it is best to study them where more widely parted and where each may have scope for a grander dis play.

Such typical localities may be found on the two neighboring creeks, Alaqua and Euchee, south of and very near to Defuniak Springs.

The phase seen near Euchee Ana consists of a sandy ferruginous clay, calcareous in spots, having innumerable shells and casts of the small *Mactra congesta*, mixed with or finished off at the top with a good deal of silicified wood and lignitic matter. This is the counterpart of the topmost layers at Alum Bluff and of the lowest bluff at Abes Spring on Chipola.

The Alaqua phase, on the other hand, has the larger shells — *Conchs Cardiums, Arcas Pectunculus*, etc., in good variety, ard a fine state of preservation. This is the formation of the upper bluffs at Abes Spring. In fact, Chipola River affords the finest opportunity for the study of all the phases of the Miocene with exception perhaps of the Wakulla rocks. There is at the Abe Spring lowest bluff more than 100 feet longitudinally of the Euchee phase, above that more than a mile of the Alaqua phase, culminating in a 60-foot bluff at the Darling Slide, and above that for eight miles are the Red Ortholax beds, and gray allied calcareous sands up to "Look and Trimble" shoals of the more indurated Chattahoochee form.

The Alaqua phase the writer learned from Mr. N. H. Darton, whom he met in Florida soon after the discovery, to regard as equivalent to his Chesapeake. The more complete studies of Dr. W. H. Dall, it appears, led him later to the same conclusion.

The younger Miocene, of the Alaqua type at least, is perfectly and largely developed on the bluffs of Yellow River, from the Alabama line to Milligen in Florida, the most northern of these beds being the low shell landing at Oak Grove, six miles south of the line.

Twenty miles north-west of Oak Grove, and across the deep sands of the synclinal valley of Black Water, the great anticlinal roll reaches Conecuh River. At Roberts, on the head of Silas Creek, the bluff washed out by the waste-way of the mill is filled with casts of the same Alaqua fossils. The clay and wood and lignitic matter of the upper part of this wash-out bluff seem identical with Coal Bluff, six miles to the south-west. No fossils are found at the Silas Bluff, though so near; and there is no reason to doubt that the lower strata of Silas are identical with the outcrop at Dixon's Chalk Hill, six miles further north, and 100 feet higher hypsometrically,— that is the same as the older underlying Grand Gulf quartzitic clays and rocks,— neither should it be doubted that these are equivalent to the calcareous clays of the Chattahoochee formation — in time. That these in precise mode and form did not pass this far west is in perfect harmony and accord with the geological history of the region.

III. In time of the older Miocene, all of Florida above water was an archipelago of small Eocene islands, located where now are the counties Suwanee, Fayette, Columbia, Alachua, Levy, Marion, Hernando, Citeus, Pasco, and Sumter,— or rather parts of them, — and there were probably a comparatively deep strait and strong current between them and the Eocene rocks of the same age in Georgia. Dr. Dall has shown by the fossils that this channel of the older Miocene period was a warm-water sea.

In this warm-water channel was laid the Wakulla rocks of the county of that name, of Jefferson, of Leon, and of Jackson, underlying the Chattahoochee beds.

Now observe upon a map how the Eocene of Jackson and Holmes extends southward to Orange Hill in Washington County, which rises boldly above the waters of the low country a height of some 200 feet. In Miocene time this must have been a notable promontory, jutting out into the shallow seas. It is not probable that the warm currents of the great bight of Georgia, either at ebb or flow, had much force to the westward of Orange Cape, and the cold waters of the Mississippi embayment, as reasoned by the same authority, reinforced by the rivers of Alabama, creeping along through estuaries, were very unfavorable to molluscan life. For which reasons, when by position and continuity, the rocks of Wakulla and of Chattahoochee shall have been traced westward of Orange Hill, it is not to be expected the fossils of *Weelaumee* and of the Red-beds will be found therein.

There remain now only two other formations, not collated and accounted for — the Pascagoula clays and the Euchee phase of the younger Miocene.

So far as known, the Euchee stops abruptly at Daw's Bluff below Milligen. This part of Santa Rosa County is depressed, whether by subsidence or by denudation does not as yet appear. By position it might be assumed that the Euchee is to the east the equivalent of the Pascagoula of the west. But the fossils are not the same; neither is it probable that the circumstances of genesis were the same in both. Proximity to the Great River rendered the laying down of every phase of the Grand Gulf unique on this continent.

Both the Pascagoula and the Euchee were estuary so far as they agree, and it is possible the small *Mactras* to the east were the representatives of the still less marine Guathodons of the west. It is said the living *Guathodon cyrenoides* of our coast is not at present found east of Mobile Bay, but the writer has found them in Kitchen-middens on Choctawhatchee Bay, in the waters of which they and the oysters are now both said to be wanting. If not exactly equivalent, both these stand in the several regions to which they belong as the youngest known formations of this Miocene group.

THE SCIENTIFIC ASPECT OF THE UNIVERSITY SETTLEMENT MOVEMENT.

BY FREDERIC A. C. PERRINE, D.SC., BOSTON, MASS.

OF the social work in our great cities by philanthropists and churchmen, there is undoubtedly a considerable proportion unfortunately carried on in such a casual manner as to afford only pain to one trained in habits of scientific investigation and scientific caution in action. Many charitably-aimed movements have been proven to be the greatest practical failures and, in spite of the high-minded intentions of their progenitors, stand condemned at that highest of courts which demands fruits for judgment.

It must, therefore, be a relief to the minds of those having a knowledge of the need for social effort and at the same time a conception of the value of true scientific methods in such effort to know that there is at present being developed a movement having for its principle aim the general solution of the problems attacked in the light of a scientific study of the conditions and the elements actually involved in the various problems as presented in the lives of our municipal communities.

This movement to which we refer, the University Settlement effort, came originally from Oxford and was the practical suggestion of Arnold Toynbee, for whom the great settlement of London has been named and from whom came the inspiration of the workers in this and other countries.

To solve the problem of heat and light or electricity while being shut out from a possible knowledge of the facts involved or a possibility of experimenting with those forces we would to-day consider to be the height of intellectual absurdity and worthy only of the *a priori* philosophers of the Middle Ages, whose opinions are of value only as curiosities.

Or once having exactly ascertained the existence and the laws of physical phenomena and not to attempt to make use of our knowledge for the practical advancement of life upon the globe and for progress in the arts we are bound to regard as lack of enterprise and the spirit of ultimate scientific progress.

But in the so-called "Social Sciences" and their application we are only gradually adopting the inductive method for gaining knowledge and have been devoting more time to the attempted ultimate solution of fundamental problems on insufficient premises than to the practical application of such knowledge as we have already gained and the actual face to face study of the conditions for the discovery of future data.

With the sense of this lack of scientific method in the study of our social questions and the feeling of urgency in the necessity for the application of such truths as we have already obtained, the men and women of the University Settlements have established, in several of our great cities, houses, to be centres for work, set down in the midst of the conditions which are to be investigated and acted upon.

While many of the people who have taken up this work are undoubtedly so directed from their affiliation with the Church and its efforts for regeneration, the methods used are essentially foreign to what has been known as "Church work," and their aim is very far from being along the lines of attacking the problems which are purely physical from the spiritual standpoint. But striking out along the grand lines of the early development of altruism from egoism though the fellow feeling for those with whom we are in personal contact, they have adopted the idea of the self-help of a neighborhood as their governing principle. With this moving principle in view the University Settlement movement is easily understood.

Primarily it is, by bringing to the view of what has been called the "submerged tenth" the lives of those successful in the battle for contentment in life through higher ideals and greater education, to create a feeling of dissatisfaction with surroundings not typifying those ideals and to open to their minds the possibility of progress through advancement in knowledge and the attitude of mind which is not content with the creature means of existence, and, as neighbors, to help all such as have already gained a desire to become more worthy citizens, men and women. As a means for such influence the work of the settlement in visiting, clubs, classes, and all kindly actions is instituted.

For those beyond the possibility of these influence, and even beyond the effect of any efforts made for social regeneration, the situation in the midst of such classes offers a possibility for the study of the conditions and the internal life and movement of the subjects to be acted upon. Studies have begun in the collection of facts and phenomena which gradually but only gradually develop the laws of social dynamics and social statics which have been so often approached from the theoretical but so seldom from the standpoint of induction and experiment.

As the facts are discovered and these laws developed the personal relations of the workers in such fields must yield to their minds the true methods of attacking and solving the problems which perhaps only first in these studies have been presented and enable them to point out to individual workers as well as to municipalities the directions of sure progress

Not by any means the least productive effort of the settlement is this unification of the direction of the efforts which various social workers have been making towards a greater advance in economic progress and the bringing of the various classes of the community into harmony with each other.

There is no one that will doubt that a common humanity actuates us all, but it is at the same time impossible to say that there is a comprehension of this fact in the minds of the individuals belonging to the several classes.

While our origin and essential characteristics may be identical, it is nevertheless true that the variations in the external conditions have so far led us to apparent hostility that the fact that there is a common point of interest has become almost completely extinguished.

Here lies the dangerous element in the growing movement towards the usurpation of the rights of the individual by the community, for on both sides there always remains a fear of oppression and of usurpation of power by the other. To counteract such a dangerous principle, in either its idea or its application, it has become more and more necessary that our heterogeneous communities should come to a knowledge of their essentially homogeneous character, a knowledge which must rest upon firmer foundations than the mere intellectual conception of a truth and be guarded in a trust across the social barriers, only to be gained by a more intimate knowledge of each other's characteristics as well as each other's conditions of life.

By the studies we are describing the knowledge necessary is gradually being obtained, and the trust accorded by both sides to these students renders possible an actual contact from one side to the other and brings about a trust in the hearts and characteristics of men separated by the wide gulfs of circumstance. Studies such as these are developing, too, the manner of education needed for the most rapid advancement of the community, settling many disputed questions of the bearings on the lives of the people of manual training, day-nurseries, model tenements, boys' clubs, and other similar efforts which have been made from above downward, based on theories founded too often on insufficient knowledge of the facts involved and carried along with too little regard for the actual results attained.

We may, in consequence, expect from this movement a fruit of knowledge gained of social conditions and the results of sociological experiments which, while being of the character of the ascertainment of scientific facts obtained through a scientific method of investigation, yet carries with it practical results in the advancement of the life of the community toward a more rational fitness to the environment and a healthy improvement in the material conditions and culture of great masses of the community.

A NEW VISUAL ILLUSION.

BY EDMUND C. SANFORD, CLARK UNIVERSITY, WORCESTER, MASS.

THE following illusion is, so far as I know, new and seems of sufficient interest to put on record. A short-pointed star of white card-board, or even a square, is placed on the spindle of a rotation color-mixer and set in rapid rotation. The resulting appearance is a white central circle surrounded by a transparent ring — most transparent at the outer edge, least transparent toward the centre. If now a piece of black card-board of a length somewhat greater than the diameter of the star from point to point be brought behind it while in rotation, the advance of the edge of the card can be followed, not only behind the transparent ring, *but also behind the opaque central circle.* It is most noticeable just within the circumference of the central circle, and is most marked when the black card is kept in motion. When the card remains stationary, the illusion weakens; and for perfectly stationary objects, like the parts of the rotation apparatus itself, it fails altogether. The portion of the central circle, through which the card seems to be seen, is also appreciably darkened by its presence. When the star is black and the card behind it white, the illusion is still present, though a brightening of the black takes the place of the darkening just mentioned.

The rationale of the illusion is easy. The outer ring is really transparent, and the edge of the card is really seen through it. The transparency of the ring strongly suggests the transparency of the centre, a suggestion that we accept the more easily because the rapid rotation changes the appearance of the central portion somewhat from its familiar resting appearance. The apparent darkening of the portion of the central part overlying the black card is to be similarly explained. It is especially interesting, however, as being clearly a psychological illusion, an "illusion of judgment," while the color illusions formerly so called are being shown to be physiological, and largely, if not entirely, due to the mutual influence of adjacent portions of the retina.

A physiological explanation of this illusion seems hardly possible; and its psychical character is further attested by the ease with which it is corrected when the card remains stationary, and the unequal degree in which it seems to affect different observers.

A SUGGESTION AS TO TOPOGRAPHIC MAPS.

BY ARTHUR P. DAVIS, LOS ANGELES, CALIFORNIA.

WHILE the scientific and technical bodies of California and other States are agitating the question of topographic maps, educating the public on the question, and endeavoring to secure State appropriations and Government coöperation for such surveys, it may not be amiss to suggest other sources that might be made to furnish valuable contributions toward the same end. I refer to the very extensive and costly surveys made by engineers, promoters, and companies to determine the necessary information for the construction of railroads, irrigation systems, etc.

I have known cases where a large number of preliminary or trial lines have been run, the aggregate cost of which amounted to many times that of a good topographic map of the region under investigation, which would have shown all that the afore said surveys can show, and a great deal beside; for it is always an open question, whether the line finally determined upon is in reality the best in all respects, or whether some other, of the many untried possible routes might not be somewhat better or cheaper. All these possible routes would be shown by a good contour topographic map, and in addition thereto it would serve as a valuable piece of data for any future additions or alterations in the work.

Another argument in favor of the contour map that ought to weigh strongly in its favor with the persons above referred to, is the philanthropic one, that such a map is a valuable and permanent contribution to science. A large percentage of the scientific discoveries that have contributed so prodigiously to the intellectual and material advancement of the human race, have been made by persons working without the incentive of financial gain, and it is safe to say that few of them would have been made, if that had been the only incentive followed.

I do not believe that engineers and capitalists in charge would prove less public spirited than others if their attention were properly directed to this matter; and if geologists and others actively interested would persistently present these facts to the proper persons, great good might result. The main point to be carried is the substitution of the plane table for the transit in making preliminary surveys.

Without a systematic method of accurate field-sketching, which is the essential principle of the plane table, topographic mapping on any extended scale is impossible. Properly handled, with triangulations to check locations, and level bench-marks to check elevations, and with stadia to assist in sketching, plane table work may be entirely accurate within any scale adopted, and serve not only for preliminary information upon which to locate routes for canals, railroads, etc., but is a permanent record of comprehensive information to guide all future engineering operations in the country included, so that ordinarily at least two or three times as much might be advantageously spent on the construction of contour maps as would be required for running preliminary lines, and still the company would profit by the substitution. If the labor expended upon the tremendous mass of material now on record in the great engineering offices throughout the west had been judiciously supplemented by field-sketching on the plane table, a very great addition would have been made to our topographic knowledge, and I believe that such results could be brought about by well-directed efforts on the part of the proper persons.

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.

Pseudoauroræ.

DR. HATCH'S description of "Pseudoaurora Borealis" in your issue of Dec. 2, 1892, calls to mind a peculiar phenomenon which I once witnessed here, and which may have some bearing on pseudoauroræ in general if not on the particular species observed by Dr. Hatch.

About three years ago, as I was returning from the business part of the village of Plattsburgh, N. Y., my attention was taken by two long, white, brilliant, quivering streamers in the south-west, which at times seemed to shoot up and nearly reach the zenith. This was an unusual direction for such a display, and I at once turned toward the north to note its character there. Buildings prevented a good view, but I saw several streamers though none so brilliant as those in the south-west. Hastily taking the phenomenon for a true display of the aurora, I hurried home, only noting on my way that the streamers were brighter now in one direction, and now in another. It was not until I had called others out to witness the display, and remained quiet myself for a moment, that I discovered that one very brilliant streamer seemed to be situated directly back of the known position of one of our arc lamps, and what was still more curious it refused to move from that suspicious position. This streamer varied remarkably in brightness, now being short and faint, and again long and brilliant. Along with these greater changes in intensity there were more rapid and lesser changes, and in addition to these a constant shimmering of the light. There were also slow wave-movements of brighter portions which ran from below upward, or crossed the streamer from left to right. It was movements of this nature, so like the curtain movements of the true aurora, that led me for the moment to refer the phenomenon to the aurora itself, and the many beams, which sometimes ran so high as to suggest a corona overhead, and which varied in relative intensity if one was moving about, only helped to confirm the error. Plattsburgh has had electric street-lamps for seven years or more, yet this one display stands practically alone by itself. The streamer which I studied most was over a lamp something more than a thousand feet away, and was viewed across some village lots with many buildings clustered around the position of the lamp. The lamp itself was thus hidden from view, though it hung over the centre of the street and could cast no high shadows save alone those of the top of the lamp and its wires. These, of course, helped to cut up the beam of light. I do not think that a dark arch was present, though I can conceive that one of Professor Hazen's shadows might have been thrown by a group of trees or buildings in such a manner as to produce one, had some convenient cloud been situated in the background. I attribute the phenomenon to the peculiar condition of the atmosphere at the time. We were in a very light frost-fog and the vertical and lateral movements of brighter waves were probably due to denser portions of this frost-cloud, drifting along with the air-currents. The varying light of the arc lamps served to make these movements seem the more complex. I think it very probable that color was in many places present, due to a halo encircling each lamp, although I do not recall having noticed it. From Dr. Hatch's description it would seem that the two phenomena are not the same, as when he retired from the lamps the "aurora" vanished, yet the phenomena I have described were seen from quite a distance, although if I moved about the streamers changed in relative brilliancy. I have many times thought of this appearance when I have looked over published reports of auroræ from voluntary observers, and it may be well to show that all unusual night-lights are not auroræ.

From Dr. Hatch's proximity to the lamp, "about the angle of 60° to the burner" (is this altitude, zenith distance, or an angle measured from some street lines?), it may be possible that his phenomenon has some relation to the halos which may be frequently seen around the arc lamps here. When near the lamps the halo is small and, under proper atmospheric conditions, very brightly colored; at a greater distance the halo is larger but the colors not so distinct. In either case if you can witness the upper half of this halo *as if it were on the celestial sphere*, you will have a large "luminous arch" "consisting of pencils of light radiating upward from a dark arc, . . . the pencils constantly changing in length, and having an apparent movement laterally" if the head is moved in the least while noting different portions of the arch. The "coloration of the pencils" will be also "unmistakable." See Dr. Hatch's reply to Professor Hazen in *Science* for Jan. 20, 1893. GEORGE H. HUDSON.

Vice-Principal State Normal and Training School, Plattsburgh, N. Y.

Continuous Rain.

A REMARKABLE phenomenon was observed in the town of Athens, Ohio, late in the fall, which has awakened wide interest, viz., continuous rain during a succession of clear, beautiful days. This was noticed extending for a considerable distance just below the crest of a hill, and lasted through the day, from soon after sunrise till about sunset. The drops of water were at no time large, but they reached their maximum size about two or three o'clock in the afternoon.

The subject attracted the attention of professors in the Ohio University, and it was soon determined that the phenomenon must be due to the precipitation of vapor which had been carried through an old railroad cut for several hundred yards. There had recently been completed and set in operation extensive brick-works, where three large ovens were continually in operation, and from which hot currents of air steadily shot upwards. In the moulding of the bricks, water is mixed with clay, and an enormous amount of hot, watery vapor was passing into the air above the ovens, supplemented by large quantities from the stacks of a large "dryer," which was kept at a high temperature. It is estimated that in all fully forty-five tons of water were at this season daily evaporated.

The plant is situated in the valley of the Hockhocking River, close to a cut made many years ago for a projected railroad, and this cut leads directly to the rise of land where the observations were made. The observer at the University Weather Station reports that the prevailing wind was at this time in a direction such as would carry the hot air, laden with moisture, through this artificial passage. The air was, in all probability, carried partly up the hill and there dissipated along the side. About this time it must have come in contact with a cold current near the crest of the hill, and precipitation followed, causing this unusual rainfall. The conclusion that the precipitation was due to these causes is strengthened by the fact that not until the manufacture of bricks at this place was begun was any such phenomenon observed, so far as is known. H. E. CHAPIN.

Ohio University, Columbus, Ohio.

Natural Selection at Fault.

IN the issue of *Science* for Jan. 20 there appears, under the above heading, an article from the pen of J. W. Slater. The conclusions there arrived at do not necessarily follow from the facts cited. That animals of the Felidæ by tormenting and playing with their victims often lose their prey, which otherwise might have been devoured, is well known to every observer. The facts, however, that this is most frequently done by the younger animals, and generally at a time when they are not greatly in need of food, are overlooked. Besides, what seems to be the most important consideration in the case, is that by means of this play that quickness and precision of motion so essential to success in procuring food are acquired, so that doubtless the gain in the end is much greater than the temporary loss occasioned by the accidental escape of a victim now and again.

In reference to the cackling of the hen, it may be that this animal has been so long domesticated that it is impossible to draw inferences with any degree of certainty from its conduct in this respect. Every house wife, though, who has kept hens, is well aware that their cackle is very deceptive, that it is generally not commenced till they have got a little distance from the nest, and may, very likely, in most cases, serve to attract attention to themselves and away from their nests. Several of the wild birds that nest on or near the ground, when suddenly disturbed, escape in a manner evidently intended to attract attention to themselves and away from their brood. The action of the domestic hen may generally serve a similar purpose, and yet at times fail or even produce an opposite result.

Neither does it appear that the human ear is any more a case in fault. The principle of natural selection does not necessarily require the loss of a useless member unless it is positively injurious — a hindrance in the struggle for existance. The outer ear is not that; it may even serve a purpose. Writers on acoustics tell us that it serves to some extent to condense or concentrate the sound-waves. Even if it serves no other purpose than to improve the personal appearance, its retention would still be in perfect accord with the theory of natural selection.

Besides, it cannot be shown that the human ear is not now undergoing a process of atrophy. Grant that the outer ear has been of no use to our fathers for many generations, it would not necessarily follow that children of to-day should be born earless. All evidence goes to show that changes of this character are so gradual as to escape notice. The fact mentioned by Mr. Slater, that, owing to disuse, the outer ear has lost its power of motion so far supports the theory of natural selection. That the ear is not entirely gone, as he thinks it should be, may be due to its still being of service or to lack of sufficient time since it became useless. RICHARD LEES.

Brampton, Ontario.

Leaf Impressions in the Eocene Tertiary of Alabama.

THOSE working geologists who are interested in what Professor Lester F. Ward[1] terms "The New Botany" may be somewhat surprised to learn that in the Eocene Tertiary strata of Alabama there is a promising and unexplored field for the paleobotanist. In fact there is reason to believe that a careful study of the plant-life existing in the Mississippi embayment during the well-marked subdivisions of the Tertiary will throw some light upon the knotty problems of the interior.

While the study by Lesquereaux of the Mississippi Lignitic was of interest and affords the present main means of correlating the trans-Mississippi Tertiary with that of the Gulf Coast, the value of this work for this purpose is somewhat diminished by the doubts as to the exact age of the several horizons in which the leaf impressions occur. On the other hand, the geological section so accurately established for the Tertiary in Alabama affords a key for the critical solution of age-problems in the Gulf Region. Between beds of marine shells, whose faunal features have been determined with relation to kindred deposits on the Atlantic border, are beds of sandy clays containing well-preserved leaf impressions. These are found in the Lower Tertiary at Bells Landing on the Alabama River, where numerous dicotyledonous leaves occur in the stratum between the Bells[2] Landing and Greggs Landing marine shell beds. In the middle Tertiary of the Claiborne group both at the typical locality[3] and on Barrows Mill Creek, a tributary of Conecuh River, Covington County, are extensive occurrences of fine fossil leaves.

The State Geological Survey of Alabama has some few specimens from each of these localities but no systematic collecting has been done and no determination of species has been made.

[1] Science, Vol. XXI., No. 521, p. 43.

[2] Bull. 43 U. S. Geol. Survey, 1887, p. 47.

[3] Am. Jour. Sci., 3d Ser., Vol. 31, 1886, pp. 2 2-209.

The fact that the exact age of each horizon referred to is clearly established in the geological column should make these beds of particular interest to the paleobotanist and should contribute materially to our facilities in correlating the much-discussed Interior Tertiaries. DANIEL W. LANGDON, JR., F.G.S.A.

Cincinnati, Ohio.

Bowser's Mathematical Text-Books.

I HAVE just read a note on "Bowser's Trigonometry" by Professor Hodgkins in *Science* of Jan. 20. Permit me to add a few words on Bowser's series, both in the way of praise and criticism, and, therefore, favoring both sides of the question. I used his analytical geometry and calculus for two years with good results. They are well adapted to the average student in arrangement, examples, and general plan, and they are more modern than most text-books of the same class. But the subject is sometimes unnecessarily complicated, as in solid analytics, where the beauty of the method of direction-cosines is seriously marred. Also, in respect to the details of accuracy of statement and logical demonstration, I am sorry to class the series among the free and easy kind of which we have so many, although among the best of that kind. The public is as much at fault for accepting and even demanding books in that style as are the authors for writing them.

Let me illustrate by his treatment and use of the method of infinitesimals. That method is at best a dangerous one, even in the hands of the masters, let alone the average student. This is sufficiently well illustrated by the errors into which Professor Bowser himself has fallen; and he should read the scoring that Clausius gave his mathematical critics on their use of infinitesimals. He will find that he is in good company. Most anything can be proved to the satisfaction of the average student, just as Professor Bowser establishes the differentials of the trigonometric functions. Thus, by trigonometry,

$$\sin(x + dx) + \cos(x + dx) = \sin x \sqrt{2} \cos\left(\frac{\pi}{4} + dx\right) + \cos x \cos dx + \cos x \sin dx = \sin x + \cos x + \cos x \, dx,$$

since $\sqrt{2} \cos\left(\frac{\pi}{4} + dx\right) = 1$, $\cos dx = 1$, $\sin dx = dx$.

Hence $d(\sin x + \cos x) = \cos x \, dx$, a false result.

Professor Bowser is more fortunate than the critics of Clausius, since he happens upon a final result that is correct; but, farther along, this good luck deserts him, in the case of a carefully-worked example (Calc., ex. 3, p. 325). Another case is ex. 8, p. 338. In view of these facts, I hope Professor Bowser will revise his demonstrations and eulogy on infinitesimals, to the decided improvement of his valuable book. A. S. HATHAWAY,

Professor of Mathematics, Rose Polytechnic Institute.

Terre Haute, Ind., Jan. 28.

Some Additional Remarks on Maya Hieroglyphic Writing.

IN a former communication, replying to some objection brought forward by Professor Thomas, I noticed that in the numerals, composed of straight lines and dots, which are seen accompanying the hieroglyphs of the Maya inscriptions, the one dot of the numbers 1, 6, 11, 16 always is supported and framed by two ornamental signs filling up the space, while no ornamental sign is seen between the two dots of the numbers 2, 7, 12, 17. I noticed this for a Copan Stela published by Alfred Maudsley (see the Figs. 1–16 in my former paper). I may add that the same applies to the inscriptions of the Palenque tablets, only that here the two dots of the number 2, like the one dot of the number 1, are framed by two ornamental signs, while the two dots of the numbers 7, 12, and 17, as a rule, are standing alone. I wish to state that although prevailing in most cases, this rule may allow some exceptions. Alfred Maudsley, page 39 of the text, gives drawings of the numerals, where an ornamental sign, similar to the two ornamental signs of the numbers 1 and 6, is seen between the two dots of the numbers 2 and 7. Maudsley does not mention where he has taken these figures. But, for instance, on the cross-tablet 1, of Palenque, in the hieroglyph V. 17, designating the twelfth day of the month Kayab, a somewhat peculiar ornamental sign, composed of two hooks, is seen between the two dots of the number.

In connection with these facts, I wish to mention that there really exists an instance of a cross between the two dots of a number in Dresden Codex 46, already mentioned by Professor Förstemann in *Zeitschrift für Ethnologie*, 23 (1891), p. 149, that, unfortunately, I had overlooked. DR. SELER.

Steglitz, Germany, Jan., 1893.

Languages of the Gran Chaco.

I WAS very much gratified to see that Dr. Brinton thinks well of my intention to publish all the material I can get hold of connected with the languages of the Gran Chaco. The following facts may be of interest to him and other Americanists on your side of our continent.

1. Dr. Brinton is quite right in giving the name of "Guaycurú" to the Abipone and other cognate dialects. The root word is *ary*, which simply means "a fierce savage." *Gu* and *curú* are simply particles.

2. The linguistic library of the La Plata Museum will comprise two series: First, the Guaycurú; second, the non-Guaycurú group.

The Guaycuru Group

a. MOCOBI. Father Tavolini's MS. faithfully reproduced; a grammar founded on same, with a preliminary discourse and other papers. An English version of the grammar.

b. TOBA. Father Barcena's MS. complete, with supplementary vocabularies by Carranza, Pelleschi, the editor, and others. A preliminary discourse on the language. An English translation of F. Barcenas Quires.

c. ABIPON. Father Dobrizhoffer's chapters on this dialect, supplemented from MSS. supposed to be Father Brigniel's, with a preliminary discourse, and most important vocabulary.

d. LENGUA. Cerviño's MS. vocabulary. Evidently a cognate dialect, with Mansfield's Payaguá. Preliminary discourse on the same subject..

e. GUAYCURÚ. An essay on Castelnan and Gibü's vocabularies.

Non-Guaycuru Group.

a. A reproduction of Father Machoni's work on the Lule language, with an essay on the suffixing dialects of the Chaco.

b. An essay on the Vilela and Chulupí dialects, to accompany Pelleschi's vocabulary.

c. *Mataco.* Pelleschi's grammar and vocabularies, with notes and preliminary discourse by the editor.

d. Possible numbers in Mataguays, Nocten (Mataco dialects), and Chiriguano (a Guarani dialect).

Dr. Moreno. director of the La Plata Museum. is doing his best to push this work forward. SAMUEL A. LUFONE QUEVEDO.

Pilciao, Catamarca, Argentine Republic, Dec. 18.

Controversies in Science.

IT might be well for scientific controversialists to bear in mind that undue heat is an indication — as in mechanics — of want of that balance that should constitute a judicial mind. The world generally views with amusement the frothy utterances of the man on the wrong side who finds himself hard pressed by reiterated facts, and judges him to be in the wrong, frequently, by his language, when he may be correct entirely. One without any knowledge of the facts of the present controversy between a few persons connected with the U. S. Geol. Survey — a survey at present under a cloud from the disbelief of Congress as to its needs and usefulness — and the upholders of "palæolithic man," would naturally incline to the side taken by Professor Wright, merely from the perfect courtesy and evenness of temper which he has preserved under exceptional circumstances. It is seldom in the course of controversy that a clergyman of good character has been so bespattered with epithets, inuendoes, and charges that would render him — if true — worthy of abrupt expulsion from any position of trust, or from any decent religious body.

It is probably because Professor Wright is so secure in his position that he can afford to pass by in silence the statements that any sane man can see are unwarranted, and the attention of the world at large may have been directed to him by these very attacks, as well as arranged on his side by their baselessness. The writer does not wholly agree with the professor; but he can thank him for a good example of a disputant. The time has passed when the progress of knowledge can be dammed by the straws of a few determined opponents, and the examples of the primordial and cretaceous controversies cannot be safely repeated. It has been the shame of America that it has been so taken up in petty fights over side issues that it has left to others abroad the building of the science of geology.

In the future the combatants in the arena had better take as their type, the old-fashioned town pump. It always works best in the cold and deliberation of winter, and the quality of its product is beyond question. When the heats of spring come it begins to diminish its flow, and during the controversial dog-days it dries up.

EDWARD P. WILLIAMS, JR.

Bethlehem, Pa., Feb. 6

BOOK-REVIEWS.

The Hemiptera Heteroptera of the British Islands. A descriptive account of the families, genera, and species indigenous to Great Britain and Ireland, with notes as to localities, habitats, etc. By EDWARD SAUNDERS, F.L.S. London, L. Reeve & Co., 1892. With 32 plates. Price, £2, 8s.

THE late well-known naturalist, the Rev. J. G. Wood, used to deliver a popular and entertaining lecture on "unappreciated insects." Among these he included the one which is familiarly termed the black beetle. To this he endeavored to reconcile feminine taste and intelligence by representing that its approach infallibly scares from our chambers the more dreaded and more vicious bed-bug. But this latter is itself one of the unappreciated, for which even a very skilful advocate will not easily conciliate our esteem. The best perhaps that can be said for it is that the barest suspicion of its presence is an incentive to cleanliness. The modern feeling about this special nocturnal terror is happily, however unintentionally, expressed in that old version of the Psalms, which brought home to the reader a deep sense of comfort and security by the wording, "so that thou shalt not nede to be afrayd for any bugges by night." It is singular that collectors of insects should have adopted for themselves as a kind of pet name the title of bug-hunters. Perhaps they have wished to wrest a weapon out of the hands of the scornful, with the feeling that it is more agreeable to call oneself names than to be called names by other people.

The students of bugs in particular, as distinguished from those who study insects in general, are comparatively few. It is probable that, for the sins of a single and not very characteristic species, a prejudice has been evoked in the public mind against the whole order to which the objectionable species happens to belong. There has been plenty of time for prejudice to gather strength, since the genus *Cimex* is said to have made its first ascertained appearance in the far-distant Liassic period. There is something wonderfully romantic in the thought of this bloodthirsty genus biding its time, waiting, craftily waiting through so many ages till man should appear upon the mundane scene with lodgings to let! But as it must be confessed that its habits, however venerable for their antiquity, have placed all its kindred more or less under a cloud, there is the more reason to acknowledge the spirited enterprise of Messrs. Reeve in publishing an expensive work on this rather neglected department of zoölogy. There is, however, good reason to think that the volume, being such as it is, will largely help to cure the neglect of the subject. The beginner learns at the outset that most of the species are vegetable-feeders, and that, from the few that are less temperate, the collector runs practically little or no risk of harm. From the beautifully colored plates it is obvious that many of the species must be in nature highly attractive. The clear descriptions of all the species at present known in Great Britain and Ireland

CALENDAR OF SOCIETIES.

Anthropological Society, Washington.

Feb. 7. — O. T. Mason, Co-operation in Anthropological Work; Clifford Howard, The Philosophy of Sin; W. H. Holmes, Early Man on the Upper Mississippi.

Biological Society, Washington.

Feb. 11.— M. B. Waite, The Destruction of Lichens on Pear Trees; C. H. Townsend, The Propagation of the Atlantic Coast Oyster on the Pacific Coast; Charles Hallock, The Geographical Distribution of the Musk-Ox; C. Hart Merriam, The Four-Toed Kangaroo Rats (with exhibition of specimens); F. A. Lucas, The Food of Humming-Birds.

Society of Natural History, Boston.

Feb. 15.—Henry W. Haynes, More Evidence of Cannibalism among the Indians of New England; R. T. Jackson, Notes on the Development of Palms; S. J. Mixter, A Massachusetts Beaver Dam.

will, in the first instance, appeal especially to British students, but from one point of view they may still more court the attention of students elsewhere, for, when specimens are unattainable, good descriptions and figures are an exceedingly welcome substitute. Every one must value the facilities for work provided by the elaborate synoptic tables.

Those who can remember the devotion to natural history of the late Mr. Wilson Saunders and the vast and admirable collections of insects which he accumulated from all parts of the world, may be disposed to believe that his son, the author of the present work, was born as well as bred an entomologist. The name of Edward Saunders, to those acquainted with his lifelong studies and with his previous writings, will be an ample guarantee that in this book also they will find the most conscientious accuracy and all the thoroughness of treatment that the subject admits. It is commonly reported in England that the revenue officers of the United States levy duty upon meteorites which descend from the sky, but probably the hemiptera pass the frontier without the least regard to tariffs, or quarantine regulations, or laws against the importation of destitute aliens. How little, then, can the free-trade precincts of Great Britain and Ireland hope to defy the invasion of any new bug that may choose to enter! But, at any rate, the collector who finds within those precincts one of the Heteroptera that has not been described in the volume now under review, may well suspect that it has been recently introduced into them from without.

In a work so sumptuously printed one may be permitted to wonder why there is no index to the plates and why no references are given in the text to the excellent figures which those plates contain. As a matter both of good taste and convenience it would surely also have been better to give in full the names of authorities, instead of such abbreviations as Muls. and Put. for Mulsant and Puton. The reader may find a chance of guessing that Boh. and Fall. stand for Bohemann and Fallen, but Lap. and Spin. and Duf. find no explanation within the four corners of the book itself. The title, "Hemiptera Heteroptera," is quite justified by the usage of other authors, and Hemiptera seems really a better title than the alternative Rhynchota, but in the division of the order into Heteroptera and Homoptera it is very unsatisfactory that the names applied to the suborders should have the same termination as that appropriated to so many orders of the Insecta. A protest may be made, too, against the use, now becoming common, of the word "asymmetrical." Those who are discontented with "unsymmetrical" ought to write "asymmetric," and be pedantic at both ends of the word. From misprints and similar blemishes the volume is very agreeably free, although there is some obscurity in the account of *Corixa*, which is said to contain twenty British species divided into four subgenera, whereas the synoptic table shows six subgenera and twenty-five species, to which a twenty sixth is doubtfully added in the descriptions.

The zeal of collectors will be stimulated to find again such prizes as *Aradus Lawsoni* and *Pygolampis pidentata*, or the greenish black *Prostemma guttula*, with scarlet legs and elytra, and antennæ pitchy brown. But the study of the group has more to commend it than the tantalizing rarity of some of the species. It is no little advantage that a great many of them are on the contrary common and easy to obtain. They do not, it is true, flaunt themselves in mid-air like butterflies or birds, but rather keep themselves quiet on trees and various lowlier plants, in mosses, in ponds, and other retreats, from all of which they can without much difficulty be induced to come forth. The search for Hemiptera is pleasantly united to the observation of plant life, and when a collection has been made, the curious shapes and bright colors of the specimens are likely to be associated with treasured memories of holiday excursions, fair scenes, and delightful rambles, that have been enlivened by this quest. Few of those who make themselves acquainted with Mr. Saunders' volume will continue to despise the Hemiptera, and few of those who take any deep interest in the Hemiptera will care to be without Mr. Saunders' volume. THOMAS R. R. STEBBING.

Tunbridge Wells, England.

Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

For sale or exchange.—I have a few copies of my translation of "Strasburger's Manual of Vegetable Histology, 1887," now out of print, which I will send post-paid for $3 or for one dozen good slides illustrating plant or animal structure. Address A. B. Hervey, St. Lawrence University, Canton, N. Y.

The undersigned has the following specimens to exchange for crystals of any eastern or foreign localities or Indian relics: tin ore, metacinnabarite, stibnite, garnierite, calenanite, hanksite, ulexite, rubellite, lepidolite, blue and green onyx, Cal. pineite, aragonite on chalcedony, cinnabar, double refracting spar, clear and clouded, and others. J. R. Bush, care of General Delivery, Los Angeles, Cal.

For sale or exchange.—A private cabinet of about 200 species of fossils, well distributed geologically and geographically. Silurian, about 40; Devonian, about 50; Carboniferous, about 80; others, about 30. Frank S. Aby, State University, Iowa City, Ia.

For exchange.—Minerals, fossils, F. W. shells, land shells, native woods, Indian relics, two vols. of Smithsonian reports, odd numbers of scientific magazines, copper cents, etc., for good minerals not in my collection, good arrow- and spear-heads and natural history specimens of all kinds. Correspondence solicited with list of duplicates. G. E. Wells, Manhattan, Kan.

For sale or suitable exchange.—A spectrometer made by Fauth & Co., Washington, D. C., according to the plan of Prof. C. A. Young. This instrument is suitable for the most advanced investigations and determinations. Cost originally $700 and has been used but little. Will be disposed of at a considerable reduction. Address Department of Physics, Ohio University, Athens, O.

I will send British land and fresh-water shells in return for those of America, any part, sent to me. I have at present about fifty or sixty species, with many varieties. W. A. Gain, Tuxford, Newark, England.

The Biological Department of Hamline University desires to offer microscopic slides of animal tissues, or whole animals, in exchange for first-class fossils. Address correspondence to Henry L. Osborne, Hamline University, Hamline, Minn.

Wants.

CAN any one inform me as to the age to which cats have lived? I have one twenty years old. Edward D. Webb, 132 W. Eighty-first St., New York.

WANTED — Second-hand. Foster's Physiology, Balfour's Comparative Embryology, Claus & Sedgwick's Zoology, Flower's Osteology of Mammalia, Vine's Physiology of Plants. Please state editions and prices asked and address Richard Lees Brampton, Ontario, Canada.

WANTED.—American Journal of Conchology, seven volumes. Parties having these for sale will please address the undersigned, stating condition and price. R. Ellsworth Call, Louisville, Ky.

A GRADUATE ENGINEER will give instruction evenings in geometry, trigonometry and surveying, mechanics, physics, mechanical drawing and general engineering construction. Five years' experience in field and editorial work on engineering journal. References furnished. C. S. H., 102 Tribune Building, New York.

WANTED.—By well-qualified and experienced science master and associate of the Royal School of Mines, London, aged 26 (at present in England), a mastership in technical college or university for any of the following subjects: Engineering sciences, geology and mineralogy, physics, chemistry and metallurgy, etc., etc. Can provide excellent references and credentials. Apply, J. G., 17 Sussex St., Rochdale, England.

A GRADUATE of the University of Pennsylvania and a practical mineralogist of twenty years' experience desires to give his services and a cabinet of 25,000 specimens, all named, with about the same number of duplicates, in minerals, crystals, rocks, gems, fossils, shells, archæological and ethnological specimens and woods to any institution desiring a fine outfit for study. The owner will increase the cabinet to 50,000 specimens in two years and will act as curator. Correspondence solicited from any scientific institution. J. W. Hortter, M.D., Ph.D., San Francisco, Cal., General P. O. Delivery.

SCIENCE

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SCIENCE

NEW YORK, FEBRUARY 24, 1893.

THE ABSENCE OF AIR FROM THE MOON

BY SIR ROBERT BALL, LOWNDEAN PROFESSOR AT CAMBRIDGE, ENGLAND.

ASTRONOMERS have long felt that the absence of air from the moon is a fact that demands some special explanation. Most of the globes in space which are known to us are encompassed by more or less copious atmospheres, why then is the moon an exception? why should there be a gaseous investment to the earth and to Venus, to Mars and to Jupiter, and why should the moon alone be devoid of such covering? The sun and other stars are also so very copiously endowed with gaseous surroundings that the total want of anything of the kind from the moon becomes all the more enigmatical.

At last a light has been thrown on the matter, and an explanation is now provided which is so consonant with the present state of physical knowledge, that I cannot hesitate to accept it. The absence of air from the moon is a necessary consequence of the kinetic theory of gases.

According to the principles of this theory, now generally accepted among physicists, any gas such as oxygen or hydrogen, is composed of molecules which move with an extreme degree of rapidity. The molecules of hydrogen, for instance, which are the most nimble of all the gases in their movements at ordinary temperatures, dash along so fast as to travel on the average somewhat more than 6,000 feet a second. Oxygen and nitrogen have movements which are generally much less than those of hydrogen. But it is to be noted that, in the course of their movements, individual molecules frequently attain velocities very much in excess of the average pace. This is the important point for our present purpose, for on it depends the explanation of the phenomenon of which we are in search.

It can be shown that the mass and the dimensions of the moon are such that if a body were projected upwards from its surface at a pace, let us say, of half a mile a second, that body would ascend to a very considerable elevation, ultimately, however, the attraction of the moon would overcome its outward movement, and the body would tumble back again. If, however, the initial pace were so much greater that it attained a certain critical amount of about a mile a second, then the missile, according to the laws of motion, would ascend from the surface of the moon and go on and on never to be again re-called by any power that the moon's attraction could put forth.

Let us suppose that the moon were now to be invested with a new atmosphere of oxygen or nitrogen. The molecules of these gases will, of course, be darting about with the velocities appropriate to their nature, but, generally speaking, the speeds with which they are animated remain within the limits of velocity which it is in the power of the moon to control. But these are only the average speeds, and it will frequently happen that individual molecules will be animated by a speed equalling or exceeding the critical pace of a mile a second; if this takes place at the upper layers of the moon's atmosphere, the little molecules will take leave of the moon altogether. Other particles follow in the same fashion, and thus it happens that an atmosphere composed of such gases as these we know could not permanently abide on the moon.

On the earth we have and we retain a copious atmosphere. The reason simply is that the earth is massive enough to require that a projectile shall attain a speed of about six miles a second before it goes off and takes leave of our globe. This velocity it would seem that the molecules of oxygen and nitrogen do not generally or ever reach. Hence it is that while the earth can retain the atmosphere with which it was endowed, the moon is unable to do likewise.

SOME ERRORS IN THERMOMETER READINGS.

BY FRANK WALDO, PRINCETON, N. J.

I HAVE understood that the long-awaited comparison of ordinary thermometers with the gas thermometer, at very low temperatures, has been carried out at the International Bureau of Weights and Measures at Sevres. However, I have been unable to get hold of any account of this work, as the official reports concerning it had not been received a short time ago even at the Weather Bureau Library. In Wild's *Repertorium für Meteorologie*, Vol. XV., which has just been received, there is an account of some careful comparisons at low temperatures, which gives results probably not very different from those obtained at Sevres; and a little summary of this will undoubtedly be of interest to some readers. In the St. Petersburg paper,[1] S. Hlasek gives a little summary of the condition of the thermometric standards of the Russian Meteorological Service from the time Director Wild took charge (about 1868) up to the present time. In the present communication, I will not trace through the various thermometer corrections as given by Hlasek, but will merely give the latest results, showing the corrections to be applied to the standard mercurial thermometer at moderate and low temperatures and to the standard spirit thermometer at very low temperatures, to reduce them to the hydrogen gas thermometer, which is the international standard.

Correction of the spirit thermometer by Geissler.		Correction of the standard mercurial thermometer, Geissler No. 10.	
At	Correction.	At	Correction.
− 5° C.	− 0.56° C.	40° C.	− 0.16° C.
− 10	− 0.72	35	− 0.16
− 15	− 0.90	30	− 0.16
− 20	− 1.10	25	− 0.15
− 25	− 1.36	20	− 0.13
− 30	− 1.68	15	− 0.11
− 35	− 1.95	10	− 0.08
− 40	− 2.23	5	− 0.05
− 45	− 2.47	0	0.00
− 50	− 2.72	− 5	+ 0.02
− 55	− 2.95	− 10	+ 0.07
− 60	− 3.15	− 15	+ 0.13
		− 20	+ 0.22
A zero-point correction of + 0.39° C. has been applied in assigning these corrections.		− 25	+ 0.27
		− 30	+ 0.25
		− 35	+ 0.31
		− 40	+ 0.36

These corrections were obtained by means of a normal (Toluène) thermometer, Tonnelot No. 4932, which had been compared with the hydrogen gas thermometer at Sevres.

[1] Die Temperatur-scalen des Physikal. Cent. Observ. und ihr Verhältness zu der International Temperatur-scale, 1892.

Since January, 1892, all of the thermometers verified at the Central Physical Observatory at St. Petersburg have been referred to the hydrogen gas thermometer at Sevres as a standard.

The thermometers verified between January, 1877, and January, 1892, require the following (additional) corrections, in order to reduce their readings to this standard:—

Temperature.	Correction of the mercurial thermometers.	Correction of the spirit thermometers.
+ 40° C.	— 0.16° C.	— 0.2° C.
+ 35	— 0.16	— 0.2
+ 30	— 0.15	— 0.2
+ 25	— 0.14	— 0.1
+ 20	— 0.12	— 0.1
+ 15	— 0.09	— 0.1
+ 10	— 0.07	— 0.1
+ 5	— 0.04	0.0
0	0.00	0.0
— 5	— 0.03	0.0
— 10	— 0.02	0.0
— 15	0.00	0.0
— 20	+ 0.01	0.0
— 25	+ 0.06	— 0.3
— 30	+ 0.07	— 0.6
— 35	+ 0.14	— 0.8
— 40	+ 0.25	— 1.1
— 45	——	— 1.4
— 50	——	— 1.6
— 55	——	— 1.8
— 60 C.	——	— 2.0

I wish also to mention the differential thermometer corrections described by Leyst in Wild's *Repertorium für Meteorologie*, Band. XIV., in which the temperature of the thread of mercury, when read, is different from that of the bulb, to which it is referred. Two cases are cited. 1. For a maximum thermometer, with separated thread (as, for instance, the Negretti and Zambra form), the thermometer is read at a different temperature from that at the time of maximum temperature, when the separation took place. For the ground-surface temperature at Nukuss, Leyst finds for a summer day a correction of + 0.73° C., and that for the average of three summer months a correction of + 0.51° C. must be applied to counteract this error. For the air temperatures the corrections ranged from + 0.10° C. to + 0.20° C. in the cases cited by Leyst. 2. The temperature of the thread of mercury and that of the bulb is not the same in the case of the wet-bulb thermometer, when the difference in the temperatures of the wet- and dry-bulb thermometers does not vanish. Ordinarily, the thread is warmer than the mercury in the bulb. At a temperature of 30° C., and a humidity of 50 per cent, there was a correction of — 0.30° C., which means, for this case, an error of 0.5 millimeters in the absolute humidity, and of 2 per cent in the relative humidity.

TO ANTHROPOLOGISTS.

Department M of the World's Columbian Exposition includes all subdivisions of anthropology and history, although generally known as the "Department of Ethnology."

The anthropological portion of the department is subdivided into the following principal sections:

1. The Ethnographical Exhibition of Native American Peoples. The representatives of these peoples will be living in their native habitations on the grounds set apart for the purpose along the eastern shore of the Lagoon immediately north of the Anthropological Building.

2. The general Ethnological Exhibit in the building.

3. The general Archæological Exhibit in the building, and the casts of the several portions of the ancient ruins of Yucatan on the grounds in front of the main northern entrance to the Anthropological Building.

4. The general Exhibit of Ancient Religions, Games, and Folklore.

5. The Anthropological Laboratories on the northern gallery of the building. These laboratories will include special rooms devoted to physical anthropology, criminal anthropology, psychology, and neurology, and will be furnished with instruments and apparatus used in research, which will be carried on during the Exposition. The laboratories will also contain diagrams, charts, and tables illustrating various researches, particularly those relating to the physical characteristics of the native American peoples, and the comparison of the same with other races. There will also be diagrams illustrating the physical characteristics and the mental and physical development of school children in North America.

6. An Anthropological library covering all subdivisions of anthropology and allied sciences. For the purpose of making this library as perfect as possible and to enable students and educators to become acquainted with the mass of literature upon the subject, it is expected that authors, societies, museums, and publishers will contribute their books and papers relating to anthropology or any of its subdivisions, such as archæology, physical anthropology, psychology, neurology, ethnology, ethnography, primitive and ancient religions, myths, legends, folk-lore, languages, primitive art, primitive manufactures, etc., etc. The transactions, memoirs, journals, and proceedings of anthropological, ethnological, and archæological societies and museums, and the special papers ("reprints," "separata") of authors, are particularly desirable. There will be printed as soon as possible a full subject and author catalogue of the library. This catalogue will receive a wide circulation, and as it is intended that it shall be a reference catalogue for students and libraries, the publisher and price of each book and paper known to be for sale in any country will be given. The library will be carefully and properly arranged in book-cases in the room devoted to it, and will be under the special charge of assistants of the department, who will permit the volumes and papers to be referred to in the room and will give information as to their price and how to obtain them of agents, societies, and publishers. It will thus be seen that it is the intention to make known through this library the works of all writers upon anthropology so far as possible and that thousands of persons specially or cursorily interested in the subject will have an unrivalled opportunity of finding just the books and papers they wish to obtain.

The library will, after the close of the Exposition, be placed in the permanent Memorial Museum of Science, which is to be established in Chicago. It is therefore particularly requested that each contribution be sent to the Anthropological Library with a presentation slip stating that it is presented to the Columbus Memorial Museum, and the same will be duly acknowledged by the proper authorities when placed in the Museum Library after the close of the Exposition. In cases that may occur when contributions to the library are sent for use during the Exposition only, all such books or papers must be distinctly indicated by the words "to be returned" written over the name and address of the owner or sender, and all so marked will be returned free of expense at the close of the Exposition. Every book and paper should be marked with the name and postoffice address of the sender. The books and papers should be sent by mail unless too bulky, in which case by express, and should be addressed, World's Columbian Exposition, Department M, Anthropological Building, Chicago, Ill.

GOLDSMITHS INSTITUTE ENGINEERING SOCIETY.

SIR FREDERICK BRAMWELL'S PRESIDENTIAL ADDRESS.

At the opening meeting of the session of the above society the president gave his inaugural address. Mr. Lincham occupied the chair, and, after an introduction, Sir Frederick said:—

Mr. Lincham, ladies, and gentlemen: I am much flattered at being selected as president of the Goldsmiths Institute Engineering Society. I am an old member of the Goldsmiths Company, having been connected with it some fifty years, and am a past-prime-warden. I have to congratulate you upon the progress you have made, and am informed that, though your society has been in existence but a few months, you now number over 100 members. You have two principal objects in view, one of which only has been pursued up to the present, viz., the visiting of engineering works, in which you were accompanied by Mr. Lincham, who has explained that which you were witnessing, a point of very great advantage to yourselves. I may say here that I think you are very highly indebted to the head of Section A for his valuable suggestion in starting the society. That during the season you have visited such places as the Arsenal, the Hydraulic Power Company at Wapping, the Tower Bridge, Messrs. Simpsons' Loam Moulding, Messrs Penn, Maudslay, the Deptford and City Electric Lighting Stations, and one steamboat, the "Dunnotar Castle."

Henceforth you are not only to continue this branch of your study, but you propose to prepare papers for reading and discussion, and to obtain the friendly services of persons competent to lecture upon engineering and cognate subjects. Now I find great difficulty in addressing you. I need not enlarge upon the importance of engineering; your presence shows you appreciate that. I hardly like to give you history, although within my own active work since my apprenticeship there has been so great a change, in mechanical engineering especially, as to afford me means for an ample chronicle.

Perhaps I may be pardoned for alluding to my early work-shop days. There were then no railways to and from the city; the Greenwich Railway was only under consideration. Most engines used steam of no more than 3 pounds pressure. There was no planing machine, no slide lathe. If an engine-crank had to be turned, the pin was tooled first, and then the shaft afterwards, by means of a hanging tool, and the throw was much what it pleased Providence to make it, so that in a double-cylinder engine it frequently happened that the two throws were not exactly the same. Boilers were fed by a feed-head, and if the pressure became greater than three pounds, the water was ejected, and thus became a sort of safety-valve.

The notions regarding steam-pressure were very vague. I have a great regard for a very interesting old book, "Belidor's Architecture Hydraulique," in which I read of a boiler, erected in France, having a heavy superstructure to keep down the pressure, and much the same construction was used in the boiler at York Rd., Charing Cross, which supplied London with water. Sir William Siemens used to say that this load of masonry was clearly for the purpose of providing a large number of missiles in case of an explosion. When quite a child I was taken by my nurse to see the water-wheels at London Bridge, which were also used for the water-supply, and even at that early date engineering had a great fascination for me. Everything then was different. The opportunities for technical learning, other than those from apprenticeship, were simply nil; that is now quite changed. This institute is sufficient to show it. You may learn and learn well, and it would be to your eternal shame if you did not; but I want you, in the pride of your strength, not to deal hardly with the older hands, but to remember that, though they had not the advantages, they made the progress, and must have had very much in them to do this when we consider their resources. One great advantage of instruction in principles is this — aspiring inventors need not attempt impossibilities. Suppose a man were to say, "I have a machine that will produce marvellous results if you will concede for its purposes that two and two make five, as I say they do." His friends would probably call in a doctor or conduct him to a lunatic asylum. But this method of stating the case is not so very absurd. Much labor has been spent on inventions, which were impossible, but where, from want of instruction, the impossibility did not make itself apparent — where the two and two could not easily be seen, which it was endeavored to make into five. The learning of the principles of mechanics will show that you cannot get more work out of a machine than you put into it, and will thus put a stop to useless inventions. Let us consider the connection of the past with the present by the great examples of progress. Boiler pressure has increased from three pounds to 150 pounds, and these pressures have been utilized by engines of continually increased expansion with single, compound, and triple cylinders. The triumphant position of the steam-jacket, though many times questioned, is worth noting. First used by Watt, he does not appear to have been aware of the principle involved. Forced draught, by which I mean a closed stoke-hold (not the closed ash-pit, which is very old); very curiously this adjunct to marine propulsion was seen by me at work in the United States as long ago as 1853. I spoke of it on my return, but no attention was paid to it until the principle found application in torpedo boats. I will read from my note-book for 1853: —

"Oct. 11, 1853. Camden and Amboy railway steamer 'Richard Stockton.' Tonnage 651. Two boilers on each after sponson, machinery made in 1852 by Haslem (?) and Hollingsworth, Wilmington, Delaware. Wheels 22 feet in diameter, 9 feet wide. Boiler $\frac{5}{16}$ of an inch thick, proved to 55 pounds, to work at 39. Actual pressure 25 pounds. Boilers have two fire-places in each; they burn anthracite coal; each one has a powerful donkey working a blower, which is on deck, and which blows into the boiler-room, the door being kept shut, and the stoker under pressure." The object of the arrangement was to prevent a tongue of flame coming from the fire in case the door should be left unlatched.

Large steamers were constructed on most unsatisfactory principles in the early days. Nothing could have been more unlike a box girder or braced structure than the wooden-built ships, but the present double bottoms and iron decks form probably as good specimens of girders as can be made, competent to carry, without straining, their own weight and that of their cargoes, while the points of support are changed at every movement by the force of the waves. I may mention also the great advances in the speed of ocean steamers, and wish I had time to describe carefully to you how much we owe to the late Mr. William Froude, who, by means of his admirable paraffin models, showed how to predict with absolute accuracy the performance of the full-sized vessel. The material employed was very easily worked, and could be remelted for further models.

Another great feature in the engineering of to-day is that of making subterranean communications by means of tunnelling with the aid of shields and compressed air. The Thames tunnel was the earliest of these great works, and the shield was in several sections, so that each could be advanced separately by a screw-jack, but there was no compressed air and the difficulties were very great, for in some places an artificial soil had to be constructed by tipping in clay. I was shown these works when in progress by the eldest Brunel. Compressed air was introduced by Sir Thomas Cochrane (afterwards Earl Dundonald), who took out a patent in 1830 (No. 6018). I knew him very well; he was a clever engineer, but, not being trained, he sometimes made mistakes in detail. His patent was for "Excavating, sinking, and mining," and included "an apparatus for compressing atmospheric air into subterranean excavations, so that its elasticity may counteract the tendency of superincumbent water or moist earth to fill such excavations," and he refers to "the undertaking which is now executing beneath the river Thames at Rotherhithe."

Now let me say a few words about electricity and its present condition. Faraday was the great author, and to him we owe the science of electrical engineering, although his discoveries have been considerably developed by many other great workers, whose names are legion, one of the greatest of these being my late valued friend, Sir William Siemens, who, though he died some eight years ago, I cannot now mention without bitter regret. You have often been told that a little learning is a dangerous thing. This is a great mistake. Learn all you can; it is only a shallow knowledge of everything as your end and aim that is wrong. Sir William Siemens used to say, "Learn one thing thoroughly, and after that a little of everything." The development of practical electricity began with the telegraph, and I remember how astonished we all were when a murderer was captured by its aid; but telegraphy is fast giving way to telephony. Electric arc-lighting was first shown at the Exhibition of 1862, applied to light-houses. Since then it has been much further developed, but the incandescent system of Edison and Swan is, after all, the most useful extension of electric lighting. I do not want to introduce political economy, but when the advance or hindrance of engineering is due to parliamentary interference, the science deserves your study. Several years ago, when the time was ripe for general electric lighting, Mr. Joseph Chamberlain, then president of the Board of Trade, introduced an act to enable electric lighting companies to be formed, but, at the same time, provided that city authorities might buy up their concerns at the end of twenty-one years at the mere cost of the material; while, should they not be then pleased to use their right, it should occur again at the end of every seven years. Only recently a change was made, on the discovery that the act was a direct hindrance to speculation, for in effect it meant, "We will let you run the risk when the scheme is not paying, but will take it as soon as you have made it successful." The vicious principle is still retained, but the same has been extended from 21 to 42 years. The result I need hardly tell you. You see it in the general installation of electricity throughout the metropolis, and electric principles I hardly need describe to you. The dynamo gives a continuous low-tension current, using, in its simplest form, two wires for its transmission, but the three-wire system is one of the most remarkable advances. A high-tension current traverses the mains, and is transformed to low-tension when entering the houses, the saving in copper being thereby enormous, while, by a switch arrangement, we can use the current at will for lighting or for power. But you yourselves saw the largest and most interesting example of this method of distribution when you visited the Deptford generating station of the London Company.

Electric welding is another application of primary importance, using either the two plates themselves as poles, or one plate as pole, and what we might call a "soldering bit" for the other pole. This method is extensively employed at Sheffield for repairing steel castings, and with great success. When I was apprenticed, there was used for similar purposes a metal known as "Beau Monteague" (laughter), and from your faces I gather it has not been entirely forgotten. It was not a method of repairing, however, only one of deceiving. I wish I had time to tell you of present-day steel manufacture, but I will simply say that, whereas it was formerly made in pounds, it is now produced in hundreds of tons.

I am now about to extol myself. The Gifford injector caused very great interest from the first, if only because its action seemed impossible of comprehension. I was myself the first to give a complete explanation of that action without the aid of mathematics. (Hear, hear). My contention was that the whole thing might be summed up in the single word "concentration," and to show this I devised an arrangement by which a head of water left one vessel and entered another, rising almost to the same height, by simply shaping the opposite nozzles with such care as to concentrate the pressure upon the smallest possible area. A similar example is that of an armor-piercing projectile. A blunt-ended shot will be flattened still further, but a hard-pointed one will receive very little deformation in entering the plate.

To close with a few remarks on technical education. For the first eight years of its existence I was chairman of the executive committee of the City and Guilds of London Institution. I am now a vice-president, but have not time to take an active share in the management. I am glad, however, to know of the good that it is doing and of its recognition of merit in those which go up for examination. I am glad to find that, even in this very early period of the existence of your society, Mr. Walter Grant has succeeded in obtaining the bronze medal of the Institute and third position in the country for mechanical engineering; while the same student has obtained a Queen's prize in advanced machine drawing, which is granted, I am told, to only a few top men. (Loud cheers). I have some further notes placed in my hand with regard to the success of other students of Section A, from which I find that in three advanced subjects (steam, mechanics, and machine drawing) there has only been one failure in each, equivalent to 12 per cent, while the grand total of all its subjects represents a success of 82 per cent, a result of a very gratifying character, which is greatly due to the excellent instruction which Mr. Lincham has given you. (Loud and continued applause.)

A vote of thanks was next proposed by Mr. Redmayne, which was carried unanimously.

Sir Frederick briefly tendered his thanks, and the proceedings terminated.

ON THE GROWTH OF THE RATTLE OF CROTALIDÆ.

BY S. GARMAN, MUS. COMP. ZOOL., CAMBRIDGE, MASS.

Since the appearance of the article on "The Rattle of the Rattlesnakes" and its evolution, Bull. Mus. Comp. Zoöl., XIII., No. 10, Aug., 1888, the study of these crotalidæ has been continued with the purpose of securing rates of growth and other particulars not fully determined at the time of publication. As the final report may be delayed for a time it seems proper in this place to refer in advance to several items which have in some extent been questioned by other writers. The point to which attention is specially directed is the acquisition of new joints in the rattle. In regard to this, variations occur in the time, none have been noticed in the method. In all cases observed the growth of a new button, causing the appearance of a new ring or joint, was connected with the process of sloughing. Growth was first detected at the time of the advent of the whiteness in the eye and under the epiderm in general This whiteness was evidence of dermal growth, which on the tail seemingly was prolonged a little after the eye had become clear or until the slough was cast. Possibly the apparent prolongation was due to a mere pushing back and hardening of the newly-grown button. The preparation for sloughing was in each instance preceded by the whitish appearance under the outer cuticle, as was stated in the above-mentioned bulletin. The milkiness, as it might be called, lasted longer on specimens kept in the shade than on others exposed to the sunshine.

A few extracts from notes on several individuals will, without further comment, suggest the results obtained.

The first case is that of a large banded rattlesnake, *Crotalus horridus*, on which the whiteness was visible on eye and button August 17 There was no mistaking the fact that the epiderm of the button was being pushed back to become a section of the looser portion of the rattle. By the 26th of the month the button was becoming darker, though the eye was still somewhat clouded and remained so until the 30th. At this date the eye was bright and clear and the new button had become dark colored and was seen to have pushed back the recent slough as the newest ring or joint of the series. It was not until September 4 that the slough was stripped from the body; it had previously separated from the new ring.

Another case is that of a prairie rattler, Massassauga, *Sistrurus catenatus*, on which the milky appearance was seen September 12. It was then but slight on either eye or button. Two days later it was very intense; by the 19th of the month it had become almost obsolete. Only about half of the new button was visible behind the small scales at that date. This snake sloughed on the 24th The newly exposed button was whitish; it became dark rapidly when placed in the sunshine.

A third case to mention is that of a snake, of the same species as the latter, kept on very short allowance of food, by which no doubt sloughing was much retarded. This one did not show the milkiness until December 11. The whiteness vanished about the 23d, and the slough was put aside on the 31st. It came off nearly entire, the exception being less than half an inch, which remained attached to the anterior edge of the newest ring.

In all cases under observation a new ring has been gained with each sloughing, whether it occurred in the fall, the winter, or the spring. The snakes are still in keeping to determine the greatest number of sloughs in a season and other points. Thus far the later studies have given very little reason indeed for modifying the conclusions published in the above-mentioned article.

CURRENT NOTES ON ANTHROPOLOGY.—XXIII.

[*Edited by D. G. Brinton, M.D., LL.D.*]

The Language of Craniology.

"Sphenocephalic, tetragonic, dolicho-meso-brachycephalic, hypsicephalic, metrio-cephalic, hypo-stegobregmatic, hypsionchobregmatic, cremnoopistocranic, chamelnogathic, euryzicic, chameprosopic, platyrrhine, chameconch, orthognathic, hyperplatopic"!!

In these few and simple words Professor Sergi, the distinguished Italian craniologist, describes a skull from Melanesia. It offers a by no means unexampled specimen of the extraordinary language which writers of that specialty have been revelling in of recent years. They seem to have swallowed the Greek dictionary, and finding its roots of difficult digestion, have regurgiated them in this unassimilable state. Let us appeal to them in the words of Horatio when he listened to the dialogue between Hamlet and Oscar:—

"Is't not possible to understand in another tongue? You will do't, sirs, really."

To make matters worse, a Greek root which satisfies a German, is for that very reason distasteful to a Frenchman. It is enough for one to say *chamaeconch*, for the other to invent *megaseme*. Even German big-wigism has at last revolted against this distressing verbosity. Professor Moritz Benedikt, of Vienna, has published an open letter appealing to craniologists to speak in some less jaw-breaking and pedantic lingo. He addresses it to Professor Sergi, and publishes it in the Proceedings of the Vienna Anthropological Society, December, 1892. May his protest have a wide circulation, and receive an attentive hearing!

Ethnography of Tribes of the Northwest Coast.

Several interesting contributions to our knowledge of the tribes of the Northwest coast have recently appeared. First may be mentioned the report on the Kootenay Indians of southeastern British Columbia, by Dr. A. F. Chamberlain, published with an introduction by Mr. Horatio Hale by the British Association for the Advancement of Science. It deals quite fully with their psychology, social organization, arts, physical characteristics, and language. In the last-mentioned respect they appear to form an independent stock. In the introduction, Mr. Hale discusses some general questions with his customary ability and fairness.

A neighboring tribe, the Shuswap of British Columbia, forms the subject of a careful paper in the Transactions of the Royal Society of Canada by Dr. George M. Dawson. He speaks of their tribal subdivisions, houses, customs, history, and mythology, and adds a long list of place-names with their significations. An excellent map is appended. He agrees with previous writers that their linguistic affiliations are with the Salish proper; but he calls attention to an ancient speech among them, now nearly extinct, apparently from some Tinné influence.

In the same Transactions, Mr. Alexander Mackenzie publishes descriptive notes on implements, weapons, and tools of native manufacture from Queen Charlotte's Island, with illustrations. In an introductory note Dr. G. M. Dawson extols the ability and dexterity of the Haida Indians, which he thinks have not been appreciated by ethnologists. He does not hesitate to claim that the incipient civilization of the Haidas "was higher than that found in any other people of the west coast of North America"; a statement which certainly requires modification.

Points in African Linguistics.

The precise relationship of the various members of the Nuba stock in equatorial Africa has recently led to some discussion in German periodicals. The Nuba stock is not negritic. The features and expression of the face, the shape of the nose, the forms of the skull, place them outside the physical characters of the true Negroes, and assimilate then in spite of their dark color to certain branches of the white race, especially the Semitic. In languages they appear to offer four independent families, one of which includes the Monbuttu, the Nyam-nyam, the Gola, and some others, the credit of defining which belongs to Dr. Friedrich Müller of Vienna, as has been shown in a late contest on the point. The intermediate physical position of this stock lends especial interest to its study.

An important warning in reference to the Bantu languages was sounded at the last meeting of the American Oriental Society by the Rev. Lewis Grout, of Vermont. He points out that the "Comparative Grammar of the South African Bantu Languages," of the Rev. J. Torrend, lately issued in London, takes as its standard the tongue of Tonga or Batonga, which is unquestionably a corrupt and mixed dialect, with many borrowed words and broken-down grammatical forms. Mr. Grout touches here upon a very important point in linguistic study. In approaching the analysis of an indigenous tongue it is extremely difficult to decide which of its dialects should be chosen as the standard — as best representing the parent stock. Yet it is most desirable, essential, indeed, to a successful analysis, that the right choice be made.

On Current Mexican Philology.

It is probable that no more independent linguistic stocks will be discovered within the area of the Republic of Mexico; but there are many within its various states of which we lack information. Within the last few years energetic efforts have been made by the Director-General of statistics, Dr. Antonio Peñafiel, to supply this deficiency. He has caused to be extensively distributed a list of nearly three hundred words to the officials and curas of parishes where the native dialects continue to be spoken, with the request that they be translated into the local idiom and returned. In this manner he has obtained a mass of new and trustworthy material which will enable linguists to classify the many obscure and little-known tongues, the names of which are preserved in the works of Orozco y Berra, Pimentel, and other writers.

It is to be regretted that these lists have not been promptly published in some cheap, accurate, and convenient form. The only instance of an issue of this *Cuestionario Filologico* which I know of is the "Vocabulario Castellano y Nahuatl," by the licentiate Cecilio A. Robelo, which was printed by his own efforts at Cuernavaca. It is very much to be commended, and to call it a vocabulary is to do it scant justice. Each word is traced to its radical, its special uses and synonyms are discussed, and its various significations are explained. If all the *cuestionarios* are filled on this model, American philology will be enriched, indeed, by our Mexican friends.

The Tale Told by the Teeth.

The development of the molar teeth of the human jaw is a history which is claimed to reveal some interesting points in the genealogy of man and the relationship of races. It is now some five years since Professor Cope urged the opinion that the tubercular forms usual in the cusps of human molars point to a reversion to the type of dentition prevailing among the lemurs, and the inference was near at hand that in the discussion of the evolution of the genus *Homo* we had better look toward a lemurian rather than a simian progenitor.

His statements were studied closely by several German writers, and also by Dr. H. F. Osborn of Columbia College, who, in a recent number of the *Anatomischer Anzeiger* (No 24, 1892), presents a summary of results, some of the weightiest taken from his own researches. He shows that the primitive form of the mammalian molar was a single cone, to which all the other cusps have been successively added. Four, five, or six cusps, and various intermediate tubercles, appear on the molars of some of the primates. The tubercles of the human molar may be considered a reversion to the lemurine type, and Dr. Osborn maintains that in comparison the quadritubercular form was a comparatively recent acquisition compared to the tritubercular.

The attempted application of these traits to racial anatomy cannot be said to have resulted in anything definite. It may vaguely be affirmed that in the molars of the lower jaw, which are the more distinctive of the two, four cusps are more frequent in the "higher" and five in the "lower" races. This is the opinion of Dr. Topinard in his latest writings on the subject. He seems to have little respect for the lemurian theory, referring to these as "animaux de transition discordante, à type non arrêté."

Professor Topinard has taken up the subject with his usual thoroughness in an article seventy pages in length in *L'Anthropologie*, December, 1892, entitled "De l'Evolution des Molaires et Premolaires chez les Primates et en particulier chez l'Homme." In this he withdraws somewhat from the position he took in his L'Homme dans la Nature and concedes that the molars must be traced back, step by step, to lemurian forms; but claims that the fundamental types of the molars are identical in man and the anthropoids; that these latter belong to the monkeys; while man as he is at present constitutes a sub order in the general order of Primates.

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THE ALPHABETS OF THE BERBERS.[1]

BY D. G. BRINTON, M.D., LL.D.

THE Berber tribes are called by some writers collectively Hamites, and by others Proto-Semites. From the dawn of history they have occupied most of the area between the Nile Valley and the Atlantic Ocean north of the Soudan. They have, also, linguistic kinsfolk in Abyssinia and in adjacent parts of East Africa. The ancient Ethiopians were of their lineage; Timbuctoo was founded by one of their chieftains, and the extinct Guanches of the Canary Islands were members of their stock. To them belonged the classical Libyans, Numidians, Mauritanians, and Getulians, and in later times petty tribes innumerable, the most prominent of which to-day are the Rifians of Morocco, the Kabyles of Algeria, the Touaregs or Tamachek of the Sahara, the Mzabis, etc.

During two short visits to North Africa in the years 1888 and 1889, I became much interested in the ethnology of this stock, which offers many most interesting problems. The one to which I shall confine myself at present is its methods of writing.

The Berber hordes of to-day, with one exception, employ the Arabic alphabet, though it fails to render some of the sounds with precision. The exception is that of the Touaregs of the Sahara. They employ an alphabet of their own, of great antiquity and disputed origin. They call it *tifinar*, which is a plural from the singular *tafinek*. As in the Berber dialects, the radicals are single or small groups of consonants, invariable, and inflected by vowel changes, we have in *tafinek* the quadriliteral radical *t-f-n-k*, as is held by Rinn; or, if the initial *t* be regarded as a neuter prefix, there will be the triliteral root *f-n k*. The primitive meaning of this root is a sign, mark, or token by which a place or thing is recognized. Peculiarly-shaped stones or ridges, which serve as landmarks, are called *efinagha* (Barth).

Strictly speaking, the word *tifinar* applies only to those letters of the alphabet which can be represented by straight lines; while a number of others, expressed by dots, receive the name *tiddebakin* (Rinn). All letters, whether simple or compound, can be and usually are written by one or other of these methods, straight lines or dots, as is shown by the alphabet presented, from Hanoteau's Grammaire Tamachek. The cursive script, however, permits the use of curved variants in some cases, all of which are shown on the alphabet I submit.

The Touareg alphabet is far from systematic. The order in which the letters are arranged is purely arbitrary; there is considerable difference in the forms of letters in different tribes; there are no vowel-points like those in modern Hebrew, and no accessory signs to represent pure vowels. What is worse, there is no rule as to whether the script should be read from left to right or from right to left, from above downward or from below upward. The assertions made to the contrary by Hanoteau and Halévy are disproved by the documents published by Rinn, which I show. They were written by native Touaregs to native Touaregs The writer sometimes begins at a corner of the page, and proceeds from right to left or from left to right as he pleases; arrived at the further margin, he turns his sheet, so as to go perpendicularly or in any other way that suits him. As the words are frequently not separated, as punctuation and capital letters are unknown, and as the sequence of the lines is not fixed, it is no easy matter to decipher a Touareg manuscript. When a native undertakes the task, he begins by spelling the consonants aloud, in a chanting voice, applying to them successively the various vowels, until he finds the words which make sense (Hanoteau).

Imperfect as this alphabet seems, it is in very extensive use among the Touaregs, both men and women. Barth found that his young camel-driver could read it with ease. Captain Bissuel writes: "A de trés rares exceptions, prés tous les Touaregs de l'ouest, hommes et femmes, savent lire et ecrire." Duveyrier makes a similar statement of the Touaregs of the north.

Most writers, one following the other, have traced the Touareg alphabet back to the Carthaginians, and have sought to identify its letters with those of the Punic writing.

Its history, however, is by no means so easy to unravel. That certain of its letters are identical with the Semitic alphabets is unquestioned; but some of them are not; and those that are alike, may they not be mere loans, or even independent derivatives, from some one common source?

The material to solve these problems must be drawn from ancient inscriptions. These are by no means lacking, and prove that an old Berber alphabet was in use in Northern Africa long before the Christian era; yes, in the opinion of some archæologists, as Collignon and Rinn, long before the founding of Carthage.

These inscriptions are of two classes, the one carved on dressed stones, such as grave and memorial tablets; the other on native rocks, *in situ*, where a smooth surface offered a favorable exposure.

A large number of the former were copied and published by General Faidherbe and have been studied by Professor Halévy. The latter explains most of the letters by the Punic alphabet, and presents transliterations and renderings of the epitaphs. His identifications, however, have not satisfied later students. I find, for instance, that while Halévy's "Essai d'Epigraphie Libyque" was published in 1875, Réné Basset, probably the most thorough Berber scholar living, writes in 1887 in his "Grammaire Kabyle": "Le déchiffrement de ces inscriptions est encore aujourd'hui sujet à contestation, au moins pour le valeur de plusieurs lettres."

This difficulty very much increases when we come to the other class of inscriptions — those engraved on the living rocks. The mortuary epitaphs collected by Faidherbe may be referred with probability to a period two or three centuries before Christ; but the rupestrian writing is of much more uncertain age. Some of it has the patine and other attributes of high antiquity; in other instances it is evidently recent. Examples of it are found in abundance on both slopes of the Atlas range from Morocco to the Libyan Plateau. Unquestionable instances have been reported from the Canary Islands by Dr. Verneau; Barth found them south of Fezzan; Captain Bernard copied some in southern Algiers; last year M. Flamand described a number of stations in southern Oran; Dr. Hamy has made an instructive study of them; and a number of other travellers have added to our knowledge about them. They are often carefully and cleanly cut into the faces of hard rocks, and are thus calculated to resist the elements for many generations.

What is noteworthy about the oldest types of these rock-writings is this: that while they contain some letters which are common to the Touareg, Libyan, and Punic alphabets, they also present a certain number which are not, and which cannot be explained by them. Thus, in the most recent article on the subject, published last year in *L'Anthropologie*, M. Flamand writes that these glyphs show "bien characterisées, des lettres Libyco-Berberes, et aussi des signes qu'il a été jusqu'ici impossible de comparer avec aucun de ces alphabets." The copies of

[1] Read at a meeting of the Oriental Club of Philadelphia, Feb. 9. (See *Science*, Nov. 18, 1892, p. 290.)

these inscriptions which I show will give an idea of some of these unknown signs. They are three in number, and fair examples of hundreds to be seen in the localities referred to. One was copied by Barth at a place southwest of Fezzan; the second by Captain Bernard, near Laghouat; the third by Captain Boucher, near Figuig. While each presents letters identical with some in the Touareg alphabet, or in the Numidian mortuary inscriptions, the majority of the letters belong to neither class.

It is the opinion of some careful students, therefore, and it seems evident, that for a portion of the ancient Libyan alphabet we must look elsewhere than to a Semitic source. The question is a new one; but there can scarcely be more than one answer to it. We must look directly to Egypt, whence the Semitic alphabets themselves must finally trace their origin. Nor does such an answer present the least historic difficulty. Earlier than the twelfth century, B.C., there were direct and much-travelled caravan routes from the heart of the Berber country into Egypt. "I have not the slightest doubt," writes Barth, "that the Imoshagh (Touaregs) are represented in the ancient sculptures of Egypt as the Tamhu and the Mashawash." We are well aware that thousands of Berber soldiers were enlisted in the Egyptian armies in the Ramesside epoch. The high culture they possessed is attested by the catalogue of spoils in the inscription of Merenptah. Unquestionably they became familiar with the various methods of writing in vogue in Egypt at that period.

In his latest work, Mr. Flinders Petrie maintains that the letters of the Phœnician alphabet were derived directly from Egypt; it is quite likely that one or more of the earliest Berber alphabets were also derived directly from the same venerable seat of culture, adopting, in part, signs identical, in part, diverse from the multiform Phœnician alphabets of the earliest epochs. Intercourse with the Semitic traders and colonists led to a greater or less unification of the methods of writing, as has occurred in so many other instances; so that the Libyan alphabet of the third century, B.C., was easily enough mistaken for a daughter, instead of a sister, of that in use by the Carthaginians. But they never reached a complete identity, and as the farther we go back, the greater seems the diversity, the theory of an independent origin appears to be alone that which will satisfy the facts in the case; and this theory has in itself a high historic probability.

The principal works to be consulted, copies of all of which from my own library I lay before you, are the following: —

Faidherbe, "Collection Complête des Inscriptions Numidiques."

Hanoteau, "Essai de Grammaire Kabyle."

Hanoteau, "Essai de Grammaire de la Langue Tamachek."

Halévy, "Essai d'Epigraphie Libyque."

Bissuel, "Les Touaregs de l'Ouest."

Basset, "Notes de Lexicographie Berbère."

Rinn, "Les Origines Berbères."

Numerous articles on the rupestrian inscriptions are scattered through the *Revue d'Ethnographie*, *L'Anthropologie*, etc. As the subject is one, I believe, entirely new to American Orientalists, and as it may possibly prove of considerable significance in the history of the development of Mediterranean civilization, this brief presentation of it will, I trust, lead to further researches.

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 Trinomial Question in Nomenclature.

I WOULD like to say just a word in relation to the article by Mr. C. Michener of San Francisco, which appeared in the Oct. 28 number of *Science*.

Whatever may be the views of others on this point, I maintain that there *is* an ethical side in nomenclature. My article was written largely from that point of view, the matter of "convenience" is of secondary importance.

When an author names and gives a recognizable description of a species, the latter becomes in a certain measure his individual property. (I feel safe in saying that this view is held by many others beside myself.) A later author who attempts to claim this species violates a law of ethics.

Mr. Michener's whole article hinges on this one point: Is there an ethical side in nomenclature? I leave my critics to answer this question. If there is, then the question arises: Shall justice be sacrificed to convenience?

Considering the matter of convenience, there is no point gained, in pursuing the course supported in the above article, which is important enough to warrant this violation of rights. Of the two evils, inconvenience and injustice, we should choose the lesser. We should put up with the inconvenience, which is at best slight. Taking the example cited: If H. and A. have described five species by the name of *malachroides*, then look each one up. It is safe to say that the necessity for doing this will not occur once in ten times. Again, let him who desires to find the characters of *H. malachroides*, H. and A., look at some later work, Greene's for instance, or any other. He will probably find, with little trouble, the genus *Hesperalcea*. If it is contained in some recent paper and he cannot find it, he is not conversant with the literature on the subject; and the sooner he becomes conversant, the better for his work.

The amount of truth which a name conveys depends entirely upon our understanding of what it represents. It is accepted by the majority of the scientific public (I refer especially to zoölogists) that the third term of the trinomial represents the founder of the species. If it were understood to represent the reviser who placed the species in its present generic position, of course Mr. Michener's argument would be valid. I know that the view here opposed is the one more generally held among botanists. But I believe it is growing in disapprobation. The opposite view is almost universally adopted by zoölogists, and is, I believe, the rational and just one. C. H. TYLER TOWNSEND.

Agricultural College, Las Cruces, N. M., Nov. 5.

Notes on the Fauna of the Dry Regions.

IN *Science* for Dec. 23, 1892, my friend, Mr. A. Stephens, records an instance of a captive pocket-mouse (*Perognathus*) living for over two years without water or any food from which any amount of moisture could have been obtained; and, from the fact of water having been offered, it is plain that its abstinence was entirely voluntary.

That many birds and mammals inhabiting the desert regions of the southwest live for many months without any other moisture than that obtained from the food they eat, is well known to those who have studied zoölogy in these regions. And the study of the various sources from which the fauna of the arid plains of New Mexico and Arizona draws its supply of moisture offers a very inviting field.

In the low deserts of these territories rain seldom falls after March or before September. Often nine or ten months pass by without rain in sufficient quantities to form pools or streams where water could be obtained by the birds or mammals of these sandy wastes.

During the summer of 1886 I made my headquarters at a mining camp near the southwestern corner of New Mexico, in the midst of the dry regions. Water could only be obtained from a small spring ten miles west of camp, and no rain fell after my arrival, on Feb. 28, until some time about the last of August.

Birds and mammals were quite plentiful about my camp, many of the former nesting and raising broods of young, which reached maturity and, in some cases, migrated before they made the acquaintance of a drop of water.

In the case of the insectivorous species some moisture was obtained from their food, which was more or less juicy. But the sparrows and seed-eating species must have thought it a "long time between drinks," as their food was of the dryest possible kind.

During the fall, after the various species of cacti had ripened their fruits, I frequently found them torn open by mocking-birds and other species, but whether for the seeds or soft, juicy pulp I could not determine, possibly for both.

Many of the small mammals and rabbits were given to gnawing the inside from the various species of globular cacti, which furnished a large quantity of pulpy material, with plenty of moisture. Several large specimens of these cacti were found that were mere shells. The mice, having entered from below, and without disturbing the position or appearance of the plant, had carried away all but the thorns and woody exterior.

Deer and antelope were rather common on the plains below camp, and, as they were seen daily and some individuals recognized by certain peculiarities, it was plain that if they left the region in search of water, it was not often or for any length of time, but more probably that they drew a large part of their moisture from their food. The different species of cacti and agave were frequently found with large pieces bitten out of them by these animals. The latter plant especially seemed to supply them with a large part of the necessary moisture.

The Indians and Mexicans living in the arid portions of the peninsula of Lower California told me that the rabbits and quail of those regions did not breed during dry seasons, the latter remaining in flocks throughout the spring and summer. This statement was verified by my own observations in the spring of 1887. No young quail or rabbits were seen, though the adults were everywhere abundant.

This habit may extend to other species in this region, as young birds seemed to me to be remarkably rare during the dry season mentioned.

Whether this habit arises from the fear that suitable food for the young may be wanting or that water in larger quantities than is to be obtained would be necessary for their early existence, I am unable to say.

Off the west coast of the peninsula, between 28° and 29° north latitude, are two islands — Cerros and Guadalupe — both of which are inhabited by large herds of wild goats, the descendants of domestic animals placed there by the whalers for the benefit of shipwrecked sailors; there are also quite a number of deer on Cerros.

On both of these islands water is found in small quantities. But during dry seasons this becomes so scarce that the large herds of Guadalupe especially suffer considerably. The sealers of that coast told me, however, that during seasons of little rain the goats drank sea-water and managed to exist until better times. This story was looked upon as a sailor's yarn, without foundation, until endorsed, in part at least, by my brother, who returned from a trip along the coast of the peninsula in June, 1892.

Goats were found on Natividad Island, a small island south of Cerros, which is known to contain no fresh water. As they were out of fresh meat, a few were shot for use on board the schooner, and a kid about one-third grown was captured and taken on board as a pet. Fresh water was offered it, supposing it would be a very acceptable variation to its fare of dry weeds; but, strange to say, after the first sip, it shook its head in disgust, and turned away. Sea-water, however, was accepted and regularly drunk. Gradually it formed a liking for fresh water, and at the end of a month would not pay any attention to salt water. That goats are rather scarce on Natividad would indicate that they did not thrive on sea-water; yet those that were killed by my brother were fat and in every way in good condition.

The story that prairie-dogs have in each colony one or more burrows reaching to water has been widely spread and is probably not without foundation; but that such is the case wherever prairie-dogs are found is by no means true. I witnessed the sinking of a well in southwestern New Mexico, in the midst of a very large colony of these rodents, the supposition being that, where "dogs" were so abundant, water could not be far from the surface. After a depth of over two hundred feet had been reached, the work was given up and the bottom reported as the dryest spot in New Mexico. In sinking to this depth, several strata of tough, slaty clay were cut that would have undoubtedly proved an impassible barrier to any burrowing rodent, had it even penetrated to that depth.

Prairie-dogs are undoubtedly fond of water when it can be obtained. I have frequently, in Colorado, found their colonies near streams, to which well-beaten trails led, and where large numbers were seen drinking daily. But where water is not to be obtained, they seem to be able to subsist upon what moisture they can get from the dry, scanty vegetation of the arid regions in which they live. A. W. ANTHONY.

Denver, Colo., Feb. 7.

Bad-Air Indicator.

PERMIT me to suggest, through your columns, something desirable to be invented if it be within the limits of science to produce it, namely, an automatic and reliable indicator of bad air. I do not in the least know whether such a thing can be made, and must admit that the only chemist to whom I have proposed the matter sees no way to construct it, but it is possible that some one might see his way clear to it. My idea is to have a plain circular disc, which might be made ornamental, which should be one or two feet in diameter, which should be placed on the walls of a room or hall, and the surface of which should be pure white when the air of the room is reasonably pure, but which should become discolored by the presence of bad air, and the color of which should deepen or darken in proportion as the impurity in the air increased. It seems to me that such an indicator, plainly making its announcement before the eyes of all, would be valuable. It may be said that our sensations are sufficient indicators of the presence of foul air, but this, I think, is not so, and the vitiation of the air in many a hall is so gradual and insidious that the great number of people may, without knowing it, be gradually forced to breath air which is most poisonous, and nearly every particle of which — to state the matter plainly — has been previously many times breathed into and out of other people's lungs. Cannot some substance or surface be so chemically prepared as to give this, the above-mentioned, indication? Is not here a good chance for the chemist and inventor? C. H. AMES.

Boston, Mass, Feb. 10.

On Chelydra serpentina.

THE snapping tortoise is not one that appeals to many as an animal of which to make an attractive pet. His appearance and his manner of receiving advances are decidedly against improvement of a reputation that contains little of the good. There is a widespread opinion that he is quite intractable, utterly savage and ferocious, and without redeeming traits. My own ideas on the subject, however, have been greatly modified by the behavior of a seventeen-inch specimen kept in a tank in a corner of one of the rooms in this museum, where he furnished a good deal of entertainment for visitors, during the summer and autumn of last year. The sulkiness brought with him gradually vanished until he began to take food from long forceps; later he would accept meat from the fingers; and still later would come out of the tank for something to eat. Eventually he gained confidence enough to traverse a forty-foot room for a sparrow, a mouse, or a snake that might be offered. He seized the food held out for him in his jaws, turning his head to one side, if necessary, to do so with advantage, then he turned himself about and, high on his legs like a little elephant, with the hinder inch or two of his tail bearing on the floor, marched gravely back to his miniature pond. Sometimes the fur or feathers of prey stood up or covered his eyes so as to prevent seeing distinctly. No matter, the jaws never loosened their grip and their owner blundered along banging against anything in the way till from one side or the other he at last managed to get into the water. Wherever food was given him, his only place to eat it was under the surface in his tank. Firmly held between the jaws whatever he wished to eat was torn in pieces by the claws of his fore feet, or, if too tough for tearing, it was at least reduced to such shape as admitted of swallowing entire. After a time "Snap," as he was named, became rather too familiar, coming out of his retreat at all times, whether called or not, whenever one entered the room. If a student came in and took a seat at a table, Snap was pretty sure to plant himself under the chair or at the feet of the newcomer to remain for an hour, more or less, as pleased him. Pushed aside, he either lay quiet or rose and stalked back to his own corner as if offended. Some might take it that his conduct indicated a fondness for company, or the possession of grateful feelings, or even an affectionate disposition; but it is not necessary in explanation of Snap's deportment to go beyond his desire for food. In the satisfaction of his hunger his interest in human beings departed. His doings are here put forward in support of nothing except that with proper treatment the snapping tortoise, one of the lowest and least likely of the tortoises, may lose his timidity, his ferocity disappearing in consequence, and become susceptible of a considerable amount of training. S. GARMAN.

Mus. Comp. Zoöl., Cambridge, Mass.

Snow Rollers.

THE article of Dr. Claypole, in *Science* No. 522, on "Snow Rollers," recalls what I saw a few years ago. The condition was like that described by Mr. Hart. There was a smooth crust of snow on which a light fall of damp snow fell. The wind changed suddenly to the north, blew hard, and I saw scores and perhaps hundreds of these snow rollers forming. The wind simply blew them along and they formed just as boys roll snowballs. I feel sure such occurrences are not uncommon here. These rollers were several inches in diameter. D. S. KELLOGG.

Plattsburgh, N. Y., Feb. 9.

The Antiquity of Man.

IN "Current Notes on Anthropology.—xxii." (*Science*, Feb. 10, 1893), Dr. Brinton has referred to certain discussions that took place at the meeting of the German Anthropological Association last August. Not having yet seen the report of that meeting, I cannot judge how far Dr. Brinton may have been misled by his authorities, but I wish to enter a decided protest as to two statements made by him. Let me premise by saying that it seems to me that it behooves Americans to maintain a strict neutrality in the international jealousies between the Germans and the French.

In regard to the importance to be attached to the celebrated "Neanderthal skull," it seems to be sufficient that it has been adopted by De Quatrefages and Hamy to set all the Germans, except Schaffhausen, against it. I did not expect, however, to find an American using such language as this about it: "The Neanderthal skull . . . was not dug up at all, but was picked up in a gully, which had been washed in the mountain side, and came from dear knows where. Probably there had been an old graveyard further up the hill, but by no means one in quaternary times." I will quote the exact language of Dr Fuhlrott, the discoverer, describing the circumstances under which it came to light. "In a wild ravine, called the Neanderthal, cleft in the Devonian limestone, is a small cavern, about eleven feet long, ten broad, and eight high, opening upon an almost vertical wall of rock about sixty feet above the level of the stream [flowing through it]. . . . The ravine has been quarried for marble. In the cavern is a bed of clay, a glacial deposit, almost as hard as stone. In this clay, at a depth of two feet, in August, 1856, a human skeleton was discovered," etc. (Hamy, "Précis de Paléontologie Humaine," p. 237). The real question in regard to these human remains is, in the words of Schaffhausen, "Whether the cavern in which they were found, unaccompanied with any trace of human art, were the place of their interment, or whether, like the bones of extinct animals elsewhere, they had been washed into it" (*Natural History Review*, 1861, p. 172). In all serious discussions it is well to stick close to the facts of the case.

The other subject, about which I dissent from Dr. Brinton's conclusions, is in regard to what he calls "the delineation of a mammoth on a bone from the Lena cave in the south of France. This was not discussed, being probably considered of questionable origin." I must own that at first I was somewhat puzzled to know just what Dr. Brinton meant by "the Lena cave in the south of France." But on looking into the recently published English translation of the Marquis de Nadaillac's "Prehistoric Peoples," p. 119, Fig. 38, sure enough, I found an engraving representing a "Mammoth or elephant from the Lena cave." Now this remarkable designation is not due to the author, who calls it a "Mammoth ou elephant de la Lena," referring to the well-known discovery in 1799 of the body of a mammoth, imbedded in the frozen banks of the river Lena, in Siberia. I suppose that scarcely any relic of antiquity is better known to pre-historic archæologists than the remarkable delineation of a mammoth upon a plate of fossil ivory, discovered by Edward Lastet, in May, 1864, in the cavern of the Madelaine (Dordogue), in southern France. It was made in the immediate presence of M. de Verneuil and of Dr. Falconer, and an account of the circumstances of the discovery was given by him in a letter to Milne Edwards, published in the *Annales des Sciences Naturelles*, 5e. ser., T. iv. (Zoöl.), 1865, pp. 353–356. That even international jealousy should "question its origin" surpasses belief. HENRY W. HAYNES.

Boston, Feb. 16.

Birds in Severe Cold Weather.

DURING the recent severe cold weather, as one of the high-school students was on his way through the belfry of the building to hoist the weather signals, he discovered a small bronze owl perched above one of the windows. It had evidently been drawn thither by the heat from the chimneys and pigeons which frequent the ventilators. On being captured by the janitor, on the day following, the bird made no resistance. It was put into a cage, to be kept for the zoölogy class. It lived but one brief day, and it was found to be emaciated and evidently died of weakness and sheer exhaustion. The taxidermist who stuffed it said that it was only one of a large number recently brought to him as victims of the cold spell. Many were found frozen in barns, and had been driven by the cold from the woods to the city.

Large numbers of snow-birds, crows, as well as English sparrows, were hovering about grain elevators, the glass works, and other similar buildings for warmth and food all through the cold period. The gathering of birds about warm chimneys, etc., in such large numbers was something unusual.

E. R. WHITNEY.

Binghamton, N.Y.

Miocene Group of Alabama.

SINCE sending you a contribution on the Miocene Group of Alabama, Dr. Wm. Dall of the Smithsonian, to whom the fossils collected had been submitted, has returned his report, naming the most of them and declaring his opinion, that they are rather of the older than a younger Miocene. This will better suit the geographical position and other facts detailed of the Grand Gulf. His final determination will be published in the Alabama Report. LAWRENCE C. JOHNSON.

Meridian, Miss., Feb. 13.

Mule-footed Hogs.

MR. J. F. RITTER of Higginsville, Mo., sends me a hog's foot, which to me is something new. It has the two larger hoofs united into one. The bones above are separate but the hoofs wholly united. He states that a farmer of the vicinity has a drove of these mule-footed hogs. By crossing breeds he has some with two cloven feet and two mule feet. I should like to know whether this is a common occurrence, or is it something new? JNO. H. FRICK.

Warrenton, Mo., Feb. 11.

BOOK-REVIEWS.

A Manual of Bacteriology. By GEORGE M. STERNBERG, M.D. New York, William Wood & Co. 886 p. 8°. $7.

THE results of the bacteriological investigation of the past decade, when massed in a huge volume like the one before us, are calculated to arouse the keenest admiration for the talent and industry that have produced them. Even in this period of breakneck *temps* in all lines of human activity and thought the progress of bacteriology seems to the world at large truly marvellous. Every year, we may almost say every month, witnesses some discovery of untold practical value. If a last word had been needed to convince the "practical man" of the ultimate advantage to the race of "pure" science and "pure" investigation that word would have been added in these latter days by the development of the science of bacteriology. To have given to the world for the first time a rational theory of infectious disease, and to have indicated the therapeutic possibilities of the future are achievements that may well make the last quarter of the nineteenth century memorable in the history of human progress.

It is eminently fitting that Dr. Sternberg, who has himself done much to increase our knowledge of bacteriology, and who was one of the pioneers in the work in this country, should give to the English-reading public their first adequate survey of the bacteriological field. His manual at once takes its place as the standard bacteriology in the English language.

The bulky volume of 886 pages is divided into four parts, the first treating of classification, morphology, and general bacteriological technology; the second of general biological character; the third of pathogenic bacteria, and the fourth of saprophytes. An invaluable bibliography, covering over 100 pages, and an index conclude the volume. The press-work is on the whole excellent, but we must enter our protest against the thickness of the paper used. A thinner paper would have given even greater satisfaction to the eye, while its use would have considerably reduced the awkward size of the book. The use of needlessly thick paper, however, is so common a failing of American bookmakers that it is perhaps hypercritical to bring it up in this instance. The plates and text figures are executed in an unusually satisfactory manner, and the photomicrographs are of the high degree of excellence to be expected from one as expert in the technique of photomicrography as the author of this book.

Among the most timely and practical portions of the manual may be mentioned the chapters on antiseptics and disinfectants, the influence of physical agents upon bacteria, the practical direction for disinfection, etc. Lengthy quotation is made from the Report of the Committee on Disinfectants appointed by the American Public Health Association, principally to keep before the public the high merit of chloride of lime as a ready and reliable disinfectant. Reference is made, also, to the use of fresh bread for rubbing down the walls of an infected apartment. This method is based on experiments of Esmarch, which seem to indicate that this is the most reliable way of removing bacteria from the walls and ceilings of infected rooms.

A long and studied chapter is devoted to the consideration of the vital questions of susceptibility and immunity. Dr. Sternberg, while disposed to accord to phagocytosis an important rôle in some diseases, is profoundly impressed — as are most bacteriologists — by the remarkable evidence adduced during the last few years in support of the "anti-toxine" theory. It is becoming more and more probable that Metschnikoff's brilliant phagocyte theory embodies at most only a partial explanation of the facts of immunity. "The experimental evidence detailed," says Dr. Sternberg, "gives strong support to the view that *acquired immunity depends upon the formation of anti-toxines in the bodies of immune animals.*"

The sections devoted to the description of such bacteria as have a recognized pathogenic significance are compiled with the fullest reference to recent investigations. Some students may, however, wish that the wealth of material had been more critically arranged and more exhaustively indexed.

A great boon to the student of bacteria from the botanical and systematic side will be the descriptions of the common bacteria of air, water, and soil. Only those who have attempted to compare and identify forms encountered in every-day experience are aware of the labor involved in the compilation of these data. Dr. Sternberg's work ought to give a strong impetus to the movement to bring order out of the existing chaos of vague "species" and vaguer "forms."

As is well-nigh inevitable in a book covering so much ground — and ground, too, that is shifting under one's feet — various errors of omission and commission are apparent. In the first place, it is evident that the index to a work of such magnitude should be thoroughgoing and should not shrink from numerous cross-references. The fact that the index before us contains under the heading "Cholera" no reference to the pages dealing with Asiatic cholera (pp. 500–509), a topic which at present is always with us, indicates opportunities for expansion. The reader who turns the pages and sees something about "alexines" (p. 261) and something about "splenic fever" (p. 327) will find in the index no entry under either of these heads.

Among oversights in proof-reading may be mentioned the substitution of "Chamberlain" for "Chamberland" (pp. 57–59), the use of "aerobic" and "anaerobic" for the more usual nouns "aerobe" and "anaerobe" (pp. 78–83), "micrography" for "micrographie" (p. 8), etc. On page 237 is a singularly involved translation from a memoir by Pasteur. The following sentences fairly represent the style: "The fowls are then in the constitutional state of fowls not subject to be attacked by the disease. These last are as if vaccinated from birth for this malady, because the fœtal evolution has not introduced into their bodies the material necessary to support the life of the microbe, or these nutritive materials have disappeared at an early age."

These blemishes, however, do not seriously mar the general excellence of the manual. It is to be hoped that Dr. Sternberg may see his way clear to the preparation of successive editions of this valuable work. In a science that is advancing so rapidly as bacteriology, new facts are constantly coming to light and compelling frequent revision of our views. Dr. Sternberg has brought the present volume well up to the latest researches and thus encourages us to hope for a second edition as soon as the progress of bacteriology shall demand it.

Discussion of the Precision of Measurements. By Silas. W. Holman, S.B. New York, John Wiley & Sons, 1892. 176 p. 8°. $2.

Professor Holman, perhaps even more than the average physicist of experience in experimental work, has made a specialty of the science of exact measurements. His work, like that of Dr. A. M. Mayer and of Dr. Rowland, has involved, more than is common, the application of refined methods of determinations of quantity to the investigation of those insensible physical phenomena which ordinary modes of measurement are incompetent even to reveal; methods formerly little known or practised in this country, but now familiar to the younger physicists through the work of these leaders in this department of research. In the volume before us are collected a series of articles originally prepared for the *Technology Quarterly* and *Electrical Engineer*, revised and given more complete and formal shape for permanent preservation, and for the use of students and their instructors, both in pure physics and in the applied science of the engineer. These studies are valuable, not only as giving useful knowledge and power of accomplishment of professional work, but as stimulating the young aspirant for learning and reputation and giving him an attitude of mind in itself desirable and fruitful of good result. As remarked by its author, "An experimental result whose reliability is unknown is nearly worthless. The grade of accuracy of a measurement must be adapted to the purpose for which the result is desired. The necessary accuracy must be secured with the least possible expenditure of labor. These statements apply no less to the roughest than to the most elaborate work which the engineer is called upon to perform; they are no more true of refined scientific research than of ever-day engineering and industrial practice." The book is thus of especial value to both classes, whose methods, indeed, are daily becoming more and more alike in their refinements, and in their purposes and applications. In modern researches, especially, in the development of the phenomena underlying the operation of the steam-engine, in the construction of the dynamo-electric machine, in the transfer and transformation of energies, of whatever kind, the contemporary engineer and physicist are working together, and sometimes each doing important work in the special field reserved to the other. Especially is this the fact in electrical physics, in which branch the department of pure science occupied by the physicist and that of applied science which constitutes engineering, blend insensibly, and their work is performed, within a large area of boundary territory, by members of both professions alike. The electrician is sometimes confounded with the electrical engineer, and the reverse. But whether the reader is proposing to work in the department of science or in that of construction, Dr. Holman's work will prove a most useful and instructive aid. Direct measurements and the theory of errors, the method of least squares and the establishment of criteria, indirect measurements and the best ways of planning their applications, estimates of precision and approximation in the solutions of the most important problems, illustrations of good work, with instructions for special cases, as for calibration of instruments, measurements of efficiency, and other similar matter, make the book one which the engineer and the physicist alike will find valuable, and they may place beside Kohlrausch as an authority, and as a useful supplement, if not a substitute, to that standard work.

The work of the publishers is, as usual, well done. We notice the imprint of Drummond, as its compositor and electrotyper, and take it to be an assurance of careful work in composition, and especially in the mathematical portion of the work. Supplementing the proof-reading of so accurate an author, it gives comforting assurance of freedom from those usually too frequent errors which annoy the reader of the first edition of a work of this kind.

Seventh Annual Report of the State Board of Health of the State of Maine, 1891. 399 p. 8°.

BY far the greater part of this report is devoted to the consideration of school hygiene and school-houses in a paper by Dr. A. G. Young, secretary of the Board. This interesting compilation should prove of value in stimulating reform in school methods and school buildings. It clearly and forcibly presents those fundamental principles of individual and public hygiene about which there is substantial agreement among sanitarians. It is humiliating to have to believe that too often those having immediate charge of such matters either disregard these principles or are ignorant of them altogether. Reform can be brought about only by adding line to line and precept to precept.

In the reports of the local boards of health it is observable that cases of typhoid fever occur with ominous frequency in the reports of the small towns where well-water is used for drinking.

The Mound Builders, Their Works and Relics. By Rev. STEPHEN D. PEET, Ph.D. Vol. I. Chicago, Office of the American Antiquarian, 1892. 376 p. 8°.

IT appears from the preface that this is the first of a proposed series of five volumes relating to the ancient history of the area of the United States. The author is well known to students of that branch as the founder and editor of the *American Antiquarian*, a specialist's journal, which has survived for many years, and is a repertory of much valuable information.

In several respects Dr. Peet's opinions about the mound-builders differ from those current in Washington or Boston. To him, "There was a mound-builders' age in this country as distinctive as the Neolithic age in Europe" (p. 31). This age "began some time after the glacial period and ended about the time of the advent of the white man" (p. 34). Geographically, he limits them to the Mississippi Valley, but nevertheless attributes to them the mica mines of South Carolina, the shell-heaps of Florida, and the rock-inscriptions wherever found. He is not in sympathy with the theory that the mound-builders were the ancestors of any of the natives met by the early explorers, but believes they had a civilization and a religion of their own, not to be identified with those of the Redskins of later date. He thinks it likely that the much-discussed "elephant pipe" and "Davenport tablet" attest their knowledge of alphabetic signs and their familiarity with the mammoth and the mastodon; and perhaps he is not wrong when he asserts of these relics (p. 47), "The evidence in their favor is certainly as reliable as that which has reference to the rude stone relics which have been described in Wright's 'Ice Age.'" He himself is not quite convinced that there were any palæolithic people in the Mississippi Valley,—in which he is in accord with some very recent debaters of that question. He says (p. 36): "We imagine that the mound-builders were the first people who occupied the territory after the close of the glacial period." Whence they came he answers as follows: "The same race that built up the ancient cities of Mexico pushed eastward and colonized the Mississippi Valley" (p. 112).

Having solved to his satisfaction these questions, Dr. Peet proceeds to describe at length, and in part from personal observation, many of the mounds, enclosures, earthworks, implements, ornaments, and other relics which he attributes to this mysterious people. He devotes chapters to their religions, their "water cult,' their "solar cult," their symbolism, and their sacrificial rites.

Much of the work, most of it, we believe, has already appeared in the pages of the *American Antiquarian;* but those who sympathize with the opinions of the author will doubtless be pleased to have his contributions collected into a convenient form. He is unquestionably an earnest and honest student of the facts before him, and the conclusions he reaches should, therefore, receive careful consideration.

Some Strange Corners of our Country: The Wonderland of the Southwest. By CHAS. F. LUMMIS. New York, The Century Co. 270 p. Illustrated. 12°.

FOR those readers who have read but a few books of travel on the Southwest, this snug little volume will be quite a revelation. The contents of the twenty-two chapters scarcely contain anything that has been written or sketched before, except a few pages on the Moqui snake dance and Indian superstitions. The thoroughness of his familiarity with Pueblo customs and folk-lore is only equalled by the graphic qualities of his style. In looking about "the strange corners" which the author describes, we are first attracted by a prairie-dog hunt, to which the Navajo Indians resort to fill their larder. White people of the Southwest never think of killing this rodent for food, because it is so difficult to attain with a rifle-ball; but these natives utilize abundant downpours of rain to conduct the floods into their tunnels, and afterwards haul up their dead bodies for a feast. To get rid of the prairie-dog plague, people have proposed to kill them with poisoned apple-quarters. The belief in witchcraft is as potent among the whites and Indians of New Mexico as it ever was during the Middle Ages. Manslaughter is committed for any act arousing even the suspicion of witchery, and the fact that one-half of the Isleta people are wizards and witches speaks loudly enough. The "finishing an Indian boy" shows principles of education in full force now, which our northern Indians began to drop as early as a century ago. In the chapter, "The American Sahara," the wide waste is delineated in colors none too sharp or cruel. Lieutenant Wheeler is mentioned by mistake as its earliest explorer instead of Lieutenant Whipple. The marvellous wealth of objects presented in Lummis's volume will attract ever and again the class of readers and tourists which seeks instruction rather than pleasure in books of travel, and they will hold it dear as a publication of really scientific value, standing far above most of the productions of our present sensation-loving period of literature.

"The Wanderings of Cochití" is another very interesting sketch from our "Wonderland" on the upper Rio Grande. It is printed in the *Century Magazine*, January, 1893, and describes and also pictures in photographic reproductions the people, customs, history, and scenery of Cochiti, one of the Quéres pueblos of northern New Mexico, and the celebrated gorge of Tyu-on-yi and its rock-carvings in the vicinity of that pueblo. The scene of Bandelier's archæologic novel, "The Delight-Makers," is placed in that locality.

First Steps in Etruscan. By F. W. NEWMAN. London, 1892.

The Etrusco-Libyan Elements in the Song of the Arval Brethren. By D. G. BRINTON. Philadelphia, 1893.

THESE two pamphlets are the latest contributions to the study of the Etruscan problem. The first is written by the eminent and venerable emeritus professor of University College, London, now close to ninety years of age. It is worth while to find a man willing to take "first steps" in any branch of learning at that time of life. The questions he examines are: By what route came the Etruscans into Italy? He inclines to believe that they came by sea from Asia Minor, and not across the Alps from the northwest, as Taylor teaches. The Etruscan alphabet he considers far older than the Lycian. The Etruscan numerals on the celebrated Toscanelli dice he reads: 1, *mach;* 2, *ki;* 3, *zal;* 4, *sa;* 5, *thu;* 6, *huth;* agreeing in this with Taylor, but at variance with Professor Sayce, who, in the *Academy*, Oct. 15, 1892, prefers the following sequence: *makh, huth, sa, ki, thu, zal.* Professor Newman does not think the Etruscan language either Aryan or Semitic, but does not proceed farther in its identification, indulging himself in this connection with the following comment on the procedures of another Etruscan student: "Mr. Isaac Taylor treats all languages outside of these two systems as if so specially allied, that he may at pleasure interpret the vocables of any one from any other, and this however different the ages of the two." Other subjects treated are Etruscan concord, words for bronze and brass, attempted translations of epitaphs, the meaning of *kle* and *kal*, etc. Most of this he frankly calls "guessing"; but it is guessing by a method.

The second pamphlet is a reprint from the Proceedings of the American Philosophical Society. Dr. Brinton has published various papers intended to show some ethnic affinity or cultural connection between the Etruscans and the Libyans. Here he takes up the venerable song of the Fratres Arvales — probably the oldest literary monument of Roman antiquity — and seeks to show the indications it presents of a connection with the Berber religions of North Africa. Of course, much of his argument turns on the third line of the song:

Satur fufere Mars limen sali sta Berber;

for which he accepts the rendering of Professor Michel Bréal:

Sata tutere, Mars; clemens satis esto, Berber.

Berber, he points out, is but the reduplication of Ver or Ber, whom Varro mentions as the chief divinity of the Etruscans; a deity who under the same name occupied the same position in the Libyan pantheon, and from whom the name Berber is derived, as well as the word Africa (A-fer-ica). The coincidence, if it is nothing more, is a most curious one, and it would certainly seem that the Etruscans borrowed their gods from Africa, if they did not come from there themselves.

Theory of Structures and of Strength of Materials. By Henry T. Bovey. New York, J. Wiley & Sons, 1893. 817 p. 8°. $7.50.

Canadian authors have been neither numerous nor productive, hitherto, and especially in the fields of science. Sir John Dawson and the able men of the Dominion Surveys, in science, and Goldwin Smith, in history, nevertheless, have led a small body of able men in the performance of work which is most creditable to that now practically independent nation. The appearance of a new work by a Canadian writer, especially in the department of applied science, is thus a somewhat important event; and the volume here offered us will receive a hearty and appreciative welcome by all who are familiar with the standing and ability of its author, and with the work accomplished by him, both professionally and in the development of technical education in his own country. The work itself is an extension, with revision, of the smaller work on Applied Mechanics issued by its author some years ago. It has the form usually considered appropriate to a work of its kind, intended for the use of classes in engineering, in the higher class of schools, such as that of McGill University with which Professor Bovey is connected. It treats of framed structures, their stresses and strains, and their materials, of earthwork and retaining walls, of friction, and of the various forms of bridges and other constructions of the engineer and the architect. The book gives more of modern and exact data than is usual in works of this sort, written, as they are apt to be, by writers drawing upon literature, rather than recent research, for their facts and principles, and unfamiliar, through practical experience, with the actual work of the profession which they assume to instruct. We find here the records of the latest investigations relative to the strength and working qualities of materials, the laws of friction, solid, fluid, and "mediate," and investigations of the direction and magnitude of stresses in the mem-

CALENDAR OF SOCIETIES.

New York Academy of Sciences.

Feb. 27.—H. Carrington Bolton, Progress of Chemistry since the Fifteenth Century, B.C., as Depicted in Apparatus and Laboratories.

bers of structures variously strained. Rankine's unique studies, and the graphics of that author and of continental writers, find illustration and useful application in intelligible and practically available shape; and the methods of connection of parts in practical construction are given in a form likely to meet the approval of the practitioner, as well as of the learner. Tables of constants for use in connection with computations of proportions of parts, and good illustrations, are distributed throughout the book. The work is somewhat extensive, even for students in engineering schools, and will prove valuable for office use as well as in the classroom. We observe that it is dedicated to Mr. Wm. McDonald, the generous donor of the new engineering buildings and equipment of McGill University; a graceful and well-deserved compliment to a man who has done more, perhaps, than any other citizen of Canada to promote this essential element of modern progress in his native State. The composition and printing are excellent; but the paper is thin, probably designedly so, in view of the fact that the volume is a bulky one at best. The book is well worth its price.

AMONG THE PUBLISHERS.

THE Century Co. is about to publish "A Handbook of Invalid Cooking," by Mary A. Boland, instructor in cooking in the Johns Hopkins Hospital Training-School for Nurses. The book is intended not only for nurses in training-schools and private practice, but for all who care for the sick. Besides recipes, menus, suggestions for the proper feeding of children, etc., a part of the book is devoted to "Explanatory Lessons," wherein the various food principles are described, with chapters on Nutrition, Digestion, Chemical Changes in Food, etc.

— J. J. Audubon, the great naturalist, wrote, many years ago, the story of his youth for his children. It was found accidentally in an old volume where it had long been hidden, and is to be printed for the first time in its entirety in *Scribner's Magazine* for March. The youth of Audubon was most romantic, and at times exciting, and his story of it is told with an ingenuous charm which makes it as interesting as a novel.

— Professor Henry Drummond will deliver the Lowell lectures at Boston this spring. The subject will be "The Evolution of Man." Professor Drummond has not yet decided as to the date of the publication of these lectures, but has taken steps to protect his copyright in America.

— Professor William Holms Chambers Bartlett, the author of "Treatise on Optics" (New York, 1839), "Synthetical Mechanics" (1850), "Analytical Mechanics" (1853), and "Spherical Astronomy" (1855), died at his home in Yonkers, N.Y., on the 11th of February, aged eighty-nine.

— Instances of the recognition of the claims of science by the general press are always worth chronicling. It is therefore not without interest that we notice that the *Queenslander* (a Brisbane weekly) is issuing a series of extended descriptive articles on the Butterflies of Queensland, the work of an entomologist writing under the *nom de plume* of "Aurelia." This, we believe, is the first attempt to accomplish a connected account of Australian Rhopalocera, and, as Queensland contains by far the larger proportion of the species inhabiting the Australian sub-regions, these contributions to science are of especial significance.

— Charles Scribner's Sons are preparing a novel and interesting contribution to the World's Fair in the form of an "Exhibition Number" of *Scribner's Magazine* to be published simultaneously with the opening of the Exposition at Chicago. It is not proposed that the text shall relate chiefly to the Fair, but, on the contrary, the leading writers and artists have been asked to contribute to the number what they themselves think will best represent them. The pages of text and illustration will be largely increased, and the appearance of the number is likely to be looked for with eagerness by all readers interested in the work of American magazines.

Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

For sale, or for exchange for books on medicine or surgery, new editions only, a large geological library, containing nearly all the State and Government Reports since 1855. Will be pleased to answer letters of inquiry and give information. Address R. ELLSWORTH CALL, Louisville, Ky.

For exchange.—Slides of Indian Territory Loup Fork Tertiary Diatoms for other microscopic fossils. Address S. W. WILLISTON, Univ. of Kansas, Lawrence, Kans.

For exchange.—Will exchange an "Ideal" Microscope of R and J. Beck, London, 2 eye pieces 3 objectives, 3 inch, 1 inch, 1-6 inch; bull's eye condenser on stand, substage condenser, mechanica stage, etc., for any of the leading makes of type writers. Particulars by mail. DELOS FALL, Albion College, Albion, Mich.

Sale, or exchange for similar material; Diatoms (Isthmia nervosa), unmounted, from San Francisco Bay. M. J. ELROD, Bloomington, Ill.

For sale or exchange.—I have a few copies of my translation of "Strasburger's Manual of Vegetable Histology, 1887," now out of print, which I will send post-paid for $3 or for one dozen good slides illustrating plant or animal structure. Address A. B. Hervey, St. Lawrence University, Canton, N. Y.

The undersigned has the following specimens to exchange for crystals of any eastern or foreign localities or Indian relics: tin ore, metacinnabarite, stibnite, garnierite, calenanite, hanksite, ulexite, rubellite, lepidolite, blue and green onyx, Cal. pineite, aragonite on chalcedony, cinnabar, double refracting spar, clear and clouded, and others. J. R. Bush, care of General Delivery, Los Angeles, Cal.

For sale or exchange.—A private cabinet of about 200 species of fossils, well distributed geologically and geographically. Silurian, about 40; Devonian, about 50; Carboniferous, about 80; others, about 30. Frank S. Aby, State University, Iowa City, Ia.

For exchange.—Minerals, fossils, F. W. shells, land shells, native woods, Indian relics, two vols. of Smithsonian reports, odd numbers of scientific magazines, copper cents, etc., for good minerals not in my collection, good arrow- and spear-heads and natural history specimens of all kinds. Correspondence solicited with list of duplicates. G. E. Wells, Manhattan, Kan.

Wants.

CAN any one inform me as to the age to which cats have lived? I have one twenty years old. Edward D. Webb, 132 W. Eighty-first St., New York.

WANTED — Second-hand. Foster's Physiology, Balfour's Comparative Embryology, Claus & Sedgwick's Zoology, Flower's Osteology of Mammalia, Vine's Physiology of Plants. Please state editions and prices asked and address Richard Lees Brampton, Ontario, Canada.

WANTED.—American Journal of Conchology, seven volumes. Parties having these for sale will please address the undersigned, stating condition and price. R. Ellsworth Call, Louisville, Ky.

A GRADUATE ENGINEER will give instruction evenings in geometry, trigonometry and surveying, mechanics, physics, mechanical drawing and general engineering construction. Five years' experience in field and editorial work on engineering journal. References furnished. C. S. H., 102 Tribune Building, New York.

WANTED.—By well-qualified and experienced science master and associate of the Royal School of Mines, London, aged 26 (at present in England), a mastership in technical college or university for any of the following subjects: Engineering sciences, geology and mineralogy, physics, chemistry and metallurgy, etc., etc. Can provide excellent references and credentials. Apply, J. G., 17 Sussex St., Rochdale, England.

A GRADUATE of the University of Pennsylvania and a practical mineralogist of twenty years' experience desires to give his services and a cabinet of 25,000 specimens, all named, with about the same number of duplicates, in minerals, crystals, rocks, gems, fossils, shells archæological and ethnological specimens and woods to any institution desiring a fine outfit for study. The owner will increase the cabinet to 50,000 specimens in two years and will act as curator. Correspondence solicited from any scientific institution. J. W. Hortter, M.D., Ph.D., San Francisco, Cal., General P. O. Delivery.

SCIENCE

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SCIENCE

NEW YORK, MARCH 3, 1893.

LAWS AND NATURE OF COHESION.

BY REGINALD A. FESSENDEN, LAFAYETTE, IND.

In a previous note (*Science*, July 22, 1892, and *Elect. World*, Aug. 8, 1891) a number of reasons were advanced for believing that cohesion is due to an electrostatic force, and it was shown that the results predicted by such a theory agree very closely with the results of experiment.

This theory was, however, only extended to the phenomena of rigidity, elasticity, and tensile strength. It was purposed to follow it with another note on the phenomena of conductivity, surface tension, solution, refraction of light, and compression of gases. Pressure of other work and the necessity of making experiments to determine some doubtful points, will prevent such publication for some time, and it was therefore judged best to give a short preliminary statement of a few of the results so far obtained.

I. Relative closeness of the atoms. It appears to be generally considered that the atoms are at distances from each other which are large in comparison with their diameters, even in the solid state. As an example of the extent of this belief may be mentioned the fact that in a recent article on magnetism, Mr. Steinmitz made the statement that Professor Ewing's theory could not be correct, unless the atoms were close together, but as they were far apart, his theory must be wrong. This conclusion has not been attacked up to the present time But the facts are that all our evidence points the other way, and it is almost absolutely certain that in the solid state the distance between the centres of two neighboring atoms is almost the same as their diameters.

For instance, from Van der Waals' equation we have, at the critical point:—

Volume of gas = 12 times the volume of the atoms themselves, or, the distance between the centres of two atoms is 2.3 times the diameter of a single atom. And this is just at the critical point, so that from the curves of volume, pressure and temperature, the solid elements must have a volume of, at the most, six times that of the atoms themselves, reducing the distance between centres to 1.8 times the atomic diameter.

Again, when a body is at absolute zero it is extremely difficult to conceive why the atoms, having no kinetic energy and the cohesive force still in existence, should not join together so closely as is possible, i.e., till they touch. (We may discard the old "force point" atom as obsolete and without reason for existence, all modern research and theory being in favor of the idea that atoms have most exact and well-defined boundaries.)

If, then, the atoms of silver in the solid state at 0° C., say, were very far apart, then, since we know its change of volume is very slight down to about −200° C., there must be a most remarkable and sudden change at some point in the last 73°. But this is not to be believed, for it is impossible for any such violent change in the space occupied by the atoms to take place without some change in the conductivity of the metals. And we know from the researches of Dewar and others that the curve of resistance is a straight one, and cuts the axis of temperature at absolute zero, if produced.

On the other side, after considerable search, there does not appear to be any reason for believing that the atoms are widely separated in a solid, and the writer would be glad to know of any such reason, other than the fact that certain mathematicians have seen fit to make the supposition because it renders some of the work on surface-tension, etc., a little easier to handle.

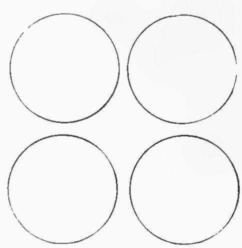
There is, it is true, one fact which is commonly considered as evidence of this nature, but which must rather be looked upon as evidence to the contrary. This is the fact that some elements have a greater volume by themselves than in combination. For instance, 45.5 cubic centimeters of potassium combine with an equivalent of chlorine to form a mass of potassium chloride which occupies only 37.4 cubic centimeters. But a simple geometric consideration will show us that even if the atoms of potassium were actually touching one another in the solid state, the 45.5 cubic centimeters would be able to contract to 31.7 cubic centimeters if the potassium were combined with an element having an atomic volume of less than 18. Similarly, 23.5 cubic centimeters sodium should be capable of combining with an element having an atomic volume of 9.6 to form a compound having an atomic volume of 16.5.

To take another example, sodium chloride should have an atomic volume of about $\{(23.5 \times .92) + 17\} \times \sqrt{\tfrac{1}{2}} = 27.02$. The actual atomic volume of $Na\,Cl$ is 27.1. $Na\,OH$ should have an atomic volume of 17. Its actual volume is 18.

$K\,O\,H$ should have atomic volume of 25.5. Actual volume is 27.

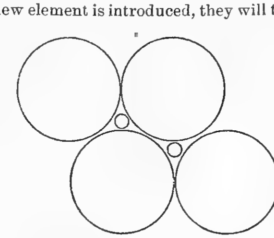
Similarly with the salts of cæsium, rubidium and the other metals which have large atomic volumes. For, of course, it is only with these elements which have great atomic volumes that this contraction on combination will be very noticeable.

The geometric explanation referred to is that in a monoplex element (element having under the conditions taken only one atom to the molecule), owing to the forces at work, the atoms will take the positions that a lot of balloons would that were fastened together by very short strings, thus,



four atoms occupying four times the space of one.

While if a new element is introduced, they will take this position



(shown in two dimensions only) where the atoms occupy a space $\sqrt{\tfrac{1}{2}} = .70$ of what they did originally. The fact that the calculated values are always a little smaller than the observed, and never larger, is one of the strongest proofs that the atoms are really fairly close together in the solid state. While this is to be regretted from a mathematical point of view, it is very satisfactory from a physical and crystallographic standpoint.

[Note.— In passing it is curious to note that the number of "space nets" into which an infinite number of points (each point similarly situated to every other point) may be arranged is sixty-six, or just the number of the well-defined elements. So that imagination may picture Spencer's homogeneous cloud of atoms splitting up into these different "space net" arrangements, each kind of net being a different element.]

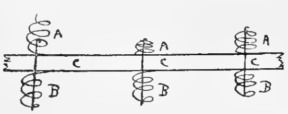
2. Solution. The chief opponent of the disassociation theory of solution is Professor Pickering; and his chief argument against it (for, of course, the disassociation theory allows the formation of hydrates as well as Professor Pickering's own hydrate theory does) is the fact that while disassociation almost always takes place with absorption of heat, solution generally emits it. This anomaly can be explained very satisfactorily by the electrostatic theory of cohesion. For whether a substance is a solid (or fluid) or a gas depends on whether the fraction

$$\text{I.}\qquad \frac{\text{cohesive force of atoms.}}{\text{repulsive force due to kinetic energy of atoms + attraction of atoms for other atoms}}$$

is greater or less than unity. We can thus turn a substance into a gas by either decreasing the numerator or increasing the denominator. The numerator we cannot change. The first term of the denominator we can increase by heating the substance, the second term by placing the substance in contact with a solvent.

In the last case the atoms of the solid part company with each other. But their cohesive force is not lost; it is simply added to that of the solvent, as shown by the increase of surface tension and of boiling point of a solution over that of the solvent. Since the solvent takes up the stress there is no necessary evolution or absorption of heat. A mechanical simile will make my meaning clear: Suppose a spiral spring, A, fixed on a board, C, which when compressed gives out heat from some reversible cause, so that it will absorb the same amount of heat in expanding. This is similar to the behavior of a gas — when compressed it gives out heat, when it expands again it absorbs heat.

But now suppose a second spring, B, placed beneath the board, C, similar in every respect to the first spring, and its axis a prolongation of that of A. Suppose an iron rod fastened to the bottom of C, extending up the centre of both springs, the rod being somewhat longer than one of the extended springs, and having a hook on the end of it.



In Fig. 1 both springs are extended. In Fig. 2, the spring A is compressed, heat being given out.

If it is now allowed to expand, the same amount of heat will be absorbed. This latter represents the turning of a solid into a gas by heating it.

But suppose, being compressed, the iron rod is hooked over the top of it. Then when it is let go it will expand and assume the position of Fig. 3. But no heat will be generated in the system, for it is evident that B will give out just as much as A absorbs. If the amount of heat given off by unit contraction of A were greater than that given off by B, the resultant effect would be a cooling of the system. If it were less, the resultant would be a heating. So we see, that while the expansion of A by itself would always absorb heat; when it is joined to B, the resultant effect depends on B.

Now, this is a very fair simile of what goes on when a solid is dissolved in a solvent. The solid loses its stress, which is taken up by the solvent, the result being an increase of cohesion between the molecules of the solvent, producing as a natural consequence increase of surface tension, lowering of the freezing point, and raising of the boiling point.

If the added electrostatic strain produces a greater amount of heat in the solvent than the loss of strain in the solid would absorb heat, the resultant would be a heating of the whole solution. Since, when a dissolved substance is plated out by electrolysis, the result resembles the cutting of the iron rod, D, in Fig. 3, there is an absorption of energy or cooling, so that work must be done to plate the dissolved substance out, and the electromotive force necessary to do this, since the ampères are constant for all equivalents, must depend on the rate at which the surface tension varies per withdrawal of unit weight of the electrolysed substance, allowing also for any heating or cooling during the electroplating.

3. Compression of gases. The ordinary formula for the compression of gases is that of Van der Waals, i.e.: —

$$\text{I.}\qquad \left(p+\frac{a}{v^2}\right)\left(v-b\right)=RT.$$

If the electrostatic theory of cohesion is correct, the equation should read

$$\text{II.}\qquad \left(p+\frac{a}{v^{\frac{4}{3}}}\right)\left(v-b\right)=RT.$$

for reasons evident to those who have read the previous note (*Science*, Aug. 22, 1892).

This is no longer a cubic, and it is pretty certain that the equation for the compression of gases should be one on account of the shape of the pressure-volume curves of carbonic acid gas. But we can transform the above equation, II., into a cubic by putting a, no longer as a constant but equal to a constant multiplied by $v^{\frac{2}{3}}$. The equation then reads: —

$$\text{III.}\qquad p+\frac{c\times v^{\frac{2}{3}}}{v^2}\Big)\left(v-b\right)=RT.$$

in which c is the same for all gases. The experimental data agree with this modified equation, as shown by table I.

Table I.

Substance.	$a \times 10{,}000$.	v (ab).
Dyethylamine	355	58
Ethyl. Acet.	348	55
Ether	324	57
Benzine	(438)	51
Ethyl. Form.	304	48
Chloroform	287	44
Acetone	273	44
Methyl. Acet.	248	39
Alcohol	236	37
Ethyl. Chlor.	227	40
CS_2	219	33
SO_2	123	24
NO_2	(74)	19

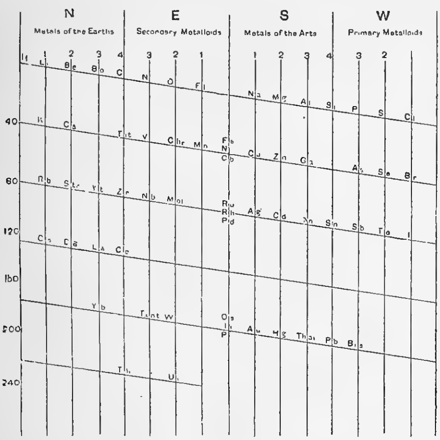
as closely as can be expected.

Table II.

Substance.	π	a	b^2	$27\,b^2$	$\frac{a}{27\,b^2}$
Ether	36.9	324	3249	87.723	36.9
CS_2	74.7	219	1089	29.403	74
SO_2	78.9	123	576	15.552	79
Alcohol	62.1	236	1369	36.963	63.8
Eth. Chl.	52.6	227	1600	43.200	52.5
Benzine	49.5	438	2601	70.227	62.3
Acetone	52.2	273	1936	52.272	52.2
Eth. Acet.	42.6	348	3025	81.675	42.6
Chloroform	54.9	287	1936	52.272	54.9
Eth. Form.	48.7	304	2304	62.208	48.8
Meth. Acet.	57.6	248	1521	41.067	60.3
Diethylam	38.7	355	3364	90.828	39
Nitrous oxide	37.1	74.2	376.4	10.116	73

This table shows that a varies as (volume) $^{\frac{2}{3}}$. Two substances do not agree with this theory — benzine and NO_2. This is owing to the fact that the data are given wrongly in the table from which this is copied (i.e., that in Ostwald's "Outlines of General Chemistry"). This is seen by the following facts. From the cubic equation we find that at the critical point, π, the critical pressure $= \frac{a}{27 b^2}$. Table II. gives the results of this calculation, and it will be seen that the values for benzine and NO_2 do not coincide with the values for π. As the values of a and b were originally calculated from π, it is evident that some misprint has crept into the tables, and there is little doubt but that if the correct values for a and b were substituted, they would fall into line and that in all cases the quantity a, in Van der Waals' equation, must be taken as equal to a quantity c, which is constant for all gases, multiplied by the atomic volume to the $\frac{2}{3}$ power.

4. Electrical conductivity. As before mentioned linked atoms cannot conduct. If we examine the enclosed cube of the elements, we see that the non-conducting elements are found on sides E and W of the cube, and these are the elements whose atoms are linked or plexed. We can tell this in the following ways:—



CUBE OF THE ELEMENTS.

1st, By their low specific heats. Those who are acquainted with chemical physics will recognize this fact and the necessary deduction. Briefly, if the kinetic energies of all molecules are the same at the same temperature, then if the sulphur molecule in solid sulphur is triatomic, or has its mass three times that of one atom; then since all the $\frac{1}{2} m v^2 s$ are equal, solid sulphur will only have $\frac{1}{3}$ the specific heat it would have if the molecule were monatomic (provided that no work is spent in disassociating the molecule.)

The standard atomic heat is 6.4. The following substances have low specific heats, and are all insulators or poor conductors: Sulphur, 5.4; phosphorus, 5.4; fluorine, 5; silicon, 3.8; carbon, 1.8.

2d, By their vapor densities. If a substance has a biatomic vapor it is not likely that it will be a monatomic solid. The following substances have two or more atoms to the molecule when in the state of vapor: sulphur, iodine, bromine, chlorine, selenium, tellurium, phosphorus, arsenic. And these are all insulators or poor conductors, while mercury, cadmium, zinc, and sodium have monatomic vapors and are good conductors.

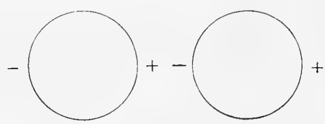
As regards metals in the allotropic state. Allotropic is a word which has been used to cover a multitude of sins. Every time an erring element goes wrong and misbehaves itself by emphasizing some of its previous peculiarities, or developes some new ones, it is stigmatized as "allotropic." For instance, we see it stated that when iron amalgam is strongly heated the iron left behind is allotropic because it takes fire in the air. But such an action does not show that any new property has been developed, it merely emphasizes a fact already well known, i.e., that iron oxidizes when exposed to air. A fine cambric needle will catch fire when held in the flame of a Bunsen burner for a second, and will continue to burn like a match after it is withdrawn. When the iron is in a finely divided state, the surface exposed is greater, and, the oxidation per unit of mass being much greater, the temperature of the iron is raised much more, thus favoring oxidation still more.

If, then, we are to use the word allotropic in this sense, we should logically speak of kindling-wood as an allotropic form of timber, for, as fire underwriters know, heavy timber is one of the most fireproof of substances. We might also speak of that form of conscience which large corporations are supposed to possess, as an allotropic conscience.

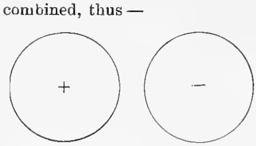
If, however, we do apply the word allotropic to such forms as Joule's iron, Cary-Lea's silver, etc., then we need another word to express the changes in the physical behavior of metals which are not due merely to the accenting of known properties but to the development of new properties, due to the joining of two or more atoms of a metal into one molecule. Polymerism might do, but it does not lend itself easily to use, and for myself I prefer to use the word plex, and to speak of diplexed iodine, triplexed sulphur, and of an element in a plexed form; though I have no doubt that if Clifford were still with us he would say that two-linked and three-linked are good enough for any honest Anglo-Saxon.

As regards the conductivity of "allotropic" elements, there is no reason to suppose that the conductivity of Joule's iron is different from that of ordinary iron. But when the elements are plexed, as we have seen above, the resistance will be much increased and the temperature sufficiently lowered, because heating increases disassociation nearly as fast as it lessens rigidity, or even in the case of those alloys or elements with negative temperature coefficients, faster.

[NOTE.—With regard to the previous paper, it may be noted that the explanation of the difference between cohesion and chemical combination, that in cohesion the atoms are charged similarly in every way except as regards position, thus —



while if any third substance short circuits the atoms they are left chemically combined, thus —



is also an explanation of a law which will probably be found true in the near future, i.e., no two substances can combine with each other without the presence of a third, thus making all chemical action the result of catalysis, plexed forms of the substances being capable of acting third substances. As regards the shortening of stretched rubber by heating, it is of course not to be supposed that the two parts of India rubber are literally contained one inside a sphere of the other, but that rubber rather resembles a tangled reel of silk embedded in jelly. If we consider any element of the jelly, and we see that it is bounded on all sides by threads of silk, and that these will act as the cell-wall of the previous paper, only "more so." The heating of rubber when stretched may be explained conversely by the compression of the jelly-substance by the cell-wall substance. R. A. F.]

THE GROOVE IN THE PETIOLE OF LEAVES.

BY AVEN NELSON, UNIVERSITY OF WYOMING, LARAMIE, WYOMING.

IN the spring of 1892, I had the pleasure of making some observations and brief studies, in conjunction with Mr. H. L. Jones, upon the origin and more particularly the function of the groove found in the petiole of many leaves, especially of Endogens.

Being at that time students in the Graduate Department of Harvard University, we laid under tribute the varied and extensive resources of the Harvard Botanic Gardens.

We first entered upon the histological study of the petiole and its groove, but soon discovered that that point of view alone would yield but meagre results: that we must depend largely upon actual experiment with plants showing this characteristic, and even more largely yet upon careful observation of the plants themselves in their habit of growth and mode of branching and the arrangement of leaves and roots on the particular plant under observation.

In order to get a starting-point it became necessary to make some guesses or suppositions as to the origin or purpose. Some of these suppositions came after a time to be so strengthened as to justify us in calling them theories, a few of which I will give with some of the facts supporting them.

In looking for the origin we do not find the groove developed as a characteristic structure earlier than in the Endogens. It is true that from the earliest differentiation of tissue into leaf we find in some instances the base of the leaf flattened and a strong suggestion of a groove as seen in some mosses, lycopods, ferns, and the bracts of the horsetails.

Finding the groove well developed in the Endogens almost without exception, and much less so in Exogens we are justified in concluding that in the Endogens there exists a necessity for such a leaf and petiole which is not found elsewhere. If, then, we can discover what this necessity is we shall, at the same time, have arrived at the origin of the peculiarity, for a plant by selection develops those structures best suited to its growth and perpetuation.

Without troubling myself at present about the reason for the difference in the habit of growth of Exogens and Endogens, but accepting these as we find them, keeping in mind always that a change in the habit of growth of one part of a plant, due to a change in surroundings, may necessitate a change in other parts as well, we will notice the habit of growth of Endogens.

Here we find plants with an unbranched stem, sometimes quite long, but usually short and often reduced to a minimum. The leaves are, as compared with the leaves of Exogens, few in number but quite large, often extraordinarily so. In these facts we can see some reasons for the grooved petiole. Like all leaves they must be attached in some way, and nature will find the most convenient way if at the same time her other purposes can be subserved. Now, where space is at a premium as in very short stemmed species or as in palms where the leaves are crowded into a terminal bud, what better arrangement than grooved flattened petioles overlapping each other can be suggested?

But it is not only the most convenient way, it is also the strongest way in which these could be attached. In a large number of plants we find the base of the petioles so closely overlapping that all are bound into so compact a mass that it would be almost impossible to pull out one without destroying the whole whorl or, I may say, the whole plant. This sheathing and overlapping of the petioles is coincident with the groove, or, I had better say, the groove is largely coincident with a sheathing base of the leaf.

So then I would say that convenience of attachment for the leaf and great strength for the plant as a whole are the first gains to be noted. As examples I may cite most palms and other Endogens with a very short stem and large leaves, or, to specify a few, Latania Borbonica, Pandanus utilis, Homalonema caerulescens, several of the Bilbergias, Crinum asiaticum, Pitcairnia hystrix, Bromelia pinguin, etc.

Before leaving this subject of strength the gain to the individual leaves by a sheathing base or grooved petiole needs to be noted.

We are all aware that from a given amount of material a stronger structure is produced if arranged in the form of a hollow cylinder than if in a solid column. Nature makes use of the hollow cylinder in grass stalks where strength and lightness is desirable, why not then, so far as possible, use the same economy in supporting those immense endogenous leaves, many of which are subjected to tremendous strain because of the long petioles necessary to carry them out into the sunlight?

As a matter of fact we do find them in all degrees from a completely sheathing base to half-cylinders, even the latter of which is greatly stronger than the same amount of material in a solid column.

Then, again, that mode of attaching the leaf gives greater strength because one side of the petiole braces the other. That could best be shown practically, yet one can easily conceive that where, for instance, the extreme margins of the petiole are attached to the stem 180° apart, each margin furnished with its fibro-vascular bundles continuous into the stem, these marginal fibro-vascular bundles act as guy ropes upon the long petioled leaf as it is swayed by the breeze.

We may further note the convenience of attachment with respect to the fibro-vascular bundles themselves. These being continuous from the leaf into the stem and the leaf attaching in a semi circle at a uniform height on the stem there is no necessity for a convergence of these bundles at a given point. But a greater number of bundles can pass naturally and directly into that part of the stem down which they continue. As examples illustrating this I would call your attention to corn and the grasses generally.

The groove having been formed in the base of the petiole it naturally persists throughout its whole length and even in the midrib of the leaf. Development in this manner, rather than a change from a grooved to a cylindrical form, represents development along the line of least resistance.

It has been suggested that the groove represents the persistance of a former condition or type and that at present the groove is merely incidental and no longer functional. This does not seem probable, however, for as noted before we find little trace in forms earlier than Endogens and again passing away in Exogens. There is nothing to show that the groove is not to-day at its highest state of development and differentiation.

It is developed, as I believe, because of the unbranched and often much shortened stem with the accompanying large leaves found in the Endogens. In Exogens, where found, it seems to perform the same functions already pointed out for Endogens, or oftener it is here merely a persistance of a former habit of growth.

In the preceding statements I believe I have pointed out the ancestral significance of the groove in the petiole. It now remains to see whether, because of a change in surroundings, this groove has secondarily taken on any new functions.

It has been suggested: 1. That the groove and axillary pockets thus formed may be regions where the absorption of water takes place. 2. That the groove guides the water to the young nascent buds in the axils and that these may absorb. 3. That the groove directs the water towards the main axis of the plant, and that in such plants the root-hair area will be found near the main axis.

If the above functions are found now to exist in some plants, I think they have been acquired secondarily and comparatively recently. The groove I consider coincident to and co existent with the endogenous type of vegetation. Furthermore, at the time when the endogenous type was the prevailing vegetation, there was no necessity for the assumption by the petioles of the above-mentioned functions. There are reasons for believing that the regions where this type of vegetation took its rise were exceedingly well watered, and the ground, being wholly shaded by the denseness of the foliage, was at all points of nearly uniform moisture, usually nearly saturated. In fact, this is still the condition where this type of vegetation is most luxuriant.

Careful microscopic examination of the tissues and their arrangement in the groove and in the axillary pocket formed by the petiole showed essentially the same structure as on other parts of the petiole — cutinized always, sometimes as heavily as outside, usually without stomata in the pocket and few in the groove. A large number were examined, and it seems justifiable to conclude that, ordinarily, the groove is not an absorbing region.

There are, however, a few anomalous conditions and structures, the use of which is difficult to comprehend. In the Bilbergias we find the base of the petiole bearing a large number of radiately branched trichomes situated in small depressions in the epidermis. It should be said that these petioles are so arranged on the stem as to form pockets capable of holding water, and that if these pockets are filled with water the trichomes, both outside and inside, will be submerged.

Also in Tulips the tightly-folded sheathing base is covered on the inside with a large number of thin-walled hair-like trichomes. In fact, the resemblance to root-hairs was quite close. The leaf was adapted for guiding any water that might fall upon it directly to this region of trichomes. Here it seems possible, as also in the Bilbergias, that the plants may have developed, because of a change in the conditions under which they grow, these additional absorbing structures.

A series of experiments, however, failed to give conclusive proof of this function. Several innocuous liquid stains were placed and retained for hours, and even days, in these axillary pockets, after which sections were made of various parts of the stem. In a few instances the tissues were unmistakably stained. Oftener, however, no trace of absorbed stain could be found.

Yucca, a plant of arid regions, possibly also absorbs water through the base of the petiole, since we find on that part of the petiole which is wholly buried among the petioles of the surrounding leaves a large number of stomata.

These stomata may absorb water trickling into this region without at other times subjecting the plant to dessication, as they would if found on exposed parts of the leaf.

Now as to the root-hair area. Do we find in these plants whose leaves direct the water toward the main axis that the area of root-hairs is near the axis, and, on the other hand, that where the water is drained outward it will fall near the region of greatest root activity?

I believe we do in a very large majority of cases. There are plenty of exceptions, but I believe they are exceptions and not the rule. As examples note all the grasses with fibrous roots, and many other Endogens growing from corms, bulbs, and rhizomes, from which grow out great masses of short, fibrous roots. On the other hand, note the forest trees, generally shedding the water outward and carrying the water toward if not to the root-hair area. But now I am not going to assert that the groove has been developed in order to direct the water inward, nor that the branches droop in order to carry it outward. On the contrary, if the root-hair areas are found as I have asserted, it is because these are the areas of greatest moisture, not that these have been made the areas of greatest moisture because the root-hairs existed there. The plant in sending out its roots seeks for moisture, and where that moisture and food is found in its most available form, it will develop root-hairs.

It does not seem then that the position of the root-hair area had any thing to do with the original formation of a grooved petiole, and I will again state that I believe the grooved petiole co-existent with and a necessity to the endogeneous type of vegetation.

THE CLEANSING FUNCTION OF HAIRS.

BY HENRY SEWELL, PH.D., M.D., DENVER, COLO

THE student of animal morphology is never so happy as in the discovery of a rudimentary organ or some structure which seems a worthless burden to its possessor; for, with an unacknowledged belief in a sort of teleology, he hopes by finding the origin of the useless appendage that the tangle of phylogeny may be loosened.

The student of animal physiology, on the other hand, is never more complacent than when to an apparently useless structure or unmeaning arrangement he can attribute some function by virtue of which the body is made a more efficient machine.

An interesting example of the subservience of form to function, which the writer has never seen mentioned, is found in the arrangement of the epidermic scales which form the outermost layer of animal hairs. The buried edges of the scales point towards the root of the hair, while the free edges project obliquely in the direction of the hair end, as the shingles on a roof point to the eaves. When a hair is drawn between the thumb and forefinger, which are gently pressed upon it, it will be found that the hair glides far more easily when pulled from root to tip than in the opposite direction. When the hair is simply rolled between the thumb and finger it will gradually move parallel to its length in the direction of the hair root. These results depend altogether on the way in which the hair-scales project from the hair axis. It is at once obvious that foreign particles clinging to the hair *in situ* would find easy the passage outward towards the tip and away from the surface of the body, but exceedingly difficult the progress in the opposite direction. Every movement of the hair, especially frictional disturbance, must set up a current of foreign particles towards the hair tip. The housewife has long known by experience how much more readily a vigorous shaking cleanses a woolen garment than one made of cotton.

The sebaceous glands opening at the mouth of the hair follicle, probably play an important part in surface cleansing; for their oily secretion sticks together the particles of shed epithelium, associated with all manner of filth, in such a manner that the "hair-rakes" can, no doubt, more easily remove them.

Ludwig long ago showed that, in the same way, the mucus secreted by the surface epithelium of the stomach and intestines agglutinates the detritus which covers the mucous membrane after digestion, and so makes possible its removal by the peristaltic action. The housewife, again, uses the same principle when she sprinkles a very dusty floor before sweeping, and finds the filth to roll before her broom.

One more reference to physiological body-cleaning: It has been found that the growth of epidermic epithelium proceeds in such a way, at least in certain situations, as to remove the worn-out cells *en masse*. Thus, on the external surface of the ear-drum, the direction of growth is such that the epithelial scales progress, pushed from below, steadily from the centre of the membrane and then along the meatus to the exterior. Foreign particles lying on the epidermis are of course carried with it.

NOTES AND NEWS.

THE first annual meeting of the Ohio Academy of Science was held at Columbus on Dec. 29–30, 1892. After some formal business, such as the appointment of committees, had been attended to, the reading of papers began. The following, among others, were read during the session: The Advantages of Arzama obliquata for Laboratory Instruction, D. S. Kellicott; The Inhabitants of a Species of Gall on Wheat Plants, F. M. Webster; Some Anticlines found in the Shales of Northeastern Ohio, Geo. H. Colton; Lantern Slides without a Negative, W. G. Tight; A Few Rare Ohio Plants, Aug. D. Selby; New Plants for the Flora of Ohio, W. C. Werner; Notes on the Distribution of Some Rare Plants in Ohio, W. C. Werner; Lichens of Ohio, E E Bogue; Leaf Variation: Its Extent and Significance, Mrs. W. A. Kellerman; Some Insect Migrants in Ohio, F. M. Webster; The Uredineæ of Ohio, Freda Detmers; Ohio Erysipheæ, Aug. D. Selby; The Development of the Berea Stone Industry, J. H. Smith; Snow-Rollers, W. S. Ford; Note on a Nest of White Ants, O. L. Sadler and Mrs. O. L. Sadler; The Histology of the Stem of Pontederia cordata L., E. M. Wilcox; Pulmonary Fistula in a Frog, J. B. Wright; Note on a Skull Pierced by a Stone Spear-Head, E. W. Claypole. In the evening the president, Dr. E. W. Claypole, delivered the annual address, taking for his subject "Devonian Ohio, or a Passage in the Making of the State." Premising that such an address should not be one intelligible only to geologists, as the majority were not specially devoted to that science, he outlined the geological history and growth of the region from the commencement of the deposition of the Corniferous Limestone to the base of the Berea Grit. The first part of the era was a time of profound peace, when a coral sea overlay all the State. This was followed by a time of depression, when the vast beds of shale were laid down. The fishes of that era, as preserved in these shales, came in for full consideration, and their immense bony plates were illustrated by numerous drawings. The leading genera were Titanichthys, Dinichthys, and Gorgonichthys. Mr. W. K. Moorehead was appointed a committee on archæology, especially with a view to the investigation of the antiquities of Ohio, and Professor G. F. Wright was made a committee on boulders.

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SOME POINTS IN THE COMPARATIVE OSTEOLOGY OF THE TAPIR.

BY CHARLES EARLE, AMERICAN MUSEUM OF NATURAL HISTORY, NEW YORK.

So much has been written in the last decade on the evolution of the horse, that I think it will not be out of place to compare some of the skeletal structures of its most generalized relative, the tapir. The tapir represents in the fauna of the present day the most generalized member of the odd-toed Ungulates, and in its osteological structure we find the closest relationship with those old Eocene Perissodactyles, which are now entirely extinct. As a whole, the structure of the tapir presents us with a most generalized form, but the extreme modification of the nasal region of the skull is a modernization, as it is called. Of all the known Tapiroids of the Eocene there is none which shows this extreme specialization of the facial region of the skull for a proboscis. Cuvier in the "Ossemenes Fossiles" compares the osteology of the American and Malayan tapir in a general way, but does not treat the subject in detail.

In the present paper, I wish to speak in particular of the comparative evolution of the foot structure in the tapir. In such widely isolated forms as the American and Malayan tapir we would naturally expect to find some differences in the details of their foot-structure, and such is the case. On evolutionary ground these differences are of great interest, but I do not wish to trouble my readers with a lot of dry anatomical details, without the latter being of some interest.

As a word of introduction I would say, that the derivation of the modern digitigrade Ungulates has been from an animal with a plantigrade foot, the latter having had five complete digits. An approach to this type is seen in the Puerco genus *Periptychus*. Another point of great importance in the structure of this primitive or ancestral foot was that the various elements of which it was composed were arranged one above the other; the serial arrangement as it is called. The carpus and tarsus of the Eocene *Phenacodus* exhibits the serial order of its elements. Now in the evolution of the foot-structure of the tapir, it has departed from the serial order above described, and with this specialization has occurred a loss of lateral toes. However, the tapir has been fortunate enough to lose only one of its anterior toes, whereas the horse and rhinoceros have lost more.

When we compare the structure of the fore feet of the common Brazilian tapir (*T. Americanus*) with that of its Malayan relative, we find considerable difference in the shape and relation of the bones of the carpus. This relationship is due to the comparative specialization in the foot-structure of the one species over the other. In the American tapir the external lateral toe is very much reduced and functionless. In the living tapir this fifth digit transmits little or no weight to the ground. Co-ordinated with the reduction of the fifth digit in this species is the growth of the median digit of the manus. Another co-ordination of the reduced size of the fifth digit in the American tapir is the large articulation of the unciform bone with the lunar. The lunar has also no contact, or a very small one, with the magnum anteriorly.

It has been observed in the evolution of the foot-structure of the Perissodactyles that in the earlier and heavier forms the fifth digit of the manus is always largely developed, and with the large size of this digit is the comparatively small size of the median. In this respect, these earlier forms approach more nearly in their foot-structure the even-toed Ungulates (*Artiodactyla*). Again, in these less specialized forms the long axis of the unciform bone is always horizontal.

The position of the unciform is co-ordinated with the large size of the fifth toe; and as a consequence there is a smaller contact between it and the lunar, than in the later and more specialized forms. We observe then, as a rule, that as the unciform begins to rotate upwards and assume the vertical position, the external lateral digit becomes more and more reduced in size.

Another correlation in reference to the large size of the lateral digit is the nearly subequal distal facets of the lunar, an adaptation which is for the equal transmission of the weight of the foot on both sides of the median axis. The magnum is also much depressed and broad in those heavy and more ancient forms.

Turning to the manus of the Malayan tapir, we find the external lateral toe more developed than in the American form. There is also less difference in size between the latter and the median toe. The lunar has a large contact with the magnum anteriorly; the latter bone being broader than in the American form. That less displacement has taken place in the manus of the Malayan tapir is shown from the fact that the unciform and scaphoid bones are widely separated, whereas in the American tapir these bones nearly touch each other. The approach of these latter bones takes place with the reduction of the fifth toe until in some species of rhinoceros they are nearly in contact.

As for the tarsus we observe that the hind foot of the Malayan tapir is broader and heavier than in the American species. A very important difference between the structure of the pes in these two forms is that in the Malayan species both the lateral metatarsals articulate with the ectocuneiform, whereas in the Brazilian form only the internal metatarsal touches this podial element.

In conclusion, we see from the above characters that the manus of the Brazilian tapir is considerably more specialized than that of the Malayan tapir; on the other hand, the pes of the former is not so much modified in structure as that of the latter species. In other details of the skeleton of the tapir, I am not aware that many differences exist. In relation to the lumbar vertebral articulations, I would observe that they are very simple and articulate by plane surfaces. In general, the Eocene Perissodactyles (*Hyracotherium*, *Hyrachyus*) have embracing vertebral articulations.

THE SPEECH OF CHILDREN.

BY A. STEVENSON, ARTHUR, ONTARIO, CANADA.

The term speech ordinarily signifies articulate vocal utterance in conventional forms, intentionally expressive of feelings or ideas. In treating of speech as a product of intelligence too much is sometimes made of the articulation factor. For articulation is not characteristic of man alone, and among the lower orders, the elephant and the dog, which do not articulate, are more intelligent than the articulating parrots. Moreover, the child, before he can articulate, employs inarticulate utterance with intentional and striking expressiveness.

The first cry of a child, whether or not we call it a rudimentary form of speech, is certainly a vocal utterance strongly expressive of feeling. Though the element of intention is absent for several months, yet there is a considerable variety of expressive quality in the child's cries during this time. This organism, indeed, is like a wind-harp responding in various tones to diverse sense-impressions.

These early cries are expressive simply of pain or distress, and their expressiveness consists partly in tone and party in intensity.

They vary in tone according to the nature of the exciting cause, and in intensity according to its degree. The range of expression is exceedingly limited, for several months being chiefly restricted within the bounds of physical suffering. Here, however, some mothers and some medical men find valuable assistance in the diagnosis of physiological disturbances.

But the young infant's cries do not always express physical feeling merely. There soon appears a quality, which, after very little development, comes to be distinguished as mental. The child H., on the eighth day after birth, was much startled by the sound of a bottle falling to the floor. She made no outcry on this occasion, but on the twentieth day a similar noise drew from her well-marked tones of fear. At the same time a general tremor was exhibited by her, such as accompanies terror in older persons. It is not to be wondered that there should be expressive emotional quality in the cries of such young children when we observe the well-marked variations in facial expression even at this early period. Physical pain shows itself in the countenance from the very first. On the fifth day H. undoubtedly manifested disgust in this way at the taste of a nauseous medicine, but her countenance immediately resumed the normal expression when a pleasant medicine was substituted. On the seventeenth day she showed great distress at the sharp and screaming cries of another child. On the thirty second day she smiled at her mother in response to fondling and caresses. On the thirty-eighth day her countenance plainly expressed wonder when she was taken into a strange room.

The young infant's cries of discomfort soon become differentiated, and among the specific utterances that emerge the cry or call of desire appears early. It is difficult to define the subtle beginnings of this form of utterance; it seemed plain to me in one case in the second month; there is no doubt that it appeared in the tones of another child in the sixth month.

On the thirtieth day the child M. gave utterance to a sound indicative of comfort and satisfaction. By the forty-eighth day this sound assumed distinct form as a low, soft oo. During the nights of the forty-fifth and the forty-ninth days M. laughed heartily in her sleep with a sound that, except for its softness, certainly resembled very much the adult "ha, ha, ha."

This last utterance, perhaps, shows merely one form of vocal capacity at the time, but it seems reasonable to assume that the other utterances described, or most of them at least, possess psychical significance in some degree. It is not sufficient to speak of such sounds as mere non-significant products of muscular movements, reflex, instinctive, or spontaneous. They are really the crude raw material of the vocal element in conventional speech. They soon grow into the sounds of language, which is the joint product of mental and bodily faculties and activities.

Just at this stage the child is in a fit condition to begin any language. These first utterances belong to the common mother-tongue of the race. What particular form of speech the child ultimately learns depends altogether on his environment. He begins wildly and indiscriminately with various sounds, but in course of time some, lacking the stimulus of example and encouragement from those about the child, fall into disuse, while other sounds, under that stimulus, are drawn out and cultivated. The speech of the adult is the result of a long evolution under the influence of environment and unconscious selection.

The observer of child language must note development along two lines, in the mind and in the vocal organs. We will follow the latter chiefly. The inarticulate cries and calls of children soon come to be interspersed with articulate sounds. The easier vowels come early. The child M. uttered freely and clearly ah and oo early in the third month. These sounds appeared spontaneously, but could afterwards be evoked in imitation.

Of the consonantal sounds, the first to appear in the cases of O. and M. were b, f, p, d, t, m, n, ng, h, k, and g guttural as an initial. O. could make these sounds in the tenth month, but for several months afterwards he could pronounce them only singly or in easy combinations. Slowly, and with greater or less difficulty, the others were acquired. At two years O. could pronounce the vowel and dipthong sounds except oi and ew, and all the consonants except th, v, and the trilled l and r. He still had considerable difficulty with the guttural g as a final, for which he substituted d until his fourth year. For k final he invariably used t for about the same period; v was sounded b for a while, th was entirely omitted as a thick sound, as a thin sound f or s was substituted until the fourth year. Various consonantal combinations were especially slow in being perfected, as ks and kw, and all combinations with s as initial. l and r gave trouble until the fifth year. At first they were omitted entirely in any situation, then y and w began to appear respectively as initial substitutes, and the preceding vowels began to acquire breadth and prolongation when l and r were medials or finals. The child M., however, acquired r final early in the second year.

The mispronunciations of children may seem arbitrary and altogether irregular to the casual observer, but in reality nearly all of them can be readily classified and arranged under law, the same law that appears in the broken speech of foreigners attempting English, and indeed in the history of the changes that have come over the sounds of English words themselves. A child is a foreigner learning the language, and he pronounces the easier sounds rather than the difficult. The omissions and substitutions of children represent the difficulties in sounding the vocal elements and combinations of our speech — difficulties that adults struggled with and overcame at so early an age that they do not recognize them as difficulties.

In the speech of the child O. during his first four years the following classes of consonantal substitutions regularly appeared.

First classifying the sounds according to the organ of articulation: —

	Sound attempted.	Sound made.	Example.	Comparison.
Labials.	f	p	Eppie = Effie	(L) pater, father
	wh	f	file = while	cough
	v	f	ofer = over	five, fifty
	v	b	balise = valise	have (O.E), habban
	p	m	moon = spoon	Polly, Molly
	m	p	Pata = Martha	Patty, Mattie
Dentals.	d	n	ness = dress	
	t	n	nats = tacks	
	th	t	cot = cloth	
	t	d	bodde = bottle	(L) duo, two

Second, classifying the sounds according to their duration in utterance: —

Spirants (sharp).	th	s	sissle = thistle	loves, loveth
	f	h	hind = find	laugh
	th	f	fumb = thumb	Fedor, Theodore
Mutes (sharp).	k	t	fots = fox	
(flat).	g	d	pid = pig	
	g	j	jive = give	joy, (L) gaudere
	b	j	janana = banana	
	d	j	jit = drink	
(nasal).	n	m	timy = tiny	lime, linden

Besides these there were several other regular permutations, which do not fall into any of the above classes. I have not observed any interchanges in the use of gutturals, palatals, or sibilants, or in the flat spirants. One child I knew of regularly interchanged the trilled spirants, as in "I rost my ling" (= I lost my ring).

The speech of children shows all the features which, in standard language, we consider as the product of phonetic decay in the various forms of aphœresis, syncope, and apocope. The omission in both cases is due to the same cause, namely, the excessive effort that would be required to articulate the sounds.

As far as my observation goes, children rarely add new elements in sounding words. Transposition, however, is not uncommon; O. regularly said kit for tick and krunt for trunk.

The ability to discriminate vocal tones, which infants possess in a remarkable degree, is a potent factor in the acquisition of language. This ability is manifested very early, showing itself in looks of distress or outcries even, in response to harsh language, and in a return to placidity when softer tones follow. Such manifestations are, of course, instinctive, and are precisely of the same nature as the exhibition of terror referred to as made by H. on the eighth day of her life at the falling of a bottle to the floor. Again, the child M. in her tenth month was trained to keep away from a hot-air register by the use of the simple word "burn," spoken to her several times with considerable intensity of warning in the tones. A soothing effect is produced on children not only by soft and musical sounds, but by sibilation, either by the voice or by such means as the rustling of paper. Almost any novel sound induces temporary distraction from crying.

Imitation plays an all-important part in the acquisition of conventional speech. In the case of O., the faculty of imitation appeared first in manual actions and during the tenth month. Vocal imitation was first observed in the thirteenth month, when, under the stimulus of the shouting of other children at play, he, also, began to shout vociferously. Shortly afterwards he began, under instruction, to imitate the sound of a watch's "tick-tick." But in this instance, and others which followed, there is no argument for an onomatopoetic origin of language, for in no case did the child originate the imitation. He merely imitated an imitation first made by his parents. From this period imitation showed itself frequently, and the child was delighted with his successful attempts; the delight was increased when these attempts were appreciated and reproduced by those about him.

It is scarcely necessary to mention that infants understand a considerable range of language long before they can speak. A child readily learns a few words for simple objects or actions before he is a year old, and some children can be taught to understand "No" as a sign of prohibition as early as the eighth month. This last is a similar development of intelligence to that early gained from an experience of pain resulting from contact with injurious objects. In his seventh month O. was accidentally allowed to touch a hot lamp chimney, and, being burnt, he would always afterwards draw back on being brought near a lamp.

We may sometimes hear it said that the first words uttered by children are nouns, in respect to grammatical function. The truth is that, though an infant's first words are commonly such as are used by us in nominal relations, yet in the infant's speech these words are not nouns, but equivalent to whole sentences. When a very young child says "water," he is not using that word merely as the name of the object so denoted by us, but with the value of an assertion something like "I want water," or "There is water," the distinction in meaning between the two expressions being shown by the child's tone of utterance.

There is no form of linguistic study more instructive and interesting than the observation of the successive and correlative processes in the growth of such interjectional expressions as this into the various and complex forms of conventional sentences. With the child O. some of the various steps along the straight line of development from the single word to the full, simple sentence were as follows: "Water," "drink water," "want a drink of water," "baby wants a drink of water," or "him wants a drink of water," "I want a drink; *baby* wants a drink of water," "I want a drink of water." No instruction was given the child in the case, and it took him more than two years to develop the conventional sentence after he had begun to use the word "water." The natural difficulty which children have in acquiring the use of the personal "I" appears in the foregoing examples. Even after O. began to use "I" as a name for himself he seemed to think it necessary to explain or justify the word to himself by repeating the statement and using instead of "I" the name "baby" or "Oscar." Occasionally he used the noun first and repeated, as in "Baby want a drink, I want a drink."

Similarly, the child was long in learning to use the objective personal "me." The earlier mode of expression was to employ the name baby, as "Papa, carry baby." Before reaching the regular use of the possessive "my," O. always expressed this relation by "its," as in "Papa, take its hand," "Mamma, wipe its eyes." Thus until nearly three years of age, the child apparently regarded himself only as object and not at all as subject. Other curious forms of expression in habitual use shortly after this were such as "I am going down, me," "I'm going home, I'm are."

Notional words were acquired before those indicating relations, and of the latter the simpler and more notional were first acquired. Vocabulary and expression developed considerably without the use of the verb "to be." Interrogative pronouns, interrogative adjectives and adverbs came into use early, the relative or conjunctive pronouns much later — nearly two years. Adverbs came before prepositions. At first the prepositional function was served by placing the related words in juxtaposition, as "See old man (with) head down." In this sentence note also the omission of the comparatively noticeless word "the."

Color names caused great difficulty, their proper application depending, of course, on a considerable development of the perceptive powers. During the early part of his third year O. used "blue" freely, but he applied it to any striking color, as to a white horse and a red book. Similarly with number names, O. could not use the simpler names properly beyond one and two until nearly his fourth year. Any number beyond two he called "nine." "No" was easily acquired; "yes" cost a great deal of effort, not in pronunciation, but in comprehension and application. At three years of age, to give an affirmative answer he would repeat the question in the form of an affirmation, or reply by "it is" or "it does." Then, after beginning to use "yes," it was applied irregularly, as in answering the question, "Will you do that any more?" O. said, "Yes, I won't." This is not a self-contradictory expression, as it would superficially seem. The child meant by "yes" that he was willing to obey, and the "I won't" defined the form the obedience would take.

The strength of the linguistic instinct in children is shown by the remarkable shifts they will make to find forms of expression for their perceptions or feelings. An examination of these shifts will show that the energy of the child manifests itself along precisely the same lines as have been taken by the languages of the races of mankind towards their ultimate forms. Thus, lacking the work "wide," O. said, "Open the door loud," extending the meaning of the word "loud" precisely as we do when we apply it colloquially to colors. So, too, he called a raccoon a "cat," just as we speak of "plumes" of horse-hair. Other illustrations I have without number, but will add only a few. "I have a headache in my neck," "There's a boat swimming," "Mamma, you never cut the toe-nails off my fingers." "Cows eats drinks of water, cows do," "I broked it (cp. wept)," "He goed," "Papa's gooder than you," "Papa can the see (i.e., light) come in here," "Which would you rather have, Mary *and* Rhoda?" "Papa's got *that* coat on," (i.e., a new coat). In the lack of a knowledge of negative forms, O. used some curious expressions. Thus, not wanting me to go out, he said, "Papa, come in; papa, stay home;" again, not wanting his coat taken off after being out, he said, "Put baby's coat on."

The most common means by which infants enlarge their powers of expression is by the metaphorical extension of terms already known, as where O. called a piece of fur "kitty." Now this ability to use words metaphorically implies the possession of the power of abstraction in some rude degree, for metaphor-forming is a mode of abstraction. It is a remarkable thing that very young children can form these abstractions. Thus O, at the age of eighteen months, having learned the name knee from a limb in a bent position, afterwards called his mother's chin "knee," and presently applied the same term to the projecting corner of a pillow. A more striking instance occurred shortly afterwards. Being a delicate child, he was just then at the stage of beginning to stand alone. He had been frequently told to "stand up like a man." The first time he tried this feat with entire success he said "man!" with much self-approbation. Within a few days he applied the same term to his doll when standing upright, and also to a long, narrow box when set upon end. All this time he was perfectly familiar with the common uses of the names man, doll, and box. Evidently, then, in the special cases noted, he was using the term man in the sense of "the upright thing," a considerable abstraction for an infant under two years.

Finally, children invent entirely new words. A few of O.'s original were "oüah" = water, "ōbō" = music, "gladdies" = dandelion flowers, "aneen" = wagon. The last word may be a case of aphœresis and substitution, but it seems hardly likely. The others are inexplicable, except as pure inventions. I know of two cases where a pair of children, besides acquiring their mother-tongue, invented a full vocabulary entirely unintelligible to any one but themselves. It is on the observation of such cases as these that Mr. Horatio Hale has very reasonably based his theory that the closest of blood relationship may exist between tribes or races of people whose languages differ in every particular.

THE GENESEE RIVER.

BY REV. BROWNELL ROGERS, A.M., CONQUEST, N.Y.

THE Genesee River rises in Potter County, Pa., about seven miles south of the State line. The average elevation of the highest hills in this county is not far from twenty five hundred feet. The valley of the Genesee reaches southward between the basins of the Susquehanna, on the east, and of the Chautauqua Allegheny, on the west. The water-shed between these three basins lies in the townships of Allegheny and Ulysses.

The river flows north-northwest into Allegheny County, New York, to the town of Caneadea, where its direction changes to north-northeast. This direction is held until the river reaches Lake Ontario. The total fall is about twenty-two hundred feet. Its entire length is not far from one hundred miles, but flowing so nearly northward it cuts across all the formations of the New York system from the Catskills to the Medina sandstone, these formations in this part of the State having a nearly uniform east and west strike. Yet, notwithstanding, there are but two localities where these formations are generally exposed, viz., at Portage and at Rochester. True, there are a few other places where the rock is uncovered, as at Mapes, and at Belmont, Allegheny County, New York, but these are only limited exposures, and do not at all compare with the gorges at Portage and at Rochester. It is this fact that makes the river such an interesting study; for these two gorges — the one at Portage about three miles in length, and the one at Rochester about seven — are post-glacial; the remainder of the course of the river being in a pre-glacial valley, which is nearly filled with drift. This old valley was several hundred feet deeper than at present, for the drift has been penetrated at various places two, three, and even four hundred feet before the bed-rock was reached, while on the hills, either side of the river, rock is struck a hundred feet or more above the present level of the water. Indeed, many of the tributary creeks have uncovered the native rock for some distance back from the river.

During the glacial epoch this old valley was undoubtedly filled with ice, for the terminal moraine forms the water-shed of Potter County. During the retreat of the ice, halts were made in at least three different places, allowing the accumulation of drift in greater quantities than elsewhere, thus damming up the already nearly-filled valley.

The first of these dams is about eight miles north of the State line, in the town of Willing. It was not so high, though, but, on the further retreat of the ice northward, the water easily found a way over the obstruction. This was on the western end of the dam, consequently this end has been almost entirely washed away. There are remnants, however, on the side of the valley at an elevation corresponding with the eastern end, which is left almost entire The second great glacial dam is at Portage. Here the drift formed so complete a barrier that the river was turned out of its course. But, instead of turning back again and flowing southward as the Allegheny River did, the Genesee was simply turned to the west, and re-entered its vallew below the dam. In plunging over the precipice, back into the old channel, strata of various degrees of hardness were exposed, the erosion of which has resulted in the formation of the present cañon, with its series of three water-falls. At the upper falls the walls of the gorge are two hundred and fifty feet high. Here the river makes a perpendicular fall of sixty feet; half a mile below, a perpendicular fall of one hundred and ten feet; and one and a half miles farther down, a broken fall of eighty feet. The summit rock at the lower falls being so soft, many changes have been produced in the falls during the last eighty years. A little south of Rochester the valley was again so completely filled as to turn the river out of its course, and again it turned to the west, cutting the gorge below the city, and north of the outcrop of the hard Niagara limestone which forms the summit of the falls at the head of the gorge. The depression occupied by Irondequoit Bay is the mouth of the old valley where it emerged from the Ontario plateau, but the valley itself is traced far out into the lake, where it opened into the old Erigan River. Had the Genesee valley not been so completely filled up throughout its entire length, we undoubtedly would have had another lake similar to Seneca and Cayuga Lakes, all of these depressions being the results of preglacial erosion. Sodus Bay and Fair Haven sustaining the same relation to these depressions as Irondequoit does to the valley of the Genesee.

ODDITIES IN BIRD LIFE.

BY C. W. SWALLOW, WILLSBURGH, OREGON.

The water ouzel (*Cinclus mexicanus*) is a very peculiar specimen of the feathered race. Here we have a bird that, from its habits, long legs and teetering motion, may easily be mistaken for a sandpiper. It may almost be called duck-like, as it is so much at home in the water, wading, swimming and diving with ease, and even walking on the bottom under water in search of food. From its shape and song it is somewhat wren-like; then again, from its bill, its song and some other points, it is quite thrush-like. The bird is not especially noted for its musical ability, yet when its sweet trills and warbles are heard in the wild forest near some rocky stream, where song-birds are rare, it is certainly charming to one that loves bird notes.

The ouzel, or American dipper, as it is sometimes called, is a western bird, found along the mountain streams between the Rocky Mountains and the Pacific coast. The birds are bluish-slate in color; darkest on top of head, back and wings. Tail nearly black. The winter plumage and young have the feathers of the throat and underparts and some of the wing feathers white-tipped, giving some specimens the appearance of being quite gray. These odd birds are about 7 inches long, with 11 inches extent of wings; wing, 3.5 inches; tail, 2 inches; tarsus, 1.1 inch; bill, .7 inch, horn-blue, yellowish at base; feet and legs yellowish. The nest, placed by or under the upturned roots of a tree or an overhanging rock or like situation, is a well-made, dome-like structure of moss and rootlets, with the entrance on one side. One nest that I examined had the entrance nearly concealed by a swinging door of moss, evidently placed there for that purpose. They are said to lay about five pure white eggs.

Perhaps one of the most odd of American birds, in habits as well as appearance, is the evening grosbeak (*Coccothraustes vespertina*). Although seemingly very widely distributed, it being reported from the New England States to Oregon and from Mexico to Canada, yet little if anything seems to be known of its breeding range and habits. Last winter, 1891 and 1892, it was quite a common bird in the vicinity of Portland, Oregon. I often observed a flock of about a score which came to a certain locality nearly every morning for a number of weeks to feed on the buds of the vine maple. I noted them from December, 1891, until April 25, 1892. This winter I have failed to see or hear one in the same localities, although it has been a much more severe winter, and would naturally lead one to expect northern birds to be more abundant than last winter, which was remarkably mild.

These birds utter a clear, bell-like chip, when flying, and occasionally when on trees; it seems to be a call note. The largest specimen I have measures as follows: Length, 8 inches; extent of wings, 13.85; wing, 4.5; tail, 3; tarsus, .75; middle toe with claw, .95; hind toe with claw, .65. They have a very heavy, cone shaped, greenish-yellow beak about .8 inches long, by .6 broad, and .65 deep at base. With their odd colors of yellow, black and white, these birds may remind one of the setting sun, night and snow. They have a black crown patch nearly enclosed by yellow on forehead and stripes over the eyes running back to the nape; a few black feathers at base of bill; neck, sides of head and throat brownish-olive, shading into yellow on the rump and underparts; wings and tail black; secondary coverts and some of the secondaries white, producing a large white blotch on each wing; under tail coverts yellow; feet and claws light brown; closed wings reaching to within about one-half inch of end of tail. The winter habits of the bird seem to be very much like the pine grosbeak (*Pinicola enucleator*), which is quite common in the eastern States some winters.

Another species that would be included as oddities is the chats (*Icteria*), represented in the eastern States by (*Icteria virens*) yellow-breasted chat, and in the Pacific coast by (*I. v. longicauda*), long tailed chat. The western variety can hardly be distinguished from the eastern except by the longer tail and perhaps brighter colors. These birds are about 7 inches or a little more in length, having an extent of wings of 9.5 inches; wing a little more than 3 inches, the tail of the western bird being about the same length; bill, .65 inch long. They are slaty-brown on the head, neck and back; wings and tail brown, tinged with yellow; throat and breast bright yellow; underparts brownish-white; yellow of the throat bordered with white; a few white feathers about the eyes, and a faint light stripe from nostril to eye. They build quite a bulky nest in bushes or briars near the ground, and lay from four to six white eggs, spotted with brown. As a songster, for variety and execution, I think they are second only to the mocking bird in Oregon; but in the eastern States I do not think they can equal the brown thrasher or catbird.

In the breeding season the chats have a peculiar habit of flying up and dropping down nearly straight, beating the air with their wings incessantly. Occasionally they will remain almost stationary in the air for several minutes, beating the air with their wings and singing. At times they flap their wings so as to be heard some distance away.

A curiosity in the owl family is the pygmy owl. One variety (*Glaucidium gnoma*) is quite often seen in Oregon. They are well named pygmies, as they are only about 7.25 inches in length and 14.5 in extent of wings; tail, 2.85, of twelve feathers; bill, greenish-yellow with lighter tip; feet and claws brownish-black. This little owl appears very much like a miniature barred owl (*Syrnium nebulosum*), as it has a smooth head with no ear tufts, and is marked much like the barred owl, being of a slaty-brown, thickly barred and spotted with white, darkest on the wings and back, lighter on the underparts. This little owl I think is more of a day-bird than most of the family, as it may quite often be seen on cloudy days out hunting for mice and small birds, or even moths and insects, which I think sometimes form part of its food. It no doubt breeds in hollow trees, but I have never found its nest.

TELEPHONING BY INDUCTION.

BY G. H. BRYAN, M.A., ST. PETER'S COLLEGE, CAMBRIDGE, ENGLAND.

THE wonderful revelations dealt out to an admiring public by some of our newspapers under such headings as "Science Notes" often afford infinite amusement to the initiated. Some recent experiments of Mr. W. H. Preece, F.R.S., on induction currents, have found their way into some of these collections of information in a form which makes them appear little short of miraculous. According to some accounts, Mr. Preece has solved the problem of "telephoning without wires." He had only to speak the word (so we are told) and the electric fluid *leapt across* the three miles of sea which separates the island of Flat Holm, in the Bristol Channel, England, from the mainland, and delivered its message with unerring accuracy into the telephone placed there for its reception. On reading such accounts as this the British public will exclaim, Oh! with a mixture of awe and admiration, and half a dozen "paradox mongers" will build up unintelligible theories of "the electric fluid and the way it radiates through the ether" or something of the kind — showing that Maxwell and Faraday are *wrong* and they themselves are *right*. Those, however, who know anything about electricity will smile when they see what impossibilities the presiding genius of the British Postoffice Telegraphs is credited with performing. In the first place they will know that either telegraphing or telephoning without wires is still an impossibility. Wires there must be, and the wires at the transmitting and receiving stations must form circuits enclosing a considerable area, but the important feature of the experiments is that the two different sets of wires may be some miles apart without any wire connecting them. Then, again, the idea that the "electric fluid" can jump across through three miles of air like a flash of lightning is absurd. What really happens is that every time that a current is passed through one circuit a current is "induced" in the other circuit, and when the current in the first circuit is stopped an "induced" current flows round the second circuit in the reverse direction to what it did before. This is the well-known principle of electro-magnetic induction, which has given rise to the induction coil, the dynamo, and indeed to most of our modern applications of electricity. The remarkable thing about the present experiments is that they show that this "induction" can not only make itself felt at such great distances, but can actually be utilized to transmit telephonic messages. At present we can only speculate as to the way this "inducing action" takes place, all that we can assert definitely is that *no* electricity passes from one circuit to the other. Even if we regard the action as magnetic, the "lines of magnetic force" do not go from one wire to the other, on the contrary they encircle the wires and do not anywhere terminate on a wire. Again, so far from the action travelling with unerring accuracy in any particular direction, the same message would be transmitted to a receiving apparatus placed anywhere in the neighborhood, provided that it was furnished with a sufficiently large circuit of wire, so that if several transmitting apparatus were in use at the same time in any particular neighborhood, the various messages would get confused.

Scientific discoveries such as this appear to be comparatively simple matters on paper, but they are usually the outcome of many years of patient experimenting. It is more than six years since Mr. Preece described some similar experiments made with the telegraph wires running up the northeast and northwest coasts of England respectively. In these experiments, however, the primary current was produced by means of a powerful dynamo, but the induced current right over the other side of England was sufficient to produce a sound "very like a wail" in the telephone employed for its detection.

Feb. 10, 1892.

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 So-Called "Cache Implements."

THERE has recently crept into archæological literature an unfortunate, because misleading, term for a well-known form of chipped-stone tool or weapon, that of "cache implement." This name has been suggested, on the ground of the erroneous assumption, that long, narrow blades of jasper, argillite, and other flakable stone were only to be found in "caches" or deposits, and then, continuing the argument, because so found, they were unfinished objects, and in time were to be disinterred and converted, by further chipping, into knives, spear-heads, and, possibly, arrow-points. There is not a scintilla of truth in this, so far as any living man now knows. It fits admirably, however, with a plausible theory by a coterie who have failed to make any important archæological discovery, and so is one of their mainstays in proving the modernity of America's native people; something that must be *proved* at all hazards; or, if not demonstrated, foisted upon the unthinking to secure the scientific prominence of a few archæological mugwumps.

When we examine a series of these "cache" implements, it will be seen that they are not too long, too broad, or too thick to be used as weapons or domestic implements, but lacking any evidence of a notched or narrowed base appear unavailable so far as the matter of attaching a handle thereto; *ergo*, an *un-handled* implement being an impossibility, they are unfinished. If, however, the reader will refer to "Remarks upon Chipped-Stone Implements" (Bulletin of the Essex Institute, vol xv., 1883) he will find there pictured just such objects as I refer to, with short wooden handles secured by a "tenacious substance probably obtained from the cactus." Now, the Delaware Indians made a most excellent glue by boiling together cherry gum and fish-bones, and so could as readily have secured handles to these plain blades, and, considering how frequently single whole specimens and broken ones are found on village sites, it is clearly obvious that they were in frequent use.

Again, such blades, measuring usually five or six inches in length, by two in width, are not the only objects found buried in large numbers. Small leaf-shaped knives are found, often as many as one hundred together; arrow-points of various patterns have been unearthed, as well as grooved axes, celts, notched-pebbles or net sinkers, and even "ceremonial objects." Certainly not one of these can be called "unfinished." It is confusing to call any one form of stone weapon or tool a "cache implement." It would be just as logical to call the specie hoarded in treasury vaults something different from the coin in circulation.

If, to return to the large blades, they cannot come under the category of unfinished objects, does this not strike a blow at the cunning inferences drawn from recent studies of quarries, where the Indian gathered his material for implement making? The various grades supposed to lead from the raw material to the finished product is a lovely picture as drawn by pen and pencil, but in truth fails to be reproduced in nature. It is but a fancy landscape, the like of which the sun never shone upon. A picture that is so seductive as to convince the unwary, but in truth befogs the onlooker; a picture that makes essay writing a pleasant pastime, but —?

The pre-history of man in the Delaware Valley is not to be read by calling large stone blades unfinished, and the ruder forms scattered in the gravel the refuse arising in manufacturing the former. If this were true, there would be less of a problem to solve, but even then there would be as many difficulties in the way of accepting the Indian's modernity and in denying the palæolithicity of such objects as have that import in other countries.

When Holmes shall drive the fog away
That now enwraps the scene,
And in the light of later day
He stands with smile serene,
And points to how in modern time
The red man came equipped
With every blessing of the clime,
From elsewhere newly shipped;
We can but hope he'll name the date
When first upon the strand
This red man stood with heart elate,
And where he chanced to land.
Then, noble efforts nobly made,
Before he seeks a rest
Point out how far is *truth* displayed,
And just how far he guessed.

CHARLES C. ABBOTT, M.D.

Museum of American Archæology, University of Pennsylvania, Philadelphia.

The Largest Trees in the World.

A RECENT article in *Science* (No. 523, Feb. 10, 1893, p. 76) repeats the old idea, which has been frequently refuted, that the *Sequoia gigantia*, or Big Tree of California is the largest tree known. It has been shown many times that these trees are surpassed in both height and girth by the gum trees of Australasia. A large number of species are known, and many of them are mentioned in Baron von Mueller's "Extra Tropical Plants," recently reviewed in these columns. An extract from this book will be of interest as giving the dimensions of some of these immense trees. Of *Eucalyptus amygdalina* it is said: —

"In sheltered, springy, forest-glens attaining exceptionally to a height of over 400 feet, there forming a smooth stem and broad leaves, producing also seedlings of a foliage different from the ordinary form of *E. amygdalina*, which occurs in more open country, and has small narrow leaves and a rough brownish bark. The former species or variety, which has been called *Eucalyptus regnans*, represents probably the loftiest tree on the globe. Mr. J. Rollo of Yarragon measured a tree which was 410 feet high. Another tree in the Cape Otway ranges was found to be 415 feet high and 15 feet in diameter where cut in felling, at a considerable height above the ground. Another tree measured 69 feet in circumference at the base of the stem; at 12 feet from the ground it had a diameter of 14 feet; at 78 feet a diameter of 9 feet; at 144 feet a diameter of 8 feet, and at 210 feet a diameter of 5 feet. [Thus, at a height in the air exceeding the height of almost every North American forest tree, this specimen had a diameter equal to most of our largest forest trees at the ground.] Other trees are known with a stem-circumference of 66 feet at 5 feet from the ground. Prof. Wilson and Colonel Ellery obtained at Mount Sabine a measurement of 21 feet 8 inches in diameter of a stem, where cut, the length being 380 feet. Colonel Ellery had repeatedly reports of trees seven axe-handles in diameter, and he met a tree on Mount Disappointment with a stem diameter of 33 feet at about 4 feet from the ground." Other species also attain enormous size. *Eucalyptus diversicolor* is known to grow 400 feet high, and trees have been measured 300 feet long without a branch! Boards 12 feet wide can frequently be obtained. *E. globulus* grows 300 feet high and furnishes ship keels 120 feet long. *E. obliqua* also attains 300 feet in height and 10 feet in diameter. A note in a recent number of *Garden and Forest* mentions a tree in Victoria 471 feet in height.

The colossal size of the trees of this genus is not the only peculiar feature they possess. Some are of exceedingly rapid growth, and are at the same time very durable. *Eucalyptus amygdalina*, for example, grew to a height of 50 feet in 8 years in the south of France. *E. citriodora* grew 20 feet high in 2 years in a district subject to protracted drought; and a trunk 40 feet long and 20 inches in diameter only broke after a flexion of 17 inches, under a pressure of 49 tons. *E. corymbosa* is very durable, fence posts that had been in the ground for 40 years showing hardly any decay. *E. globulus* grew 60 feet high in 11 years in California, and in Florida 40 feet in 4 years, with a stem a foot in diameter. The writer has seen trees in California, two years after planting the seed, 20 feet high; and the wood, although easily cut when green, becomes almost as hard as iron when dry. In Guatemala it grew 120 feet in 12 years and had a stem diameter of 9 feet. Railway sleepers made of *E. leucoxylon* were quite sound after being laid 24 years. Piles driven for a whaling jetty in 1834 were taken out in 1877 perfectly sound, although the water swarmed with Teredo. This was *E. marginata*. Still more remarkable is the fact that some species withstand excessive heat and also a considerable cold. *E. microtheca*, for example, resists a temperature of 18° F. in France and 154° F. in central Australia. Besides serving as a timber tree, many species of *Eucalyptus* are used medicinally, producing a volatile oil very useful in treating various infectious diseases, like scarlet fever, especially when applied externally. Grown in malarious districts, they possess the power of purifying the air. Altogether, the genus may be classed as one of the most remarkable in the whole world.

JOSEPH F. JAMES, M.SC.

Washington, D.C., Feb. 24.

Fern Frost.

AT Greensburg, Indiana, on the morning of Jan. 24, the trees and fences were fringed with a beautiful feathery frost. It was really a snow frost, but the flakes or aggregations of crystals were fern-shaped instead of star-shaped. Every branch of a tree or wire of a fence bore a line of snow-frost on its south side, making a downy fringe of one-half inch, or more, in length. A weeping willow tree and a fence of wire-netting were most striking in this decoration.

The barometer was 30.15; temperature, 16; moisture about 90; the air seemed perfectly still, but on wetting the finger and holding it above the head the north side was cooled, showing that there was some movement and from what direction. This showed why the fern frost was arranged on the south side of twigs and other objects. There could have been no perceptible wind during the formation of this fern frost, for I could not touch a branch or twig without causing much of this fluffy frost to fall; and, later, little local breezes caused little snowfalls from the trees. However, during the formation of this frost there must have been just enough atmospheric movement to prevent deposition on the north side, while on the south side of twigs, etc., there was a region of still air in which the moisture was crystallized.

The ultimate crystals of each fern-like flake were prisms and hexagonal plates. The parts formed by prisms and very small hexagonal plates corresponded to the rachis and basal portions of pinnæ, while the expanded portions of pinnæ and pinnules were represented by hexagonal plates alone. The terminal plates were the largest. They diminished in size as they approached the axils, where they were replaced by delicate elongate prisms.

These fern flakes are simply modifications of star-flakes. Each fern-flake is one ray of a star, the point of attachment to the twig or wire corresponding to the centre of the star. Their attachment to a fixed support was a condition of unusual development, some being more than one-half inch in length. The completed star would have been gigantic compared with a star-flake formed in a snow cloud.

Some of these fern flakes were still further modified so as to represent a half ray, resembling one-half of a fern frond divided longitudinally. Perhaps in such a one the axis of the fern-flake represented the line of demarcation between still air and moving air.

This was a kind of snow-cloud hanging on the trees, formed under the concurrence of particular conditions of temperature, moisture, and atmospheric movement. The conditions that favor the fringe-like, or one-sided, arrangement of frost must be very unusual.

W. P. Shannon.

Greensburg, Ind.

On the Use of the Compound Eyes of Insects.

My personal knowledge of Dr. Dallinger enables me to accept without hesitation his statement in *Science* of Jan. 6 (p. 11) that the wood-cut on page 908 of "The Microscope and its Revelations" corresponds in every particular with the photograph from which it was taken. I should, however, like to put myself right with your readers by explaining that the photograph to which I referred as "the original" was a positive print exhibited at the meeting of the Royal Microscopical Society on Nov. 19, 1890, by Professor Bell, who said that it had been sent by Professor Exner to Dr. Sharpe, by whom it was lent for exhibition on that occasion. I examined this photograph with much interest at the close of the meeting and took the opportunity of making a sketch of it in my note-book at the time. This sketch undoubtedly shows the letter R to be the right way about, with the church facing towards the left; and although after a lapse of two years it might not have been possible to trust entirely to memory in the matter, it is impossible to suppose that I made otherwise than a true copy of the picture which I held in my hand. I therefore infer that the photograph to which Dr. Dallinger refers must have been printed the reverse way to the one which I saw as above stated.

R. T. Lewis.

Ealing, London, S. W., England.

AMONG THE PUBLISHERS.

The publishers of Mrs. Helen Mather's "One Summer in Hawaii," the Cassell Publishing Company, announce a new edition of that book. The present state of affairs in Hawaii have renewed interest in the subject. Mrs Mather describes the people, their manners and customs, the natural resources of the island, and gives a personal description of Queen Lilliuokaulani, by whom she was entertained. The book is filled with illustrations showing the scenery and public buildings, and gives portraits of the Queen and her predecessors in office.

— G. P. Putnam's Sons announce for early publication "The Empire of the Tsars and the Russians," by Anatole Leroy-Beaulieu, translated from the third French edition by Mme. Ragozin; "Outlines of Roman History," by Professor Henry F. Pelham, of Oxford University, a work particularly designed for reading classes and higher-grade students; "Studies of Travel in Greece

CALENDAR OF SOCIETIES.

Anthropological Society, Washington.

Feb. 21.—Mrs. Matilda Coxe Stevenson, The Foundation of the Zuni Cult; Miss Kate Foote, Dual Civic Functions: A Study in the Evolution of Institutions; Thomas Wilson, Early Man in the Mississippi Valley.

Biological Society, Washington.

Feb. 25.—Sheldon Jackson, The Introduction of Reindeer in Alaska; M. B. Waite, Variation in the Fruit of the Pear due to Difference of Pollen; E. M. Hasbrouck, On the Development of the Appendages of the Cedar Waxwing; F. A. Lucas, The Food of Humming-Birds.

Philosophical Society, Washington.

Mar. 1.—Waldemar Lindgren, Two Neocene Rivers of California; Marshall McDonald, A Study of the Gulf-Stream in Relation to the Tile Fish.

Appalachian Mountain Club, Boston.

Feb. 27.—C. Willard Hayes, Through Alaska with Lieutenant Schwatka; an account of exploration in the Yukon Basin in 1891, and the first crossing of the St. Elias-Wrangell Range.

Mar. 8.—Edouard A. Martel of Paris, will be read by Frank W. Freeborn, The Land of the Causses. The Caves of Bramabiau, Dargilan, Padirac, etc.; Philip Stanley Abbot, His Ascent of the Weisshorn.

Society of Natural History, Boston.

Mar. 1.—E. S. Morse, A Curious Aino Toy; C. Willard Hayes and M. R. Campbell, The Structural Features (Geomorphology) of the Southern Appalachians.

Agassiz Scientific Society, Corvallis, Ore.

Feb. 8—Charles Pernot, Smokeless Fuel.

and Italy," a volume of historical and archæological papers by the late Professor Freeman; "Venice: an historical sketch of the Republic," by Horatio F. Brown. New issues in their various series will be "Napoleon," by W. O'Connor Morris, in Heroes of the Nations; "Story of Poland," by W. R. Morfill, in Story of the Nations; "The Silver Situation in the United States," by Professor F. W. Taussig, in the Questions of the Day Series. "Vertebrate Embryology," by Dr. A. Milnes Marshall, professor of zoölogy in Owens College, England, and C. H. Hurst, demonstrator of zoölogy in Owens College; and "A Junior Course in Practical Zoölogy," by the same authors.

— D. Lothrop Company announce "In the Wake of Columbus," an illustrated account of travel along the track of the great discoverer; "From Cordova to Cathay," by Frederick A. Ober, who was the special Columbus commissioner sent out by the World's Fair directory to gather facts and relics.

— The January *Century* has been out of print for some time, and of the February number the publishers now have unfilled orders for more than five thousand copies awaiting a new edition. A large first edition of the March *Century*, containing the Reminiscences of Napoleon at Elba, will be ready on the first day of March.

— At the recent meeting of the Indiana Academy of Science, Dr. Robert Hessler, of Indianapolis, read a paper on "An Extreme Case of Parasitism." It was a case of that extremely rare and almost extinct form of the itch known as "Norway Itch," the *Scabies Norvegica* of Hebra, and who first described it in 1852. The paper was prefaced by some remarks on the itch mite and on the itch. It was not until 1835 that the mite *Sarcopter scabiei*, De Geer, was universally recognized as the cause of the itch. There is no uniformity among medical authors concerning the scientific names for the mite. *Acarus scabiei* and *Sarcoptes hominis* are frequently given in medical works. The size is also variously given, from "very minute almost microscopic" up to "the size of a pin-head." Scabies, or the itch, is the result of the presence of the human itch mite on the body. Occasionally, although rarely, mites from the domestic animals produce a similar eruption on the human body. In an ordinary acute or epidemic case of the itch the number of mites is quite small, probably rarely exceeding one hundred adult animals. Norway itch is so rare that modern treatises on skin diseases, especially those of our country, do not describe it, very few even mention it. The writer is inclined to believe that a case of this kind corresponds, medically, to a "freak" or "sport" of the naturalist or evolutionist; it shows us what was formerly of frequent occurrence — owing to uncleanliness and a want of proper parasiticides. The afflicted man when first seen was covered with thick, creamy-white, leathery scales. "He was covered with scales like a fish." Some of these scales measured over one inch in diameter and one-tenth inch in thickness. These scales were not crusts or scabs, they were overgrowths of the skin due to increased cell activity from the irritation of the mites. A constant shedding of these scales was going on, a handful could be gathered daily. In a search for the cause of this skin eruption, the doctor found the mites and at once established the diagnosis. The epithelium, that is, the scales, were found to be full of mites and eggs and riddled with burrows or passages. Under appropriate remedies the mites were soon exterminated. The cause of the disease once removed, the skin soon regained its normal character and the patient was cured. Dr. Hessler made a calculation of the total number of mites and eggs on the body of the man when first seen. Pieces of scale of a definite size were stained, imbedded, sectioned and mounted in serials. Diagrams were made of each section, indicating the position of the mites and eggs, and the count made therefrom. A simple calculation gave the figures for the entire body. Here are the results: Eggs and empty shells, 7,004,000; mites in all stages of development, 2,009,000.

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SCIENCE

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Much pains has been taken to render the bibliography complete, and the author is indebted to Dr. Franz Boas and others for several titles and important suggestions; and it is hoped that this feature of the book will recommend it to collectors of *Americana*.

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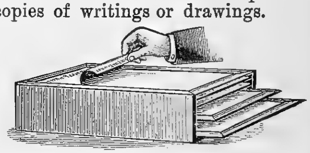

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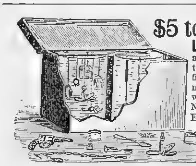

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SCIENCE

NEW YORK. MARCH 10, 1893.

THE BOTURINI-AUBIN-GOUPIL COLLECTION OF MEXICANA.[1]

BY D. G. BRINTON, M.D., LL.D., PHILADELPHIA, PA.

IN the year 1736, an Italian of long lineage but light purse landed at Vera Cruz His name was Lorenzo Boturini Benaduci, and the business which took him to Mexico was the collection of the arrears of a pension due some of the descendants of Montezuma who then resided in Portugal. This naturally led him to a study of the native history of Mexico, a pastime which soon grew to an enthusiasm, when he learned that the Blessed Virgin herself had appeared and talked with a poor Indian on the hill of Guadalupe. Fired by a noble frenzy, he decided to devote his whole life to these two objects,—the collection of every document which would throw light on the ancient history of the indigenous population, and the vindication of the apparition of Our Lady of Guadalupe.

To these aims he gave up nine consecutive years, and all the money that he could borrow or beg; for his own supply of that useful article was uncomfortably limited. But a foreigner, a begging foreigner. and that foreigner an archæologist, was a combination too repugnant to the Spanish constitution to be stomached long; so, in 1745, the vice-regal government seized Boturini, threw him into prison, and sequestrated his collections of books and manuscripts, so precious in his eyes, as he pathetically wrote. "That I would not exchange them for gold, nor silver, nor diamonds, nor pearls." How the true spirit of the collector breathes in those lines! But, alas! he was destined never to see them again. Removed from prison, he was sent to Spain for trial, where he died in 1749. His priceless collection was presented by the viceroy to the University of Mexico, whence it was scattered to different private and public owners.

Boturini was born in 1702. Precisely a century later, J. M. A. Aubin was christened, in a little town in the south of France. He was destined to partake of the same divine antiquarian fervor, and to re-collect for all posterity the scattered jewels of his predecessor's cabinet. With a liberal fortune and the best of introductions, he resided in Mexico from 1830 to 1840, and with singular tact and energy succeeded in securing a large part, and the best part, of the documents gathered with such toil by the Italian antiquary. He brought them to Paris, where he lived surrounded by them for fifty years, making very little use of them himself. and never permitting a single student so much as to look at them. Why this misanthropic narrowness? The reply should be guarded. A cloud hung over Aubin's lonely life. He, too, was imprisoned; as he claimed, by malignant enemies; but on a charge which forever blasts a life. I even heard indignant protests at his mere presence, when poor, old, and senile, he was led into the hall of the Congress of Americanists, in 1890, at Paris.

Enough of this sad subject. At any rate, M. Aubin merits the lasting gratitude of investigators that he preserved with scrupulous care his wonderful collection. When I saw him in 1890, it was no longer his. Financially ruined by investments in "Panama," he had accepted an offer for the whole of it from M. Eugene Goupil, a native of Mexico, French on the paternal side, tinctured with the blood of the native race through his mother. He bought it, not as an antiquary, but as an enlightened lover of his country and an intelligent patron of antiquarian studies. He placed the manuscripts in native tongues, Spanish or Latin, the wondrous colored pictographic scrolls on maguey paper, or on skins of animals, the ancient Codices, maps, and titles, in the hands of M Eugene Boban, a distinguished antiquary, well known in the cities of Mexico, New York, and Paris, from his long residence in them all. To his kindness I owe the privilege, enjoyed by few, of a leisurely inspection of this wholly unrivalled collection of Mexicana.

M. Boban's task was to make an analytical catalogue of the three hundred and seventy-two pieces of which the collection consists. He has completed that task in a manner in the highest degree creditable to his own scholarship and to the discriminating liberality of M. Goupil. His work is comprised in two very large quarto volumes of text, together of more than a thousand pages; and a third thick volume or atlas, containing photographic reproductions of some of the most remarkable documents. Yet this huge publication is but the mere beginning of the labor which must be expended on this mass of material before its value is extracted. As for myself, after seeing what it contains, I made up my mind that all that has yet been written about Mexico previous to the conquest has no more importance than have the histories and descriptions of ancient Egypt which were composed before the method of hieroglyphic interpretation was discovered.

The title of M. Boban's work is:—

"Documents pour servir à l'Histoire du Mexique. Catalogue Raisonné de la Collection de M. E. Eugene Goupil (Ancienne Collection J. M. A. Aubin)." Paris. Ernest Leroux, Editeur, 1892. Price, 180 francs.

The first volume begins the catalogue with the celebrated *Historia Chichimeca*, an ancient Codex on agave paper, painted in blue, green, and brown, and giving in hieroglyphic characters the history of pre-Columbian Mexico, from A.D. 963 to 1428. It was translated by the early native chronicler, Ixtlilxochitl, and for that reason all the ten leaves of which it consists are reproduced in phototype with the explanations. Following this, a full description is given of what are called the "Maps" of Tlotzin, Quinatzin, and Tepechpan, long pictorial scrolls, partly published by M. Aubin, relating the migrations and traditional history of the Nahuas. Next comes the curious *Codex Cruciformis*, an original. painted, figurative manuscript relating to Tezcuco and Tenochtitlan. It is painted in four quarters, of thirteen compartments each, somewhat like a Maltese cross, whence the name given it.

The famous *Tonalamatl*, or "Book of Days," is then taken up. This is an original, hieroglyphic book of eighteen leaves, magnificently colored in red, black, green, and brown. Its purpose is that of a religious and divinatory calendar, serving at once as a ritual and as the basis for astrological prognostics. None of the documents in the collection presents to the eye a more striking appearance than this venerable pictograph, concealing under its strange and vivid coloring the dark wisdom of the Aztec diviners.

Relating to the same subject, perhaps, is a remarkable painting on a tanned deer-skin, representing a disk with fifty-two points, that being the number of years in a Mexican cycle. A phototype is given of this, and M. Boban thinks it is intended to prescribe days for the worship of the sun, *Tonatiuh*; but it is more likely to be simply the computation of a calendar.

Another historical pictograph is the *Codex Mexicanus*, an original, of forty-seven leaves, narrating the history of the Mexicans from their departure from the mysterious land of Aztlan down to the year 1590. This is native work, though late in the sixteenth century. The *Codex de Vergara* is another figurative document, defining boundaries and titles, whose date is 1528. Like many of these title deeds, it contains valuable hints as to the nature of the Mexican pictographic system.

The collection is peculiarly rich in books written in the Nahuat language. There are the *Historia Tolteco-Chichimeca*, the *His*

[1] Read before the Numismatic and Antiquarian Society of Philadelphia, March 2, 1893.

toria de Tlascala, the *Codex Chimalpopoca*, the *Anales de Cuauhtitlan*, manuscripts of Ixtlilxochitl, Leon y Gama, Father Pichardo, and others. Very curious are the catechisms of the early missionaries written in the Mexican hieroglyphic characters, the maps, charts, plans, "Titulos de Tierra," legal documents, and royal ordinances, throwing light on the early history and settlement of the territory of Mexico.

M. Boban concludes his long and arduous task by adding a comprehensive and well-arranged index to his volumes; and I should not omit to mention that he increases the practical value of his work by inserting a series of biographical notices and many quotations and references to contemporary Mexican archæological literature.

I have reserved the best piece of news to the last. I learn from good authority that it is the intention of the enlightened M. Goupil finally to concede to scholars the access to this marvellous storehouse of American antiquity by placing it in the possession of the Manuscript Department of the Bibliothèque Nationale. Certainly no one in this generation will more deservedly receive the thanks of all genuine Americanists than the donor of such a treasure to public use.

TIME-PERIODS OF THE MAYAS.

BY PROFESSOR CYRUS THOMAS, WASHINGTON, D.C.

In "Current Notes on Anthropology," *Science*, Feb. 10, reference is made by Dr. Brinton to the article on "Time-Periods of the Mayas," by Dr. Förstemann, in *Globus* (Bd. 63. No. 2). In closing this notice, he remarks that "Dr. Förstemann's discussion of the subject amounts to a demonstration,"—an asserton I think he will find it difficult to maintain. I presume, however, it was based on Dr. Förstemann's well-known ability as an investigator in this line, his long and faithful study of the time-symbols of the Maya Codices, and his great caution in presenting conclusions, rather than on a thorough examination of the data.

I am indebted to Dr. Förstemann for several valuable suggestions in my work in this line; it was through one of these, given in a private communication, that I was led to the evidence on which I base some of the objections offered here to his conclusions.

He believes that the different steps by which the Mayas reached their final calendar with the year of 365 days, consisting of 18 months of 20 days each, were as follows: First, the period of 20 days, next the period of 18 months, giving the year of 360 days; next, the year of 364 days, formed by adding four days at the end of the eighteenth month, at which time the division into periods of 13 days was introduced; and, finally, the year of 365 days, by adding another day at the end of the eighteenth month. The evidence on which this is based he believes he finds in the Codices, chiefly in the Dresden Codex. He believes he finds evidence of the use of all these years, as also of the Tonalamatl or Sacred year of 260 days in the latter Codex.

We take first his basal or cyclical period: —

1
19
0
0 } or 14040 days, found in the right column of Dres., p. 73.

There is no doubt that this denotes, as he contends, 14040 days, or 39 years, if we count 360 days to the year. "From this," he adds, "proceed two series, of which one has the difference 65, . . . while the other increases by 54." He alludes to the series running through the upper division of pp. 71–73, where the difference is 54; and that running through the middle and lower divisions of the same plates, where the difference is 65 (see our "Aids to the Study of the Maya Codices," pp. 333–337). It is to be noticed, however, that there is no connection between his typical number and these series, and why he has thus referred to them is not apparent. On the contrary, it appears from the 9 Ix below it to belong to the right-hand series of the upper division. I also made the mistake in my "Aids" (p. 337, note) of connecting this 9 Ix with one of the series mentioned.

The point he makes is, that this number is divisible by 360, and that the two series referred to can be explained on this theory, hence it is presumable a year of this length was used in constructing them. Now it must be conceded that if these series can be explained and traced out in accordance with the usual calendar of 365 days to the year, and the four year-series, Dr. Förstemann's argument loses its force, and falls short of a "demonstration."

Let us see if this can be done. For this purpose we present here a part of the series in the middle division of the plates alluded to.

1						
4	19	16	13	9	6	3
15	10	5	0	15	10	5
4 Manik	4 Ik	4 Caban	4 Eb	4 Manik	4 Ik	4 Caban.

This series, which begins with the number and day at the right, ascends, and is to be read from right to left, the difference being 65 days, or 3 months and 5 days, if the numbers are intended to denote days, months, and years. The 19 in the 6th, or next to the left-hand column, is evidently the same as 1 unit of the third order and one of the second, or 1 year, 1 month (counting 360 days to the year). If the year contained only 360 days, it must have commenced year after year with the same day unless there was an arbitrary change. On this theory the numbers in the lower line of numerals (with one exception) might denote the day of the month. For example, Caban would be the 5th day of the month if the year began with Ben, or with Ix counting from the last day of the month; Ik the 10th, Manik the 15th, and so on through the entire series, and also in numerous other series. This would seem to be a sufficient "demonstration" of the theory, and was considered so by me in my "Aids," but the numeral system in the Maya calendar is exceedingly deceptive. Before this is conceded, it is necessary to overcome the following objections: The figures in the middle row do not give the months correctly nor those in the upper the years. The 3, 5, in the first column, really donote the 5th day of the 4th month. While the 1 in the left-hand column, if taken in this way, would refer to the second year. Moreover, if the numbers in the "month" and "day lines" were intended to denote the numbers of the months and days of the months there could be no blanks, such as we see in

13
the 4th column above (0). That the symbol represented by the cipher signifies "nothing," is admitted by Dr. Förstemann, and is proven by the number in the month line. As upon the theory of 360 days to the year, all the years should begin with the same day, while this method of counting time remained in vogue, the different series based upon this method should be referred to years commencing with the same day. This, however, is not the case, as the series now under consideration pertains to a year commencing with Ben; while the long series on pp. 52–58 can be reckoned only in years beginning with Lamat. Nor is it possible to bring these series into harmony in this respect upon the theory of a year of 360 days unless we assume there were arbitrary changes, which amounts to begging the question. It is also inconsistent with this theory that the series on pp. 63–64, which Dr. Förstemann believes to be founded on the year of 364 days, gives precisely the same results in the respect mentioned as the other series referred to. In truth, it is impossible that the "day" and "month lines" of numerals should indicate the days of the month and numbers of the months throughout a series extending over several years, except upon the theory of 360 days to the year. We are forced, therefore, to the conclusion, even on Dr. Förstemann's theory, that these series are only successions of intervals in which the columns of numerals simply denote the sum of these intervals at the various steps.

We will now proceed in our attempt to explain the series on pp. 71–73, of which a portion is given above, by the usual calendar system of 365 days to the year and the four year-series. No difference between the two systems will appear until we reach the end of the first year of the series. As this is reached in passing from the 5th to the 6th column, { 16 / 5 / 4 Caban } and { 19 / 10 / 4 Ik, } we start with 4 Caban of the 5th column. As before stated, this series proceeds from right to left and is to be counted from the last day of the month — or these days considered as the first of the month, as Dr. Seler concludes. As Caban, counting in this way, is the 5th day of the month in Ben (Ix) years, we take it as our starting point. As the figures in this column $\left(\begin{matrix}16\\5\end{matrix}\right)$ show that 16 months and 5 days, or 325 days, have been counted up to this point, our 4 Caban must be the 5th day of the 17th month. It follows from this that the starting-point of the series is 5 Ben and that the year is 5 Ben (or 5 Ix, counting from the last day of the preceding month). If there are 365 days in a year there will be 40 days (out of the 65) to count in this year and 25 in the next. As the year (including the 5 added days) will end on 5 Caban, the next will begin with 6 Ezanab (a Cauac year). Counting forward 25 days in this year, we reach 4 Ik, which is the day under the 6th column, but it is the 5th and not the 10th day of the month. This is not an accidental hit, but has been found true in all these series so far as I have tested them, except that in some the months begin with the usual days as the series on pp. 63-64.

But this is not all, the same result will be obtained in the series we are examining if we start with 4 Caban of either of the other three years, except that 4 Ik in the Kan (Akbal) years will fall on the 20th day of the month, in Muluc (Lamat) years on the 15th, and in Ix (Ben) years on the 10th.

It follows, therefore, that these series can be traced and explained as well upon the theory of 365 days to the year as 360. That the series on pp. 46–50 can only be followed out on the usual calendar system is admitted by Dr. Förstemann, and it was through a suggestion he kindly gave me a year or two ago that I was induced to examine it on this theory.

Is it therefore legitimate, in view of these conflicting results based upon the Codex and Calendar, to say that Dr. Förstemann's discussion "amounts to a demonstration?" Does not what has been shown do away with his conclusions so far as they are based upon the supposed year of 360 days? If all the series susceptible of being tested can be explained satisfactorily in conformity with the usual Calendar, is there any necessity of resorting to any other theory?

It is somewhat strange that Dr. Förstemann should consider the series we have been referring to, the sum total of which is $\left\{\begin{matrix}5\\1\\0\end{matrix}\right.$ or 1820 days, as based on the year of 360 days; and yet refer that on pp. 63–64, which has precisely the same sum total, to the year of 364 days. Both are divisible by 364 and neither by 360, and the numerals in both are given on the same plan, the only difference being that in one case the intervals are 65 and in the other 91. Is this a sufficient basis upon which to found the theory of such a radical change in the calendar system? Yet it seems to be the only foundation for this conclusion. That there must have been steps of improvement in the calendar to bring it nearer and nearer the true year is admitted, but is it likely that these various stages of progress showing years of different length will be found in one and the same Codex? It is only necessary to state that this series can also be counted by the usual calendar. In speaking of the divisors of 364, Dr. Förstemann says: "The number 364 is, however, not merely equal to 4×91, but also 28×13, and this seems to have been the cause of the year being divided into periods of 13 days, as the period of 20 days was a natural divisor of 360 days." As the steps in the formation of the calendar indicate periods of usage of the different years, we must conclude, if this supposition be correct, that the division of the year into periods of 13 days was not in vogue during the time the year of 360 days was in use. Nevertheless, we see by the red numerals attached to the days that it is used in connection with the series on pp. 71–73, which he thinks is based on the year of 360 days. In this we have another illustration of the objections which present themselves to the supposition that years of different length were used in the same calendar.

There is another consideration which, according to the opinion accepted by most archæologists, stands opposed to the idea that the year of 360 days should be found in the Dresden Codex. It is that the time-system used on the Palenque "Tablet of the Cross" is that of the usual calendar except that the count is from the days usually given as the last of the month. This is susceptible of proof beyond any reasonable doubt. If, as is generally supposed, this tablet is one of the oldest records remaining in which calendar dates are used, and antidates the Dresden Codex, is it probable we shall find an older year in the latter?

Dr. Förstemann's suggestion that the series on p. 24 and pp. 46–50, especially those on the latter plates, refer to the revolutions of the planet Venus, appears to rest upon a surer foundation than his theory in regard to the year of 360 days. It is a singular fact that the series on p. 24 is divided into periods of $\left.\begin{matrix}8\\2\\0\end{matrix}\right\}$ or 2920 days, which is an exact multiple of 584; and that the series on pp 46–50 is not only divided into periods of 2920 days, but these are subdivided into periods of 584 days. As will be seen by referring to the plates of the Codex 46–50 or to my "Aids" (p. 298), the red counters at the bottom of each of the five plates are $\begin{matrix}11 & 4 & 12 & 0\\16 & 10 & 10 & 8\end{matrix}$ or 236, 90, 250, and 8, the sum of which is 584, the length of the apparent revolution of the planet Venus. As the numeral series (the word "numeral" is used specially here) runs through five pages, the period 584 being repeated in each, we have a total of $\begin{matrix}8\\2\\0\end{matrix}$ or 2920 days. But the "numeral series" is only one-thirteenth part of the entire series, for when one horizontal *line* of the day columns at the top has been traced through the five pages to its end on p. 50, we return to p. 46 and trace the second *line* through, for they connect according to the red counters, and so continue until we have traced the thirteen lines ending with 1 Ahau, the lower right-hand day-symbol on p. 50. Thus we see that the entire series embraces a period of 37960 days, or exactly 104 years of 365 days, a fact noticed by Dr. Förstemann. Yet this is not all that we find in this respect on these five plates. They contain two other precisely similar series. The one which has been referred to is based on and relates only to the month symbols which form the upper line of the text in the middle division; the next, using the same series of days and numerals, is connected with the month symbols forming the upper line of the text in the lower division, and the third with the month symbols in the lower line of the lower division. Dr. Förstemann also alludes to these three series. As each series embraces 104 years, we might suppose the three together to form one great cycle, or Ahau-Katun, of 312 years, but, unfortunately, there seems to be no other connection between them than that they are divided into the same intervals and same days. This is evident from the fact that the upper series (not counting back the 11 months and 16 days with which it begins) commences with 3 Cib, the 4th day of the month Yaxkin in the year 11 Ben (or 11 Ix, counting from the last day of the month); the second or middle series from 3 Cib, the 8th day of the month Zac in the year 4 Muluc (or Lamat);[1] the last or lower series with 3 Cib, the 19th day of the year 4 Ezanab (or 4 Cauac, counting from the last day of the month).

If we count back 11 months 16 days from the first date given in each series, thus reaching the initial day, the following singular result is obtained: the first is found to commence with 3 Yimx, the 13th day of the month Mac in the year 10 Muluc (or 14th day, counting from Lamat); the second, on 2 Yimx, the 18th day of the month Kayab in the year 3 Kan (19th day, counting from Akbal); the third, on 2 Ymix, the third day of the month Xul in the year 4 Cauac (4th day, counting from Ezanab). Therefore, if we arrange them to follow one another in time, we shall find an interval between the first and second of 19 years, and between the second and third of 27 years. It is therefore probable that these three series cover substantially the same period, the dates of the different series falling, in most cases, in different months of the same years; or, in other words, that the periods embraced overlap one another. The great length of the series, and their failure to connect, present the chief reasons for doubting Dr. Förstemann's suggestion in regard to their meaning. On the other hand, there is an oft-repeated glyph in the text which seems

[1] It is strange that the author of the Codex has, in this single instance in all these long series, counted from the 1st day of the month.

to give credit to the theory. Notice of this, however, will be reserved for a subsequent paper.

Attention is called again to the series on pp. 63–64, in order to remark that, by counting back from 13 Ix 91 days, we find that the series commences with the first day of the year 12 Kan. Then, by tracing it through, according to the usual year of 365 days, we find that it ends with 13 Akbal, the last day of the year 3 Kan, omitting the five supplemental days at the end. Adding these five days, the total — 1825 — is exactly divisible by 365. However, it seems that the series should be extended 42 days more to include the other days of the last column (see "Aids," p. 330); in which case neither 365, 364, nor 360 would be an exact divisor of the sum total.

We refer next to Dr. Förstemann's theory that the long series on pp. 51–58 refers to the length of the lunar month. As he admits, the number of days, counting to the last, is 11960, though the sum of the intervals between the columns, as shown by the final numeral, is 11958. These intervals are generally 177 days, but 9 of 148 days occur at nearly equal steps, and 6 of 178 at irregular steps. He finds that by multiplying 29 by 3 and 30 by 3 and adding together the products he obtains 177; that the sum of the products of 29 by 2 and 30 by 3 is 148. To obtain the 178, he finds it necessary to arbitrarily add 1 to the products of 29 by 3 and 30 by 3. Next, he finds that by multiplying 177 by 54 — the number of times this interval occurs in the series — 148 by 9 and 178 by 6, and adding thereto 6, he obtains as the sum of the products 11958. He ascertains in this way that 29 occurs 198 times and that 30 occurs 207 times, making together 405, and that 11958 divided by this sum gives 29.526 days, which falls short of the lunar month but one four-thousandth part of a day. As he adds 6 days to his several products to obtain the number 11958, would it not be as well to add 8 days, making 11960, the true length of the series, which, divided by 405, gives as the quotient 29.53 days, precisely the desired figures?

Notwithstanding my high appreciation of Dr. Förstemann's ability as an investigator, and of his great caution in presenting conclusions, I cannot help thinking that his love for numerical coincidences, created by his long study of the time series of the Dresden Codex, has, in this instance, led him to accept as satisfactory what he would have hesitated to approve had it been presented by any one else. Now, 11960, the true length of the series, embraces precisely 46 periods, or sacred years, of 260 days, so often repeated in the Codices, the whole series and each of these periods commencing with 12 Lamat and ending with 11 Manik, initial and terminal year and month days, according to the method of counting from the last day of the month, which I had not discovered when my "Aids" was written. Is it not, therefore, more reasonable to conclude that the chief relation of the series is to this sacred period? This inquiry is certainly pertinent in view of the fact that neither 29 nor 30 appears singly or in multiple at any point in the series, that the total is first lessened by subtracting 2 and the products increased by the addition of 6. It is proper, however, to admit here that the interval 178, which is an increase by 1 of the usual period of 177 days, is difficult to account for, but such difficulties occur at many points in this Codex, and Dr. Förstemann's attempt at explanation involves so many assumptions as to cause us to hesitate before accepting it.

In order to show the uncertainty of the method adopted in regard to the last mentioned series, we will apply it to one not referred to by Dr. Förstemann, running through the lower division of pp. 30–33. In this case the total sum is 2340 days, and the uniform interval 117. Now if we multiply this interval by 5 we obtain 585, but one day more than the time of the apparent revolution of Venus. Or, if we multiply 584 by 4 and add 4, we obtain 2340, the number of days in the series; and the result is obtained by a less addition than that made by Dr. Förstemann in obtaining the lunar period. Now let us try another experiment in order to find the lunar period, thus: $29 \times 3 + 30 \times 1 = 117$ and 2340 divided by $117 = 20$. This will give us 60 periods of 29 days and 20 of 30 days, and dividing 2340 by 80, the sum total of these, we obtain 29.25 days, lacking only about one-fourth of a day of the correct time. Finally, we observe that 2340 days equal 9 of the sacred years of 260 days each, probably the real basis of the series, as 13 and 20, from which the latter is formed, are both factors here — $9 \times 13 = 117$, $13 \times 20 = 260$, and $260 \times 9 = 2340$.

If we turn to the series on pp. 46–50, in which Dr. Förstemann thinks he finds the Venus period, and apply the method of figuring above alluded to, we shall obtain some curious results. As we have seen, the intervals which together make the 584 days are 236, 90, 250, and 8 days. Are these intervals arbitrary, depending upon arrangements by the priests or by the scribe, or should we infer that they always depend upon the periodicity of certain natural phenomena, and hence form factors or multiples of time-periods? Although the latter may be generally true, the proof of which seems to be the chief object Dr. Förstemann has in view in his mathematical search, yet there are many of the intervals and periods which apparently defy all efforts to fit them into place. That 13, 20, and 18 will most frequently appear is to be expected, as they are always factors, but the coincidences in regard to other supposed time-periods (aside from the ordinary and sacred years) are to be regarded with doubt unless there is something more found than the occasional appearance thereof as factors. For instance, if we take 236, one of the intervals mentioned above, we find that it can readily be made to coincide with the lunar period; thus: $29 \times 4 + 30 \times 4 = 236$. This will give as the time of a revolution 29.5 days, which varies less than an hour from the true period. Yet for all this shall we conclude that here we find allusion to the moon's period? By no means, for this is only a recurring interval; and the others, which go to make up the 584, — the 90, 250, 8, — do not coincide with the moon's revolution or any other known time-period; 90 and 8 are factors of 360, but this number, as we have found, is one of the counters in these series.

The supposition that the revolution of Mercury is indicated by the numerals on p. 24 is certainly based on very slender data. This is found only in the fact that 115, the time of a revolution, is a divisor of the large number 11960, which is a multiple of 260, on which it is doubtless based. Why he has referred in this connection to p. 24 is not apparent. I do not find any relation here between a 1 Ahau and 4 Ahau (the latter is found but once on the page); nor do I find the number alluded to (11960) as the terminus of a series or an interval. There are two series on the page, or one series in which the interval varies. That which occupies the lower three-fifths of the right, commencing at the bottom, running to the left and up, has 2920 as its interval, of which 115 is not a factor. The interval of the other, the terminal columns of which are found at the left below, is 2200. This is not divisible by 115. Therefore, so far as I can see, Dr. Förstemann's only basis for the supposition that the Mayas had ascertained the period of the revolution of Mercury is found in the fact that the large number 11960, which is found several times in the Codex, is divisible by it. Can it be said that a conclusion based on no other evidence than this "amounts to a demonstration?"

That Dr. Förstemann has made progress in the study of the Codices by calling attention to the relations of these numerals to one another is cheerfully admitted, and that he has thrown light upon their meaning and suggested lines of investigation regarding them is undoubtedly true. Yet his discussion in the paper alluded to cannot be considered a "demonstration," when the same data may be used legitimately to lead to quite different results from those he obtains. The explanation which accords with the known Maya Calendar should be accepted in preference to that which requires a radical change, especially when that change is so radical as to wipe out the chief land-marks by which the Mayas were accustomed to reckon time.

Allusion has been made to the method of counting from the last day of the preceding month, — or, as Dr. Seler holds, commencing the months (and hence the years) with the days usually counted the last. Although not essential to the present discussion, we may say in reply to the suggestion which will arise in the mind of the reader, that the first method would necessitate beginning the count of the days from the last day of the preceding year, that this may furnish an explanation of what has hitherto been an unsolved problem — the numbering of the Ahaus. By counting in this way we can readily see why the first Ahau of a Grand Cycle or Ahau-Katun would be numbered 13.

NOTES ON THE FORMATION OF THE IRON ORES.[1]

BY JAMES D. ROBERTSON, E.M., JEFFERSON CITY, MO.

AMONG the deposits of iron ores in Missouri, those of specular hematite in the sandstones of the Ozark region, although not so well known as the porphyry ores, are an important and extremely interesting class. These deposits are found in caves in the Ozark series of limestones and sandstones, heretofore considered of Lower Silurian age, and the equivalent of the Calciferous sandrock of New York State. From recent investigations of the Missouri Geological Survey, there is reason to believe that these rocks are of Cambrian age.

At Cherry Valley, in Crawford County, there is a large deposit of specular hematite of this class. It occupies a depression in the strata formed by erosion and undermining. The ore is a good one for foundry purposes, and is smelted at the Midland Furnace, about six miles distant from the bank. The ore in the upper portion of the bank is harder and more silicious than that found deeper. It carries silica in the form of amorphous yellow jasper and also as quartz. The quartz is found in very perfect, singly terminated crystals, both colorless and amethystine, and of a dark, smoky color, the latter being due to included crystals and fragments of hematite. Specimens are in the Survey collection of perfect crystals of amethystine quartz, studded with crystals of specular hematite and frosted with minute acicular crystals of goethite of a beautiful golden-brown color. The ore has many vugs or cavities, which are lined completely with crystals of hematite, magnificently colored — red, golden-brown, peacock-blue, green, etc. While examining a number of these specimens lately, the writer's attention was drawn to the peculiar appearance of one, the markings of which had a great resemblance to the stem of a crinoid, while in another part of the same specimen there was a somewhat indistinct impression of a stem and cup of a crinoid. The specimen in question was submitted to Professor Van Hise of Madison, Wis., and by him to Dr. Birge, professor of zoölogy in the Wisconsin State University. These gentlemen say that there is not the least doubt in their minds of the organic origin of the peculiar markings of this specimen. Since then one more specimen of like nature has been found.

These organic remains, occurring in the deposit of specular ore just described, have a significance perhaps not wholly seen at first glance. They indicate that certainly a portion of the ore in these deposits has been formed by direct replacement, molecule by molecule of limestone by the iron from ferruginous waters. They also suggest that the chert, which is so abundant in these limestones, was the source of the silica in the ores. It is thus very probable that the iron was originally substituted as carbonate and subsequently concentrated and oxidized. The following terse statement by Professor Van Hise in regard to the deposit formed in a peculiar manner in Wisconsin is applicable here:[2] "The chemistry of the process assumes the following: that the oxygen of the percolating waters is sufficient to oxidize iron carbonate not in solution, and set carbon dioxide free; that the resulted carbonated waters are sufficient to take iron carbonate in solution; that if such waters bearing dissolved carbonate are mingled with waters bearing oxygen, the iron carbonate, or a portion of it, will be precipitated; that silica may be carried in percolating waters; that a carbon dioxide solution sufficient to precipitate silica by dilution may be made so weak in carbon dioxide that it would be capable of taking silica into solution. All of these facts and principles of chemistry are so well known that no discussion of them or reference to authority is needed."

One more question affected by these specimens is that of their age. While the fossil forms are too indistinct to identify the species, they are undoubtedly of Lower Carboniferous age. This suggests the idea that Lower Carboniferous rocks extended at least to this distance over the central portion of Missouri, and further that these deposits of iron ore had at least not ceased forming until after this period. These ore deposits, as stated before, are found in Cambrian rocks, but the precise period of their formation cannot be determined from our present knowledge of the facts in the case. So far, but one deposit of Lower Carboniferous rocks has been found in place anywhere near here, and that is a very small one. The writer has found fragments of chert with Burlington fossils on the hills some thirty miles west of here, but, with these exceptions, knows of no deposits of these rocks nearer than St. Louis County.

It is evident, therefore, that erosion has played an important part in removing the mantle of Lower Carboniferous rock, and it is probable that this agency at the same time prepared the underlying Cambrian rocks for the reception of the iron ores.

It is not within the scope of this article, however, to demonstrate the questions of the origin and age of these deposits, but merely to record the occurrence of these organic forms and to suggest their probable meaning. These questions are treated in a much fuller manner by Mr. Frank L. Nason, in the recently issued report of the State Geological Survey, on the Iron Ores of Missouri.

[1] Published with the approval of Arthur Winslow, State Geologist of Missouri.

[2] Am. Jour. Sci. (III), vol. xxxvii., p. 43.

NOTES AND NEWS.

AN important meeting of the Victoria Institute, London, England, took place last month, when Mr. J. W. Slater, F.C.S., F.E.S., read a paper in which he traced the difference between life and the physical forces, and reviewed all those experiments and arguments by which some had sought to prove that a key to the origin of life had been obtained. Contributions to the discussion of the question were made by Sir George Stokes, Bart., V.P.R.S., who stated that Lord Kelvin's recently alluded to suggestion that the germs of life on this earth might have come from the bursting of a remote star, was only intended by him to refer to the possible transmission from one part of the universe to another of life germs, but that the first origin of life itself we must all refer to God. Professor Lionel Beale, F.R.S., in supporting Mr. Slater's views, said that an absolute line must be drawn between the living and the non-living. Living matter was distinguished from all other matter by a property, power, or agency, by which its elements were arranged, directed, and prepared to combine according to a pre-arranged plan for a definite purpose. There was no gradual transition from the non-living to the living. Life was a special position independent of and not in any way related to the physical forces, it had nothing in common with any material forces, powers, or properties, and holding in the cosmos a remarkable and peculiar place. Professor Bernard of Dublin pointed out that all evidence went to show that vital forces are unique and not comparable with any other forms of energy. Dr. Rae, F.R.S., contributed some valuable remarks, as also did Dr. Biddle, the Revs. R. Collins, M.A., J. H. Clarke, and W. A. Pippet. Dr. F. Warner, M.D., F.R.C.P., made several valuable remarks on the question, which was also spoken on by Dr. Shettle of Reading, Dr. Schofield, and others. Dr. Schofield was very interesting in those remarks in which he pointed out what may be called the history of the controversy in regard to life and the physical forces, and in concluding he specially referred to the dictum of Professor Huxley, viz., "Life existed before organism and is its cause." What that cause was the Christian philosopher fully recognized.

— J. B. Lippincott Company announce for immediate publication a new (third) edition of the "Life of Benjamin Franklin," edited from original manuscripts and from his printed correspondence and other writings, by Hon. John Bigelow. Since the appearance of the previous editions the author has been able to secure considerable new and important information never before published, which is incorporated in the new work. This edition also contains several additional interesting illustrations. The work is bound in three volumes, as heretofore. A new edition of "Our Own Birds" has just been issued by J. B. Lippincott Company. The volume contains a natural history of the birds of the United States, revised and edited by Edward J. Cope, Corresponding Secretary of the Academy of Natural Sciences, Philadelphia. Although the first edition was profusely illustrated, twelve new half-tone plates have been introduced, which greatly add to the value of this edition.

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AN OLD VOLCANIC ERUPTION IN IOWA.

BY CHARLES R. KEYES, DES MOINES, IOWA.

IN the extreme northwestern corner of Iowa there is a small area of crystalline rocks commonly known under the name of the Sioux Quartzite or Sioux "Granite." These are the only strata in the State showing any decided traces of being changed through dynamic influences. Everywhere else within the limits of the province the rocks are so horizontal in their position, so undisturbed by mountain-making forces, and so unaltered in lithological characters, that it is generally taken for granted that all the strata in the State are sedimentary in origin and repose essentially as they were originally laid down in the waters of the great interior sea which once occupied the heart of the American continent.

The Sioux quarzite is a hard, vitreous mass with undulating bedding planes. Its geological age is regarded as much greater than that of any other formation in Iowa; not excepting even the old Cambrian sandstone of the northeastern portion of the State.

Although the area of the Sioux quartzite is quite extensive, no other crystalline rocks have been noted in the neighborhood until very recently. It is, then, of considerable interest to know that Professor G. E. Culver has lately discovered in the midst of the Sioux quartzite, of southeastern Dakota, in Minnehaha County, within three miles of the Iowa boundary, a large mass of trap, which extends for more than a mile along one of the tributaries of the Big Sioux River. A microscopical examination of these rocks shows it to be a well-pronounced, coarse-grained, olivine diabase, with such minerals as hornblende, black mica, and apatite present in addition to the feldspar, augite, and olivine.

The presence of this massive basic rock of unmistakable eruptive origin is very suggestive of the agencies that have been at work to some extent in changing the old sandstone. Further investigations will doubtless disclose other similar types of intrusive rocks in the Sioux quartzite in all three of the States already mentioned.

But the occurrence of this black trap rock, which has undoubtedly been cooled from a molten condition, is made even more interesting by other discoveries of still more recent date. During the past few years a number of deep wells or borings have been made at different places in northwestern Iowa. The depths reached are from 1200 to 2000 feet. Several of these borings are of special interest, inasmuch as they pass through all of the sedimentary rocks into the crystalline beds below, penetrating them in some cases to the extent of several hundred feet. A typical gray granite has been recognized in some instances; in others different types of eruptive rocks. One of the latest borings in this part of the State is the well at Hull, in Sioux County. At a very considerable depth a number of beds of flint-like rock were passed through. The different layers were separated by sand and gravel several feet in thickness, if the records are to be relied upon. Some of the flint-like fragments were sliced by Mr. S. W. Beyer of the Iowa Agricultural College, and upon microscopical examination proved to be what is known to geologists as quartz-porphyry — a truly igneous rock or lava, very acid in character, and essentially identical with granite, but cooling under somewhat different physical conditions.

The presence of the several sheets of quartz-porphyry, which are to be regarded as different lava flows, show conclusively that volcanic forces were very active in northwestern Iowa in ancient times. The position of the lava beds seems to indicate, as will be pointed out by Mr. Beyer in his discussion of the subject in the forthcoming Annual Report of the Iowa Geological Survey, that the flow of the molten rocks probably took place toward the close of the Carboniferous age, immediately after the coal of the Mississippi basin had been deposited.

Mr. Beyer puts forward, therefore, two explanations: —

1. That the flow took place during palæozoic time, perhaps in the Carboniferous, the lava being secularly poured out over an old sea-bottom.

2. That, as a whole, the different flows were contemporaneous and in point of time post-Carboniferous. In this case the intercalations are to be regarded as the results of the subterranean lava flows — the lava following along lines of least resistance and flowing between the strata.

It makes little difference which of these two views is accepted, for certain it is that here in northwestern Iowa there is every reason for believing that there were at one time active volcanic agencies at work not unlike those seen to-day in southern Europe, around the shores of the Mediterranean Sea.

THE PERMIAN IN PRINCE EDWARD ISLAND.

BY F. BAIN, NORTH RIVER, P. E. ISLAND.

THE study of the Permian in North America hitherto has not been satisfactory. The areas studied west of the Mississippi and in Virginia exhibit the lower part of the formation which in organic remains so closely resembles the Upper Carboniferous that a clear and satisfactory periodic distinction is not observable. In the Gulf of St. Lawrence, however, where a long-continued and regular subsidence marked the close of the palæozoic, we have a perfect series of the Permian strata, three thousand feet in depth, recording the gradations of life in this district between the close of the Carboniferous proper and the beginning of the Mesozoic.

The Island of Prince Edward, in the southern part of the Gulf of St. Lawrence, is composed of red sandstones and shales, mostly Permian, capped in the central district by a denuded fragment of the Trias. Where these Permian beds stretch across the Northumberland Strait and appear on the coast of New Brunswick, they are seen to repose unconformably on the Carboniferous. Here the distinction between the two formations is very apparent. The Carboniferous is a coarse, gray marsh deposit, bearing numerous remains of *Calamites* and *Cordaites* and a few *Lepidodendra*. The Permian consists of fine, red marine deposits, bearing as their characteristic organisms *Walchia*, *Tylodendra*, *Baiera*, *Pecopteris arborascens*, and *Calamites gigas*.

In the lower part of the Permian the flora has marked Carboniferous affinities, but there is always a clear and distinct difference. On St. Peter's Island, for example, there is a marsh deposit of the Permian. The gray and brown sandstones and gray bleached clays contain but few calamites, and these of small size, except the giant *C. gigas*. *Cordaites* is also inconspicuous, but remains of *Tylodendra* and *Walchia* are in great profusion, and *Annularia* frequent. At Gallas Point there is the same abundance of *Tylodendra* and *Walchia* with *Dadoxylon* and *Pecopteris*, and here, as in the other localities, *C. arenasceus* begins to take precedence of the older Carboniferous calamites. At Mimimigash is an extensive fern deposit in red clay shale. *Pecopteris arborascens* is abundant and is in magnificent development. Its great, heavy fronds are seen nine feet in length, and its features rich and well developed. *Alethopteris nervosa* is common, but *Sphenopteris*, *Neuropteris*, and *Cyclopteris* are sparsely represented. *Annularia* is abundant. *Cordaites* and *Calamites* hold a minor place, but *Equisitum rogersii* is in magnificent development, and branchlets of *Walchia* everywhere in abundance.

In the Upper Permian about Charlottetown, the Carboniferous features of the formation are almost lost. *Dadoxylon, Tylodendron, Walchia, Palissya* and *Baeria* mingle with *Voltzia, Pterophyllum, Podozamities, Clathropteris*, ferns of Mesozoic type, and abundant *Equisetacœ*.

At Carleton a bressiated conglomerate contains many osseous fragments of considerable size, which in structure have a reptillian aspect.

This series of deposits appears to have closed in an important glacial period, for on its summit rests not only drift fragments, which must have come from the distant hills of New Brunswick, but a well-marked glacial moraine, now consolidated into a firm mass of conglomerate five hundred yards in length, occurs in the valley of the Mill River, reposing on the summit of the Permian and underlying the horizontal Trias.

The Trias contains no good deposits of plants, but such remains as we find are quite distinct from those of the underlying formation. Even the ubiquitous *Walchia gracilis* has disappeared and a new form taken its place. *Voltzia, Palissya, Baiera* mingle with a few inferior *Cycads*, and the accumulations of the ancient sand reefs are everywhere penetrated by the repent stems of *Equisetœ* and their peculiar bulbous nodes.

This meagre flora is but the representative of Mesozoic plant life when the district was recovering from the desolation of a great glacial period. Later deposits are entirely wanting, but the chance occurrence of a high-typed Mesozoic reptile, the *Bathygnathus borealis* (Leidy), in these early beds, clearly establishes their systemic standing. The whole of this series of deposits is exceedingly interesting as illustrating the transition of plant life from the Carboniferous to the Triassic.

NOTES ON THE WING-COLOR OF NORTH AMERICAN LOCUSTS BELONGING TO THE SUB-FAMILY ŒDIPODINÆ AND ITS SEEMING RELATION TO CLIMATIC CONDITIONS.

BY LAWRENCE BRUNER, STATE UNIVERSITY, LINCOLN, NEB.

ONE of the many features that have been noted in the study of our North American locusts during the past ten or a dozen years is the color-variation of the wings of the different species of locusts of the sub-family Œdipodinæ. As all students know who have had anything to do with these insects, some have yellow, others orange, still others red, and a very few have their wings blue. While this is true, perhaps it has not been generally noted that the presence or absence of humidity seems to have some influence upon these color-variations in the different representatives of this group that are to be met with throughout the country. That such must be the case, I think there can be no doubt. But little investigation is necessary to show that along the Atlantic slopes and even in the interior of the continent as far westward as the eastern edge of the great plains, red or orange is the characteristic color. On the plains and in other arid districts of the west and southwest the red and orange give place almost entirely to yellow. In the mountains red re appears, while at a certain elevation and under peculiar conditions blue takes the place of both. In some species we find both red- and yellow-winged individuals. There are also those in which yellow- and blue-winged individuals occur. Nor are these wing-color variations confined strictly to special genera. We find both the red and yellow appearing in species of Arphia, Hippiscus, Derotmema, Trachyrhachis, Psinidia, Lactista, Tomonotus, Dissosteira, etc.; while the blue and yellow are common to representatives of Leprus and Trimerotropis.

We find the red-winged species most common in humid regions, the yellow-winged in more or less arid regions. In the United States the blue-winged forms are found chiefly in mountainous regions just between the dry and wet conditions. At Pueblo, Colorado, *Leprus wheeleri* occurs with either blue or yellow wings. Near Ogden and Salt Lake City are found both this species and *Trimerotropis cyaneipennis*. They occur most abundantly a little below the upper shore-line of the ancient Lake Bonneville, and from that point up and down the mountain slopes for several hundreds of feet. Below there are to be found yellow-winged species of Trimerotropis, above red-winged Arphias. Blue-winged locusts are also to be met with on the lava beds of the Snake River Plains, on the alkali flats of portions of Montana, Wyoming, Nevada, and California, and in the Coast Range of mountains in southern and Lower California.

This same variation in wing-color among the representatives of the sub-family was also observed in Mexico, where the writer had an opportunity of visiting a number of different regions from which specimens were secured. The dry interior contained most yellow-winged and the humid "tierra calientes" furnished most red-winged species; while the midway mountain regions were the characteristic home for a blue-winged locust.

The following species are found with both red and yellow wings, viz.; *Hippiscus tuberculatus*, the prevailing color red, but in the Big Horn Mountains of Wyoming yellow-winged specimens are not uncommon. *Hippiscus*, here in Nebraska, seems to furnish about an equal number of specimens of each color. A couple of others of the genus are know to have the same wing-variations. *Psinidia sucerata* in the East is normally red-winged, but in the West is yellow-winged. Two of our Arphias, at least, have either red or yellow wings, while *Trachyrhachis pardalina* may be either the one or the other — the red being most common eastward and the yellow-winged westward upon the plains, and red again in the Sierra Nevadas.

So characteristic does this variation in color of the hind wings of these insects appear, that I have about come to the conclusion that an examination of a fair representative collection of these insects would be a sufficient index of the climate of the region from where they came. Possibly I may be wrong. If so, I would be pleased to hear the views of others who have made this feature more of a study than I have.

CURRENT NOTES ON ANTHROPOLOGY.—XXIV.

[*Edited by D. G. Brinton, M.D., LL.D.*]

The Problem of Life.

"LE Probléme de la Vie" — such is the title of the latest work of that thoughtful and learned writer, the Marquis de Nadaillac. The great and serious theme he has chosen is handled with a masterful acquaintance with facts and a severely critical spirit. The sweep of his horizon is most extended. He begins with a statement of the possible methods of formation of the terrestrial globe, the first appearance of organic life upon it, and the succession of animal and vegetable organisms which have one after another occupied its surface, down to the beginning of the quaternary period. These questions fill about one-half of the three hundred pages of the volume. The remainder is an anthropologic study. The antiquity of man, the growth of his physical powers and intellectual faculties, and his identity throughout all ages, are the points which receive especial consideration.

The results of this long and patient research are unfortunately negative. "We must resign ourselves to the avowal that science can teach us nothing either about the first appearance of organized beings on the earth, or about their succession in time, or their rapid multiplication in space" (p. 176). "I look as vainly down the vista of the unmeasured past as I do in the present for any positive evidence of the evolution of organisms or the transformation of species" (p. 178). "As far as we wander, as widely as we search, everywhere the individuals of each species reveal the same uniformity of action, the same psychical fixity." Man alone shows the power of indefinite progress. "Before such facts, who will pretend that man and beast ever sprang from one common ancestry?"

Such is the author's conclusion.

The Early Iron Age in Central Europe.

With the general employment of iron, a new era arose in central Europe, one which gave birth to that high culture which has since focussed there the civilization of the world. An intense interest, therefore, surrounds this remote period. History is silent about it, and archæology alone can guide us. This wondrous science reveals two diverse civilizations in that area during the early iron age, separated probably rather by a few hundred years of time than by a few hundred miles of space.

The first is represented by the remarkable cemetery of Hallstatt, near Salzburg. This locality discloses a people skilled in working bronze, gold, and iron, manufacturers of richly decorated and gracefully formed pottery, lovers of ornaments of amber, glass, and agate, and accustomed to cremate their dead. We may place them 500–800 B.C.

The late iron age is the La Téne period, one or two centuries before the Christian era, deriving its name from a station in western Switzerland. By that time the working of iron had reached a singular perfection; glass, gold, silver, and precious stones were frequent; the dead were buried in stone coffins, and a local coinage was for the first time issued in metallic pieces, now popularly known by the name "rainbow keys."

Recent studies on this period are those of Dr. Jakob Heierli of Zurich, in the December number of the Proceedings of the Vienna Anthropological Society, who describes a La Téne station in eastern Switzerland; one by Dr. L. Niederle, in the Report of the International Congress of Pre-History at Moscow, discussing the age of iron in Bohemia; and an address by Von Troltsch before the German Anthropological Society with reference to it in southern Germany.

Enigmatical Stone Implements.

In *Science*, Jan. 6, Mr. Walter Hough describes a form of polished stone implement with grooved surfaces, and suggests that these utensils were employed in beating out fibrous bark for clothing, paper, etc. This suggestion is not improbable, and has been accepted by some curators. In the Trocadero Museum, Paris, these stones are labeled "Armatures de maillet à battre les fibres d'agave." In the University Museum, Philadelphia, one bears the label, "Pounder said to have been used in pounding the agave in making pulque." There is no doubt of the correctness of this identification. The Mexicans called these implements *amatequini*, paper beaters, from the verb *amauitequi*. Mr. Hough is also right in surmising that the Mexican paper was not made from the agave alone. Other materials were the bark of the "Cardia," a tree of the family Boraginaceæ, and palm leaves, *hojas de palma*, which Boturini says made the finest of all. An article on the *amatequini* may be found in *La Nature*, Dec. 15, 1888.

Another strange implement or ornament is the stone yokes or collars which are found in eastern Mexico. In the *Internat. Archiv für Ethnographie*, 1892, Dr. Ernst of Caracas has an interesting article on these. He believes them to be memorial tokens of great individual achievements and worn as signs of power and dignity, on certain ceremonial occasions. Mr. Strebel, who wrote an article some years ago on the same subject, entertained a similar opinion. As they are quite heavy, often weighing about sixty pounds, some have supposed they were intended to fasten the victim to the sacrificial stone, the *techcatl*. They are evidently not adapted for this, however. I would suggest that they were the stones used in the game of ball, *tlachtli*, described by the early writers, enclosing the aperture through which the ball was to be driven. Some are closed with an armature, one of which is figured by Dr. Ernst. They are to be distinguished from the stone yokes from Porto Rico.

Recent Researches in South American Ethnology.

South America offers as large an unexplored region as Africa, and one with as promising possibilities. Strange that it has not attracted more attention from adventurous travellers! One of these, M. Henri Coudreau, has accomplished three expeditions, at the instance of the French government, into the far interior of Guiana. His general results have appeared in various works, as "La France Equinoxiale," "Chez Nos Indiens," etc. Lately, his linguistic collections have been edited by the competent hand of M. Lucien Adam, in a volume forming Tome XV. of the *Bibliothéque Linguistique Américaine*, published by Maisonneuve, Paris. It contains ample and carefully prepared vocabularies of the Ouayana, Aparai, Oyampi, and Emerillon dialects The first two are shown on abundant evidence to be members of the Carib stock, while the two latter are Tupi dialects.

Ernesto Restrepo Tirado is a young and active archæologist of the Republic of Colombia, equally enthusiastic in field-work and in historical studies; as is well shown in his "Estudios sobre los Aborigenes de Colombia," the first part of which, a volume of 180 pages with a good map, was published in Bogota last year. It begins with an extraordinary list of the tribes who occupied the territory at the time of the conquest, largely drawn from the epic of Juan de Castellanos. That Mr. Restrepo had the courage to read the 110,000 verses which compose this epic is reason enough to entitle him to our profound respect. Of course, a great part of his study refers to the Chibchas, who had the highest culture of any Colombian tribes. They were, however, not the most skilful workers in gold. This honor belonged to the Quimbayas, upon whom he has written a long essay, separately published. As their wealth led to their early and complete destruction by the Spaniards, their ethnic affinity has not yet been determined.

The University of Zurich possesses the rare treasure of five skeletons of members of the Alakuluf tribe of Tierra del Fuego. It seems these wretched islanders were taken to Europe to show in museums, and by some strange fatality all died at Zurich of pneumonia. Dr. Rudolph Martin has worked up their osteology and published his results in the *Vierteljahrsschrift der Natur. Gesell.* in Zurich. He finds the skulls well shaped, mesocephalic, with relatively large cubical capacity, 1590 cubic centimetres, and the horizontal circumference greater than that of the modern Parisians, as reported by Broca. The torsion of the humerus was less than in Europeans, and two of the humeri showed perforation of the fossa of the olecranon. The study is an exact and an interesting one.

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.

A Reply to Professor Hathaway.

I HAVE just read the note of "praise and criticism" on my books by Professor Hathaway in *Science* of Feb. 17. Kindly allow me a few words in the way of reply. Passing over the first part of his note, and thanking him for any praise of my books which he has given them, I come to what he calls his "illustration of my treatment and use of the method of infinitesimals." He says: "Thus, by trigonometry,

$$\sin(x+dx) + \cos(x+dx) = \sin x \sqrt{2} \cos\left(\frac{\pi}{4} + dx\right)$$
$$+ \cos x \cos dx + \cos x \sin dx$$
$$= \sin x + \cos x + \cos x\, dx,$$

since $\sqrt{2} \cos\left(\frac{\pi}{4} + dx\right) = 1$, $\cos dx = 1$, $\sin dx = dx$,

Hence $d(\sin x + \cos x) = \cos x\, dx$, a false result."

Of course, it is a "false result"; who would expect anything else when the work in it is false? But this is Professor Hathaway's work; not mine. His statement, made above, that "$\sqrt{2} \cos\left(\frac{\pi}{4} + dx\right) = 1$," is *not true*. For,

$\sqrt{2} \cos\left(\frac{\pi}{4} + dx\right) = 1 - dx$, as any mathematician can see.

Therefore, $d(\sin x + \cos x) = \cos x\, dx - \sin x\, dx$, a *true result*.

Professor Hathaway has given the above illustration, as he says, to show how I "establish the differentials of the trigonometric functions"; though I should have never known it if he hadn't told me; and I deny that I should ever have taken this roundabout way. I hope that Professor Hathaway will not give the credit of his "false result" to the infinitesimal method, which he says "is at best a dangerous one, even in the hands of the masters, let alone the average student." I think, on the contrary, that the method is a safe one, when well understood. "In the hands of the average student" any method is dangerous. In view of Professor Hathaway's illustration, I do not feel called upon yet to "revise my eulogy on infinitesimals." E. A. BOWSER.

Rutgers College, New Brunswick, N. J., Mar. 2,

A Question of Evidence.

IN a recent number of *Science* I ventured to express the hope that a new era was dawning in American archæologic science, and that the department of geologic archæology especially would experience a needed renaissance. I laid particular stress upon the deceptive and meagre nature of the evidence already on record and ventured to point out the demands of the future with respect to certain lines of research. Some of my statements relating to the character of the evidence have given rise to sharp comment on the part of defenders of the paleolithic theory. I strongly deprecate personalities in scientific discussion and hesitate to refer in a critical way to the legitimate work of other investigators, desiring to restrict myself to such criticism as is absolutely necessary for sifting the evidence and getting at the truth; but the generalized statements by means of which I attempted to describe the old archæology are not sufficiently trenchant to be effective; more definite and detailed characterization must, it seems, be given. This can best be accomplished by means of illustrations drawn from the writings of those defenders of the faith who make most vociferous claim to superiority of knowledge and profundity of research. Numerous illustrations are at hand, but I will refer only to the work of those who have unfairly reviewed or offensively referred to the positions taken by me. Attention has been called in Professor Wright's work, "The Ice Age," to a number of papers bearing on the paleolithic question, written by Mr. H. W. Haynes of Boston. In these papers, twelve in number, I have carefully sought references to original observations on the glacial archæology of this country, and find to my surprise that they are limited to two lines and a quarter of text. These lines include, also, reference to the discoveries of Professor Wright, Dr. Abbott, and two others present on the occasion. The record reads as follows: "Several *implements* were taken by the others, *either from the gravel, or the talus* on the river bank, in my presence, and *I found five myself.*"[1] The italics are my own, and call attention to essential features of the finds and to the fact that Mr. Haynes's investigations are expressed in five words — quite sufficient no doubt for the presentation of the matter, since the articles found were probably all modern pieces from the talus. Now, any one could find these objects in the talus at that day, and no one now attaches any value to such finds save three or four advocates of the paleolithic theory in America who hesitate to acknowledge, or fail to see the shortcomings, of their early work. The chances are a hundred to one that all talus finds and all the finds made by Mr. Haynes are Indian shop-rejects left by native workmen who utilized the argillite bowlders and masses that outcropped in the face of the bluff. But whether they were from the talus or not, I would call attention to the fact that the language used by Mr. Haynes in describing the discoveries indicates practical "ignorance" of the only essential points of the discussion of fossil man. In the first place had he known that the things he picked up "either from the gravel or the talus," as he states it, correspond exactly with the ordinary modern quarry and shop-rejects of the Trenton region, he would certainly not have ventured to class them with European paleolithic implements and to build a monument to American antiquity and to himself upon them; and, in the second place, had he known that the only legitimate proof of the antiquity of such specimens in America is geologic proof, he would not have failed to properly discriminate between those articles obtained from the gravels in place — if there were such — and those obtained from the talus. From his language it is evident that at that time he had no comprehension of the real problems involved, and could not have appreciated the necessity of the discriminating observation now considered essential by scientific men; consequently, his observations made in archæologic obscurity and geologic darkness amount to naught, and no subsequent patching-up can redeem them.

[1] Haynes, H. W. Proc. Boston Soc. Nat. Hist. Vol. XXI., p. 132.

Professor Wright, who is vigorously championed by Mr. Haynes, does not claim to have found any relic of art in the gravels, and hence probably knows nothing, from his own observation, favoring the glacial age of man in America, and I was led, in a review of portions of his published work, to question his judgment in writing so much on the finds of others, and accepting all statements that came to hand without apparent attempt at discrimination. Mr. Haynes has been more successful in his finds, having added five unverified turtlebacks to the long list of "paleolithic" strays. He may not have broken Professor Wright's record in number of papers published, but he has been less discriminating in the use of unsound data. Having little knowledge of native art and less of geology, he has rarely touched the subject of glacial man without adding to its obscurity. His most pronounced shortcoming is, however, in the line of original research; when the three lines recording his complete achievements in the American field are cut down to five words, as quoted above, and these words reduced to their *real bearing* upon the question of glacial man in America, we have only the punctuation left! It would be difficult to find within the whole range of scientific writing three lines containing less of science or evincing a greater degree of incompetence to treat of the subject discussed, than these.

Another example of "that half wisdom half experience gives" may be cited. In a recent publication, Mr. Haynes avers that I have rashly and wrongly characterized the work of other investigators; yet a hurried glance into his part of that work convinces me not only that I shall be acquitted of this charge, but that I may now safely venture farther. I am constrained, therefore, to suggest that perhaps Mr. Haynes's investigations of paleolithic man in Egypt — in the only field in which he can possibly lay claim to having added a single new fact of importance to the data of archæologic science — will not require more than five words for their proper record. A brief summary of these researches may be given.

Scattered over the surface of the ground in the valley of the Nile he found several implements of supposed St. Acheul type and numerous examples of other flaked objects of ordinary and extraordinary shapes. We learn, however, in his own words, that "Quaternary deposits do not occur in the Nile valley, so far as I am aware, though they have been found in various parts of the Sahara."[2]

The "implements" of St. Acheul type are assumed to be paleolithic because of their looks. This is the "evidence" of the ordinary paleolith hunter, and it does not appear of the least consequence to him that the quaternary deposits which alone could furnish the only real element of proof of antiquity — the geologic element — are not found in the Nile valley at all, but are said to exist somewhere in Sahara. These enormous leaps from meagre data to full-blown conclusions are characteristic of the past archæology, and awaken feelings of amazement in the minds of practical students to-day. Even if analogies of form in implements are allowed to have a definite value in cultural or chronologic correlations in Europe and adjoining lands, it must be insisted that in America, until types of flaked objects other than those found commonly in Indian shop-refuse heaps are established, the test of antiquity shall be a geologic test.

The two illustrations given serve to indicate my reasons for raising the question of competency with respect to the evidence relied upon to establish a paleolithic glacial man in America. Observations of the class cited, howsoever greatly multiplied, can never amount to proof, demonstrating rather the lack of it. My position with respect to this point need not be misunderstood: when a single artificial object is found that can be fully and satisfactorily verified geologically, I shall gladly join hands with other students in making it a nucleus about which to arrange all that are clearly fellows with it. Then, and not till then, will uncertainty become certainty, and not till then can the question of the grade of glacial art be taken up and profitably studied. I only ask that the evidence relating to glacial man be properly scrutinized, and that meanwhile paleolithic man in America shall bide his time.

[2] Haynes, H. W. "The Fossil Man," Popular Science Monthly, Vol. XVII., p. 358.

While awaiting the discovery of new evidence tending to establish a glacial man in America, I have undertaken to analyze the old testimony as embodied in the writings of investigators of the American questions, and short papers covering part of this ground will soon appear. I had not anticipated this present diversion, however, as I had thought of Mr. Haynes only as a convenient verifier of that large class of unfortunate "paleoliths" whose pedigree happens to be shaky. My work was intended to bear only upon that of real investigators, such as Abbott and Cresson and Metz, who have for years sought earnestly, if not always effectively, for the evidence that is to make symmetric the culture development of two hemispheres. Those writers who undertake to use, and defend the evidence collected by, these students, will do well to remember that they shine by borrowed light, and should for much-vaunted modesty's sake, if not for science sake, keep well within reach of its limited ray.

If my "rash" assertions, hitherto made, respecting the nature of the testimony relied upon to establish a glacial, paleolithic man in America, lead finally to a just estimate of the real evidence and to the establishment of a firm basis for future operations in this great field, I shall feel amply repaid, notwithstanding the storms of sharp words and the streamlets of doggerel the publication of these views seems destined to call forth.

W. H. HOLMES.

Washington. D.C.

The Neanderthal Skull.

IN reference to Professor Haynes's observation in *Science*, Feb. 24, p. 107, that, not having seen the report of Professor Virchow's address, he could not judge "how far Dr. Brinton may have been misled by his authorities," I beg permission to furnish both him and other readers of *Science* the opportunity of judging, by quoting Virchow's precise words about the place and surroundings of the Neanderthal skull. They are as follows: —

"Für die Beurtheilung dieser Gebeine ist es von Wichtigkeit zu erwähnen dass dieselben aus keiner Höhle herstammen; auch hat man sie nicht an ihrer Lagerstätte aufgefunden, niemand hat sie ausgegraben, sie sind in Bezug auf die geologischen Verhältnisse, unter denen sie sich befanden, nicht Gegenstand der Beobachtung gewesen. Sie wurden gefunden in einer Schlucht, die zunächst eines Bergabhanges sich gebildet hatte; durch diese Schlucht waren Wasser herabgekommen und hatten allerlei herausgespült; wo die einzelnen Stücke früher gelegen hatten, wusste niemand. Darunter befanden sich auch das Bruchstück des Schädels."

Professor Haynes refers to the finder, "Dr. Fuhlrott" (evidently meaning Fullroth). This person's statements are seriously questioned by Professor Virchow, apparently from information derived from Mrs. Fullroth, who imparted it in unsuspecting innocence of the grave decisions involved; as the Professor gleefully narrates. Virchow's earlier report will be found in the Verhand. der Berliner Anthrop. Gesell. for 1872.

D. G. BRINTON.

Philadelphia, March 1.

Aerial Bubbles.

THE account of "snow-rollers" in your recent issue recalls an atmospheric phenomenon which was beheld here by two witnesses of unimpeachable character several years ago, of which no account has ever been published. Towards sunset, late in April, 1886, on a warm, thawing day, the snow rapidly disappearing, two men, Capt. John E. Hetherington and Mr. Marcus Sternberg, as they rode down the long hill towards this village from the east, saw what appeared to be innumerable spherical bodies floating in the air like soap-bubbles. Both men saw and wondered at the appearance for some moments before either spoke. Capt. H. then said, "I wonder whether I am dreaming?" The other rubbed his eyes and echoed the sentiment. "Well," said the captain, "I wonder if you see what I see; what do you see?" They questioned each other, and both agreed as to their impressions. An orchard lay along the lower and northwesterly side of the road, and all in among the apple-trees were thick, gently-descending multitudes of these bubbles, pretty uniform in size, say, 8 or 9 inches in diameter, apparently; none less than six; no small ones being observed.

The two observers state that they carefully fixed their attention on particular bubbles, in order to compare notes, and saw them seem to rest on the bough of a tree, or the top board of the fence, and then gently roll off and disappear or go out of sight. The sun was sinking and dropped below the opposite hills as they reached the foot of the long descent and entered the village, and the appearance came to an end. But up to this time the air seemed to be filled with these transparent floating spheres. The position of the observers with regard to the light seems to have made some difference as to seeing well this or that large aggregation or swarm that one or the other pointed out. The bubbles were highly colored, iridescent, gave the same sort of reflections as soap-bubbles, and apparently vanished individually in much the same way. All these points I have ascertained by repeated conversations.

Captain Hetherington (Lieuteant Colonel by merit) is widely known for his extensive apiaries, the largest in the country, and is an exceptionally good observer. Mr. Sternberg also is a gentleman of intelligence and careful observant character.

The only theory I have been able to form to account for such a phenomenon is, that if a certain kind of dust floated off in the air, each particle composed of some sort of saponaceous envelope, enclosing a highly expansible centre or core, like ammonia,— particles of this character expanded by the warm air, and at the same time moistened, might, under very nice conditions, produce such an effect.

I will add, *apropos* of snow-rollers, that Mr. Sternberg states that, years ago, he once saw, in Schoharie County, what he called "auger borings" of snow; which he described as spiral rolls, about two inches in diameter, and broken into fragments of various sizes, like the borings turned out by an auger.

HENRY U. SWINNERTON, Ph.D.

The Parsonage, Cherry Valley, N.Y.

Hardy Towhee Buntings.

HAVING noticed the effect of the recent severe weather on the crows near Washington, which Dr. Ridgway gives an account of in *Science* of Feb. 10, I was greatly surprised to find the towhee bunting (*P. erythropthalmus*) evidently wintering here. During the second week in January last, I observed two individuals and heard the notes of others. As the towhee seems to get most of its food upon the ground, its presence during deep snows and severe cold rather surprised me. The authors of the U. S. National Museum Bulletin, No. 26 (Avi Fauna Columbiana), say of the towhee: "Chiefly a spring and autumn migrant. A few breed with us, but none remain during the winter." It usually makes its appearance here in the first warm weather in March, and I have found it to breed quite abundantly in suitable localities. During the same cold snap I picked up numbers of dead goldfinches, juncos, and native sparrows, evidently victims of the weather. The turkey vultures (*C. aura*) also suffer from the cold and are sometimes found unable to fly, their plumage being coated with snow and ice. In order to prevent the extermination of the bob-white during the past winter, a Virginia sportsman's club furnished quantities of wheat-screenings to any persons who would place the same in localities frequented by the birds.

ALBERT B. FARNHAM.

Bennings, D.C.

The Speech of Children.

THE paper in *Science* of March 3, having the above title, by Mr. A. Stevenson, has much interested me. In the fifth paragraph, on page 120, the author says: "The child apparently regarded himself only as object and not at all as subject." This conclusion is reached by the child's use of the third person in speaking of himself. It seems to me inconceivable that a conscious being should regard himself other than as subject. The peculiarity of expression — a common enough one in children — I believe to exist, first, because the child hears himself constantly referred to as the object, and, second, because of the wrong and foolish method of conversation employed — not necessarily by the child's parents — when talking to him. Such examples as "Baby kiss mamma," "Does Freddie love his auntie?" "Is little Mary cold?" etc., can hardly lead to an early conception of correct verbal expression. HOWARD LILIENTHAL, M.D.

New York, 43 East 29th Street, March 6.

Solidungulate Pigs.

THE "mule-footed hogs" inquired about by Mr. Jno. H. Frick, in *Science* of Feb. 24, p. 107, are described and figured in my article entitled "On a Breed of Solid-Hoofed Pigs Apparently Established in Texas," Bull. U. S. Geol. and Geogr. Surv. Terr., Vol. IV., No. 1, Feb. 5, 1878, p. 295. ELLIOTT COUES.

Smithsonian Institution, Washington, D. C., March 1.

BOOK-REVIEWS.

Original Papers on Dynamo Machinery and Allied Subjects. By JOHN HOPKINSON, M.A., D.Sc., F.R.S. New York, W. J. Johnston Company.

THIS volume is a collection of the papers on electro-technical subjects which Dr. Hopkinson has published at various times during the last fourteen years.

It will be unnecessary to speak of the great value of these papers, for a number of them have passed into the text-books and form a part of the education of every technical student, and there is probably not an electrician in the country who has not found himself obliged to obtain the greater part of the remainder in some form or other. But a book of clippings from engineering journals is never so satisfactory as a bound volume, and the electrical profession will accord a warm welcome to this little book, the more so as it contains several papers which have hitherto been difficult to obtain. Of the eleven papers here collected, five are on electric lighting and dynamo-electric machinery, two on transformers and transformer tests, two on theory of alternating currents, one on an electrostatic effect in conductors carrying alternating currents, and one on electric light-houses. The first five contain the "epoch making" work on characteristic curves, and on efficiency tests of dynamos. (In passing, it may be noted that the paragraph on page 36, on the use of the characteristic to find the lowest speed at which a machine can be run and yet produce an arc, is given wrongly in Professor S. P. Thompson's "Dynamo-Electric Machinery," page 273.) But to technical readers the most interesting portion will be the papers on alternate currents and transformers, included in which is an account of the recent tests on the Westinghouse transformer, of importance as showing that the old accusation of poor all-day efficiency can no longer be made against the commercial transformer. These treat of the parallel and series running of alternators, the design of transformers, the effect of capacity in transformers, the power consumed in alternating current arcs, etc.

The advantage that this book has over the papers as originally printed is the fact that most of the errors and misprints have been corrected. A few yet remain, however. On page 155, 2μ should read 2π; $sine\ 2\pi / T(t + \tau)$ should read $sin\ 2\pi / T(t - \tau)$; the sign of the solution of the differential equation for H should be $-$ instead of $+$. On page 157, $e\gamma$ should read 2γ; through the whole of this part of the book H' is printed instead of $\dot{H}$. This would be objectionable if intentional, but it seems to be an accident, as on page 179 the dot is used instead of the stroke, but placed wrongly.

Electricity and Magnetism: Being a Series of Advanced Primers of Electricity. By EDWIN J. HOUSTON, A.M., Professor of Natural Philosophy and Physical Geography in the Central High School of Philadelphia. New York, W. J. Johnston Co.

FROM the preface we learn that this book is meant for the "general public" and the increased "number of those to whom a knowledge of the laws of electricity has become a necessity of every-day business life." While it is proverbially hard for a specialist to decide what the public want, it may be doubted if they will see much to choose between this and the scores of similar books which have been published. It is possible, however, that the name on the title-page may prove an attraction to many. On inspection the book is found to treat of the simpler theoretical principles. technical subjects, such as the dynamo, arc-lamp, etc., taking up about fifty lines out of the three hundred pages which comprise the book.

As in most books of the class, there are numerous inaccuracies; to mention a few: on page 23 a black surface is stated to be a worse radiator of light than a white one; whereas, of course, the reverse is the case; carbon is given as an exception to the rule that the conducting power of metals decreases with rise of temperature; the "conducting power of all alloys or mixtures of different metals" is stated to be "very much less than that of any one of the metals of which they are composed," in forgetfulness apparently of the fact that Matthiessen gives a long list of alloys whose conductivity is the mean of their constituents, etc.

The idea of giving references and extracts from books which should be read by those desiring a fuller knowledge of electricity than can be gained from the primers, can be considered a good one. It may, however, be questioned if the quotation from Professor Ayrton's book, "Practical Electricity," would give a reader the impression that it is a book on electrical laboratory work, and whether there is any necessity of quoting the author's "Electrical Dictionary" and "Physical Geography" so often among the selections from standard works, especially where, as on page 161, under "Extracts from Standard Works," the author quotes his dictionary as quoting Fleming, where the extract could, with no loss, have been made directly from the original. The chapter on Electrical Work is one of the best in the book, and the unscientific reader can hardly fail to understand the ideas treated of completely. R. A. F.

Contributions from the Botanical Laboratory of the University of Pennsylvania. Vol. I., No. 1.

Bulletin of the Scientific Laboratories of Denison University, Granville, Ohio. Vol. VII.

IN these days of enormous multiplication of books, magazines, journals, proceedings of societies, etc., there should always be reason for the establishment of a new serial. The avenues of publication are already so numerous that it is almost impossible to keep track of all. The agricultural experiment stations have vastly increased the amount of literature dealing with scientific results, and the comparatively new departure of universities, in issuing periodical publications, is one rather to be deprecated than encouraged. It would seem far better, for example, to do as Columbia College in New York, and Harvard University in Cambridge do, that is, to publish articles in established periodicals or scientific serials, rather than to originate new ones. Columbia College publishes the "Contributions from the Herbarium" in the Transactions of the New York Academy of Science, while Harvard University prints "Contributions from the Chemical Laboratory" in the Proceedings of the American Academy of Arts and Sciences.

These remarks are induced partly by the recent appearance of No. 1 of Vol I. of "Contributions from the Botanical Laboratory of the University of Pennsylvania" and Vol. VII. of the "Bulletin of the Scientific Laboratory of Denison University." Both of these are creditable publications. The former contains some valuable papers upon Dionœa and other subjects, and the latter is a catalogue of the flowering plants and ferns of Licking County, Ohio. With the Philadelphia Academy, the Franklin Institute, and the American Philosophical Society, all issuing serials in Philadelphia, the *raison d'etre* for a new serial there does not appear. The case of the Denison University is not quite parallel, but most probably there would be little difficulty in arranging for the publication of such papers in other places.

In the University of Pennsylvania contributions we have the following papers: "A Monstrous Specimen of *Rudbeckia hirta*," by J. T. Rothrock; "Contributions to the History of *Dionœa muscipula*," by J. M. McFarlane; "An Abnormal Development of the Inflorescence of Dionœa," by John W. Harshberger; "Mangrove Tannin," by H. Trimble; "Observations on *Epigœa re-*

pens," by W. P. Wilson; "A Nascent Variety of *Brunella vulgaris*," by J. T. Rothrock; and "Preliminary Observations on the Movements of the Leaves of *Melilotus alba* L. and Other Plants," by W. P. Wilson. Numerous new points are brought out by the studies of Dr. McFarlane on Dionœa. Among others he notes that two touches of the sensitive hairs are usually necessary to cause closure of the leaf. What he calls "memory power of the protoplasm," that is, response to a second stimulus when the first had no appreciable effect, he finds is sharply retained for from 30 to 45 seconds; and in from 55 to 60 seconds the effect of the first stimulus is lost. He also found that the hairs were not alone sensitive, although they were most so. But both outer and inner leaf surfaces show a marked degree of sensitivity. It was observed, likewise, that, although falling water, like rain, had little or no effect, immersion in water caused closure of the leaves as soon as the water touched the hair. Although three is the normal number of hairs on each blade of the leaf, our author has seen seven on one and six on the other half of a leaf; and he says leaves are frequently observed with from 8 to 13 hairs. "Such facts give countenance to the view that the sensitive hairs were once more numerous and diffuse in distribution, a condition still retained by *Drosera*." The hairs are jointed just above the base, and this seems to be the special irritable centre.

The epidermal cells of the leaves are stated to be admirable objects for observing the continuity of protoplasm. After proper treatment, the method being described, there are seen "along each side 18 to 30 protoplasmic bridges, which are slightly constricted on either side of the cellulose wall, and form a central swelling at the passage through the pore aperture. The transverse or oblique walls are traversed by 5 to 8 similar processes, so that the protoplasm of each epidermal cell is linked to that of neighbor cells by 50 to 75 fine connecting threads, and these again collectively are united with the cylinder of sensitive cells in the irritable hair." Various other subjects are considered, but they cannot be referred to here.

In the Bulletin of the Denison University, above mentioned, we have a catalogue of 945 species of plants occurring in Licking County, Ohio. Mr. H. L. Jones, the author, gives a list of the herbaria consulted, a short sketch of the county geology, and other facts. Among them are the times of flowering of the plants, and we note that in November 44 species bloom, in December 11, in January 14, in February 9, in March 17, and up to July 530. Thus no month of the year is without some flowers.

JOSEPH F. JAMES

Washington, March 1.

An Introductory Manual for Sugar Growers. By FRANCIS WATTS, F.C.S., F.I.C. London and New York, Longmans, Green & Co. 151 p. Ill. 8°.

IN the rapid extension of agricultural chemistry and scientific agriculture, a vast amount of tabular matter has been prepared, thousands of analyses have been made, and yet the results are neither satisfactory nor proportional to the work done. Professor Whitney has recently placed the position very clearly in saying, "There has been no satisfactory interpretation as yet of much of the work which has been done on the chemical composition of soils and plants, and the results of plat experiments have in most cases been very conflicting and uncertain." In this country the government experiment stations are issuing bulletin after bulletin of valuable and interesting reading; but even they, with all their superior advantages, have, as yet, fallen far short of their purpose. One reason for this is in the pre-eminence given to analysis and in the slighting of "condition," which latter feature forms a prominent part in the opening chapters of "An Introductory Manual for Sugar Growers," by Mr. Francis Watts, government chemist at Antigua, W. I. The first half of this interesting little book may be perused with profit by agriculturists the world over, presenting as it does a remarkably clear and intelligible dissertation on the elements of agricultural chemistry, treating first of soils, then of plant life and plant food, and finally of manures and fertilizers. The remainder of the work is devoted exclusively to the sugar industry, beginning with the planting and cutting of

CALENDAR OF SOCIETIES.

Anthropological Society, Washington.

Mar. 7. — George H. Boehmer, Pre-Historic Naval Architecture of Northern Europe; George R. Stetson, Mental Atrophy in the Working Classes.

the cane, and passing on in natural sequence to the mill, the treatment of the juice, the manufacture of the sugar, the testing of the sugar solutions, and finally to the molasses and fermentation.

Throughout, the book is one for the practical man, and much important detail has been embodied within its pages. One of the best chapters is that treating of "Condition or Heart," plant food, and drainage, the first constituting, as Mr. Watts says, "a large portion of the science of agriculture." Analyses are, of course, given, but they are not asked to do duty for the whole, as is often the case in agricultural treatises. It is interesting to note, too, the remarks upon the assimilation of atmospheric nitrogen by the Leguminosæ, in which Mr. Watts follows Berthelot. Schlösing and Laurent have, indeed, recently denied the fixation of nitrogen by the action of microbes beneath the surface of the soil, but their theory of chlorophyll action needs far greater proof than they have offered. Berthelot is a good leader. Chapter III. deals with the sugar cane, treating of the preparation of the land, the manner of manuring and weeding, the cutting of the cane, and the utilization of the trash. Chapter IV. is of general interest, though the facts are applied in a particular manner to sugar growing. The collection, retention, and value of pen manures, the application of green dressing and of chemical manures, including potash, phosphates, superphosphates, sulphate of iron, etc., forms together an interesting study. The fallacy, which is so common, of supposed increase in manurial value of the excreta as compared with the food eaten, is here spoken of, as is also in a previous chapter the practice of burning the trash under the impression that thereby its value as a fertilizer is increased.

In the chapter dealing with the mill and the extraction of the juice, the various types of the former are compared and diagrams given. The application of hydraulic attachment to the rolls is mentioned, and a comparison is made of the results from crushing and those obtained by maceration and diffusion. The succeeding chapters treat of the juice, tempering, clarifying, filtering from the scum and the utilization of the latter, the manufacture of the sugar, in open pans, with strain, and in vacuum, and finally of the testing of the solutions and syrups. The production, composition, and uses of the molasses with the recovery of the sugar therefrom, and finally the nature of ferments and fermentation with the yield of alcohol and the forms of the stills employed, constitutes the subject matter of the concluding chapters. There are in addition tables of the temperature of steam at varying pressure, a list of the elements with their symbols and atomic weights, and a table of the densities, etc., of saccharine solutions.

We should be pleased to see this book in a second edition much enlarged and amplified, and trust that it is but the beginning of a series. C. P.

AMONG THE PUBLISHERS.

SIR ROBERT S. BALL, F.R.S., the well-known English astronomer, has just completed an "Atlas of Astronomy," containing numerous beautifully printed telescopic views of planets, the sun's corona, etc., and diagrams of orbits. There are many star maps, and a series of twelve plates devoted to the moon, showing its aspects on consecutive days from the third to the fourteenth, making seventy-two plates in all. An introduction of nearly sixty pages gives a comprehensive explanatory text. The Atlas is published by D. Appleton & Co.

— Bulletin No. 40 of the United States National Mueum is No. IV. of the Bibliographies of American Naturalists, published by the government. This one is by L. S. Foster, and gives the writings of the ornithologist, Geo. N. Lawrence. A portrait faces the title-page, and in the course of the 124 pages, 121 titles are enumerated. Under these titles are given all facts connected with them. The species given in each are enumerated, together with the locality and page. A very full index gives ready reference to any species mentioned.

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SCIENCE

NEW YORK, MARCH 17, 1893.

WHERE IS THE LITRE?—A MODERN SCIENTIFIC PUZZLE-PICTURE.

BY STEPHEN H. EMMENS, YOUNGWOOD, PA.

IN *Engineering News* of Oct. 20, 1892, appeared an article on Fuel-Gas Values, in which I gave a table entitled "Some Metric Constants," designed to show the variations of value to be found in the text-books even with regard to so fundamental a matter as the volume of the litre. The publication of this table has caused me to receive a letter of protest from my friend, Mr. Latimer Clark, F.R.S., who, as all the world knows, takes rank among the foremost living authorities on the subject of weights and measures; his "Dictionary of Metric and other Useful Measures" being a permanent masterpiece. This letter contains much that is interesting to the scientific world, as will be seen by the following quotations which include all the material passages:—

"I have looked over the varying list of values and it is not very difficult to account for the discrepancies. Many of them have taken the values as defined by Act of Parliament, and as published by the Board of Trade. But all the world has known for years past that this valuation is very far wrong, and therefore the more careful writers have endeavored to correct the error as far as they were able by using the best results they could obtain or hear of. Some of them, however, are not quite so easily explained (S A. Ford, for example).

"For the past thirty years no scientific writer or worker has used the Board of Trade official value of the cubic inch of water, viz., 252.458 grains. This is the simple cause of the discrepancies you point out. You have been a little hard on me in the matter, and your article would certainly lead any one to suppose that I had given three different values for the litre, which is very far from being the case. After the book was all printed ready for issue, the new Board of Trade measurements came out and I rewrote and reprinted a great part of it in order to make it conform to the new legal definition of the Board of Trade. Up to September, 1891, I had always assumed the cube decimetre and litre to be identical. . . . At page 57 I call especial attention to the change, in the footnote, and again in the article 'Water,' at page 90, and I give there a table of the volumes of the litre and cube decimetre. Then, again, at page 103, I give a special note on the capacity of the litre. I beg you to read these with care, for it is evident that you have read hastily and have never put your back into the question. If you had read carefully you would have found abundant warning against confounding the litre with the cube decimetre. They are practically the same, and can be differentiated only by means of the most costly apparatus used by the most skilful physicists and with extraordinary precautions; but then you were writing from a scientific point of view and you ought to have read carefully.

"Then in reference to the 'cube inches into litres,' page 47. You ignore the six places of decimals given in the first column, and pass on to the subsidiary column of reciprocals where only two are given, and by some process you expand them into five places of decimals, some of which are, of course, sure to be wrong. Strangely enough, too, while going to this trouble, you fail to notice that on this line and the one above it ('into cube decimetres') the two figures are given differently, viz., 61.04 and 61.0270. This would certainly have caught your eye if you had been really studying the question, but I fancy you were more intent upon writing a rattling article for the press.

"I hope you will find some opportunity of correcting the impression that my book is not trustworthy, for it is at the present day the *only* book that gives the English measures correctly.

"I note that in the constants you have adopted, you use 28.3127 as giving the number of 'litres in a cube foot.' I do not quite see what you take this from, but in England the number is 28.3110, while the number of cube decimeters is 28.3153.

"In the United States the metre is by law = 39.37 inches, but in England it is 39.37079 inches. From the latest measurements, however, the U. S. number is likely to turn out more accurate than the English number."

In order that this letter may be clearly understood it is desirable to quote the published statements to which it refers. These are as follows:—

1. The reference to Mr. Clark's book in my table appeared thus:—

"Authority.	Cu. inches in 1 litre.	Cu. feet in 100 litres.	Litres in 1 Cu. ft.
Dictionary of Metric Measures, by Latimer Clark, F.R.S.	61.0364	3.5322	28.3110
Ditto	61.04	3.5323	28.3093
Ditto (cube decimetre.)	61.0270	3.5316	28.3153"

2. After directing attention to some current arithmetical inaccuracies on the subject of the heat-value of natural gas, I remarked as follows, in the paper concerning which Mr. Clark has written me:—

"Considerations of space forbid my entering at further length into the correction of published errors. Every careful man who has ever consulted a text-book will grimly admit the justice of this remark; even though he may willingly agree with me in sincerely thanking the Trautwines and Haswells and Gmelins and Clarks and Thomsons and Favres and Regnaults and Berthelots, and all the brilliant compilers who have done so much good and worthy work in aiding the progress of knowledge."

3. The foot-note at p. 57 of Mr. Clark's book is:—

"The litre was designed to be the volume of a cube decimetre of water in vacuo at maximum density, but is actually somewhat greater. It is now understood as the volume of one kilogram of water freed from air, at maximum density and weighed in vacuo. It is, therefore, dependent on the dimensions of the kilogram and not of the metre. The litre used in these tables has the capacity above defined; the equivalent weight of water employed is not the kilogram but the actual weight in air (see 'Water')."

4. The article "Water" at page 90 of Mr. Clark's book is:—

"The weight of the cube inch of water at 62° F., used in the following table and throughout the work, is not the old and well-known cubic inch of 252.458 grains, but the newer determination by the Standards Department of the Board of Trade, viz., 1 cubic inch of distilled water, freed from air, at 62° F., weighed in air against brass weights, barom. 30 inch = 252.28599 grains. This measure has already been legalized. It is distinguished by the date 1890. The old weight of the cubic inch was legalized by Act of Parliament in 1824, and when used it is distinguished by that date.

"The gramme of water is very commonly considered identical with the cubic centimetre, and the kilogram is similarly taken as equivalent to the cubic decimetre or litre, but these relations are only true when they are weighed in vacuo and at maximum density, 4° C. The litre of water (1 kilogram in vacuo at 4° C.) when weighed practically, that is, against brass weights in air, barom., 30 inches, loses 16.5 grains, owing to displacement of air, and then weighs at 4° C. only 998.93 grammes instead of 1,000. The difference is, of course, greater at ordinary temperatures. In addition to this, the kilogram, and therefore the litre, is supposed to be intrinsically heavier than the cubic decimetre of water in vacuo by about 120 milligrams or 1.85 grains, owing to slight errors in the original determination. Dr. O. J. Broch (International Committee of Weights and Measures, Annales de Chimie et Physique, tome X., February, 1887) remarks that the centimetre employed in fixing the dimensions of the original kilogram of water would appear to have been $\frac{1}{25000}$ longer than the present standards. The freedom from air was also a point which was not regarded at that period.

Weight of Distilled Water, Free from Air, Weighed against Brass Weights, Barom. 30 Inch.

Volumes.	In grains		In grammes	
	62° F.	4° C.	62° F.	4° C.
Cube inch of 1824	252 458	252.741	16.3591	16.3773
" " " 1890	252.286	252.568	16.3479	16.3662
" " weighed in vacuo	252.556	252.839	16.3654	16 3837
Cube foot (62.2786 lbs.), 1890	435950.2	436438.2	28249.11	28280.73
Gallon (10 lbs)	70000.	70 78.3	4535.93	4541.0[illegible]
Litre (1 Kilog. in vacuo)	15398.6	15415.8	997.814	998 930
Cube decimetre	15396.3	15413.5	997.662	998.779
Cube centimetre	15.3963	15.4135	.9977	.9[illegible]88

"Water increases in volume from its maximum density at about 4° C. (or 39.2° F.) to that at 16$\frac{2}{3}$° C. in the ratio of 1 to 1.001120 (log. 0.0004863). At the same time its density or specific gravity diminishes in the same ratio, or as 1 to .998881 (log. $\bar{1}$.9995137). These figures are taken from government reports. The true maximum density is said to be at 3.945° C., but 4° C. is the accepted standard."

5. The "Note on the capacity of the litre" at p. 103 of Mr. Clark's book is:—

"The relation between the British and Metric measures of capacity depends on the value which we assign to the litre.

"This value may be obtained as follows. The litre is the volume of one kilogram of water at 4° C. in vacuo. If we suppose the litre of water to be raised in temperature to 62° F., its weight will not change, but its volume will have expanded to 1.00112 litres (Chaney, Proc. Roy. Soc., No. 294, Sept., 1890). If 1.00112 litres at 62° F. weigh 1 kilogram, or 15432.35 grains, then 1 litre will weigh $\frac{15432.35}{1.00112} = 15415.08$ grains.

"If we bring this new litre into the air, and weigh it against brass or bronze weights, it will sustain a further loss of weight, due to the buoyancy of the air. This will amount to 16.491 grains, as described below, and the weight of the litre in air at 62° F., Bar., 30 in., will then be:—

	15415.08 grains
Less loss by weighing in air	16.49
Weight of the litre in London at 62° F.,	15398.6 grains

"The original litre has, therefore, lost 17.25 grains by its expansion in volume, and 16.49 grains by the buoyancy of the air acting on it and the weights which counterbalance it. Having thus ascertained that the litre of water at 62° F. weighs 15398.6 grains, and the cubic inch 252.286 grains, we easily find that the litre contains 61.0364 cubic inches.

"The loss of weight in air is thus calculated. Mr. H. J. Chaney, warden of the standards, who has recently re-determined the weight of the cubic inch of water (Chaney, Proc. Roy. Soc., No. 294, Sept., 1890), finds that one cubic inch of ordinary air, containing an average proportion of moisture and carbonic acid, weighs in London .3077 grains at normal pressure and temperature. 61.0364 cubic inches, therefore, weigh 18.781 grains. The weights, if of bronze, have a specific gravity of 8.4, and if of brass of about 8. Taking a mean density of 8.2 we get $\frac{18.78}{8.2} = 2.29$ grains due to the displacement of air by the brass weights. Deducting the 2.29 grains from 18.78, the displacement due to the water, we get 16.49 grains, the value used above.

"It would not be possible to measure the litre or the cubic decimetre strictly as defined by the French Statutes, for they prescribe that the water shall be weighed at 4° C. in measuring vessels which are to be correct at 0° C. There is a similar anomaly in the definition of the American gallon."

6. The values given at p. 47 of Mr. Clark's book are:—

	Multiply.	Divide.	Log.
Cube inches into cube decimeters,	.016386	61.0270	$\bar{2}$.21448
" " " litres,	.016384	61.04	$\bar{2}$.21441

7. The values given at p. 57 of Mr. Clark's book are:—

	Multiply.	Divide.	Log.
Litres into cube feet,	.035322	28.3110	$\bar{2}$.54804
" " inches	61.0364	———	1.78559
" cube centimetres, or gram., water at 4° C.	1000	———	3.00000

8. The values given at p. 32 of Mr. Clark's book are:—

	Multiply.	Divide.	Log.
Cube feet into cube metres,	.02832	35.3166	$\bar{2}$.45202
" " " decimeters,	28.3153	———	1.45202
" " litres or kilogs. of water 4° C.	28.3110	———	1.45196

9. The values given at p. 24 of Mr. Clark's book are:—

	Multiply.	Divide.	Log.
Cube decimetres into litres,	1	———	0.00000
" " " cube feet,	.03532	28.3153	$\bar{2}$ 54798
" " " " inches,	61.027	———	1.78552

10. The values given at p. 61 of Mr. Clark's book are:—

	Multiply.	Divide.	Log.
Cube metres into cube feet,	35.31658	———	1.54798
" " " " inches,	61027.05	———	4 78552

The foregoing quotations, together with Mr. Clark's letter, form a very excellent puzzle-picture, in which, presumably, the litre is somewhere to be found. Before, however, I adventure upon the search, let me clear away four small clouds that might otherwise befog the expedition.

First, Mr. Clark is mistaken in imagining that I had not read his book carefully and that I "confounded the litre with the cube decimetre." No. 1 of the foregoing quotations shows that in my table I specifically drew attention to the distinction between the two measures in question.

Second, Mr. Clark is mistaken in imagining that, with reference to the values given at p. 47 of his book, I "failed to notice that on this line and the one above it ('into cube decimetres') the two figures are given differently, viz., 61.04 and 61.0270." No. 1 of the foregoing quotations shows that the two figures in question must have "caught my eye"; for I duly included both of them in my table and took care to show that one referred to the litre and the other to the cube decimetre.

Third, Mr. Clark is mistaken in imagining that my "article would certainly lead any one to suppose that (he) had given three different values for the litre." Any careful reader of the table (vide quotation No. 1) would see that I cite Mr. Clark as having given two different values for the litre and a third value for the cube decimetre, which is, in very deed, the case.

Fourth, quotation No. 2 shows that I took some pains to preclude any impression that Mr. Clark's book is not trustworthy.

Coming now to the main question, let us commence our investigation by summarizing the statements in Mr. Clark's book and letter as to the various measures that all come under the common denomination of "litre." They are as follows:—

A.—"LITRES" PROPER.

1. "Litre = 1 cube decimeter, or $\frac{1}{1000}$ cube metre, very nearly. The volume of 1 kilogram water at 4° C. . . . It is now understood as the volume of 1 kilogram of water, freed from air, at maximum density, and weighed in vacuo" (p. 57). The accepted temperature of maximum density is 4° C. (p. 91). The weight of 1 Kilog. of distilled water, free from air, in vacuo at 4° C., is 15432.35 grains (p. 103); and the weight of 1 cubic inch (of 1890) of water under the same condition is 252.839 grains (p. 91). Hence the volume of the standard litre is $\frac{15432.35}{252.839} = 61.036272$ cubic inches.

2. If a standard litre of distilled water, free from air, be weighed in London against brass weights in air at 62° F., barom. 30 in., the result will be 15398.6 grains (p. 103); and the weight of 1 cubic inch (of 1890) of water under the same conditions is 252.286 grains. Hence the volume of the "London" litre (of 1890) is $\frac{15398.6}{252.286} = 61.036284$ cubic inches.

3. The volume of the "London" litre (of 1824) is $\frac{15398.6}{252.458} =$ 60.9947 cubic inches.

4. The value of $\frac{15398.6}{252.286}$ adopted by Mr. Clark is 61.0364.

5. The weight of 1 kilogram of distilled water, free from air, in vacuo, at 62° F., is $\frac{15432.35}{1.00112} = 15415.0851$ grains, and the loss by weighing in air is 16.491 grains. The weight of the litre in London at 62° F., barom. 30 in., is thus 15398.594 grains; and this, divided by 252.286, gives 61.03626 cubic inches as the volume of the London litre (of 1890).

6. On the basis of 61.03626 cubic inches per litre, the number of litres in 1 cubic foot is $\frac{1728}{61.03626} = 28.31104$.

7. On the basis of 61.0364 cubic inches per litre, the number of litres in 1 cubic foot is 28.310975.

8. The value of $\frac{1728}{\text{Cu. in. per litre}}$ adopted by Mr. Clark is 28.3110.

B.—CUBE DECIMETRES.

1. The English metre is 39.37079 inches. Hence the English cube decimetre is 61.027051 cubic inches.

2. The weight of 1 cubic decimetre of distilled water, free from air, weighed in air against brass weights, at 4° C., bar. 30 in., is 15413.5 grains; and the weight of 1 cubic inch under similar conditions is 252.568 grains. Hence the volume of the standard cubic decimetre (English 1890) is $\frac{15413.5}{252.568} = 61.027129$ cubic inches.

3. The value of $(3.937079)^3$, adopted by Mr. Clark, is 61.0270 cubic inches.

4. The U. S. metre is 39.37 inches. Hence the U. S. cube decimetre is 61.023377953 cubic inches.

5. On the basis of 61.027051 cu. inches per cube decimetre, the number of cube decimetres in 1 cubic foot is $\frac{1728}{61.027051} =$ 28.31531.

6. The value of $\frac{1728}{\text{cu. in. per cu. dec.}}$, adopted by Mr. Clark, is 28.3153.

7. The number of U. S. cube decimetres per cubic foot is $\frac{1728}{61.023377953} = 28.31702$.

C.—CONVERSION VALUES.

1. "Cube centimetres into cube decimetres (litres)"—divide by 1000 (p. 17).

2. Cube centimetres "into litres"—divide by 1000.05 (p. 17).

3. Cube decimetres "into litres"—multiply by 1 (p. 24).

4. "Kilogram = 1000 grammes = 1 litre, or 1 cube decimetre water, 4° C. Miller, in 1856, found the kilogram = 15432.349 grains in vacuo. It was originally intended to be the weight of a cubic decimetre of water at maximum density in vacuo. It is now a definite mass of platinum and is slightly heavier than the cubic decimetre of water" (p. 50).

5. "Cube metres or steres (= 1000 litres very nearly) into litres"—multiply by 1000 (p. 61).

"Cube metres or steres into cube decimetres"—multiply by 1000 (p. 61).

"Cube metres or steres into cube feet"—multiply by 35.31658 (p. 61).

"Cube metres or steres into cube inches"—multiply by 61027.05 (p. 61).

6. "Kilograms (or litres) of water into cube inches"—multiply by 61.170 (p. 92).

"Cube feet of water into litres, 62° F."—multiply by 28.311 (p. 93).

"Cube feet of water into kilograms 62° F."—multiply by 28.249 (p. 93).

From this summary it will be seen that Mr. Clark's book and letter present us with quite an extended range of choice for the value of a litre, viz.:—

Standard litre (1890)	61.036272	cu. inches.
" decimetre	61.027051	" "
" " (weighed in air)	61.027129	" "
London litre (1890)	61.03626	" "
" " (1824)	60.9947	" "
Clark " (1890)	61.036284	" "
" " "	61.0364	" "
" decimetre	61.0270	" "
" "	61.02705	" "
U. S. "	61.023377953	" "
"kilogram" (in vacuo, 4° C., 1890)	61.104666	" "
" (in air 62° F.)	61.170	" "

and, in addition to these, I may quote the following from Table 1 of the before-mentioned article on "Fuel-Gas Values," viz.:—

Authority.	Cu. ins. in litre.
U. S. Dispensatory, 16th ed.	61.0280
G. Gore, LL.D., F.R.S.	61.024
Professor V. B. Lewes, F.C.S.	61.024
Professor J. D. Everett, F.R.S.	61.022
Trautwine (said to be U. S. Standard)	61.0254
"	61.024425
Haswell (said to be by Act of Congress)	61.022
"	61.02524
Gmelin	61.0267
W. Crookes, F.R.S.	61.02709
Thomson and Tait	61.02432
S. A. Ford	64.99008

The suggestion made by Mr. Clark that these discrepancies may for the most part be explained by the difference between the 1824 and 1890 standards is obviously insufficient if the difference he refers to be that of the cube inch value; for as the 1824 value is 60.9947 it clearly was not adopted by the authorities above quoted. Some other explanation is, therefore, required; and as so consummate an authority as Mr. Clark appears unable to advance one, I may perhaps be allowed to hint that the cause of the varying values is to be found in sheer laxity of calculation. I know that so commonplace a theory is rather shocking, and I duly blush as I advance it; but, really, when I find Mr. Clark himself deliberately adopting the value 61.0364 as the quotient of $\frac{15398.6}{252.286}$ and adopting it as the basis of his book, whereas the true quotient is 61.036284, or, if four places of decimals be used, 61.0363, I may plead for pardon with some assurance of the same being accorded. The example here cited is even still more to the point; for the value 15398.6 is adopted by Mr. Clark as the result of the calculation $\frac{15432.35}{1.00112} - 16.491$, whereas the true result is 15398.594 and this divided by 252.286 gives 61.03626.

But let it not be imagined that I make these remarks in any fault-finding or critical spirit. I am too conscious of my own short-comings to be willing to sit in the seat of judgment. In the before-mentioned table, for example, I derived Mr. Clark's second value of "cubic feet in 100 litres" from his figure of 61.04 cubic inches per litre. The calculation was, of course, $\frac{6104}{1728} = 3.5324$; and yet, when I corrected the proof of the article, I inadvertently allowed the value to appear as 3.5323. So I must ask my scientific brethren to understand that my observations are not intended as any disparagement of the "Dictionary of Metric Measures" or as casting any adverse reflection upon the other text-books I have quoted, all of which I regard as admirable examples of scientific work and as trustworthy as reasonable mortals can expect them to be.

And so we come back once more to our question, Where, after all, is the litre? Our puzzle-picture turns out to be of a kaleidoscopic variety and appears in a different aspect to every observer. In spite of the much-lauded simplicity of metric measures, we find that the "litre" has as many meanings as the "pound," that it is addicted to the reprehensible habit of impersonating its fellow-measures, that the virtue of its mother centimetre is open to grave suspicion, and that its own constancy is no better than it should be. What, then, are we to do? The answer to this question appears to me to be plain enough, and, indeed, constitutes the object I have had in view in originating and pursuing the discussion. The lesson of the litre teaches us the importance of a duty that is too often neglected, namely, the prefixing (or affixing) to every scientific paper, or treatise, a table, or other statement, setting forth the values assigned to the constants employed by the author. If this be done, it matters not one whit whether the values chosen are in accordance with the most rigorous determinations or depart therefrom. If any reader choose to attach different values he can then do so; whereas under the present system of every man being a hidden law unto himself, the perusal of a scientific work is not a process to which the phrase "emollit mores" can be justly applied.

Another lesson that we may learn from the litre is the futility of a besetting scientific sin, namely, the Affectation of Accuracy. The owner of that holy and hosannád thing, the "scientific conscience," is apt to deem himself "not as other men" and smiles complacently at the thought that he has expended long years and a fortune in determining, for example, that a cubic inch of water under certain conditions weighs 252.28599 rather than 252.28598 grains. And yet the same gentleman will, from his lofty pedestal of physics, look down with much pity, if not with absolute contempt, upon the equally conscientious entomologist who (vide *Nature*, Nov. 17, 1892) wears away a thinking and working lifetime in determining whether a certain insect walks upon more than three legs at once. The results of the most refined investigations are but approximations to the truth, after all; and in most cases of scientific work an approximation sufficiently close to the truth to serve all practically useful purposes can be arrived at easily and expeditiously. Accuracy, therefore, may often be, in the true sense of the term, excessive, even if intrinsically trustworthy; but when we consider that what appears accurate to one generation is regarded as inaccurate by the next, we must surely deem it but a poor thing to boast of. Take, for example, Mr. Clark's confession that up to September, 1891, he "had always assumed the cube decimetre and litre to be identical"; a confession which, coming from so distinguished an authority, is tantamount to a demonstration that most other physicists shared the same erroneous impression, and therefore that the much-vaunted accuracy of modern work in physical science has not existed to the full extent claimed. And yet we all know that the work has been really magnificent and solid, both in its contributions to the world's store of knowledge and in its advancement of the welfare of mankind. This certainly teaches us that reasonable care in scientific measurement is sufficient care, and that extreme care is, by the very nature of things, doomed to fail of its object.

A PRESUMABLY NEW FACT RELATIVE TO THE CEDAR WAXWING (*AMPELIS CEDRORUM*), WITH REMARKS UPON THE IMPORTANCE OF A THOROUGH KNOWLEDGE OF FIRST PLUMAGES.

BY EDWIN M. HASBROUCK, WASHINGTON, D. C.

It is considered by every one that the individual waxwing possessing wax tips on both secondaries and rectrices is in the highest development of plumage, while a high development of plumage in any species whatever is usually accorded to the older birds.

Coues states that, "Specimens apparently mature and full-feathered frequently lack the wax-tips"; that "their normal appearance is unknown," and that "birds in the earliest known plumage may possess one or more." Beyond this little appears to be known.

In a somewhat extensive series of waxwings in the National Museum, in my own and other collections, appendages on the wings were developed in forty-five, fifteen displayed the ornaments on both wings and tail, while the remainder, apparently adult birds, were entirely unadorned. (It might be well to state that the females as well as the males possess these tips, although less frequently, while some specimens examined showed the ornaments on both wings and tail.) Now, the natural conclusion from this would be that those birds possessing wing-tips only were older than those having none at all, while the fifteen on which both wings and tail were adorned were even older and were in the highest perfection of plumage. This is disproved by the fact that four birds of the year still in the striated plumage, taken in August, September, and October, respectively, display very distinct tips on the secondaries; and if on the secondaries at this early age when older birds possess none at all, why should they not also appear on the tail-feathers? The supposition of older birds only being adorned being disposed of, the question arises, When do these horny appendages appear? and on this I am able to throw considerable light.

It was in the summer of 1884 that I was spending a month at Port Byron, N. Y., when I ran across a nest of the waxwing, containing four young, every one of which had the wax tips on tail and wings perfectly developed. These birds were nearly fledged, although unable to fly, and I had good opportunity to observe them. Not being interested in collecting birds at that time they were not preserved, a circumstance to be regretted, but the full import of these appendages being developed in nestlings was appreciated.

The following table for the calendar year shows the conditions of specimens examined. So regularly and so nearly is it completely filled that it is evident that an examination of a larger series would undoubtedly fill the gaps.[1]

Month.	Wings.	Both.	None.
Jan.	♂		♂ ♀
Feb.	♂	♂	♂ ♀
Mar.	♂ ♀		♀
Apr.	♂ ♀	♂	♂
May	♂ ♀	♂	♂ ♀
June	♀	♀	♂ ♀
July	♂	♂	♂
Aug.	♂ ♀ ♂ im	♂	♂ ♀
Sept.	♂ ♂ im		♂ ♀
Oct.	? ♂ im		♀
Nov.	♂		♂ ♀
Dec.	♂		♂

With this evidence it is apparent that these handsome ornaments are by no means a sign of age, but are, on the contrary, a purely individual development, appearing sometimes in their highest perfection in the nestling, while in an adult they may be entirely absent or barely beginning to appear; or again, appearing a few months after attaining first plumage, to go through a regular course of growth and development. Inasmuch as an individual with wax on both tail and wings is exceedingly rare, and the August and September birds are just beginning to acquire the tips it would be interesting to know just how often this development in the nest occurs, and this is published mainly with the hope of eliciting further information on the subject, and of prompting those in the field to be on the watch the coming season.

The importance of thus studying the first plumages cannot be too highly estimated, for not until comparatively recent years has a careful and thorough study of the life-history of each and

[1] In this table an attempt has been made to show merely that both sexes are adorned for each month in the respective columns. In a number of instances several individuals were found for each.

every one of our birds been deemed of any great importance by ornithologists. Of late, owing to the discovery of numerous errors that had crept into our nomenclature, careful attention has been paid to a species from the time of its advent into the world to a period when beyond all doubt it has reached its maturity. To the collector who accumulates a series, it is only too apparent how great is the difference between individuals, and that his series is not complete until each and every phase of plumage from various widely separated localities is represented.

Late in the season, while the full migration is at its height, a bird is secured which for the life of him he cannot name; in vain he searches the literature, compares specimens, and puzzles and worries only to find it at last an old acquaintance flitting under new colors. I have in mind a young man who, although not an accomplished ornithologist, ought to have known better, and who essayed to publish a list of the birds of the locality in which he lived. One winter he secured a bird entirely unknown to him, and in his dilemma sent it to the Smithsonian for identification; on its return the label bore: "American Goldfinch in winter plumage." This may be a little foreign to the subject but it shows how necessary was a thorough knowledge of the life-history of the species. Nor was it so very long ago that the "Gray Eagle," which for years was accorded specific rank, was found to be but an immature phase of *Haliœetus leucocephalus*, while *Oidemia perspicillata trowbridgii* was shown to be but a seasonal variation of *perspicillata* proper. Even to this day it appears not to be generally known that the Golden Eagle takes from three to five years to acquire its full plumage; that the Bald Eagle attains his highest plumage at the age of three, the various intermediate stages being known as the Black Eagle, Gray Eagle, etc., and that the Little Blue Heron is pure white the first year, mottled and variegated with blue in every conceivable manner the second, and attains the perfection of its plumage only at the age of three; yet such are the facts. These are but isolated cases, while any day may bring about the unification of some two forms which at present are considered at least sub-specifically distinct.

BOILING-POINT AND RADIUS OF MOLECULAR FORCE.

BY T. PROCTOR HALL, CLARK UNIVERSITY, WORCESTER, MASS.

WHEN a bubble of its own vapor exists in a liquid the pressure, P, upon it is the sum of the three pressures:—

A, due to the air,

W, due to the water above the bubble,

C, due to molecular cohesion.

Let us suppose, for convenience, that the bubble is so close to the surface that W may be neglected. When the radius, r, of the bubble is large compared with R, the radius of molecular force (i. e., the distance at which a molecule ceases to exert a a sensible attraction), the pressure, C, over its diametral plane is equal to the surface tension, T, across the circumference. That is to say,

$$\pi r^2 C = 2\pi r T$$
$$\text{or } C = 2T/r.$$

Then $P = A + 2T/r$; and the temperature must be such that P is balanced by the molecular energy of the vapor if the bubble is to continue to exist. As r increases $2T/r$ decreases, and for bubbles of ordinary size may be neglected in comparison with A, the ordinary pressure of the air. Hence the lowest possible boiling point of a liquid is such that the vapor pressure is just sufficient to overbalance the air pressure. But at one or more points in the liquid the temperature must be very much higher, or no bubbles of vapor could be formed. This condition occurs whenever a liquid is boiled in a rough vessel.

If a liquid be uniformly heated no bubbles can be formed until the temperature is such that $P = A + C$ for the whole liquid when the bubbles are first formed. When this point is reached bubbles are formed everywhere, the pressure upon them decreases very rapidly as they increase in size, and the liquid explodes. The explosion point, like the boiling-point, depends in part upon the pressure of the air, but has a definite lower limit when $A = 0$.

Unfortunately the value of C in terms of the surface tension cannot be calculated directly for the explosion point; but a probable value may be found as follows:—

When a U-shaped wire in an inverted position is drawn up from a liquid, in many cases a film is formed between the wire and the liquid surface. For a pure liquid the thickness, k, of the film is nearly constant, though it varies greatly in some solutions. The film has a measurable tension, $2T$, across every linear centimetre on its surface. In other words, a force of $2T$ is drawing apart, against the forces of cohesion, a liquid whose section is $(K \times 1)$ square centimetre. It seems probable, therefore, that the liquid will give way at every point when the expansive force opposing C becomes $2T/k$ on each square centimetre; so that at the explosion point

$$P = A + 2T/k.$$

In 1861, Dufour (Comptes Rendus, 52, p. 986) succeeded in heat ing water to 175° C., and chloroform (which boils at 61°) to 98°, under ordinary air pressure, without explosion. Assuming that these are approximately the explosion points of water and chloro form, we may calculate, from the known values of the surface tensions and the vapor pressures at these temperatures, that the value of k for water is 123 $\mu\mu$ (1,000,000 $\mu\mu$ = 1 mm.), and for chloroform 200 $\mu\mu$. From a solitary case, which may be only a coincidence, it would be rash to generalize; yet it is interesting to notice that the ratio of these two values of k is almost exactly the ratio of the molecular diameters of water and chloroform.

Now R, the radius of molecular force, is known to lie somewhere between k and $k/2$ (see Jour. Chem. Soc., 1888, p. 222). Hence, if the preceding equality of ratios be found to hold for other liquids we shall have the theorem that the radius of molecular force is proportional to the diameter of the molecules.

Quincke (Pogg. Ann., 137, p. 402, 1869), by a method likely to give results a little too low, measured R and found for water 54 $\mu\mu$, a value which is in close accord with that given above.

The experimental determination of the explosion points of different liquids requires no complicated apparatus and would have considerable scientific interest. I make the suggestion for the use of any one who has time and inclination for research without the advantages of a well-equipped laboratory.

DR. GEORGE VASEY.

DR. GEORGE VASEY, Botanist of the Department of Agriculture, died, in the City of Washington, March 4, 1893. He was born on Feb. 28, 1822, at Scarborough, Yorkshire, England, and came to America when a child. He graduated from Berkshire Medical College at Pittsfield, Mass., in 1848, and settled in Illinois, where he practised his profession for twenty years. He was appointed Botanist to the Department of Agriculture in April, 1872, and held the position until his death. As Botanist to the Department he was Honorary Curator of Botany in the U. S. National Museum, and it is largely from his efforts that the present herbarium of over 25,000 species has been accumulated and arranged. His main work has been upon grasses, and among other papers he has printed "Descriptive Catalogue of Native Forest Trees of the U. S.," 1876; "Grasses of the United States: A Synopsis of the Tribes with Descriptions of the Genera," 1883; "Agricultural Grasses of the United States," 1884; "Descriptive Catalogue of the Grasses of the United States," 1885; "Report of Investigations of Grasses of the Arid Regions," two parts, 1886–87; "Grasses of the South," 1887; "Agricultural Grasses and Forage Plants of the United States," a revised edition, with 114 plates of "Agricultural Grasses," 1889; "Illustrations of North American Grasses; Vol. I., Grasses of the Southwest," 100 plates with descriptions, 1891; Vol. II., Part 1 of the same, "Grasses of the Pacific Slope and Alaska," 1892; "Monograph of the Grasses of the United States and British America" (Vol. III., No. 1, Contributions from U. S. National Herbarium) 1892.

He was a delegate from the Department of Agriculture and the Smithsonian Institution to the Botanical Congress in Genoa, last September, returning immediately after the adjournment of the congress. He was a member of the Biological and Geographical Societies of Washington, and a Fellow of the American Association for the Advancement of Science. He was taken sick on Feb. 28, and died after a short illness on the morning of March 4, of constriction of the bowels.

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CALIFORNIA PICTOGRAPHS AND HIEROGLYPHICS.

BY MRS. THEODORE H. HITTELL, SAN FRANCISCO, CAL.

THE study and investigation of the works of the earliest inhabitants of a country is now a science in itself, and is receiving more and more attention in all parts of the world.

Too little has heretofore been done in California, Alaska, Nevada, and Arizona to investigate, gather up, and preserve the relics and works of the prehistoric races which inhabited these western territories, and as there is now but little left, that little should without delay be carefully sought out and put in such shape as to remain a permanent possession. Of much, on account of our own carelessness we have been despoiled, and much that yet remains has been more or less defaced and injured.

Government, as well as scientific societies, should look to the preservation of what remains of the structures, tools, utensils, and weapons of the aborigines, and by all means endeavor to gather together and preserve by photographs the cipher writings which are yet to be found and which, year after year, by the corroding hand of time and the more destructive hand of ruthless vandalism, are becoming more and more defaced and ruined.

The cipher writings yet to be found from Alaska to Arizona, if carefully gathered and studied, might enable us to learn many very important facts concerning the customs of the redskins and their early history.

In the Sierra Nevada Mountains, near the so-called Summit Soda Springs, about fourteen miles south of Donner Lake and at an elevation of about 6,000 feet above the level of the ocean, the attention of tourists is attracted by numerous inscriptions incised in the rocks.

The most prominent, and the most inviting of attention of these, are those cut in the granite rocks, about a hundred feet high, which stand nearly isolated on the right and on the left of the headwaters of the North Fork of the American River.

The stream there is almost a little torrent and dashes over the rocks in cascades and from there it plunges into and through a mountain gorge towards the lower level far below.

To a person standing near the fountain-head of the river, on the rocks against which it chafes and which it is gradually but surely wearing away, and who takes note and truly appreciates the grandeur of the scenery, there comes a feeling of awe and reverence. It elevates the soul and calls forth a spirit akin to religious worship.

It was here in this sublime region that an unknown people left pictographs on the rocks pertaining doubtless to their history and religion. The seasons of centuries since then have come and gone; the snows of uncounted winters have covered them; succeeding springs and summers unnumbered have decked the mountains with yearly verdure, and the river has been rushing on and on and cutting its bed deeper and deeper. All this we know; but we know nothing of those who wrote these ciphers on the monumental rocks. They have long since passed away.

Only with the help of science and long study and comparison, can we hope to gain an inkling of the meaning these ciphers were intended to convey, and add, perhaps, some important facts to the ancient history of California—a subject now so full of interest and becoming daily of more and more interest to the world.

According to the Report of the Bureau of Ethnology at Washington, pictographs of the North American Indians are found at Santa Barbara and San Diego in California, and in Nevada, Arizona, Oregon, Idaho, and Utah.

In Nevada great numbers of incised characters of various kinds are found on the rocks flanking Walker River. These are waving lines, rings, and what appear to be vegetable, animal, and human forms. Among the copies of pictographs obtained in various portions of the Northwestern States and Territories by Mr. Gilbert, one kind is referred to as being on a block of basalt at Revielle, Nevada, and is mentioned as Shinuma or Mosquis.

This suggestion is based upon the general resemblance to drawings found in Arizona, and known to have been made by Mosquis Indians.

In Oregon, numerous boulders and rock escarpments at and near the Dalles of the Columbia River are covered with incised or pecked pictographs. Human figures occur; but other forms predominate. From Lieut. J. H. Simpson's Topographical Bureau Report we take the following: "At the Rio de Zuni, in 1849, we met Mr. Lewis, who had been a trader among the Navajas, and according to his statement had seen inscriptions on a rock on his travels to and fro. He offered to guide us. He led us to a low mound. We went up and found inscriptions of interest, if not of value; and of them some dating so far back as 1606. The rock is since mentioned as Inscription Rock." The following letter, addressed to Lieut. J. H. Simpson, was written by Danatiana Vigil, Secretary of the Province of Santa Fé, on October 19, 1849.

"*Sir:*—The engravings which are sculptured on the rock of Fish Spring, near the Pueblo of Zuni, copies of which you have taken, were made in the epoch to which they refer. I have an indistinct idea of their existence; but, although I have passed the place some three times, I never availed myself of the opportunity to observe them. The other signs or characters noticed are traditional remembrances, by means of which the Indians transmit historical accounts of all their remarkable successes. To discover these sets by themselves is very difficult. Some of the Indians make trifling indications, which divulge, with a great deal of reserve, something of their history, to persons in whom they have entire confidence. The people who inhabited this country before its discovery by the Spaniards were superstitious and worshipped the sun."

Mr. G. K. Gilbert discovered etchings at Oakley Springs, Eastern Arizona, in 1878, relative to which he remarks that an Orabi chief explained them to him and said that the Mosquis make excursions to a locality on the Colorado Chiquito to get salt. On their return they stop at Oakley Springs and each Indian makes a picture on the rock. Each Indian draws his crest or totem, the symbol of his genus. He draws it once, and once only on a visit.

From Alaska to Arizona many inscriptions on rocks are found. Of some of them photographs have been taken. But so far as we know none are as extensive or of such variety and of so ancient a date as those situated near the source of the American River.

These pictographs seemingly resemble and are written in much the same way as the Chinese ciphers where each figure is a word and has a full meaning, and seemingly they should be read from right to left.

Max Müller says, in writing of the American aborigines: "Though the Indians never arrived at the perfection of the Egyptian hieroglyphics, they had a number of symbolic emblems, which were perfectly understood by all their tribes. For instance, power over man is symbolized by a line drawn in the figure from the mouth to the heart. Power, in general, by a head with two horns. A figure with a plant as head and two wings, denotes a doctor skilled in medicine. A tree with human legs, a herbalist. Night is represented by a finely crossed or barred sun, or a circle with human legs. Rain is figured by a dot or semicircle filled with water and placed on the head. The heavens with three disks of the sun is understood to mean three days' journey; and landing after a voyage is represented by a tortoise. But there is no evidence to show that the Indians of the north ever advanced beyond the rude attempts which we have thus described."

Lord Kingsborough's publication of "The Mexican Hieroglyphics" shows a higher developed intellect among that people and cannot be placed in the same category with those of the aboriginal Indians of the United States. They are colored, written on paper, and are in many respects equal to the hieroglyphic inscriptions of Egypt.

These most interesting books with their colored picture-writings, copies of which are in the possession of our California Academy of Sciences, are worthy of the cost expended on them and the attention given them by scientific men. They give an idea of the true condition of the inhabitants of Mexico before the landing of Cortez. Max Müller says:—

"One of the most important helps towards the deciphering of the hieroglyphics is to be found in certain American books, which, soon after the conquest of Mexico, were written down by natives who had learned the art of alphabetic writing from their conquerors, the Spaniards. Ixtilxochitl, descended from the royal family of Tezeuca, and, employed as interpreter by the Spanish Government, wrote the history of his own country from the earliest time to the arrival of Cortez. In writing this history he followed the hieroglyphic paintings as they had been explained to him by the old chroniclers. Some of these very paintings which formed the text-book of the Mexican historian, have been recovered by M. Aubin, and as they helped the historian in writing his history, that history now helps the scholar in deciphering their meaning.

It is with the study of works like that of Ixtilxochitl that American philology ought to begin. They are to the student of American antiquities what Manetho is to the student of Egyptian hieroglyphics or Berosus to the decipherer of the cuneiform inscriptions.

A small part of the hieroglyphics found at the source of the American River, which I have thus described, have been photographed by Mr. Jackson, principal of the Sacramento Art School.

But, for the sake of science, it would be well, it seems to me, to have the whole of the rock-inscriptions photographed and preserved for ethnological and scientific research.

INVOLUNTARY RECOLLECTION.

BY JAS. W. DONALDSON, ELLENVILLE, ULSTER COUNTY, N.Y.

IF one will but organize himself into a society for "psychical research" and, cultivating a habit of introspection, observe carefully even his own mental processes, he will find much to interest and confound him.

And perhaps no other operations of his mind will furnish him with more occasion for thought and investigation or prove more interesting and suggestive than some of the vagaries of *involuntary* recollection.

There are few persons indeed of ordinary intelligence to whom this at times strangely spontaneous habit of memory is not a familiar, recognized experience, exciting more or less their wonder and speculation.

For example, we can all recall occasions when, though however earnestly engaged with other thoughts, we have all at once awakened to the discovery that we were at the same time unconsciously humming a snatch from some old song, or mentally repeating a fragment of prose or verse learned in our childhood, which we had supposed was long since buried so deep down under the *débris* of years as to be beyond the hope of resurrection; yet there it was, as fresh and vivid as ever, having, with a dash of its old-time irrepressibility and abandon, burst in upon our consciousness again without so much as asking "by your leave."

We may remember, too, that it has often happened that these unexpected visitants were of a character to cause us much discomfort and humiliation, for we have found by sad experience that we may not easily "pluck from memory a rooted sorrow," nor "raze out the hidden troubles of the brain;" and, worse than all, that the "damned spot" will never "out," however frantic and agonizing may be our entreaty. Indeed, it is impressed upon us that, if there be any of our memories which are more perverse and persistent than others, it is the erratic, or disreputable ones, which we have thoughtlessly garnered and forced into unnatural companionship with our graver and better impressions. These will return again and again in spite of us, and it seems, as if with malicious intent, that they often delight in choosing opportunities when it is most to our embarrassment and mortification.

Perhaps we are at a funeral, and have become touched and subdued by the saddening ceremonies, or at church, earnestly engaged with its impressive services, when, all at once, without warning, one of these irreverent sprites of memory, with cap and bells and many a comic antic, breaks in upon our serious mood, and, wantonly disregarding the sanctities of the occasion, makes mouths at its solemnities. Or it may be that sometime when in the midst of a scene of innocent mirth and jollity the ghost of an unavailing remorse, or the shadow of an event in our life full of shame and agony, may suddenly appear to sadden and sober us and dissipate our enjoyment.

The writer recalls an incident in his own experience illustrating the sometimes strange unexpectedness of this phase of recollection.

Many years ago he was moved to memorize certain quaint and amusing verses found in a newspaper. On a March day long after, as he was riding out of Albany, and in a comfortable and complacent mood listlessly gazing out of the car window upon the bedraggled and cast-off garments of a rough and dissipated winter, suddenly these verses, committed over thirty years before, broke in upon his thoughts and began to reel themselves off with the startling abruptness and unmanageable spontaneity of a wayward alarm-clock.

Perhaps it was more than twenty years since they had last occurred to him. He tried in vain to discover what in all that dreary, forbidding landscape, or in the nature of his thoughts, had set this jangling waif of memory agoing, but could not in any way account for it; nothing in his mind seeming to bear the remotest relation to it. Apparently, as if obedient to some unexplained law of periodicity, this disreputable tramp of the brain had, in its vagabond wanderings, rounded its period, and, with an impudent smirk and an affected wail of distress, there it was again, begging, "for Christ's sake," a dole of recognition at the open door of an unwilling and repelling consciousness.

Possibly, if we accept the later, and what seems the more reasonable, conception of consciousness, that is, that it is not all of memory, but merely one of its phases or conditions, and a dependent, unstable one at that, we can the better account for some of these freaks of spontaneous recollection.

It is evident that a normal brain has more or less control over that which shall cross the threshold of consciousness, for we know many persons have the faculty of so absorbing themselves with any certain line of thought as to be seemingly quite oblivious for the time to everything else not pertinent to it.

But, while it may appear that they are generally successful in thus holding the door against a besieging host of interloping and disturbing recollections, yet even they, too, sometimes fail to make the exclusion completely effectual.

Indeed, because of the very intensity of their thinking and their unusual turmoil of brain, they are likely to arouse and quicken other associations having certain constituent elements in common with those entering into the texture of their main thought, and these, too, may sneak into cognition along with the invited guests in spite of their every precaution.

Again, with the majority of persons, "mind wandering" is more or less a besetting infirmity. The spring which holds the door of their consciousness either has a congenital weakness or has become more and more impotent because of disease or approaching senility, and is therefore capable of offering little resistance to any strays of memory which may seek to enter. In fact, so degenerate do some minds become, that consciousness, once a guarded and sacred preserve, is now a commons or thoroughfare through which any vagrant, motley procession of thoughts may troop at will without let or hindrance.

No doubt, too, this open, unguarded condition of consciousness may come upon us at times simply from our relaxing nervous tension, as when we unharness the will and turn it loose, and lapse generally into a state of mental passivity and listlessness. It is then that, finding the door left ajar, these unbidden recollections oftenest make their intrusive entrance.

Perchance, too, Unconscious Cerebration may take advantage of the situation to display and call attention to some of its remarkable curios, and, abusing the opportunity, lug in with these certain annoying remembrances which we would it had left in undisturbed oblivion.

Perhaps one of the most significant and suggestive revelations that comes to us from a thoughtful observation of these extraordinary phenomena of involuntary recollection, is the abundant proof they furnish us of the unexpected and marvellous tenacity of our impressions.

It is made very manifest that those potential organic conditions which were set up and established in the original process of developing these impressions are still preserved to us intact, and need only the proper excitant or stimulus to revive and rehabilitate them for us again and again.

Being well assured of this, it would seem profitable for us to inquire to what extent, not yet realized, can we, by a deliberate and persistent exercise of the will, control and compel these conditions of revival.

We are all conscious of doing a good deal of recollecting by *voluntary* effort, but it is mostly those ordinary experiences which are comparatively recent and fresh. When it comes to making labored and prolonged effort to restore some elusive and faded image of a remote past, we are easily discouraged, and, even though it be a momentous event in our lives, a vivid and complete recollection of which might save us from dishonor or utter ruin; yet, after making a few hopeless and abortive attempts to remember, we are apt to give up in despair, when, perhaps, had we been fully possessed with an abiding faith in the enduring nature of our impressions and in the possibility of our reviving them, no matter how remotely fixed, we might have hopefully and courageously continued our efforts, even for days or weeks if necessary, until the missing fact was again brought into the fold of consciousness.

Surely, if, as has often happened in human experience, grave accidents or emergencies have resulted in so quickening and rehabilitating certain conditions of the brain as to fully restore to the person recollection of events long supposed to be irretrievably lost, it demonstrates the reasonableness of our employing and confidently relying upon systematic and patient effort to compel the same active and exalted mental conditions to produce the same happy result.

THE ARRANGEMENT AND NUMBER OF EGGS IN THE NEST.

BY DR MORRIS GIBBS, KALAMAZOO, MICH.

ALL birds have a system or arrangement in depositing their eggs in the nest, and there are very few species, if any, in which some peculiarity is not to be seen, if careful observation is made. Many birds so plainly and invariably show a tendency to a set arrangement that their habit is generally known. It is of these well-known examples that we will speak.

The loon or great northern diver always deposits two eggs. They are almost perfectly elliptical in shape and lie side by side. The eggs are invariably found at over three-fifths of the distance from the front edge of the nest depression, that is, at about two-fifths of the long diameter from the rear end of the elongated hollow or nest proper. From the position of the eggs one can tell how the bird sits on the nest, as we may reason that, with these long-bodied birds, the abdomen, which supplies the direct heat, is well back from the front of the hollow. This theory is verified by watching the incubating bird. The turtle dove, night-hawk, whippoorwill, and common domestic pigeon, each of which lays two eggs at each setting, deposit the eggs side by side, although this arrangement is frequently interfered with in the case of the tame bird, not rarely with the result that one of the eggs does not hatch.

The spotted sandpiper and killdeer plover, and I presume most of the other snipe and plover, lay four eggs at a clutch. The eggs are arranged in the nest, or on the bare ground, with their small ends together, and, as they are pyriform in shape, they join in to perfection. The eggs of the snipe and plover groups are proportionately exceeding large for the size of the bird, and the saving of space by this arrangement undoubtedly answers a purpose. It is impossible to offer a solution to this problem of order at present, unless we may suggest that it is a wise provision of some ruling power, which so ordains the arrangement which best admits of the bird's covering the eggs thoroughly. It is fair to doubt if a sandpiper could cover her four large eggs if they were arranged in any other position besides that in which they are found, with the four smaller ends pointing to a centre. This species has a small body and is not provided with loose, fluffy feathers, so well supplied to many grouse and other birds which lay many eggs. On two occasions the order of the eggs in nests of the spotted sandpiper was broken by us; an egg being turned about with its point presented outward. One of these nests was deserted, perhaps from the intérference, but in the other the order was found restored within a day.

Perhaps no bird in America, certainly no other in Michigan, equals the common bob-white or quail in the number of eggs it sets upon. This species not infrequently lays eighteen eggs, and even more are found in one nest, but I can assure the readers that with any other shaped eggs the bob-white could never succeed as a successful setter. I will suggest that my friends with collections at hand compare a set of twenty eggs of the quail with twenty eggs of equal dimensions in longer and shorter diameter of any other species, and observe which lot occupies the smaller space. We may say, for illustration, that the bob-white's egg is triangular, and fits in as no other egg, to my knowledge, can.

With all birds which lay a good-sized clutch, so far as my observations go, the eggs are deposited in almost an exact circular group. The bird must use excellent judgment in thus arranging them, for it is only by this order that they can all be covered properly. Not infrequently when a grouse is startled from her eggs she tumbles one of her treasures from its bed. If the egg is not too far removed, it will almost invariably be found returned to its exact position in the nest within a few hours.

I have been informed that the brown pelicans steal eggs from one another's nests, in order to fill their complements, or at least take possession of those they find lying on the ground and roll them into their nests. Although this does not seem at all likely, for various reasons, I cannot dispute it authoritatively, and, moreover, there were strong proofs that such was the case in many nests that I examined in Florida. These nests, which were near together, often contained four eggs, never more; one to three of which were ready to hatch, the others being fresh, or nearly so. And, again, there would be eggs in the same nest with young over a week old, or young of ages quite ten days variation. But one point was ever observable, the young, or eggs, or both, never exceeded four in number, showing, even if the charge of abduction is proven, that the old birds know their limit.

The cow-blackbird, in imposing its eggs on the care of other birds, not rarely fails in the arrangement of affairs. It is fair to allow that the cow-bird is perfectly able to distinguish its own eggs from those of the blue-bird, chipping-sparrow, and others, which differ radically in size and color from its own speckled, tough-shelled eggs; but I believe it often fails to distinguish its eggs from the quite often similar ones of the chewink and oven-bird. And this failure accounts for its depositing as high as four and five eggs in the nests of the chewink, where there was but one egg of the owner; and again laying four eggs in an oven-bird's nest, which contained no eggs at all of the owner,—both cases undoubtedly oversights, which resulted from its inability to distinguish. It is reasonable to allow that cow-birds have limits as to the number to be deposited, otherwise some unfortunate warbler or other small bird would be overwhelmed. As it is, the cow-bird studies the limits of endurance in its victims and rarely exceeds the bounds. The most eggs I ever found in a nest infested by cow-birds was nine, and the species generally lays only two or three eggs, thus generally keeping the outside limit to six or seven, with the owner's eggs.

THE USE OF THE TERM "CARBOHYDRATES."

BY W. E. STONE, PH.D., PURDUE UNIVERSITY, LAFAYETTE, IND.

IT has frequently happened in the history of chemistry that names and terms have lost their original significance so soon as the knowledge of the bodies to which they were applied has become more extended. "Organic" chemistry is better named the "chemistry of carbon compounds;" the "aromatic" bodies have disappeared in the broader designation of benzine derivatives. In the same way it appears that we have reached, or already passed, a transition stage in the use of the term "carbohydrates." Treatises on chemistry still retain the old definition of the term, while those familiar with recent progress in this field no longer feel themselves restricted to these ancient limits. It is the purpose of this paper to consider the present status of this subject.

Von Lippmann, in his work "Die Zuckerarten und ihre Derivate," adopts Fittig's view that the carbohydrates are derived from the hypothetical heptatomic alcohol $C_6H_7(OH)_7$, which, by loss of water, forms the simple or complex anhydrides, $C_6H_{12}O_6$ or $C_{12}H_{22}O_{11}$, known as sugars. His treatment ignores the existence of any carbohydrate with less than six carbon atoms, although he says that, with the (at that time, 1882) slight knowledge of the constitution of the carbohydrates, it was impossible to regard this definition as final and complete.

In 1888 appeared Tollens' "Handbuch der Kohlenhydrate," in which the definition of "carbohydrates" was limited strictly to the bodies composed of C, H, and O, containing six carbon atoms, or some multiple of six, and H and O in the same proportion in which they are found in water. But already Kiliani had shown that arabinose, which had long been regarded as a true carbohydrate on account of all its reactions, had really the composition $C_5H_{10}O_5$. Moreover, it had already been established that the best known sugars, such as dextrose, levulose, galactose, and arabinose, had the constitution of aldehydes or ketones of the hexatomic, respectively pentatomic, alcohols. In anticipation, therefore, of evident progress along this line, Tollens remarks in his preface that such bodies as arabinose and the impending erythrose might well be regarded as carbohydrates, but he retains the hexatomic nature as a requirement for the "true carbohydrate," and puts all non-conforming but similar bodies under the head of "den Kohlenhydraten nahestehenden Körper."

Up to this time a sort of understanding had prevailed that the carbohydrates were exclusively products of natural forces. It had also been noted that these bodies gave certain reactions, which were also presented as a basis for the classification given.

These reactions, as stated by Tollens, are :—

1. Reduction of alkaline metallic solutions.
2. Rotation of polarized light.
3. Subject to alcoholic fermentation by yeast.
4. Formation of levulinic acid.
5. Formation of characteristic compounds with phenylhydrazin.
6. Certain color reactions.
7. Solubility, either before or after hydrolysis.
8. Decomposition by heat.

All of which hold strictly true for the hexatomic carbohydrates. This classification was probably as liberal as the state of knowledge at that time would justify.

But this classification is evidently arbitrary and ought not to have weight in comparison with any classification based on chemical constitution. If a similar constitution can be proven for a series of bodies, the fact that they respond to certain reactions will only be additional proof of their relationship. Such reactions must, of course, be general in their nature, while special reactions will only serve to characterize individuals. In this way the class of carbohydrates must eventually include only bodies of certain constitution, while the characteristic reactions will be limited to a smaller number, of more general application. A similar development has taken place in the manner of classifying the hydrocarbons, alcohols, acids, glycerides, etc.

Of the carbohydrates conforming to the old definition, dextrose, levulose galactose, and mannose are types. They respond to the reactions given and have been found to possess the constitution of ketones or aldehydes of the hexavalent alcohol, $C_6H_{14}O_6$. But we know two bodies of the formula $C_5H_{10}O_5$, arabinose and xylose, which are also aldehyde alcohols, and which give the same reactions as their homologues, with the exception of fermentation and the formation of levulinic acid. Again, we know an aldehyde of the tetratomic alcohol erythrit, called erythrose, of the formula $C_4H_8O_4$, which responds to the same general reactions as its homologues. Glycerose, $C_3H_6O_3$, has also been studied and found to correspond to the others of the series in constitution and general reactions. It is even fermentable with yeast like the regular carbohydrates, which shows this to be an intermittent reaction when applied to an homologous series. Beginning again with the group $C_6H_{12}O_6$, we find that there have been prepared synthetically three other homologues representing aldehydes, respectively of the hept-, oct- and nonatomic alcohols. These also respond to the general reactions given, except that they do not form levulinic acid. Heptose and octose do not ferment, but nonose, with its multiple of three carbon atoms, is fermentable.

It is no argument against the carbohydrate nature of these bodies to say that they do not occur in nature, since two of the hexoses (galactose and mannose) have never been found free, but are only known as derivatives of certain natural products. In this respect they are on precisely the same footing as arabinose, xylose, erythrose, and glycerose.

It appears, therefore, that we have an homologous series of aldehyde or ketone alcohols of the general formula $C_nH_{2n}O_n$ with these common properties : 1° sweet to the taste; 2° optically active; 3° reducing alkaline metallic solutions; 4° yielding with phenylhydrazin characteristic crystallinic compounds. Other reactions, such as great solubility, decomposition by heat, and color reactions, are less characteristic, although possessed in common. Those containing three, or multiples of three, carbon atoms undergo alcoholic fermentation with yeast, and this periodical reaction seems an additional argument for their common nature. Individually they yield, when heated with strong acids, characteristic derivatives; for instance, the pentoses yield furfurol; the hexoses levulinic acid ; others have not been carefully studied in this direction.

Following are the members of this homologous series which are known, although several additional isomers are possible:—

Triose, $C_3H_6O_3$.— Glycerose.

Tetrose, $C_4H_8O_4$.— Erythrose.

Pentose, $C_5H_{10}O_5$.— Arabinose, xylose.

Hexose, $C_6H_{12}O_6$.— Dextrose, levulose, galactose, mannose, all in isomeric forms.

Heptose, $C_7H_{14}O_7$.— Heptose.

Octose, $C_8H_{16}O_8$.— Octose.

Nonose, $C_9H_{18}O_9$.—Nonose.

By the definition of carbohydrates, now extant, only the hexoses are included. It is the purpose of this paper to propose the extension of this term to all members of the homologous series, on the basis of a common constitution, viz., as aldehydes or ketones of the normal polyatomic alcohols of the aliphatic series. As characteristic properties of all these, must follow their behavior toward polarized light, toward alkaline metallic solutions, and toward phenylhydrazin.

Such a classification would exclude the bodies of the cellulose group, of which there are many, more or less well defined. But it is not yet evident that they possess a constitutional relation to the bodies under discussion, and have certainly no claim to be classed with the aldehyde or ketone alcohols because convertible into them.

As for the disaccharides of the hexoses, to which belong sucrose, lactose, etc., if it be true, as supposed, that they are anhydrides or ether-like forms of the hexoses, then they are entitled to a place among carbohydrates as derivatives or modifications of the same.

E. Fischer proposes to apply the name "sugars" to all the members of this homologous series, to which he has lately added the glycol-aldehyde $C_2H_4O_2$ as the simplest possible example. The popular conception of the properties of a sugar are not, however, easily reconciled with the properties of some of these bodies, while "carbohydrates" at least possess some reference to their impirical composition. With regard to glycol-aldehyde, moreover, its optical inactivity would exclude it from the list under the conditions here proposed, although its constitution undoubtedly satisfies the requirements.

ELECTRICAL NOTES.

Variations in Resistance.

IN a recent article in the *Philosophical Magazine* appears a paper by Mr. Fernando Sandford, entitled "A Necessary Modification of Ohm's Law." Why it should have been given this title does not appear, for it nowhere calls in question the law which goes by Ohm's name. A better title would have been "On the Variation of Resistance of a Conductor with Change of the Medium Surrounding It." The facts observed are of interest, though not new, as it has long been known that the resistance of a wire changes when immersed in different gases. Chatelier, for example, found that the resistance of a silver wire changed enormously when immersed in hydrogen gas, and that if left in it for some time its temperature coefficient changed also. Mr. Sanford has extended the list considerably, his experiments, though made with a wire of one metal only, i.e., copper, embrace a great variety of mediums, both liquid and gaseous. That the variation is due to the causes noticed in the experiments of M. Chatelier and not to heating of the conductor, as proposed by some, is probable from the following considerations. The total heat generated in the wire, using the ordinary coefficients of emissivity for polished copper, would not raise the temperature of the wire more than the one ten-thousandth of one degree centigrade, and the increase of resistance from this cause would be inappreciable. But the effect of a thin film on the wire would be far different. It was first pointed out by Mr. Kennelly to the writer that the extremely thin film of tin on electric conductors was sufficient to lower the resistance of moderately small wires as much as five per cent. If we suppose that when a wire is placed in a gas like SO_2 a thin film of a compound of the copper and the gas is formed, only the one twenty-five-thousandth of an inch in thickness, it will account for all the phenomena observed by Mr. Sanford. For, as the wire experimented on was one millimetre in diameter, the formation of a layer $\frac{1}{25000}$ of an inch thick would reduce the cross section of the copper by two-tenths of 1 per cent, and therefore increase the resistance by 0.2 per cent, or nearly the maximum change observed by Mr. Sanford. This thickness of film is not much greater than the thickness of the films which cause the iridescent colors on steel, being about three to five times as thick; so that we see that the slightest action of the gases on the surface of a wire would change the resistance quite appreciably, and on exposure to air the wire would recover itself again. It should be added, moreover, that such films would not necessarily be visible.

An easy way of settling the question would be to use wires of different diameters. With a wire whose diameter was .0035, or No. 40 B.W.G., and which is furnished for commercial purposes, the resistance should vary as much as one and a half per cent, while with a wire one centimetre in diameter it should be inappreciable. R. A. F.

A JOINT meeting of the Scientific Alliance of New York, in memory of Professor John Strong Newberry, will be held at Columbia College, Monday evening, March 27, 1893, at 8 o'clock. An address will be given by Professor H. L. Fairchild, "A Memoir of Professor John Strong Newberry." Remarks will be made by others, and a number of letters regarding Professor Newberry will be read.

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.

Does the Ether Absorb Light?

WHETHER or not light is absorbed in any degree by the ether through which it passes has been argued a good many times, and to-day is not settled on any experimental basis. That it is not so absorbed to any considerable degree is evident from the light from such distant stars that reaches us. From theoretical considerations some have concluded that many more stars would probably be seen by us if in some way their light was not stopped by the ether, and that the midnight sky would or should be brighter than it really is.

In all the treatments of the subject which I happen to have seen, there is one important element which has not been considered at all, and to me it seems as if that one would account for the limit to the number of stars we see without assuming that the ether possesses the ability to transform energy within itself, which would be the case if the energy of waves like light waves were changed into any other kind of energy not capable of affecting our eyes. This fact is, that, in order to see, some energy is needful. I mean that there must be some limit to the amplitude of the vibratory movement beyond which we could not see, simply because the energy of the wave is insufficient; so that no matter what the intrinsic brightness of a given light may be, if it be far enough removed from an observer it will cease to be visible, simply because the energy of the waves is too small to excite the sensation. As the energy of such radiant energy on unit area varies inversely as the square of the distance, and as the amplitude of the vibrations at the initiating atoms or molecules can at best not exceed the diameter of the atoms or molecules, the extreme minuteness of the amplitude at the distance of the fixed stars from us shows how exceedingly delicate is the eye for perceiving it at all. The enormous frequency of the waves gives them a degree of energy they could not otherwise have; but if there were no amplitude there would be no energy, and it is to be conceived that if space be illimitable and the number of stars be infinite, yet with eyes constituted like ours only the light of stars within a limited space would be visible, and such optical data would give no reason for holding that what could be seen was the whole, nor for the conclusion that the light from more distant stars was absorbed by the medium through which it was distributed.

The photographic work done in this field testifies to the same conclusion when we are presented with the image of a star which had never been seen. The photographic plate acts cumulatively and if one minute's exposure is not enough, take ten minutes or ten hours, but the eye cannot so act; if one cannot see an object in a second, he can see it no better by continued looking. I conclude, therefore, that we have no evidence that the ether absorbs any of the energy of the ether waves. A. E. DOLBEAR.

Tuft's College, Mass., March 9.

Natural Selection at Fault.

IN your issue of Feb. 17, Mr. Richard Lees replies to the rather misleading article of Mr. J. W. Slater in your issue of Jan. 20, and takes, it appears to me, the right view of the case as regards the Felidæ, but misses it when he attempts to account for the hen's cackle. No one reason will account for the latter. Frequently the hen that is a member of a large barn-yard flock may be observed cackling at the top of her voice prior to the laying of the egg, and it has been my observation that in 9 cases out of 10 this is due to the fact that she has found a usurper in her nest in the person of another hen engaged in egg-laying. Close observation, covering many years, leads me to think that the cackling after the egg is laid has nothing whatever to do with nest-disclosure or nest-hiding, but is simply a notification to the cock of the flock that the important task of the day is accomplished.

There is no time at which the hen is so susceptible to the gallant attentions of her liege lord as just at the end of this cackling period. I have frequently observed this of our barnyard fowls, of guinea hens, both domestic and in the wild state, and of pea-fowls. In my opinion, the cackle is intended to notify the male bird of the Barkiss-like condition of his mate.

As to the case of the cat tribe, it is so common to see a mother cat in the country bring field-mice, young rabbits, moles, or ground squirrels in to her kittens and watch their playful antics with them, that the conclusions arrived at by Mr. Lees are irresistible. This winter an intelligent house-cat, on a farm where I have been studying winter life in field and woods, led me some distance to where several grain-ricks had stood during the fall. I soon saw that she wanted me to turn over the fence-rail floor that still remained there, that she might capture the field-mice living beneath. This I did, while Tabby caught four mice in quick succession. The first one she gulped down at a rapid rate, the second she played with a little while, the third she played with much longer and, half-devoured, left to her eldest son, a full-grown Tom who had accompanied us, and the fourth she barely wounded and also turned over to his tender mercies. In a word, while hunger was a dormant passion, she quickly devoured her prey, after that her instinctive disposition to practise and keep perfect the arts whereby such elusive game is captured was paramount.

Mr. Slater is in error in thinking that a comparatively few now possess the power to "wag the ear." This power is common among the West Indian half-breeds and the Maya and other derivatives of Mexico and Central America, and many whites have the power who hardly realize the fact. It is not uncommon to observe this if one will suddenly say to a companion, "What was that noise?" If Mr. Slater will say this in a semi-startled way, he will notice that in no inconsiderable number of cases there will be a slight instinctive movement of the muscles in question, more or less pronounced. Nor is the ear that Darwin illustrates in his "Descent of Man" as being allied to the pointed type belonging to our Simian relatives as uncommon as many may imagine. It is my observation that this peculiarity of the fold in question is oftenest to be observed in women, and in many of these cases the persistence of the wisdom teeth is also a characteristic. I have in mind two cases of this sort, one of a man, the other of a woman, both residents of one of our leading cities, and their social and intellectual forces. The latter is a remarkable reversion to an earlier type, in ear, in teeth, in length of arm, in painless childbirth, in flexibility of hand-joints, and in other marked characteristics. It appears to me that the ear, like the vermiform appendix, the suspension of the viscera, the position of the orifice to the bladder, and the unprotected condition of certain main arteries, is yet in a transitional state, and not fully adapted to the newer human conditions imposed by the erect position and the artificialities of civilization.

EUGENE MURRAY AARON.

Philadelphia, March 6.

BOOK-REVIEWS.

Die Zukunft des Silbers. By EDUARD SUESS. Vienna and Leipsic, Braumüller. 1892 227 p.

DR. SUESS is eminent as a geologist, and it would be impertinent on the part of the present writer to attempt a criticism, or even an exposition, of his views on the geological and metallurgical conditions which affect the production of the precious metals. Dr. Suess's conclusions are similar to those which he gave to the world some fifteen years ago, in his monograph on the "Future of Gold," published in 1877. He believes that the production of gold is likely to be limited in the future, and will not supply sufficient gold to meet the monetary consumption and the consumption in the arts. He believes also that the production of silver will not progress as rapidly, or that its depreciation will descend as far, as is often supposed. He believes that gold must eventually cease to be used as a standard of value; while the production of silver is likely to continue at a comparatively equable pace, making that metal eventually the basis of the world's money. International bimetallism, even if it were practicable, would be only a half-way measure, paving the way to the ultimate adoption of the single silver standard.

To this line of reasoning, the economist who, like the present writer, believes that the gold standard works to reasonable satisfaction, would answer in some such fashion as this. If it were true that all exchanges were effected by the actual use of coined money, undoubtedly the monetary supply of gold would not suffice. at the present range of prices; and on that supposition the maintenance of the gold standard must be accompanied by a fall in prices, which would in many ways be distressing. But the fact is that in modern communities gold is used but to an insignificent extent as a medium of exchange. The great bulk of the exchanges are effected by credit substitutes of various sorts. Much the most effective of these is the modern machinery of banking, by means of which, especially in countries like the United States and England, an enormous volume of transactions is settled with an insignificant use of coin. So far as retail transactions are concerned, bank notes, government notes, silver as a subsidiary coin, do the greater part of the money work in all civilized communities. Gold, therefore, acts in the main simply as a measure of value or a standard of value; something in terms of which the values of commodities are expressed, and into which all other forms of currency are convertible. It performs its function very largely by being held as a reserve in the great central depositaries, serving simply to sustain and regulate the circulating medium. The evidence does not indicate that the supply of gold is insufficient for this purpose. On the contrary, large accumulations of gold have been made in recent years by civilized countries; by Germany in 1873, by the United States in 1879, by Italy in 1883, by Austria in 1892–3, without causing, in the opinion of the present writer, any appreciable difficulties. It is not impossible that in the distant future the supply of gold will prove insufficient, and that some change may be made by the great civilized countries in their standard of value. But such a change for the visible future is highly improbable. The drift of the time is toward the gold standard in all the great countries; with a constant development and use of credit substitutes, but with gold as the sole basis. So far as we can see into the future, this policy will work no harm, and will conduce greatly to stability and convenience in the circulating medium.

So far as silver is concerned, it is undoubtedly true that the method of occurrence of silver ores makes it probable that each individual find will soon be exhausted. The great bonanzas, of which the Comstock lode was the first in the United States, have soon given out, and the great and rapid increase in the production of silver has been due to successive lucky finds. Geologically speaking, therefore, the enormous increase in production, which has taken place in the last twenty-five years, may be regarded as temporary. But historically speaking, it is impossible to say that these finds will not continue for a period of great length in human history. The hard fact is that the production of silver has increased with extraordinary rapidity in the last twenty years, and that as yet there are no signs of relaxation. If this process continues, the decline in the value of silver cannot be checked. If it ceases, the price of silver in terms of gold is likely, at best, to remain where it now is. In either case, there is no ground for supposing that silver will come to be used on the same terms as gold by civilized nations, still less that it is likely to displace gold, as Dr. Suess predicts.

F. W. TAUSSIG.

Harvard University, Cambridge, Mass.

How to Manage the Dynamo. By S. R. BOTTONE. New York, Macmillan & Co.

THIS little book is meant, as its author tells us, for steam engineers who are called upon to take care of dynamos, without having any previous training or knowledge. As this class is a rather large one, there is no doubt but that there will be a considerable demand.

The book is very clearly written, and contains just about all that the men for whose benefit the author is writing will require to know. There are some sins of omission and commission, however. Among the former may be mentioned the fact that the alternating-current dynamo is not touched upon, or very briefly. No instructions are given as to what should be done in case, when a dynamo arrives, it is found to be connected for running in the opposite way to which the foundations, etc., necessitate its turning. Instructions for reversing the connections of a series machine when it fails to start up or under compounds would also be of use. The writer has had on many occasions to travel several hundred miles to remedy these simple troubles. A warning about the necessity and method of keeping armatures and fields free from moisture when lying boxed up, might also be added with benefit. Among the latter may be mentioned the direction to lay an armature on waste (page 22), as the latter is often full of pieces of iron, etc., which might ruin the insulation. Sand-paper is preferable to emery for polishing of commutators, as the latter frequently contains particles of iron. The remedy proposed on page 30 for a shunt dynamo which will not pick up is impracticable.

Altogether, this is a very useful and clear little book.

R. A. F.

Electrical Experiments. By G. E. BONNEY. New York, Whittaker & Co.

THIS book is a collection of simple experiments with magnets, induction coils, influence machines, and plating baths Mr. Bonney is already favorably known through his books on the making of induction coils, electroplating, etc., and this volume is quite up to the others

Manual of Irrigation Engineering. By H. M. WILSON, C.E. New York, J. Wiley & Sons. 1893. 351 p. 8°. $4.

WHILE text-books and elementary treatises on the general subject are threatening to flood the market and the profession, the promise of an overplus of good treatises on special branches of engineering, by competent specialists, is by no means serious. Good works of the latter class are always welcome to the average practitioner, and this seems to be one of the kind which is likely to prove both useful and welcome. It is written by an author who has had experience in America, Europe, and India, and contains the fruits of both original investigation and discreet compilation. The book is prepared mainly with reference to the needs of the engineer having charge of work of this kind in the western portion of the United States, and includes accounts of the current methods there in use, as well as of such systems observed abroad as are most likely to prove useful in this country. The collection and distribution of water, but not its application to crops and to its minor uses, constitute the subject chosen for treatment. Much new material is here published, especially relating to earth-dams and elevation of water by pumping The author makes application, in a very sensible manner, of the principle, too little recognized by writers on engineering subjects, that. while no hesitation should ever be felt in regard to the use of mathematics in the development of the subject in hand, its use should always be confined to the minimum quantity, and the most elementary methods, consistent with the effective accomplishment of the purpose in view. The average reader, even though a professional and a practitioner, does not purchase his library with the view of admiring the scholarship, the pedantry, or even the genius of authors.

The importance of this subject may be realized when it is stated, as by this writer, that 25,000,000 of acres are made fruitful in India alone by irrigation; in Egypt there are about 6,000,000, and in Europe about 5,000,000 acres. In the United States, where this process of conquering nature has but just begun, are now about 4,000,000 acres of irrigated lands. Thus, about 40,000,000 acres of soil are made to produce crops: land which would otherwise have remained desert.

The book is well and freely illustrated, and its typography is that always seen in the technical works of its publishers. It is

CALENDAR OF SOCIETIES.

Biological Society, Washington.

Mar. 11.—Frank Baker, Recent Discoveries in the Nervous System; Vernon Bailey, The Burrow of the Five-Toed Kangaroo-Rat; E. M. Hasbrouck, The Breeding of the Bald Eagle near Mount Vernon (with exhibition of eggs).

New York Academy of Sciences, Biological Section.

Feb. 13.—A paper on the "Functions of the Internal Ear" was presented by Dr. F. S. Lee, based upon study of dog-fish. The results of experiments were given, showing that the semi-circular canals are sensory organs for dynamical (rotational) equilibrium, otolithic parts for statical (resting) equilibrium. Each canal appreciates movement in its own plane, and by a definite functional combination of canals all possible rotational movements are mediated. This theory explains compensating movements of eyes, fins, and trunk. The method of experiment was that of sectioning the branches of the acoustic nerve and stimulation (by rotational movements) of the swimming fish. In a paper by Bashford Dean, on the Marine Laboratories of Europe, a series of views were shown of the stations of Naples, Banyuls, Roscoff, Plymouth, Arcachon, the Helder, and St. Andrew's. H. F. Osborn described the foot of Artionyx, the new member of the order Ancylopoda Cope. It is distinguished from Chalicotherium by the character of auce and pes, which present a marked resemblance to the Artiodactyla, while Chalicotherium represents these structures as found in Perissodactyla. Both genera are ungulate in aucle joint, but the phalanges terminate in claws, and, in view of the double parallelism between these two forms and the two subdivisions of Ungulates, it was suggested to divide the Ancylopoda into the Artionychia and Perissonychia.

Society of Natural History, Boston.

Mar. 15.—H. C. Ernst, Cultures of a New Pathogenic Bacillus, Illustrating Methods of Isolation; Warren Upham, Deflected Glacial Striæ in Somerville, Notes on the Tertiary Strata of the Fishing Banks Between Cape Cod and Newfoundland.

handy in form and weight, and, in our opinion, will be found useful to all engineers engaged, or likely to be engaged, in any kind of hydraulic work.

AMONG THE PUBLISHERS.

OF the International Education Series published by the Appletons more than twenty volumes have now been issued, one of the latest of which is "Rousseau's Emile," abridged, translated, and annotated by William H. Payne of the University of Nashville. It is not a mere series of extracts, but a judicious condensation, forming a continuous work and giving as much of the original as readers of our time are likely to care for. The important and wide-reaching influence of Rousseau's work has been due in the main to his perception of the grand truth, previously too little regarded, that the child's faculties have a certain natural course of development, and that, if education is to be successful, it must be in harmony with that development. Unfortunately, he knew but little of what that course of development really is, and his practical plans for meeting it were about as inappropriate as they well could have been; and whoever should adopt them would find in the end, if not sooner, that he had followed anything but nature. Rousseau's notions that the child ought not to do anything against his will, that children have and can have no morality, and that all wickedness is weakness, are both false and mischievous; and many others of like character might be cited. Moreover, though a vehement democrat in politics, he would refuse the benefit of education to the poor, expressly saying that "the poor man has no need of an education," and he held that women ought to be educated merely to please men. Mr. Payne is clearly conscious of these faults in Rousseau's work, and sharply animadverts on some of them in the short but very suggestive notes that he has furnished to this volume. Moreover, he does not hesitate to attack some of the educational fads of the time; and his comments add much to the value of the book. Indeed, we think the public would be glad to receive from him an independent work of his own, in which his views might be stated more at large.

— Charles Scribner's Sons have in preparation "How to Know the Wild Flowers," by Mrs. William Starr Dana, with 100 illustration by Marion Satterlee.

— The latest issue in Scribner's series on the Great Educators treats of "Froebel and Education by Self-activity." The author is an Englishman, Mr. H. Courthope Bowen, who is an enthusiastic disciple of Froebel, and has had much practical experience of kindergarten work. We cannot say, however, that his book is a quite satisfactory treatment of its theme, the literary form of it being in some respects defective. There is a good deal of repetition in it, as indeed the author himself admits, and the sentences are often loaded down with parenthetic expressions which make them awkward and sometimes obscure. The first two chapters relate the principal events of Froebel's life, the processes of his own education and his various experiences and experiments as a teacher. Then follows a notice of his philosophy, which, however, Mr. Bowen makes no more intelligible than others have done, and then an exposition of his theory of education. The remainder of the book is devoted to a description of the kindergarten and other contrivances that Froebel designed, with some account of his relation to earlier and later educators, thus giving on the whole as full an exposition of his views and methods as most teachers will desire. As to the value of those methods themselves, we have not space to speak largely; but we cannot help thinking that both Froebel and Pestalozzi are at the present day greatly overrated. Their methods are only adapted to a few years of early childhood, and are not perfect even for that period; while their prejudice against book-learning was little short of barbarous. Nevertheless, whatever is good in their systems we want, and we trust that our teachers will not fail to appropriate it.

Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

For sale—A Zentmayer new model U. S. Army Hospital monocular stand. Price $110, will sell for $75. Address H. C. Wells, No. 151 Broadway, New York.

For sale—A complete set of the third series of the American Journal of Science (1870–1893) handsomely bound in single volumes in dark brown half morocco. Address G. H. Williams, 803 Cathedral Street, Baltimore, Md.

For sale, or for exchange for books on medicine or surgery, new editions only, a large geological library, containing nearly all the State and Government Reports since 1855. Will be pleased to answer letters of inquiry and give information. Address R. ELLSWORTH CALL, Louisville, Ky.

For exchange.—Slides of Indian Territory Loup Fork Tertiary Diatoms for other microscopic fossils. Address S. W. WILLISTON, Univ. of Kansas, Lawrence, Kans.

For exchange.—Will exchange an "Ideal" Microscope of R and J. Beck, London, 2 eye pieces 3 objectives, 3 inch, 1 inch, 1-6 inch; bull's eye condenser on stand, substage condenser, mechanica stage, etc., for any of the leading makes of type writers. Particulars by mail. DELOS FALL, Albion College, Albion, Mich.

Sale, or exchange for similar material; Diatoms (Isthmia nervosa), unmounted, from San Francisco Bay. M. J. ELROD, Bloomington, Ill.

For sale or exchange. — I have a few copies of my translation of "Strasburger's Manual of Vegetable Histology, 1887," now out of print, which I will send post-paid for $3 or for one dozen good slides illustrating plant or animal structure. Address A. B. Hervey, St. Lawrence University, Canton, N. Y.

The undersigned has the following specimens to exchange for crystals of any eastern or foreign localities or Indian relics: tin ore, metacinnabarite, stibnite, garnierite, calenanite, hanksite, ulexite, rubellite, lepidolite, blue and green onyx, Cal. pineite, aragonite on chalcedony, cinnabar, double refracting spar, clear and clouded, and others. J. R. Bush, care of General Delivery, Los Angeles, Cal.

For sale or exchange.—A private cabinet of about 200 species of fossils, well distributed geologically and geographically. Silurian, about 40; Devonian, about 50; Carboniferous, about 80; others, about 30. Frank.S. Aby, State University, Iowa City, Ia.

Wants

WANTED—Second-hand copy Tryon's Systematic Conchology. Please quote condition, date, price. H. L. Osborn, Hamline University, St. Paul, Minn.

A COMPETENT TEACHER of botany in college or university is open to engagement. Address L., Box 86, Rochester, Mich.

CAN any one inform me as to the age to which cats have lived? I have one twenty years old. Edward D. Webb, 132 W. Eighty-first St., New York.

WANTED — Second-hand. Foster's Physiology, Balfour's Comparative Embryology, Claus & Sedgwick's Zoology, Flower's Osteology of Mammalia, Vine's Physiology of Plants. Please state editions and prices asked and address Richard Lees Brampton, Ontario, Canada

WANTED.—American Journal of Conchology, seven volumes. Parties having these for sale will please address the undersigned, stating condition and price. R. Ellsworth Call, Louisville, Ky.

A GRADUATE ENGINEER will give instruction evenings in geometry, trigonometry and surveying, mechanics, physics, mechanical drawing and general engineering construction. Five years' experience in field and editorial work on engineering journal. References furnished. C. S. H., 102 Tribune Building, New York.

WANTED.—By well-qualified and experienced science master and associate of the Royal School of Mines, London, aged 26 (at present in England), a mastership in technical college or university for any of the following subjects: Engineering sciences, geology and mineralogy, physics, chemistry and metallurgy, etc., etc. Can provide excellent references and credentials. Apply, J. G., 17 Sussex St., Rochdale, England.

SCIENCE

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QUERY.

Can any reader of *Science* cite a case of lightning stroke in which the dissipation of a small conductor (one-sixteenth of an inch in diameter, say,) has failed to protect between two horizontal planes passing through its upper and lower ends respectively? Plenty of cases have been found which show that when the conductor is dissipated the building is not injured to the extent explained (for many of these see volumes of Philosophical Transactions at the time when lightning was attracting the attention of the Royal Society), but not an exception is yet known, although this query has been published far and wide among electricians.

First inserted June 19, 1891. No response to date.

N. D. C. HODGES, 874 BROADWAY, N. Y.

SCIENCE

NEW YORK, MARCH 24, 1893.

THE COMPARATIVE METHOD OF STUDY.[1]

BY GEORGE H. JOHNSON, SC.D., ST. LOUIS, MO.

WE are all acquainted with the word evolution as used in natural history to express the doctrine that many different forms of animal and vegetable life have been derived from a common ancestry. The use of the word in a more general sense in astronomy and sociology is almost as well known. Philosophers, historians, and inventors, as well as naturalists, have discovered in the present generation that evolution, defined simply as the theory which recognizes not only the formal and external connection and correspondence of entities and events, but also the causal connection,— so far as that may exist, — is not only true but is also exceedingly useful and fruitful as a method to guide thought and investigation.

If we would make the most rapid progress on the current of modern thought, we must be in the current and know its trend, and to determine its trend we must know both its present condition and the previous conditions through which it passed to reach the present; just as in determining the direction of a river current we must use floats to show the path traced by the surrounding water, and in determining a comet's orbit we must use several positions to compute its future course, so in all subjects which involve human personality and social progress there are certain well-determined positions formerly occupied, which, compared with the present, will indicate the future.

A good historian, for example, is not satisfied with his narrative until he has shown the events in their proper relation to each other. Indeed, the true scholar, the man of profound mind and practical learning, is not he who has apprehended and remembered the greatest number of great and useful facts; it is he who has a systematic, correllated knowledge of facts. Let us consider one or two illustrations.

In some sciences it seems as if human thought moved in a kind of cycle, so that by going back far enough a period or phase in the history of the science or doctrine will be found which is very much like its present condition, or like any other condition which we may be considering. Even back to the time of Solomon, we have his authority for the proverb that there is nothing new under the sun, and if it was true then it must be true more particularly at the present day. This does not teach that there is no real progress in human knowledge, nor that there are no new combinations of public events such as constitute history. But it is evident that all motion and change in human thought, all changes in national life, all movements in literature, art, and even science, are not progressive. Indeed, there is very much intellectual motion to only a little progress. In this respect the human mind may be compared to the limbs of a child. A little girl, when she is excited and hurried, will jump up and down and make very little forward motion, notwithstanding — or rather because of — her eager desire to do so. She wastes a large amount of energy simply in crossing a room. Her movements are not properly co-ordinated; she does not know what muscles to use in order to attain the desired position. How true this is of the intellectual movements of men!

A man in his anxiety to get ahead rapidly in the world, to acquire more money, to get some fame or honor, position or power, will make many senseless and useless actions which do not at all hasten the attainment in view. We all do it more or less; a man would be supremely wise or fortunate who was not occasionally humiliated by discovering that his most carefully laid plans and most deliberate actions had very unforeseen effects. To regard the world as a stage and all the rest of mankind as players is an instructive as well as amusing way of contemplating our fellowmen. To see politicians standing in a high place and reaching forward and upward to grasp more power and influence, and in their eager and irrational motions losing their foothold and falling prostrate, is a spectacle which is constantly before our eyes, at least the reaching is constantly before our eyes and the falling is periodically manifest at election time.

But the same disposition is seen, and similar accidents happen, to men in all walks of life. That is to say, that the child who is learning to walk, and the man who is learning to control himself and other men, go through many motions and actions which are not really helpful for the purpose in view. But these misguided actions are educational in their results, so that if a man lives long enough he is likely to become measurably wise by virtue of his own mistakes.

Men often act with very little sound theory or experience to guide them. In many respects we are like a man blindfolded, who hears some noise but is doubtful from whence it comes; if he has the opportunity to explore the room in all directions he is likely to find the right corner before long, even though his hearing at first directly misleads him. And so in all human knowledge, if we only have opportunities to make enough mistakes we are likely to have ultimately some measure of successful intellectual progress. We need not experiment with what is dangerous, nor carelessly make mistakes, but when we have no sure knowledge to guide us we should learn as much as possible from the experiments made by ourselves and others; thus, proving all things, we may hold fast to that which is good.

Very much of vaunted human progress is at best only up along the arc of a helix, so that there is much motion to only a little rise; and, after making a complete turn or cycle, society is found just where it was before, except that it is a little higher in experience and therefore enjoys a wider horizon and clearer view. As mountain railroads wind in grand loops and horseshoe curves around the valleys and up to the passes, making miles of road to gain a few hundred feet in altitude, so human knowledge moves in a kind of cycle such that it is possible to stop at any point and look back and down at the corresponding point in time past.

Does any heretical doctrine arise in the Church? The expert in ecclesiastical history will have no difficulty in telling us when the same heresy long ago produced a similar dissension, and quite likely he will be able to show that again and again the same doctrine in only slightly different form has been the ground for divisions in the Church. Where several of these recurring phases can be definitely located it might be possible to investigate the law of their recurrence or, as the physicist would say, to investigate the period of the vibration or undulation. The weather prophets, who employ themselves and amuse the public by predicting when we shall have the coldest day of winter and the hottest day of summer, might, by the use of some statistics and mathematics, derive formulas designed to express the periods of moral, social, and intellectual movements.

One reason why such formulas are impracticable is because they would contain an infinite number of terms; that is to say, the time of recurrence of any sociological phenomenon is a function of an infinite number of conditions. But most of these conditions are insignificant compared with a few of the most important ones, and so we could apply to this problem the method of finding approximately the value of an infinite but rapidly converging series.

For example, consider a financial panic, which is never the result of a single cause. At certain times the failure of a great bank is enough to precipitate a widespread and disastrous panic;

[1] Abstract of a paper written for a popular audience.

at other times, when the wave of public confidence is high and strong, such a failure has no perceptible effect beyond those immediately concerned. To predict the time of the next panic it would be necessary first to determine the periodic laws of speculative eras, expansion and contraction of the currency, over production, and other principal causes, and then combine them to find when like phases coincide. Just as two rays of light of opposite phases may by interference annihilate each other, so two social movements or tendencies, both of great power and effect, may, when they enter as terms in the formula of another movement, cancel each other by reason of their opposite signs or phases. On the contrary, all of the greatest movements of the social world, such as the founding and spread of Christianity, the fall of Rome, the Reformation, the colonization of America, and the French Revolution, have been the result of the synchronous combinations of many causes or terms of the same sign and phase, so that the sum of the whole — even if we neglect the infinite number of small terms — is one of transcendent magnitude.

The great advantage of the philosophical study of history is that by this method the constituent elements of events and the movements to which they belong are made apparent, and for this purpose we must be provided with the data for expressing the trend and phase of all the political, philosophical, and religious movements to which they are related. The complexity of the problems involved is indicated by the fact that different scholars arrive at such contradictory conclusions. Nevertheless, it is believed that this modern method of investigation will revolutionize all the social sciences as it has already revolutionized political economy, and that after the method has been more systematically applied to modern statistics, and the number of terms considered has been increased, the conclusions or results reached by different authorities will be less and less discrepant, and that thus we may hope ultimately to reach a certainty and precision, in the social and metaphysical sciences, which will be comparable to the precision of physical data. If we throw a pebble into the air we can express mathematically the motion of the earth toward the pebble as well as the motion of the pebble toward the earth, and we might perhaps express in a similar way the effect which the repair of a roof in San Francisco would have upon the prices of building materials in New York, and *vice versa*. More than this, is it not possible that a new psychology will be able to weigh and measure the volitions, tastes, and emotions of the mind, so that this science as well as history and political economy may become partly quantitative?

The methods of mathematics can be applied to the metaphysical sciences more extensively than has been done heretofore. These methods have already been applied, in a limited way, to all subjects having much statistical data, also to logic. Indeed, mathematical forms and analysis may be used in any science, as chemistry, which is subject to quantitative treatment The qualitative analysis must always precede the quantitative analysis in any science, but most subjects are now so fully developed that it is time for original research to be directed to the quantitative treatment. This is being done in a kind of tentative way at several universities, and it is believed that the comparative, quantitative method of investigation will be as useful in other sciences as it has already proved to be in political economy and philology.

NATURAL SELECTION AND USE-INHERITANCE.

BY VICTOR YARROS, BOSTON, MASS.

EVOLUTIONISTS will be extremely gratified to learn that Mr. Spencer has resumed the discussion of the subject of the factors of organic evolution. Since the publication, several years ago, of Mr. Spencer's controversial essays on this subject, the so-called pure-Darwinians have practically enjoyed a monopoly of the field; and some of the more rash biologists have even allowed themselves to advance the claim that the use inheritance hypothesis was utterly discredited. Mr. Spencer's unsatisfactory state of health, it was understood, necessitated his neglect of this and many other "unsettled problems" and the concentration of his attention on ethical questions,— the part of his synthetic philosophy rightly regarded by all as the crown of the whole. Students of evolution were anxious to hear "the other side," the answers to the formidable objections of Professor Weissman and his disciples or co-believers, and the announcement of Dr. Romanes's "Darwin and After Darwin," a part of which work was to treat elaborately the question of the number and relative importance of the factors of organic evolution, was received with great pleasure. But no one realizes more keenly the transcendent importance of the question of the inheritance of acquired characters than Mr. Spencer, and he is to be congratulated upon the kindness of fortune that has enabled him to spare some time and energy to the further consideration of the subject, use-inheritance *vs.* sexual selection. It will conduce to firmness of grasp and clearness of understanding to quote here certain passages from Mr. Spencer's preface to his "Factors of Organic Evolution."

"Though mental phenomena of many kinds," wrote Mr. Spencer, "and especially of the simpler kinds, are explicable only as resulting from the natural selection of favorable variations; yet there are, I believe, still more numerous mental phenomena, including all those of any considerable complexity, which cannot be explained otherwise than as results of the inheritance of functionally-produced modifications. What theory of psychological evolution is espoused, thus depends on acceptance or rejection of the doctrine that not only in the individual, but in the successions of individuals, use and disuse of parts produce respectively increase and decrease of them.

"Of course there are involved the conceptions we form of the genesis and nature of our higher emotions; and, by implication, the conceptions we form of our moral intuitions. If functionally-produced modifications are inheritable, then the mental associations habitually produced in individuals by experiences of the relations between actions and their consequences, pleasurable or painful, may, in the successions of individuals, generate innate tendencies to like or dislike such actions. But, if not, the genesis of such tendencies is, as we shall see, not satisfactorily explicable.

"That our sociological beliefs must also be profoundly affected by the conclusions we draw on this point, is obvious. If a nation is modified *en masse* by transmission of the effects produced on the natures of its members by those modes of daily activity which its institutions and circumstances involve, then we must infer that such institutions and circumstances mould its members far more rapidly and comprehensively than they do if the sole cause of adaptation to them is the more frequent survival of individuals who happen to have varied in favorable ways."

The above expresses Mr. Spencer's view of the profound importance of the indirect bearings of the purely biological argument upon the factors of organic evolution. Now that we have refreshed our memory on this point, let us proceed to give a brief but careful summary of Mr. Spencer's latest contribution to the controversy, to be found in an article, entitled "On the Inadequacy of Natural Selection," in the *Contemporary Review* for February. We preserve as far as possible Mr. Spencer's style.

Students of psychology are familiar with the experiments of Weber on the sense of touch. He found that different parts of the surface differ widely in their ability to give information concerning the things touched. By actual measurements he showed that the end of the forefinger has thirty times the tactual discriminativeness which the middle of the back has. Between these extremes there are gradations. The inner surfaces of the second joints of the finger can distinguish separateness of positions only half as well as the tip of the forefinger. The innermost joints are still less discriminating, their power being equal to that of the tip of the nose. The palm of the hand and the cheek have alike one fifth of the perceptiveness which the tip of the forefinger has, and the lower part of the forehead has one-half of that possessed by the cheek. The crown of the head is far less discriminating, and the breast still less.

What is the meaning of these differences? How, in the course of evolution, have they been established? If "natural selection" or survival of the fittest is the assigned cause, then it is required to show in what way each of these degrees of endowment has advantaged the possessor to such extent that not infrequently life has been preserved by it. It is reasonable to assume that the parts have not become so widely unlike in perceptiveness without some cause, and, if the cause alleged is natural selection, it becomes necessary to show that the greater degree of the power possessed by this part than by that has conduced so much to the maintenance of life that an individual in whom a variation had produced better adjustment to needs, thereby maintained life when some others lost it, and that among the descendants inheriting this variation there was a derived advantage such as enabled them to multiply more than the descendants of individuals not possessing it. Can anything like this be shown?

That the superior perceptiveness of the forefinger-tip has thus arisen, might be contended with some apparent reason, as such perceptiveness is an important aid to manipulation. But how about the back of the trunk and its face, or the tip of the nose, or the thigh? The survival of the fittest cannot explain these differences of perceptiveness. But if there has been in operation a cause which it is now the fashion to deny, the various differences are at once accounted for. This cause is the transmission of inherited traits or characters.

(Here Mr. Spencer records some experiments which show that constant exercise of the tactual nervous structures leads to further development, to greater discriminativeness. The perceptiveness of the finger-ends of the blind who read from raised letters and of compositors is greater than that of the finger-ends of other people.)

Now, if acquired structural traits are inheritable, the gradations in tactual perceptiveness are the result of the gradations in the tactual exercises of the parts. The trunk has but little converse with external bodies, and it has but small discriminative power; what power it has is greater on its face than on its back, corresponding to the fact that the chest and abdomen are more frequently explored by the hands, this difference being probably in part inherited from inferior creatures. The middle of the forearm and the middle of the thigh are obtuse, having rare experience of irregular foreign bodies. The tip of the nose has considerable tactual experience, hence its greater perceptiveness. The inner surfaces of the hands are more constantly occupied in touching than are the back of the hand, breast, forearm, forehead, while the tips of the fingers come into play not only when things are grasped, but when things are felt at or manipulated. If then it be that the extra perceptiveness acquired from extra tactual activities, as in a compositor, is inheritable, the gradations of tactual perceptiveness are explained.

The tip of the tongue exceeds all other parts in power of tactual discrimination; why such perceptiveness? Its functions of moving food during mastication and of making many of the articulations constituting speech, are not materially aided by extreme perceptiveness, and natural selection cannot have caused it. But assume inheritance of acquired traits, and there is no difficulty, for the tongue-tip has, above all other parts of the body, increasing experiences of small irregularities of surface. It is in contact with the teeth, and either consciously or unconsciously is continually exploring them. There is hardly a moment in which impressions of adjacent but different portions are not being yielded to it by either the surfaces of the teeth or their edges. No advantage is gained; it is simply that the tongue's position renders perpetual exploration almost inevitable; and by perpetual exploration is developed this unique power of discrimination.

Thus the law holds throughout, from this highest degree of perceptiveness of the tongue-tip to its lowest degree on the back of the trunk; and no other explanation of the facts seems possible.

But some biologists might contend that *panmixia* affords an adequate explanation of the facts. So Mr. Spencer, after pointing out that the explanation by *panmixia* implies that these gradations of perceptiveness have been arrived at by the dwindling of nervous structures, and hence makes an unproved and improbable assumption the basis of the argument, proceeds to establish that, even with this objection passed over, it may with certainty be denied that *panmixia* can furnish an explanation. As this part of the essay is left unfinished, it would be unwise to attempt an abstract of the Spencerian criticism of the *panmixia* explanation. We shall return to the subject as soon as Mr. Spencer brings his argument to a close.

FEEDING-LINES OF A LIVING LAND GASTEROPOD ON LICHENED SLATE.

BY J. B. WOODWORTH, SOMERVILLE, MASS.

IN searching for fossils in the Carboniferous rocks of Attleboro, Mass., about three years ago, I found on the surface of a vertical stratum of micaceous slaty sandstone, in an old quarry, what at first glance appeared to be annelid trails resembling the form known as *Nereites* common in the Silurian. Further examination showed me at once, however, that these markings were caused by the gnawing away of a drab-colored crust of lichens and dust which concealed the real appearance of the rock. The trails were in the form of bands about one-quarter of an inch wide, wandering over the surface of the outcrop, or curved back and forth on each other, so as to approach but rarely cross. These bands or trails were made up of a series of crescentic cross-markings united alternately right and left with the next adjacent in the series so as to form a continuous, closely pressed, sigmoid line, which in itself constituted the whole of the trail. The trail was evidently the feeding-line of some animal. Another occurrence which I have more recently observed in Bristol County, Mass., exhibited a trace of slime along the feeding-line, such as is left by slugs or land snails, thus showing that the feeder was probably a gasteropod.

Ebenezer Emmons, in the *Agriculture of New York*, Vol. I., 1846, p. 68, describes a trail found upon the surface of the fine green slate of Salem, Washington Co., N. Y., included in his "Taconic System," to which he gave the name *Nemapodia tenuissima*. The figure of this trail on pl. 14. fig. 1, of that work, agrees closely with the Attleboro trails. In an explanatory note, p. 365, Emmons states that this trail has been shown, he thinks, satisfactorily by his friend Dr. Fitch, "to be formed by some living unknown animal." It seems to me highly probable that the trail observed by Emmons, and shown to be not a fossil by his friend Dr. Fitch, was also that of a gasteropod. Conchologists may be familiar with the animal which makes these tracks, if I am right in thinking that they are made by gasteropods at all. As yet I have been unable to catch the animal at its work.

NOTE ON THE GENERIC NAME CHIROTES.

BY LEONHARD STEJNEGER, CURATOR DEPT. REPT. AND BATR., U. S. NAT. MUSEUM, WASHINGTON, D.C.

THE application of the law of priority necessitates the abolition of Cuvier's name *Chirotes* for the "Two-handed Ground Worm." No less than three generic names, formally proposed and diagnosed, have priority over *Chirotes*, none of which is preoccupied, and which in turn would have to be adopted, should any of the older ones for some reason become unavailable.

Bonnaterre seems to have been the first to give a Latin name to La Cepede's Cannelée, and to recognize its generic distinctness. However, by sheer carelessness he neglected to do so and a solitary "*B*" stands for the generic name he intended to impose. It may be assumed that he meant to call it *Bipes*, but we have nothing to do with assumptions. At the same time he included as the second species of his intended genus, Pallas's *Lacerta apus*, under the name *B sheltopusik*.

Latreille, however, saw the incongruity of uniting the two in the same genus, and expressly restricted[1] the name *Bipes* to the *B. canaliculatus*. The genus was thus formally established, named, diagnosed and restricted in 1802 as *Bipes*. Bonnaterre's other species he made a separate genus, *Sheltopusik*,[2] renaming Pallas's species *Sheltopusik didactylus*.[3] The latter will therefore stand as *Sheltopusik apus* (Pall.). It will be observed that this

[1] "Nous ne connoissons encore qu'une seule espèce bien distincte de ce genre."

[2] Latreille, Hist. Nat. Rept., II., 1802, p. 271.

[3] Latreille, tom. cit., p. 273.

generic appellation antedates Daudin's *Ophisaurus*, of the type which is our glass-snake, *O. ventralis*. Boulenger regards both species as congeneric, and if he respects the law of priority he will have to call the latter *Sheltopusik ventralis*. It is hardly probable, however, that any American herpetologist will follow him in placing the Eurasiatic species with two posterior legs in the same genus as the totally legless North American species.

According to the above there can be no doubt but that the generic name for the *Chirotes* must stand as *Bipes*, and the family name will, accordingly, be *Bipedidæ*. The synonomy of the genus is as follows:—

Bipes LATREILLE.

1802.—*Bipes* LATREILLE, Hist. Nat. Rept, II., p. 90 (type *B. canaliculatus*).

1804.—*Microdipus* HERMANN, Obs. Zoöl, p. 289 (same type).

1811.—*Bimanus* OPPEL, Ord. Rept., p. 45 (same type).

1817.—*Chirotes* CUVIER, Regne Anim., 1st ed., II., p. 57 (same type).

Species: *Bipes canaliculatus* Bonnaterre.

THE RAVAGES OF BOOK WORMS.

AT a meeting of the Massachusetts Historical Society, held Feb. 9, 1893, Dr. Samuel A. Green, after showing two volumes that had been completely riddled by the ravages of insects, as well as some specimens of the animals in various stages, made the following remarks:—

For a long period of years I have been looking for living specimens of the so called "book-worm," of which traces are occasionally found in old volumes; and I was expecting to find an invertebrate animal of the class of annelides. In this library at the present time there are books perforated with clean-cut holes opening into sinuous cavities, which usually run up the back of the volumes, and sometimes perforate the leather covers and the body of the book; but I have never detected the live culprit that does the mischief. For the most part the injury is confined to such as are bound in leather, and the ravages of the insect appear to depend on its hunger. The external orifices look like so many shot-holes, but the channels are anything but straight. From a long examination of the subject I am inclined to think that all the damage was done before the library came to this site in the spring of 1833. At all events, there is no reason to suppose that any of the mischief has been caused during the last fifty years. Perhaps the furnace heat dries up the moisture which is a requisite condition for the life and propagation of the little animal.

Nearly two years ago I received a parcel of books from Florida, of which some were infested with vermin, and more or less perforated in the manner I have described. It occurred to me that they would make a good breeding-farm and experiment station for learning the habits of the insect; and I accordingly sent several of the volumes to my friend Mr. Samuel Garman, who is connected with the Museum of Comparative Zoölogy at Cambridge, for his care and observation. From him I learn that the principal offender is an animal known popularly as the Buffalo Bug, though he is helped in his work by kindred spirits, not allied to him according to the rules of natural history. Mr. Garman's letter gives the result of his labors so fully as to leave nothing to be desired, and is as follows:—

MUSEUM OF COMPARATIVE ZOÖLOGY, CAMBRIDGE, MASS.,
Feb. 7, 1893.

DR. SAMUEL A. GREEN, BOSTON, MASS.

Sir:—The infested books sent for examination to this Museum, through the kindness of Mr. George E. Littlefield, were received July 15,1891. They were inspected and, containing individuals of a couple of species of living insects, were at once enclosed in glass for further developments. A year afterward live specimens of both kinds were still at work. Besides those that reached us alive, a third species had left traces of former presence in a number of empty egg-cases.

Five of the volumes were bound in cloth. On these the principal damage appeared at the edges, which were eaten away and disfigured by large burrows extending inward. Two volumes were bound in leather. The edges of these were not so much disturbed; but numerous perforations, somewhat like shot-holes externally, passed through the leather, enlarging and ramifying in the interior. As if made by smaller insects, the sides of these holes were neater and cleaner cuttings than those in the burrows on the edges of the other volumes.

The insects were all identified as well-known enemies of libraries, cabinets, and wardrobes. One of them is a species of what are commonly designated "fish bugs," "silver fish," "bristle tails," etc. By entomologists they are called *Lepisma;* the species in hand is probably *Lepisma saccharina*. It is a small, elongate, silvery, very active creature, frequently discovered under objects, or between the leaves of books, whence it escapes by its extraordinary quickness of movement. Paste and the sizing or enamel of some kinds of paper are very attractive to it. In some cases it eats off the entire surface of the sheet, including the ink, without making perforations; in others the leaves are completely destroyed. The last specimen of this insect in these books was killed Feb. 5, 1893, which proves the species to be sufficiently at home in this latitude.

The second of the three is one of the "Buffalo Bugs," or "Carpet Bugs," so called; not really bugs, but beetles. The species before us is the *Anthrenus varius* of scientists, very common in Boston and Cambridge, as in other portions of the temperate regions and the tropics. Very likely the "shot-holes" in the leather-bound volumes are of its making, though it may have been aided in the deeper and larger chambers by one or both of the others. The damage done by this insect in the house, museum, and library is too well known to call for further comment. Living individuals were taken from the books nearly a year after they were isolated.

The third species had disappeared before the arrival of the books, leaving only its burrows, excrement, and empty egg-cases, which, however, leave no doubt of the identity of the animal with one of the cockroaches, possibly the species *Blatta Australasiæ*. The cases agree in size with those of *Blatta Americana*, but have thirteen impressions on each side, as if the number of eggs were twenty-six. The ravages of the cockroaches are greatest in the tropics, but some of the species range through the temperate zones and even northward. An extract from Westwood and Drury will serve to indicate the character of their work:—

"They devour all kinds of victuals, dressed and undressed, and damage all sorts of clothing, leather, books, paper, etc., which, if they do not destroy, at least they soil, as they frequently deposit a drop of their excrement where they settle. They swarm by myriads in old houses, making every part filthy beyond description. They have also the power of making a noise like a sharp knocking with the knuckle upon the wainscoting; *Blatta gigantea* being thence known to the West Indies by the name of drummer; and this they keep up, replying to each other, throughout the night; moreover, they attack sleeping persons, and will even eat the extremities of the dead."

This quotation makes it appear that authors as well as books are endangered by this outlaw. With energies exclusively turned against properly selected examples of both, what a world of good it might do mankind! The discrimination lacking, the insect must be treated as a common enemy. As a bane for "silver fish" and cockroaches, pyrethrum insect powder is said to be effectual. For a number of years I have used, on lepisma and roach, a mixture containing phosphorus, "The Infallible Water Bug and Roach Exterminator," made by Barnard & Co., 7 Temple Place, Boston, and, without other interest in advertising the compound, have found it entirely satisfactory in its effects. Bisulphide carbon, evaporated in closed boxes or cases containing the infested articles, is used to do away with the "Buffalo Bugs." Very respectfully yours,

SAMUEL GARMAN.

MR. FREDERICK VERNON COVILLE has been appointed botanist to the Department of Agriculture in place of Dr. George Vasey, deceased. Mr. Coville has been for some years past one of Dr. Vasey's assistants.

CURRENT NOTES ON ANTHROPOLOGY.—XXV.

[*Edited by D. G. Brinton, M.D., LL.D.*]

Points in Chinese Ethnology.

DR. GUSTAVE SCHLEGEL, who is Professor of Chinese Literature in the University of Leyden, has undertaken to resolve a series of problems relating to the identification of various mysterious peoples mentioned by the early Chinese historians. They have more than special interest, because they bear on the question of the peopling of America from Asiatic sources.

As early as 500 A.D., there is a description of a tattooed people, *Wen-chin*, living 7000 *li* northeast of Japan. Dr. Schlegel identifies them with the inhabitants of Ouroup, one of the Kurile islands; but adds that, in historic times, every tribe from the island of Yezo to Greenland had the habit of tattooing, except the Ghiliaks and Itulmens of Kamschatka. He would also place the "Land of Women," *Niu-kouo*, said to be 1,000 *li* east of Fusang, somewhere in the southern portion of the Kurile Archipelago. In an article on "The Land of Little Men," he maintains the important thesis that the Tungusic stock at one time occupied the whole of the Japanese archipelago. Professor Schlegel's essays may be had of E. J. Brill, publisher, Leyden.

An interesting study of Chinese gilds recently appeared in the *Yale Review*, from the pen of the accomplished sinologue, Mr. Frederick Wells Williams. Within a few pages he exemplifies the great extent and completeness of the gild system among the Chinese, and illustrates the singular similarity of their laws to those of mediæval and modern trades unions in Europe and America.

Progress in American Archæology.

The science of American archæology and ethnology owes a large and increasing debt to Professor F. W. Putnam, who represents those branches in the faculty of Harvard University, and who is also Curator of the Peabody Museum, and Chief of the Department of Ethnology and Archæology of the World's Columbian Exposition. All these posts he fills admirably, as any one will see who will read his Report of the Peabody Museum for 1892, just issued. One fact will be sufficient: that within the last two years he has engaged, trained, and sent into the field — and the field means the whole American continent, from Greenland to Tierra del Fuego — about one hundred assistants and students, actively interested in collecting archæological and ethnological material. He says with pardonable pride and entire justice: "Never before has such an extensive field of anthropological research been covered in two years' time." A brief reference to the results obtained is included in the report. Naturally, the exploration of the wonderful ruins of Copan, Honduras, is most prominently alluded to. In connection therewith Professor Putnam cannot refrain from a mild indulgence in his favorite *manie*, hinting at the discovery of "several facts pointing to Asiatic arts and customs as the origin of those of the early peoples of Central America." (Shade of Brasseur de Bourbourg!)

With like enthusiasm, though on a less scale, the Department of Archæology of the University of Pennsylvania sent several explorers to the field in 1892, and has added largely to its collections by their efforts; while the National Museum, the Bureau of Ethnology, and the Smithsonian Institution will show in time by their reports that the year was also singularly fruitful for them.

The Study of Hair.

A note which I inserted in *Science*, Nov. 4, on this subject led Mr. Mott, F.R.G.S., to send me a reprint of an article which he read some time ago before the Leicester Literary and Philosophical Society. The position he maintains has at any rate the merit of novelty. Arguing (not quite correctly) that the highest races of man are the hairiest, he maintains that this is the result of natural selection; that, therefore, these hairiest types will increase, while the more naked forms will be eliminated; "until in a few centuries men and women will be clothed with natural garments of fine soft fur"; and the occupation will be gone of both Parisian milliners and the "old clo' man!"

More practical are the observations, in the last number of the Zeitschrift für Ethnologie, on the prevalence of moustaches in women, by Dr. S. Weissenberg. He was struck with their frequency in Constantinople, and on several occasions counted the number of visibly hirsute upper lips on the women between eighteen and fifty years of age whom he met in the streets. He found it to be about ten per cent of the total number, which he justly claims is a high rate. On reading his article, I made similar observations in the streets and stores of Philadelphia, and found the moustached women between the ages mentioned to be less than three per cent; but I attribute little value to this statement; for I happen to know that the depilatory "Rusma" has an active sale in the drug stores, and that more than one physician makes a profitable little specialty of destroying unsightly hairs by electro-puncture. These considerations interfere with ethnographic observations. I have noticed more moustached women in Madrid than in any other city. Dr. Weissenberg believes the Armenian women present this peculiarity most frequently. The Madridlenas will surely push them close. In negresses and mulattoes it seems quite absent.

Recent Craniological Studies.

Professor Sergi, of the University of Rome, has lately published two applications of his "taxinomic" method in craniology; the one to a collection of 400 skulls from Melanesia, the second to a smaller assortment from Sardinia. In the former, he begins by speaking of the "chaos" in the descriptions of the "Crania Ethnica" of De Quatrefages and Hamy, averring that nothing less than the thread of Ariadne could guide one in such a labyrinth. He then describes his own "method," and very prudently gives a vocabulary of the astonishing Greek descriptive terms which he has coined, a few of which were mentioned in *Science*, Feb. 24, 1893. The result of his study he claims to be the annihilation of the Papuas as a race, and he demands that the name be stricken from the ethnographic lexicon, as the alleged Papuas are a compound of many varieties, not confined to Melanesia, as the term is generally understood, but extending over Australia, many islands of Polynesia and Micronesia, and even to the Andaman Islands. A few only of these varieties are localized, as, for instance, the hypsistenoclitobrachymetopus stenocrotaphicus neocaledonensis!!

Applying his method to skulls from Sicily and Sardinia, from a moderate number Professor Sergi defines thirteen varieties in the former isle, and eight in the latter, and intimates that this does not at all exhaust the types. He believes that by noting such types, "we can follow the migrations and diffusion of the varieties which have peopled Italy, and resolve many problems in anthropology and ethnology hitherto obscure." He regards the long narrow cranium as that most ancient in the two islands, and it is a form still common among the inhabitants.

However much we may admire Professor Sergi's enthusiasm and the nicety of his observations, it must appear evident to the unbiased observer that his results are open to serious questionings. I find that in any collection of skulls, whether from Melanesia, Sicily, or Sardinia, he discovers by his "method" a new type in at least every twenty; he adduces no evidence to show that these "types" correspond to any ethnic distinction, whether physical or psychical; he makes no effort to show negatively that these various types are not from children of the same parents and same lineage; nor that the same types may not be found in perfection among races the most distant and of no ethnic relationship. I am sure that some of the types he describes are as truly American as they are Sicilian or Melanesian. The conclusions arrived at by such reasoning are, I submit, like those of other authorities which he himself stigmatizes as "not merely incorrect but misleading."

An interesting point in the anatomy of the skull is discussed in a late number (July, 1892) of the Proceedings of the Berlin Anthropological Society. It is in reference to the frequency of that enlargement of the palate known as the "torus palatinus." It is present in three-fourths of the Slavic Poles, in about one-half of the Sibiric tribes, in about one-fourth of the American Indians and Europeans of Aryan race; while it is quite absent among Jews and Gypsies. What its ethnic significance is, if it has any, remains for future investigators to determine.

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THE EVOLUTION AND USE OF THE AFTERSHAFT IN BIRDS.

BY HUBERT LYMAN CLARK, PITTSBURGH, PA.

THE presence of an aftershaft on the contour-feathers of the body has long been recognized as a taxonomic character of some value in the classification of birds, but little, if anything, has been published regarding its history or use. Admitting that every part of the body either has some function, the exercise of which has tended to preserve and strengthen it, or else that, if functionless, it is gradually being atrophied, it is necessary, before the evolution of any organ can be followed through all its stages, to first discover if it has any function, and, if so, what it is. The probable history of the aftershaft can best be traced in this way, and so the first point to be settled is the question of its use. The primary function of feathers is the retaining of heat, and, while forming a non-conducting covering, it is still essential that the weight be as little as possible. As the feathers differentiated other functions, some becoming long and stiff for flight and steering and some taking on new shapes and colors simply for ornament, every change which made the coat of contour-feathers more compact was a distinct gain to the bird. If any feather with a well-developed aftershaft be examined, it will be seen that the smaller shaft lies exactly underneath the larger and its vanes are closely appressed to those of the main feather, thus practically doubling its thickness and increasing its heat-retaining power with the least possible loss of compactness. Now, on the other hand, if a feather is examined which has only a very small aftershaft, the latter is not closely appressed to the main shaft and adds almost nothing to its thickness or warmth. Exceptions will, of course, be easily found to these rules, but the fact remains that where the aftershaft is vigorous it gives plain evidence of adding warmth to the plumage, while it is clear that it can have no secondary function of ornament or locomotion. Another reason for believing that the aftershaft is functionally of no importance, except when an assistance to greater warmth, is found by examining the list of birds which lack it. They are as follows:—

1. Some Ratitæ (Ostriches, Rheas, Apteryx?).
2. Diomedinæ (Albatrosses).
3. Steganopodes (Gannets, Pelicans, Cormorants, etc).
4. Lamellirostres (Ducks, Swans, Geese), except Flamingoes and some Ducks.
5. Columbæ (Pigeons).
6. Cathartidæ (American Vultures).
7. Striges (Owls).
8. Pandionidæ (Ospreys).
9. Cuculidæ (Cuckoos).
10. Alcedinidæ (Kingfishers).

Since there are known to science to day about ten thousand species of birds, only one-tenth of which are included in the above list, the absence of an aftershaft may certainly be considered exceptional. If it is functionally of any importance, why should it be wanting in the albatross, though present in the petrel? Or wanting in many ducks and present in others? And indeed to assign it any function common to all birds except to those in the above list, while wanting in all of them, will be readily found impossible. That it may be the cause of greater warmth receives a proof of negative value from our knowledge that, while very large in all the other Ratitæ, it is totally wanting in those species which inhabit the open plains and deserts of the tropics, where it is not desirable to retain too much heat. Still further evidence appears in the facts that all of the birds which lack an aftershaft (except owls and pigeons) are supplied with a thick coat of down beneath the contour-feathers, and all of the groups except Striges, Lamellirostres, and a few Steganopodes are most largely represented in the tropics and warmer temperate countries. While much of this evidence is very general, some of it purely negative, it seems undoubtedly true that the aftershaft, when not serving as an additional heat-retainer, is wholly functionless.

The conclusion is now unavoidable that the aftershaft, if functionless, must, according to our original proposition, be undergoing a process of gradual atrophy. That such is the case admits of little doubt. It must, however, be kept in mind that its possible function as a heat-retainer is admitted, and in cases where this function has been sufficiently exercised, atrophy, if ever begun, has been stopped. Illustrations of this may be found all through the class, but two will be sufficient to show the point. In the Casuaridæ (Cassowaries) the aftershaft is of equal size with the main shaft, and its function is undoubtedly the same. It is practically a second feather, and, since compactness of plumage is of no advantage to non-flying birds, it has continued to exercise its function, and atrophy has never begun. In the Gallinæ, one of the oldest and most generalized groups, where compactness of plumage is very desirable, not only because it creates less friction in flight, but also because, being essentially ground birds, they are greatly exposed to cold and damp, the aftershaft is large and thick, but entirely different from the Cassowary's. Increasing compactness of the plumage has greatly modified it, but atrophy has not occurred because it still exercises to an important degree its function as a heat-retainer. In the Passeres, on the other hand, the condition of the aftershaft shows evident loss of function and consequent atrophy, being very small and weak. Perhaps in no better way can the degeneration of the aftershaft consequent on its loss of function be proven than by an examination of the feathers of the wing. As is well known, the chief function of the primaries and secondaries is no longer heat-retaining but locomotive, and they entirely lack an aftershaft in all flying birds. But this change of function has undoubtedly been brought about gradually, and on the elbow of the wing are several feathers, very slightly different from the contour-feathers, which grade by almost imperceptible differences into the fully-developed secondaries. If these feathers are examined in any bird with aftershafted plumage, as, for example, the ruffed grouse (Bonasa umbellus), a very evident aftershaft will be found on the smallest ones, but decreasing rapidly in size as the main shafts become flight-feathers, until, on the true secondaries, they are either wholly wanting or represented only by a slight meeting of the vanes.

It will be noticed that throughout the preceding argument, the assumption has been made that the aftershaft is a degenerated and not a recently evolved part of the feather. That is, that it was originally characteristic of feathers in general and its condition as known to us is worse than formerly, rather than that it is an acquired character, which never occurred where it is now wanting. The truth of this assumption must now be proven, or the foregoing statements are meaningless. The first reason to be offered for believing it to be a primitive part of the feather is found in the facts of its occurrence and development in the different orders of birds. In the Casuaridæ, which is admitted to be one of the very oldest families of modern birds, we find a very large aftershaft, indeed, as already said, it is practically one-half of the feather. In the other Ratitæ, in which it is wholly wanting, local causes, such as excessive heat, have destroyed its usefulness, and its loss is easily explained. Large aftershafts are also found in Opisthocomus, the Gallinæ, and the Penquins, all old and little specialized groups; while, on the other hand, in the most highly organized birds, as the Passeres, the aftershaft is very weak, and in many peculiarly specialized birds, as the owls, American vultures, ospreys, and kingfishers, it is wholly wanting. The second reason for considering it primitive is the process of its development during the formation of the feather. It is needless to republish here the history of a feather's growth, but it may be well to call attention to one or two points. When the malpighian layer covers the feather-papilla, it would naturally be thinnest on the sides. The increased thickness above and below would cause greater pressure on the papilla along the median line on both surfaces, thus causing the grooves in which the rhachis and hyporhachis subsequently develop. Now, it is known that both these grooves occur in those feathers which have an aftershaft, and it is much more probable that, though now the upper groove is the larger, they were originally of equal size, than that the lower groove is a secondary development; because it is difficult to assign any possible reason for its ever beginning at all as a secondary characteristic. The foregoing facts give warrant to the following theory of the evolution and subsequent degeneration of the aftershaft.

Paleontology shows us that flight was an accomplished fact long before birds were evolved, and, since it requires tremendous muscular energy, it would be an obvious advantage to the hypothetical avian ancestor to decrease his weight and, at the same time, increase the non-conductability of his covering. When, therefore, feathers were first evolved from scales, the object in view was increase of heat-retaining power combined with decrease of weight. The most natural way of improving scales in this direction would be to make them thicker and, at the same time, hollow, and continued development along this line would result in making them more or less quill like. Then by dividing longitudinally and at right-angles to the axis of the body the number would be doubled without taking up any more space on the body, an obvious advantage. Constant subdivision, making them more adjustible, more coherent, and more compact, would finally bring about a condition very similar to that of the down-feathers of many birds especially in the first plumage. From this condition it is not difficult to trace the gradual development into a contour-feather in which shaft and aftershaft are of equal size, such a condition, in fact, as we find in the Cassowaries. But in this condition the feathers cause far too great friction with the air to admit of rapid flight, and so there came about the natural evolution of the more coherent, pennaceous feather with its comparatively smooth surface. But the natural curve of the lower half of this primitive feather was up and outwards and in direct antagonism to the down and inward curve of the main shaft, and so, being a hindrance to the required compactness, it gradually gave way and degenerated to its present condition. The rest of the story has already been told; how, where the aftershaft has adapted itself to its sole function as heat-retainer, it is still strong and useful, but in all other cases it is either wholly lost or on the rapid road thereto. Whether subsequent investigations and discoveries in paleontology and histology confirm this theory remains to be seen, but, for the present, it is at least plausible and open to few objections.

BRITISH STONE CIRCLES.[1]

BY A. L. LEWIS, LONDON, ENGLAND.

No. 1. — Abury.

THE largest circle of stones in the world was that of which the remains — few when compared with the magnitude of the structure of which they formed part, but by no means inconsiderable in themselves — are to be seen at Abury, in Wiltshire. Abury village is six miles from Marlborough station (Great Western Railway); it occupies the site of the circles and is mainly built of fragments of the stones which composed them. The monument when complete consisted of a circle of one hundred stones (more or less), of which thirteen still remain above ground and at least sixteen more are buried, some of these are of great size, more bulky than any at Stonehenge, but unshaped and without the cross-pieces which distinguish the latter monument from all other circles. The diameter of this circle was about eleven hundred feet, or eleven times that of the outer circle at Stonehenge; inside it were two other circles, — north and south, — both over three hundred feet in diameter. Dr. Stukeley considered that there was a smaller circle inside each of these, but there is now nothing remaining of them, and it has been doubted whether they ever existed. In the centre of the northern inner circle there were three very large upright stones, forming a "cove" or three sides of a square, of which the open side was toward the northeast, and of these stones two still remain, besides which there are now only three stones of the northern inner circle or circles and five of the southern, and a single stone, which Stukeley said stood in the middle of the latter, has long since disappeared. The total number of stones composing the inner circles, "cove," etc., was, according to Stukeley, eighty-nine.

The circles (and the greater part of the village) are surrounded by a deep ditch, outside which is a high embankment. Aubrey, the first writer who noticed this monument, made a very imperfect plan of it in 1663, in which he represented an avenue of stones leading down in a straight line to the present main road, near the River Kennet, and another avenue of stones leading from the end of it, also in a straight line, but at a right-angle, to a smaller circle on Overton Hill, near the line of large barrows which crosses the main road from Marlborough before it reaches the point where the road to Avebury leaves it. Stukeley delineated these as one avenue running in a curved line about a mile long between the great circles at Abury and the smaller one on Overton Hill, and thought that it represented a serpent, of which the Overton Hill circle formed the head, and the Abury circles some convolutions of the body, the tail being represented by another avenue, which left the great circles near where the church now stands, and curved away to the left, passing two large stones called the "long stones," which are still to be seen,[2] though of the rest of the alleged second avenue nothing remains *in situ*, so that some archæologists think it never existed, especially as Aubrey, who visited the circles more than fifty years before Stukeley, has not left any notice of it. Stukeley, however, spent much more time at Abury than Aubrey did, and obtained much information from the inhabitants as to the former position of stones which had been destroyed within their remembrance, and, as there is much stone used in causeways, etc., over the marshy ground on that side of Abury, it is probable that an avenue of some sort did formerly exist there, but this a point for the visitor to investigate for himself.

The circle on Overton Hill and the end of the avenue adjoining it were destroyed before Stukeley went to Abury, but there are several stones of the other part of the avenue standing and fallen by the side of the road which leads from the main road at West Kennet to Abury village, and in a meadow under the left-hand hedge of the main road there are four fallen stones of the avenue, and, as these follow the curve which the road makes between the barrows and the turn to Abury, they seem to show that Stukeley was right in delineating a single curved avenue in place of the two, meeting at right angles, which Aubrey shows in his plan. This is another point for the visitor to verify, and he will do well to follow the avenue from these four stones to its junction with the circles at Abury, and, having inspected the latter, to go out past the church to the "long stones," and to the Beckhampton Inn, which is on the main road by which he will return to Marlborough, stopping on his way to climb Silbury Hill, the largest artificial mound in Europe. This attracts attention by its regular shape and flattened top, and, as it is due south from the circles at Abury, probably formed part of the monument; it has been dug into, but nothing has been found to show it to be a sepulchral mound, like the smaller barrows which are so numerous in this district. Human remains were found round the Overton circle, but none are known to have been found at Abury, so that it does not appear that the object of these circles was, as some suppose,

[1] It has been thought that many Americans who, when in England, visit Stonehenge may not be aware how many remains of a similar character, which they might also wish to inspect, exist in the British Isles; and the editor of *Science* has accordingly made arrangements for a series of short articles, which shall give a description of each of the principal circles and state what points should be noted and how it may most easily be visited.

[2] These are probably the last survivors of another large circle.

the commemoration of the dead; but the fact that the "cove," or holy of holies, in the cêntre of the northern circle, faced the sun when rising at midsummer has been regarded as indicating sun-worship to have been the chief purpose of this vast monument, which was in all respects so suitable for a place of assembly for a tribe or nation.

A short distance to the north of the main road from Marlborough to Abury are the remains of a dolmen called the "Devil's Den," and there is another at Rockwell, four miles northwest from Marlborough and two miles northeast from Abury. There was also a circle at Winterbourne Basset, four miles north from Abury, but it is not worth the trouble of a visit, as only three or four stones remain.

PHYSIOLOGICAL CONTRIBUTIONS FROM MISSOURI BOTANICAL GARDEN. I.

BY J. CHRISTIAN BAY, MISSOURI BOTANICAL GARDEN, ST. LOUIS, MO.

THE PLANT CELL.

IN the early part of this year, Professor von Sachs, of Wuerzburg, published a paper on the theory of cells: Beitraege zur Zellentheorie in "Flora," 1892, Heft 1, pp. 57–64. The leading thought of this publication seems to me to form, when combined with the following suggestions, the key and basis for deductions from the very long and interesting series of facts which forms the results of investigations of the later years in the functions of vegetable cells, both mechanical and physiological.

It is not difficult to trace how, even since the epoch of natural philosophy ("die Naturphilosophie"), the science of vegetable physiology has been in want of a solid foundation, a base, upon which the results of investigations in the phenomena of the life of vegetable cells could be firmly built. In the *Botanische Zeitung* a lance was in vain broken for the old theory; somebody then in vain put out the question, what Schleiden would give us instead of the old natural philosophy. Schleiden made no answer, because he had none to give.

The physiology of the plant cell having had since that time no leading exponent is, I suppose, the reason why at present that science merely consists of a series of very interesting, suggestive facts, but without the necessary conjunction with regard to points of view leading to general results.

A great many prominent men have devoted their lives to the study of vegetable cells, and we must allow that botany has now progressed as far as zoölogy, but only with regard to the accumulation of facts, in animal biology the cellular physiology of Virchow, dating from 1858, has arrived at a very high stage of development. Therefore, when thinking of the construction of a comparative physiology of animals and plants, it will be a most thankworthy task to collect all of the thrown facts concerning the physiology (qua biology) of the plant cell and arrange them from a general point of view.

The reason why the botanical part of cellular science has not brought forth general results during this long period is also to be sought in the definition of the cell body in botany. Very few physiologists would allow that the plant cell as well as the animal cell is an organism. Still this definition is to be looked at as a necessary foundation for a clear perception of the phenomena of botanical cellular physiology, both mechanical and chemical. As far back as 1848, one of the most prominent physiologists, N. Pringsheim (De forma et incremento stratorum crassiorum in plantarum cellula observationes quaedam novae. Halae, 1848, p. 38.) reminded us that "cellula est individuum," Hilger and Husemann, Weiss, and A. Zimmermann have told us almost the same, but still we find such definitions as "Grundorgan" (Frank), "Elementargebilde," "Formelemente" (G. Haberlandt). In his excellent "Lectures," Vines calls the plant cell "the physical basis of life." It must be remembered that Huxley ("Physical Basis of Life") only spoke of the protoplasm as the bearer of life. And Huxley himself, when he gave this most ingenious definition, did not see in protoplasm the *physiological basis of life.* Life never rested on a physical basis, nor consisted in physical matters alone.

Nobody will doubt whether a yeast cell is an organism or not.

Professor R. Pedersen, of Copenhagen, for six years my teacher in physiology, first mentioned these facts to me in the winter of 1891, acknowledging the results of this consideration for the evolution of cell theory in botany. Never this explanation was said with regard to the fact that said definition subsequently would form the key to cellular physiology in botany and, I may add, to comparative physiology of animals and plants.

The question is of considerable importance, because the accumulated facts now need a basis. The proposition of Sachs in his recent paper must be said to have come in due time. Yet it evidently ought to be connected with the given definition of the cell. Now we shall be able to arrange the facts in a system, see where vacant spaces may be, and fill up the voids, but up to the present time we were unable to do so.

Taking the "energids" as a basis of vegetable life, Sachs found "a real unity as a basis for the plant body," when we allow an energid to be "one nucleus with that protoplasm which surrounds it and which is commanded by the same nucleus." Then, looking forward, we shall see as one of the necessary results that the cell, often containing more than one nucleus, is really an organism, never an organ. Even without this deduction we may acknowledge the cell as an organism, because it acts as an organism.

Mechanics not being life, life is not mechanics; physiology alone is the science of the functions of life. Therefore, to understand the latter we must find a good physiological foundation for it.

By this explanation I hope to have been able to show that investigations in the life of the plant cell ought to be brought into another trace in the future. More than usual plant physiologists must be aware that they want — as Sachs says — "a scientific language, according to the true scientific idea."

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.

An Alleged Mongoloid Race in Europe.

ALTHOUGH it is not usual, and often impracticable, for writers to reply directly to the various criticisms passed upon their books, yet, as an interested reader of *Science,* I may perhaps be allowed to say some words with regard to a review of my "Testimony of Tradition," contributed to your issue of Feb. 10 (p. 82), which I have not had an opportunity of seeing until to-day. This I desire to do in order to remove more than one misapprehension of my meaning in the work reviewed.

"The very slender basis for the whole theory," says the reviewer, "is the syllable *Fin.*" In this he is greatly mistaken. Linguistic comparisons in this direction are certainly made, and considerable stress laid upon them, but these are entirely subsidiary to the important statements quoted in the first chapter. Briefly, these are to this effect: Wallace, a clergyman in Orkney during the second half of the seventeenth century, states that "Finn-men" were at that time occasionally seen off the coasts of Orkney, each "Finn-man" being the solitary occupant of a small skiff. In particular, he specifies the years 1682 and 1684, and another writer (Brand), who confirms his account, gives instances in or about the years 1700 and 1701. Their skin-boats, and the dress and usages of the people themselves, as described by these writers, identify them at once with Eskimoes, i.e., an Eskimo-like race. Of this there can be no reasonable doubt. Both writers state that one of their skin-boats was then preserved "as a rarity" in the Hall of the Edinburgh College of Physicians, and it is added that another specimen was preserved in the parish church of Burray, Orkney. The former statement is confirmed by an entry of the year 1696 in the minute-book of the Edinburgh College of Physicians, which I copied from the original writing and published in my book (p. 10). The writer first quoted (Wallace) is stated — in quite another connection — to have been noted for his "veracity;" and this is quite borne out, in this instance, by the evidence of two contemporary writers and the minute-book of the College of Physicians.

That these skiffs were "kayaks," and that their occupants were practically Eskimoes, is what I have never seen called in question. The first writer (Wallace) conjectures that they actually *were* Eskimoes from Davis Straits. This also is the explanation offered by some critics, whose preconceived notions prevent them from entertaining the idea that certain European castes, within comparatively recent times, may have been (ethnologically) Eskimoes. Wallace's son, editing his father's book in 1693, thinks it "a little unaccountable how these Finn-men should come on this coast," from so great a distance as Greenland. And Brand (1701), who calls them also "Finland-men," regards it as "strange" and "wonderful" that they should come even from Finland,—assumed by him to be their home. Orkney tradition, which styles them "Finns" and "Fin-folk," connects them with a certain island in Orkney, with Shetland, with Norway, with the Faroe Islands, and even with Iceland.

It is perhaps within the bounds of possibility that Greenland "kayakkers" made their way to the Orkneys, *via* Iceland, the Faroes, and Shetland, about the year 1680. But this assumption seems to me so unreasonable that I cannot entertain it. Were the "Finn-men" of 1701 the immigrants of 1682, and had they been living in retirement about the Orkneys all that time? Or, when pursued by the Orkney fishermen, as they often were, did they retreat on each occasion to Greenland? But it is futile to suggest questions such as these. It is much more reasonable to assume that the stories about "Finns" and "Fin-folk" (though blended in modern times with impossible stories about *seals*) have an actual historical basis, and relate to a people whose home was in Europe and not in America.

I dare not trespass further upon your valuable space, or I would say more with regard to the various points selected for criticism. I shall only add that the reference to the Delaware Finns of the seventeenth century is not my own, but is *quoted* (at p. 36); that "Finn" and "Lapp" were once used interchangeably, though now distinct; and that, according to C. F. Keary ("The Vikings," p 157), the Scandinavian peninsula, almost as far south as the 60th parallel, "was Lapp or Finnish territory" in the ninth century, which allows of possible surviving remnants at a much later date in that region,—not to speak of the British Isles.

DAVID MACRITCHIE.

Edinburgh, March 7.

A Possible Source of Confusion as to the Origin and Character of Certain Shells.

IT is quite possible that in studying the fossils of a single stratum of rock or even so small a fragment as a hand specimen, one may find examples over which he pauses. Wide divergences may exist between shells that lie side by side. They have evidently been deposited from the same waters. Apparently, they have flourished under like surroundings of depth and character of water. Yet one example may bear traits of fresh-water origin, while another may be as distinctly of marine growth.

The key to the anomoly may probably be found in what is now going on along our lake shores. Take as illustration the intermingling of marine and lacustrine forms on the borders of Lake Champlain. In favorable places there are found closely packed accumulations of unios and related shells. The waves that have brought these to the shore have at the same time been gnawing at the banks of clay of the Champlain epoch. In these are imbedded saxicava and associated forms. The clay is worked over by the waters; the finer particles drift out into the lake, the coarser with the liberated shells sink down among the unios. So a firm stratum is made from forms now existing in the waters and those that long ago flourished there. These deposits await the phenomena that have consolidated like ones along shores in older geological time, after which shells of different origin and character may be broken from the same rock.

HENRY M. SEELY.

Middlebury, Vt.

BOOK-REVIEWS.

A Microscopic Study of Changes due to Functional Activity of Nerve Cells. Reprinted from the *Journal of Morphology.* By C. F. HODGE. Boston, Ginn & Co.

THE present investigation is the beginning of a new line of research, and Professor Hodge is to be congratulated on his successful pioneer work. It consists of an account of a long series of patient observations made upon the spinal nerve-cells of the frog and the cat under the influence of stimulation through the spinal nerves. The general conclusion is that stimulation of the nerve-cell produces changes, in the structure of the cell, which are visible to the microscope. The most noticeable and tangible of these changes is the shrinking of the nucleus. This shrinking of the nucleus was seen in all of the experiments described, and that it was not a pathological change was proved by the fact that a rest after the stimulation caused in a few hours a recovery of the nucleus to its normal size. Perhaps the most interesting results of the whole series of experiments was a comparison of the nerve-cells of the spinal cord and brain in animals killed in the morning after a night's rest, and similar animals killed at night after a day's activity. In every case a very striking difference in the microscopic appearance of the nerve cells was manifest. The whole line of work is extremely suggestive and very promising of important results in the future.

The Naturalist on the River Amazons. By HENRY WALTER BATES. With a memoir of the author by Edward Clodd. Reprint of the unabridged edition. New York, D. Appleton & Co. 395 p. Map. 8°.

AMONG the thousands of volumes that crowd the shelves of our great libraries there are few that have ever reached the honor of a second edition. Fewer still attain a third and fourth, and rare indeed is the instance of one that, decade after decade, and generation after generation, continues to delight the human soul. The vast majority of printed books are as ephemeral as the May-fly, born and dying in the same hour, read and forgotten as we read and forget the gossip of a Sunday paper. Those volumes that, no matter how often they are reprinted, are always fresh and new, and which give delight to the younger as they did to the older generation, we christen "classics." Some have come to us from ancient Greece and Rome: others from the Middle Ages: some from more recent days. In no single century, however, are there more than a small number that ever reach the pinnacle of public approval and become designated as classics. The more books there are the greater the numbers that are cast aside; so that in our time, when thousands of volumes are being poured from the press year after year, the chances that any one will be successful in achieving the highest honor are slight indeed. A book must possess more than usual worth: give to the jaded world some new ideas, and be couched in language to be read by old and young with equal pleasure. Books like the one at present under review belong to that category which includes such volumes as White's Selbourne, Darwin's Voyage, and Wallace's Malay Archipelago,—books which have fulfilled the requirements of classics, and which have been accorded that title by a grateful public.

No one can err, we believe, in placing Bates's "Naturalist on the River Amazons" among the foremost books of travel of this age; and no one who has read it, but recalls its graphic pages with delight. Pages that bring to those who have not seen with material eyes the wonders of the tropic zone, images of delight; and that recall to those who have seen these wonders visions of never-to-be-forgotten pleasure. It is said of the ornithologist Gould, who had long desired to visit the forests of the Amazons, that, meeting Bates after the appearance of his book, he exclaimed: "Bates, I have read your book; I have seen the Amazons!" It is now thirty years since the first edition appeared. Since then many others have been printed, mainly based, however, upon the second edition. This, upon the advice of his publisher and to his lasting regret, Bates abridged to a considerable extent. The public is, therefore, most grateful to have reproduced, as in the beautiful volume before us, the unabridged words of the author, written when time had not dimmed nor distance distorted the vividness of the images he depicts. But the volume also contains a sketch of the life of Mr. Bates, with extracts from his journals and letters, together with an abridgment of the celebrated article on mimicry in butterflies, which placed its author at once in the front rank of philosophical naturalists. The book, therefore, becomes practically a new one, which every lover of such literature should not fail to possess.

The life of Mr. Bates, aside from his travels on the Amazons, was an uneventful one. He was born in Leicester, on Feb. 8, 1825, and was apprenticed at the age of 13 to a hosiery manufacturer. He inherited from his mother a tendency toward dyspepsia, and so was always far from strong, but he early manifested a love for natural history, and spent all his spare time hunting butterflies and beetles. He began to write out his notes at an early age, and before he was 20 years old he had published some of his observations. It was while at Leicester that he made the acquaintance of Wallace, and the two friends went, in 1848, to the Amazons. His companion remained four years, but Bates himself stayed for seven years longer, and returned to England in 1859, with about 15,000 species, 8,000 of which were new to science, and with a wealth of observations that occupied his pen for many years.

Bates was the first to point out the curious fact of mimetic analogies between various species of butterflies, and to suggest that the cause of the mimicry was natural selection. He also suggested that the reason for the mimicry lay in the unpalatableness of the mimicked species, so that the mimickers, although themselves edible, escaped their enemies by taking on the form and coloration of the species that birds would not eat. An abstract of this paper, with a colored plate, is given in Dr. Clodd's memoir.

Among the more interesting portions of this part of the volume are a few extracts from Bates's journal. It is to be regretted that there are not more. In the following he records some of his impressions of Lyell.

"Sir Charles Lyell has the appearance of a fidgety man not well at ease with himself. He is very greedy of fame, and proud of his aristocratic friends and acquaintances. He does not seem to be a very ready man; his learning does not appear to be at his fingers' ends; so that when a subject is suddenly presented to him he has difficulty in collecting his scattered thoughts and bringing forth what he knows upon it. But then he is getting an old man now. Mr. Davidson told me he was a very hesitating writer, and re-wrote every sentence three or four times on the average. . . . But, like a well-bred gentleman, Sir Charles can become very sociable, and evidently likes a good dinner with brilliant conversation: Darwin says he likes to hear himself talk. At the Geological Club . . . he made me laugh by retailing a very good thing. The conversation ran on the comparative merits of the scientific hypothesis of the origin of man and the biblical man. 'Why,' says he, 'the question resolves itself into few words: Is man modified mud, or modified monkey?'"

It was due to the urgency of Darwin that Bates began and continued to write his travels; and on the appearance of the book in 1863 it met with cordial praise from all quarters. His style is direct and concise. While many writers would have given a long account of the outward voyage from England, Bates disposes of it in three lines, and plunges almost at once into the luxuriant forests that were to be his home for eleven long years. His first walk in the forest was taken with Wallace on the day of their arrival at Pará, and a part of it is thus described: —

"As we continued our walk the brief twilight commenced, and the sounds of multifarious life came from the vegetation around. The whirring of cicadas, the shrill stridulation of a vast number and variety of field-crickets and grasshoppers, each species sounding its peculiar note; the plaintive hootings of tree-frogs, — all blended together in one continuous ringing sound, — the audible expression of the teeming profusion of nature. As night came on, many species of frogs and toads in the marshy places joined in the chorus; their croaking and drumming, far louder than anything I had before heard in the same line, being added to the other noises, created an almost deafening din. This uproar of life, I afterwards found, never wholly ceased, night or day; in course of time I became, like other residents, accustomed to it. It is, however, one of the peculiarities of a tropical — at least a Brazilian — climate which is most likely to surprise a stranger. After my return to England, the death-like stillness of days in the country appeared to me as strange as the ringing uproar did on my first arrival at Pará."

The fact of a struggle for existence among animals is generally recognized by all, but the same struggle among plants is not so easily observed. In the luxuriant forests of the tropics the fact is forced upon all observers, and Bates gives a striking example of it. A parasitic tree occurs very commonly near Pará, which has received the appropriate name of the Murderer Liana or Sipó. It is described and the fact commented upon as follows: "It springs up close to the tree on which it intends to fix itself, and the wood of its stem grows by spreading itself like a plastic mould over one side of the trunk of its supporter. It then puts forth from each side an arm-like branch, which grows rapidly, and looks as though a stream of sap were flowing and hardening as it went. This adheres closely to the trunk of the victim, and the two arms meet on the opposite side and blend together. These arms are put forth at somewhat regular intervals in mounting upwards, and the victim, when its strangler is full-grown, becomes tightly clasped by a number of inflexible rings. These rings gradually grow larger as the murderer flourishes, rearing its crown of foliage to the sky, mingled with that of its neighbor, and in course of time they kill it by stopping the flow of its sap. The strange spectacle then remains of the selfish parasite clasping in its arms the lifeless and decaying body of its victim, which had been a help to its own growth. Its ends have been served — it has flowered and fruited, reproduced and disseminated its kind; and now, when the dead trunk moulders away, its own end approaches; its support is gone, and itself also falls.

"The Murderer Sipó merely exhibits, in a more conspicuous manner than usual, the struggle which necessarily exists amongst vegetable forms in these crowded forests, where individual is competing with individual and species with species, all striving to reach light and air in order to unfold their leaves and perfect their organs of fructification. All species entail in their successful struggles the injury or destruction of many of their neighbors or supporters, but the process is not in others so speaking to the eye as it is in the case of the Matador. The efforts to spread their roots are as strenuous in some plants and trees as the struggle to mount upwards in others. From these apparent strivings result the buttressed stems, the dangling air-roots, and other similar phenomena. The competition amongst organized beings has been prominently brought forth in Darwin's 'Origin of Species;' it is a fact which must be always kept in view in studying these subjects. It exists everywhere, in every zone, in both the animal and vegetable kingdoms. It is doubtless most severe, on the whole, in tropical countries, but its display in vegetable forms in the forest is no exceptional phenomenon. It is only more conspicuously exhibited, owing perhaps to its affecting principally the vegetative organs, — root, stem, and leaf, — whose growth is also stimulated by the intense light, the warmth, and the humidity. The competition exists also in temperate countries, but it is there concealed under the external appearance of repose which vegetation wears. It affects, in this case, perhaps more the reproductive than the vegetative organs, especially the flowers, which it is probable are far more general decorations in the woodlands of high latitudes than in tropical forests."

It is as much in the reflections that the varied phenomena under observation give rise to as in the descriptive portions that the value and charm of the book lie. There is always something new. Now it is the colossal trees, then the wonderful profusion of insect life, or the graphic pictures of free life in the forest. Nothing is more striking than the difference between the fauna on the two banks of the great Amazons, and Bates refers to this in numerous places. So, too, the wonderful fact that certain insects, especially the butterflies, mimic others is of vast interest. Then the great variation presented by some forms of insects, so that at the two ends of a series we have what are commonly called distinct species, while there are intermediate forms presenting every gradation between them. Those naturalists who claim that species do not originate *now* in a state of nature, would certainly find it a difficult task to refute or explain on any but one hypothesis the facts given by our author. One case in particular is of great interest. It would appear that two butterflies, species of *Heliconius*, are extremely abundant in the forests along the river. They inhabit, however, different sections of the country, one of which in moister than the other. One species occurs in the dry forests, the other in the moister ones. One species, *H. melpomene*, is black with a large crimson spot on its wings; the other is *H. thelxiope*, in which the wings are beautifully rayed with black and crimson and have a number of bright-yellow spots. Both have the same habits, and they have long been regarded as perfectly distinct species. We quote now Mr. Bates's words: "There are, as might be supposed, districts of forest intermediate in character between the drier areas of Obydos, etc., and the moister tracts which compose the rest of the immense river valley. At two places in these intermediate districts, . . . most of the individuals of these *Heliconii* which occurred were transition forms between the two species. Already, at Obydos, *H. melpomene* showed some slight variation amongst its individuals in the direction of *H. thelxiope*, but not anything nearly approaching it. It might be said that these transition forms were hybrids, produced by the intercrossing of two originally distinct species; but the two come in contact in several places where these intermediate examples are unknown, and I never observed them to pair with each other. . . . These hybrid-looking specimens are connected together by so complete a chain of gradations that it is difficult to separate them even into varieties, and they are incomparably more rare than the two extreme forms. They link together gradually the wide interval between the two species. One is driven to conclude, from these facts, that the two were originally one and the same; the mode in which they occur and their relative geographical positions being in favor of the supposition that *H. thelxiope* has been derived from *H. melpomene*. Both are, nevertheless, good and true species in all the essential characters of species; for, as already observed, they do not pair together when existing side by side, nor is there any appearance of reversion to an original common form under the same circumstances."

We have already so far overstepped our space that we must reluctantly refrain from quoting further. We would, however, call particular attention to the account given of Termites on pages 209–214; that on Fire Ants on page 227; on Monkeys on pages 331–345; and on the general features of ant life on pages 355–363. The remarks upon floating pumice on pages 263–264 are well worthy the consideration of students of geographical distribution, and those on page 169 are commended to the student of comparative philology, as indicating a method of the formation of dialects among savage tribes.

One word more and we have done; for even at the risk of tiring the patient reader we add one more quotation. After a life of eleven years spent in the Amazonian forests, certainly Bates was well qualified to judge between that life and civilized man's. He had formed a love for the country, and he took leave of it with regret. The desire, however, of once again seeing his parents and of enjoying the pleasures of intellectual society drew him from this "Naturalists' Paradise." "During this last night on the Pará River," he says, "a crowd of unusual thoughts occupied my mind. Recollections of English climate, scenery, and modes of life came to me with a vividness I had never before experienced during the eleven years of my absence. Pictures of startling clearness rose up of the gloomy winters, the long, gray twilights, murky atmosphere, elongated shadows, chilly springs, and sloppy summers; of factory chimneys and crowds of grimy operatives, rung to work in early morning by factory bells; of union workhouses, confined rooms, artificial cares and slavish conventionalities. To live again amidst these dull scenes I was quitting a country of perpetual summer, where my life had been spent, like that of three-fourths of the people, in gypsy fashion, on the endless streams or in the boundless forests. I was leaving the equator, where the well-balanced forces of nature maintained a land surface and climate that seemed to be typical of mundane order and beauty, to sail towards the North Pole, where lay my home, under crepuscular skies, somewhere about fifty-two degrees of latitude. It was natural to feel a little dismayed at so great a change; but now, after three years of renewed experience of England, I find how incomparably superior is civilized life, where feelings, tastes, and intellect find abundant nourishment, to the spiritual sterility of half-savage existence, even if it were passed in the Garden of Eden. What has struck me powerfully is the immeasurably greater diversity and interest of human character and social conditions in a single civilized nation than in equatorial South America, where three distinct races of man live together. The superiority of the bleak north to tropical regions, however, is only in its social aspect; for I hold to the opinion that, although humanity can reach an advanced state of culture only by battling with the inclemencies of nature in high latitudes, it is under the equator alone that the perfect race of the future will attain to complete fruition of man's beautiful heritage, the earth."

JOSEPH F. JAMES

Text-Book of the Embryology of Man and of Mammals. By DR. OSCAR HERTWIG. Translated by Professor E. L. Mark. New York, Macmillan & Co. $5.25.

PROFESSOR MARK has done a great service to English science by translating this text-book of embryology. The appreciation of the book abroad is shown by the fact that the edition which is now translated is the third edition since its original publication in 1866, the third edition of the first part of the text-book being demanded before the second part was ready for publication. The valuable scientific researches of Hertwig are very well known by all naturalists, and his name alone is sufficient to indicate the reliability of the work in hand.

The title, "A Text-Book of Embryology of Man and Mammals," does not adequately express the scope of the book, for while it is in details largely confined to the study of mammals, there is so much of general embryology within its covers as to give it a value as a general text-book of vertebrate embryology. As such a text-book it is of the greatest value to a student, and it is safe to say that at the present time there is no text-book so well designed to give the student a general knowledge of vertebrate embryology as the present one.

The subjects treated comprise all matters of importance connected with invertebrate embryology. They are treated in a masterly style, and the facts and discussions are in all cases brought up to date. In the chapters on the sexual products and their fertilization may be found a summary of the essential facts of our present knowledge upon this important subject. The chapter on cleavage discusses the general matter of the segmentation of eggs, giving the various types of such segmentation, their relations to each other and defining the terms used in descriptions in various text-books. The chapter on the gastrula is especially valuable, for it gives in a clear, logical, but concise manner, illustrated by valuable and intelligible drawings, our present ideas as to the application of the gastrula theory to the embryology of vertebrates It is a subject which is always puzzling to the student of embryology to understand the gastrulation of the vertebrate egg, and Professor Hertwig has done very much toward making this difficult subject intelligible. Not the least valuable part of this section is an outline history of the gastrula theory, tracing our knowledge of embryology of the germ layers from its infancy to the present time. The gastrula theory is accepted by Professor Hertwig in its fullest sense. The chapter on the formation of the body cavity gives Professor Hertwig an opportunity of explaining clearly his "coelomthorie" which he does in a clear style, and the significance of which he makes plain by its historical consideration. In addition to the above, there are considered in the first part of the work the segmentation of the vertebrate embryo, the origin of connective tissues, the method of formation of the external form of the vertebrate body and a study of the fœtal membranes of reptiles, birds, mammals, and man. In all of these sections the aim of Professor Hertwig is not only to give facts but to give a logical connective account of the significance of the facts and a logical understanding of the various phases in the development of the vertebrate body, and he has greatly added to the value of the discussions by short historical accounts of the development of our knowledge on the various topics.

The second part of the book is devoted to the development of special organs, and here the author is more confined to the mammals and gives less consideration to the other vertebrates. The method of consideration is that of the study of organs according to their origin in the different layers of the body. These are considered, therefore, under four heads. The organs of the endoderm include the alimentary system in general; the organs of the ectoderm include the nervous system; the organs of the middle germ-layer include the muscles, the urinary and sexual organs. Professor Hertwig's views of the body cavity lead him to the formulation of a fourth layer of the vertebrate embryo, which he calls the intermediate layer or mesenchyme, and the last section of the text-book studies the development of the organs from this mesenchyme. These, according to Hertwig, are the circulatory system and the skeleton.

The special merits of this book are the logical treatment and its consideration of the embryological facts as parts of a system. The general method of the treatment of the subject is a comparative rather than a physiological one, and the text-book will give the student an insight into comparative anatomy but very little consideration of the physiology of the developing embryo. In one of the two sections, it is true, the mechanics of development are considered, but in general, the text-book is a morphological rather than a physiological study. This is, of course, a natural outcome of the line of work in which Professor Hertwig has been so successfully engaged for so many years.

Not the least valuable part of the book consists in the abundant literature. Some fifty pages are devoted to giving the titles and references to the most important papers of vertebrate embryology. The book has, also, another feature, somewhat rare in German scientific books, but of extreme value to students, in the form of short, logical, but intelligible summaries at the end of every section giving in outline the important conclusions.

On the whole, the text-book of Professor Hertwig is probably the best general study of vertebrate embryology that has appeared in the English language up to the present time, and it can be most heartily recommended to all interested in these subjects.

Chemical Lecture Experiments. Non-Metallic Elements. By G. S. NEWTH, F.I.C. London and New York, Longmans, Green & Co. 323 p. 8°.

A BOOK of chemical lecture experiments, carefully classified and systematically arranged, cannot but be welcome to many. Moreover, a book from a practised hand, such as Mr. G. S. Newth, chemical lecture demonstrator in the Royal College of Science, South Kensington, has a particular value in that its experiments are so given as to be readily repeated and are not, as is often the case, merely a statement of the reaction with a few confusing details. Mr. Newth has chosen his experiments well and has described them in clear concise language. The book has a two-fold purpose in easing the labors of the lecturer and of the student alike. For the former it supplies a useful repertoire of lecture experiments and will surely be gladly received, removing, as it does entirely, the humdrum search for such examples and reactions as can be suitably and successfully demonstrated on the lecture table. This, as every lecturer knows, is by no means a small item in the preparation of a lecture, and, moreover, being important, it cannot be carelessly or hastily done. The experiment must be quickly and successfully performed or the interest of the student is turned to illy-concealed ridicule, and the lecturer is, so to speak, lost.

To the student the book appeals in providing a ready reference to serve as a companion in the lecture room, and in supplying the deficiencies of his notes. Indeed, it may in most cases entirely relieve him of the necessity of taking notes upon the experiments themselves, drawings of the apparatus, etc., and he will thus be enabled to devote his attention to the explanations

CALENDAR OF SOCIETIES.

Anthropological Society, Washington.

March 14.—Major John W. Powell, A Study in Psychology.

Geological Society, Washington.

March 22.—G. K. Gilbert, An Open Fissure; G. P. Merrill, Remarks on the So-Called Onyx Marbles or Travertines; C. D. Walcott, The Algonkian Rocks of the Grand Canyon of the Colorado.

Chemical Society, Washington.

Feb. 9.—W. H. Krug, A New Method for Estimating Furfurol-Hydrazone; E. E. Ewell and H. W. Wiley, On Some Products of the Cassava Plant. Professor Wiley describes the plant as it occurs in Florida, and says there is every reason to believe that, if the attention of capitalists is called to it, a large quantity of land now covered with pines could be profitably cleared and devoted to the cultivation of the cassava plant. A minimum average yield is four tons of roots per acre, which may be readily increased by proper fertilization to eight or ten tons per acre. Maize could not compete with cassava if the same intelligent cultivation is applied, and there is a prospect that the cassava will eventually take the place of maize in the production of starch, glucose, etc.

March 9.—W. D. Bigelow and K. P. McElroy, Determination of Lactose in Presence of Invert Sugar and Sucrose.

Philosophical Society, Washington.

March 18.—W. H. Holmes, Traces of Glacial Man in the Trenton Gravels; Asaph Hall, The Planet Mars.

and arguments of the lecturer. For the further aid of the student, equations are given representing the various reactions described, and every reasonable detail of the operation is recorded. Not only for the student in the lecture room is the book valuable, but also for the many who have not the advantage of college demonstration and who are compelled to rely largely upon their own resources. For the convenience of such teachers as may not have ready access to books of reference, Mr. Newth has added in the form of an appendix a number of important and useful tables, thirty-four in all, not often found in the smaller works on chemistry. C. P.

An Introduction to Qualitative Chemical Analysis by the Inductive Method. A Laboratory Manual for Colleges and High Schools. By DELOS FALL, M.S. Boston and New York, Leach, Shewell & Sanborn. 8°.

IN this age of text-books it is difficult to be original, and the most that our authors have aimed at is perfection and revision of arrangement. Mr. Delos Fall, M.S., Professor of Chemistry at Albion College, has, however, recently given to the student of chemistry a new manual of qualitative analysis decidedly unique in its character. As he asserts in his prefatory note, this manual is designed to impart but little chemical truth directly, aiming rather to lead the student to gain that truth himself as nearly as possible at first hand and as a product of his own thinking. A preliminary chapter intended for both the student and the instructor gives the general plan of the book and describes the method to be used. This consists essentially in leading questions calculated to bring the student on by his own research from the fundamental ideas of chemical theory to the more advanced practical application of his knowledge to systematic qualitative analysis.

The plan as described by the author is a combination of (1) original investigation and (2) reference to authorities. "In the beginning (1) will be a very small factor and (2) correspondingly large. As experience in manipulation, observation, and interpretation increases (1) will increase and the necessary dependence on (2) will decrease. To the experienced chemist (1) is very large and (2) very small; in other words, he is his own authority." Under the guidance of an able instructor the book will be most valuable, and in such a case will, as the author has himself demonstrated, produce accurate, enthusiastic, and independent students. C. P.

The Batrachians and Reptiles of the State of Indiana. By OLIVER P. HAY. Indianapolis, William P. Burford, Printer and Binder.

THE present work consists of a list of the reptiles and batrachians found in the State of Indiana, with a description of their characteristics and with analytical keys for the determination of species. The work describes 81 species in all, and is accompanied by a few plates illustrating the subject. The design of the author is to make a key which shall be usable by those who are not specialists, and he has therefore appended a glossary, explaining the use of all scientific terms, and his general method of treatment is such as to make the book intelligible even to a novice.

The Birds of Indiana. By AMOS W. BUTLER. From the Transactions of the Indiana Horticultural Society for 1890.

THIS little pamphlet consists simply of a list of the birds found in Indiana, either as residents or as temporary migrants. No characteristics of species are given, although a large number of illustrations are inserted, taken from Coues's "Key to North American Birds."

THE price of Dr. Sternberg's work on bacteriology, reviewed in *Science*, Feb. 24, should have been stated as $8, cloth; and $9, sheep; Wm. Wood & Co., New York, publishers.

Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

For sale—A Zentmayer new model U. S. Army Hospital monocular stand. Price $110, will sell for $75. Address H. C. Wells, No. 151 Broadway, New York.

For sale—A complete set of the third series of the American Journal of Science (1870-1893) handsomely bound in single volumes in dark brown half morocco. Address G. H. Williams, 803 Cathedral Street, Baltimore, Md.

For sale, or for exchange for books on medicine or surgery, new editions only, a large geological library, containing nearly all the State and Government Reports since 1855. Will be pleased to answer letters of inquiry and give information. Address R. ELLSWORTH CALL, Louisville, Ky.

For exchange.—Slides of Indian Territory Loup Fork Tertiary Diatoms for other microscopic fossils. Address S. W. WILLISTON, Univ. of Kansas, Lawrence, Kans.

For exchange.—Will exchange an "Ideal" Microscope of R and J. Beck, London, 2 eye pieces 3 objectives, 3 inch, 1 inch, 1-6 inch; bull's eye condenser on stand, substage condenser, mechanical stage, etc., for any of the leading makes of type writers. Particulars by mail. DELOS FALL, Albion College, Albion, Mich.

Sale, or exchange for similar material; Diatoms (Isthmia nervosa), unmounted, from San Francisco Bay. M. J. ELROD, Bloomington, Ill.

For sale or exchange.—I have a few copies of my translation of "Strasburger's Manual of Vegetable Histology, 1887," now out of print, which I will send post-paid for $3 or for one dozen good slides illustrating plant or animal structure. Address A. B. Hervey, St. Lawrence University, Canton, N. Y.

The undersigned has the following specimens to exchange for crystals of any eastern or foreign localities or Indian relics: tin ore, metacinnabarite, stibnite, garnierite, calenanite, hanksite, ulexite, rubellite, lepidolite, blue and green onyx, Cal. pineite, aragonite on chalcedony, cinnabar, double refracting spar, clear and clouded, and others. J. R. Bush, care of General Delivery, Los Angeles, Cal.

For sale or exchange.—A private cabinet of about 200 species of fossils, well distributed geologically and geographically. Silurian, about 40; Devonian, about 50; Carboniferous, about 80; others, about 30. Frank S. Aby, State University, Iowa City, Ia.

Wants

WANTED—Second-hand copy Tryon's Systematic Conchology. Please quote condition, date, price. H. L. Osborn, Hamline University, St. Paul, Minn.

A COMPETENT TEACHER of botany in college or university is open to engagement. Address L., Box 86, Rochester, Mich.

CAN any one inform me as to the age to which cats have lived? I have one twenty years old. Edward D. Webb, 132 W. Eighty-first St., New York.

WANTED — Second-hand. Foster's Physiology, Balfour's Comparative Embryology, Claus & Sedgwick's Zoology, Flower's Osteology of Mammalia, Vine's Physiology of Plants. Please state editions and prices asked and address Richard Lees Brampton, Ontario, Canada

WANTED.—American Journal of Conchology, seven volumes. Parties having these for sale will please address the undersigned, stating condition and price. R. Ellsworth Call, Louisville, Ky.

A GRADUATE ENGINEER will give instruction evenings in geometry, trigonometry and surveying, mechanics, physics, mechanical drawing and general engineering construction. Five years' experience in field and editorial work on engineering journal. References furnished. C. S. H., 102 Tribune Building, New York.

WANTED.—By well-qualified and experienced science master and associate of the Royal School of Mines, London, aged 26 (at present in England), a mastership in technical college or university for any of the following subjects: Engineering sciences, geology and mineralogy, physics, chemistry and metallurgy, etc., etc. Can provide excellent references and credentials. Apply, J. G., 17 Sussex St., Rochdale, England.

A GRADUATE of the University of Pennsylvania and a practical mineralogist of twenty years' experience desires to give his services and a cabinet of 25,000 specimens, all named, with about the same number of duplicates, in minerals, crystals, rocks, gems, fossils, shells, archæological and ethnological specimens and woods to any institution desiring a fine outfit for study. The owner will increase the cabinet to 50,000 specimens in two years and will act as curator. Correspondence solicited from any scientific institution. J. W. Hortter, M.D., Ph.D., San Francisco, Cal., General P. O. Delivery.

SCIENCE

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SCIENCE

NEW YORK, MARCH 31, 1893.

ON THE ETHNOLOGICAL CHARACTERISTICS OF THE HUMAN NASAL CANALS, CONSIDERED AS AN ECONOMIC ADAPTATION.

BY WILLIAM C. BRAISLIN, M.D., BROOKLYN, N.Y.

THE human voice bears a constant relation to the physical construction of the voice-producing organs.

Deep, manly tones are the constant sequelæ to a larynx of large proportions, to long and slowly vibrating vocal cords; while the childish treble and the high-pitched voice of the female are the natural productions of the smaller larynx with its shorter, and, consequently, more rapidly vibrating, vocal cords.

Of equal importance in the modulation, tone, and indefinable individual peculiarity of the voice, is the construction of the nose and pharynx. The anatomical construction of these organs is as varying as is that of the facial features, and, to the trained eye, the individual peculiarities are as apparent.

It is not to be wondered at, therefore, that the racial peculiarities, in regard to the anatomical structures, of the internal nares are particularly striking.

The external anatomical construction of the nasal organ characteristic of the native African, is as distinguishing an ethnological differentiation as are the color of his skin, the texture of his hair, and the development of his brain.

Corresponding distinctions characterize the *internal* structure of this portion of the upper air tract of this race. Immediately on passing the anterior openings of the nares, the human nasal canals spread out into deeper and wider channels. These cavities, separated by the *septum narium*, are corrugated on their opposite surfaces by the three pairs of turbinated bodies, whose "curled-leaf" surfaces increase enormously the extent of surface area of the nasal mucous membrane.

The bones of the cranium taking part in the formation of the nasal cavities, so differ in the African race as to make these canals wider, shorter, and less deep than those of other races. The bony ridges coursing their long axes, known as the turbinated bones, are also more blunt and less prominent and have less of the "curled-leaf" character.

Measurements of a single specimen of an average skull of either race will suffice to show this difference in bony framework.

	Negro.	White.
	Centimetres.	Centimetres.
Width of anterior nasal canals	2.7	2.5
Height of anterior nasal canals	2.6	3.5
Length of nasal canals (from anterior nasal spine to posterior nasal spine)	5 2	5.3
Length of nasal canals (from anterior nasal spine to posterior-superior angle of vomer)	7.0	7.2
Length of skull	19.0	19.0
Breadth of skull	14.0	14.0

The continuance of these peculiarities in the lineal descendants of the negro race as seen in America to-day, is markedly striking to the student of the comparative anatomy of this region.

One, indeed, cannot fail to note in any of the clinics devoted to the diseases of this region the increased advantage which accrues to the possessor of the wider nasal canals when pathological conditions, resulting in thickening of the lining mucous membrane, attack this portion of the anatomy.

One of the most prominent American larynxologists was enabled, by means of the more patent condition of the nasal canals in his negro patients,[1]— the American descendants of this race,— to make a contribution of certain facts to medical science, not otherwise easily obtainable.

Mr. Wallace[2] supposes that the origin of the African race was due to a migration of some part of the race of ancestral man from the great Euro-Asiatic plateau, and that his present characteristics are the result of physiological modifications due to climatic influence.

If we accept this likely explanation in regard to the special anatomical construction of the nasal cavities of the negro and his descendants, our logical conclusion is that they must present characteristics specially adapted for preparing the inspired air of a tropical climate for reception into the lung structures. We must suppose that ages of contact of his nasal mucous membrane with the atmosphere of a tropical climate have brought about, by the laws of natural selection, a nasal construction the most nearly adapted to dealing with the problem of the irritating qualities of a tropical atmosphere, of any existing race.

It does not seem probable that the evolution of the construction of the nasal canals, characteristic of the African type, has been due to any reason of better serving the purposes of the gustatory sense. The custom of regarding the nose as the organ of smell has, very properly, become modified to that of regarding it as primarily an organ of respiration. It is the proper channel for the conduction of air to the lungs for purposes of oxidation.

As has been stated, the means of warming, of moistening, and of freeing the air from dust and other irritating qualities are here most admirably afforded. The *sense of smell* must, indeed, also be regarded as a protective provision for the avoidance of substances irritating to the more delicate lung substance.

Had its purposes demanded an assistance to the procuring of food or to the avoidance of poisonous food, a development of the gustatory sense, such as is found in the dog or other animals, would have been found in the human species. This, however, is not the case. The acute sense of smell is not one highly developed or inherent in man. Indeed, this sense is soon entirely lost in conditions of the nasal canals which interfere with its respiratory function.

The difference we have noted in the anatomical construction of this portion of the upper air tract — the nasal canals — is naturally resultant in different pathological effects in the different races as regards the particular portions of the respiratory tract most frequently becoming the seat of disease processes under the influence of atmospheric irritation. Less protection is afforded the lung structures of the negro race on account of the anatomical structure of his nasal canals, since less opposition to the irritating factors of the atmosphere is presented, by reason of the more patent and more direct course which the inspired air encounters. That the lung structure of this race in the United States suffers from the irritation caused by degenerating atmospheric conditions seems to the writer to be evinced by data of the Tenth Report of Vital Statistics of the United States, compiled by Dr. John S. Billings.

Among many other facts of great significance in this compilation we are shown that the number of deaths per 1,000 from consumption — the disease *most* dependent, perhaps, upon irritating qualities of the atmosphere — is not only greater among the colored portion of our population, but it is in direct proportion

[1] American Journal of the Medical Sciences, 1883.

[2] "Darwinism," p. 460. Macmillan.

greater the more severe the rigors of climate encountered in the respective *areas of territory* from which statistical returns are cited. Thus in the Gulf coast region the proportion of deaths from consumption per one thousand deaths is about equal among the whites and blacks; but in the Middle Atlantic coast region the difference of numbers shown is a very distinct one.

In five specified areas of territory the exact proportion is as follows:—

The Number of Deaths from Consumption in 1,000 *Deaths from all Causes.*

	Whites.	Colored.
Middle Atlantic Coast Region	140.9	175.1
South Atlantic Coast Region	88 0	105.5
Gulf Coast Region	115 8	120.6
The Interior Plateau	138.4	126.7
The Ohio River Belt	150.7	238 1

Thus, while in the first case there is a difference of 34.2, in the third — the Gulf coast region — there is only a difference of a little less than 5; while in the fifth case a difference of 87.4 in the number of deaths per 1,000 from consumption exists.

We have, therefore, it seems to the writer, sufficient grounds upon which to advance the theory that the more patent condition of the nasal canals in the colored race is largely responsible for the more frequent occurrence in this race of lung disorders, as compared with the white races, in the United States.

Diseased conditions of the nasal canals in the dark race resulting in stenosis are especially rare; while, as every physician engaged in the clinical study of these disorders will testify, a condition of stenosis is one of the most common, and among the first, symptoms of disease involving this portion of the air tract in individuals of European descent.

It is unnecessary to say that the inference must not be drawn that a condition of stenosis is a safeguard against lung disorders. Any condition of the nasal passages which results in or necessitates *mouth-breathing* directly favors diseases of the lungs.

When a condition of the nasal passages exists prohibiting the free passage of air through them, the professional services of a physician or surgeon should be sought to remedy this defect. And since, as has been said, the nasal passages should be considered an integral part of the respiratory tract, the remedy should constitute not merely the rendering of this organ patent, but should also aim to restore it to a condition in which it may regain the ability of performing its normal functions of moistening, of warming, and of purifying, by freeing from irritating factors, the inspired air.

It is impossible to state how far the remedial effects of selection or evolution are modifying the peculiarities of anatomical structure of the nasal passages of the black race in the United States.

That these peculiarities become less prominent along with other racial peculiarities of the descendants of the negro race is, however, very evident to the accurate observer. To the untrained eye, the external characteristics of this organ are undergoing modification, and to clinical observers a like change is noted in the internal structure; but just how far this may be due to the admixture of white blood, and how far to selective and developmental modification, is beyond the power of the writer to estimate.

PROFESSOR W. S. BAYLEY of Colby University, Waterville, Me., has collected into a volume, with a separate title-page and index, notes on mineralogy which have appeared during 1892 in the *American Naturalist*. Professor Bayley is the editor of the Department of Mineralogy and Petrography of the *Naturalist*, and these notes summarize the papers that have been published during the past year. The volume will prove useful to those who wish to be posted on the literature of these two branches of science.

THE EFFECT ON THE COLLEGE CURRICULUM OF THE INTRODUCTION OF THE NATURAL SCIENCES.[1]

BY W. L. POTEAT, WAKE FOREST COLLEGE, N.C.

THE natural sciences are at last firmly lodged in the college curriculum. They are a recent importation. Their exact position and relations are scarcely yet settled, and one may easily fall into the mistake, on the one hand, of unwarranted precision in setting forth their present status, and, on the other, of over-confidence in predicting their ultimate influence upon the culture of our higher institutions of learning. Our observation, however, has probably extended over a period sufficiently long to yield some reliable results, which at this stage of it may well be brought together.

I. What were the circumstances under which the sciences gained a place in our educational machinery?

The college curriculum in its present form is the result of a gradual growth from very ancient and rude beginnings. As in a living organism, the successive modifications of the bulk and complexity of its structure have been closely dependent upon its environment. It responds with great sensitiveness to changes in the world about it. Hence it comes to pass that the apparatus and methods of culture of one period and race differ more or less widely from those of all other periods and races. The history of this development is inextricably intertwined with the progress of external events. We must look, therefore, without, if we would find the explanation of the last great modification of the means of education.

Of course, science in some form and to some extent had a place in education long before the period which I now have in mind. On the other hand, in some quarters it may be said to be still fighting for recognition even at the present moment. Moreover, periods glide insensibly into succeeding periods. There are no sharp lines in nature. For that reason there can be none in history. And yet, in order to avoid confusion and irksome modifications of every statement, I must be allowed to draw a somewhat arbitrary line and consciously to foreshorten the stages of a continuous advance.

For reasons which seem to me sufficient, I draw the line at 1859, the date of the publication of the "Origin of Species," and characterize the 35 years following as the period of science in education. It will, perhaps be agreed that no book in the domain of science, not even excepting the work of Bacon or Newton, has produced an influence so far-reaching and so profound. This date I fix upon the more willingly, inasmuch as it marks the new birth of the science of biology, which has affected all departments of human thought more deeply and permanently than all the other sciences. And it I have chiefly in mind on the present occasion.

The characteristic feature of the intellectual life of the period since the publication of the "Origin of Species" is the ferment precipitated by its doctrine. And the education of the period, in its spontaneous adjustment to external conditions, wears the same unsettled complexion, with science for its dominant tone. The middle decades of this century are unrivalled in all the thrilling history of the development of natural knowledge. The "Report on the Progress of Science" during the twenty years next following the Revolution of 1789 and read before the Emperor Napoleon in 1808, while it records some great names, contains nothing to match the record of the forties and fifties. And the next thirty years carried the wave of discovery and generalization but little higher. But about 1840 the spirit of scientific inquiry grew more intense, laid under contribution a larger number of rarely equipped minds, and pressed forward to attack the problems of the physical universe with a degree of vigor, boldness, and consecration which could not fail of brilliant achievements. Since that epoch the application of machinery to industrial production and to locomotion and intercommunication has revolutionized our common lives and given us new standards of comfort and activity. This revolution in the external aspects of modern civilization, it must be observed, "has been preceded, accompanied,

[1] Abstract of a paper read before the North Carolina College Association at Raleigh, Feb. 25, 1893.

and in a great measure caused by a less obvious but no less marvellous increase of natural knowledge, in consequence of the application of the scientific method to the investigation of the phenomena of the material world." The three great achievements which give our period its unique position in the annals of science are, the doctrine of the molecular constitution of matter, the doctrine of the conservation of energy, and the doctrine of evolution. They relate and unify an otherwise bewildering chaos of observation and experimentation. They have not, as Professor Huxley has said, fulfilled Bacon's conception of the aim of science and superinduced new forms upon matter, but they have in a sense created nature anew. They have given it a new voice. They have invested it with a new dignity and fascination.

Now, the subjects of study, under the stimulating influence of these great generalizations had, near the beginning of our period of science education, multiplied with amazing rapidity. And each new comer at once upon arrival challenged the pre-emptive right of its predecessors to the whole territory of education. Moreover, it was at once apparent that many of the new subjects yielded themselves with great hopefulness to the function of mental culture and had, besides, an important bearing on the practical conduct of life. Should the new knowledge, which in a thousand quiet ways was spreading into the thought of the times and recasting it, be kept dark to the minds of the young? Should they be left to the sudden and possibly disastrous shock of it when they should emerge from their cloistered life in college and find it all abroad and confronting them in every path?

It *was* resisted at the threshold. Nor should we be surprised. Conservatism is not passivity, mere resistance. It is rather an active force. It is not rest, but momentum. Whatever interposes itself to modify or deflect this current must be prepared for a collision. Illustrations abound throughout the history of education. Cato the Censor opposed strenuously the introduction of Greek into the Roman education. "Believe me," he wrote to his son, "the Greeks are a good-for-nothing and unimprovable race. If they disseminate their literature among us it will destroy everything." Again, we find that in the sixteenth century Latin and Greek, which in the nineteenth have held the ground against science, had themselves to win their way into the schools against "the 'Parva Logicalia' of Alexander, antiquated exercises from Aristotle, and the 'Questiones' of Scotus." Thomas More wrote to the dean of a school in London in which the new learning was recognized, "No wonder your school raises a storm, for it is like the wooden horse in which armed Greeks were hidden for the ruin of barbarous Troy."

But there are two features of the resistance to science in the curriculum, which, so far as I know, are peculiar to this last growth-pain of the educational ideal. The first springs out of the fear that what may be called the poetry of life will be rudely dealt with by the scientist, who comes upon the stage with the clatter of retorts and instruments, with a pigeon-hole for every sentiment and a physical test for every phenomenon of the soul. The inimitable Charles Lamb, on the side of prose, supplies an illustration of this feeling in the essay on "The Old and the New Schoolmaster," wherein he confesses his sins against science, saying, "I am a whole encyclopædia behind the rest of the world," while he but poorly conceals his disgust at the pretensions of the modern successors of "those fine old pedagogues who believed that all learning was contained in the languages which they taught." Representing the poets, John Keats, in "Lamia," exclaims sadly: —

> "Do not all charms fly
> At the mere touch of cold philosophy?
> There was an awful rainbow once in heaven:
> We know her woof, her texture; she is given
> In the dull catalogue of common things."

In Poe's "Sonnet to Science" we meet the same regretful aversion. A still more recent voice is raised in the prose and poetry alike of the late Mr. Matthew Arnold.

I own that I share in some measure this repugnance to bare, unrelated facts and the spirit of irreverence. But it is coming to be generally recognized that science does not rest in analysis, which is but its method to reach a higher synthesis. A catalogue of isolated facts, accumulated it may be with the infinite pains of an army of workers in field and laboratory, is of small value or significance except as it may contribute to the establishment of some great generalization or unifying conception. And, further, I doubt that the wholesome sense of mystery is dissipated by the progress of science. Her torch grows brighter with each passing year and shoots its rays farther into the surrounding darkness, but mystery walks ever at her side. She springs more questions than she solves. And so an increasing reverence is not only consistent with a widening intelligence, but in its higher and richer phases is dependent upon it. I believe, with the weighty testimony of George Eliot and Herbert Spencer and the practical illustration of the late Poet Laureate, that the knowledge of processes and causes, so far from clipping the wings of the imagination, in reality enlarges the sphere of its flight.

The second peculiar feature of the opposition to science in the curriculum alluded to above is the fear of its effect upon religious beliefs in the minds of the young. It would be easy to multiply illustrations of the supposed antagonism between religion and science, for it has had an unbroken succession from the trial of Socrates to the trial of Briggs; but I forbear. Here, again, the opposition is melting away as the limitations and real bearing of scientific inquiry are perceived.

So, then, we may repeat what was said in the beginning. The battle of the natural sciences for recognition in the schools is won. Universally won in theory, but the actual occupation of all the conquered territory is yet to be effected. As a rule, the entrance has been made in the higher institutions first. In England, the study of the earth and its productions is still but scantily represented in the instruction afforded by its great fitting schools. The case is much the same in our own country. Even where the sciences are taught in the primary and high schools it is too often book science, which is usually better not taught at all.

In North Carolina we may not say that so much as a beginning has been made in science teaching in our public schools and academies. I would respectfully submit it to the wisdom of this Association whether it should not take it upon itself to promote in some practical way the introduction of the natural sciences into these schools. Might not the colleges and State University help forward this reformation by publishing certain elementary courses in science as required for entrance? So far as I have been able to ascertain, Trinity, Wake Forest, and Guilford are the only institutions in the State that make such requirements.

In order to learn the position of the natural sciences in the higher education in North Carolina, I have made a canvass of the leading colleges, with the following tabulated result, which takes no note of elective classes, but only of prescribed:

Prescribed Recitations per week for four years for Bachelor of Arts:

	Total.	Nat. Sci.	Biology.	Percentage in Nat. Sci.	Percentage in Biology.
Davidson	65	4	0	6.1	0
Elon	69	10	.5	14.4	.7
Guilford	72	10	0	13.8	0
Trinity	67	4	0	5.9	0
University	61.5	6.5	0	10.5	0
Wake Forest	64	10	2	15.6	3.1

II. We may now consider specifically the effects which the natural sciences have produced upon the college curriculum.

1. The first which I shall mention recalls the physicist's doctrine of impenetrability. When science entered, room had to be made for it. That necessitated a movement of the constituent molecules of the curriculum upon one another, with the result of relaxing its rigidity. From the solid it passed to the semi-fluid state.

In America three expedients have been employed in the accomodation of the new subjects in the four years' college course. At first they were treated as "extras." Later they were admitted on terms of equality with the languages and mathematics, and all suffered some abatement in extent and thoroughness, it being held that elementary knowledge of all was more valuable for the purposes of a liberal education than extended knowledge of the remainder in case of the omission of science. The third expedient is as yet new, but has more than approved itself as the only one that can meet the conditions. I refer, of course, to the elective system. It is liable to abuse, perhaps it has been abused; but, under carefully weighed restrictions, it adds greatly to the culture-power of any curriculum. The disadvantages of the rigid curriculum are too apparent for statement. How many men have not achieved distinction in spite of the inflexible grind of the old college mill. On the other hand, how many single-gifted men have not been headed off and imprisoned in the unvarying meshes of collegiate requirements. Emerson speaks somewhere of "those classes whose minds have not been subdued by school education."

2. Closely associated with the relaxation of the rigidity of the form of education is the new conception of educational values that has resulted from the introduction of science instruction. The study of antiquity has lost somewhat of its prestige as a preparation for the life of to-day. But if the Greek and Roman life and literatures have lost their supremacy in general, they have not lost their disciplinary and quickening power for a certain order of minds. And to erect a scientific curriculum which should rigidly exclude these, as I believe Mr. Spencer proposed, would be a blunder only less disastrous than the reorganization of their old monopoly which was disintegrated by science.

3. I now mention last the catalytic force of science in the curriculum. Its presence has wrought the rejuvenation of the older subjects by supplying the illustration of a new and contagious method. They have acquired a new point of view, and in their treatment the emphasis is not now where it once was. They are immensely the gainers in educational value and in vitality. The ease and promptness with which they have responded to this scientific influence is the best guarantee of their permanence in the scheme of culture. The "new psychology," the "new political economy," and the "new history" may be mentioned as illustrations of this transformation. The Latin and Greek languages are no longer an end in themselves, but merely a means to the reproduction of the wonderful thought and life of the Latin and Greek peoples. Even theology, which, according to Macaulay, is the most rigid and unprogressive of all the systems of human thought, is showing signs of movement in response to the influence of the natural sciences — in particular, of biology.

THE MARINE BIOLOGICAL LABORATORY.—SIXTH SEASON, 1893.

IN addition to the regular courses of instruction in zoölogy, botany, embryology, physiology, and microscopical technique, consisting of lectures and laboratory work under the constant supervision of the instructors, there will be a number of lectures on special subjects, by members of the staff. A course of lectures in Embryology will be given by Professor Whitman; on the Morphology of the Vertebrate Head, by Dr. Ayers; and two or more courses in Invertebrate Zoölogy, by Drs. Bumpus, McMurrich, Rankin, and Morgan. There will also be ten or more evening lectures on biological subjects of general interest. Among those who may contribute these lectures may be mentioned, in addition to the instructors above named, the following: Drs. E. A. Andrews, Johns Hopkins University; Howard Ayers of the Allis Lake Laboratory; Professors W. G. Farlow, Harvard University; William Libby, Jr., Princeton College; J. M. MacFarlane, University of Pennsylvania; C. S. Minot, Harvard Medical School; E. S. Morse, Salem; H. F. Osborn, Columbia College; John A. Ryder, University of Pennsylvania; W. T. Sedgwick, Massachusetts Institute of Technology; E. B. Wilson, Columbia College.

The Laboratory is located on the coast at Wood's Holl, Mass., near the laboratories of the United States Fish Commission. The building consists of two stories, and has 33 private laboratories for investigators and 5 general laboratories — two for beginners in investigation in zoölogy, one for teachers and students receiving instruction in zoölogy, one for botany, and one for physiology. The Laboratory has aquaria supplied with running sea-water, boats, a steam launch, collecting apparatus, and dredges; it is also supplied with reagents, glassware, and a limited number of microtomes and microscopes. *No alcohol can be supplied beyond what is required for work in the laboratory.*

By the munificence of friends the library will be provided not only with the ordinary text books and works of reference, but also with the more important journals of zoölogy and botany, some of them in complete series.

The Laboratories for Investigators will be open from June 1 to Aug. 30. They will be equipped with aquaria, glassware, reagents, etc., *but microscopes will not be provided.* In this department there are 33 private laboratories for the exclusive use of investigators.

Those who are prepared to begin original work under the guidance of instructors will occupy tables in the general laboratories for investigators, paying for the privilege a fee of fifty dollars. The number of such tables is limited to 20.

An elementary course in vertebrate embryology will be introduced this season, designed to meet the needs of those who have completed the general courses in the Students' Laboratory. The study will be confined mainly to the fish egg as the best type for elucidating vertebrate development. Each member of the class will be supplied with material and be expected to work out each step in the development from the moment of fecundation. The aim will be not only to master the details of development but also to acquire a thorough knowledge of the methods of work. Methods of preparing surface views, imbedding in paraffin and celloidin, various methods of staining and mounting, drawing, reconstruction, modelling, etc. The course will thus combine just what is needed as a preparation for investigation.

This course will open Wednesday, July 5, and continue six weeks, and it will be conducted by Mr. Lillie and Professor Whitman. The fee for this course will be fifty dollars, and the class be limited to ten.

Applicants should state what they have done in preparation for such a course, and whether they can bring a complete outfit, viz., a compound microscope, a dissecting microscope (the Paul Mayer pattern made by Zeiss is the best), camera-lucida, microtome, etc. In case these instruments are furnished by the Laboratory, an additional fee of ten dollars will be charged therefor. No applications for less than the whole course will be granted.

The Zoölogical Laboratory for teachers and students will be opened on Wednesday, July 5, for regular courses of six weeks in zoölogy and microscopical technique. The number admitted to this department will be limited to fifty, and preference will be given to teachers and others already qualified. By permission of the director and by the payment of additional fees, students may begin their individual work as early as June 15, but the regular instruction will not begin before July 5.

Though more advanced students who may wish to limit their work to special groups will have an opportunity to do so, the regular course in zoölogy, in charge of Professor Bumpus, will embrace a study of the more typical marine forms and elementary methods of microscopical technique. The laboratory work, outlined below, will be accompanied by lectures.

July 5–8. Study of the Lobster. (General anatomy — methods of injecting — preparation of histological material.) July 11–15. Cœlenterates (*Campanularia*, *Tubularia*, *Metridium*, *Mnemeopsis*). July 17–22. Vermes (*Nereis*, *Balanoglossus*, and *Phascolosoma*, *Polyzoa*, *Bdelloura*). July 24–29. Echinoderms (*Asterias*, *Arbacia*, *Echinarachnius*, *Thyone*); Mollusks (*Venus*, *Sycotypus*, *Loligo*). July 31–Aug. 5. Crustaceans (*Branchipus*, *Pandarus*, *Lepas*, *Idotea*, *Talorchestia*, *Cancer*, *Limulus*). Aug. 7–15. Vertebrates (*Amphioxus*, *Raja*, *Teleost*).

The tuition fee is thirty-five dollars, payable in advance. Applicants should state whether they can supply themselves with simple and compound microscopes, or whether they wish to hire. Microscope slides, dissecting and drawing implements, bottles, and other supplies, to be finally taken away, are on sale at the Laboratory. Further information in regard to this department may be had by addressing Professor Hermon C. Bumpus, Wood's Holl, Mass., to whom applications for admission should also be made.

The Botanical Laboratory for Teachers and Students will be opened on Wednesday, July 5. The laboratory work in botany will be restricted to the study of the structure and development of types of the various orders of the Cryptogamous plants. Especial attention will be given to the study of the various species of marine Algæ which occur so abundantly in the waters about Wood's Holl, and students desiring to give their entire attention to these plants will be encouraged to do so. The fungi and higher Cryptogams will receive less attention than the Algæ, but will be studied in fewer types. Lectures will accompany the laboratory work. The course may be outlined somewhat as follows:—

First week. *Cyanophyceœ:* Lyngbya, Calothrix, Rivularia, Stigonema, Tolypothrix, Anabæna. Second week. *Chlorophyceœ:* Spirogyra, Ulva, Enteromorpha, Chætomorpha, Bryopsis, Vaucheria, Oedogonium; *Phœophyceœ:* Ectocarpus, Mesogloia, Leathesia, Laminaria, Fucus, Sargassum. Third week. *Rhodophyceœ:* Batrachospermum, Nemalion, Callithamnion, Chondriopsis, Rhabdonia. Fourth week. *Phycomycetes:* Mucoe, Sporodinia, Peronospora, Cystopus, Achlya; *Uredinei:* Aecidium, Uredo, Puccinia, Uromyces. Fifth week. *Basidiomycetes:* Agaricus, Lycoperdon; *Ascomycetes:* Microsphæra, Sordaria, Peziza, Physcia. Sixth week. *Muscmeœ:* Riccia, Madotheca, Marchantia, Mnium, Tetraphis, Hypnum; *Filicineœ:* Dicksonia, Adiantum, Equisetum, Lycopodium, Marsilia, Selaginella.

The tuition for students in the regular course of laboratory work and lectures is thirty-five dollars, payable in advance; for students engaged in investigation the tuition is fifty dollars.

Students are expected to supply their own instruments, or to pay an extra fee for those borrowed from the Laboratory. Applications should be addressed to William A. Setchell, 2 Hillhouse Avenue, New Haven Conn.

The Physiological Laboratory will be open from June 1 to September for investigators.

Rooms, accommodating two persons, may be obtained near the Laboratory, at prices varying from $2 to $4 a week, and board from $4.50 to $6. By special arrangement, board will be supplied to members at The Homestead at $5 a week.

A Department of Laboratory Supply has been established in order to facilitate the work of teachers and others at a distance who desire to obtain materials for study or for class instruction. Certain sponges, hydroids, starfishes, sea urchins, marine worms, crustaceans, mollusks, and vertebrates are generally kept in stock, though larger orders should be filed sometime before the material is needed. Circulars giving information, prices, etc., may be obtained by addressing the collector, F. W. Walmsley, Wood's Holl, Mass.

Wood's Holl, owing to the richness of the marine life in the neighboring waters, offers exceptional advantages. It is situated on the north shore of Vineyard Sound, at the entrance to Buzzard's Bay, and may be reached by the Old Colony Railroad (2½ hours from Boston), or by rail and boat from Providence, Fall River, or New Bedford. Persons going from Boston should buy round-trip tickets ($2.85).

The Annual Report of the Trustees, containing an account of the organization and work of the Laboratory, may be obtained from the secretary, Anna Phillips Williams, 23 Marlborough St., Boston.

The officers of instruction are: C. O. Whitman, director, head professor of zoölogy, University of Chicago, editor of the *Journal of Morphology.* Zoölogy — A. Investigation, Howard Ayers, director of the Allis Lake Laboratory; J. Playfair McMurrich, professor of biology, University of Cincinnati; E. G. Conklin, professor of biology, Ohio Wesleyan University; F. R. Lillie, fellow in zoölogy, Chicago University. B. Instruction, H. C Bumpus, professor of comparative anatomy, Brown University; W. M. Rankin, instructor in zoölogy, Princeton College; Pierre A. Fish, instructor in physiology and anatomy, Cornell University; A. D. Mead, fellow in zoölogy, University of Chicago. Botany — W. A. Setchell, instructor in botany, Yale University; W. J. V. Osterhout, Brown University. Physiology — Jacques Loeb, assistant professor of physiology, University of Chicago. Ryoiche Takano, artist; F. W. Walmsley, collector; and G. M. Gray, laboratory assistant.

ELECTRICAL NOTES.

THE paper by Dr. Sumpner on "The Diffusion of Light" is one of the most important pieces of work which has recently been published, especially from the practical side. It shows us at once how to calculate the amount of light necessary to illuminate a room of any shape or size, provided only that we know the material used for decorating it. Hitherto this has been done on the happy-go-lucky plan, for, although a rule has been laid down by Mr. Preece to the effect that one candle-power should be used for every square foot of floor space, the well-known antipathy which mathematics bears to Mr. Preece has caused this formula to be looked upon with suspicion; and in this case with reason. The work of Dr. Sumpner is, however, of an entirely different class, and his results may be depended upon for making practical calculations. The principal result of his work is a knowledge of the immense effect that the material covering the walls of a room has on the amount of light required to illuminate it to a given degree. We learn that the amount of light reflected from a newspaper or piece of foolscap is equal, within 10 per cent, to that reflected from a good glass mirror. The following figures may be of interest (deduced from his results):—

I.	II.
Black cloth,	100
Dark-brown paper,	87
Blue paper,	72
Yellow paint (clean),	60
Wood (clean)	50
Wood (dirty),	80
Cartridge paper,	20
Whitewash,	15

Column I. gives the material covering the walls of the room of a given size, and column II. the proportionate number of candles necessary to light it. It will be seen that it takes nearly six times as much candle-power to illuminate a room papered with dark-brown paper as it does to illuminate to an equal degree a whitewashed room. While, of course, we cannot sacrifice æsthetics to economy, it is evident that by suitably choosing the paper of a room, no inconsiderable saving in gas bills may be effected. R. A. F.

NOTES AND NEWS.

A NEW society has been organized in Washington under the name of the "Geological Society of Washington." The officers are: President, C. D. Walcott; vice-presidents, S. F. Emmons and W. H. Holmes; secretaries, J. S. Diller and Whitman Cross; treasurer, Arnold Hague; council, G. F. Becker, G. H. Eldridge, G. K. Gilbert, G. P. Merrill, and T. M. Chatard. The members are classified as resident and corresponding, the dues of the former being $2 and of the latter $1 per annum. The meetings are held on the second Wednesday of each month from October to May, inclusive. The membership already numbers 108. The members need not be geologists themselves: to have an interest in the subject is sufficient to entitle one to the privileges of the society. Its object is the presentation of short notes on work in progress rather than the reading of elaborate papers. The first scientific meeting was held March 8, at which, after an introduction by Major J. W. Powell, Director of the Geological Survey, a paper was presented by Mr. H. W. Turner, on the Structure of the Gold Belt of the Sierra Nevada. Mr. S. F Emmons then read a paper on the Geological Distribution of Ore Deposits in the United States.

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HYDROGRAPHIC AREA OF THE RIO WANQUE OR COCO IN NICARAGUA.

BY J. CRAWFORD, CAPE GRACIAS AL DIOS, NICARAGUA.

ABOUT four miles west from the town Ocotal, capital of the Department of Nueve Segovia, in Nicaragua, at about Long. 86° 40′ west (from Greenwich) and Lat. 13° 30′ north, the waters in the large creeks Somote-grande and Maculiso, unite and form the commencement of a river, known to all persons living on its banks, for fully three-fourths of its length, from its mouth up as Rio Wanque,[1] for the remaining fourth as Rio Coco or Rio Segovia.

The general course of this river, for the first ninety miles from its commencement down to the mouth of a confluent, the Rio Phantasma, is eastwardly and from thence to its disembogue into the Caribbean Sea at Cape Gracias a Dios, is about 22° east from north, but it is very sinuous, changing its course every three-fourths of a mile to every two miles of its length as it flows rapidly near to or along the southern side of "The mountain system of New Segovia."[2]

The important creeks and rivers are herein named in the order they enter the Wanque River, commencing at the most westerly.[3]

Rio Somote-grande, rising on the south side of Dullsupo Mountain ridge,[4] and flowing southeasterly to where it unites with the Rio Maculiso, and forms the Wanque River.

Rio Maculiso, draining the southern side of the mountain range, Ococan (to the N. E. of the Dullsupo Mountains), composed in part of the mountain ridges, Maculiso, Santa Maria, and Ococan (about Long. 86° 50′ W., and Lat. 13° 20′ N.) and flowing southeastwardly until uniting with the Rio Somote-grande, and forming the Segovia or Wanque River.

Rio Depilto, receiving its waters principally from the southerly sides of the mountain ridges Ococan, Depilto, and Jalapa, and flowing southwardly, between moraine ridges for a part of its route until confluent with the Segovia or Wanque on the southeast side of Ocotal.

Rio Palacaquina, percolating from old volcanic ridges on the southwest, it flows northeastwardly until its waters enter the Wanque or Segovia at the Indian village of Telpanaca.

Rio Jicore has as its principal hydrographic area the southern sides of the mountain ridges of Jalapa, Jicore, Encino, and Murar, in the Encino Range of mountains, and also the Quilali and San Juan del Panaca Mountains in the Quilali Range, and flows southeastwardly until entering the Wanque River at Pueblo Quilali.

Rio Phantasma flows from the south, draining ridges that form the Phantasma Range in the mountain system of Matagalpa.[5]

Rio Quă, from the southwest, rising in the Quă Range in the Matagalpa System of mountains [5]

Wa-wa-lee Creek, from the northeast; it drains a part of the short ridge Ventura, in the mountain system of Nueve Segovia.

Kilambe Creek, from the south, rising from a long mountain ridge of that name.

Rio Opoteka enters the Wanque River from the northward and drains the southern side of the Opoteka Range in the mountain system of Nueve Segovia.

Rio Wanblau, from the southeast, joins the Wanque River near the head of the long series of cascades in the Wanque known as Ke-y-on; it drains the northeastern termination of the ridges Wan-blau, Keyon, and Pene Blanca (about 7,000 feet altitude above the Caribbean Sea, the highest mountain in Nicaragua) in the mountain system of Matagalpa.

Ya-male Creek flows from the west into the Wanque near the foot of the cascades Keyon.

Peas Creek, from the southeast, gathers its waters from a low ridge that is within four leagues of the Rio Bokay, to the south.

Bolemaca Creek, flowing from cerros of that name (that are composed of cryptocrystaline limestone intersected by numerous interusculating veins now filled with crystallized calcite), eastwardly into the Wanque River; the mountain Bolemaca is in the system of Matagalpa.

Oulawas Creek, flowing from the east from a cerro named Kay-ān that is composed of marble and compact limestones.

Rio Bokay, from the southeast and east, about one-half of the size of the Wanque River, drains the hydrographic area on the east side of the mountain ridges Pene Blanca and Barbar, and the north side of the Wanblau Mountains; its general course is northeastwardly, near to and parallel with the Wanque River in that part of the country.

Wylawas, Attawas, and Saccos Creeks come from the eastward, draining, through placer gold mines at their heads, a part of the western side of a long lateral moraine of glacial epoch, unstratified deposits of clays, gravels, boulders, and sands.

Six creeks, flowing eastwardly from the mountain system of Nueve Segovia, examined and names not recorded, but reported by the Sambos to have along near their banks numerous groves of large-sized mahogany, cedar, walnut, and rosewood trees.

Naga-was Creek, from the northwestern end of the long lateral moraine above mentioned, flowing northwardly through placer gold mines to the Wanque River.

Rio Wash-pook, draining the northeastern end of the lateral moraine above mentioned, and entering the Wanque from the southeast; several of its tributaries drain placer gold mines, also lodes containing gold.

From the mouth of the Rio Wash-pook, east of north and north to the Caribbean Sea, is the delta of the Wanque River, embracing several lagoons and lakes and intersected by several inter-connecting natural canals. There are three long series of cascades and low falls in the Wanque River. The most westwardly commences a few miles below (N. E. of) the mouth of the Rio Opoteka, at the locality named Ke-y-on, where the river has eroded about 3,000 feet in depth from the present altitude at the south end of the mountain ridge Opoteka, and about 2,700 feet depth from the altitude at the northern terminatoin of the Keyān Mountains. The other two long series of cascades are below the

[1] Rio "Coco," "Segovia," "Wanx," or "Wanque." Coco, abbreviated from Cookra, the name of the aborigines once living on its banks, has precedence because of antiquity. Segovia, the next oldest name, was given to it by the Spaniards, and it is now known as Rio Segovia by the Latin and North Americans and Europeans living near it and near its headwaters in the department of Nueve Segovia, and is the official name used by the Government of Nicaragua for that part of this river. At its mouth, however, it is officially referred to as Rio Coco or Wanx. Wanque is the name invariably used by the Sambos (a mixed semi-civilized people) living along two-thirds of its length from Cape Gracias up the river. Also, the Sumo Indians, living along one of its largest tributaries, the Rio Bokay, always name it Rio Wanque.

[2] So named by Élie de Beaumont. For its direction, locality, etc., see Professor Joseph Prestwich's Geology, London, 1886.

[3] Recorded in this paper because convenient, at present, for reference to locate lodes and deposits of valuable minerals and metals, and groves of valuable trees discovered near to these rivers and creeks.

[4] The locality is known as "rin on del burro" (i.e., resembling dimly a mule) and is a landmark guide in that part of Nicaragua, where no roads have been made and the paths are often dim.

[5] This mountain system was so named in 1889 by the author of this paper and examined by him on its southern ridges up to the water-dividing ridge in 1890.

mouth of the Saccos Creek and continue with short intervals between the two series, for about 75 miles. These series of cascades, the Ke-y-on, Saccos, and Naga-was, impede the navigation of the Wanque River; passing up or down over them being quite dangerous even on rafts or in the light oval-bottom canoes, the only kind of transportation at present on this river: consequently the area drained by about 250 miles of this Wanque River (including, also, the hydrographic area of the Rio Bokay, from the mouth of Rio Qua to the mouth of Rio Wash-pook) is a "terra-incognito," about which neither the citizens nor the Government of Nicaragua have any reliable information; however, rafts of logs of cedar, mahogany, walnut, nispero etc., can be safely floated over the cascades and down the Wanque River to Cape Gracias a Dios.

The soils in the area drained by the Wanque River and its tributaries, extending about one hundred miles from the Caribbean Sea coast are fertile, alluvial, delta lands, suitable for the production of plantains, bananas, bread-fruit, rice, ginger, sugar-cane, grasses (Teocinte, Guinea, and Para are indigenous), and along the sandy rim of the sea-coast cocoanuts grow to large size and are numerous on each tree. From thence up the hydrographic area of the river the lands are composed, usually, of a deep and fertile soil constituted generally of a large percentum of partly decomposed organic matter, mixed or inter-laminated, to depths of five to fifty feet, with deposits, in situ, of disintegrated and partly decomposed lime — alkali — or ferriferous rocks. These lands support dense forests of trees and jungles of vines and plants of semi-tropical and warm-temperate vegetation, and are suitable for the annual or semi-annual yield of full crops of rice, corn, ginger, bananas, plantains, also annual yield of căcă, mangoes, coffee (the Liberian species), vegetables, bread-fruit, oranges, lemons, limes, etc., and lands on the sides of mountains and their elevated planes, above 1,500 feet altitude above the Caribbean Sea or Gulf of Mexico, are suitable for tobacco, almonds, grapes, coffee (the Arabic species), corn, oranges, lemons, limes, vegetables, etc. (All the lands in Nicaragua are below the frost-line.) From the Rio Jicore, up westwardly on the area drained by the Segovia or Wanque River and its tributaries, the lands are generally sand, gravel, and clay; in this region the rocks have not disintegrated so rapidly nor to such depths by meteorological influences as they have down the Wanque River below the Rio Jicore, or the vegetable matter, alkalies, and alkaline earths have been removed by rain water. In some parts of the western headwater localities, especially just northeast of the town Ocotal, the surface lands are largely drift from moraines, and not desirable for agricultural purposes.

The climate has a wet and a dry season each year; the former continuing over the area from the sea-coast up the Wanque, to about the mouth of the Opoteka, for fully seven months. However, in a part of that time, the rain showers, although two or more times a day, are only of a few minutes' duration; for about five months in each year there are no, or but a few, showers of rain; enough, however, to counteract in part the effects of rapid evaporation.

The temperature is semi-tropical from Cape Gracias up the Wanque River to the mouth of Rio Opoteka, especially in the lower valley lands, but cooler on the mountains' sides and on the elevated planes, where the temperature varies in the year from 22° C. to 32° C. From Opoteka up to the western sources of the Wanque River, the temperature is — between low land and mountain planes — from 12° C. (sometimes) at night to 30° C. at noon, the average daily temperature being about 27° C.[1]

Several large mineral springs are found at the heads of the western tributaries to this river, some of them containing salts of the alkalies or alkaline earths, others salts of iron. No sulphur springs were discovered. Some of these are cool water, others tepid, others boiling waters. The principal localities observed, where springs of mineral waters were found, are:—

A large spring of tepid water containing sulphate of magnesia as its principal salt (and other sulphates) located one league west from the town of Ocotal, near to the margin of the Segovia or Wanque River. Several large springs of boiling water were found near the foot of an arenaceous limestone ridge, about four leagues northwest from Ocotal (rose quartz is found in the creek of hot waters, in pieces of various size, some containing a cubic foot). This creek has a temperature of about 50° C., two miles distant from the springs, where it enters Rio Maculiso.

Several large springs of boiling waters are found near the Pueblo Jalapa (on Jalapa Creek, a confluent to Rio Jicore) one of them containing lithia salts in quantities to be easily recognized.[2]

Among the most valuable and useful in the industrial arts, medicines, etc., of the flora observed in the area drained by this river and its tributaries, the principal ones, found in such large numbers and in such large size that the collecting and exporting of them could be made a profitable industry, were noted more particularly at the following named localities; these areas, however, represent only a few of the many similar groves and forests that were not entered and examined.

From the western sources of the Wanque or Segovia River, down its area to the Rio Phantasma are, in order as observed from the west, oak (quercus), pine (pinus silvestrus), sarsaparilla, ipecacuana, liquid amber, balsam (Peru and Copaiva), nispero, tamarind, mora, walnut (a variety of Ingulaus cathartica), copal-chi, and other species of cinchona, ironwood, ebony, mahogany, cedar (a variety of junipuris), guana (a cellular, endogenous tree of specific gravity near cork and compressible like cork), ginger, fibrous plants (in great variety and luxuriance), cā-cā (Theobroma, indigenous), grasses, bananas, plantains, oranges, lemons, also a great variety of beautiful orchids and flowering plants.

From the Rio Phantasma to Laccos Creek, including the area drained by the Rio Bokay and its tributaries are mahogany, cedar, walnut, ironwood, ebony, nispero, mora, guana caste, guana, cinchonas, rosewood, guttapercha, tamarind, also numerous large groves of India rubber trees (usually Syphonias) of small size (the larger ones having been felled by collectors of India-rubber), grasses (Teocinte, Para, and Guinea are indigenous), cā-cā (Theobroma), bamboo (attaining fourteen inches in diameter), ginger, bananas, plantains, etc.

From Laccos Creek to the Caribbean Sea are principally tamarinds, palms ("pebepias"), bread-fruits, and cocoanuts.

The types of man living or existing in the hydrographic area of the Wanque River of about 15,000 square miles are,—

From the western headwaters of the Wanque to Rio Phantasma, Latin-Americans and partly-civilized Indians, interspersed by a few Europeans and North Americans. (Negroes in one Pueblo, Cindad Viejo.) Their ancestors came in the 18th century from Belize or the island of Jamaica to fell cedar and mahogany trees for some company of Englishmen; the chief occupation of a majority of the people in that region being agriculture or mining on small scale and by primitive processes (i.e., sharpened sticks and machetes; no plows, no hoes).

This area embraces about 4,000 square miles and contains about 16,000 population.

From the Rio Phantasma along the banks of the Wanque River — east of north — to the Caribbean Sea are Sambos.[3] They live

[1] My reliable thermometers were broken early in each expedition to northeastern Nicaragua, i.e., in 1890, when exploring the mountain system of Matagalpa, and this year, 1892, when examining and exploring the hydrographic area of the Rio Wanque, Segovia, or Coco.

[2] Analyses of some of these springs are reported to have been made, but the name of the chemist is not given. The author of this paper has not yet had opportunity to analyze or have analyzed any of these waters.

[3] The Sambos are a mixture of Caribs, Negroes, and Europeans, the Negro predominating, although they have straight hair and nose, and exhibiting distinctly the rapid retrogressive influence towards Primates, caused by this mixture of the types above named; a small profile or sideview of the faces of a majority of this people would be mistaken easily at first glance for that of an anthropodia monkey. A part of this people, however—a mixture of Negro, Sumo, Indian, and European (usually Spanish)—are less rapid in their degenerating tendency. The Sambos have a language largely of modifications from the Sumo Indians, the English, and the Spanish. They claim allegiance to the King of the Moskoes (Mosquitoes) Indians, but have no laws, no government, no officials, no schools, no agriculture, no churches, no religious faith, no conception of a future existence after death on earth (although sometimes they bury food, clothing, and cooking utensils with their dead); without an idea of moral responsibility, they have become experts in deceptive practices; they are excellent canoe-men, navigating the river up and down through the cascades without injury to themselves or their canoes (made oval bottom in one piece from a cedar or mahogany log). They live on roots, fruits, fish, and wild animals (one variety of monkeys included). The women invariably do the greater part of the work, such as collecting wood, fruits, and roots, cooking, carrying water, etc., and "dress" in a short-sleeve jacket extending to their waists; then down to their ankles is covered by a wrapper of one piece of cloth tied by withes of bark or by pieces of vines around their waist. All are lazy, idle, and brutish.

in small communities and number about 2,000, also about forty Latin-Americans live on the banks of this part of the river and on the banks of the Rio Bokay.

On the Rio Bokay and its tributary, the Rio Amaca, there live about 300 Sumo Indians, descendants from the Cookras. Their houses are better constructed than those occupied by the Sambos; the houses of both Sumos and Sambos being only roofs of palm-leaves, tied onto upright posts of—generally—bamboo; no walls; only dirt floors.

The Sumos are a superior people — intellectually, physically, and morally — to the Sambos. They have a language modified from that of their progenitors, the Cookras (aborigines), and including some English words. They have numerals to twenty, but like the Sambos are very dull in numbers. They have no laws, no religious faith, no schools, no agriculture, no feast days. They bury cooking utensils, clothing, and food owned by their dead with the cadaver. They have small-area, community patches of bananas, which they never cultivate.

Nicaragua has no officials in the territory claimed by her from the mouth of the Rio Phantasma, northeastwardly, to near Cape Gracias a Dios. It is a lawless part of her territory.

The mountain system of Nueve Segovia, composed largely of Eozoic and early Palæozoic systems of rocks, is delineated at the earth's surface, on its northwestern side, by the Rios Patuca (emptying into the Gulf of Mexico) and Choluteca (flowing southwestwardly into the Gulf of Fonseca), from the Mesozoic, Kainozoic, and recent formations in Honduras, which have all been elevated into mountains whose meridional trend is at nearly right angles to the about 22° E. of N. and W. of S. alignment of the ranges in the mountain system of Nueve Segovia. On the south side of this Nueve Segovia system flows the Wanque River for about three-fourths of the entire length of the mountains, and at its southwesterly side is the Rio Negro (a confluent of the Choluteca, flowing around the western termination of the mountain system). It is, so far as known, a monogenetic system, and its anticlines expose Eozoic and Palæozoic formations in systems of sinuously outcropping rocks. The largest exposed areas of Eozoic and Palæozoic rocks are on the Ococan Range at the mountain ridges Maculiso, Santa Maria, and Ococan, and consist largely of granite, slates, gneiss, and extensive deposits of both iron ores (Titanite, Magnetite, and Hematite) and limestones, interstratified at two localities, and of graphite. The mountain ridges Dullsupo (southwestwardly from the Ococan Range) and of Depilto, Jalapa, Jicore, Quilali, Ventuo, and Opoteka (to the northeast from Ococan) have *cine del cerros* of Eozoic and early Palæozoic rocks, and exposed on their sides are upper Silurian, Devonian or carboniferous — including the Dios — system of rocks.

The mountain system of Matagalpa has the Wanque River at its northern, and the Matagalpa River at its southern surface boundary. Its present southwestern termination is about Long. 86° 45′ W. and Lat. 12° 36′ N. (a few leagues southwestardly from the Pueblo San Dionesia); to the southwest of this termination, on to the Gulf of Fonseca, the mountain ridges have been levelled to a low plane, on which are numerous subsequently elevated intumescent hills and knolls. In this plane the localities once occupied by the mountain ridges are now marked by the continuation,— southwestwardly,— from the present termination of the ridges to the Gulf of Fonseca, of lodes that in several places are rich in gold; and along near to the lodes are many small knolls or low ridges of pinkish and purplish colored argillaceous gangue of an aluminate of gold (containing $Au\ Al_2$) in a percentum that could be made profitable to miners. The present southeastern terminations (it has two) of this mountain system are: the most northerly, the low limestone ridge and hills terminating at the Rio Princapulka, a few miles west from the Pueblo Quequena (lower carboniferous limestone there), the other eastern termination is more southerly and is known as the Barbar Mountains, where it breaks off abruptly with jagged edges (not partly smoothed or slickened side surfaces as usual with faults). The general course of this mountain system is parallel with that of the Nueve Segovia system to the north; from its western termination, along three-fourths of its length, to the Wanblau Mountains; from thence to its eastern terminations the entire system of ranges and ridges have been bent into a crescent (the concavity facing northeastwardly), or this system of mountains deflects in a curve southeastwardly from the Wanblau Mountains and then northeastwardly to its present termination at the Barbar Mountains, excepting the limestone ridges on the north side of the system; these continue parallel with the Nueve Segovia system until they terminate near Quequena in lower carboniferous limestone.

This bend or deflection in the Matagalpa system of mountains forms the hydrographic area of the Rio Bokay, which has eroded a channel through the ridges of limestone. The central range, Pene Blanca, has an exposed longitudinal axis of Eozoic granites and gneiss, flanked usually by formations in the Palæozoic system of rocks, and those by Mesozoic or later deposits, limestones occupying a larger area in this than in the mountain system of Nueve Segovia. The Matagalpa system of mountains is probably polygenetic; the long, low, much-eroded ridges of limestone at the north side of the system were evidently elevated subsequent to the curving of the other ranges composing this system.

The minerals and metals in most common use and of the greatest value discovered in the hydrographic area of the Wanque River are, and their localities are as follows:—

In Dullsupo Mountains are lodes of ores of silver and copper.

In the Maculeso Mountains are lodes of ores of silver, tin, arsenic, antimony, also gold in the gangue in one lode examined, between two different geological formations, and it appears to be a very valuable gold-containing lode.

In the Santa Maria Ridges are lodes containing gold, and gold and ores of silver, also ores of silver and copper, and a deposit of galena and silver in a fissure 21 feet wide and traceable for more than three miles, also deposits of iron ores and limestones.

In the Ococan Mountains is a large deposit of lower Silurian or earlier iron ores (Hematite and Titanite) and limestone, the latter between two thick deposits of iron ores.

In the Depilto Mountains the lodes are rich in galena and silver, also lodes containing gold and ores of silver.

In the Leuje Hills (east about thirty miles from Dullsupo Mountains) are lodes and deposits rich in gold that is generally about 925 fine.

In the mountains of Pericon and San Juan del Panaca the lodes contain silver ores and gold, often in particles of size to be seen by the unaided eye; and further to the eastward, in the mountains (and creeks) Quilali (the Quilali is a tributary of the Rio Jicore) grains and small masses of platinum have been found associated with gold.

Crossing the Wanque River, about one mile west from the Rio Phantasma is a large strata of early Palæozoic, argillaceous slate in which are numerous veins, 1 Mm. to 2 Cm. wide, having a gangue of quartz and ores of silver, and also deposits of silver ores have been interlaminated with the slate.

From the mouth of the Rio Phantasma, northwardly to the mouth of the Rio Washpook, the difficulties and delays at present associated with the transportation of necessary instruments, provisions, etc., from place to place, are so great (even to a naturalist accustomed to excursions along and over mountains, and through forest jungle, and up and down through the cascades and "rapids" in rivers and creeks) that examinations of sufficient accuracy to record were made only at a comparatively few places, sufficient, however, to ascertain that the numerous lodes in this large area usually contain, as the principal metals, gold and ores of silver; also there are large deposits of marble and of cryptacrystaline limestone; also placer mines rich in gold have been discovered, especially in the area drained by the southern and eastern tributaries to the Rio Bokay and at the heads of Wylawas, Attawas, Laccos, and Nagawas Creeks and the southern and southeastern tributaries to the Rio Washpook.

The gold in these placer mines are in interlamina crevices in slate rocks and in superimposed stratified and partly stratified deposits of drift (sands, gravels, small boulders, and clays) that was eroded and transported by rain floods during the Champlain Epoch from terminal and lateral moraine deposits of the Glacial Epoch. The placer mines containing gold in the eastern and northeastern part of the hydrographic area of the Rio Bokay, also the placer mines of gold along tributaries of the Wylawas, Attawas, Laccos, and Nagàwas Creeks and the Rio Washpook have all been eroded and transported by currents of water from the lateral moraine (about 60 miles long and 300 to 1000 feet altitude above the level of the valleys) that extends northeastwardly from the Barbar Mountains (the easterly termination of the Matagalpa system of mountains) to the Rio Washpook; and on the southeastern side of this series of terminal and lateral moraines are the placer mines, also quite rich in gold, discovered in 1889 at Principulka.

P.S.—Since writing the above an opportunity occurred to pass through and hurriedly examine a part of "the placer" mines containing gold along one of the headwater confluents of Nagawas Creek (tributary to Rio Wanque) and they gave such results from panning as to indicate much gold in the deposits, although no satisfactory estimate of the quantity of gold in the cubic yard of the gold-containing gravels was made because the examination was hurriedly made and the "bed rock" on which the gravel deposits rested was either not reached or not examined at any place in that locality. These are drifts eroded and deposited by floods from the Glacial Epoch lateral and terminal moraines in that region.

NATURAL AND ARTIFICIAL CEMENTS IN CANADA.

BY H. PEARETH BRUMELL, GEOLOGICAL SURVEY DEPT., OTTAWA, CANADA.

IN the last report of the U. S. Geological Survey on the mineral resources of the United States, and under the heading of Cement, particular stress is laid upon the fact that there has recently been discovered in California an extensive deposit of natural cement rock, and the fact of its importance to the State is spoken of at length. The knowledge that a good, yet cheap, cement is of importance to any district has led the writer to prepare the following brief statement regarding cements in Canada.

We have in this country a practically illimitable store of materials applicable to the manufacture of natural and artificial hydraulic cements, of both of which we are now producing a considerable quantity, the production for 1891 being about 93,473 barrels of all kinds. Of this, however, the greater part was of natural cement, and the total production altogether that of the provinces of Ontario and Quebec.

Over a considerable portion of the Dominion are to be found the following materials, which are or may be used in the manufacture of cement: Argillaceous and pure limestones, magnesian limestone, marl, and clay. Of the limestones, probably the best known in Ontario is that constituting a band about eight feet thick and of Niagara age. This band is quarried along its exposure on the Niagara escarpment between Thorold and St. David in Lincoln County, and consists of a bluish-gray argillaceous limestone overlying black bituminous shales. Again, at Limehouse, in Holton County, the Niagara affords a good cement rock. The band here is nine feet thick and rests upon eight feet of bluish shales. As may be supposed, the shales underlying the cement rock in both the foregoing instances form a very distinct quarry floor, thus minimizing the danger of mixture with inferior rock. At Rynal station, Wentworth County, a similar cement rock is quarried. Many other bands of limestone and magnesian limestone in the Niagara formation in Ontario are known to possess hydraulic properties, though at present no others than those noted are being utilized.

Throughout the Onondaga formation, which is developed in Canada only in Ontario, are many beds of hydraulic cement rock, the best known being those of the Saugeen valley and vicinity and those in the neighborhood of Paris. The lower beds of the Lower Helderberg (Waterlime group) also afford impure magnesian limestones eminently suitable for the manufacture of cement.

In Eastern Ontario, cement is made from an impure limestone found at Napanee Mills, in Addington County, and in the township of Nepean, Carleton County, there is developed a bed of argillaceous magnesian limestone of Chazy age, from which the so-called "Huce cement" is made. An analysis of the crude Nepean rock gave Delesse:—

Carbonate lime	45 30
Carbonate magnesia	12.77
Alumina and iron oxide	12.52
Insoluble argillaceous residue	19.77
Water and loss	9.64
	100.00

In the Province of Quebec natural cement is made in Quebec City from a bluish-black dolomite, and at the Mountain Portage, on the Magdalen River, Gaspe County, is found a black dolomite, which is said to possess strong hydraulic properties. A similar band has also been noticed on the Grande Conde, six miles below Great Pond River, in the same county.

An analysis of the Magdalen River rock gave Delesse:—

Carbonate lime	43.17
Carbonate magnesia	32.12
Alumina with iron oxide	4.10
Insoluble (fine clay)	20.30
	99.69

Many other bands of rock suitable for the manufacture of natural cement are known in Canada, but the foregoing is thought sufficient to illustrate their geographical and geological distribution.

For the making of Portland cement, suitable clays and marls or limestones are found at many places in that juxtaposition necessary for economical and profitable working, mention will therefore be made only of those points whereat works are situated. These are, Hull and Pt. Claire, in Quebec; Napanee Mills, Marlbank, and Ocorn Sound, in Ontario. At Hull, Pointe Claire, and Napanee Mills clay and limestone are used, while at Marlbank and Ocorn Sound the cement is produced from clay and marl, which occur in quantity and of singular purity. Of the materials wherefrom the Ocorn Sound cement is produced the following analyses are available.

Marl.— Analyst, Ed. Chapman, Ph.D., Toronto.

Carbonate lime	96.41
Carbonate magnesia	1.64
Carbonate iron	0.42
Intermixed sand, clay, and organic material	1.16
Moisture	0.37
	100.00

Clay, underlying marl.— Analyst, R. R. Hedley.

Moisture	1.42
Silica	62.26
Alumina	14.70
Ferric oxide	3.22
Lime	5.28
Magnesia	0.63
Carbon dioxide	10.09
Potassium oxide Sodium oxide	2.64
	100.24

Of the various manufactured natural cements, the following analyses only are at hand:—

	I.	II.	III.	IV.
Lime	53.55	43.05	39.70	52.49
Magnesia	2.20	18.02	9.58	Traces.
Silica	29.88	28.43	—	27.40
Alumina and iron oxide	12.70	10.50	19.74	12.16
Insoluble argillaceous residue			30.98	
Sulphate lime	1.58	—	—	7.95
	99.91	100.00	100.00	100.00

I. Thorold, by Delesse.
II. Napanee Mills, by W. M. Smith, Syracuse, N Y.
III. Hull, by Delesse.
IV. Quebec, by Delesse.

As to the relative qualities and tensile strength of the various Canadian cements, it has been thought best to say nothing. as "comparisons are odious." Much information and many schedules of testing operations may, however, be found in recent reports of the City Engineers of Toronto and Montreal. In these reports the various Canadian brands are shown in comparison with most of the prominent European and American natural and artificial cements.

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.

Prehistoric Remains in America.

THERE is one fact in regard to the prehistoric and protohistoric remains of North America which does not appear to have received the attention it deserves.

If we examine carefully the descriptions and figures of these remains so far as published and attempt to classify them, we soon find ourselves forced to admit that there are two well-marked, general classes of types, the one belonging to the Pacific and the other to the Atlantic slope. The characteristics which distinguish these two classes are both numerous and well-marked. Geographically, the Rocky-mountain range appears to be the dividing line as far south as the Rio Grande, Mexico, and Central America, belonging to the Pacific slope section.

Although the remains of the Pacific division present many types, varying in the different sections, yet there is such a strong general resemblance, on the one hand, of those found from Southern Alaska south to the Isthmus (excepting a gap in California), and, on the other hand, such a strong contrast with those of the Atlantic slope as to justify the conclusion that this arises from ethnic distinctions and indicates different races. Mr. Swan has long been calling attention to the resemblance between the types of the region inhabited by the Haida Indians and the remains of Mexico and Central America, and no one who will make the comparison will fail to be convinced. Professor Dall, who has studied the manners, customs, and remains of the Northwest Coast, reaches the same conclusion. I cannot enter into details in this brief article, but ask any one who doubts the correctness of this conclusion to compare the figures given by Ensign A. P. Niblack, in his work on "The Coast Indians of South Alaska and Northern British Columbia," with those found on the monuments of Mexico and Central America, and then with the types of the Atlantic slope. It is true that the former are modern, yet the resemblance both in general character and combination to those of Mexico and Central America is too marked to be overlooked, while no such resemblance to those of the Atlantic slope is observable.

Do not these resemblances on the one hand and differences on the other have an important bearing on the question, "From whence did America (or rather North America) derive its original immigrants?" That the works of the two slopes present two distinct classes of types cannot be denied. That there is in California a break in the continuity of the types of the Pacific slope, which seems to indicate an overflow from the Atlantic side, only serves to emphasize the above conclusion. The marked similarity between the types of the Pacific slope and the Pacific Islands has been referred to by Professor Dall (3d Ann. Rep. Bur. Eth., pp. 147–151), who finds that they have prevailed "from Melanesia to Peru and from Mexico to the Arctic." In summing up, he remarks that "the mathematical probability of such an interwoven chain of custom and belief being sporadic and fortuitous is so nearly infinitesimal as to lay the burden of proof upon the upholders of the latter proposition." Professor Dall does not argue from this a common origin of the people possessing these characteristics; but believes they have been "impressed" upon the inhabitants of the western coast from the Pacific side. Notwithstanding this disclaimer, does not the evidence indicate two streams of original immigration, one to the Atlantic and the other to the Pacific coast? Ensign Niblack, although disclaiming any inference to be drawn therefrom as to relationship, gives a list of resemblances between the customs and works of the New Zealanders and Haida Indians that is certainly remarkable.

The idea that America was peopled by way of Behring Straits is somewhat losing its hold on the minds of students, and, as a usual result, there is a tendency to swing to the opposite extreme. Drs. Brinton and Hale are inclined to believe, chiefly from linguistic evidence, that the first settlers came from Europe to the North Atlantic coast. The former says in his "Races and Peoples," pp. 247–248, "Its first settlers probably came from Europe by way of a land connection which once existed over the North Atlantic, and that their long and isolated residence in this continent has moulded them into a singularly homogeneous race, which varies but slightly anywhere on the continent and has maintained its type unimpaired for countless generations. Never at any time before Columbus was it influenced in blood, language, or culture by any other race."

Now it may be that settlers came from Europe to the North Atlantic coast, but the evidence is decidedly against the remainder of the above quoted paragraph, which is, in fact, somewhat self-contradictory. For, if the settlement was at one point, by one race, and this race was never influenced by another, it is difficult to imagine in what respect the moulding process acted. However, the chief objection is to the theory of a single original element, and the assumption that it was never influenced in pre-Columbian times by any other race or element. The facts set forth by Professor Dall and confirmed by Ensign Niblack are too apparent to be set aside by any theory or mere declaration. Even without the evidence presented by these parties, the differences between the archæologic types of the Pacific and Atlantic slope are sufficient to outweigh any argument that has been presented against intrusive elements. CYRUS THOMAS.

Washington. D.C.

Some More Infinitesimal Logic

PROFESSOR BOWSER, in his reply to me in *Science*, Mar. 10, does not recognize the logic of his calculus in the example in question. The only reasons given in his calculus that would permit the use of $\cos dx = 1$ are, the axiom (?), page 12: —

"An infinitesimal can have no value when added to a finite quantity and must be dropped."

And, page 37: —

"Because the *arc dx* is infinitely small, . . . its *cosine* equals 1."

If, for these reasons, $\cos dx = 1$, then, for the same reasons, $\sqrt{2} \cos\left(\frac{\pi}{4} + dx\right) = 1$.

Four out of the five axioms on page 12 are misleading, not to say incorrect. The orders of infinitesimals or infinites to be retained in an expression do not depend upon the expression, but upon the use that is to be made of it. Sometimes we must use $\cos dx = 1 - \frac{dx^2}{2}$ or $= 1 - \frac{dx^2}{2} + \frac{dx^4}{24}$, etc. Quite prominent mathematicians have failed to do this properly in instances where they would naturally use great care. Reasoning on infinitesimals is at best of a slippery character. I have referred in my former article to an example (Ex. 3, p. 325) where Professor Bowser obtains a result that is easily verified to be incorrect; yet the logic of his work seems correct, not only to the average, but to the best students; and it must have seemed right to Professor Bowser, or he would not have inserted it.

The second proof of the differential of the logarithm, pp. 29–31, is another example of false logic. The same proof is found in Olney, p. 25; Taylor, p. 24; Hardy, p. 31; and is the only proof relied upon by some of these authors. This is quite a list of mathematicians who have indulged in infinitesimal reasoning of the value zero, and who will probably learn of it for the first time through this article. It is easily seen that the logic is false by the fact that it applies step for step when d is replaced by $\triangle$, the finite difference symbol, giving the result $\triangle \log z = m \frac{\triangle z}{z}$, with m independent of z, which is absurd.

I am not opposed to the method of infinitesimals when properly presented. It is logically only an abbreviation of the method of limits, and I should, for my own satisfaction, always want to test new results obtained by it, with the method of limits in full. I should be glad to see Professor Bowser revise his book. There are some good things in it. I trust Professor Bowser and the other authors mentioned will take my criticisms in the spirit they are intended. We are all liable to make mistakes, and if I should indulge in book-writing to any extent, there would no doubt be some sins of that kind of my own commission, especially in the subject of infinitesimals. A. S. HATHAWAY.

Rose Polytechnic Institute, Terre Haute, Ind., March 16, 1893.

Color of Flowers.

WILL some of the readers of *Science* tell me what to use for preserving the color of flowers when pressing them?

JEANNE NEAL.

Saint Joseph, Mich.

BOOK-REVIEWS.

Extinct Monsters. By REV. H. N. HUTCHINSON, B.A., F.G.S. New York, D. Appleton & Co.

THIS book is, as the author states, a popular account of some of the larger forms of ancient animal life. It is impossible to say too much in favor of the proper kind of popular science. The only argument that scientific research can advance for itself, is that the results of its work will appeal to mankind in general. Scientific investigators must therefore encourage in every possible way all attempts to render science popular and cherish every successful writer in this line. To write popular scientific works is an extremely difficult matter, and there are few in the world who are capable of it. The scientist who is best familiar with the facts is usually either unable to put his facts in a form to be enjoyed by the general reader, or is afraid of losing caste among his friends by doing so. But there is no scientist who does not hail any popular exposition of scientific truth.

There are two faults into which a writer of popular science is liable to fall. If he is too much of a scientist he becomes too technical, and if he is not enough of a scientist he becomes too discursive and too much inclined to fill his pages with rhetorical flourishes. The present book does not fully avoid either of these two faults. At times the reader is led along through a series of rhetorical exclamation points and feels that the author is endeavoring to amuse rather than instruct; and at other times he finds technical terms used which he certainly cannot understand in their proper significance. The book aims to reach those unacquainted with geology, but assumes a knowledge of the succession of geological ages and considerable familiarity with the different strata of rocks. Probably the book would be more instructive if the author had treated his subjects in a little more systematic way, and had not been quite so desirous of introducing popular names on one page to please his non-scientific readers and scientific names on the next page to satisfy his sense of scientific consistency.

But, in spite of the trifling inperfections, it must be stated that this book is an emphatic success as a bit of popular writing. The style is easy and interesting. When one takes up the book, he is inclined to read page after page and chapter after chapter without any desire to lay the book down. The author has skilfully interspersed striking incidents connected with the discovery of special fossil types in such a way as to add vivacity and life to the whole.

The most valuable and interesting part of the whole to all must be the figures in which the book abounds, drawn by J. Smit. These figures are partly skeletons, and represent our present actual knowledge of the hard parts of the extinct monsters as collected in the museums of the world. But the figures which will most appeal to the reader are the restorations of these ancient monsters in the flesh. Of course, restorations of extinct monsters have been made many times, and they have been constantly changed as new facts are discovered. The author would not pretend that his restorations are final. but it can be claimed fairly, and will be easily admitted, that the restorations, as given in the figures of the present book, are the best that have been made up to the present time, and are certainly nearer the truth in each case than those which have preceded them.

One is very naturally inclined to feel, after a cursory reading of this book, that the ancient world was filled with nothing but monsters, and perhaps the author would have given a better picture of ancient life if he had interspersed with his monsters some of the smaller but no less interesting types of animals. But, on the whole, the book is a success as a bit of popular writing, and can be recommended to all.

Advanced Lessons in Human Physiology. By OLIVER P. JENKINS, Ph.D. 60 cents.

Primary Lessons in Human Physiology. By the same author. 30 cents. Indianapolis, Indiana School Book Co.

THESE two little books are published in the Indiana State series of school text-books, and are designed, one for primary schools and the other for advanced schools. One is glad to see a departure from the plan of teaching simple anatomy and the introduction of a physiological basis of treatment. The physiology of man is studied from the standpoint of general biological truth, and the student may here actually learn something of the laws of life.

Interpretation of Nature. By PROFESSOR NATHANIEL S. SHALER. Boston, Houghton, Mifflin & Co. $1.25.

IT is not very common that a person of as much scientific repute as Professor Shaler of Harvard ventures even indirectly to discuss in print the question of the relations of science to theological problems, and for this reason there is especial interest in the pages of this little book. Professor Shaler, in his preface, tells us that his first contact with natural science had the effect of leading him far away from Christianity, but that of late years further insight into the truths of nature have forced him back again towards the grounds from which he had departed. The body of this publication is a discussion of various problems of natural science for the purpose of pointing out how it is that the discoveries of science fail to be in themselves a satisfactory answer to man's questions as to the philosophy of nature. The different chapters of the book are not and do not pretend to be arguments upon the subject of the relation of theology and science. They are rather thoughts upon certain phases of scientific truth and a general inference as to lack of satisfaction which the mind can find if it rests in scientific truths alone. He discusses the general appreciation of nature historically, and then more in detail the general subject of biological evolution, especially in its philosophical aspect. The general conclusion of the whole is as to the lack of a satisfactory foundation for thought in science itself, and the unavoidable feeling which must come to a thoughtful student of some power unknown and lying deeper than the phenomenon which science studies on the surface. Even in regard to the scientific aspect of the doctrine of Christianity, Professor Shaler tells us that "the doctrine of Christ is the summit and crown of the organic series." One cannot but be forcibly reminded of Spencer's grand generalization that scientific fact and religious thought are both truths, and that the final outcome of study is to be a fundamental union of the two.

This book of Professor Shaler's will be especially interesting to two classes of readers. First to those who have passed through somewhat of this same mental history as that which Professor Shaler points out as his own. This will include a body of scientists who had learned to look deeper than the phenomena and to wonder concerning the underlying truths, a class of thinkers which seems to be a growing one at the present time. A second class is the great body of readers who are and always have been in thorough sympathy with religious teachings, and will rejoice to see a scientist of such high standing taking a position so in harmony with the most advanced religious truth. While, on the other hand, that body of scientific thinkers who repudiate all forms of theological truth will probably fail to have much sympathy with the conclusions reached in these pages. No one, however, who has a thoughtful mind can fail to find much of interest and significance in this trenchent discussion of the interpretation of nature by Professor Shaler.

Formation of the Union, 1750–1822. By ALBERT BUSHNELL HART. New York, Longmans, Green & Co. $1.25.

THIS is the second volume in the series of Epochs of American History, and is by the editor of the series. It is written in an excellent narrative style, clear and bright, and much more carefully finished than the style of most of our younger historians. It is not well adapted for beginners, since it can hardly be understood and appreciated without some previous knowledge of the period it covers. For those who possess such knowledge, however, even in outline, this book will be both entertaining and useful. It is devoted, as the author in his preface remarks, to "the study of causes rather than of events, the development of the American nation out of scattered and inharmonious colonies." Though it embraces the period of the Revolution and the War of 1812, it contains very little military history, the author holding that, though military movements are of great interest to professional soldiers, "the layman needs to know rather what were the means, the character, and the spirit of the two combatants in each case, and why one succeeded where the other was defeated." The causes of the Revolution are set forth with great clearness in a brief space; the true character of the struggle is pointed out; and the reasons for the success of the Americans are made apparent. Then follows a lucid exposition of the difficulties and distresses which showed the necessity of a stronger national government, and of the successful efforts of the wisest leaders in framing and establishing such a government. Professor Hart, however, saw clearly that, though the Union was now formed, it was not yet securely founded; and so he follows its fortunes through the trying periods of Washington's, Adam's, and Jefferson's administrations, and even for many years after. The result is a philosophical view, comprehensive and clear, though necessarily brief, of the formation of the Federal Union and of its early struggles for recognition abroad and security at home. The growth of the national territory from the peace of 1783 to the last acquisition from Mexico in 1853 is shown in a map, and several other maps illustrate other aspects of the period under review. We commend Professor Hart's book to students of American history as an excellent review of an important period.

Proof of Evolution. By NELSON C. PARSHALL. Chicago, Charles H. Kerr & Co.

THIS little book is one of a series of popular lectures given before the Brooklyn Ethical Association. As a popular lecture it was bright, interesting, and instructive, though somewhat flippant and inclined to sacrifice logic for effect. One cannot but regret that the author ever committed it to print. It tries to cover the whole ground of evolution, astronomical, geological, and biological, and all in the course of 60 brief pages. One cannot but have a feeling of dissatisfaction upon reading the book. The subjects are of necessity too briefly treated to be intelligible, and show too frequently a failure of appreciation of the results of recent science. Perhaps the book may have one purpose that the author desires, of making its readers hungry for more, but it certainly cannot give one any adequate idea of the subjects outlined.

AMONG THE PUBLISHERS.

THE Open Court Company of Chicago have published a book by Dr. Paul Carus, entitled "Truth in Fiction." It consists of twelve short stories of various types, and all designed to impress some moral or philosophical lesson, and particularly to illustrate

CALENDAR OF SOCIETIES.

Anthropological Society, Washington.

Mar. 28.—Major John W. Powell, A System of Psychology (continuation of former paper).

Biological Society, Washington.

Mar. 25.—L. M. McCormick, A Hybrid Between Pyranga rubra and Pyranga erythromelas; E W. Doran, Development of the Intestine of Tadpoles; Theobald Smith, The Bacteriology of Potomac Water and its Bearing on Sanitary Problems; B. T. Galloway, Experiments in Preventing Rusts Affecting Cereals.

Philosophical Society, Washington.

Apr. 1.—O T. Mason, The Philosophy of Folk-Lore; W. H. Dall, A Miocene Climate in Arctic Siberia; F. H. Bigelow, The Model Globe, Showing the Magnetic Forces that Produce the Diurnal Variations of the Needle.

New York Academy of Sciences, Biological Section.

Mar. 13.—Professor T. D. Quackenbos, in a paper on the Saibling of Lake Sunapee, distinguishes in this a fourth variety of New England charr, demonstrating that the present abundance of this Salvelinus is accounted for not from its introduction and natural increase, but from destruction of inimical forms within recent years, which has given a greater available food-supply. Professor G. S. Huntington, on "Anomalies of Pectoralis major and minor," referred to the value of these as often presenting reversions. He emphasized the evolutionary tendency in man to proximalization of the points of attachment of the shoulder muscle group, referred to cleavage variations in anterior portion of brachio-sphalic sheet, and compared these with ontogenetic characters in anthropoids. Human anomalies in this group are best interpreted by cynocephaloids, and not by the higher forms. Professor E. B. Wilson, "On Regeneration and the Mosaic Theory of Development," presented a brief critique on the latest results of Roux and Weismann.

and enforce the special doctrines of the author. Some of them are directed against philosophical agnosticism, a doctrine for which we have as little respect as Dr. Carus has; another shows the folly of the antagonism between laborers and their employers; while still another, which we cannot regard as very successful, illustrates the author's disapproval of utilitarian ethics. The best of them all are those in which some distinctly moral lesson is drawn, especially "The Chief's Daughter," which tells how a certain tribe of savages were led to abandon the custom of human sacrifices, while at the same time it shows the moral superiority of the spirit of man over the blind forces of nature. We do not agree with all of Dr. Carus's views, but we are always pleased with the moral earnestness and the desire to be useful which characterize all his works.

— Students of American geology and paleontology are well aware of the importance of the Memoirs of T. A. Conrad on the Tertiary Fossils of the United States. These memoirs are practically out of the market, very few copies exist even in private libraries; while few of the fossils figured in them are figured elsewhere. Mr. Gilbert D. Harris, at the Smithsonian Institution, is projecting a reprint of the Eocene or earlier volume, and it has been thought that the more extensive and later Miocene monograph might appropriately be issued by the Wagner Free Institute of Science, Philadelphia, provided a sufficient number of subscriptions shall be received to measurably cover the expense. It is proposed to reprint the text of the "Medial Tertiary" (about 100 pages) verbatim; to reproduce the original plates by a process of photo-engraving, and to insert a brief introductory chapter and a table showing the present state of the nomenclature of the species contained in the work; the whole forming a volume in octavo of about 150 pages with 49 plates. It is obvious that all libraries of reference and students of geology and paleontology will find the work indispensable; and it is hoped that the response will be such as to render it practicable to undertake the reprint without delay. Professor Wm. H. Dall, Paleontologist to the U. S. Geological Survey, has consented to supervise the reprinting, with the collaboration of Mr. Gilbert D. Harris, and to supply an introduction. If subscriptions can be obtained for 150 copies, at $3.50 each, including postage, the work will be undertaken, although that amount will not repay the expense of the publication. Subscriptions may be addressed to the Wagner Free Institute of Science, Philadelphia, Pa.

— Charles L. Webster & Co. have published a new work by Henry George, in which he criticises Herbert Spencer's utterances on the land question. It is entitled "A Perplexed Philosopher," and assails Mr. Spencer for having changed his views without adequate reason. In his work on Social Statics, which he wrote in early manhood, Mr. Spencer maintained essentially the same theory about property in land that Mr. George holds now; but in his later writings he has repudiated that theory, and now advocates the system of individual property in land as in everything else. Mr. George is able to show that some of his opponent's reasons for his change of view are not conclusive; but he goes much further and charges him with intellectual dishonesty and with the desire to curry favor with the British aristocracy. We don't believe, however, that Mr. George's charges will find acceptance except with the fanatical advocates of his own doctrines; for Mr. Spencer's change of view admits of a much more reasonable explanation. He has been for many years the staunchest upholder of individualism in all its forms and a violent opponent of socialism and of all efforts to extend the influence of the State; and it is obvious that the advocate of such doctrines could not long continue to favor the communal ownership of land. Mr. George's criticisms are incisive, and, as we have remarked, some of them are well taken; but we doubt if his book will change any man's views on the question in controversy, or help in the least to make his own doctrines more acceptable.

Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

For sale—A Zentmayer new model U. S. Army Hospital monocular stand. Price $110, will sell for $75. Address H. C. Wells, No. 151 Broadway, New York.

For sale—A complete set of the third series of the American Journal of Science (1870–1893) handsomely bound in single volumes in dark brown half morocco. Address G. H. Williams, 803 Cathedral Street, Baltimore, Md.

For sale, or for exchange for books on medicine or surgery, new editions only, a large geological library, containing nearly all the State and Government Reports since 1855. Will be pleased to answer letters of inquiry and give information. Address R. ELLSWORTH CALL, Louisville, Ky.

For exchange.—Slides of Indian Territory Loup Fork Tertiary Diatoms for other microscopic fossils. Address S. W. WILLISTON, Univ. of Kansas, Lawrence, Kans.

For exchange.—Will exchange an "Ideal" Microscope of R. and J. Beck, London, 2 eye pieces 3 objectives, 3 inch, 1 inch, 1-6 inch; bull's eye condenser on stand, substage condenser, mechanical stage, etc., for any of the leading makes of type writers. Particulars by mail. DELOS FALL, Albion College, Albion, Mich.

Sale, or exchange for similar material; Diatoms (Isthmia nervosa), unmounted, from San Francisco Bay. M. J. ELROD, Bloomington, Ill.

For sale or exchange. —I have a few copies of my translation of "Strasburger's Manual of Vegetable Histology, 1887," now out of print, which I will send post-paid for $3 or for one dozen good slides illustrating plant or animal structure. Address A. B. Hervey, St. Lawrence University, Canton, N. Y.

The undersigned has the following specimens to exchange for crystals of any eastern or foreign localities or Indian relics: tin ore, metacinnabarite, stibnite, garnierite, calenanite, hanksite, ulexite, rubellite, lepidolite, blue and green onyx, Cal. pineite, aragonite on chalcedony, cinnabar, double refracting spar, clear and clouded, and others. J. R. Bush, care of General Delivery, Los Angeles, Cal.

For sale or exchange.—A private cabinet of about 200 species of fossils, well distributed geologically and geographically. Silurian, about 40; Devonian, about 50; Carboniferous, about 80; others, about 30. Frank S. Aby, State University, Iowa City, Ia.

Wants.

THE undersigned desires specimens of North American Gallinae in the flesh for the study of their pterylosis. These species are especially desired: *Colinus ridgwayi, cyrtonyx montezumae, deudragapus franklini, lagopus welchi, tympanuchus cupido* and *pedioecetes phasianellus.* Any persons having alcoholic specimens which they are willing to loan or who can obtain specimens of any of the above are requested to communicate with Hubert Lyman Clark, 3922 Fifth Avenue, Pittsburgh, Pa.

WANTED—Second-hand copy Tryon's Systematic Conchology. Please quote condition, date, price. H. L. Osborn, Hamline University, St. Paul, Minn.

A COMPETENT TEACHER of botany in college or university is open to engagement. Address L., Box 86, Rochester, Mich.

CAN any one inform me as to the age to which cats have lived? I have one twenty years old. Edward D. Webb, 132 W. Eighty-first St., New York.

WANTED — Second-hand. Foster's Physiology, Balfour's Comparative Embryology, Claus & Sedgwick's Zoology, Flower's Osteology of Mammalia, Vine's Physiology of Plants. Please state editions and prices asked and address Richard Lees Brampton, Ontario, Canada

WANTED.—American Journal of Conchology, seven volumes. Parties having these for sale will please address the undersigned, stating condition and price. R. Ellsworth Call, Louisville, Ky.

A GRADUATE ENGINEER will give instruction evenings in geometry, trigonometry and surveying, mechanics, physics, mechanical drawing and general engineering construction. Five years' experience in field and editorial work on engineering journal. References furnished. C. S. H., 102 Tribune Building, New York.

WANTED.—By well-qualified and experienced science master and associate of the Royal School of Mines, London, aged 26 (at present in England), a mastership in technical college or university for any of the following subjects: Engineering sciences, geology and mineralogy, physics, chemistry and metallurgy, etc., etc. Can provide excellent references and credentials. Apply, J. G., 17 Sussex St., Rochdale, England.

SCIENCE

Eleventh Year. Vol. XXI. No. 531. | April 7, 1893. | Single Copies, Ten Cents. $3.50 Per Year, in Advance.

Contents.

Entered at the Post-Office of New York, N.Y., as Second-Class Mail Matter.

QUERY.

Can any reader of *Science* cite a case of lightning stroke in which the dissipation of a small conductor (one-sixteenth of an inch in diameter, say,) has failed to protect between two horizontal planes passing through its upper and lower ends respectively? Plenty of cases have been found which show that when the conductor is dissipated the building is not injured to the extent explained (for many of these see volumes of Philosophical Transactions at the time when lightning was attracting the attention of the Royal Society), but not an exception is yet known, although this query has been published far and wide among electricians.

First inserted June 19, 1891. No response to date.

N. D. C. HODGES, 874 BROADWAY, N. Y.

SCIENCE

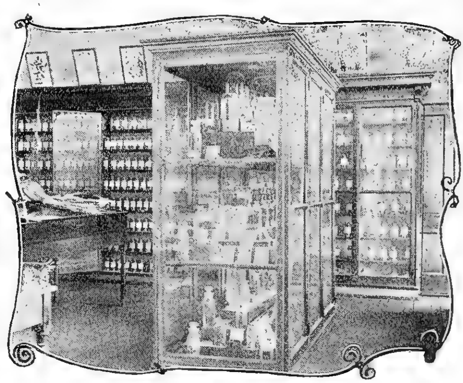
NEW YORK, APRIL 7, 1893.

THE WORK OF A BOTANICAL LABORATORY IN PHARMACEUTICAL MANUFACTURE.

BY JOHN S. WRIGHT, LABORATORIES, ELI LILLY & CO., INDIANAPOLIS, IND.

BOTANY is constantly growing in economic importance, each year we see some new and valuable practical application of it in industry or science. The many botanical laboratories maintained in connection with agricultural experiment stations testify as to its important work in agriculture. Brewers have seen so much practical value in botanical study of yeast that laboratories for this purpose have in some instances been established in connection with their plants; while it plays such an important part in the study of disease that many well-equipped hospitals and quarantine stations are provided with facilities for botanical investigation in the line of bacteriology.

GENERAL VIEW IN LABORATORY.



CRUDE DRUG COLLECTION AND MUSEUM CASE.

In pharmacy, botany has always occupied an important place, our first drugs were of botanic origin, and each year has added new plant products to drug-lists until now the hundreds of botanic drugs make, at least, a fundamental knowledge of botany a requisite for scientific pharmaceutical work, so that it now forms a part of every thorough course of study in pharmacy.

Though it bears this close relation to pharmacy, many persons, who should be acquainted with the facts, ask what service a botanical laboratory can render a pharmaceutical manufacturing establishment, so this article shall attempt to explain briefly the equipment and work of such a laboratory; as the writer is aware of the existence of but one such in this country, the article is of necessity an account of it.

Facilities for good systematic work are of the highest importance in such a laboratory, and in the one referred to there exists an herbarium of many thousands of species, representing the flora of the United States, and containing numerous forms from Europe and other foreign regions. An essential feature of the laboratory equipment is a jar collection of crude drugs representing nearly all the authenticated botanical products which enter into the manufacture of medicines. In connection with this, a line of crude drug adulterants is being collected.

A dissecting microscope, a good compound microscope with line of eyepieces, objectives, and other accessories provide means for microscopic work. A hand microtome, a large Bausch & Lomb laboratory microtome with reagents, and all necessary materials for staining and mounting sections comprise the outfit for histological work.

In addition to the study of herbarium specimens and cured plants, means are provided for work upon living plants, through the erection, in the laboratory, of a large glass propagating case, in which seeds are germinated and plants grown for study.

The laboratory is supplied with current botanical and microscopical journals, with standard texts and manuals on botany and pharmacy for reference. Briefly, in its equipment, the laboratory is not essentially different from that of any college in which systematic and structural botany are taught, except in point of number of workers for which it is arranged.

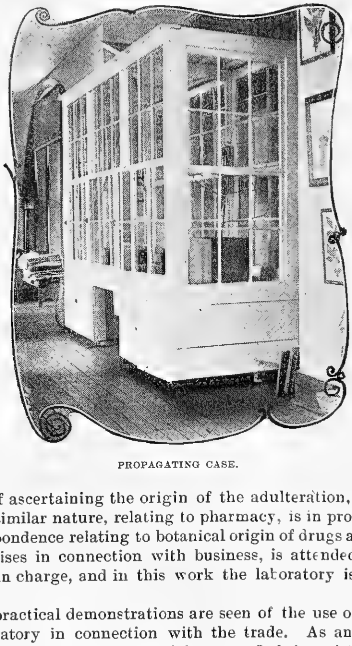
The laboratory was founded to provide accurate and scientific means for examining and identifying crude drugs. Many hundreds of botanic products find their way into medicine. These include roots, barks, leaves, flowers, fruits, and, in many instances, entire plants. As the list comprises drugs of widely diverse values and effects, and many are liable either to intentional or unintentional adulteration and substitution, it becomes necessary to give each lot of drugs received careful inspection. These examinations are made by the botanist in charge, and, in cases of the least doubt, literature giving the physical appearance of the drug is consulted; the herbarium sheet specimen is used, and, if the material be leaf, flower, fruit, or herb, it is easily proven to be false or true. After identification, the drug is further examined as to its physical condition, compared with the standard jar specimen, and, if found to be in proper condition, labeled and passed into stock.

In all this work the herbarium has a constant use and is essential to careful examination of many drugs. It is highly valuable in the detection of substitutions and adulterations, and in many cases is the only means by which the determination of these sophistications is rendered possible. While it is important to know of a substitution or an adulteration, it is almost equally important to know of what it consists. Were the herbarium less general in its character, including only recognized medicinal plants, a great part of its usefulness would be destroyed, as only by careful comparison can some vegetable adulterants be located in their proper genus or species, the parts being generally too fragmentary for using ordinary methods of determination.

Many drugs are received, the physical appearance of which alone is not a safe criterion for verification; barks, leaves, stems, and roots often arrive in a crushed and broken condition, which renders it very hard to tell whether or not they are what they purport to be. In such cases the appeal is to the microscope, and here an outfit for histological work has its use. Sections are prepared; the cell-structure and arrangement of tissues almost unerringly reveal the identity of the material. A set of slides of the officinal parts of plants has been commenced and will form a valuable part of the laboratory's equipment.

In the examination of powdered drugs, the compound microscope is indispensable; crystals, starch granules, and fragments of cells often betray adulterations at a small outlay of labor. In addition to the use of the microscope in drug inspection, it is a necessity in investigation along the line of pharmaceutical botany.

Interesting and practical results are expected from the cultivation of medicinal plants in the propagating case. Medicinal plants are grown from the seed with the purpose of learning more about their life history; seeds of adulterants are germinated



PROPAGATING CASE.

in hope of ascertaining the origin of the adulteration, and other work of similar nature, relating to pharmacy, is in progress.

Correspondence relating to botanical origin of drugs and plants, which arises in connection with business, is attended to by the botanist in charge, and in this work the laboratory is of much service.

Daily practical demonstrations are seen of the use of a botanical laboratory in connection with the trade. As an aid in the examination of drugs alone the laboratory finds its existence justified. As a means for investigation, it has great value, and through such means alone can some things, very important to the trade, be worked out.

The discoveries and determinations of adulterations of jalap, cubebs, Arnica flowers, Calendula flowers, and many other important drugs was only possible because botanists worked upon them; it cannot be said to have been otherwise, as pharmacists who have made these discoveries have had botanical training, used botanical methods, and succeeded in so far as they were good botanists.

The laboratory referred to in this article was founded primarily to provide accurate and scientific means for determining plant products used in manufacture; the acquisition of an herbarium which includes representatives of families and genera not medicinal, the provision for plant culture, histology, and microscopy is in recognition of the fact that botany in a broad sense has a direct and practical bearing on pharmacy.

ON THE EMERGENCE OF A SHAM BIOLOGY IN AMERICA.

BY CONWAY MACMILLAN, UNIVERSITY OF MINNESOTA, MINNEAPOLIS, MINN.

THOSE whose attention, during the past fifteen or twenty years, has been directed towards the various phenomena attendant upon the establishment and modification of university *curricula* will scarcely have failed to notice, in certain quarters, an interesting eructation of courses in biology. Upon even a casual examination these courses, in almost every case, turn out not to be courses in biology at all, but courses in zoölogy masquerading under an attractive but deceptive name. Chairs of biology occupied by men practically ignorant of one-half of the content of the science they profess to teach are not unknown in institutions otherwise altogether reputable. This ignorance of theirs is not merely the normal failure to push beyond the beach-line of the great unknown ocean of truth, but is a failure to comprehend or admit that the ocean extends away equally in both of two directions rather than in one alone. When one remembers how intolerant are most men of liberal education when they discern through the thin veil of pretence the deformity which it tries to hide, it seems remarkable that more vigorous protests have not already arisen against the sham biologist and the sham biology. It is because the writer believes that opportunities for a development of the true biology are lost, sometimes, through the mistaken acceptance of the sham, that he ventures upon the unpleasant task of pointing out what, after careful examination, seem to him the places where the healing cautery should be applied.

First of all, it is important to note what should be the proper limitation of the term "biology." Historically and etymologically it is still to be defined as by Lamarck and Treviranus — both distinguished botanists — who invented it. It is indeed the science of living things; it is that vast mass of knowledge bearing upon the organized world of plants and animals. Biological science is therefore to be set over against physical science in the broadest sense, and is to be considered as a generic name, under which are grouped the specific sciences of botany, zoölogy, and doubtless also psychology, if that is to be considered as co ordinate with zoölogy rather than as one of its subdivisions. Here, then, is the proper definition: "Biology is the science of living things." These are the two groups of subject matter: Plants and animals.

In Germany, and sparingly elsewhere in Europe, a limited and secondary meaning is imparted to the word "biology." Of this use an excellent example is furnished by Wiesner,[1] who groups together the various phenomena of inter-relation between plant and environment under the name of *Pflanzenbiologie.* To this restricted use of the term, Strasburger[2] very properly objects, characterizing it as "fälschlich bezeichnet." This employment of the term, as if it were synonymous with Œcology, does not, however, seem to be prevalent in America, where is to be found the third and most misleading use of the word — as generally exclusive of botany and sometimes also of zoölogy. For example, at Columbia College their exist together departments of botany and biology,[3] and, upon examination of the courses offered in "biology," it appears that they are almost purely courses in animal biology, and indeed this modified term is quietly brought forward in a foot-note. At Columbia College, then, it is apparent that the subject of botany, since it stands by itself under its own organization, is supposed, at least by the "biologists" of that institution, to be quite without the pale of their own science. And a further examination of the circular shows that the biological work is in the hands of zoölogists, both the professor-in-charge and the adjunct-professor being known to the scientific world only through zoölogical research and not through botanical.

The department of biology, then, at Columbia College seems to the writer to have false colors flying at the mast-head.

It is concerning the false use of the word "biology" in some American institutions that I wish particularly to speak. I have

[1] Biologie der Pflanzen, Wien (1889).

[2] Bau und Verrichtungen der Leitungsbahnen, vorwort viii., Jena (1891).

[3] Columbia College Circular of Information, Pt. iv., pp 44-45 (1892-93).

not at present time to discuss the fundamental absurdity of courses in "general biology"—as if it were possible to plunge boldly into comparative study of plants and animals before one has studied plants and animals themselves. It is as if one should enter upon analytical statics and follow it up by geometry and the calculus. The peculiar badness, upon the botanical side, of all so-called text-books of general biology is sufficient to emphasize the point — at least upon the minds of botanists. It is, indeed, impossible to write anything of value upon any subject in which one is not somewhat of a specialist, and the inability of zoölogists to say something worth reading upon the anatomy of *Pteris*, for instance, is not at all to their discredit, but merely marks them as of common flesh with the rest of mankind.

Harvard University is probably the innocent cause of the biology heresy which of late years has spread over the country. With that openness of vision and clearness and accuracy that has from early days characterized what, in biological lines, must be universally recognized as the first institution in America and one of the first in the world, there has not yet appeared any trace — that I am aware of — of the false or sham biology. The two sister-sciences of zoölogy and botany, each splendidly equipped both in the matter of laboratories and libraries, and men, have there developed side by side, as have physics and chemistry in most of the American universities. Botanical science, especially, with its millions of capitalization, has found a congenial home at Harvard. And precisely here seems to have been the difficulty. The endowment, the gardens, the laboratories, the museums, the libraries, the men were not to be easily had by any new institution that might spring up. And yet if the new institution were to be ambitious, it could not willingly see itself in a confessedly subordinate position. How then, without the lavish expenditure of wealth, was the dilemma to be faced?

One finds in the register of a well-known Maryland university [1] a confession of the truth concerning botany, where it is stated, "a third permissible line of specialization commencing at this stage, namely, botany, has always been contemplated since the organization of the biological department, but as yet is not available because of lack of money." While frank confession is held to be good for the soul, it is not certain that higher moral value would not have attached to an honest naming of the zoölogical courses that *were* provided for.

This acknowledged inability of Johns Hopkins University to provide a well-balanced course in biological sciences, together with the unwillingness of that institution to expose her weakness has led to much of the sham biology work that springs up from time to time over the country. The so-called department of biology there is manned by zoölogists, and the men who graduate — many of them honestly enough mistaken — are ready to take upon themselves not the name that belongs to them but that of "biologist" An interesting example of the large views of biological science which may develop in the Johns Hopkins doctor of philosophy lately came under my notice and has some illustrative value. A certain "biologist," some time since, published a pamphlet supposed to convey information concerning biological instruction in America. I do not know what the zoölogists thought of it, but it received a very chilling reception at the hands of the botanists.[2] On account of the particularly shabby treatment accorded the botanical work of the University of Minnesota, I took occasion to administer a mild rebuke to the author of the pamphlet. In reply I was assured that, while he had studied at Johns Hopkins University, he had learned that botany was of value "for teaching children." The cool effrontery of this would have surprised me had I not known the marvellous, sometimes continuous, sometimes sporadic, always insular capabilities of the Johns Hopkins biologist for blatant philistinism in regard to things botanical.

Were it not for the injustice worked upon young men attracted by such wrecker-light use of the word "biology," and, hopelessly injured in their conceptions of what they suppose to be their specialty, it would be far from my thought or wish to draw attention to any weakness in an American university. All know that the struggle for existence has its meaning even for the universities as for other organisms But protective mimicry in a university curriculum is not a pleasing phenomenon. In this particular case too much is at stake, both for the botanist and for the zoölogist, to make science the virtue that it generally is. In days of sharp specialization, such as those in which we live, it must be a source of regret and alarm to well-balanced zoölogists to see so many of those who might be ornaments to their profession led astray by a will o' the wisp chase after the unattainable. Better far to be a respectable zoölogist than a biologist with only one cerebral hemisphere. And the botanists, too, seeing what delusions may gain currency, are dismayed at the spectacle of some distinguished zoölogist perpetrating a confidence-game upon a board of trustees, assuring them that he proposes the establishment of a biological department, and then appearing with little more than mere zoölogy. The most alarming thing of all, both to zoölogist and to botanist, is that, after successfully establishing a school or department of zoölogy under the false name of biology, it should be possible for the mental vision of the founders to become so curiously warped that they will insist with vigor and with all the air of a righteous enthusiasm that the school or department actually *is* biological and that instruction really *is* given in biology. For, to the zoölogist, this must indicate one of two things, either that his *confrere* is unable to comprehend what biology is, or that he is ambitious, in regular old-style, eighteenth-century regardlessness, to announce himself a polymath, and therefore, perforce, a smatterer. And, to the botanist, it indicates the willingness of the "biologist" to make use of means that cannot with self-respect be duplicated by himself in the pushing forward of one line of biological science at the expense of the other.

Fortunately, in America the sham biology has as yet an uncercertain foot-hold. At such institutions as Harvard, Pennsylvania, Cornell, Michigan, Minnesota, Leland Stanford, it has no standing. The only critical point which need be particularly considered at present is the new Chicago University. Here one sees again the anomaly of an able animal morphologist announced in the *Programs* as a professor of biology, and one's suspicions are aroused that the same sad blunder is to be made in the west which has already disfigured the biological work of at least one eastern institution of learning. In the announcement of biological work,[3] one finds an exceedingly fair presentation of the illogical character of a "school of biology," and the promise is made that in a few years the school will probably be broken up into several departments. The definition of biology is offered, and one finds it unimpeachable. Apparently, however, there is even here a danger, for, when one turns a page or two, it appears from the classification that botany is held to be co-ordinate with neurology or animal physiology, rather than with zoölogy in the broadest sense. This error in classification is perhaps an inadvertency and perhaps a natural enough one-sided grouping, such as might perhaps be expected of some specialist in a botanical line if he were to try his hand at the organization of a school of biology with zoölogy "not yet provided for."

It is probable that, after all, the better way to develop well-balanced departments in biology is to place the task in the hands of both botanists and zoölogists rather than in the hands of either. There will then be scarcely so much danger of narrowness of view impeding the freest and best evolution. At any rate, this is the plan which has succeeded so brilliantly at Harvard University, and the other plan is the one that has failed so grievously at Johns Hopkins University. It will be a matter of regret if Chicago is really willing long to preserve the present unfortunate attitude, for it must be confessed that the instruction now offered there under the name of Biology is, after all, the half-science, the sham biology.

I make the point that, for educational purposes, "biology" is either a superficial smattering of natural-history facts and methods — and in this case not of any value — or a strong, uniform presentation of the facts both of botany and of zoölogy — and in this

[1] Johns Hopkins University Register, 1891–92, p. 113.

[2] Botanical Gazette, editorial, vol. xvii., p. 260.

[3] Programme of Courses in Biology, University of Chicago (1892–93).

case a very different thing from a sham biology which is principally, or all, zoölogy.

THE AURORA.

BY W. A. ASHE, F.R.A.S. (RETIRED), QUEBEC, CANADA.

SOME notes resulting from a study of the Aurora extending over many years, and pointing out how some of the better known theories fail to account for known conditions of the phenomena, may interest the readers of *Science.*

I regret, that after having endeavored to show how the present theories fail, that I have no theory of my own to advance. I have done a good deal of theorizing on different subjects, at intervals in a somewhat busy life, so that there are few who have a better opportunity of knowing how deceptive evidence is which is *sought for* to support a theory; in other words, how faulty—yet how plausible—the result, when the observed facts are (unconsciously) made to fit the theory, instead of the theory the facts. Argument with such a theorist is futile. To use Professor Swift's words, in *Science* of Dec. 9, " . . . auroræ frequently occur when no spots are visible on the sun, . . . sun-spots are often seen when auroral exhibitions . . . are entirely absent, . . . the advocates of the theory . . . answer to the former, that sun-spots *may* have been on the *other* side of the sun, and, to the latter objection, that there *may* have been auroræ visible in the Arctic or Antarctic regions, or in both." I do not credit those who pin their faith to a connection between the two classes of phenomena, with having to go so far for an excuse, as they generally utterly ignore the want of coincidence, and instead of discrediting their theory (and I need not add that one failure should have very many times greater weight than one coincidence) calmly ignore it, and proceed with their cumulation. I do not wish to be understood as thinking that there are not dispassionate investigators in this matter; I am only pointing out what I believe to be a very common human peculiarity, and one which I believe does much harm in so far as permitting of the propagation of theories which had else died, still-born, on their authors' hands.

"The evidence of the correctness of a theory or hypothesis increases with the number of facts it is capable of satisfactorily explaining. It diminishes with the number of facts it does not explain, and with the number of different ways in which similar phenomena can be explained. A *single fact,* inconsistent with any theory or hypothesis, is sufficient to overthrow it," is a statement of fact that will be most useful to us in theorizing, and serve to measure some existing theories with.

Any theory of the Aurora must account for the following, amongst other, peculiarities, which seem to me to be characteristic of the same. A. —That they most frequently occur in the colder half of the year, being limited, approximately, by the same isothermal lines as far as the southern limit, in the northern hemisphere, of their visibility is concerned, and not depending in this on latitude. It would seem, then, that temperature is a factor in the required theory. B. —Auroral displays do occur in the summer season, when their situation is more equatorial, and, perhaps as a rule, they cover a larger area than the average winter display. It would seem, then, that on the transference of the maximum winter displays from one hemisphere to the other, these displays may take place in intermediate situations. C.—From my experience in these latitudes, summer displays of limited extent seem to be concurrent with a drop in the temperature considerably below that corresponding to the average of the date in question. D. —My experience has been that auroral displays do not occur during generally unsettled weather, requiring (although the particular locality of the display may be largely overcast, permitting only of the aurora being seen behind the clouds or through the interstices) that generally elsewhere the weather should be clear. As though clouds on the horizon of the display (*not* of the observer) intercepted the influence producing the same. E. —The typical aurora, from which are many departures as pointed out by Professor Swift in the communication mentioned, is a narrow circular arch in that part of the heavens away from the sun, the concave side of which is usually well defined, and beneath which is absolute darkness, into which streamers *do not* descend; the convex side of this arch is, generally, illy defined, from which streamers proceed and the light of which is very much less intense than that of the concave side; conveying to me the impression of the light, the visible effect of the influence, being completely cut off by the interposition of the solid mass of the earth, it being assumed to be the intercepting horizon at the altitude of the display. F. —(Speaking still of the typical auroral arch). It is on the lower and brighter side where the greatest horizontal movements and the greatest contrasts in the intensity of its light (forming, amongst other outlines, so called "curtain-folds") are seen. As though at the horizon of the display, our atmosphere, acting as a lense, concentrated the light (the visible effect of the auroral influence) in just such a way as a spherical, atmospherical, lense would, having its centre "stopped" out by such a body as our earth, in which the densest part being next the earth, the greatest relative variation in its homogeneity would exist and the greatest variation in the transmitted light (the visible effect of the auroral influence), resulting in just such movements as we have seen in the typical arch. G. —It has been constantly noted, that two or more observers, situated, say, 100 miles apart, view occasionally, if not always, totally distinct auroral outlines, differing, at times, radically; so that one observer may report a display differing entirely in class and details from the other at the same instant, or even reporting the entire absence of a display when the local conditions were such as would have permitted its being seen had it existed. From this, it appears to me, we must conclude that the light (the visible effect of the auroral influence) has no material existence in that part of the heavens in which it is seen, else, all observers, so situated on the earth that the point of display is above their horizon and this particular point not obscured by clouds, should see the same display, modified only in detail owing to the effects of perspective attributable to the different points of view. H. —There is an intimate relation between the aurora and magnetic storms; not sufficient to permit of our concluding the one is Cause and the other Effect, but sufficient, I think, to permit of the supposition that both are Effects of a common Cause. These appear to me to be some of the more self-evident peculiarities of the typical Aurora.

The theory in connection with the aurora which appears to have the greatest hold on the investigator and the general public, is one which supposes a connection between these displays and certain disturbed — sun-spot — areas of the sun. If one were to accept the evidence that is brought forward to support this supposition, without taking into account the evidence which has, unintentionally, been suppressed, or perhaps it would be better to say, "not advanced," it would be a very hardened sceptic who would not admit that this question had been settled for all time. In *Astronomy and Astro-Physics*[1] it is concluded that auroral displays recur at intervals which exactly correspond with that of the solar rotation, and at the instant when this disturbed area is at the eastern "limb" of the sun. Dropping for a moment the discussion of the cumulative evidence, it is interesting to note the peculiar nature of the force which proceeds from the solar area in this case. If this influence is at its maximum on the appearance of the area on the eastern limb, and not continuous to the western limb, it is evident that the maximum effects are produced horizontally and in one direction only from the sun's surface. It is not impossible that this is so, but it is an unfair assumption to make, apart from any knowledge of a similarly acting force in nature, and in direct opposition to what experience, in other matters, would suggest as the direction in which such a source of energy would produce maximum results. As to the fact of maximum auroral displays occurring at the instant when the disturbed solar area has reached the eastern limb, the coincidence cannot be as great as claimed, or else the occasions on which this has happened have been given undue prominence in collecting facts to suit the theory, for in a communication to the Royal Astronomical Society[2], the Astronomer Royal states, in

[1] Reprint No. 113.

[2] "Monthly Notices," March, 1892.

discussing sun-spots and associated magnetic disturbances over the period 1880–92, "Most of these magnetic disturbances occurred when an exceptionally large spot was visible on the sun near the centre of the disc, or about the time of some great change in a sun-spot." It should be quite evident, then, that this marvellous coincidence between certain positions of the disturbed solar surface and auroral displays is, to say the least, not such a hard-and-fast rule as the exponents of the theory claim. Even did we admit that the evidence put forward was not as discordant as pointed out, and accepting the statement that, "Under the physical conditions existing in interplanetary space" (a matter admitting of considerable discussion even yet), "cosmical dust and debris, there sufficiently abundant to shine by reflected sunlight as the zodiacal column, furnish a conducting medium well fitted to convey by induction these solar electro-magnetic impulses to vast distances."[1] The single fact, as explained under section "G," that different observers see unlike auroras at the same instant at their several points of observation, is conclusive proof, to my mind, that this "cosmical dust and debris," either without or within our atmosphere, have not been made luminous by the conveyance of the "solar electro-magnetic impulses," as the visible aurora under this theory would require.

[1] Reprint Astronomy and Astro-Physics, No. 113.

NOTES AND NEWS.

A PRINTING Exposition is to be held at the Grand Central Palace, New York City, from May 1 to June 1 next. It is intended to show, by object lessons on a magnificent scale, the history, and progress of the printing trade since the establishment of the first press in this city 200 years ago by William Bradford. The aim is to show in operation the first rudimentary press, and the latest perfected web press; also type-setting and moulding, electrotyping, stereotyping, and photo-engraving processes, color work, etc.

—Professor J. Mark Baldwin of the University of Toronto has accepted the position recently offered to him as Stuart Professor in Psychology in Princeton University. A suite of rooms in North College have been set apart for a laboratory for experimental psychology, and a liberal appropriation made for its equipment in time to begin work next September. Professor Baldwin intends to offer advanced courses, both graduate and undergraduate, in all the departments of psychological work.

— An interesting discovery of the rare trout, *Salvelinus oquassa*, in a mountain lake in the vicinity of Ottawa, Canada, the capital of the Dominion, is recorded in the last number of the Ottawa *Naturalist*, by Mr. J. F. Whiteaves, zoölogist of the Geological Survey. *S. oquassa*, the blue-backed trout, sometimes called the "Rangeley Lake Trout," is stated by Jordan and Gilbert ("Synop. Fishes N. America," 1883, p. 318) to be the smallest and handsomest of our trouts, and as yet known only from the Rangeley Lakes in western Maine. In 1891, Mr. V. C. Nicholson of Ottawa visited a small lake known as Lac de Marbre, lying in the Laurentian Hills, in the Township of Wakefield, Province of Quebec, a few miles from Ottawa. He noticed the difference between some trout he there took and the ordinary brook trout (*S. fontinalis*) which occurred plentifully in adjoining lakes and streams. Mr. Nicholson was so impressed with the fact that these were of a different species that he procured a living specimen, which is now to be seen alive in one of the aquaria of the Government Fisheries Department Exhibition at Ottawa. The specimen was referred to Mr. Whiteaves, who determined it to be the above species. The occurrence of this rare fish in Canada will be of interest to ichthyologists.

—Mr. G. W. Lichtenthaler, one of the most earnest, energetic, and eminent of American conchologists, died at San Francisco Feb. 20. For twenty years he has done nothing but travel and collect, and his vast collection embraces 6,000 or 8,000 species of shells, 1,000 species of marine algæ, and 500 species of ferns, besides many thousands of duplicates. This entire collection he bequeathed to the Illinois Wesleyan University at Bloomington, Ill., the city which has been his home for most of his life. In addition to this valuable collection he bequeathed $500 to put it in suitable shape for preservation. This gives the Illinois Wesleyan University one of the most valuable conchological collections of the country. The ferns and algæ are from every part of the world, and the ferns have a complete collection of those of the Sandwich Islands, and nearly a complete collection of those of North America. The entire collection will be arranged as speedily as possible, and will be accessible to all students of the subjects, as well as to others.

—The series of Saturday lectures, complimentary to the citizens of Washington, given for some years under the auspices of the Philosophical, Anthropological, and Biological Societies of Washington, was discontinued two or three years ago. It is now proposed to resume the series under the auspices of the Anthropological Society, and to arrange the lectures in such manner that each course will serve as a logical introduction to the study of the Science of Man in some of its various aspects. The lectures will be delivered in the lecture room of the U. S. National Museum, at 4.30 P.M., on the dates specified. Citizens of Washington and their friends are cordially invited to attend. The course provisionally fixed for the present season (1892–'93) of the Anthropological Society is as follows: Saturday, Mar. 25, The Human Body, by Dr. D. S. Lamb; Saturday, Apr. 1, The Anthropology of the Brain, by Dr. D. Kerfoot Shute; Saturday, Apr. 8, Status of the Mind Problem, by Professor Lester F. Ward; Saturday, Apr. 15, The Elements of Psychology, by Major J. W. Powell; Saturday, Apr. 22, The Earth, the Home of Man, by W. J. McGee; Saturday, Apr. 29, The Races of Men, by Dr. Daniel G. Brinton; Saturday, May 6, The Evolution of Inventions, by Dr. Otis T. Mason; Saturday. May 13, Primitive Industries, by Thomas Wilson.

—In the summer of 1892, courses of instruction were offered by professors and instructors of Cornell University in botany, chemistry, mathematics, philosophy, physics, English, French, German, drawing, and physical training. The Summer School has now been made an integral part of the university, and, for the summer of 1893, courses are offered in the following subjects: Greek, Latin, German, French, English, elocution, philosophy, pedagogy, history, political and social science, mathematics, physics, chemistry, botany, drawing and art, mechanical drawing, and physical training. Without excluding others qualified to take up the work, these courses are offered for the special benefit of teachers. They afford a practical scheme of university extension, by which the teachers themselves are taught under university instructors, by university methods, and with access to university libraries, museums, and laboratories. The courses are open to women as well as to men, and the same facilities for work are extended to these students as to the regular students of the university. The amount of work implied in these courses is so great that students are advised to confine their attention to one or two subjects. Opportunity will be given for original research under the guidance and with the assistance of members of the instructing corps. Inquiries regarding these courses should be addressed to those in charge of the several departments. The Sage College for Women, a spacious and well appointed dormitory on the university grounds, will be open during the session of the Summer School to women students and to gentlemen with their wives. Inquiries regarding board and rooms may be addressed to Professor Geo. W. Jones; or applications for board and rooms at Sage College, to the manager, Mr. E. P. Gilbert.

— Messrs. D. Appleton & Co's list of spring announcements includes "The United States," by Elisée Reclus, which forms the third volume on North America in Reclus's great work, "The Earth and its Inhabitants;" "Appleton's Annual Cyclopædia for 1892," which will be issued immediately, and, like Reclus, is sold by subscription; "The Principles of Ethics," Vol. II., by Herbert Spencer; "The Laws and Properties of Matter," by R. T. Glazebrook, a new volume in the "Modern Science Series"; "Appleton's Guide-Book to Alaska and the Northwest Coast," by Miss E. R. Scidmore, which will be uniform with "Appletons' Canadian Guide-Books."

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EARTHQUAKES IN AUSTRALASIA.

BY GEORGE HOGBEN, M.A., SECRETARY SEISMOLOGICAL COMMITTEE, A.A.A.S., TIMARU, N. Z.

SEISMOLOGY is a branch of science that until quite recently received very little attention in Australasia. This could hardly be said to be due to the lack of phenomena; though, with one or two exceptions, the earthquakes that have taken place, even in New Zealand, — the seat of the worst disturbances, — have been very mild in character. The stimulus the subject has received lately was given by the Australasian Association for the Advancement of Science, an association which has done so much in other departments to encourage systematic scientific work. As one of the research committees, the A. A. A. S. appointed a committee to report upon seismological phenomena in Australasia; and I think I cannot make a better beginning of what I have to say on the subject than by setting forth in brief the work this committee is attempting to do.

In the first place it has set itself to compile a list of all recorded earthquakes (within the area of its investigations) up to the present time, including in that list all the important details as far as they are given in the existing records.

In the next place we have attempted to provide for the future recording of earthquake shocks in all the colonies according to a uniform system. These records include, among other details, the exact time of the beginning of each shock, the time being checked by the standard telegraph time of each colony through the medium of the Public Telegraph Departments.

Inasmuch, also, as it is, to a large extent, as part of a world-system of observations that our observations in Australasia may become useful, we propose to do for the islands of the Pacific, as far as circumstances will admit, what is being done for the Australasian colonies. There, of course, exact time-observations are generally out of the question; but with the aid, already largely promised, of missionaries, consuls, and other residents, much more, we trust, will be done than at first appears possible.

To secure uniformity in recording the intensity of earthquakes, the Committee have adopted as a common standard the Rossi-Forel scale of intensity. Though rough and variable to a slight extent, it has the advantage of being a recognized standard and is suited to the nature of the evidence at our command.

The materials obtained are used, where sufficient data are to be had, in the determination of the origins of the shocks. In many cases the epicentrum and velocity of propagation can be found; and in a few instances the facts are sufficient in number and accuracy for a more or less probable determination of the depth of the centrum or actual source of disturbance. With the advantage of easy reference to a standard time in most parts of the Australasian colonies, and with increased experience and skill on the part of the observers, it is hoped that accurate observations may become more and more common; in fact, in New Zealand, where the present system has been in use for three and a half years, we find that this is the case. It is true that we have very few seismographs; but the great value of time observations based upon a universal standard time has been fully shown by Major Dutton in his report upon the great Charleston earthquake of August 31, 1886, and his conclusions in that respect are fully borne out in our experience.

The want of special instruments cuts us off from any direct means of determining the amplitude and intensity of the shocks; but the field of research already indicated will give us enough to do for some time to come.

If it be asked what we expect to accomplish by our investigations, I reply that any general theories relating to earthquake phenomena must be based upon observations in all parts of the world, and we aim at making our work of sufficient value to count as a part — only a small part, perhaps — of the materials required for solution of many of the interesting questions arising out of, or connected with, seismology. For example, the nature of the interior of the globe, whether solid or liquid, or solid but potentially liquid, — a problem discussed in such an interesting manner by Osmond Fisher in "The Physics of the Earth's Crust," — would receive considerable light from the determination of the depth of earthquake-origins. If no earthquakes, let us suppose, could be shown to come from a greater depth than twenty-five miles, we should have a strong presumption that at about that depth there was a great change in the condition of the interior; and with a very large number of instances we might have something like a proof of such a break in continuity. The physicists have been at war over this point for some time, and without undue conceit we may say that a definite solution is at least as likely to come from seismology as from any other branch of physics.

In another paper I hope to give a short account of the results already obtained from our observations in this part of the globe. I trust, however, that the editor will allow me to say here that I shall be very glad to communicate with or receive hints from any one engaged in seismological work in America (North or South), especially with reference to earthquakes occurring on or near the coast of the Pacific.

THE PREFIX AQ- IN KITONAQA.

BY ALBERT S. GATSCHET, VINITA, INDIAN TERRITORY.

UP to the present only two scientists are known to have studied seriously the Kootenay or Kitonāqa language, which is spoken by about one thousand Indians in northwestern Montana and in the adjacent parts of British America. These two investigators are Dr. Franz Boas and Dr. A. F. Chamberlain; both have collected a large amount of lexical material and a considerable body of ethnological texts. Chamberlain's report on the tribe and language forms one fascicle of the publications of the British Association for the Advancement of Science, which contains the Transactions of the Edinburgh Meeting of 1892, and is entitled, "Eighth Report on the Northwestern Tribes of Canada," with preface by Horatio Hale (octavo, pp. 71).

The prefix āq- plays a great part in this northern language, for the large majority of the substantives, many particles, and other terms begin with it. The q- is pronounced like the Spanish j and the German ch in *lachen*. It appears from Chamberlain's long list of the substantives begining in āq-, that this prefix should really be spelt āqk-, for -k is always following the first two sounds.

These two sounds easily combine with each other in many of the Indian languages. In Peoria and Cheyenne the k- alternates with qk-, and in Tonica of Louisiana every k- may be spelt qk- as well, for this is simply an "expansion" of the simple sound k-. Chamberlain ventures no derivation or explanation of this prefix, and Boas is also doubtful concerning its origin.

There is a linguistic family in Oregon, the Kalapuya, the dialects of which show exactly the same peculiarity concerning the substantive nouns. In the Atfálati dialect, once spoken near Gaston and Wappatoo Lake, west of Portland, almost all substantives begin with a-, as apúmmeig, *woman.* Among the few exceptions, I now remember only mäntäl, *dog.* All adjectives of Atfálati begin in wa-, u-a-, ua- in their form for the singular, and this coincides exactly with the radix of their numeral for *one.* Although what we call *articles* do not frequently appear in American languages. the proclivity of these to agglutinate with their nouns is a well-known fact though more so when the article is suffixed than when prefixed to the noun. *cf.*, the Dakota, Otomi, Basque, and Scandinavian. In the Chonook jargon the French article *le, la* was by the Indians fused into one word with the noun following: Lipipan, *le ruban;* liblō, *le bleu*, or purple; lilu, *le loup.* Thus I argue that the Atfálati numeral for *one* became an indefinite article *a* and was coalescing with the noun following it into an inseparable unit.

The same thing occurred in the case of the Kitonāqa prefix āq-, āqk-. We find it, though pronounced somewhat differently, as o'kē, ō'kwē, *one*, the first numeral. *cf* , aiwōm tla ō'kwē, *ten and one*, viz., "eleven;" in what Chamberlain calls the independent form of the substantive and adjective, which through this addition differs from the form as used in composition; tlū, *snow*, āqktlū (independent form). The same radical also occurs in āqktō, *bear one year old;* aqksākes kō'kwes, *one leg;* ā'qkī, *and, again, more* (perhaps "one more" originally).

I therefore consider this prefix as an obsolete indefinite article, which has gradually fused into one solid body with the noun following; we are at leisure to consider it now as a definite or an indefinite article in its original state. It was once *an article* and is now fossilized, like the a- of the Kalapuyan dialects, into the body of the word.

MAMMOTH CAVE IN MARCH.

BY H C. HOVEY, D.D , BRIDGEPORT, CONN.

I HAVE long been curious to see the great cavern amid wintry surroundings. The capricious season is not without charms to one who can appreciate nature's changing moods. As our train pulled out from Louisville we saw that the tumultuous yellow flood had wholly obliterated the falls of the Ohio, as well as the costly canal around them, and had inundated the broad flats by the great bend below to a breadth of twenty miles. The storms of rain and snow swept over the Kentucky hills that guard the line of the Louisville and Nashville Railroad, but could not wholly hide the rugged grandeur of their naked crags and pointed peaks; while the torrents, rolling southward between bright-red ocherous banks, were far more interesting than their dry courses could be in sultry August. There are said to be five hundred caves in Edmondson County, and several of these are lauded by their owners as rivals to Mammoth Cave. This petty jealousy cropped out in the remarks made to us on our arrival at the Glasgow Junction, where we had to change cars, to the effect that Green River had broken into Mammoth Cave so as to make its avenues impassible; that visitors were not admitted at this season; that the hotel was literally dropping to pieces and had been closed; and, in short, that we had better turn our steps in some other cavernous direction. This local jealousy has occasionally even taken the malignant form of wanton injury to the estate and ugly threats of violence to the manager. Whenever a grander cavern than Mammoth shall actually be discovered (which may sometime be the case), let its claims be allowed; but thus far it stands as the noblest specimen of its kind. As such it has an interest for all patriotic Americans. True, our interest is weakened slightly when we find ourselves taxed fifteen cents per mile on the Mammoth Cave Railroad — a tariff never relaxed by the Nashville company even for excursion parties of hundreds of passengers; and it is further impaired on finding the ancient hotel, if not literally dropping to pieces, yet far from luxurious, or even thoroughly comfortable. It is a great architectural curiosity as having been evolved from a log-cabin germ planted in 1812, but it fails to meet the demands of the modern travelling public. While admiring the good taste that keeps the surrounding forest intact in its native wildness, we should appreciate better walks by which the woodland charms might be made more accessible. We would also respectfully remark that these are days when electric lights are quite generally used, in preference to lard-oil lamps, and nowhere would they be more serviceable than in illuminating the grand subterranean realm of Mammoth Cave, as has long been done at Luray. It is our conviction that the owners of this splendid estate could make no more remunerative investment than by the timely adoption of these friendly suggestions.

And yet justice should be done to the improvements already made by the enterprising manager, Mr. H. C. Ganter, about the hotel and grounds, and especially within the cavern itself. One of the first localities we explored on this visit was Audubon Avenue, the first right-hand branch from the main cave, which when we last saw it was heavily encumbered throughout with great fragments of limestone that made the going very tedious. These have all been removed at great expense, some of them being dumped into a deep ravine, and others piled up in formidable, yet shapely, walls. One object of all this is to prepare the way for the practical cultivation of mushrooms on a scale equal to that at Frèpillon and Méry, in France. Over $5,000 have already been spent in this work under the direction of skilled gardeners, and ultimate success is looked for. Another striking change accomplished recently is the opening for the public of what is to be known henceforth as Ganter Avenue, and which has hitherto been passable only for the guides and hardly for them. It is a wonderful fissure, or rather series of fissures, extending through solid limestone for 8,500 feet, as actually measured by us. The passage, until recently widened, used to be for a great distance only about eight inches wide. But by patient drilling and blasting it has been enlarged so that persons of ordinary size have no serious difficulty in going through. Indeed, it has already been threaded by perhaps a thousand visitors. It twists and winds in the most curious manner, more than two hundred turns having been actually noted, and it is well worth seeing for its own sake. But the main advantage derived from it is that when River Hall is flooded, as it is liable to be during more than half the year, tourists can thus gain the crystalline regions beyond and reach the extreme end of the "long route;" and should they ever be caught there by a sudden rise of the waters, they have this safe way of exit always available. At the time of my visit Echo River, Lake Lethe, the Styx, and the Dead Sea were all united into one vast body of water, extending from Bacon Chamber to Cascade Hall, its depth from surface to bottom being at least 100 feet; and the water was backing up into Gorin's Dome, the Bottomless Pit, and all other pits in the cavern; but not a drop in Ganter Avenue, through which we safely passed to the regions beyond and returned dry shod. The temperature, both of the water and air, is uniformly 54° F. all the year round; the exceptions being in localities where a strong draft lowers the mercury a degree or two, or where the warm air from the lamps, fireworks, etc., gathers in close domes, whence it cannot immediately escape. On the whole, I do not hesitate to recommend Mammoth Cave as a delightful winter resort. The climate is salubrious always, and the sole difference in the cave itself from its summer condition is in the subterranean waters; and even here, if suitable boats were provided, guests might enjoy a charming sail, and they would find the passage-way over Lake Lethe endowed with the same marvellous echoing peculiarities that have made Echo River so famous. By the way, I have never heard mention made of the quite different but equally wonderful acoustic properties of the Chief City. This is an immense hall, 450 feet long by 175 feet wide (as measured by us) in which many Indian relics are found. Stationing ourselves at its opposite sides, as far apart from each other as we could get, we had no difficulty in conversing in ordinary tones or even in the very softest whispers, every faintest sound being faithfully carried across the hall.

It is not my object now to describe the familiar wonders of the great cavern, always the same, winter and summer, and that have been described a thousand times. But I take pleasure in directing public attention to two adjacent caverns, belonging to the Mammoth Cave estate, and that are seldom visited, though each for different reasons should challenge admiration. The White Cave (so named eighty years ago on account of the whiteness of its formations) is entered at a point half a mile from the hotel. Its floor is cut by numerous channels, through which water runs so pure as to be almost invisible, leading to exquisite pools with ruffled and incurved rim, none of them being more than two or three feet deep. The roof is for the most part low and fretted with numberless dainty stalactites. Advancing, we find the floor encumbered with huge blocks of limestone, and the cave divided longitudinally by a wall of noble stalagmites far beyond anything of the sort to be seen in the adjacent larger cavern. It ends in a profound pit, named by us Bishop's Dome, for our guide, Eddie Bishop, who, so far as is known, was the first to descend to its bottom, which feat he accomplished in our presence. It is supposed that the White Cave is connected with the Mammoth Cave at some point near the end of Audubon Avenue, or possibly at Little Bat Avenue; but this remains yet to be proved.

For ten years past I have heard of Dixon's Cave, but had never been informed that it was in any way remarkable, except for having possibly been at some remote period the original mouth of Mammoth Cave, and even this seemed to be a matter of doubt. Being desirous of seeing it, simply for the sake of completing my work, I donned my usual cave attire, and sallied forth one March morning with Bishop the guide. Snow had fallen to a depth of four inches, through which the brave daffodils in the garden lifted their golden heads, while the more modest spring flowers that had been tempted to bloom too soon lay hidden under the wide, snowy blanket. The ice-laden trees glistened in the vernal sunshine. As we broke our way through the budding underbrush of the oak opening, tracks were visible of rabbits, foxes, and wild turkeys. After going thus for several hundred yards, we were confronted by a wide chasm in the hillside, into whose yawning gulf great moss-grown forest-trees had plunged headforemost. Creeping under or climbing over their prostrate trunks, we gazed awe-stricken into the mightiest cavern-mouth I ever saw. The whole cavern is a single hall, which, by our measurement, is 1500 feet long, from 60 to 80 feet wide, and from 80 to 125 feet high, gradually curving from southeast to due south; the dimensions being quite uniform from end to end and from top to bottom. The roof is decorated here and there by alabaster stalactites, and at the time of our visit it was also appropriated by myriads of hibernating bats, clinging in great clusters like swarms of bees. The floor was long ago gone over by the saltpetre miners of 1812, who had left the rocky fragments piled in what might be described as stony billows lying across the cave, each wave being 40 feet through at the base and rising 25 or 30 feet above the true floor. At the extreme end the mass of nitrous earth seemed not to have been disturbed, over which we climbed to the very roof, and amid whose nooks we diligently sought a way of access to Mammoth Cave. We did not succeed; but subsequent outside measurements satisfied us that we had reached within 60 feet of the desired goal, and that by suitable excavation the connection might be made. Before leaving Dixon's Cave, I stationed Bishop at the inner end, while I gained a point midway where I could see the white sunlight as it was reflected from the snow, and then had him ignite three Bengal lights. The effect was indiscribably grand as their brilliant illumination crept through the black darkness till it cast my shadow on the fainter sunlight itself, like a giant spectre, and finally blended with the outer light, thus enabling me to take in at a single glance the vast dimensions of what may be justly styled the most magnificent subterranean hall in the known world. On our return to the hotel, we made our way by the mouth of Mammoth Cave and saw it environed by trackless snow, its mosses and vines spangled with silver, and the wild, pattering cascade falling from the rocks above to the rocks below as it has done for ages. And, turning away, I echoed with all my heart the guide's naive exclamation, "I fairly love old Mammoth Cave."

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.

Anatomical Nomenclature.

As the years go by the movement for a thorough and scientific revision of biological nomenclature gains in depth and strength, and we have every reason to believe that great and lasting benefits will accrue to science as the result of these attempts to increase the precision and fitness of our scientific language. Believing that every increment, however small, is a distinct gain if it only possess the qualities above mentioned, I propose the following modifications in anatomical nomenclature for the consideration of all anatomists interested in this important work.

In my paper on the vertebrate ear,[1] I brought out in considerable detail the two following considerations regarding the morphology of the auditory nerve and made certain suggestions looking to an improved nomenclature of these parts. In the first place it was shown that anatomists had not adequately recognized the true nature of the auditory nerve owing to the persistence of the older view of the nature of the auditory organ, which was regarded as a morphological unit. Such a well-defined unit could only be supplied by an equally well-defined (single) nerve. It was there for the first time proposed to recognize in our terminology the fact that the auditory nerve is composed of two very thoroughly separated parts, both as concerns their central ends and their peripheral origin.

In the second place it was brought out that these two parts showed certain important anatomical relations to two other cranial nerves from which these branches of the auditory had in all probability arisen during phylogenetic development. The names proposed are N. auditorius ramus utricularis and ramus saccularis, or the utricular and saccular nerves, respectively. This nomenclature is based on a very extended study of the comparative morphology of the acoustic apparatus. These terms are superior to and in every way perferable to the other current designations such as N. cochleæ and vestibuli, or N. superior and inferior.

The terms, N. vestibuli and N. cochleæ, are ill-chosen, from the fact that the morphology of the "vestibule" and its parts as conceived by the anatomists who first proposed this term has no real existence. On the other hand, the term N. cochlearis is unsuitable, not to say inadequate, from the fact that this nerve is not solely a cochlear nerve since its trunk contains nerves to the "vestibule" as well, viz., the saccular and posterior ampullar nerves wherever these are not provided with separate foramina. The central relation of these two nerves is always with the mass of cochlear fibres in those forms possessing an enlarged cochlear apparatus, as well as in the more primitive condition of the auditory organ.

While engaged in reconstructing our anatomical nomenclature it is very desirable that we choose those terms which express the present condition of our knowledge and give promise of being adequate for the future as well, for, I take it, the recent movement for a betterment of biological nomenclature is dominated by the universal desire for as simple, short, and expressive a terminology as shall be adequate not only to the science as it exists to-day, but also to its expanded condition in the not distant future. None of these conditions are fulfiled by any of the terms yet applied to the ear-nerve except the two, utricularis and saccularis.

No broad-minded anatomist will desire to retain names in human anatomy that are inapplicable to all other vertebrates possessing the homologous arrangements of the parts under consideration. Not all vertebrates, not even a majority of them, possess a cochlea, consequently we should have to provide another name for the same nerve in lower forms or else have the anomaly of an animal without a cochlea provided with a "cochlear nerve."

[1] A contribution to the Morphology of the Vertebrate Ear, etc. Journ. Morph., VI., 1892.

In every aspect of the matter the sense-organ must be present before its nerve can have a separate existence. The terms "utricularis" and "saccularis" are in all respects suitable and descriptive of the things to be named. Both of the nerves to which they are applied supply parts of the same organ complex which forms a well differentiated structure, and since both parts must have very similar functions it is certainly unadvisable to leave out of the designation all reference to the accepted idea as to the function which they subserve. Consequently, I hold that the names which I used in my memoir on the ear are the most suitable and the best grounded terms yet proposed for a revised nomenclature. The names may be used in full as Nervus acusticus utricularis and Nervus acusticus saccularis, or abbreviated to N. ac. utric. and N. ac. sac., or, since they are not liable to become confused with other nerve names, we may write simply N. utric. and N. sac. For the branches of each of these nerves we may write respectively:—

N. utric. { ramus cristæ anterioris.
" " externæ.
" maculæ utriculi.

N. sac. { ramus cristæ posterioris.
" " cochlearis.
" maculæ sacculi.

Howard Ayers.

The Lake Laboratory, Milwaukee, Wis., Mar. 20, 1893.

The Neanderthal Skull.

I have waited in the hope that some one more competent than myself would take up this matter, but, this failing, I am induced to send a short note on the enquiry into the reality of our venerable troglodyte.

Dr. Brinton quotes very high authority in his letter; few higher than Virchow could be found. But it appears to me that the whole story was not given. We are all concerned to know the exact truth and value of these old relics of pre-historic man. But just now the iconoclasts are abroad in the land, and they may, as they have done in days past, go too far on that side.

The Neanderthal skull has never been unequivocally accepted as a type, chiefly because it stood so long alone. But a race has been named after it by some anthropologists, provisionally at least — the Canstadt, etc.

The evidence in favor of its authenticity has been before the world for many years almost unchallenged, and, with all respect to the eminent men engaged in the controversy, I submit that it is not quite in accord with logic or with scientific method to base an objection against the positive testimony of the discoverer on the mere recollection of his surviving widow nearly forty years after the discovery was made.

Waiving all other considerations, we know how treacherous is the memory of an event in which we were not deeply interested (and which we only in part comprehended) after half a lifetime has passed since it occurred. And that Frau Fuhlrott was in this mental condition is obvious from Professor Virchow's own admission, that she made this statement to him in entire unconsciousness of the weighty results involved. This of itself is sufficient to greatly reduce its value.

But there is yet another important element in the problem to be considered. In Sir C. Lyell's "Antiquity of Man" he thus describes the place: "I visited the spot in 1860 in company with Dr. Fuhlrott (sic), who had the kindness to come from Elberfeld expressly to be my guide, and who brought with him the original fossil skull." "The spot is a deep and narrow ravine. The cave occurs on the precipitous southern or left side of the winding ravine, about sixty feet above the stream and a hundred feet below the top of the cliff." He then gives a sectional view, showing an opening to the surface, and adds, "Through this passage the loam which covered the floor and possibly the human body to which the bones belonged may have been washed into the cave below." "There was no stalagmite overlying the mud in which the human skeleton was found." "The loam, which was five feet thick, was removed and the human skull was noticed near the entrance, the other bones lying farther in on the same horizon. The skull and bones had lost so much of their animal matter as to adhere strongly to the tongue, agreeing in this respect with the ordinary condition of fossil bones of the post-pliocene period."

The loneliness of the Neanderthal skull has been much relieved by later discoveries, especially by that of Professors Lohest and Fraipont at Liège, but waiving this and keeping to the main point it is not easy to understand how testimony so direct and explicit can be at once overthrown by a recollection of an uninterested party after 35 years interval. It will be at once seen how widely Sir C. Lyell's description of the ground, written by an eye-witness, differs from that given in the first letter on the subject in *Science*. Moreover, Lyell's description shows that not the skull alone, but other bones, and probably the whole skeleton, were present. Our low-browed palæolithic (?) ancestor has still enough material left to make out a good case.

E. W. Claypole.

Akron, O., March 29.

Prehistoric Coil Pottery.

In the dim past when primeval men occupied this continent, no one knows for how long a period, they raised mounds, dwelt in caves, or built towns that are now below the surface of the earth. In all this long era they used flint or stone implements for all edged tools, hammers, axes, spears, etc. At the same time having no

COIL POTTERY.

metal pots or kettles, a rough earthen ware was used for cooking and for all other uses for which we now use iron, tin, and wooden vessels. There is somewhat of a resemblance in many of the stone implements all over the world. It is only recently that it has been discovered that there is a similar resemblance in much of the pottery of this early age, especially in the coil pottery. This pottery was made by rolling clay into long strings like cord, and while soft beginning with one end to coil it round and round, increasing the size of the bottom till it assumed the desired dimensions, then shaping it up the sides (just as straw hats are made) till the required form and size was attained (see illustration). The most extraordinary part of the investigation is that this ware made in the same manner is found in the mounds of Florida and Ohio, in the cliff-dwellings of New Mexico and Arizona, in the buried cities of the cañons of these territories, also in the Connecticut Valley and under the ancient shell-heaps of Cape Cod, Mass. What a long period of time it must have taken to have this art disseminated over so vast a territory at this early age. According to the uses these pots were intended for, so were they made large or small, thick or thin, and of various shapes. It was a common practice to use some sharp instrument to dint or work up some fanciful designs without obliterating the lines of the coil; in some cases they are beautifully marked, looking like carved black oak, others made of light-colored clay in very fine coils prettily indented forming neat designs. Some of the best ware is handsomely smoothed and rubbed to almost a polished surface before baking. All are smoothed inside, before they were dry; probably some of those intended to withstand heat have plumbago mixed in the inner surface of the vessels. There are many fanciful designs of this ware, some very large jars, pots of all shapes, bowls, cups, pitchers, etc. HENRY HALES.

Ridgewood, N. J.

The Sense of Boundary in Dogs.

I HAVE followed with much interest the discussion in *Science* caused by the recent communication of my friend, Dr. Hall, entitled "Is there a Sense of Direction?"

Dr. Hall's query recalls to my mind a striking example of animal intelligence which I witnessed in a dog, and of which I sent a brief notice at the time to the London *Spectator*.

Some eight years ago I was staying with friends who had a full-blooded Irish deer-hound. On the adjoining estate lived a pointer. Our dog was scarcely more than a year old, while our neighbor's dog was quite well along in life. The dogs had never been friendly; indeed, from the first, the pointer manifested a decided aversion to the young deer-hound. Whenever the old dog caught his youthful neighbor trespassing he would immediately drive him back over the boundary between the estates. Both dogs, even when going at full speed, would invariably stop the moment our dog had crossed the line. The two estates are virtually continuous, there being neither hedge nor fence separating them. The dividing line runs between two stone posts about a foot in height and some two hundred feet apart. These posts, of the existence of which I was quite unaware, until the singular behavior of the dogs called my attention to them, are in the summer time usually hidden by the grass, and in winter are often buried under the snow. I mention them, not because I think it at all probable they served as guides to the dogs in determining the boundary line, but merely because they enabled us to observe more accurately the phenomenon in question.

This exhibition of canine intelligence was first observed by my neighbors, who kindly pointed it out to me. It was repeated almost daily for several months, and was a constant source of amusement and wonder to those who witnessed it. The question arises, How did the pointer know where the line ran, and how did his canine neighbor know when he was safely across it? The only answer which occurs to the writer is, that dogs (some dogs, certainly) possess a very acute sense of boundary.

Whether this sense is shared by other animals I am unable to say, though, on this point, it is possible that some of your readers may be able to throw light. The question is certainly an interesting one from its bearing on the general question of animal intelligence. F. TUCKERMAN.

Berlin, Germany, Feb. 28.

The Results of Search for Paleolithic Implements in the Ohio Valley.

THOSE engaged in the recent discussion of Glacial Man have had little to say of the Ohio Valley. Without laying any claim whatever to geologic skill, I will submit some extracts from my private journal. These are submitted from the standpoint of a "field searcher" who knows nearly all the village sites and primitive remains of southern Ohio.

"May, 1891. Found in ash-pits near the Little Miami River, at Fort Ancient (Warren County), several objects of the character of those in the United States National Museum labelled from New Jersey and District of Columbia, commonly called paleoliths. These are in various styles — broken and whole, rude and well formed, large and small. Pottery fragments, bones, and flint chips side by side with the rough forms.

"Spent a large part of three days in inspecting the river banks, gravel strata and river bars. Pottery, several celts, arrow-heads, and paleoliths numerous. Two hearths discovered, the one six and the other nine feet below the surface. A modern brick was found lying just above one of them. Rough implements were gathered from the village sites and in the clay and sand of the river banks. No implement was seen protruding from the gravel layers.

"Rowed up the stream all day Saturday. Three experienced field-searchers were in the boat. No gravel bank was seen which contained implements. We saw no spot in clay bank, on village site or bar where only rude implements of paleolithic type (or approaching that type) were found. The rude objects, finished objects, pottery, etc., are always found together. Careful searching long continued might reveal isolated paleoliths. The river frequently washes cans, bricks, etc. out of its banks and transports them to remote parts. Just so it might carry a piece of pottery or a paleolith to a gravel bar and deposit it. A finder of an implement thus deposited would attach to it great importance, especially so were he a stranger in the valley."

This important point has been overlooked in the discussion. So far as Ohio goes, I think I am safe in saying, Dr. Metz is the only thorough archæologist who claims to have found paleoliths in the drift. All others have been found by travellers or persons not familiar with the prehistoric sites of occupation. Professor Wright does not claim to have found them himself. How is it that those of us who spend all of our time in archæologic work cannot find them? Were they so numerous in drift, surely we could see them whether we knew anything about geology or not. The type is fixed in everyone's mind, and while a searcher might not be able to name the deposit in which the implement occurred, he certainly could tell the implement when he saw it!

Dr. Cresson — strong in "paleolithic faith" — never found one specimen while he was for four months in my camp in Paint Valley, Ross County. Yet he often searched the creek banks or gravel exposures. My men, all good specimen hunters, quick to see an artificial object, could never find them in any kind of stratified gravel. I lay no claim to a knowledge of the gravels, but had implements been found in them geologists from Columbus or Cincinnati would have examined and named the deposits for me. During the coming summer I will spend as much time as possible in a further search for implements like those found by Metz and Mills. Any number can be found on the surface, but as yet I have not been able to find one in gravel layers. Probably my eyes are not sharp enough!

WARREN K. MOOREHEAD.

5,215 Washington Ave., Chicago, Ill., Mar. 24.

Probable Causes of Rainy Period in Southern Peru.

IN your issue of Oct. 21, Professor A. E. Douglass of Arequipa Observatory presents important facts evidencing a former rainy period in that region which is now nearly rainless. This change he attributes to a considerable increase in the elevation of the Andes in recent geological times. A most serious objection to this theory is, that in order to entirely cut off the precipitation from the trade-winds, an average height of broad mountain range not exceeding 6,000 to 8,000 feet would be necessary. Our experience in the Hawaiian Islands is that the trade-winds rarely surmount 5,000 feet of mountain, and, if they do this, they still more rarely carry much rain over that height, nearly all the moisture being precipitated upon the windward slope. It seems impossible to suppose that the Peruvian Andes were not more than at least one-half their present height during any recent geological period.

I would suggest that the glacial period was the cause of the former moisture of the climate of Peru. During the reign of ice in the southern hemisphere, it seems probable that the weather of the temperate zone was transferred to the tropic — was pushed towards the equator. Peru would at that time have enjoyed the westerly gales now prevalent in southern Chili and Patagonia, together with the heavy rains accompanying those winds.

In support of the very recent existence of such temperate zone climates in the tropics, I will adduce a fact stated to me by Professor A. B. Lyons of Oahu College, who recently found on the now arid slopes of Diamond Head buried land shells, *Achatinellœ*, of a species now only found upon the cold and wet summit of Kaala, 3,700 feet above the sea. This fact indicates that the present dry and warm climate of southeastern Oahu has been a change from one formerly cold and wet, such as would probably have existed during the ice age.

In this connection, it will be important to inquire whether any evidences exist of similar changes of climate in southern and Lower California? SERENO E. BISHOP.

Hono'ulu, Dec. 7, 1892.

A Peculiar Eye.

RECENTLY, while dissecting the eye of a domestic animal, the crystalline lens was found to be divided into three lobes by deep clefts on the front (?) side. The lobes are equal and the clefts extend entirely through, so that the posterior surface is cut near the margin, making the lobes triangular in form with rounded outlines, and only slightly connected at the central point and for about one-half the radial distance outward. The eye had been kept several days, had been frozen, and was so opaque from drying and also from distribution of pigment through the aqueous humor that the interior was invisible before dissection, but one who saw the eye when quite fresh stated that it had an unusual appearance. The form appeared so remarkable to the writer that it is mentioned in the hope of drawing from some one better posted in the morphology of the lens some explanation of the peculiar structure. C. D. MCLOUTH.

Muskegon, Mich.

Speech of Children.

APROPOS of Mr. A. Stevenson's interesting article on the speech of children, in *Science* for March 3, and Dr. Howard Lilienthal's communication in the succeeding number, it occurs to me, a bachelor who has never had much opportunity to become acquainted with young children, since he was a child himself, to inquire whether it would not be a rather difficult matter to teach a very young child the use of the first person, singular. Would he readily distinguish between the proper uses of the various words applied to himself: his name when he was spoken of, "you" when he was spoken to, and "I" or "me" which he should use in speaking of himself? Pronouns are, after all, only words used for convenience instead of nouns, and I cannot see why a person, young or old, cannot think of himself subjectively by his own name as well as by the use of the personal pronoun.

FRANCIS H. ALLEN.

West Roxbury, Mass., Mar. 20, 1893.

Singing of Birds.

I SHOULD be greatly obliged for any communications respecting the relation of singing in birds to the expression of the emotions. I have, of course, in mind the rival theories of Darwin (origin of song by sexual selection) and Herbert Spencer (song expressive of joyful emotion in general). Does the male sing only, or principally, during the time of courtship? Or is the presence or answering call of the female immaterial?

Good observations can be made incidentally, and with very little trouble, on the commoner species. And the only approach to a settlement of the question seems through statistics. I hope that the readers of *Science* will assist me in investigating the matter on this basis. E. B. TITCHENER.

Psychological Laboratory, Ithica, N. Y.

BOOK-REVIEWS.

Third Annual Report of the Geological Survey of Texas, 1891. E. T. Dumble, State Geologist. Austin, 1892. 461 p. Pl. 16.

THIS volume is quite a bulky one and contains information on a variety of subjects. The State geologist in his annual statement mentions the work that has been carried on during the year covered by the report and gives abstracts of the work of the various assistants. The papers accompanying the report are: Houston County, by W. Kennedy. In this county none of the formations are older than the Eocene. Various sections are given and a considerable portion of the report is devoted to economic geology. Mr. Kennedy also contributes a description of a section from Terrell, Kaufman County, to Sabine Pass, on the Gulf of Mexico. Many details of sections are given, which are of interest mainly to those familiar with the region. Mr. W. T. Cummins has a report upon the geography, topography, and geology of the Llano Estacado, with notes on the country to the westward. This is of considerable interest, as it touches upon a region of which comparatively little is known. The region is likely to be of considerable importance, however, in the development of the State, as recent discoveries have shown the possibility of securing water in sufficient quantity to permit of cultivation over the larger portion of its area. Mr. Cummins, in a foot-note, refers to the various theories advanced to account for the name, discarding them all and retaining at the last the name itself. We prefer to adopt the idea of Professor Hill that the name *Llano estacado* refers to the palisade character of the escarpment which nearly surrounds the area and makes it one of the most characteristic *mesas* of the country. The conclusions of Mr. Cummins in regard to the geological structure are that the Quaternary is represented in places on top of the Llano; the Tertiary is exposed at various places in cañons penetrating the edges of the Llano; that the Cretaceous underlies the southern part, forms part of the escarpment on the eastern and southwestern sides, and for a short distance along the northern side in the vicinity of Mt. Tucumcari, New Mexico; and, finally, that the Triassic forms the basal member of the escarpment on all sides. Water can readily be procured in almost all parts of the Llano, although not flowing artesian wells. A good idea may be had of the extent of the area when we observe that no less than 29 counties are included in it. The paper is followed by a discussion of the geology of Tucumcari, New Mexico, in which the author contends from the Cretaceous age of strata previously regarded by Marcou and Hill as Jurassic. It is rather singular that Mr. Cummins concludes that a single specimen of a fossil plant occurring in the beds "is sufficient to establish the fact that the strata are no older than the Cretaceous." This specimen is imperfect, the nervation is "indistinct," but it is concluded to be a dicotyledon, and upon this ground to be of Cretaceous age. The leaf is called *Sterculia drakei*, a new species, and seemingly a new genus.

Mr. N. F. Drake follows with a paper on the Triassic of Northwestern Texas, and Professor E. D. Cope and Dr. R. W. Shufeldt describe some vertebrates in another paper. Dr. V. Sterki gives a list of shells collected in a dry salt lake near Eddy, New Mexico, and J. A. Taff discusses the Cretaceous area north of the Colorado River. The last paper in the report is on "Trans-Pecos Texas," by von Streeruwitz.

It is unfortunate that the "Library Catalogue Slips" should not have been made with more regard to accuracy. In the three slips there are no less than thirteen errors. J. F. J.

The Journal of Geology. Vol. I, No. 1. January-February, 1893. Chicago, The University of Chicago. 112 p.

THE first number of a new publication dealing with scientific matters is always eagerly scanned. It was announced some time ago that the Chicago University expected to issue a magazine from its geological department, and the initial number of *The Journal of Geology* has now come from the press. Its editors are: T. C. Chamberlin, R. D. Salisbury, J. P. Iddings, R. A. F. Penrose, Jr., C. R. Van Hise, C. D. Walcott, and W. H. Holmes. There is besides a corps of associate editors: Sir Archibald Geikie (Great Britain), H. Rosenbusch (Germany), Charles Barrois (France), Albrecht Penck (Austria), Hans Reusch (Norway), Gerard de Geer (Sweden), J. Le Conte (California), G. K. Gilbert (Washington, D.C.), H. S. Williams (Yale University), J. C. Branner (Leland Stanford, Jr., University), G. H. Williams (Johns Hopkins), I. C. Russell (University of Michigan), and Geo. M. Dawson (Canada). These names ought to be a guarantee of an excellent journal. There are, to be sure, several journals already in the field, such as the *American Journal of Science*, the *American Geologist*, and the *American Naturalist*. The first two of these occupy the geological field to a large extent, and the third to a more limited degree. These are more or less dependent upon private enterprise, whereas the new *Journal of Geology* has the advantage, as an editorial states, "of being published under the auspices and guarantee of the University of Chicago, and will be free from the usual embarrassments attending the publication of a scientific magazine." In other words, it will not be depenpent upon a large list of subscribers for support. It is significant that the list of editors is largely made up of former members of the U. S. Geological Survey, but it is to be sincerely hoped that this will not prevent a free discussion in its pages of subjects upon which those outside of the Geological Survey happen to hold opinions opposed to those of the editorial staff. The editor-in-chief says: "It is our desire to open the pages of the *Journal* as broadly as a due regard for merit will permit, and to free it as much as possible from local and institutional aspects." He likewise states what may be assumed to be the field aspired to be occupied by the new *Journal*, when he says that "there seems to be an open field for a periodical which specially invites the discussion of systematic and fundamental themes, and of international and intercontinental relations, and which in particular seeks to promote the study of geographic and continental evolution, orographic movements, volcanic co ordinations, and consanguinities, biological developments and migrations, climatic changes, and similar questions of wide and fundamental interest." This is assuredly a high and broad field, and to successfully cultivate it will require an equally broad and cosmopolitan management.

All the leading articles in the present number are by members of the editorial staff. The table of contents includes the following papers: "On the Pre-Cambrian Rocks of the British Isles," by Sir Archibald Geikie; "Are There Traces of Glacial Man in the Trenton Gravels?" by W. H. Holmes; "Geology as a Part of a College Curriculum," by H. S. Williams; "The Nature of the Englacial Drift of the Mississippi Basin," by T. C. Chamberlin; "Distinct Glacial Epochs and the Criteria for their Recognition," by R. D. Salisbury. There are also editorials, a review of a paper by James Geikie, analytical abstracts of current literature, and acknowledgments of articles donated to the Geological Departments of the University. The *Journal* will be issued semi-quarterly at the price of $3 per annum.

Proceedings of the Ninth Annual Convention of the Association of Official Agricultural Chemists. Bulletin No. 35, U. S. Department of Agriculture, Division of Chemistry. 243 p. 8°.

THE report of the Proceedings of the Association of Official Agricultural Chemists is looked forward to with expectation by every analyst. The carefully recorded laboratory experience with the "old" methods and the suggestion and regulation of the new, form together a valuable annual hardly to be dispensed with by any engaged in practical analytical work. The report of the meeting held in Washington Aug. 25, 26, and 27, 1892, being the ninth of the series, is fully as interesting as those of previous years, and, moreover, there is a very apparent improvement in the nature and method of discussion. The contents are familiar to all, being in brief as follows: Address of the President, Mr. N. T. Lupton, report on dairy products, on phosphoric acid, potash, nitrogen, soils, ash, cattle foods, sugar, fermented liquors, etc., with papers on the particular determinations, and, in conclusion, the official methods adopted in each case for the coming year.

C. P.

Matter, Ether, and Motion. By A. E. DOLBEAR, Ph.D., Professor of Physics, Tuft's College. Boston, Lee & Shepard.

THIS book is written apparently for those who, having never made such a study of scientific work as would enable them to read scientific treatises, are disirous of getting a clear idea of the chief results of scientific investigation. For such this book will have a considerable value; which, however, would have been greater if the author had refrained from including a good many of his own theories. For instance, it is not good that the reader should be told, cocksuredly and in italics (pages 235–7), that "electricity is a phenomenon of rotating molecules." If the author had merely stated it as his own theory, the reader

CALENDAR OF SOCIETIES.

Society of Natural History, Boston.

April. 5. — George Lincoln Goodale, On Some Aspects of Australian Vegetation.

would not be so apt to fall into error as when (as in the case considered) an entirely incorrect theory is embedded, labelless, in a mass of facts.

The book is very clearly written and the style is good. The only misprints in the book appear to be those on pages 80 and 81, where the velocity of light is twice given as 185 miles per second.

Magnetism and Electricity. By ARTHUR WILLIAM POYSER, M A., Headmaster of Wisbech Grammar School. London and New York, Longmans, Green & Co.

THIS book is arranged in the same way as Silvanus P. Thompson's text-book, and is intended for the same purpose. In some ways it is more complete than the latter, and the type is larger and clearer. The illustrations, especially, are well drawn, and it is with delight that one realizes the absence of most of the old stereotypes which have done duty in so many scores of text-books. This is explained in the preface, where we learn that out of the 317 engravings in the book, more than 200 are from original drawings. At intervals throughout the book are given the directions for some 200 experiments, evidently carefully selected, and there are also twenty four sets of exercises given, with the answers to the more difficult examples in them. The book seems one of the best suited for teaching purposes that has appeared and is brought quite up to date. It needs amplifying, however, in those parts which relate to magnetic circuits and permeability. R. A. F.

Catalogue of American Localities of Minerals. By EDWARD S. DANA. From the 6th Edition of Dana's Mineralogy. New York, J. Wiley & Sons, 1893. 51 p. 8°.

THIS catalogue will serve a useful purpose in enabling students of mineralogy to readily ascertain just where in any State or Territory the best localities for minerals are. The information is classified under States, beginning with Maine and ending with Alaska, and followed by the Canadian provinces. A few general remarks are made under each State, Territory or Province, and then follow the counties and the towns where the mineral localities are. It would have been made more valuable by the addition of an index to the minerals mentioned, since then it would have been possible to ascertain in a few moments where any particular species occurs.

AMONG THE PUBLISHERS.

S. C. GRIGGS & Co., Chicago, announce for early publication a work by Elizabeth A. Reed, author of "Hindu Literature," etc., entitled "Persian Literature, Ancient and Modern."

— Mr. William Salter, the author of "Ethical Religion," has issued, through Charles H. Kerr & Co. of Chicago, a small book, entitled "First Steps in Philosophy." It is very plain and simple in style and as free as possible of technical terms; and in these respects is well adapted to its purpose. It omits so much, however, that it can hardly be deemed a sufficient introduction to philosophical study. It gives no general view of the problems of philosophy nor of the methods of studying it. The general theory of knowledge is not touched upon; and the question of theism is not raised, the first part of the book being wholly devoted to the doctrine of perception and the nature of matter. The author's views on these points are substantially those of the idealists; though he shows a certain leaning towards realism, and evidently is not quite satisfied with the idealistic theory. The second part of the book, which treats of ethics, is much more satisfactory, and will be read with interest even by those who do not accept the author's views. He discusses the nature and foundation of duty, criticises the doctrines of intuitionalists and utilitarians, and gives as his own theory one substantially like that of Hegel, which regards the complete realization of everyone's nature as the supreme end of action. Though not wholly satisfactory, this little book may serve to awaken the philosophic impulse in minds naturally susceptible of it, and be the propedeutic to more elaborate studies.

SCIENCE

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SCIENCE

NEW YORK, APRIL 14, 1893.

PHYSICAL SCIENCE IN THE SECONDARY SCHOOLS.

BY CHARLES F. MABERY, PROFESSOR OF CHEMISTRY IN THE CASE SCHOOL OF APPLIED SCIENCE, CLEVELAND, OHIO.

In view of the fundamental changes in methods of education within the last twenty-five years in testing experimentally the educational value of subjects which formerly were not recognized as a part of liberal training, it is to be expected that the secondary schools should await the results of such trials in the higher institutions. The time and energy devoted to the comparative efficiency of different methods or the comparative value of different subjects in these schools should be extremely limited. They can afford no loss of time in uncertain paths. It is more reasonable and economical to leave all teaching in experimental methods to the mature judgment and experience of educators who have devoted their lives to this subject, and who have at their command the ample resources of the college or the university, with no constraints in the employment of their best judgment, such as unavoidably exist in the common schools.

With little thought it might seem that the teaching in the common schools, with aims and methods widely divergent from those in the higher grades, can be critically studied only by those who have them in charge. Upon close examination, however, it is evident that this difference is only one of degree, and experience has shown clearly that the best suggestions for elementary training come from persons engaged in higher teaching, or from persons who are able to adapt methods of higher teaching to the wants of younger pupils. Indeed, the methods universally accepted as the most worthy are the results of study and investigation in the fields of advanced knowledge.

Nevertheless, with the best methods and all the wisdom and judgment of generations of experienced educators, any system of education may fail utterly if it is not supported by teachers who have an enthusiastic interest in imparting knowledge. Without the personality of such a teacher, one subject as well as another may fall into a tedious, uninteresting routine Even in laboratory training, in which it is not difficult to maintain a lively interest, the teaching may easily take a form which fails to accomplish the especial objects for which it is intended. The study of natural phenomena under skilful guidance results in the production of self-reliant students.

In the domain of natural and physical science, instruction may now be considered as having passed the experimental stage, not only in the higher grades, but in the common schools, and the educational value of such instruction is recognized as a part of liberal education. Aside from the practical information, which is a part of general knowledge, the characteristic benefits of scientific training appear in the thorough discipline in methodical habits of study and an intelligent use of the perceptions. If these are the results of the study of science in the higher grades, why may not the same methods, simplified and properly applied, form a part of the means for the development of younger pupils? When we consider the great breadth of the field of knowledge and the limited span of the average human life to compass it, it seems a very short intellectual step from the development in the mind of the child to the more mature condition of the youthful intellect as it passes through the various stages of collegiate training.

In most high schools attempts are now made to teach physics and chemistry, but under very adverse circumstances. There are certain difficulties to be overcome in the proper development of such teaching, but they should not be looked upon too seriously. The earlier condition, in which Latin and Greek were selected as soon as the student had covered the ground of the elementary English branches, is, happily, adjusting itself on a reasonable basis. Perhaps it is of more importance that the wide range of subjects included in the average high-school course must result in a slight and superficial knowledge of many things rather than a thorough training with reference to correct habits of thought and study in any direction. Probably the more serious hindrance to laboratory teaching in chemistry and physics to classes of any magnitude is the expense of the necessary appliances and a lack of knowledge of a proper and economical expenditure of time and energy.

The utility of physical science properly taught as a means of mental culture and discipline has been fully demonstrated in the rigorous tests it has withstood in the severe criticism of modern educational methods. The particular value of such teaching is manifest in the opportunities it affords for accurate observation, exercise in methods of inductive reasoning, and practice in recording the impressions in the form of notes. The most satisfactory as well as the most convenient method of imparting knowledge of the principles of physical science to classes is by lecture-table demonstration. Text-books may be used as an aid, but the personality of the instructor behind illustrative experiments is the most direct, and in fact the only method whereby an eager interest can be aroused in the pupil. Lecture-table demonstration and laboratory practice under the immediate oversight of an intelligent instructor should proceed hand in hand.

There is still another view, which, it seems to me, is worthy of consideration. Probably no one will deny that practical knowledge should be imparted whenever it is consistent with proper mental discipline. Certainly there are important reasons for including as much practical information as possible in any high-school course. Most of the young men who graduate engage in business, and a comparatively small number of the young women continue their studies beyond the high school. Any young man in business has a constant use for knowledge of the chemical composition of substances, their physical and chemical properties, and their uses. Much of such information may easily be included in elementary courses of instruction. Every young woman should understand the principles of ventilation, of sanitary appliances, the applications of weights and measures in the household, and the ordinary chemical changes which are the basis of the preparation of foods, as well as the influence of temperature upon such changes. The ordinary chemical changes in bread-making, in fermentation, in decay, and similar operations should be common knowledge; yet there are, doubtless, very few of the young lady graduates of the high schools who possess a correct knowledge of this subject.

I am well aware of the apparent difficulties in the way of developing laboratory instruction, and I shall venture to propose methods which may be readily applied in any high school. If it is granted that the results of suitable instruction in elementary physical science are worthy of the effort, these difficulties are limited to two directions, and they may be easily overcome. Perhaps the most serious obstacle is the expense of equipment and maintenance of laboratory practice. In a room 40′ by 30′ forty-eight desks may be arranged with ample accommodations for ninety-six students working in two divisions, or for one hundred and forty-four students working in three divisions, with separate drawers and lockers for the apparatus of each division, and with all necessary hood-space and sinks. The cost of the arrangement of such a laboratory, including all gas-fitting and plumbing, and all reagent bottles, in fact fully equipped, except with apparatus for individual students, should be less than $1,500. The cost of apparatus needed by each student should not exceed

$5. The cost per year of chemicals and of apparatus to provide for breakage should not exceed $6. A class of twenty-five students can be accommodated in a room 20′ by 27′, and the expense of a complete equipment should not exceed $700. For smaller classes the cost should be proportionally less. These estimates are based on the results of extended experience in the construction and arrangement of several laboratories, which it has been my fortune to superintend.

The second obstacle mentioned above has reference to the large teaching force which would apparently be required in such instruction, but it would seem that it is rather a result of a want of knowledge of the best methods of teaching physical science, both on the part of executive boards and of many teachers themselves. From the results of my own experience in similar grades, as well as in more advanced instruction, I am convinced that this difficulty is only apparent. In a high-school course of three or four years, physics should be taught during the junior year and chemistry in the senior year.

In chemistry, two hours a week should be devoted to lecture demonstration, with two afternoons, of two hours each, to laboratory work, and one hour to a recitation on the subjects of the lectures and laboratory practice. The same method should be adopted in the physics of the third year, although from the nature of this subject perhaps a text-book may be used more freely.

The same laboratory will serve for both physics and chemis ry, and in physics the same apparatus will serve for different students. Hitherto the chief difficulty in teaching experimental physics has been the high cost of the apparatus; but suggestions concerning inexpensive forms of apparatus have recently been given for the benefit of the secondary schools by professors of physics, especially by the professors at the Jefferson Physical Laboratory of Harvard University, and such apparatus is for sale by the dealers at a small cost. The same instructor may have charge of physics and chemistry, and the success of such teaching would depend upon his particular qualifications. He should be allowed at least eight hours a week to prepare for class-room and laboratory exercises, with some aid from the janitor or other servant. He should still have considerable time which could be devoted to such other teaching as might seem expedient, perhaps in some other branches of science. In the high schools outside of the larger cities the annual salary of an instructor should be between $600 and $1,500, depending upon the size of the school. One instructor can easily teach a class of thirty members; in the larger schools, laboratory assistants would be necessary; lady teachers with suitable preparation are very successful in laboratory teaching, and this service could be combined with other duties.

I am aware that excellent training in elementary physical science is given in some of the high schools in the larger cities; but, notably throughout the West, such teaching, when it is given at all, is usually confined to routine text-book methods, with little, if any, experimental illustration, at least by the students themselves. Such a system as the one herein described requires certain small expenditures, but the efficiency of the high-school instruction would thereby be greatly improved, and the public would soon appreciate the importance of sustaining efforts leading to broader and more practical training.[1]

If at first the governing boards of high schools should feel the need of suggestions in the preparation of plans and estimates for equipment of laboratory rooms, I am sure that professors in charge of laboratories would gladly render such assistance. The success of this system requires a knowledge of special methods, which many teachers do not possess, but they are enabled to acquire it in laboratories which are open during a part of the summer vacation. The chemical laboratory of Harvard University was first opened during the summer of 1873 for the benefit of teachers, and many now have charge of responsible teaching through the knowledge acquired by continuous attendance during successive vacations.

[1] Every citizen is directly interested in the welfare of the public schools, and all parents will heartily support any endeavor looking towards the attainment of the greatest amount of useful knowledge, as well as the best mental development for their children.

What has been said about physical science in the secondary schools may apply in a different sense and on a higher plain to the condition of scientific training in many colleges. The increasing demand for the admission of college graduates to advanced standing in schools of science should be encouraged, since the discipline of a collegiate course is an excellent foundation for advanced scientific study, provided it includes thorough instruction in the elementary branches of science. A college course should offer, as a part of its required work, comprehensive training in general and descriptive chemistry and descriptive physics with extensive laboratory practice in both subjects. Most colleges can also give elective instruction in qualitative chemical analysis, with some additional study in quantitative analysis. Graduates from such courses, which should also include French and German, are well qualified to enter the junior year in the best scientific schools. Unfortunately, at present, not all colleges give a sufficiently thorough drill in elementary physical science, in consequence of which many graduates who desire to enter schools of science labor under a serious disadvantage from a want of the more elementary knowledge. Most colleges, doubtless, feel that they devote as much attention to scientific subjects as is consistent with the thorough general training that is expected in a college course. While this may be true in part, it must be admitted that thorough training in physical science should now have as important a place in a college course as mathematics or the ancient languages. It is not to be expected that the college can provide the expensive equipment for the study of science that is the foundation of the school of science. But every college can afford the small expenditure that will thoroughly equip and maintain working rooms for the use of elementary physics and chemistry, with sufficient instruction to render this study interesting and profitable. The feeling of mutual interest and dependence between the secondary schools and the scientific schools, and perhaps in a less degree between the college and the scientific school as a professional school, should be promoted and encouraged; and whatever aid it is possible to render in either direction should be cheerfully granted.

OUR VACANT PUBLIC LANDS.

BY F. H. NEWELL, WASHINGTON, D.C.

THE total area of the public lands vacant in 1892 has been estimated by the Commissioner of the General Land Office at, in round numbers, less that 568,000,000 acres, these being located in 25 states and territories. Of this total by far the greater part, as is generally known, is in the western half of the United States and mainly west of the 100th meridian. Taking therefore the Dakotas, Nebraska, Kansas and Texas, and the states and territories to the west of these, numbering in all 16, these contained nearly 542,000,000 acres, or about 95 per cent of the vacant public lands. The remaining 26,000,000 acres in the nine political divisions to the east of the states named may be considered as of little value, at least for homesteads. A great part of this is in the swamps of Florida and Louisiana or in what are generally considered non-agricultural regions of Arkansas, Michigan, Minnesota and Wisconsin. The very fact that these lands have not been taken up, although open to settlement for many years, testifies as to the doubts or failures of would-be settlers.

The rate at which the public lands are being sold is also shown in the reports of the officer above mentioned, from which the following figures have been culled:

Disposal of Public Lands.

1890	12,798,837	acres
1891	10,477,700	"
1892	13,664,019	"

During the year 1892, the disposal of lands has been abnormal in quantity, owing doubtless to several causes, but mainly from the legislative or official side rather than from increase of settlement. As a rule it may be said that the sales of public lands have been steadily decreasing year by year until 1892, when they suddenly rose far above the average. This is shown by the following brief statement of the original homestead entries:

Comparison of Original Homestead Entries.

1888	6,676,616 acres,	a decrease of 917,734
1889	6,029,230 "	" " " 647,386
1890	5,531,679 "	" " " 497,551
1891	5,040 394 "	" " " 491,285
1892	7,716,062 "	an increase of 2,675,668

Taking the average annual disposal of the public lands at 12,000,000 acres, and assuming all the vacant land susceptible to entry, it would be entirely taken up in less than 50 years. As a matter of fact, however, only a small portion of this vast area can be acquired under the operations of the present laws or is suitable for homestead purposes. A great part consists of high mountains or deeply-eroded plateaus, of sterile lava-covered plains, or is too rough to be valuable for agricultural purposes. What may be considered as the choicest portions of this vacant public land, where the soil is deep and rich and can be readily tilled, are at present almost valueless on account of the aridity of the climate. While on the one hand mountains, canyons and lava plains cannot be removed, yet on the other the aridity, or at least its effects, can be modified to a certain extent, and lands with fertile soil now useless can be added to the producing farm areas of the country. This aggregate area, however, is relatively small, and at the present rate of disposal of public lands it is a question of only a few years when every available acre will be taken.

Under the operation of existing laws, the rate of disposal of vacant public lands must naturally be constantly diminishing, and it follows, that the probable time of disposal of the lands must be indefinitely prolonged. This decrease in sales or number of homestead entries is, of course, not due to diminution of the demand, for each year this is growing greater and greater, but is the result of scarcity of supply. As previously stated, the more available lands have been taken, and each year the choice is more limited, and men are compelled, by circumstances, to enter upon lands which a few years ago they would not have considered worth taking up. In this state of affairs public interest is being turned to questions bearing upon the reclaiming of portions of the remaining public lands, and greater eagerness is shown in developing all the resources by which these may become valuable.

The results of the eleventh census of the United States, as they have been published, cast light upon some points hitherto obscure, bringing out the condition of development of the western part of the United States, as well as of the whole country. Among other facts, the enumeration has shown that the area irrigated in 1889 was 3,631,381 acres. The scattered patches which go to make up this amount were located from points west of the 100th meridian to the Pacific coast, with the exception of the western part of Oregon and Washington. The total land surface of this area, deducting the 36 counties of western Oregon and Washington, is 1,380,175 square miles, or 883,312,000 acres. The area irrigated thus formed about four-tenths of one per cent of this vast country, which contains nearly all possible combinations of soil and climate, ranging from the smooth, almost barren plains, with scanty vegetation to the high, rough mountains, whose peaks are covered with snow throughout the year, and whose slopes have been clothed with thick forests.

Looking at this vast extent of arid and sub-humid land in a broad way, it is possible to distinguish four great classes, according to the amount of moisture received, or the water supply available, as shown by the character of the vegetation, viz., desert, pasture, fire-wood and timber lands. These may be defined as follows: The desert land is that within which the water supply is so scanty that cattle cannot obtain sufficient for drinking purposes, and the vegetation so ephemeral that it has little value for pasturage. The soil, however, is often rich, and when watered, produces large crops. These desert areas of the United States are, however, rarely without vegetation, and the large amount and variety of plant life are often matters of astonishment to the traveller.

The second class, the pasture land, may be said to embrase all of the Great Plain region which, on account of prevailing aridity, is useful mainly as pasturage. The localities at which agriculture is possible are relatively of insignificant size, although of great importance in a grazing country. It also includes the valley lands within the Rocky Mountain region and the rolling hills on which native grasses grow.

The fire-wood land may be defined as that fringing the timbered areas, and intermediate in character between the pasture land and the high, rough, forested slopes or plateaus. It includes also precipitous hillsides found at an elevation too low to receive a large or constant supply of the moisture which falls upon the more heavily timbered areas.

The fourth class embraces the forested areas upon the high mountains where the conditions are such that trees have been able to attain a size suitable for timber. With this understanding, the following table is given:

	Acres.
Desert land	64,000,000
Pasture land	620,912,000
Fire-wood land	115,200,000
Timber land	83,200,000
Total	883,312,000

Of this total, as above stated, less than 568,000,000 acres still belong to the general government.

The irrigated and irrigable lands are mainly included within those divisions which in their natural state have been considered as desert or pasture land. In a general way, it may be stated that fully nine-tenths of this area is covered with a fertile, arable soil which only lacks sufficient moisture in order to be of value for agriculture. If this proves to be the fact, then out of this total of, in round numbers, 616,000,000 acres of arable lands less than six-tenths of one per cent was irrigated in the census year. As to the reclaimability of a large portion of this area, the question of water supply obviously must first be discussed.

CONTRIBUTIONS FROM THE LABORATORY OF THE YORK COLLEGIATE INSTITUTE.

BY C. H. EHRENFELD, YORK, PA.

Effect of Burning on the Volume of Limestone.

In the York, Pa., courts recently, a case was tried which involved the question whether limestone shrinks by being burned. The matter was submitted to me to be tested. On consulting authorities I found the statement given that no shrinkage occurs; but no method was given for making the test. Hence I devised methods as follows: Several pieces of limestone of varying firmness of texture were taken, and permanent marks made upon them. The distance between these marks was accurately measured. The pieces were then burned in a gas furnace at a high heat for about seven hours. After cooling, the distances were again measured, and were found to be unaltered. The pieces were then slaked with water, to ascertain if the burning was complete. Another test was made in the following manner: The pieces of stone were dipped into melted paraffin and quickly removed in order to coat them with a very thin layer of paraffin, sufficient to render them impervious to water, but not enough to add materially to their volume. Their volumes were then determined accurately by lowering them into a graduated vessel partly filled with water. After being burned, the pieces of stone were again dipped into melted paraffin and the volume determined as before. It was found that no change whatever had taken place.

Water in the Spheroidal State.

While carrying on a piece of work recently which involved the use of a common Liebig condenser, it was noticed that where the stream of waste water fell into the water-trough, the bottom of which was rough, small globules of water were formed, which darted out on all sides and ran on the surface of the water to the sides of the trough, eight or ten inches distant. Frequently they would rebound from the side and start back, but would soon disappear. The globules varied in size from an eighth of an inch in diameter to very minute. Sometimes while running along they would gradually decrease in size until they would disappear, while others would disappear in an instant. In a few cases the size suddenly decreased to about one-half the original diameter, the globule then continuing on its course without further change, until it at last suddenly disappeared. Sometimes two globules would run together, combine, and continue on their course as one globule of increased size. In other cases, instead of combining they would rebound from each other like rubber balls. This rebound also took place when they ran against an air bubble. In one case a globule about one-eighth of an inch in diameter reached the side of the trough and rebounded, but it was reduced in size to about one-half of its original diameter. It was noticed, also, that they did not all move with the same velocity: some shot across the water with great rapidity, while others moved very deliberately, both kinds of movement taking place at the same time and in the same direction. In rare instances the globules stopped and lay at rest on the surface of the water until their final sudden disappearance. The rapidity was always greatest at the beginning. In order to ascertain how rapid a current there might be (the water was about a quarter of an inch deep) bits of wood were floated on the surface. The current thus indicated was many times slower than the movement of the globules.

Particular attention was given to ascertain at what place the globules originated. The falling stream made a circular depression in the water about an inch in diameter. The globules seemed to spring up from the outer edge of this depression, fall back on the surface of the water, and then run rapidly away as described above. The thought suggested itself to me that many, if not all, of the observed phenomena could be accounted for by rapid whirling motion of the globule. The gradual slackening of the motion, the fact that some stopped on the surface of the water, the quick rebound from the sides of the trough, are all effects which can easily be produced by a rapidly whirling ball on a plain surface, like the well-known movements of a billiard ball. This would also account for the phenomenon of a ball of water floating on water, without blending with it, somewhat on the same principal that stones can be made to skip over the surface of water without sinking at once; or more remotely, as the pitching of a curve in base-ball. The conditions, too, at the place of origination of the globules, were just such as would produce a sharp twisting motion. The falling stream was first turned to the side by the bottom of the trough and then upward, until at the top of the rebound the little globules sprang out.

I do not recall ever having seen the above explanation given, and so it is offered for what it is worth.

The temperature of the water was never above 30°C., which would preclude the common explanation for high temperatures. Afterwards the same effects were obtained, on a smaller scale, when the prongs of a large vibrating diapason were dipped into water to show the effects of vibration.

THE HIEROGLYPHICS AND SYMBOLS OF ANCIENT MEXICO.

BY FRANCIS PARRY, F.R.G.S., LONDON, ENGLAND.

The inquiry into the construction of the hieratic writing of the Maya people, drags its extended length over many a passing decade, and does not go forward by leaps and bounds. So it has been with the investigation of the groundwork of the symbolism of the temples, the carved slabs of Palemké, the monoliths of Copan, the profusely ornate external walls of the numerous temples of the Yucatan peninsula. This symbolism is the very foundation of the whole matter, the essence of the spirit pervading the sacerdotal mysteries of Central America.

Mainly graven on stone, its variations are noticeable at a date far from and greatly preceding the manuscripts, consisting of the limited number of four, that have been transmitted to us. These written records, probably because of their being in a form affording an easier study than the numerous drawings representing the many sculptured remains of ancient Mexico, have had the attention of the book student fixed upon them in no ordinary degree. This concentration of thought has been a hindrance to progress, inasmuch as it surveyed a comparatively narrow field, and, observation not reaching far enough, the rise of the hieroglyphic forms, the initial composition of the hieratic writings, and the evolution of religious thought, giving life and spirit to the whole, has been but partially traced.

In order to obtain a firm grasp of the situation, the view should be extended, and broadened to the utmost bounds of our knowledge. Primitive rock scratchings, the roughest sculptured stones, the cup and ring incised carvings of prehistoric times, — each and every source of information should be called upon to contribute material.

In all study connected with hieroglyphics, in fact in all scientific research, an endeavor to find radicals, to establish simply foundation truths, and follow the processes of Nature or the compositions — the artistic productions — of the fertile brain of man from the lowest source, is the surest way of following the ramifications of evolution.

Persistent efforts to break up the mass of concrete Maya symbols have, during a century, given results that have been disappointing. Had the clue been discovered the entire outline of the sacerdotal system must have been traced. The United States Government has, however, largely contributed towards the attainment of a perfect knowledge of these ancient mysteries, by lavishly aiding inquiry and publishing from time to time records, the work of professors, accompanied by engravings which, as ideographic forms are a main feature of the system, are invaluable when the consummation of the inquiry is about to be reached.

To state that the end has been reached would be to assume the subject of Maya symbolism is exhausted. I may, however, confidently predict we are on the high road to the desired goal and announce the striking of a vein, the discovery of the lode, and invite scientists to scrutinize my observations upon that Maya relic, "The Sacred Stone." The whole question of its identity, is treated in a popular manner in a monograph entitled, "The Sacred Maya Stone of Mexico and its symbolism." The stone had been misnamed, and its use conjectured. Supposed to be connected with the ancient Aztec ritual or sacrifices, it was given an incorrect place chronologically, historically.

In the museums of the United States and throughout the archæological collections of Europe, it has been classed as sacrificial. That excellent serial, "Archives International d'Ethnographie," published in Leiden, has in Volume III. an exhaustive disquisition on the many varieties of the stone by Herr Strebel of Hamburg. The conclusion he arrived at is the rejection of the nomenclature of the museums. In this result I heartily concur, but taking an independent view and a new departure, I venture to assert and am prepared to prove it to be a relic of paramount interest. Its earliest archaic type is the key to opening out a vista of a nature worship of wide extent, and the ornate, highly finished examples demonstrate evolution, in religious thought, a recognition of combined natural forces, and *solve the mysteries*.

CURRENT NOTES ON ANTHROPOLOGY.—XXVI.

[*Edited by D. G. Brinton, M.D., LL.D.*]

The Ethnic Study of Religions.

A SUGGESTIVE sketch on "Recent movements in the historical study of religions in America" appears in a late number of *The Biblical World* from the pen of Professor Morris Jastrow, Jr. He details the progress of the historical and comparative study of religions, both in this country and in Europe, and very properly urges its importance as a branch of instruction in universities and similar institutions.

It appears, however, that it is now generally taught as a branch of psychology, ethics, speculative philosophy or doctrinal instruction. This is unfortunate, as these are not the real and nearest relations to religions. Their closest ties are to ethnic characteristics, and only by the light of these can they be clearly comprehended. This is nowhere better illustrated than in the religions of the two great branches of the White Race, the Semites and Aryans. As Dr. Heinrich Schurtz points out in his "Katechismus der Volkerkunde," Christianity, which is ethnologically a polytheism, has been and remains as distasteful to the Semite, as are his localized monotheisms to the Aryan. "The greatest triumph," remarks Mr. G. L. Gomme, in his excellent little book, "Ethnology in Folk-lore," "of the Aryan race was its emancipation from the principle of local worship." It is tied neither to Mecca nor Jerusalem.

These characteristics of religions which obtain historic permanence, find their roots in marked ethnic features, as the tendency to abstraction among the eastern Aryans; and the sphere of their influence is limited by these. Proselytes of another race do not accept the religion as it is taught them, because they cannot. They are proselytes in name only. As Karl von den Steinen remarks of the Christianized natives of Brazil, "They understand its real doctrines about as much as they do the theory of spectral analysis." Only when the historical and comparative study of religions is prosecuted definitely as a branch of ethnology can it attain the best results.

The Stature of the Most Ancient Races.

Has the species of man increased or diminished in stature since it first appeared on this planet? Have his bones increased or diminished in solidity and weight? Have the relations in these respects between the two sexes always been as they are now?

These are some of the very interesting questions approached by Dr. J. Rahon in a recent paper in the Memoirs of the Anthropological Society of Paris, entitled, "Recherches sur les Ossements Humains Anciens et Préhistoriques." It occupies about sixty pages, and is the fruit of most laborious and creditable investigation, both in the collection and digestion of facts.

His conclusions may be briefly stated. Comparing the earliest quaternary skeletons found in western Europe with those of the present population, the former belonged to what we should call medium-sized people, with an average stature, of the males, of 1.63 metres. The tribes of neolithic times varied scarcely at all from this measurement; but the proto-historic nations, the Gauls, Franks, Burgundians, etc., ran the figures up to a mean of 1.66 for the males; since their epoch it has been steadily, though slowly, descending, at least in France, until the average of the Parisian men of to-day is 1.62 metres.

In all ages, the women have averaged about ten centimetres less in height than the men. The bones of both were rather heavier and more powerful in ancient times.

Incidentally, Dr. Rahon shows that the height of the men of Cro Magnon has been over-estimated; that of the man of Spy under estimated; that the Guanches of Teneriffe averaged but one centimetre above the French of to-day, and osteologically were very similar to the Cro Magnon people; that from the most remote time the human body has retained the same porportions; and other suggestive inferences.

The Character of the Glacial Epochs.

The "glacial period" has its greatest interest because it seems to have occurred about the time that man first appeared on earth. Two careful studies of it have recently appeared in *Das Globus*, one by Dr. von Ihering, in an article on the "Palæo-Geography," of South America; the other by Dr. Nehring, in reference to Europe.

In spite of some recent claims to the contrary (see *Science*. March 11, 1892, p. 146) Dr. von Ihering is positive that the birthplace of the human race need not be looked for in South America. Its chief land-mass was once connected with Australia and Africa; but this connection was broken in middle tertiary times. Sometime in the pliocene it first became connected by a land-bridge over Florida and Cuba with North America, and an extensive interchange of mammals took place. The Pampas are pliocene, and show no signs of glacial action. This appears in the pleistocene, and the great glaciers of South America were contemporaneous with those of North America.

Dr. Nehring has occupied himself with tracing the distribution of the steppe fauna into Central and Western Europe in quaternary times. His conclusion is that it extended widely in this direction at a certain period, which he believes marks an interglacial epoch, covering thousands of years, and characterized by a comparatively dry and mild climate, and a notable diminution in glacial activity. The displacement of the steppe fauna, which then flourished in Germany and France, by an Arctic fauna, points to the re-establishment of glacial conditions.

Geologists as well as naturalists are fully alive to the multiple bearings of glacial events on diverse branches of science. The new *Journal of Geology*, started this year by the University of Chicago, has its initial number principally made up of contributions on glacialism. One of them, by Mr. W. H. Holmes, on "Glacial Man in the Trenton Gravels," is distinctly archæological. He sets forth the difficulties in the way of accepting the evidence advanced, and, while rejecting it as inadequate, does so in a fair and unprejudiced tone.

Ethnography of Central America.

Among those whose published studies have considerably aided in the advancement of knowledge concerning the geography, archæology and ethnography of Central America, M. Désiré Pector, consul of Nicaragua at Paris, deserves an honorable position. He has been for years an active officer in the Société Américaine de France, and in the Congrès International des Américanistes. Among the various articles which he has recently issued, one touches on the origin of the name America. This has been derived by Marcou and others from the native word "Amerrique," applied to a chain of mountains on the Atlantic coast of Nicaragua. M. Pector, however, shows that the correct form is "Amerrisque," and rejects the Marcou hypothesis.

In a more extended study, M. Pector takes up a large number of the native geographical names of Central America, and attempts to trace their etymology. It is in part an appendix to an earlier essay on the localisation of the principal tribes of that region at the time of the conquest. Unfortunately, many of the Central American languages are so little known that their methods of compounding words are obscure, and such studies can at present be little more than gropings.

The archæology of Salvador affords him another theme, which he treated in the Archiv. Internat. d'Ethnographie last year, apropos of Montessus de Ballore's book on the subject.

The field which M. Pector has chosen for his studies is one rich in itself, and abounding in significance for the ancient ethnography of both American continents. In that narrow isthmus were centred and compressed the migratory streams from the north and south; and the problems of those migrations must look there for their solutions.

The Republic of Costa Rica lies at its southern extremity; and, concerning its ethnography, two recent works deserve prominent mention. The one of these is by Señor Manuel M. de Peralta, a pamphlet bearing the title, "Apuntes para un Libro sobre los Aborigenes de Costa Rica," Madrid, 1893. With a great deal of care and a singularly thorough knowledge of sources, the author has collected a surprising amount of material regarding the names, localities and affinities of the tribes who inhabited the region at the time it first became known to European observers.

Complementary to this, giving, on the other hand, the condition of the native tribes as they are to-day, is the Viaje de Exploracion al Valle del Rio Grande de Terraba, of Mr. H. Pittier, Director of the Physico-Geographical Institute of Costa Rica, (printed at San José de Costa Rica). The author is primarily a botanist and geologist, but his observations on the Terrabas, Bruncas and allied tribes are fresh, and full of information.

A MEETING of the Essex Institute, Salem, Mass., in memory of its late president, Henry Wheatland, will be held at Academy Hall, Salem, Monday, April 17, 1893, at eight o'clock P.M. Vice-President Goodell will preside, and addresses are expected from Honorable R. S. Rantoul, Professor E. S. Morse, Rev. E. C. Bolles, D.D., and others.

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GLACIATION IN AUSTRALIA.

BY T. S. HALL, CASTLEMAINE, VICTORIA, AUSTRALIA.

EVIDENCES of one or more glacial epochs are plainly visible in Australia, and the more closely is study directed to the subject, the more widely spread are the glacial deposits found to be. As long ago as 1861 Selwyn, then the Director of the Geological Survey of Victoria, noted that several conglomerate beds in various parts of the colony were evidently the results of ice-action, although no striated stones were visible. In 1877 Professor Tate of Adelaide announced the discovery of a glaciated surface near that city, and toward the close of 1889 Mr. E. J. Dunn found grooved stones in Victoria. Since then the Mining Department of Victoria has issued a report by Mr. Dunn of one of these conglomerate beds near Heathcote. The deposit covers about 36 square miles, and consists of a base composed of dark indurated clay, through which are scattered masses of rocks of various kinds—granites, syenites, gneisses, schists, quarzites, slates, shales, conglomerates, etc., etc. Many of the granites are not known in Victoria, *in situ*, and their origin can only be guessed at at present. In one or two places glaciated surfaces are seen and the striæ run north and south. The largest "erratic" known is a block of extra-Victorian granite, weighing about 30 tons. The thickness of the beds is estimated at about 400 feet. The bed rock is of Lower Silurian age, and is tilted at a high angle. Intercalated beds of sandstone occur in places, and show the deposit to be still nearly horizontal. In a paper recently read before the Royal Society of Victoria, Messrs. Officer and Balfour record grooved pebbles, "contorted till," and glaciated surfaces near Bacchus Marsh. The deposit there has, moreover, been heavily faulted.

The age of the Victorian deposits has not been precisely fixed as yet. At Bacchus Marsh the beds are overlain by fresh water sandstones containing *Gangamopteris*, *Schizoneura*, and *Zeugophylletes* (?), and which are stated by M'Coy to be of Triassic age. The age of the glacial beds is then perhaps Palæozoic. No fossil remains have as yet been found in the glacial beds themselves, but doubtless careful washing of the clays will yield evidences of life, as it has done in other countries. Small outliers of these beds are found widely scattered over the colony, from north to south, and on both sides of the Dividing Range. They extend into New South Wales, and may be looked for, Dunn says, at the foot of the western slopes of the Great Divide Similar beds occur on the eastern edge of the great Queensland Downs

Mr. Dunn draws a parallel between these beds and the Dwyka conglomerates of South Africa, which are of Triassic age. If the parallel prove a good one, then we have evidence of an enormous extent of glaciation at the close of the Palæozoic or the beginning of the Mesozoic, extending nearer the equator than that of the Northern Hemisphere, during the last great ice age. The South Australian beds at Hallett's Cove, near Adelaide, before alluded to, are of Tertiary age. Here the glacier path can be traced for about two miles, and moraine *débris* is in abundance. Traces of more recent glacial action are recorded from the neighborhood of Mount Koscius Ko, but these are of local origin, and are pėrhaps due to a greater elevation of the region, as no glaciers exist in Australia at the present time.

SECRETS OF THE ATMOSPHERE.

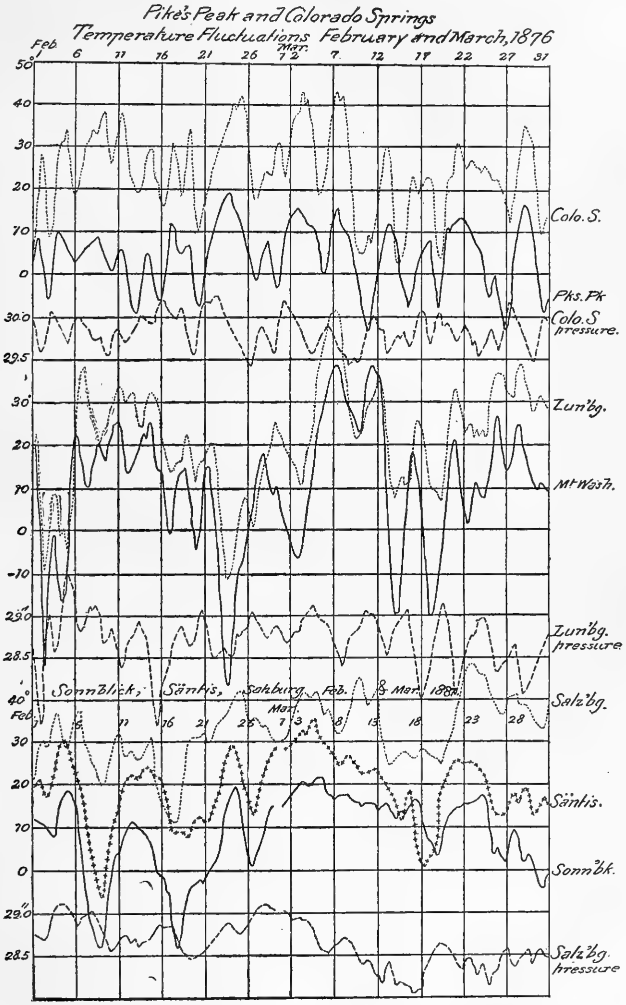
BY H. A. HAZEN, WASHINGTON, D.C.

IN the March number of the *American Meteorological Journal*, Professor Harrington treats at some length the subject, "Exploration of the Free Air." and urges the great necessity of such an enterprise. For more than eight years the present writer has insisted that by no other means will it be possible to set the science of meteorology upon a firm basis and rid it of mere speculations and theories which too often have served to prevent its advance in the past. Professor Harrington quotes a graphic description of an experience of the aeronaut Wise, in which he seemed to be thrown or attracted back and forth in an ominous thunder-cloud. Several such have been described by aeronauts, who unfortunately had not the instruments requisite to give very necessary information in these cases and to make them of avail in a scientific study. The description of these mysteries make us long for something more tangible and definite.

To my mind there is no research of so great importance in the whole range of science as that of a few well conducted ascensions, with accurate instruments, in the midst of a rain-storm and on all sides of a low area. Ordinarily, balloon voyages have been made during clear weather and for the benefit of a great assemblage, so that this field, or the problem of ascertaining the secrets of the air, has been almost entirely neglected up to the present. A single illustration will show the extreme necessity of systematic work in this line.

It may not be generally understood that there has been an extraordinary revolution in meteorology within the past six years. During this revolution the whole convection hypothesis of storm generation, without the least doubt the most important of all the theories of orthodox meteorology, has been attacked and completely overthrown. The significance of this defeat cannot be exaggerated and should be fully set forth. The convection theory is fully advanced in Professor Ferrel's last book, published in 1890, "A Popular Treatise on the Winds," p. 228. "On account of the non-homogeneity of the earth's surface, comprising hills and valleys, land and water, and dry and marshy areas, all with different radiating and absorbing powers, and also on account of the frequently irregular and varying distribution of clouds, it must often happen that there are considerable local departures of temperature from that of the surrounding parts; and if it should so happen, as it frequently must, that this area is of a somewhat circular form, and the air has a temperature higher than that of the surrounding part of the atmosphere, then we have the conditions required to give rise to a vertical circulation, with an ascending current in the interior, as described above. But unless there is some source of heat by which this interior higher temperature is kept up, this circulation soon ceases, for the interchange of air between the interior and exterior parts of the air comprised in the circulation tends to continually reduce the difference of temperature upon which the circulation depends, and to bring all parts to the same temperature. . . . In the case of a moist atmosphere with the unstable state for dry air, we have the same energy for originating and maintaining a vertical circulation as in the case of dry air, with the additional energy of all the latent heat of the aqueous vapor set free in its condensation in the ascending current, and this latter is a continuous source of energy as long as moist air is being drawn in from all sides to supply this current."

These are the careful words of one who devoted more than thirty years to a most thorough study of this whole question. I am well aware that certain disciples of Professor Ferrel are trying to take advanced ground on this vital question, and are striving to show that we may have a storm with lower temperature in its upper portion, but it seems to me this is a fatal error, and if persisted in must overthrow the convection hypothesis. I do not see how there can be any middle ground in this matter. If we accept Ferrel's views, we must stand by the convection hypothesis. It is well known that I have taught for many years that the convection hypothesis is disproved by the most convincing facts, and cannot possibly be sustained. It is none the less true, however, that the temperature in our storms up to great heights is vastly higher than that of the surrounding air, and in our high areas it is vastly less. These facts are absolutely established by observations on mountain-tops. I give here curves showing the fluctuation of temperature at the base and summit of the following stations: Pike's Peak (14,134 feet, highest meteorological station in the world), and Colorado Springs (5.950 feet); Mount Washington (6,279 feet), and Lunenburg (1,100 feet); also at Sonnblick (10,170 feet), and Salzburg (1,434 feet), in Austria. In the last set of curves I have added Säntis (8,202 feet), situated 165 miles west of Sonnblick. Any one studying these curves must be convinced that the temperature fluctuations are precisely the same at the summit and base of each high station. Now it is a universal law that on the approach of a storm the temperature rises at sea-level, and with a high area there is a decided fall, especially in the colder months. This law is abundantly borne out in these curves, for a comparison between the pressure curve (broken) at the base and the temperature curve (dotted) shows opposite phases between the two, and the temperature maxima and minima occur earlier at the summit than the pressure minima and maxima at the base. This is shown most clearly at the stations in the United States, but it can be seen also at the Austrian stations.



To my mind, it is impossible to conceive of a normal storm, seeming to move at the rate of 30 or 40 miles per hour along the earth, which does not have a higher temperature up to a height of at least 5 miles, and probably much higher. The reverse of this must also be true in the case of a high area. In fact, I utterly fail to see how an area of high pressure can have a rapid motion unless in its centre there is denser air, brought about by a greater degree of cold, and this, as I understand it, is exactly the view of Professor Ferrel. These views were generally held up to 1886, and I am not aware that any one disputed them, except as regards the pure convection hypothesis.

In 1886 M. Dechevrens wrote a paper in which he tried to show that the usual law of the relation of pressure and temperature at the earth's surface was exactly reversed at the height of Mount Washington (6,279 feet), and that at that point a fall in temperature occurred with a fall in pressure, and *vice versa*. This was a most astonishing result, and seemed to disprove the whole convection hypothesis. His research consisted in studying the pressure and temperature fluctuations at the summit without any reference to the passage of storms or high areas at the base.

In this study he ignored the fact that after the passage of a storm the cold wave following would tend to contract the air below the summit, and hence the pressure would continue to fall, and the minimum would not be reached till some time after the passage of the storm at the base. Exactly the reverse conditions would be found on the passage of a high area. A full analysis of this proposition, with curves showing these effects, will be found in Annual Rept., Chief Signal Officer, 1882, pp. 897–902.

In addition to this difficulty in comparing these fluctuations directly, there is another almost as serious, which lies in the fact that the maximum and minimum points in the temperature oscillations occur several hours earlier at the summit than they do at the base. It will be seen readily that both these conditions would tend to bring the minimum of temperature at the same time as the minimum of pressure at the summit. How closely these conditions of temperature at the base agree with those at the summit we have already seen perfectly demonstrated in the curves given above. This paper of Dechevrens was translated and published with comments dissenting strongly from the views advanced in the *American Meteorological Journal*, August, 1886, pp. 297–314.

It is probable that these researches would have attracted very little attention had it not been for a study by Dr. Hann of observations at Sonnblick (10,170 feet) on almost exactly the same lines as those pursued by Dechevrens, and with the same result. The first paper by Dr. Hann was published in April, 1887, in the *Meteorologische Zeitschrift*, and this was followed by others in the same and other journals, the last and most thorough research of all, of 86 pages, appearing in April, 1891. In this study the conditions were still farther complicated, from the fact that Sonnblick lay in southern Europe, where very few, if any, normal storms or high areas pass. High areas, with a pressure of 30.7″, have been known to hover over this region for three weeks at a time. Such conditions are unheard of in the United States, and their effect can be at once recognized by comparing the fluctuations in the lowest series of curves with those in the other two in the diagram already given. It is easy to see that under such conditions the stagnant air above would become abnormally and cumulatively heated day by day, while the lower air in a clear sky would be abnormally cooled, and there would at times appear to be a reversal of temperature. With all these disadvantages, however, it will be seen that the temperature curve at the base of Sonnblick, in its larger oscillations, agrees almost exactly with that at the summit.

One of the most remarkable results found by Dr. Hann was that the maximum and minimum points of temperature lag a day behind those of the maximum and minimum air-pressure. It seems almost incredible that such a deduction could have been made. It seems as if it could only have been by confusing or comparing the minimum air-pressure oscillation of a storm with the minimum temperature oscillation of the following high area. An examination of the diagram already given brings out this fact most clearly. In nearly every case, both at Pike's Peak and Mount Washington, the minimum of air-pressure occurs a day earlier than the minimum of temperature accompanying the succeeding high area. I am sure that no one will make this mistake on studying the diagram. The maximum of temperature accompanying a storm occurs about a day earlier than the minimum of pressure accompanying the same storm. The minimum of temperature accompanying the succeeding high area has nothing whatever to do with the previous minimum of pressure, and a proper study of the diagram shows at once the truth.

It seems to me my position in this matter is brought out most clearly and distinctly in the last paper by Dr. Hann, of 86 pages, in April, 1891. Speaking of fluctuations of pressure and temperature at p. 367, Dr. Hann says: "Für die Erdoberfläche sind dieselben seit Langem bekannt." "For the earth's surface have these been for a long time well known." I am sure that every one will admit that at the earth's surface as a storm comes up the temperature rises and is the highest during the storm. As a high area advances, the temperature falls, and is generally vastly lower during such pressures than during storms, especially in the winter season. On page 370 Dr. Hann gives the temperature conditions during barometer maxima, and on page 375 the conditions with minima. The sea level station at the base of Sonnblick was Ischl, and I give here the temperature in both maxima and minima during the colder months.

Temperature at Ischl, Cold Months.

	Barometer Reading.		
	Maximum.	Minimum.	Difference.
	Centigrade.	Centigrade.	Centigrade.
October	8.4°	6.5°	1.9°
November	2 8	2.4	0.4
December	− 2.6	− 3.3	0.7
January	− 1.5	− 0.3	− 1.2
February	1.0	− 4.8	5.8
March	4.0	− 5 1	9 1
Mean	2 0	− 0.8	2 8

This table shows that at the base of Sonnblick during every month except one the temperature is higher in a maximum barometer reading than in a minimum, and the average difference is 2.8° C., or 5.0° F. This exact and marked reversal of the universal law is very significant and proves conclusively that there has been either a most serious error in studying or selecting out the different cases, or that the universal law does not hold for this region.

It would seem that Dr. Hann himself now recognizes the difficulty in using these records, for he says, in a paper on Ben Nevis (4,406 feet), *Meteorologische Zeitschrift*, December, 1892, p. 457: "Wie Herr Buchan schon in seinem ersten Bericht hervorhebt, zeigt auch der Ben Nevis sehr häufig die Erscheinung hoher Temperatur und grosser Trockenheit selbst mitten im Winter, sobald er in das Gebiet eines Barometermaximums zu liegen kömmt. Es treten dann auch öfter die sogenannten Temperaturumkehrungen auf, was hier besonders bemerkenswerth ist wegen der freien Lage von Fort William am Meere, welche eine Stagnation kalter Luft an der unteren Station ausschliesst." "As Mr. Buchan showed in his first report, very often Ben Nevis has the appearance of higher temperature and greater dryness in winter as soon as a barometer maximum lies in that region. It shows then often the so-called temperature reversal, which here is especially remarkable because of the free position of Fort William on the sea, which prevents a stagnation of cold air at the lower station." The question seems to be clearly set forth in these words, and it is probable that the advocates of the view that in the centre of our high areas there is a rise in temperature at some height above the earth will be willing to stand or fall by the proofs at Ben Nevis.

The date of this abnormal heat was Dec. 31, 1883.[1] Another occasion was on Nov. 18, 1885, and a third on Feb. 5, 1886. These are the only marked cases from December, 1883, to February, 1886, though there were minor cases of no importance on Jan. 16 and Dec. 22, 1884, and on Nov. 10, 1885. I have made a careful search of all the published observations for maximum barometer readings in the four cold months, and have found 70 cases. That is to say, out of 70 cases, only 3 show a marked departure from the law that there is the same oscillation of temperature at the summit as at the base of Ben Nevis. But this is not all. On Dec. 31, 1883, the motion of the high area was quite slow and the wind on Ben Nevis almost a calm, thus causing a stagnant air. On Dec. 28, or three days earlier, the temperature at the summit began falling, and in 24 hours it had fallen nearly 22° F., or *more* than at the base in the same time. This shows that the usual law was acting even in this case and that the subsequent rise was due to an abnormal condition and not to the fact that the temperature was higher in the centre of the high area than on either side.

In the other case cited by Dr. Hann on Nov. 18, 1885, the conditions were exceedingly abnormal, as the high area moved from the *east* toward the *west*. It would be impossible to reason as to the ordinary temperature conditions in a high area from such a case. It is an interesting fact that in the latter case the oscillation of temperature at the summit was precisely the same as at the base, except that the fall and rise at the summit was a little greater than at the base, and it took place about 24 hours later, instead of earlier as is usually the case. The usual law of lower temperature in the centre of a high area is abundantly borne out at Ben Nevis, and I have found the reverse law in the centre of a low area also true at that station.

I have thus dwelt at some length upon these studies for the reason that they have been largely accepted by European meteorologists and have served to overthrow nearly every hypothesis that has been regarded invulnerable in the past. Is there not here the best proof in the world of the extreme need of an exploration of the atmosphere at the seat of these disturbances? Meteorology needs, above all things else just at present, a full and complete setting forth of the facts to be gleaned in the upper atmosphere. An array and study of these facts would give us a good foundation on which to lay the corner-stone of a good and exact science. It would be of inestimable value in forecasting the weather and in removing our ignorance, which is so serious a drawback at present.

We do not know positively the simplest conditions in the atmosphere. Glaisher once left London in a pouring rain and emerged into clear sky after rising only 800 feet. At another time he found rain falling in a cloud 15,000 feet high. In this country no rain has been observed in balloon ascensions above 9,000 feet, and it is probable that a large part of our rain forms at a height less than 6,000 feet. We do not know the thickness of a rain-cloud nor its temperature. Some think the temperature must be higher than that of the surrounding air, else the storm would quickly cease; others think that no rain can form unless the temperature is lower than the outside air. Our books are full of speculations as to the dynamic heating of the air and the conditions needed to originate and maintain our storms and high areas. The evidence seems quite clear that all these theories, often contradictory among themselves, would not account for a tithe of the energy displayed, and an exploration is needed to determine this fact, or to establish the truth.

Is there an ascending current in our storms, or a descending one in our high areas? These are theories of the deepest interest. The evidence seems to show that there is not a transfer of an air-mass in any direction, either up or down or horizontally, in our storms or high areas. Professor R. H. Scott, after giving all possible sources of rain formation, decides that the only one that can be maintained on theoretical grounds is that rain is formed in an ascending current of warm, moist air. A determination of this question would be of inestimable value in all studies and researches as to the natural or artificial formation of rain.

[1] Misprinted 1884.

In several ascensions in this country it has been found that there seem to be rather definite layers of moisture even in a clear sky. Sometimes two layers have been found at different heights. These would seem to be exceedingly significant facts. Do these layers serve as conductors for electric currents, as seems to have been very guardedly stated by Professor Loomis? How do these layers thicken as a storm comes up, or, rather, is the thickening process a precursor to the storm? Does this thickening in a certain definite direction show in what direction the storm will subsequently move, or is it caused by the conditions accompanying the storm? Do these layers rise or fall, or what is their movement under different atmospheric conditions?

What relation does the dust in the atmosphere bear to these layers? Is there an increase of dust in definite layers? Is dust needed to produce this thickening? It seems to me the careful and painstaking investigations of Professor Barus in cloud condensation must bear valuable fruit as soon as he turns to the ordinary conditions in our storms, and for this the study can be prosecuted only with great difficulty, except in nature's own great laboratory.

A serious drawback in the past to successful balloon exploration has lain in the lack of suitable instruments. Professor Glaisher often took up instruments enough to stock a meteorologic observatory, and in a single ascension once broke nearly $500 worth. What is needed is an instrument that can be read very quickly, once a minute if possible, and, at the same time, do its work very accurately. A sensitive aneroid will give the pressure, and a sling psychrometer will give the moisture conditions. Various rather singular objections have been raised to this instrument. One is that it will give 5° too high temperature under strong insolation. This experiment has been tried, and it is known that under the strongest insolation possible the temperature will be less than .8° higher in the sun than in the shade. Another objection raised has been that it will give a lower relative humidity in bright sunshine than in shade. This is entirely wrong, because the muslin coating of the wet bulb is a vastly better absorber of heat than the bright bulb, and hence, if anything, in bright sunshine the relative humidity must be higher than in shade. It is also said that the heat of the balloon will tend to raise the temperature of the sling thermometer because it cannot be used far enough away from the basket. In a comparison between the sling thermometer and another so-called standard (aspiration thermometer) the greatest difference between the two occurred when the balloon was moving horizontally, and the least when the balloon was ascending most rapidly, so that this objection utterly fails. The true criterion of an accurate instrument is that it shall give the same temperature of any stratum in a rapid ascent and descent, and this is fulfilled in a marked degree by the sling psychrometer. I have used this instrument for over eight years and in five balloon voyages, and am satisfied that it is a perfect instrument and one that responds at once to any demands put upon it.

The expense of ballooning in the past has been enormous, and a serious drawback to its prosecution. One is amazed to read that in certain high ascensions, to five miles and over, the balloon of 90,000 cubic feet capacity was filled plump full, thus necessitating the carriage of about a ton and a half of ballast. This ballast had to be poured out and more than half the gas wasted before reaching the height desired. It is no wonder that the aeronaut was completely exhausted with his labors with the ton of ballast. All this gas that had to flow out, because of expansion, was a dead loss, say, $150 for each ascension, and after landing the remaining gas was emptied. All of this expense can be avoided, as I am firmly convinced. It is well known that if a balloon leaves the earth at all, it will rise till the envelope is plump full. If the balloon will rise when two-fifths full of gas, it will continue to do so till it has reached more than five miles, the limit desired at present, though there is no reason why ultimately we may not ascend to the extreme limit to which hydrogen may carry us by the use of a pneumatic cabinet. It is proposed to employ a small balloon with hydrogen gas. A balloon of 20,000 or 30,000 cubic feet will easily carry two men when half full, and the enormously less labor of handling it, as compared with that of handling one of 100,000 cubic feet, can hardly be estimated. The risk in a high wind of the smaller balloon is vastly less than of the larger. Every way the smaller balloon presents advantages over the larger. The first cost of a balloon of 20,000 cubic feet would be $600. The cost of a half charge of gas need not be over $30, and may be less. It is hoped that the balloon will be sufficiently tight to hold its gas for a long period. In Europe balloons have been made with gold-beaters' skin that have leaked only ½ of 1 per cent in 24 hours. I think the leakage of a cloth balloon when properly made need not be over 4 or 5 per cent, but the figures in this country are exceedingly meagre and unsatisfactory. After an ascension it will be a very simple matter to conserve the gas, and, if wished, an addition may be made at the landing-point with gas from a flexible holder, which may be easily transported from point to point.

An interesting problem presents itself as to the behavior of the gas in a rapid ascent or descent. Theory indicates that in a rapid expansion dry gas will cool 1° F. in 186 feet ascent, so that at 25,000 feet the temperature would be about 130° lower at the centre of the balloon than at the outside air, provided the ascension was quick enough to prevent the heat from striking in. Now experience in balloon ascents shows that the gas in a balloon is invariably *warmer* than the outside air. Exactly the reverse is true in a rapid descent, both as regards theory and practice. Whether this is due to the fact that the envelope retains its heat or not, it still remains that we have here apparently a means of making our ascensions with the loss of little or no gas at the valve. At the highest point our gas will be cooled and lose its buoyancy, which allows a fall in the balloon, which is always greatly accelerated as we approach the earth, and after landing the balloon may be anchored till the sun's heat has warmed the gas, which will enable another trip with the same gas.

The risk in such ascents has been greatly exaggerated by some from the serious and often fatal accidents that have attended jumping with parachutes and ascending in hot-air balloons. The modern balloon, with its very long drag-rope and rip-cord, are very safe. Even in case the balloon should burst, the envelope catches in the netting and acts like a parachute in breaking the fall. Mr. Wise, the veteran, once ascended to the height of a mile and purposely exploded his balloon in order to show that there was no great risk in such an adventure. In one case, Mr. King and a married couple were in a balloon which exploded at the height of a mile, and without serious consequences It should be noted that a new balloon will not explode. Glaisher reports having ascended with a balloon full of gas at the rate of 4,000 feet per minute; this was a remarkable feat. It is not the intention to ascend faster than 1,000 feet per minute, and at this rate the danger of bursting is almost nothing.

Some may think that such observations may be made at vastly less risk, expense, and discomfort on mountain tops. Undoubtedly there are some observations of temperature that may be made in this way, but even in this case we cannot tell just what effect the summit will have. Observations of rainfall, clouds, electricity, etc., are entirely impossible on mountain-tops, for the reason that these have a peculiar action of their own entirely different from that of the free air. It seems probable that the mountain acts like a point in the atmosphere from which there is a continuous discharge of electricity, as in the case of a point on the conductor of an electric machine.

The exploration of the atmosphere cannot be carried on in Europe to as good advantage as in this country, for the reason that they do not have the normal low areas and high areas travelling at some velocity that we have. The conditions of the atmosphere are so different in the two countries that we must make our own researches. I trust I have shown the great need of such exploration. I know of no endowment of $5,000 or $10,000 that would pay so rich and immediate a harvest as this for ballooning. Thousands are spent in visiting the inhospitable north, while a field just at our hands, which may be explored at vastly less expense and risk, and which promises immeasurably greater returns, is left unexplored and unvisited.

March 31, 1893.

LOSS OF DRY MATTER BY THE SPROUTING OF CORN-SEEDS.

BY E. H. FARRINGTON, CHEMIST, AGRICULTURAL EXPERIMENT STATION, CHAMPAIGN, ILL.

SEEDS of the corn-plant were placed in damp cotton and left to sprout in the dark for nine days. Four of these seeds partially sprouted, then moulded, failing to develop further. They lost by this treatment 9 to 18 per cent of the dry matter in the original seed.

Two seeds, under the same conditions for nine days, sprouted and developed a corn-plant. The root and stem of these plants each measured two to three inches, and their weight was from three to three and one-half times that of the original seed. It was found, however, that when the water was dried out of these young plants the dry matter in them was 20 and 31 per cent less than the seed contained.

Several estimations were made of the per cent of water and dry matter in a sample of corn. These results were used for estimating the weight of water and dry matter in the corn which was taken from the same sample and sprouted.

Details of Weights in Grams.

	Dry Matter.	Water.	Total.
Weight of seed before sprouting.........	0 271	0.042	0.313
After nine days sprouting in damp cotton, plant with seed attached, weight..	0.187	0.747	0.934
Gain or loss of plant over seed..........	− 0.084	+ 0.7 5	+ 0.621
Per cent gain or loss was of weight in the seed....................................	− 31.0	+ 1680	+ 198
Duplicate observation gave.............	− 19.8	+ 1945	+ 239

This shows that in sprouting the white plant had taken up water but lost in dry matter.

This experiment was repeated June 3, 1892, by sprouting the seed in the soil of a corn-field instead of cotton. One week after planting, four of the plants were dug up. They were about two inches above ground and had two green leaves. The shell of the seed still clung to the plant. The root was about five inches long, making a total length of about ten inches from tip of leaf to end of root.

The weight of these green plants, free from soil, was about four times that of the seed planted, but they contained from 58 to 79 per cent only of the dry matter in the original seed.

During the week these seeds were growing the climatic and soil conditions were ideal for corn.

Details of Weights and Measurements.

Plant No.	Weight in Grams.			Measurement of Plant. Inches.			Dry Matter.	
	Seed.							
	Dry Matter.	Total.	Green Plant.	Tip of Leaf to Seed.	Above Ground.	Roots.	In Plant.	Per cent of that in Seed.
1	0.416	0.479	1.638	4	2	5	0.331	79.3
2	0.357	0.412	1.447	4½	2	5	0.210	58.8
3	0.347	0.450	1.549	3½	2	4	0.273	78.6
4	0.393	0.457	1.454	1½	3½	4	0.310	78.2

Two weeks after the seed had been planted, five plants were cut at the surface of the soil, and the weight and measurements of each plant above ground was compared with the weight of the seed. This shows that corn-plants, having a height of ten to fourteen inches above ground, weighed when green four to eight times as much as the seed, but the dry matter in these plants was from 86 to 130 per cent only of that in the seed planted.

Details of Weights and Measurements.

Plant No.	Weight in Grams.			Height of Plant above Ground. Inches.	Dry Matter.	
	Seed.		Green Plant.		In Plant.	Per cent of that in Seed.
	Dry Matter.	Total.				
1	0.378	0 437	3 235	14	0 493	130 4
2	0.346	0.400	1.790	9½	0.3 0	86.7
3	0.395	0.456	2.470	11½	0.435	110 1
4	0.404	0.466	2 610	11	0.348	86.1
5	0.424	0.490	3.540	12	0.437	103.0
Growth above ground of two plants three weeks after planting.						
1	0.348	0 402	16.60	21½	1.826	521 6
2	0.413	0.477	18.60	20½	2.045	495 4

ELECTRICAL NOTES.

Some of the practical results of Dr. Sumpner's work on photometry were alluded to in a previous note. As the Proceedings of the Physical Society are not generally accessible, and most of the abstracts given are rather brief, it may be worth while to give a short account of some of the more theoretical results.

The first is the practical demonstration of the very approximate accuracy of the cosine law of reflection of such substances as white paper, tracing cloth, and white cloth. From this follows the remarkable result, confirmed by experiment, that placing a piece of white paper behind a source of illumination more than doubles the illumination at a point normal to the plane of the paper, while the placing of a mirror in the same position does not quite double it. The reason of this is at once seen to be the fact that the reflecting power of white paper and the mirror are about the same, but that, of a given amount of light falling on the paper, in consequence of the cosine law, the greater part is reflected normally to its surface, whereas in the case of the mirror, the absorption of the glass is greatest in the case of the light falling perpendicularly to it, and so the greater part of the light is given off in directions which are not normal to the surface.

In the discussion following, it was pointed out that no known shape of the roughnesses would lead to the mathematical deduction of the cosine law, so it is probable that the phenomenon of diffusion of light is of a somewhat more complicated nature than is generally supposed. It is to be hoped that the definitions used by Dr. Sumpner will be generally employed in photometric work. They are as follows:

1. Candle-power.— The candle-power of a lamp is measured by the ratio of the illumination of the light considered, to that of a standard candle, both sources being at the same distance from the object illuminated.

2. Illumination.— The unit of intensity of illumination is that given by a standard candle at a distance of one foot.

3. Unit quantity of light.— Unit quantity of light is the quantity of light which falls on a surface of one square foot placed at a distance of one foot from a standard candle, and so that a normal drawn to the surface at any point, passes through the source of light.

The name candle-foot is given to the unit quantity of light.

From the definition, a source of light, candle-power X, gives out a total quantity of light equal to 4 π candle-foots.

4. Brightness.— This definition only applies to solids which become sources of illumination, either through incandescence, as heated platinum, or through reflection, as paper exposed to sunlight, i.e., only to such substances as obey the cosine law.

A surface has unit brightness when a point at a distance of one foot from a surface of one square foot of the substance, and so placed that a normal drawn from any point of the surface passes through, the point, is illuminated with unit intensity.

From the definition, it follows that the total quantity of light given off by one square foot of surface of brightness, X is πX.

One interesting result, following from the considerations which lead to the last of these definitions, is that given by Dr. Sumpner, as it affords an explanation of snow-blindness.

The total quantity of light reflected from the snow will nearly equal the amount which falls on it. Therefore, if C be the intensity of the illumination of the sun at the surface of the snow, the brightness of the snow at a distance of one foot from it will be C/π. Therefore, if the observer is standing so that the snow-field subtends a solid angle of 90 degrees, we may easily find that the illumination at the point where his eye is, is nearly C, or that the effect is nearly the same as if he were looking straight at the sun.

R. A. F.

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.

Early Attempts at Storm-Warning.

IN reading Haweis' "Music and Morals," I found on page 368 a statement of interest to meteorologists. Writing of the famous Strassbourg tower, he says: "The second bell, recast in 1774, is named 'The Recall' or 'Storm-Bell.' In past times, when the plain of Alsatia was covered with forests and marsh land, this bell was intended to warn the traveller of the approaching storm-cloud as it was seen driving from the Vosges Mountains towards the plain."

Probably Kopp, Günther, van Bebber, or Hellmann, in their records of antiquarian research, have mentioned this early attempt at storm-warnings, but I do not remember having seen anything about it.

FRANK WALDO.

Princeton, N.J., April 5.

Pre-Historic Remains in America.

IN his letter in *Science*, March, 31, under the above title, Professor Cyrus Thomas misunderstands the quotation which he makes from my "American Race." He observes, "If the settlement was at one point by one race, and this race was never influenced by another, it is difficult to imagine in what respect the moulding process acted." Is it? Plainly the moulding process acted by modifying the intrusive population to another and a fixed racial type by long subjection to an environment to which previously it had never been exposed. Nothing is better recognized than such a process; it is taken for granted by all writers, as, for instance, by Dr. Braislin in the same number of *Science* in which Prof. Thomas's letter appears; and why such an objection should be offered to my statement, it is even more "difficult to imagine."

The general theory advanced by Professor Thomas of a fundamental difference between the civilizations of the Atlantic and Pacific groups, is one for which I have never found any evidence. He must know that the ancient civilization of the Mississippi Valley offers as strong, if not stronger, traits of analogy to that of Mexico and Yucatan than does that of the Haidahs. Consider the designs shown on the engraved shells, so well shown in the beautiful monograph of Holmes, or the copper work of the mounds of Ohio and Georgia! In view of such evidence, how could Prof. Thomas write, that "no such resemblance to those of the Atlantic slope is observable?" Is he not also aware that both the Nahuatl and Maya languages trace their affinities exclusively to the eastern and not to the western water-shed of the continent?

As for the "mathematical probability" referred to by Dr. Dall, it is illusory. We find "interwoven chains of customs and belief" of the most seemingly fanciful and artificial character in nations so remote that the theory of transmission is impossible —such as Niblack shows between New Zealanders and Haidahs, or as Morgan adduced between Iroquoiean and Dravidean tribes. These do not depend on transmission, nor yet on chance, but on the unalterable principles of human psychical development, which proceeds under fixed laws, operates largely on the same or similar materials, and produces identical or analogous results.

In conclusion, I repeat what I have said more than once before, that I challenge any one to cite a single American language showing clear traces of Asiatic or any other foreign influence; or a single native American art or industry obviously traceable to foreign culture. D. G. BRINTON.

Philadelphia, April 5.

Auroras.

SINCE 1572 there have been 106 auroras seen as far south as the Mediterranean in Europe or Virginia in this country, and exhibiting features constituting displays of the first magnitude. In making up this list, the records consulted have been sufficiently complete to insure that very few, if any, displays, having the geographical extent indicated, have been omitted. The list comprises, practically, all the really great auroras during the past 420 years, few, if any, of which would have failed to be visible even in full moonlight or strong twilight. It is a very curious fact, that very few of these splendid displays reported from large numbers of localities and attracting the attention of even the most indifferent, fall near the solstices, while they are most numerous near the equinoxes. This peculiarity has long been known, but that the distribution is real and not factitious, depending upon twilight in the summer and cloudiness in the winter, is best shown by admitting only those auroras which are certainly on a sufficiently grand scale to insure that they will without fail be seen and widely reported. The monthly distribution of the displays belonging assuredly to this class during the past 420 years, is as follows:

January	6
February	17
March	14
April	8
May	3
June	0
July	4
August	4
September	14
October	21
November	12
December	3
Total	106

M. A. VEEDER.

Lyons. N.Y.

The Palæolithic Man Once More.

IN the first number of the new *Journal of Geology*, published under the auspices of the University of Chicago, Mr. W. H. Holmes, in the capacity of co-editor in "Archeologic Geology," has given to the world a long and labored article, in which he endeavors to demonstrate that because he has failed to find any evidence of the existence of the palæolithic man in the Trenton gravels, therefore no such evidence has ever been found by any one else. In his characteristic style he designates as "gravel searchers, unacquainted with the nature of the object collected and discovered, and little skilled in the observation of the phenomena by means of which all questions of age must be determined," several of the foremost men of science of our time, who claim to have discovered such evidence there. As he also makes

CALENDAR OF SOCIETIES.

Anthropological Society, Washington.

Apr. 11.—Frank Hamilton Cushing, Zuni Song and Dance.

Biological Society, Washington.

Apr. 8.—J. W. Chickering, The Botanical Landscape; Frederick V. Coville, Characteristics and Adaptations of a Desert Flora; C. W. Stiles, Notes on Parasites,—the Cause of "Measly Duck," with Microscopic Demonstration; R. R. Gurley, Natural Selection as Exemplified by the Cackling of Hens.

Geological Society, Washington.

Apr. 12.—Symposium—Subject: The Age of the Earth, taking as a basis for discussion the article by Mr. Clarence King in the *American Journal of Science* for January, 1893. The discussion was opened by Mr. Gilbert, and many others participated.

the astonishing statement in the same article that "most of these so-called gravel implements of Europe are doubtless the rejects of manufacture," pre-historic archæologists, and especially those of Europe, will draw their own inferences as to the qualifications for pronouncing an *ex cathedra* decision of the man who made the statement (*Science*, Nov. 25, 1892) that "there is not in the museums of Europe or America a single piece of flaked stone found in place in the gravels of America and satisfactorily verified that can with absolute safety be classified as an implement at all."

I have had occasion elsewhere to refer to Mr. Holmes's fondness for making startling assertions, instancing his statement about the Indians, in the same article in *Science*, that their "quarrying was accomplished mainly by the aid of stone, *wood and bone* utensils, aided in some cases perhaps by fire. With these simple means the solid beds of rock were penetrated to depths often reaching twenty-five feet."

The readers of *Science* have lately had an opportunity of observing also that Mr. Holmes "strongly deprecates personalities in scientific discussion." HENRY W. HAYNES.

Boston, March 30.

BOOK-REVIEWS.

The History and Theory of Money. By SIDNEY SHERWOOD. Philadelphia, J. B. Lippincott Co. $2.

THIS book contains twelve lectures delivered in the university extension course at Philadelphia last year before a company of bankers and others interested in the subject of finance Half the lectures are professedly historical and the other half theoretical; but the historical element is really predominant throughout them all. This seems to us a mistake, for the history of money cannot be properly understood nor appreciated without a previous acquaintance with the theory, and in these lectures the theory is not stated with anything like the clearness and fulness which the subject demands. However, it was expected that the attendants on the lectures would read and study for themselves during the progress of the course, the books recommended for their use being named in this volume; and such reading would supplement the instruction given in the lectures. Mr. Sherwood, who is attached to the Wharton School of Finance in the University of Pennsylvania, shows a thorough familiarity with his subject, and, what is quite as important, he has no hobbies to ride, and is not prone to extreme or one-sided views. He begins by showing what money is for, what purposes it fulfils in the world's economy, and then proceeds to treat of the different kinds of money in use, with remarks on coinage, on the history of the precious metals, and on government notes and bank notes, with brief discussions of some of the many economic questions which those subjects involve. The lectures are expressed in a plain and straightforward style, which the hearers could readily understand, and they were evidently enjoyed by those who listened to them. For our part, however, we have found the discussions at the end of each lecture, and which are here reported in brief, the most interesting part of the book as well as the most suggestive. There were many persons in the audience well equipped with both theoretical and practical knowledge of the subject, and their discussions with Mr. Sherwood and with one another called up many points that were not touched upon in the lectures, and presented various and sometimes conflicting views. Among the debaters was a lady of socialistic proclivities, whose remarks and questions added variety and piquancy to the scene, though she did not appear to have many sympathizers. On the whole, though it cannot be regarded as an adequate scientific treatise on money, this book will certainly have an interest for all who care for its subject.

AMONG THE PUBLISHERS.

J. B. LIPPINCOTT COMPANY announce as in preparation "Vagaries of Sanitary Science," by F. L. Dibble, M.D.

Exchanges.

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For exchange.—Will exchange an "Ideal" Microscope of R. and J. Beck, London, 2 eye pieces 3 objectives, 3 inch, 1 inch, 1-6 inch; bull's eye condenser on stand, substage condenser, mechanica stage, etc., for any of the leading makes of type writers. Particulars by mail. DELOS FALL, Albion College, Albion, Mich.

For sale or exchange.—I have a few copies of my translation of "Strasburger's Manual of Vegetable Histology, 1887," now out of print, which I will send post-paid for $3 or for one dozen good slides illustrating plant or animal structure. Address A. B. Hervey, St. Lawrence University, Canton, N. Y.

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Wants.

THE undersigned desires specimens of North American Gallinae in the flesh for the study of their pterylosis. These species are especially desired: *Colinus ridgwayi, cyrtonyx montezumae, deudragapus franklini, lagopus welchi, tympanuchus cupido* and *pedioecetes phasianellus.* Any persons having alcoholic specimens which they are willing to loan or who can obtain specimens of any of the above are requested to communicate with Hubert Lyman Clark, 3922 Fifth Avenue, Pittsburgh, Pa.

WANTED—Second-hand copy Tryon's Systematic Conchology. Please quote condition, date, price. H. L. Osborn, Hamline University, St. Paul, Minn.

A COMPETENT TEACHER of botany in college or university is open to engagement. Address L., Box 86, Rochester, Mich.

CAN any one inform me as to the age to which cats have lived? I have one twenty years old. Edward D. Webb, 132 W. Eighty-first St., New York.

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WANTED.—By well-qualified and experienced science master and associate of the Royal School of Mines, London, aged 26 (at present in England), a mastership in technical college or university for any of the following subjects: Engineering sciences, geology and mineralogy, physics, chemistry and metallurgy, etc., etc. Can provide excellent references and credentials. Apply, J. G., 17 Sussex St., Rochdale, England.

QUERY.

Can any reader of *Science* cite a case of lightning stroke in which the dissipation of a small conductor (one-sixteenth of an inch in diameter, say,) has failed to protect between two horizontal planes passing through its upper and lower ends respectively? Plenty of cases have been found which show that when the conductor is dissipated the building is not injured to the extent explained (for many of these see volumes of Philosophical Transactions at the time when lightning was attracting the attention of the Royal Society), but not an exception is yet known, although this query has been published far and wide among electricians.

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N. D. C. HODGES, 874 BROADWAY, N. Y.

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SCIENCE

NEW YORK, APRIL 21, 1893.

LABORATORY INSTRUCTION IN PHYSICS.

BY D. W. HERING, UNIVERSITY OF THE CITY OF NEW YORK.

THERE are various practices, and seemingly but two clearly defined methods of teaching physics in college laboratories. The first method, which may be called the progressive one, treats the general subject of physics by going through its various divisions successively, until the whole ground has been covered, whether thoroughly or superficially. For students who have had no preliminary training in physics, this method is the only practicable one if they are to begin their study in the laboratory. The other may be called the method of analysis. It assumes that the pupil has received a fair course of instruction in the principles of the science before he enters upon laboratory work. Then it is a matter of indifference whether his first exercise is one in optics, or in electricity; in radiation, or in specific gravities. He will examine a body of any sort with reference to its various properties, taking account of as many as he can, which in some instances may embrace nearly the whole range of physics. This method then does not present the different features of physics so much as the physical features of different things.

At first sight it would appear as if the method that is pursued for the direct purpose of learning the science would be the one best fitted to give an acquaintance with it, and perhaps this would be true if sufficient time could be given to it to deal with the various branches of physics with tolerable thoroughness, but laboratory work by an untrained pupil is slow at best, and time is limited. It is important, therefore, to follow the plan that will give good results without loss of time.

If physics as a science were distinctly progressive in its nature, one step being essential to a comprehension of the next, and therefore of necessity a preliminary to it, there could be no question as to the best order of proceeding in teaching or in learning the subject. There would still be room for question as to how much should be done by the teacher in experimental illustration with discussions, before putting the pupil to experimenting on his own account, but the order of dealing with the subject in any case would be determined beforehand. But it is thus progressive to only a limited degree. Except for the principles of mechanics, which permeate the entire science, physics, in all its diversity, may be dealt with regardless of the order in which the subjects are taken up And this exception is not always recognized. Among recent standard text-books which are meant to be especially adapted to laboratory practice, but which mean to omit none of the elementary principles of physics, there is every variety in arrangement of topics. One begins with specific gravity and air pressure, follows with dynamical principles, and presents light as the final subject. Another begins with magnetism, introduces the last third of the work by dynamics, and closes with sound. Still another begins with properties of matter and dynamics and ends with light; while a fourth begins with the mechanical powers and closes with magnetism and electricity. Even the special divisions, as electricity, for example, can scarcely be said to be developed from one principle that necessarily comes first, to another that can be reached only at the end of a well-defined series. Some classification of topics can always be made, but the tendency to-day is to diminish rather than increase the number of classes. Considerations of intrinsic difficulty, or length of time that can be given without interruption, or the season of the year when sunny days may be expected, or other special points may lead to a preference as to the order of subjects, but there is little in the nature of the subjects themselves to determine it.

The status of the student when he is to enter upon the work which this paper is to discuss, will depend upon the manner in which he obtained his first training in physics. He may have acquired his early knowledge by experimentation from the beginning, or he may have been taught from descriptive text-books supplemented by experimental lectures from the teacher, or he may have had a combination of both. In the first case, he had to find out principles and laws as well as (to him) disconnected facts by his own experimentation; in the second, he has been made acquainted with the leading laws and properties and perhaps has had some opportunity to verify and apply them. Whether an attempt to learn physics from the beginning by practice is profitable or advisable has been much discussed, and it is outside of our purpose to enter upon that question. It is a plan that has grown in favor greatly of late, and has been insisted upon by Harvard College, as a preparation for those who are to pursue the subject in college. Let us suppose the pupil to have acquired a general, though elementary, acquaintance with the principles of the science,— that he has reached the standard of at least a well-prepared college junior. For this he has probably been called upon to cover the whole range of the subject whether by experiment, or by recitations and experimental lectures. The advocates of the two methods of preparation will find points to offset one another in the results attained. The experimental student will have acquired his knowledge in a very valuable way, by objective study, by the inductive method. He will have "learned to do by doing." This has become a favorite idea with educators in almost every branch of learning, and its advantages are undeniable in most lines of work, but they are not equally great or equally obvious in all branches or at all stages. It is a most effective way so far as it goes, but in physics the experiments concerning any one point, or involving any law, will have been so small in number under the best opportunities, that the student must infer the law from instances altogether too few and too little varied, to justify an inference. Potent as the inductive method has been in science, its demonstrations are never incontestable, they never rise above a moral certainty, and do not even approach it, if the instances upon which the conclusions are based are not numerous, or else very accurate. The student will in reality have done nothing more than illustrate a point, doing in a crude way what the lecturer before a large class does in a better way. Still the experiment and its results will impress themselves upon him because he did the work himself. In this he will have the advantage of the lecture-taught student. The knowledge of the latter, however, is likely to be more correct as to principles. On the whole, the two classes may be said to approach the higher laboratory practice about equally well equipped: the former better prepared for manipulation with perhaps less readiness to appreciate the science; the latter better prepared to discriminate as to principles, but less expert in determining them. Didactic and experimental instruction are now so well combined in some secondary schools as to make their work superior to that offered in many colleges. Having been fairly well taught by any method, we may suppose the student ready for practical work somewhat more advanced than is to be had in secondary schools, or even in the general course of physics in an average American college. What plan shall be followed in his laboratory work? Presumably that plan is best which is best fitted to accomplish its purpose. What is the purpose of his work? Usually not independent research or original investigation. Work of that class is generally undertaken only by graduates or special students, who are not obliged to accomplish a definite amount in a given time. The higher laboratory work of the college undergraduate is for the purpose of making him practically familiar with physical laws, not in one particular branch of physics, but throughout the whole subject; for training in making and reducing scientific observations; for acquiring skill in manipulating and adjusting apparatus; all which is to result in giving him a good general knowledge of physics, if he follows the study no further, or to fit him for independent research if such is his design for the future. It is thus intermediate in its thoroughness and definiteness between the preparation in elementary general principles of the science, and the work of the graduate or the advanced special student.

What is likely to be the experience of a student in the college laboratory under what we have called the progressive method, supposing he has time enough in prospect to cover the entire field? He will begin probably with a dozen companions in his division, with the topic placed first in the order chosen, say properties of matter, and dynamics. One of the first operations he will be called upon to perform will be that of weighing a body. The skillful use of a delicate balance, will involve the critical study of the balance itself. This will afford a good exercise in dynamics. To reduce to weight *in vacuo*, will necessitate the reading of the barometer, and an application of the laws of Boyle and of Charles, for the effect of temperature and pressure upon gases. Thus he will have been carried at once beyond the immediate subject of physics to which he was intending to apply himself. However, it was merely an excursion. He may continue with this work until he has learned several modes of weighing, and with several types of apparatus. He must learn to measure time. Here he will be introduced to the use of the pendulum, perhaps the chronograph, and other devices for comparing intervals of time. The method of coincidences will be especially serviceable, if he has a seconds clock and a reversible pendulum, by which to determine the accelerative force of gravity. Atwood's machine, besides illustrating the laws of falling bodies, will serve for critical work in mechanics, if the effect of friction, and the mass of the large pulley are to be considered. Various other exercises in mechanics may be given him; he will hardly go on with less than these, and to each of these he will have given enough time and attention to become proficient in work of this kind, and will have given attention to as little else in physics as possible.

If he passes next to the subject of heat, he will probably remain at this until he has dealt, if possible, as fully with its various phenomena. So far as these phenomena involve mechanics he will have had some especial preparation by his previous work, and now he will be doing that work over again. For instance, in hygrometry the same principles relating to the effect of temperature and pressure upon the volume of a gas, will have to be considered. In specific heats, he will again go over the same kind of work as to masses and densities that he performed with the balance, and so on. But he will do nothing in electricity, even with the heating effect of an electric current, because he has not yet come to electricity. He will do heat pretty thoroughly but not completely. It will be so in each branch he studies. In every one, to do an exercise which is nominally one of a particular class, he must employ principles of classes previously studied. Not until he has gone over the entire range of topics, from the first to the last, will he have taken account of all the principles meant to be included in his course, but when he has done so he will have had a most exhaustive training, for he will then have done nearly everything, not once or twice only, but very many times. His training by that time ought to be excellent, and his knowledge extensive and acute. But to reach such a stage would require longer time or more exclusive devotion to physics than is usually provided for in an undergraduate course. If any thing less is done, however, it means not the omission of certain exercises, but of all the exercises pertaining to one or another entire class of topics. For instance, he may omit sound, or light, wholly. That would make a serious break in his course. By any of the usual arrangements, therefore, the whole subject will be obviously more or less disjointed if it is regarded as made up of members. Fortunately, the highest treatises seek to unify it instead of to dismember it.

How will the student fare by the method of analysis? For an example, give him a piece of plate glass of convenient size. Of the various determinations regarding this specimen, some will be qualitative, others quantitative. Let him determine:

1. Whether it is regular, and if so, its form....... Qualitative
2. Its dimensions, giving area, thickness and volume.................................... Quantitative
3 Its mass (weighing)........................ Quantitative
4. If C.G.S. units are employed, this leads at once by dividing mass by volume, to density..... Quantitative
5. But check this by weighing in water, for sp. gr. Quantitative
6. If the plate is of considerable size, say 25 cms. × 30 cms. and a small spherometer is available, test the surface for flatness, and map out irregularities.............................. Quantitative
   (This will serve to show the meaning of instrumental limitations as to precision and accuracy)
7. If possible, compare this with the irregularities of surface, shown by reflection of light, with telescope, or by interference bands when in contact with true "flat."................... Qualitative
8. Determine its index of refraction.............. Quantitative
9. Its hardness................................. Qualitative
10. Its color, by absorption in spectrum.... ... . Qualitative
11. Whether it is homogeneous, by transmission of polarized light............................ Qualitative
12. Its specific heat..... .... Quantitative

Some of these may be out of the reach of many laboratory equipments or only determinable with the help of instruments too delicate to be put into the hands of any but the best students, but still other determinations might be made. Undoubtedly by the time the student has finished such an analysis he will have a very complete knowledge of the specimen he has been working upon, and although, in the instance here cited, the object under scrutiny may seem a trivial one, and the knowledge of its properties no useful addition to his stock of information, not every one may be so. Yet what a range of physics was involved even in this apparently useless analysis! In scientific training it will not have been useless.

As another example, suppose a steel rod be given him to examine. Cutting off a piece about 10 cms. in length, he might determine its dimensions, mass, density, and specific heat. With the long portion he can ascertain its rigidity (by torsion), Young's modulus (by flexure), velocity of sound in it (by longitudinal vibrations), and compare this with the velocity determined from the ratio of elasticity to density. He might magnetize a short piece, say 20 cms., by permanent magnets, and by timing its oscillations, and observing the deflection it gives a needle, determine its magnetic moment, strength of pole, and strength of field in which it swung. Let him then demagnetize it by heating, remagnetize by electric current, and compare its moment now with what it was before. These latter, though not properties of the steel itself, are obtained as consequences of its magnetic character. He might also employ such a magnetized bar to determine its moment of inertia experimentally, and check by calculation from its mass and dimensions. Thus he will have brought into application numerous principles of mechanics, of acoustics, of heat and of magnetism, each of which gives opportunity for work of any required degree of care and precision, involving all the fundamental operations of weighing, measuring, and timing. By the former method he will learn to what extent any one quality is found in numerous specimens examined; by the latter, to what extent these numerous and varied qualities are found in the one specimen examined; by the former he learns one feature of many things, by the latter, many features of one thing. But in learning the one feature he confines his attention chiefly to a few principles of science and needs extend his knowledge of physics no further than to apply these few principles, no matter to how many objects, and there is always the danger of breaking off his work with only a partial view of the science; whereas in learning many features, though confined to only one object, each feature involves one or more distinct principles of science, and the many of them represent a wide range of scientific knowledge. This gives to the student, therefore, indeed it imposes upon him, a broad culture none the less deep because of its breadth, even if he has had time for the analysis of but one specimen, while the other almost inevitably results in confining his labors and his attainments within narrow limits. Whenever it can be done, a determination of any sort should be made by two processes as nearly independent of each other as possible. For example, the radius of curvature of a lens might be determined by comparing the size of an object, as a scale, with that of its image formed by reflection from the lens surface; and it might be calculated from spherometer measurements. While there are some points in physics which the progressive method would reach and the method of analysis miss, the latter would the more readily lend itself to such twofold determinations.

There are operations such as the calibration of a thermometer, determining a rate of vibration, adjusting special forms of apparatus and determining their constants, etc., that cannot be classified in any simple manner. An attempt to adhere strictly to any clearly defined method throughout the whole course of physics would be unwise and unprofitable. The recognition of a method and of its legitimate limits, however, cannot fail to be of service to a judicious instructor. The limitations of the laboratories themselves in many cases compel a departure from any method and cause the work to degenerate into an unsystematic performing of experiments. It must be admitted, too, that such is the character of the work in some instances where the equipment is very complete.

A NEW METHOD OF CHILD STUDY.

BY J. MARK BALDWIN, TORONTO, CANADA.

The current discussions of the more elementary mental processes show that we lack clearness in our conceptions of the earlier stages of mental life. This is evident enough to call out frequent appeals for "scientific" child study. The word "scientific" is all right, as far as it goes; but as soon as we come to ask what constitutes scientific child study, and why it is that we have so little of it, we find no clear answer, and we go on as before accepting the same anecdotes of fond mothers and repeating the inane observations of Egger and Max Müller.

Of course there are only two ways of studying a child, as of studying any other object—observation and experiment. But who can observe, and who can experiment? Who can look through a telescope and "observe" a new satalite? Only a skillful astronomer. Who can hear a patient's hesitating speech and "observe" aphasia? Only a neurologist. Observation means the acutest exercise of the discriminating faculty of the scientific specialist. And yet most of the observations which we have in this field were made by girls who, before their marriage, knew less about the human body than they did about the moon or a wild flower (having got this latter information from Steele's "Thirteen Weeks") or by a father who sees his child when the boy is dressed up, for an hour a day, and who has never slept in the same room with him in his life; by people who never heard the distinction between reflex and voluntary action, or that between nervous adaptation and conscious selection. Only the psychologist can "observe" the child, and he must be so saturated with his information and his theories that the conduct of the child becomes instinct with meaning for mind and body.

And as for "experiment," greater still is the need. Many a thing a child is said to do—a little judicious experimenting—a little arrangement of the essential requirements of the act in question—shows it is altogether incapable of doing. But to do this we must have our theories, and have our critical moulds arranged beforehand. That most vicious and Philistine attempt in some quarters to put science in the straight-jacket of barren observation, to shut out the life-blood of all science—speculative advance into the secrets of things—this ultra positivistic cry has come here as everywhere else, and put a ban upon theory. On the contrary give us theories, theories, always theories! Let every man who has a theory pronounce his theory! This is just the difference between the mother and the psychologist—she has no theories, he has. She may bring up a family of a dozen and not be able to make a single thrustworthy observation: he may be able from one sound of one yearling to confirm theories of the neurologist and educator, which are momentous for the future training and welfare of the child.

In the matter of experimenting with children, therefore, our theories must guide our work—guide it into channels which are safe for the growth of the child, stimulating to his powers, definite and enlightening in the outcome. All this has been largely lacking, I think, so far, both in scientific psychology and in applied pedagogy. The implication of physiological and mental is so close in infancy, the mere animal can do so much to ape reason, and the rational is so helpless under the leading of instince, impulse, and external necessity, that the task is excessively difficult—to say nothing of the extreme delicacy and tenderness of the budding tendrils of the mind. Experiment? Every time we send a child out of the home to the school, we subject him to experiment of the most serious and alarming kind. He goes into the hands of a teacher who is not only not wise unto the child's salvation, but who is on the contrary a machine for administering a single experiment, to an infinite variety of children. It is perfectly certain that two in every three children are irretrievably damaged in their mental and moral development in the school; but I am not at all sure that they would fare any better if they stayed at home! The children are experimented with so much and so unwisely, anyhow, that it is possible that a little experiment, intentionally guided by real insight and psychological information, would do them good.

With this preamble, I wish to call attention to a possible method of experimenting with young children, which has not been before noted to my knowledge. In endeavoring to bring questions like the degree of memory, recognition, association, etc., present in an infant to a practical test, considerable embarrassment has always been experienced in construing safely the child's responses. Of course the only way a child's mind can be studied is through its expression—facial, lingual, vocal, muscular; and the first question, i.e., What did the infant do? must be followed by a second, i.e., What did his doing that mean? And the second question is, as I have said, the harder question, and the one which requires more knowledge and insight. It is evident, on the surface, that the farther away we get in the child's life from simple inherited or reflex responses, the more complicated do the responsive processes become, and the greater becomes the difficulty of analysing them, and arriving at a true picture of the real mental condition which lies back of them.

To illustrate this confusion, I may cite about the one problem which psychologists have attempted to solve by experiments on children, i.e., the determination of the order of rise of the child's perception of the different spectral colors. Preyer starts the series of experiments by showing a child various colors and requiring the child to name them, the results being expressed in percentages of true answers to the whole number. Now this experiment involves no less than four different questions, and the results give absolutely no clue to their analysis. It involves, 1, the child's distinguishing different colors simultaneously displayed before it (i.e., the complete development of the child's color sensation apparatus); 2, the child's ability to recognize or identify a color after having seen it once; 3, an association between the child's color-seeing and word-hearing memories, by which the name is brought up; 4, equally ready facility in the pronunciation of the various color names which the child recognizes: and there is the further embarrassment, that any such process which involves association, is as varied as the lives of children. The single fact that speech is acquired long after objects and some colors are distinguished, shows that Preyer's results are worthless as far as the problem of color perception is concerned.

That the fourth element pointed out above is a real source of confusion is shown by the fact that children recognize many words which they cannot pronounce readily. Binet, who represents the second phase in the development of this experimental problem, realized this, and varied the conditions by naming a color and then requiring the child to pick out the correspondingo color. This gave results different not only from Preyer's, but also from those which Binet reached by Preyer's method. For example, Preyer's child identified yellow better than any other color, a result which no one has confirmed.

The further objection that colors might be distinguished before the word association is established at all, is also seen by Binet, and his attempt to eliminate that source of error constitutes what we may call the third stage in the statement of the problem. He adopts the *méthode de réconnaissance* as preferable to the *méthode d'appellation*. This consisted, in his experiments, in showing to a child a colored counter, and then asking the child to pick out the same color from a number of different colored counters.

This reduces the question to the second of the four I have named above. It is the usual method of testing for color-blindness. It answers very well for color-blindness; for what we really want to learn in the case of a sailor or a signal-man is whether he can recognize a determined color when it is repeated; that is, does he know green or red to be the same as his former experience of green or red. But it is evident that there is still a more fundamental question in the matter — the real question of color perception. It is quite possible a child might not recognize an isolated color quality when he could really very well distinguish color qualities side by side. It is the question just now coming to the front, the question of absolute *vs.* relative recognition, or immediate *vs.* mediate recognition. The last question is this: When does the child get the different color sensations (not recognitions) and in what order?

A further point of criticism of Binet's results serves to illustrate my argument. Binet rules out the influence of the word memories which were necessary to Preyer's results by his *méthode de réconnaissance*. The child recognizes again the color just seen. Now any one who has followed the course of recent discussions of recognition must know that the mediation of word associations is not ruled out in these cases in children of 3 to 5 years old or even younger. Lehmann finds colored wools are recognized when the colors are those whose names are known (*Benennung's association*), and that shades which have not peculiar names, or whose names are not known, are not recognized. Scripture has shown that an unobserved or unintelligible element — a *Nebenvorstellung* — may serve as the link of recognition without rising again to clear consciousness a second time. It is, of course, useless, if these results be trustworthy, to attempt to get recognitions clear of word memories after color names have once been learned by the child. It would seem that the question ought to be taken up with younger children. Binet's experiments were in the interval between the child's 32d and 40th weeks. It is perhaps a confirmation of Lehmann's position, that the colors least recognized in Binet's list are shades whose names are less familiar to children: his list, in order of certainty of recognition, is red, blue, green, rose, maroon, violet, and yellow by the *méthode d'appellation;* and, by both methods together, red, blue, orange, maroon, rose, violet, green, white, and yellow.[1]

This color question may suffice to make clear the essentials of a true experimental method. Only when we catch the motor response in its simplicity is it a true index of the sensory stimulus in its simplicity. I have accordingly attempted to reach a method of child study which would yield a series of experiments whose results would be in terms of the most fundamental motor reactions of the infant, which could be easily and pleasantly conducted, and which would be of wide application. The child's hand-movements are, I think, the most nearly ideal in this respect. The hand reflects the first stimulations, the most stimulations, and, becoming the most mobile and executive organ of volition, attains the most varied and interesting offices of utility. We have spontaneous arm and hand movements, reflex movements, reaching-out movements, grasping movements, imitating movements, manipulating movements, and voluntary efforts — all these, in order, reflecting the development of the mind. The organs of speech are only later brought into use, and their use for speech involves an already high development of mind, hence the error in Preyer's results. It has accordingly seemed to me worth while to find whether a child's reaching movements would reflect with any degree of regularity the modifications of its sensibility, and, if so, how far this could be made a method of experimenting with young children.

Before speaking, however, of applications, I may adduce one or two other considerations which tend to show that some such dynamogenic method is theoretically valid. Féré showed that sensory stimulations of all kinds increase the maximum hand-pressure. Colors (seen) have regular and each its peculiar effect upon movement. Tones have similar influence. The ticking of a watch is more clearly perceived if a sound is heard at the same time. Further, the reaction-time of hand-movements is shorter if the stimulus (sound, etc.) be more intense. There is an enlargement of the hand, through increased blood-pressure, when a loud sound is heard. These, and a variety of other facts upon which the law of dynamogenesis rests, seem to afford justification for the view that the infant's hand-movements (say) in reaching and grasping will be an index of the kind and intensity of its sensory experiences. Magendie[2] long ago suggested measuring changes in sensibility by the corresponding changes in blood-pressure.

Further, it is not necessary to embarrass ourselves with the question whether the hand-movements are voluntary or not. However we may differ as to the circumstances of the rise of volition, it is still true that after its rise the child's reactions are for a long time quite under the lead of its sensory life. It lives so fully in the immediate present and so closely in touch with its environment, that the influences which lead to movement can be detected with great regularity. In this case the sensations, which are movement-stimuli, become what we may call "effort-stimuli," and the child's hand-efforts become our indications of the relative degree of discrimination, attractiveness, etc., of the different sensations.

Suppose we hang a piece of meat up over Carlo's head and tell him to jump for it. His first jump falls short of the meat. He jumps again and clears a greater distance. Why does he jump farther the second time? Not because he argues that a harder jump is necessary to secure the meat, but because by the first jump he got more smell, blood-color, and appetite-stimulus from the meat. Now suppose it be a red rag instead of meat, and Carlo refuse to jump a second time. This is not because he concludes the rag would choke him, but because he gets a kind of sensation which takes away what appetite-stimulus he already had. The thing is a thing of sensational dynamogeny or "suggestion," and the child-state up to his 24th month (more or less) is just about the same.

The following questions, I think, might be taken up by this method: —

1. The presence of different color-sensations as shown by the number and persistence of the child's effort to grasp the color.
2. The relative attractiveness of different colors measured in the same way.
3. The relative attractiveness of different color combinations.
4. The relative exactness of distance-estimation as shown by the child's efforts to reach over distances for objects.
5. The relative attractiveness of different visual outlines (stars, circles, etc.) cut in the same attractive color, etc.
6. The relative use of right, left, and both hands.
7. The rise of imitative movements.
8. The rise of voluntary movements.

I am quite aware of the meagreness of this list; but one has only to remember the fact that there is no such thing yet as a psycho-physics of the active life, that this side of psychology is *terra incognita* to the experimentalist.[3] If the method proves reliable in one-half of these questions, then so much gain. I have applied it to questions 1, 2, 4, 6, and 7 with results, some of which I have already published in this journal. Other papers will be devoted to these detailed applications.

[1] Calculated from Binet's detailed results (Revue Philosophique, 1890, II., 582 ff.) by Mr. F. Tracy.

[2] Féré, "Sensation et Movement," p. 56.

[3] I see no reason that a method could not be devised for testing the motive influences of presentations in terms of the time elapsed since their experience. I have announced elsewhere (*Proceedings of Congress for Exper. Psychology*, London, 1892), the first results of a research conducted upon adults by such a method.

JAPANESE NURSERY NOTES.

BY ALBERT S. ASHMEAD, M.D., NEW YORK CITY.

IT has often been said that the Japanese are the most interesting, the strangest, even the quaintest, people we know. In no regard is this truer than in the care they take of their babies. Such a strong foundation is more necessary than elsewhere, to a nation where man is born to remain a baby his whole life. We, destined to exercise stronger and more serious minds, would be, at the very beginning of our existence, deteriorated by such ingenuous, untiring care.

I have spoken in another article[1] of the long-continued lactation of Japanese women, as benefiting both mother and child; also of the care taken of pregnant women, in which a solicitude displays itself, at the same time, clever and loving. This tender and intelligent attention paid to the born baby is the second part of that unique Japanese system of rearing healthy and happy men, which makes European and American ladies forget that so many other Japanese conceits are a severe shock to their feelings. During the dentition period the children have an extra diet, consisting of fish and small crustaceæ. Japanese not being a carnivorous people, this is natural enough. If they ate meat, they would give their children beef very likely. But it is certainly to the advantage of the bony structure of the child to be, on first entering the adult course of eating, fed in the Japanese manner.

The abominable diaper is unknown to the Japanese. They use only a breech-clout, which is removed at the moment of defecation. The child then is put in such a position that its legs straddle the arms, his body and head resting against the abdomen of the parent, who, gently rocking it in a certain rhythmical, tentative fashion, and accompanying his action with a kind of low whistling, reminding you of a lullaby, gives his offspring its first lesson in personal cleanliness, which, to the Japanese mind, is exceedingly next to godliness. It will be seen how, by this method, unnatural positions are avoided, a thing the more important that Japan is the country of worms, distomata, etc.

It is known probably to every reading person that Japan, like all oriental lands, is, for obvious reasons, furnitureless. It does not even know the cradle. As Diogenes made a cup of his hollow hand, thus the Japanese mother makes a cradle with the back of an older child, an ambulating, delightful cradle, where it stays from morning to night, and is unrhythmically rocked according to the chances and sports which the day offers to its patient and loving victim. Her back, of course, is its first cradle; when it wants the breast, it reaches over or under her arm for it.

The cause of the absence of furniture is the presence of tropical vermin. This awful presence is probably also the cause of the carpetless state of the nursery. The floor is covered with stuffed straw mats, thick and elastic; it is the usual floor of a Japanese house. The floor is mopped every day with salt water; it is, in fact, a chlorine wash. It must be remembered that in Japan the dirt of the street is not carried into the room, sandals and shoes being left at the front door. The necessity of keeping the floor in a sanitary condition is more important in Japan than anywhere else, because of the national habit not only of sitting, but sleeping on the floor.

There is a singular difference between the carriage of Japanese children and the way in which our children walk and move about. The Japanese urchin, whose feet never knew the unkind pressure of tight shoes, and, in fact, no pressure at all, walks more erect, is more sure-footed. In fair weather he wears flat straw-sandals; in these sandals the big toe is widely separated from the others, which gives the child a surer foundation. In wet weather he must maintain his equilibrium on his stilt-like wooden clogs, which keep his feet dry, at the same time compelling him to acquire an extraordinary power over his own motions.

There is in Japan no kissing, not even in the nursery. All the dangers, which have been so elequently described in newspapers some time ago, arising from the touch of lips, in human love directly, and at the communion table indirectly, are avoided by the national aversion for labial contact.

The sexes are separated at an early age, and the separation is maintained until marriage. After marriage the husband has a right to annex to his household as many concubines as his means allow. If his wife is delicate, she will perhaps suggest some friend of hers, who will prove rather an ally than a rival. At any rate, there will be no diminution of the friendship between the two women. When pregnancy occurs, a second concubine may be suggested, and no such addition ever troubles the quiet waters of a Japanese household. It is incredible of what amount of peace and, consequently, happiness, the absence of the green-monster alone may be the cause.

When she loses a child, the Japanese mother does not wring her hands and look up to heaven; she sits with folded hands, sunken head, her eyes looking into her lap. Japanese grief has been very eloquently described by my colleague in Japan, Professor Wernich, and I think it will be a good winding-up of this little article if I quote a passage of his remarkable book, "Geographico-Medical Studies:" "However often I have witnessed the death of dear relations, children, for instance, or husbands, I never had occasion to observe the wringing of hands, to which European women of the lower classes are so much addicted. A bitter sorrow was expressed through deep sinking of the head, grasping the hands together, shedding of tears. That strong mental agony, which digs into the soul, so to speak, and takes hold of it, like a bodily pain, seems to be unknown to them. They never 'turn to heaven their faces bathed in tears,' an action which to us seems not only natural and in perfect accord with the essence of grief, but is considered as beautiful and as a worthy subject of artistic representation. In prayers the Japanese mother does not lift her eyes to heaven; with bent head, the body somewhat shrunk together, with hands put together by the palms and slightly raised to the level of the chin, she sends her humble prayer, for quite concrete things, you may be sure, to Buddha."

[1] "On the Non-Existence of Rachitis in Japan," Medical Record, Oct. 11, 1890.

NOTES AND NEWS.

PROFESSOR MARTIN, on account of his serious and prolonged ill health, has tendered his resignation of the professorship of biology which he has held in the Johns Hopkins University since 1876.

—The third volume of "Hermetic Philosophy," by Styx, has just been issued from the press of J. B. Lippincott Company of Philadelphia, completing the work. It is quite different in form from the other two volumes, being a dialogue in imitation of Plato on the question, "Can virtue and science be taught?" The author says in his preface: "Having already written two volumes on the essential teachings of the Hermetic Philosophy, and finding that they are not profitable attractions, as we hoped they would be, we have concluded to vary the performance." Whether the new volume will prove more attractive than the earlier ones or not may perhaps be doubted; but it is more readable, and contains much less of the peculiar stuff known as occultism than they did, though it contains enough for most readers. A large part of the book is taken up with ridicule of the "Christian scientists," which is suggestive of the dispute between the pot and the kettle; but a good deal of space is also devoted to setting forth the theosophical doctrine of reminiscence and reincarnation, or, in other words, the transmigration of souls. The question about the nature of virtue and whether virtue can be taught is discussed in various aspects; but the light that is shed upon it is rather what Milton calls "darkness visible." The production of several works of this kind in these times seems to us a singular phenomenon, hardly to be accounted for on ordinary principles; but it is apparently due to the general break-up of the old creeds, which has left a vacuum that men seek to fill with some doctrine or other, true, false, or nonsensical, as the case may be. For our part, we cannot conceive how any one can write such books, and, as we read them, we cannot help thinking that the authors do not really believe the doctrines they set forth.

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Attention is called to the "Wants" column. It is invaluable to those who use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The "Exchange" column is likewise open.

EVIDENCE OF TWO PRE-MORAINIC GLACIAL MOVEMENTS.[1]

BY G. D. SWEZEY, DOANE COLLEGE, CRETE, NEBRASKA.

The valley of Rock River, running southward through southern Wisconsin and northern Illinois, is a very deep pre glacial erosion gorge, cut through Lower Silurian rocks and filled with assorted, incoherent glacial sands and gravels. Artesian wells go down several hundred feet through these gravels before striking rock. The region lies to the south of the great terminal moraine.

Along the bluffs of this valley, at various points, there is exposed a conglomerate, composed also of glacial gravels; it is a pudding stone, thoroughly indurated, so as to make an excellent rock for cellar-walls and the like; in some cases it is cemented together by a calcareous matrix, in other cases there is a large percentage of iron hydroxide in the cementing material.

I have long known some of these outcrops, and have been puzzled by them; but have supposed that they were due to local causes affecting the gravels which everywhere fill the broad valley; but some more careful observations this summer reveal these facts:—

1. They rise, in every case that I have observed, ten to twenty feet above the river bottoms.

2. They are overlaid in some cases by the boulder-clay, containing unassorted, striated pebbles.

3. They occur, so far as I can discover, and I am pretty familiar with the region, which was my native county, only on the extreme edge of the bluffs overlooking the broad river bottoms, or on the bluffs of valleys of some width which were tributaries to the main valley when its latest bottoms were formed. They appear, in other words, to be remnants of older gravels which once filled the valley, but were mostly cut away by the floods which deposited the later, unconsolidated gravels now filling the valley and constituting its flood-plain at a level of ten to twenty feet below the top of the conglomerate. In one place the extreme face of the bluff, immediately below an outcrop of the conglomerate, was made up of layers of the light-colored, incoherent gravels, alternating with dark, iron-stained material, evidently derived from the older conglomerate, which then formed the bluffs against which the stream washed.

This distinction of age is confirmed by the occurrence of the ground moraine of the latest glacial movement in this region, overlying the consolidated gravels.

There is no decisive evidence that the interval between these movements was one of great duration, but the striking contrast in appearance between the loose gravels and the conglomerate tends to impress one with the idea that the latter is relatively very old. So striking is this appearance, that at one exposure which I visited I found the owner of the field laboriously digging up an outcropping mass of this conglomerate, somewhat harder and redder than usual, under the supposition that it was a meteorite, which he purposes to take in to Chicago next year for an exhibit.

A further consideration, which would seem to imply a considerable interval between the two movements, is that the following succession of events would seem called for to explain the facts: —

1. A glacial movement bringing the material of which the conglomerate is composed, and which includes about the usual proportion of local and remote ingredients.

2. A melting of the ice and floods, surpassing in extent those of the later epoch, for the conglomerate, as before stated, lies regularly at a higher level than the later gravels.

3. Another forward movement of the ice to account for the ground moraine overlying the conglomerate; and

4. Another melting of the ice to deposit the later gravels.

[1] Paper read before the Nebraska Academy of Sciences, Dec. 27, 1892.

SOME NOTES ON LIGHTHOUSE APPARATUS.

BY J. KENWARD, C.E., F.S.A., BIRMINGHAM, ENGLAND.

In 1851, the United States possessed four sea-lights on the dioptric system. In 1891 the number was of sea-lights 138, of harbor lights 526, in addition to about 100 of the small apparatus called range-lenses and lens-lanterns.

This magnificent progress in forty years reflects the highest credit on the Government and on its nautical and engineering officers. Under official auspices in 1851, a most exhaustive enquiry was promoted into the merits of the dioptric or refracting system of lights of Augustin Fresnel in comparison with the catoptric or reflecting system which it had begun to supersede. The result having been ascertained to prove a sevenfold superiority for the dioptric system, the government authorized the lighting of the United States coast-line on an imposing scale, and it has ever since taken a watchful and intelligent interest in the advancement of lighthouse science, and in the gradual provision of the best forms of optical and mechanical apparatus.

The steps of progress, indeed, in lighthouse design and construction have been many and important. The first home of this industry was in France, where the illustrious mathematician and physicist to whose practical genius the lenticular system is due, lived his short life, dying in 1827. The celebrated Tour de Cordouan, at the mouth of the Garonne, was the first lighthouse to receive the new installation of his lenses. The names of Leonar Fresnel, brother of Augustin, of Soleil, Letourneau, Lepaute, Sautter, Barbier, Degrand, Allard, Reynaud, Bourdelles, Bernard, and others follow in brilliant succession in France, as engineers, constructors, or contributors to the literature of the subject; while in the United Kingdom the great family of the Stevensons, Mr. James Chance, Dr. John Hopkinson, Sir James Douglass and Mr. Wigham of Dublin, may be cited as equally distinguished.

Nor have the authorities of the United States, while availing themselves fully of the labors and researches of all these experts, been backward in adding American names to the list of honor. To mention only three, General Alexander, Major George Elliot, and Major D. P. Heap are worthy, in their special work, of the country of such men of science as Professor Henry and Professor Newcomb.

It is particularly to Major Heap of New York that credit is due not only in selecting the most novel and striking forms of apparatus produced in Europe, but also in promoting the design and construction in the States, of the lanterns, lamps, clockwork, pedestals, etc., which are indispensable to it. Major Heap is the author, too, of an excellent compilation on lighthouses.

Let me glance at some of the past achievements and present resources of lighthouse science.

During the past ten or fifteen years the great extension of commerce, the opening of new ports, the multiplication of steam vessels of all classes, and the striking acceleration of their speed, have affected lighthouses and lightships in the three essential points of number, power, and distinctiveness. The chief maratime countries of the world — the United States, Great Britain and her colonies and dependencies, France, Holland, Italy, and Denmark, have endowed their coasts with an imposing array of lights, and their needs are not nearly satisfied. It was eloquently said by the Secretary of State at the International Marine Conference at Washington in 1889: "The spoken languages of the world will continue to be many, but necessity commands that the unspoken language of the sea shall be one." Thus the signal-lighting of the sea-coasts and of the ships traversing the sea is a work truly and emphatically international — a work which neither in its magnitude nor its variety suggests any notion of finality. For instance, the English Admiralty published in 1862 forty notices to mariners in relation to lights, buoys, dangers, etc., in all the work. In 1892 its similar notices amounted to over 600.

The considerations of power or intensity and of distinctiveness follow naturally on the numerical increase of lighthouses. The early illuminating apparatus of Fresnel and his successors were mainly confined to two forms, fixed and revolving, the latter being approximately from six to eight times as powerful as the former, the four-wick vegetable oil flame being the brightest illuminant in both. Subsequently composite lights of both fixed and revolving sections were adopted, as well as more effective flames. Next followed the important enhancement of revolving apparatus by the holophotal system of Thomas Stevenson, who also introduced the use of condensing prisms and of mirrors for fixed lights, and who lastly, after many minor improvements, suggested the maximum size of lens yet attained, called the hyper-radial — a light particularly suited to great headlands and other stations where ranges of visibility of thirty or forty miles are necessary. The increase *pari passu* of the potency of lamps — thanks chiefly to the unwearied and intelligent labors of Sir James Douglass with petroleum, and of Mr. Wigham of Dublin with gas — has given due effect to these great developments of dimension. The first-order revolving light shortly to be established on Heceta Head, Oregon, the work of Messrs. Chance of England, is an example of the holophotal system with twenty-six prisms of a radius of 920 millimetres. Mosquito Inlet, Florida, erected in 1887, is an example of a hyper-radial fixed light of 1330 millimetres radius, with prisms, on the holophotal system. This was constructed by Messrs. Barbier & Co. of Paris. Dondra Head and Barberyn, Ceylon, are examples of hyper-radial revolving lights without prisms, and with lenses of 80° vertical angle. These lights are the work of Messrs. Chance.

Power or intensity of beam has also been attained by superposing one lens apparatus on another, increasing *pro tanto* the total effectiveness of the light. Mr. Brown of Lewisham, England, was the first (in 1859) to propose this arrangement of lenses, and Mr. Wigham of Dublin the first (1872) to carry it out in some fine Irish lights constructed by French makers. Messrs. Chance have since constructed striking examples of the biform type for Bishop Rock and Round Island, Scilly Isles, for the Bull Rock, Ireland, and for the Eddystone, English Channel.

A further method of intensifying lighthouse beams is the electric illuminant. Here the United States have not been backward in following the example of Great Britain and France, though the American use has been more conspicuous in buoys and beacons than in sea-lights like the St. Catherines in England, the Isle of May in Scotland, or the Cap Grisnez or Ushant in France.

It is, however, beginning to be understood that the electric light in its present condition is not, save in a few cases, to be too strongly recommended for lighthouse service. Its cost, when applied to large apparatus, both for instalment and for maintenance is very considerable, and in thick weather its superiority of penetration to the rays of gas or oil lamps of the present imposing dimensions is a much controverted point. It is, indeed, a mixed question, for the lighthouse engineer and the financial secretary, to be determined according to the nautical and economical conditions of each station. Yet it must never be forgotten that the true way of estimating the combined effectiveness and expense of a light is to divide the units of first cost and annual maintenance by the units of power or intensity.

A more important consideration seems to be that of distinctiveness. The early French plan of making a portion of the appartus of fixed optical sections and a portion of revolving optical sections has the obvious disability of inequality of range where the light is white throughout or red throughout, so that a vessel observing the flash at a certain distance can only see the fixed beam at a much less distance, and can only know the true character of the light after an interval more or less prolonged. When color is used for the revolving portion this difference is no doubt much diminished, but it is still too great. A light wholly of revolving sections is preferable for all purposes in these days of high speed and multiplied traffic. And it is the revolving light that affords a much greater number of characteristics than the fixed. The group-flashing system in double and triple series by optical combinations, first introduced by Dr. John Hopkinson and Messrs. Chance in 1875, has been adopted all over the world; and the gas group-flashing system of Mr. Wigham, of about the same date, has been extensively used in Ireland. Combinations of red and white flashing lights, where the former is rendered as powerful as the latter by special optical contrivances, have been repeatedly employed, especially by Messrs. Chance.

A very valuable form of distinction largely adopted in modern times, is the occulting light, that is, a fixed beam interrupted by a short darkness instead of a long darkness interrupted by a short flash as in a revolving light. The sharp contrast of dark and light is thus substituted for the old fixed light, and this is particularly valuable for ports and harbors where shore and ship lights are now so much more numerous and powerful than before. A very simple form of clockwork, designed by the writer, gives movement to the occulting screens for small lights and to the vertically dropping screen which is preferable for sea lights where the occulting system is accounted powerful enough for a sea-light. In occulting lights care should be taken that the duration of darkness should be sufficient to affect the eye sensibly, as, for instance, two seconds at least. There is a growing tendency, also, to make the flashes of revolving lights too short in duration or too quick in recurrence. The difficulty of identifying a light and of taking a bearing by it is thus much increased, and the wear and tear of the mechanism for rotating the apparatus becomes very serious despite the recent expedient of a mercury trough in which the framework revolves.

The most approved optical and mechanical arrangements for our lighthouses would be of little avail if the aliment oil or gas, which sustains the greater number of them, were of unsuitable quality. As regards gas there need be no other provision than that made for the town or harbor supply near the lighthouse, and the only need is to use it with adequate pressure in an appropriate burner in single or multiple jets or rings. As regards oil, the different vegetable varieties, the chief of which was colza, have now nearly fallen into disuse, giving place to petroleum in some of its many forms. The luminiferous properties of good oil and good gas are almost equal, but the cost of petroleum is not more than one-fourth that of vegetable oil, and not greater than that of gas, while its extreme pliability and convenience make it quite as valuable as gas. The lighthouse world is indebted to an American, Captain Doty, for first showing, twenty years ago, how mineral oil could be used in a multiple-wick burner, and it is indebted also to America for the largest and best supplies of the oil itself. The only drawback has been the undoubted greater risk of fire and explosion, but even this has been obviated by the introduction of the variety called "heavy mineral oil," which, having a flashing point of 240° to 270°, is almost absolutely safe. It is now generally used in Europe.

The improvement of burners fit for mineral illuminants has proceeded, as I have said, well nigh to perfection in the hands of the Trinity House of London and of their late engineer, Sir James Douglass. His six-wick burner is of the power of about 900 candles consuming about $\frac{9}{16}$ of a gallon per hour; his ten-wick burner is of about 2,200 candles, consuming about $1\frac{7}{10}$ of a gallon per hour. I believe that no form of the Doty or any other oil-burner equals this. The pressure-lamps of Messrs. Chance used with such burners seem to secure the maximum of advantage in the focus of any dioptric sea-light. Very much excellent work, however, in the way of improvement of burners, has been achieved by Major Heap and by Mr. Funck, his assistant at Tompkinsville.

It is a singular fact that, despite all the improvements of the dioptric system and the vital urgency of the matter, the side and mast lights of vessels still remain to a large extent in so imperfect a condition. In Paris and Birmingham, the only seats of the manufacture of dioptric lights, ship lights with true lenses have long been constructed on the same principles of the sea-lights which have a radius thirteen times as great. The writer has long urged, both publicly and privately, the employment of more powerful lights at sea, and more particularly the equalization of the power of these lights by using electricity in incandescent lamps of unequal intensity, in the colored side lights, so that meeting or passing vessels shall understand the course and character of each other at much greater distances than are now sanctioned by statutory rules. At the International Marine Conference in Washington, in 1889, the subject of ship lights was amply discussed with reference to azimuthal ranges and vertical divergences, and the conclusions formulated are being now internationally adopted. But the question of *greater intensity of beam* and of *equality of beam* does not appear to have been considered in relation to the greatly changed conditions of vessels thronging the high and narrow seas in these days, and to the ever-increasing frequency of accidents by collision at night. I earnestly hope that the authorities of the United States will yet again take the initiative in effecting this final improvement in ship lights.

In closing for the present these few remarks on lighthouses it is impossible not to give expression to feelings of admiration for the liberal and enlightened policy of the United States in maintaining the lighthouses of their immense coast-line free of toll to all the maritime world. America sets a shining example to many an older country in this as in many other ways May her maritime prosperity abundantly increase!

A JAPANESE SICK WITH SCARLET-FEVER.[1]

BY ALBERT S. ASHMEAD, M.D., NEW YORK CITY.

I HAVE been introduced to a Japanese gentleman, aged 23, living in Brooklyn, who is undergoing treatment by Dr. Benjamin Ayres for scarlet-fever. As this is the first case of scarlet-fever I have ever seen in a Japanese, I report it to you. To-day is the twenty-eighth day of the disease. There has been no temperature during the last two weeks. Desquamation has been general for three weeks, mostly behind the knees and about the shoulders. He has now scaly desquamation on the palms and soles; noticed first by the patient on the backs of the hands. The throat showed very marked symptoms and is even now very distinctly red and inflamed. Highest temperature 103½; no albumenaria.

I content myself with this short sketch, as, I think, Dr. Ayres will make a more complete report.

I am the more interested in this case, as it is supposed that the Japanese have an immunity from scarlet-fever. I have tried, without success, several times to inoculate a Japanese subject with the disease, in the hope of producing a protective virus. More recently I inoculated two children who had been exposed to the contagion of scarlet-fever with the blood-serum from a blister on the body of a child who, having had scarlet-fever previously, was artificially immune.

These children, whether protected or not, did not take the disease. More recently still, I have inoculated two cases of scarlet-fever with pure blood-serum from a blister on the body of an adult, who was also artificially immune. The inoculations were made in the arms on the third, fourth, and fifth days. In these latter cases there was no effect if diminished desquamation is not to be considered as one. Both cases ran a mild course. It is my opinion, on which, having so little to go upon, I would not insist too strongly, that blood-serum from an artificially immune subject has a virtue, if not curative, at least preventive. Dr. Seward of the Willard Parker Hospital promised me to make a further investigation in the scarlet-fever ward of his hospital.

I have given you these facts to show you what reasons I have to be particularly interested in the case on which I have summarily reported.

[1] Communicated to the Tei-I.-Kwai.

ELECTRICAL NOTES.

IF a student of molecular physics had been asked a few months ago for an explanation of the phenomenon seen when an electrical discharge is passed through a Geissler tube, he would not have hesitated in his reply. He would have shown, from the researches of J. J. Thomson and others, that the phenomenon, in the case of the non-striated discharge, is akin to that of electrolysis, that disassociation was a necessary accompaniment; that, in the case of the striated discharge, the electricity was carried partly by convection and partly by electrolysis, that this was shown by the fact that the conduction did not proceed with the velocity of light, that each stria was a place where electrolysis was taking place, and each dark band a place where the electricity was carried by convection, that the reason why the discharge was not produced with mercury vapor is that it cannot be disassociated, and that the reason that it takes place so readily with other gases is that the converse is the case.

But the recent work of Herr Hertz and Dr. Lenard has caused considerable doubt to be thrown on some parts of this theory. Not that the theory as given above may not be true after all, but it must first explain the phenomena discovered by the above-named scientists, and at present this seems difficult.

A short account of them is as follows: If we take a Crookes tube, i.e., a tube in which exhaustion has been carried to such an extent that the discharge is no longer visible, except where it strikes upon the glass, or some other solid or phosphorescent substance, we find that, as the exhaustion progresses, the rays issuing from the cathode, and producing incandescence or phosphorescence, instead of passing directly from the cathode to the anode, tend to move in a straight line, normal to the cathode. This discharge has been supposed, one might almost say proved, by Crookes, in a series of most masterly experiments, to consist of highly charged atoms of gas, repelled with great violence from the cathode. As the exhaustion becomes more and more thorough, fewer and fewer atoms are left in the tube, and consequently the trajectories of the atoms become more and more nearly straight lines, and, if the tube is bent at an angle between the electrodes, the discharge will strike against the glass.

If this is the real nature of the discharge, it would seem on first sight that it should not be able to pass through a metallic substance. Yet it has been discovered by Herr Hertz that this is not the case, that it passes readily through thin metal plates. From these two facts, that the discharge takes place in straight lines, and that it passes through thin metal plates, Dr. Lenard conceived the idea that it should be possible to produce the discharge in a Crookes tube and make it pass out into the air, and the experiment, when tried, proved successful.

The apparatus used was as follows: A Crookes tube, whose two ends we will call A and B, had the cathode electrode sealed in at A. This was of the usual form, and projected some distance into the tube. The anode consisted of a tube of aluminium, only a little smaller than the size of the glass tube containing it, and surrounding the cathode. On the discharge taking place it would, instead of passing directly from the cathode to the anode, as in the case where the gas was not so much rarefied, proceed normally from the cathode and out of the open end of the aluminium tube constituting the anode and strike against the glass at the other end of the Crookes tube. In these experiments, that end was cut off, and a metal plate cemented across the opening. In the middle of this metal plate a small hole, 1.7 millimetres, was drilled, and this was covered by a sheet of aluminium, .0003 millimetres thick. Consequently, when the discharge struck against the aluminium plate, the latter being permeable to it, it passed out into the air. This was shown by a luminous discharge just outside the sheet of aluminium, and by the fact that phosphorescent substances placed there behaved in the same manner as when exposed to the cathode discharge in a Crookes tube. If, in place of air, other gases were made to surround the aluminium plate, very different effects were obtained. If the gas was hydrogen, the discharge, after passing through the aluminium window, was not scattered so much. If carbonic acid gas, the scattering was much greater. Dr. Lenard points out that, as all gases at the same temperature have the same number of molecules to the cubic centimetre, this shows that it is not the number but the kind of molecules which determines the scattering. But perhaps the most important experiments were those in which the discharge was allowed to pass into another tube which had been exhausted so far as possible. It was argued that if the cathode discharge was due to the projection of atoms from the cathode that it could not take place in an absolute vacuum. The tube into which the discharge was to pass was exhausted as far as possible, i.e., until a twenty-centimetre spark would not pass from one electrode of the absolute vacuum tube to the other. Notwithstanding this extreme exhaustion, the discharge passed freely through, as was shown by the phosphorescence of substances placed at the other end. The conclusion which Dr. Lenard draws from this experiment is that the cathode rays are really processes in the ether, and not due to the movement of atoms.

On account of the difficulty of obtaining an absolute vacuum, Dr. Lenard's results cannot be accepted as final. Even at the exhaustion obtained by him it may be calculated that there are quite a sufficient number of atoms left to produce the phenomenon (using the results of J. J. Thomson and Chattock in the calculation), even neglecting the number contained in the layer of air on the sides of the tube, and which would be driven off into the tube so soon as the discharge began to pass. Moreover, it is quite possible to conceive that a discharge of atoms from the cathode, on reaching a thin metal sheet, and being abruptly stopped by it, might propagate an electric disturbance proceeding from the other side of the sheet of metal, and so drive off another set of charged atoms. If there were any way of obtaining an absolute vacuum, of course the question could be answered definitely, but this is impossible, and we must wait for further results before attempting an explanation. R. A. F.

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.

Low Temperatures.

IN your issue of Jan. 27, page 50, it is stated that the Franklin Search Expedition, under Lieutenant Frederick Schwatka, in 1879-80, experienced a temperature of $-71°$ C.

This is an error, as I have heard Lieutenant Schwatka in many conversations refer to it as "seventy-one degrees below zero, Fahrenheit."

I enclose a copy of a letter now in a collection belonging to my brother:—

TACOMA, WASH., Sept. 15, 1892.

On the third of January, 1880, my Arctic exploring party encountered a degree of cold of seventy-one below zero, Fahrenheit, or one hundred and three degrees below the freezing-point of that scale, the coldest we noted on the trip, and the coldest ever encountered by white men travelling in the field, for that day we moved our camp some twelve miles. It will be a cold day when that record gets left. FREDERICK SCHWATKA.

FRED. G. PLUMMER.

Tacoma, Wash., Feb. 11., 1893.

Where is the Litre?

IT must be a source of regret to all interested in metrology that so much time was expended in the preparation, and so much space in the publication of the leading article in *Science* for March 17, entitled "Where is the Litre?" etc. Even if the instruction contained in the article be reinforced by the amusement which it furnishes, the result is quite incommensurate with the labor which must have been involved in its production.

Ignorance of the recognized principles of metrology has led to certain conclusions which will generally be harmless on account of the very magnitude of their errors. The sermonizing finish to the article, beginning with the sentence, "In spite of the much lauded simplicity of metric measures," etc., may, however, mislead a few readers whose ideas have been befogged by the perusal of the previous three pages. It will be well to remind them, therefore, that the apparent bewildering confusion as to the value of the litre has no relation whatever to the "simplicity of the metric system." Indeed, the confusion might have been rendered vastly greater, the alleged case against the metric system much stronger, and the entire article more picturesque, if the author had introduced the "gus" of Arabia, the "pik" of Egypt, and the "sun" of Japan, the value of each of which in metres must always be a matter of considerable uncertainty.

The following simple statements may be of value. It is generally agreed among metrologists that *natural* standards of length and mass are not at present easily attainable. Our knowledge of physical or astronomical constants must continually increase in precision as methods and instruments are improved. Such constants are, therefore, unsuitable for standards, because standards should, first of all, be invariable as far as possible. Artificial standards can be made of more convenient dimensions, can be multiplied with almost any required degree of precision, and their invariability is perhaps as well assured as that of any suggestive national standard.

It was originally proposed to derive the metre from the dimensions of the earth. We know that the metre is not the one ten-millionth of the quadrant of the meridian passing through Paris, but that fact does not in the slightest degree lessen the value of the metre as a unit of length. Its value is so nearly that, that it is exceedingly convenient to use in ordinary calculations relating to the earth, not requiring a high degree of precision.

It was also proposed originally to establish some sort of a simple relation between the unit of length and the unit of mass. As length and mass have no natural relation to each other, any numerical ratio must depend on a physical constant, namely, the density of some selected kind of matter. The determination of this must be a matter of experiment, and its value can never be absolutely known. For this reason any relation between the unit of length and the unit of mass must always be an approximation. The unit of mass must, therefore, be an artificial, independent unit.

The new international prototype of the metre is, in length, an exact reproduction of the old metre of the archives, as far as can be determined by the most recent and most perfect means of comparison. The new international prototype kilogramme is identical, in mass, with the old kilogramme of the archives, as far as can be determined by the most precise and delicate weighings ever made.

It was originally intended that the mass of the kilogramme of the archives should be that of a cubic decimetre of pure water at its maximum density. As this involves the knowledge of a physical constant, it was not possible to realize this relation exactly, and it never will be possible.

In determining volumes which do not exceed a certain limit, it has been found that greater accuracy can ordinarily be secured by the indirect method of determining the mass of a liquid of known density, than by direct geometrical processes. The application of the latter requires simple forms whose linear dimensions may be easily and accurately measured. The former depends only on the accuracy attainable in mass measurement and density determination.

This method of volume measurement has usually been regarded, however, as a matter of convenience only. Thus, the U. S. gallon is defined as a volume of 231 cubic inches; in standardizing measures of capacity in gallons, it has always been customary to use the indirect mass-density method. The mass of water which has been assumed to represent this volume has varied from time to time as our knowledge of the physical constants involved advanced.

The litre was originally assumed to be identical in volume with the cubic decimetre, and there could be no possible objection to confining the term *litre* strictly to this meaning. But, as noted above, it being vastly more convenient to use the mass-density method in determining volumes, much of the uncertainty of precise volumetric work would be avoided by defining the litre as the volume of a kilogramme of water at maximum density.

Recognizing the wisdom of this course, the International Committee of Weights and Measures, in October, 1880, resolved that in its publications and in its official use the term *litre* should be used to express the volume of a kilogramme of pure water at maximum density. The one-thousandth part of this, that is to say, the volume of a gramme of pure water at maximum density is called the *millimetre*, and the abbreviation *ml.* is used to stand for it.

The *litre* and the *millilitre*, therefore, are not precisely identical with the cubic decimetre and the cubic centimetre. The difference, however, is very small, and may safely be neglected in all ordinary operations. Where a high degree of accuracy is required, it will usually be found that the results are primarily obtained by the mass-density method, and that no correction is required.

The International Bureau is engaged in an elaborate investigation of the relations of mass, volume, and density in pure water, and, when the results are available, they will doubtless satisfy the most exacting demands. T. C. Mendenhall.

Washington, D.C., April 14.

On the Teaching of Biology.

If the article "On the Emergence of a Sham Biology in America," by Mr. Conway MacMillan, printed in *Science* for April 7th, 1893, had appeared in a special journal, it would not be worth while to notice it, but since *Science* reaches many people who are not specialists in any branch of biology, it may not be a waste of time to point out some of its special merits.

The author of the article looks over the courses offered in biology in some of the leading universities of the country, and, finding that botany does not receive adequate treatment, he apparently becomes fired with the serious purpose of exposing what he illogically calls a "sham" science.

The Johns Hopkins University, which has done as much as, if not more than, any other single institution in the country, for the advancement of biological science in America, during the last seventeen years, is stigmatized in a way which will highly amuse those who are acquainted with its work. This institution is accused of dishonesty in naming its zoölogical courses. "Injustice," "wrecker-light use of the word 'biology,'" "protective mimicry in a university curriculum," "perpetrating a confidence game upon a board of trustees," are some of the choice phrases which are indulged in. These flattering remarks are not limited to the institution; they extend even to its graduates. "The cool effrontery of this would have surprised me had I not known the marvellous, sometimes continuous, sometimes sporadic, always insular capabilities of the Johns Hopkins biologist for blatant philistinism in regard to things botanical."

Of course it is not necessary to take such criticism as this seriously. The tone of the article is so thoroughly bad, and the looseness of statement so completely inconsistent with anything bordering on scientific accuracy, that sober criticism is well nigh impossible.

The chief merit of the paper lies in pointing out the great value which a good course in general biology, such as that given for many years at the Johns Hopkins University, may possess for an average student, who will follow it with a fair degree of fidelity. Such a student would have learned what Lamarck, Treviranus, and Bichat comprehended, and what Huxley and the school of biologists who have been inspired by his teaching have striven with signal success to inculcate,—that the study of biology is not, as this erratic writer supposes, two disciplines, but *one discipline*, the study of living phenomena, in which the distinction between plant and animal, in the widest sense, is one of secondary importance.

A student who had followed this general biological course with a fair degree of success would have learned that "biological science is *not* to be set over against physical science in the broadest sense," but that in this broadest sense biology is a physical science, coördinate with chemistry and physics. In biology there is no natural cleavage into two branches, botany and zoölogy, any more than there is a natural constriction of chemistry into the studies of minerals and the compounds of carbon, because the plane of division in either case would be a purely imaginary one. An appreciation of this truth does not conflict with the obvious fact that biologists in general find it convenient to specialize either in the direction of the study of plants or the study of animals. Biology is often primarily divided, for convenience, into study of living structure and study of function, or into morphology and physiology, because the study of living structure is one subordinate discipline, and the study of function is another. For further convenience we may further classify these sub-sciences, according to their subject-matter, into vegetable morphology and animal morphology on the one hand, and into vegetable and animal physiology on the other.

Let an institution that sets about to teach biology do all it can to put before its students the principal facts of morphology and physiology of both plants and animals, but to pronounce its work, if well done, a "sham," through its inability to cover the whole field, is, to say the least, a very flagrant misuse of language. The title of Mr. MacMillan's article is misleading, and the whole tone of it is characterized by this glaring misuse of words. He does not distinguish between a "sham" science and a science too much "restricted" or "narrowed." Even if we grant the most that is said in regard to the teaching of biology at some of the institutions named, all that would be proved would be that the science of biology had been too much restricted at these places, not that there was any element of "sham" in it. The work which the Johns Hopkins University has done for the study of biology in this country proves conclusively that there has been no element of "sham" in its methods.

I find in the Johns Hopkins University Circulars for March, 1893, No. 104, eleven courses offered to students in the biological department, including seminaries and clubs. One course is announced in "Cryptogamic Botany"; the rest have reference almost exclusively to animal physiology and morphology. An elementary course in botany has been given at this university for years, and lecture courses in vegetable morphology and physiology of a more technical nature have been offered from time to time, showing that the study of plants is far from being ignored. The biological work of this university, as is well known, has been chiefly devoted to the study of animal physiology and morphology, and the work that it has undertaken it has done eminently well. Nothing could be more unjust than any inference that this university has encouraged its students to undervalue the study of plants. On the contrary, it has regretted that it has had no fully equipped botanical laboratory to offer its students, and it has uniformly advised them to go to institutions better equipped in this department for the special study of plants.

It is not possible for every institution to take the same color with reference to the special lines of scientific investigation, but this is a different thing from saying that it is not desirable for every institution to have a well-balanced curriculum. In most of the smaller colleges the man at the head of his department is the only teacher in it, and if he is a botanist his work will soon take on a botanical tinge; if morphologist or physiologist, his special work is sure to come to the front. This explains a good deal of the "sham" element that Mr. MacMillan has discovered in American biological teaching.

The stimulus which comes from the association of specialists in a large educational centre is undoubtedly very helpful, but as soon as students commence to leave the elementary stages of their work, and to enter upon special lines of investigation, their sympathies immediately diverge with increasing rapidity. It is therefore desirable that this loss of sympathy on the part of one specialist for the work of another, should be postponed as long as possible. One means of accomplishing this in a large university, in the case of biology for instance, is undoubtedly to present the whole subject in the fullest manner, especially in the elementary courses.

There is no doubt that every biologist, whatever the special line of work to which he devotes himself, should have the same training up to the point of specialization, in at least chemistry, physics, morphology, and physiology. The attitude of mind which Mr. MacMillan displays comes from a lack of this early comprehensive training, or at least from the lack of profiting by it. If while himself a student at the John Hopkins University, he had determined to get all there was in the admirable elementary courses which are there offered in general biography, zoölogy, animal physiology and embryology, instead of interesting himself from the first mainly in plants, he would not only have been enabled to take a broader view of his specialty, but would not have committed himself to the position in which this article places him.

Mr. MacMillan incidentally remarks that he has "not at present time to discuss the fundamental absurdity of courses in 'general biology,' as if it were possible to plunge boldly into comparative study of plants and animals before one has studied plants and animals themselves. It is as if one should enter upon analytical statics and follow it up by geometry and the calculus." Here again Mr. MacMillan demonstrates the urgent need of a good course in general biology for botanists as well as for zoölogists. Here the analogy drawn is false. Zoölogy and botany do not bear a similar relation to biology that geometry and the calculus bear to the higher mathematics. The instruments for solving problems in botany and zoölogy are essentially the same, such as good observation, sound reasoning, a knowledge of technical methods, and of the other physical sciences.

It is not necessary for the student to examine a large number of organisms in order to come face to face with the fundamental properties of living things, and this fact proves that Huxley and his successors are right in insisting that the study of biology is one discipline. To teach the student this, and to lead him to discover some of the wider agreements and differences of living organisms, is of more intellectual value to him than to conduct him at the start to the more special study of either plants or animals. This is true whether he is to become a specialist in biology or not.

Some of the chief merits of Mr. MacMillan's paper have now been pointed out. A subordinate merit which it possesses is that of calling attention to the defect in many institutions of not including botany in their curriculum, or in not giving it the prominence which it deserves. If he had limited himself to pointing out this defect, without casting slurs upon honored institutions and their graduates, in an offensive way, his article might have done good. FRANCIS H. HERRICK.

Adelbert College, Cleveland, Ohio, April 15th, 1893.

A New Source of the So-Called Mexican Onyx.

LOVERS of the beautiful, in the way of high-grade material for decorative work, will be pleased to learn of the recent discovery, on the peninsula of Lower California, of extensive deposits of the so-called Mexican onyx. The new find is some 150 miles southeast of San Diego, and 50 miles from the Pacific coast. The material, as is the case with that of Mexico proper and other sources, is a travertine (i.e., a spring deposit) and not stalagmitic. The deposits are essentially superficial, the material in many instances so occurring as to be taken directly from the surface of the ground by means of bars and without previous stripping. The colors are light green, rose, and white, variously veined and tinted, and of great beauty, while in compactness of texture, susceptibility to polish and freedom from flaws, the material leaves little to be desired. A company has already been organized for working the deposits, and the first shipment has reached St. Louis, to be cut and polished for exhibition at Chicago during the World's Columbian Exhibition. GEORGE P. MERRILL.

Washington, D.C.

BOOK-REVIEWS.

The Metaspermæ of the Minnesota Valley. A list of the higher seed-producing plants indigenous to the drainage basin of the Minnesota River. By CONWAY MACMILLAN. Minneapolis, 1892. 839 pp. 2 Maps. 8°.

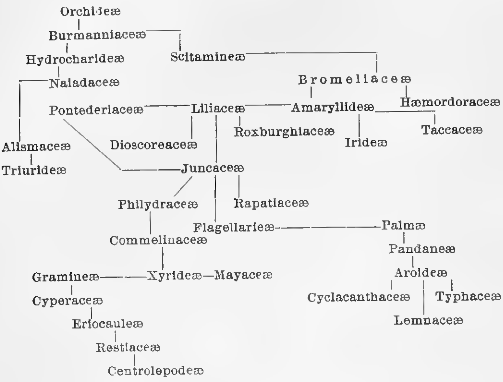
BOTANISTS will examine this volume with interest, because of the numerous new features it presents. It is the first of the botanical reports of the Geological and Natural History Survey of Minnesota, and, while entirely local in its character, it is very far beyond the usual local catalogue. It contains a record of 1,174 species and varieties, distributed among 407 genera and 106 families. Under each family reference is made to the place of its original characterization, the number of genera and species, living or extinct, it contains, and its distribution in a very general way. Under each genus we have the synonomy as fully as may be, again with a reference to the number of species and their more detailed distribution. Finally, under each species and variety the synonomy is given, still more detailed distribution, and mention of herbaria where specimens are to be found. It will thus be seen that, while it is a catalogue of plants, it is one in a wider sense than the majority of such publications. Its interest and value to botanists lie not alone in the various facts above referred to, but because it discards the time-honored arrangement of orders, such as is found in the ordinary manuals and text-books, and introduces the newer and more natural system of classification. It contains, besides, a discussion of the factors upon which classification is based, principles of geographical distribution, and extraordinary statistical detail respecting the plants named in the list.

We turn first to the classification and nomenclature. We well recollect when we first began to study botany, the feeling of satisfaction that was felt at the seeming stability of the science. We had been familiar with the discussions of zoölogists and geologists regarding the condition of nomenclature in their respective branches, and the botanical manuals gave no sign of changes that were to come, or indicated the presence of dangerous ground: But rumblings of the coming eruption were soon heard, although it was not until the publication of that amazing book of Kuntze's, "Revisio genera plantarum," which has turned everything upside down and set the whole botanical world by the ears, that the full violence of the eruption was realized. Against many of the suggestions of this reformer there has been open revolt, but upon the whole the effect has been good. It is true it has compelled those who learned their botany some years ago to learn much of it over again, and has made our latest text-books obsolete or old-fashioned, but it has also put the science upon a more stable foundation.

The discussion of generic and specific names has introduced the perennially fertile subject, a natural classification of orders. The plan of placing Ranunculaceæ at the head of Anthophyta and Gramineæ at the foot is so familiar that scarcely any other seems possible. It has been recognized, however, that the system was very faulty, and numerous endeavors have been made to change it. As long ago as 1883 the present writer, in an article entitled "On the Position of the Compositæ and Orchideæ in the Natural System,"[1] pointed out that the old arrangement was far from being the best; and he made some suggestions as to what families should take the highest rank. He suggested that among dicotyledons Compositæ should be regarded as the highest, inasmuch as here is found the largest production of seed (the end of all plant life) with the least expenditure of material, and, at the same time, with ample provision for cross fertilization. The immense number of species and their great range were also cited to prove their high position. The impossibility of arranging the orders in a strictly natural and yet lineal system was recognized, but it was suggested that the Labiatæ were somewhat parallel with the Compositæ in their differentiation; while with that order were associated, as near allies, Verbenaceæ, Boragineæ, and Scrophularinæ. Among polypetalous orders Leguminosæ was placed highest, followed closely by Rosaceæ, Saxifragaceæ, Umbelliferæ, and Ranunculaceæ. Among monocotyledons the Orchideæ were accorded the highest rank, mainly because of their large numbers, wide distribution, varied form, and elaborate means for cross fertilization. At the same time, a general scheme was proposed, which is reproduced here. In it, it will be observed that there are four general lines of descent, viz., from Orchideæ, Liliaceæ, Palmæ, and Gramineæ. The relative rank of the smaller orders is not that which has been followed in the volume under review, but the

[1] American Naturalist, December, 1883. Also read in the Minneapolis meeting of the A. A. A. S.

Chart of the Monocotyledons.



placing of Orchideæ at the head of one alliance, and Gramineæ of another, agrees with the general scheme. Again, among the dicotyledons Compositæ is regarded as the highest and Ranunculaceæ is placed well down in the scale. The whole arrangement is that of Engler and Prantl,[2] but it corresponds so well with the provisional outline suggested by the writer of this that he has called attention to it.

Professor MacMillan's plan in the citation of authorities is in all cases to preserve the original specific name, *except* when this is the same as the genus, when the next oldest name is substituted. This is in accordance with the rules adopted by the botanists of the American Association for the Advancement of Science. All may not agree to this exception, but to the present writer it seems the better plan. Another feature, more or less of an innovation in botany, is to use lower-case letters instead of capitals in all specific names, no matter what the source from which they have been derived. This is the plan adopted in some of the other sciences, notably in paleontology, and it is being rapidly adopted by botanists.

Inasmuch as the old divisions of the Dicotyledons — Polypetalæ, Gamopetalæ, and Apetalæ — have been discarded, a new series of terms is needed, and those adopted in the volume are as follows: —

Division A. — Protophyta.

Division B. — Metaphyta.

Plants where sexuality has not been (A) or where it has been developed (B). Under B we have: —

(I.) Gamophyta, which develop sexual plants from their eggs without any spore-producing structure intervening, such as pond scums, black mold, and algæ like *Œdogonium;* and

(II.) Sporophyta, in which the fertilized egg is divided into a cellular structure capable of growth, and consists of a spore from which sexual plants are produced.

Then comes the division of (II.) into (1) Thallophyta, (2) Archegoniata, and (3) Metaspermæ. In (1) are included the great mass of sea-weeds, algæ, and fungi. In (2) we have Chara, Nitella, coniferous trees, the extinct Lepidodendron, etc., and in (3) we have those forms producing seeds in a closed ovary. Finally the Metaspermæ are divided into two groups: (*a*) Archichalydeæ, without a perianth or having one made up of separate leaves, about equal to the old groups Apetalæ and Polypetalæ, and (*b*) Metachlamydeæ, in which the perianth leaves are united, and

[2] Natur. Pflanzenfam , 1887-1893.

CALENDAR OF SOCIETIES.

Anthropological Society, Washington.

April 18.— J. Owen Dorsey, Siouan Phonetic Types; James Mooney, The Indian Messiah and the Ghost Dance (illustrated by Lantern Slides); Henry Gannett, Estimates of Wealth.

Society of Natural History, Boston.

April 19.— J. B. Woodworth, Traces of a Fauna in the Cambridge Slates; Charles P. Bowditch, Ruins of Central America.

which equals the old group Gamopetalæ. The classification may be tabulated as follows:—

- A.—Protophyta.
- B.—Metaphyta.
  - I. Gamophyta.
  - II. Sporophyta.
    - (1) Thallophyta.
    - (2) Archegoniatæ.
    - (3) Metaspermæ.
      - *a.* Chalazagameæ (a single genus *Casuarina*).
      - *b.* Porogameæ.
        - * Monocotyledons.
        - ** Dicotyledons.
          - † Archichlamydeæ.
          - †† Metachlamydeæ.

While following the classification of Engler and Prantl to a large extent, Professor MacMillan takes issue with them upon some points. One of these is the nature of the Mycetozoa, or, as they are more commonly called, Myxomycetes. He believes them to be animals rather than plants. It is difficult for the present writer to see why the motility of the plasmodium of these plants, really the only animal-like feature they possess, should be regarded as very different in character from the motility of the swarm-spores of such universally recognized plants as *Hydrodictyon*, *Œdogonium*, *Volvox*, and others. If motility in conjunction with lack of chlorophyll be evidence of the animal nature of the Myxomycetes, why should not the stationary *Hydra*, multiplying (as it can) when cut into small pieces, and possessing chlorophyll, be considered a plant? Or where are we to place *Dionæa*, *Drosera*, and the like, that present movements analogous to, if not precisely the same as, the contractile powers of many animals? The hard and fast lines between the animal and the vegetable kingdoms have long since been broken down, and there is as much justice in placing Myxomycetes with vegetables as in placing Protista with animals. The fact of motility, the absence of chlorophyll, or both combined, can scarcely be sufficient to overbalance the preponderance of facts showing the distinct vegetable nature of the slime-molds.

There is a long and interesting discussion of the relationships of the metasperic flora of the region, in which the general features of geographical distribution and the factors concerned with it are considered. The history of the region is regarded as of prime importance, and past time has been a most potent factor in the work. As the Metaspermæ have existed on the earth since as far back as Jurassic times, this history begins then. Glacial time, however, was the period most immediately concerned with the present distribution. Many plants previous to that epoch lived in Minnesota which were driven away never to return, or which were entirely exterminated; while, on the other hand, many species were found after the close of the period entirely unknown before. The Sequoias, once widely spread over the continent, but now occupying so restricted a range, he considers to be an indication of the great competition existing between plants in Tertiary times, their great height and giant bulk showing the magnitude of the struggle. So, on the other hand, the Compositæ are regarded as representing a type that permitted wonderful variation, and hence great adaptability to changed conditions.

The 150 pages devoted to statistics of the flora can only be mentioned here. Even a detailed statement of the heads treated of would give but a faint idea of the elaborateness of the discussion. A very full index (66 pages) gives easy reference to all species, genera, and orders mentioned. Altogether this is the most elaborate catalogue of plants of a limited district it has ever been our lot to examine. Its use of modern classification and nomenclature will make it of very great interest and value to all systematic botanists, and, while all may not agree with the author in his many suggestions or innovations, there are none but will recognize the enormous amount of labor put into the volume. It is greatly to be desired that other limited floras be as completely dissected and discussed. JOSEPH F. JAMES.

Washington, D.C., March 27.

QUERY.

Can any reader of *Science* cite a case of lightning stroke in which the dissipation of a small conductor (one-sixteenth of an inch in diameter, say,) has failed to protect between two horizontal planes passing through its upper and lower ends respectively? Plenty of cases have been found which show that when the conductor is dissipated the building is not injured to the extent explained (for many of these see volumes of Philosophical Transactions at the time when lightning was attracting the attention of the Royal Society), but not an exception is yet known, although this query has been published far and wide among electricians.

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SCIENCE

NEW YORK, APRIL 28, 1893.

THE TOPOGRAPHIC MAPS OF THE UNITED STATES GEOLOGICAL SURVEY.

BY W. M. DAVIS, HARVARD COLLEGE, CAMBRIDGE, MASS.

OUR national Geological Survey was charged by Congress with the preparation of a geological map of the United States. As no suitable topographic map existed to serve as the base of geological coloring, the Director of the Survey proceeded to develop a topographic corps for the purpose of producing a map that should serve his needs. This action has from time to time called forth discussion; as, for example, recently in *The American Geologist*. Information on the subject from the standpoint of the several scientific governmental bureaus may be found in the "Testimony before the Joint Commission of Congress," in 1885. It is not my intention to discuss this subject here, as I have been more interested in the examination of the great geographical product of the Survey, than with the association of geographical work with one bureau of the government or another. The recent appearance of the first folio of the "Geologic Atlas of the United States," marking the beginning of a most important national publication, without parallel in magnitude in other countries, makes some account of the topographic maps of the Survey appropriate at the present time.

The first season of topographic field-work under the re organized survey was in 1882. Previous surveys had done much work in the West; but their styles of publication were discordant. Belts of litoral country had been mapped by the Coast Survey; similar belts along the Great Lakes had been mapped by the Lake Survey; and extensive maps were in progress under the Mississippi and Missouri River commissions. The areas covered by these various surveys and by the topographic division of the Geological Survey are indicated in the annual reports of the Director. No duplication of earlier work of acceptable character has been attempted; but the results gained by all the surveys are reduced to a uniform style of publication, on a plan that will in time embrace the entire country.

Details of the plan adopted for the topographic survey are presented in the annual report of the Director for 1884–85. Our country includes an area of 3,000,000 square miles; hence the need of economy. The elaborate methods applicable in smaller European nations could not be introduced. An astronomical and computing division took charge of the determination of fundamental points, where needed. A triangulation division extended the determination of points from those furnished by the astronomical parties or from previous work by the Coast or other Surveys. The Appalachian triangulation, for example, is illustrated in the sixth annual report of the Survey. While this triangulation suffices for the needs of the proposed maps, it is not of such accuracy as would be required in determining the figure of the earth; being in this respect different from the fundamental triangulations of our Coast Survey and of most foreign countries. The saving of time and money, demanded in the survey of so great an area, required this modification of former methods. Numerous topographical parties were placed in the field to fill in the spaces between the smaller triangles. The plane table was employed in many districts; traverse surveying was adopted elsewhere. Railroad levels have been extensively utilized. Much sketching between established points has been required; and to any one who carries the published maps into the field, it will be apparent that the details of topographic form are generalized, and are occasionally incorrectly represented. on account of the rapidity with which the work was carried on. It was difficult at first, as it may be still, to secure the services of a sufficient number of experienced topographers; but their practice in the field has added to their expertness in sketching streams, ridges, and contours. Much effort has been given to their improvement; practical field instruction has been given to the newer men during the summer months, and voluntary meetings for discussion of methods have been held during the winter in Washington.

Those who wish to examine the style of work followed in the preparation of various sample sheets, should consult the eighth annual report, in which Mr. Gannett has included several graphic illustrations of the number and distribution of points determined by intersections, or of lines run by traverse. Yet it is manifest that much is left to sketching, and, judging from the amount of country covered by a single party in a season's work, the sketching must often be rapid.

In common with many others, I wish that the accuracy of the surveys might have been greater; but I presume that all questions of method, scale, style, cost, and equitable distribution of work were duly considered by Major Powell, Director, Mr. Gannett, Geographer, and other members of the Survey, and decided as seemed best in view of all controlling circumstances. Not least among these controls is the disposition of Congress to support the work. Judging by the appropriations recently voted, it would take the greater part of a century to survey the country by the more elaborate European methods. Our grandchildren and not ourselves would enjoy the products of the work, and few of us are so unselfish as to be satisfied with so long a postponement.

As to publication, three scales were at first adopted; 1:62,500; 1:125,000; and 1:250,000; corresponding to about one, two and four miles to the inch. The map-sheets were divided according to lines of latitude and longitude, a sheet on the smallest scale covering a "square degree," while four and sixteen sheets of the larger scales are required for the same area. Each sheet is named after its chief town and State; and on the later sheets, the name of the four sheets adjoining it are printed on its sides: the four corner-wise sheets might be to advantage indicated in the same way. According to the scale of the map and the complication of the topography, contours are drawn with vertical intervals varying from 10 to 200 feet. Each sheet is printed in three colors, as described in the sixth annual report: black for the artificial features and names, all lettering following a consistent plan; blue for the hydrography, brown for the contours. All the sheets thus far issued are printed very clearly, and as a rule accurately; the earlier ones by Bien of New York, the latter ones by the Survey itself.

According to information lately received from Mr. Gannett, the number of sheets completed up to June 30, 1892, is as follows: 295 on scale of 1:62,500; 338 on 1:125,000; and 61 on 1:250,000; making a total of 694. The total area surveyed, but not all engraved, to that date was 547,000 square miles, distributed over forty-two States and Territories. In future, only the inch-to-a-mile scale will be employed, and the surveys of the current year are conducted with regard to this new feature of the plan.

Before speaking of certain sheets of the contoured maps, mention should be made of several smaller scale maps of the country as a whole. The most important of these is the nine-sheet map, on a scale of 1:2,500,000, with contours for 100, 500, 1000, 1500, 2000, and higher thousands of feet.

A limited edition of this map has been prepared for office use without the black print of artificial features and names, thus giving desirable prominence to the natural features. This is an extremely valuable map, as it contains all available information concerning the relief of our country, expressed in a form most generally useful. A smaller sheet, including the whole country, represents increasing altitude by a series of nine brown tints; a very effective presentation destined to serve as the original of many a map in our school geographies.

A visit to the office of the Survey in Washington discloses the great fund of information there gathered on the topographic map-sheets. Unhappily the sheets are not at present published for general distribution; they are chiefly for the use of the members of the Survey in their geological studies. The edition of each sheet as printed is stored in a room containing a great number of shelves, each one marked in front with the name of its map. Hardly a sheet can be drawn from its shelf without revealing some interesting features to the geographer, the geologist, or the engineer.

It is only of the first of these interests that I shall speak. Although when examined on the ground that they represent, the maps are not so accurate in detail as we might wish, still the graphic view of the country that they present is so vastly more minute than any to which we have hitherto had access that it is something of a revelation to look over them. A great fund of geographical information is stored up in that map-room. One may look forward to an auspicious geographical day when the maps are generally distributed, and when that universal study, home geography, is enriched by the illustrations that they will afford. Some of the smaller eastern States have already reached this happy position, by means of two-party arrangements between themselves and the national Survey. The first of these was New Jersey. After the Coast Survey had furnished the triangulation, and after about half of the mapping had been borne by the State, the Geological Survey sustained the remainder of the cost of field-work; but in this State, the form of publication is somewhat different from that adopted for the rest of the country. The sheets are larger, and overlap to a considerable extent, so that there is no place that is not well within the boundary of one sheet or another. The publication of the seventeen inch-to-a-mile sheets and of the shaded relief map of the State on a scale of five miles to an inch, has been duly noted in *Science* at the time of their appearance. The separate sheets may be obtained from the State Geological Survey for 25 cents a piece, or $5 for the complete atlas of twenty sheets. Massachusetts was the next State to take advantage of the two-party arrangement. The triangulation here was taken chiefly from the old Borden survey. Now the whole State is covered in 54 inch-to-a-mile sheets; and a four-sheet map, on a scale of four miles to an inch with contours every hundred feet, has been published. Like the New Jersey atlas, the 54 sheets of Massachusetts may be bought of the Commissioners (address, Commonwealth Building, Boston) for six dollars. Composites of the sheets about Boston and Worcester have been published by the Appalachian Mountain Club for use in field excursions and otherwise. Rhode Island is next to be mentioned. Here the area is so small that the several sheets covering the State, with parts of Massachusetts and Connecticut next adjoining, have been mounted on rollers, and through the active interest of the State Commissioners, the local Legislature has been induced to make a special appropriation of $3,500 for the distribution of the mounted map to all public schools and libraries within the State,—a wise and liberal step towards better public education. The mounted map is also sold by J. C. Thompson, Providence, at $2 a copy. It is manifest that large States may follow this plan, by mounting grouped sheets in roller maps about four by five feet; so that every school should be provided with a large map of its own part of the State. If one may judge by the small appreciation that teachers generally have of the physical features of their own regions, such a distribution of maps is greatly needed.

In other parts of the country, there are no States completely mapped as yet. A large area of country has been covered along the central and southern Appalachians, and the first folio of the Geologic Atlas was a map from this district, with others soon to follow. Missouri and Kansas have the good fortune to be represented by a large number of contiguous sheets; and in the western States and Territories the maps of the older surveys have been redrawn and printed on the new uniform plan. The various lines of interest suggested by these maps would lead me to write many pages, were they all followed; and I shall therefore limit myself here to the one which takes my first attention, the physical features of our home geography. This may be illustrated by a brief reference to five maps from Missouri,—the Tuscumbia, Versailles, Warsaw, Clinton, and Butler sheets.

These sheets run from east to west, partly across the central part of the State, somewhat south of the Missouri River, and include the greater part of the basin of the Osage River. The eastern course of the Osage, towards its mouth in the Missouri, is seen to be extremely tortuous in a steep-sided valley, trenched two or three hundred feet below the level of the surrounding upland. The meanders of the river are peculiar in not being, like those of the Mississippi, spread upon a flat flood-plain. High spurs of the upland occupy the neck of land between every turn of the stream. Evidently, the meanders are not of the ordinary type. It has been suggested that they result from the jointed structure of the rocks that the river traverses; but this is hardly possible, for it would not explain the manifest relation that exists between the size of the river and the radius of its swinging curves: the larger the stream, the larger the radius. I have therefore supposed that we have in this curiously curved valley an illustration of a process long recognized in the case of various rivers of northern France. Briefly stated, this is as follows: Once upon a time, a river, long active, had worn down its basin to a surface of faint relief. Its valley sides had wasted away so as to oppose little interference to its lateral swinging. Its slope had become very gentle, and its current had taken to a deviating path, peculiar to old streams, which so generally meander on their flat flood-plains. Then the region that it traversed was evenly raised to a greater altitude, and the faithful stream once more turned to the task of cutting down its channel close to base-level and carrying away all the waste that was fed into it. But in doing so, it retained in the new cycle of its life the meandering course that it had attained in its old age in the previous cycle. Although its activities were rejuvenated, its habit of swinging from side to side was still preserved. It behaves as if it were on a flood-plain, although the flood-plain, on which it learned this behavior, has been consumed. The Seine and Meuse have extremely meandering channels in deep and rather steep-sided valleys; and I have learned from my most obliging and well-informed correspondent, Mr. E. de Margerie, that the above explanation is current regarding them. The Mosel also has a deep meandering gorge between the Eifel and the Hunsrück, in western Germany. In this country, I have supposed that the meanders of the north branch of the Susquehanna, in the plateau of northeastern Pennsylvania, might be thus explained; and the incredible turns of the unpronounceable Connedogwinit, opposite Harrisburg, seem to be of the same kind, except that the Susquehanna learned its swinging habit on a Cretaceous lowland flood-plain, while the Connedogwinit was taught in late Tertiary times. On looking over the Missouri maps, I concluded that the Osage was another example of the same inherited habit; and in this case there is a neat little bit of confirmation on the western of the map-sheets named above that deserves mention.

If it is true that the curved course of the Osage is inherited from a flood-plain whose level lay across the top of the present valley when the land lay lower, we must suppose that, after gaining elevation to the present altitude, and thus gaining permission to cut down towards a new base-level, advantage would be taken of this permission first in the lower part of the river, and that the deepening of the channel would gradually work backwards up stream. Good fortune brought us upon the river while it is, as we may say, just in the act of thus adjusting its valley to the new altitude of its drainage area. We see the lower course already deepened, while the upper course still preserves its part of the flood-plain from which the curves of the lower river were inherited. The upper branches of the Osage flow upon broad flood-plains meandering freely, skirted by back-swamps, and frequently cutting off their curves and leaving ox-bow lakes to one side of their newer course. These upper branches preserve what the lower part of the river has already lost: they tell us what it has been, while it foretells what they shall come to be.

Of course, while only the maps are before me, and the Osage is a long thousand miles away, I do not wish to assert that this sketch of its history is demonstrably true; although I am strongly persuaded that an examination of the region on the ground would discover evidence confirmatory of it. The upland is built of nearly horizontal Paleozoic rocks. If they had stood at their present height above the sea ever since the date of their deposition, they would now be worn down close to sea-level, without retaining any distinct relief. Their narrow valleys show that this supposition is out of the question. The rolling upland in which the narrow valleys are incised is itself a surface of denudation; and as its reliefs are faint, with long gentle slopes and broad open valleys, beneath whose floor the narrow deeper valleys are incised, I am driven to the belief that the upland was for a long time a lowland, and that its gentle eminences are merely the remnants of a once higher mass. The dates at which this older denudation was carried on, and the later date at which the uplift to its new altitude was given, are not well determined; although from analogy with more eastern parts of the country, where the dates of such changes have been better made out, I am inclined to say that the Missouri upland was a lowland well into Tertiary time; and that the new trenches of the Osage and its neighbors were begun in consequence of an uplift somewhere about the close of Tertiary time.

These are suggestions rather than conclusions; but they still serve to illustrate the incentive to geographical study that the topographic maps supply. We all knew that there was a fertile field for study in our home geography; every one in his own district enjoyed cultivating his patch of the field; but now through the publication of these maps, it is as if the whole field was opening to all of us; and a rich geographical product is promised to all who enter it.

SUN-HEAT AND ORBITAL ECCENTRICITY.

BY ELLEN HAYES, WELLESLEY, MASS.

THE reader of Sir Robert Ball's important work, "The Cause of an Ice Age," needs no reminder that its argument rests upon a foundation of theoretical astronomy. To secure the essentials of the discussion one must read between the lines. It is the object of the present paper to select and arrange a few of the more simple inter-linear readings, in the hope that they may be serviceable in that borderland where astronomy, geology, and meteorology have each a claim.

1. "There can be no doubt that when the eccentricity is at its highest point the earth is, on the whole, rather nearer the sun, because, while the major axis of the ellipse is unaltered, the minor axis is least." ("The Cause of an Ice Age," p. 79). This is equivalent to saying that the mean distance of the earth from the sun is a function of the eccentricity of the earth's orbit, and is, moreover, such a function that when the eccentricity is a maximum the function is a minimum. The mean or average length of the radius-vector of an ellipse depends on the law assumed in regard to its variation. From the standpoint of geometry, disregarding kinematical and dynamical considerations, the simplest assumption is, that the vectorial angle is the fundamental variable. If the equation to the ellipse be written

$$r = \frac{a(1-e^2)}{1+e\cos\theta}$$

and r' be the mean length of the radius-vector, we may easily show that

$$r' = \frac{1}{\pi}\int_0^\pi r\,d\theta = a\sqrt{1-e^2}. \qquad (1)$$

But in any investigation dealing with the amount of light or heat received by the earth a different assumption should be made; for it is clear that if the earth moves most slowly when in aphelion the effect is the same as if it were, on the whole, farther away from the sun. Assuming that the time is the fundamental variable and that the radius-vector sweeps over equal areas in equal times, we may find the average of the radii-vectores corresponding to the successive equal time-intervals. Consider a point moving in a circle whose centre is one focus of the ellipse. Let its areal velocity be equal to that of the point describing the ellipse, and suppose that when the radius-vector of the ellipse has swept through 180°, the radius, r_0, of the circle has swept through the same angle. Then

$$r_0^2\frac{d\theta_0}{dt} = r^2\frac{d\theta}{dt} = 2c = \frac{\pi a^2\sqrt{1-e^2}}{T},$$

where $2T$ is the periodic time. Integrating between the limits 0° and 180°

$$\pi r_0^2 = \pi a^2\sqrt{1-e^2}, \quad \text{or} \quad r_0 = a\sqrt[4]{1-e^2}. \qquad (2)$$

r_0 is thus a minimum when e is a maximum, and *vice versa*. The value r_0 in (2) is greater than the value r' in (1), as we might have known in advance by simply comparing the two assumptions respecting the law of variation of r.

Developing the factor

$$\sqrt[4]{1-e^2}, \quad r_0 = a\left(1 - \frac{e^2}{4} - \frac{3e^4}{32} - \ldots\right).$$

The present eccentricity of the earth's orbit is 0.01678. According to Leverrier it cannot exceed 0.077747. To take $r_0 = a$, the average of $a(1+e)$ and $a(1-e)$, that is, of the aphelion and perihelion distances, is therefore a close approximation to the mean value obtained with the assumptions above made. Laplace, in stating Kepler's third law, says, "The squares of their times of revolution are as the cubes of the transverse axes of their ellipses." (*Méc. Cél.*, II., i., § 3). He uses the term "mean distance" in speaking of the satellites of Jupiter and Saturn, but not in such a way as to indicate that he meant the semi-major axis. Gauss, in his first mention of the semi-major axis, says, "Hinc semi-axis major, qui etiam distantia media vocatur, fit $= \frac{p}{1-ee}$". ("Theoria Motus," p. 4). Similarly, Sir John Herschel uses the terms "mean distance" and "semi-major axis" as interchangeable.

2. "The total quantity of heat which the earth receives during each complete revolution will be inversely proportional to the minor-axis of the ellipse." (p. 79). Let dh be the heat-increment received in the time dt, and μ the rate of variation of heat at a unit's distance. Then, since the quantity of heat received varies directly as the time and follows the law of the inverse square,

$$dh = \mu\frac{dt}{r^2}.$$

But from Kepler's second law,

$$r^2\frac{d\theta}{dt} = 2c, \quad \text{or} \quad \frac{r^2}{dt} = \frac{2c}{d\theta}. \quad \text{Hence}$$

$$h = \int_0^\pi \frac{\mu\,d\theta}{2c} = \frac{\mu\pi}{2c} \qquad (3)$$

From this it appears that the quantity of heat received in passing from one end of the major-axis around to the other varies inversely as the areal velocity. But

$$2c = \frac{\pi a^2\sqrt{1-e^2}}{T},$$

and since the length of the year is constant and the major-axis is constant, the areal velocity is to be viewed as a function of e alone. Suppose e becomes e' and let c' denote the new value of the areal velocity. Then $h' = \frac{\mu\pi}{2c'}$, and therefore $h : h' :: c' : c$. But $c : c' :: b : b'$; hence $h : h' :: b' : b$. Again, if we substitute $\frac{\pi ab}{T}$ for $2c$ in (3),

$$h = \frac{\mu\pi T}{\pi ab} = \frac{\mu T}{a^2\sqrt{1-e^2}} \qquad (4)$$

Hence the amount of heat received in one year is the same that would be received if the earth were to move for a year in a circle whose radius is $a\sqrt[4]{1-e^2}$.

This accords with the result (2) already found for the mean distance of the earth from the sun. In a paper on the "Intensity of the Sun's Heat and Light" (Smithsonian Contributions to Knowledge, IX.), L. W. Meech calls $\frac{2\pi}{a^2 n \sqrt{1-e^2}}$ "the sum of the intensities during a complete revolution." In this expression n is the mean daily motion and equals $\frac{\pi}{T}$. Substituting $\frac{\pi}{n}$ for T and making μ equal to 1 in (4), the latter reduces to Meech's formula.

3. "If any two chords of the earth's orbit, as AX and BY, be drawn through the sun, S, the amount of heat received in passing over the arc AB equals the amount received in passing over XY." (p. 82). Samuel Haughton ("New Researches on Sun-Heat," 1881) proves by another simple application of Kepler's second law that the quantity of heat received by the earth in a given time is proportional to the angle described in that time by the radius-vector. For

$$r^2\, d\theta = 2c\, dt,$$

$$d\theta = \text{increment of true anomaly},$$

$$\frac{dt}{r^2} = \frac{d\theta}{2c} = \text{heat in the time } dt.$$

This is but a mathematical translation of the argument given by Herschel in "Outlines of Astronomy," 5th ed., § 368 b. The statement made on page 82, "Cause of an Ice Age," is verified by an employment of Haughton's expression. For since

$$dh \propto \frac{dt}{r^2}, \qquad dh \propto \frac{d\theta}{2c}; \text{ hence}$$

$$h \propto \frac{\theta_2 - \theta_1}{2c}. \text{ Now } ASB = XSY = \theta_2 - \theta_1,$$

and the proposition is established. The law that "the amount of heat received in any given interval is exactly proportional to the true anomaly described in that interval" appears to have been first published by Lambert in his "Pyrometrie," 1779.

4. "The total heat received by the earth from equinox to equinox is equal to that received while completing its journey around the remaining part." (p. 83). The preceding demonstration does not involve the inclination of the chords to each other, neither does it involve the direction of either chord. Hence we may make X coincide with B and Y with A, and let the one resulting chord be the line of equinoxes, and the proposition follows.

5. "If δ be the sun's declination the amounts of heat received by the Northern Hemisphere and the Southern are to each other as $1 + \sin\delta$ to $1 - \sin\delta$." (p. 175). Draw a circle representing a section through the centre of the earth (regarded as a sphere). Let the horizontal diameter produced represent the celestial equator projected in a right line EE'. Through the centre of the circle draw AA', making an angle δ with EE'. AA' will be the axis of the cylinder of heat-rays falling upon the earth when the sun's declination is δ. Draw a diameter, DD', perpendicular to AA', and at the upper extremity of DD' draw an element, TT', of the cylinder. To this draw a parallel, CC', intersecting EE' at the circumference of the circle. TT' and CC' evidently include the portion of the cylinder falling on the Northern Hemisphere. If $2R$ is the length of the diameter, the perpendicular distance between TT' and CC' is seen to be $R + R\sin\delta$. Hence if $\frac{2H}{r_0^2}$ be the quantity of sun-heat falling perpendicularly on an area equal to the section of the earth at the mean distance r_0 from the sun in the unit of time, $\left(\frac{R + R\sin\delta}{2R}\right)\frac{2H}{r_0^2}$ is the part falling on the Northern Hemisphere, while the remainder, $\left(\frac{R - R\sin\delta}{2R}\right)\frac{2H}{r_0^2}$, falls on the Southern Hemisphere. These amounts are to each other as $1 + \sin\delta$ to $1 - \sin\delta$.

One or two other propositions will be discussed in a subsequent article.

ON A PHYSIOLOGICAL CLASSIFICATION OF THE OPHIDIA — WITH SPECIAL REFERENCE TO THE CONSTRICTIVE HABIT.

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THE writer would be the last to suggest a classification of any group of animals whatsoever based upon physiological data alone. Function, unless correlated with definite variation of structure, is never to be depended upon as a means of establishing specific differences. In illustration of this, one has only to cite the numerous examples of change of function, not simply within historic times, but even within the memory of living man, owing to variation in the environment of the creatures themselves. Witness the Kea, or New Zealand parrot, and the baboon of South Africa, both of which have become carnivorous since the introduction of sheep into this region; the bees of England, which, in certain districts, have within the last twenty years become frugivorous; and certain colonies of bats, inhabiting the islands of the Gulf of Paria in Trinidad, which have of late years taken to fishing, and have in consequence abandoned their nocturnal habits, and are now strictly diurnal beasts of prey. It is true that in certain isolated cases a change of function is followed by very slight variation of physical structure. In that of the domestic cat the intestine has certainly become elongated, and has probably undergone a further process of elongation in consequence of its less purely carnivorous diet; in particular, the duodenum has become more extended within recent centuries, if one may judge from analogy when comparing the creature with its wild prototypes.

In the case, however, of serpents, the family resolves itself into three groups so naturally in accordance with the manner in which they take their food, as to suggest the justification of a natural grouping founded on this basis.

If we had a specimen of every kind of snake before us, and could watch them in the act of feeding, we should see that they perform this process in three different manners. The majority, numbering probably 1,000 or 1 200 out of the 1,800 known species, simply catch the creatures on which they prey by the prehension of their jaws and long curved teeth, and work them gradually into the gullet on what we may call general principles.

A great disproportion exists between the size of the captor and of the captive. If the serpent be very much larger than the animal which it swallows, the latter is probably engulfed alive; but if, as is commonly the case, the captive is of large diameter proportionately to the œsophagus of the serpent, it is suffocated or crushed to death in the act of swallowing. As may be expected, the serpents that feed in this manner are such as live on what may be termed soft food, — frogs, lizards, fish, or other snakes.

But with the remainder we find two special provisions for the slaughter of the prey previous to deglutition — provisions so remarkable as to place the possessors in an entirely different category to the preceding. In one of these, and by far the smaller of the two subdivisions, numbering probably not more than 220 species altogether, or about one-eighth of the whole number of snakes, we find the death of the prey is encompassed by the injection of a morbific fluid, the venom. That this in the majority of cases serves as ammunition for the destruction of the captive cannot be doubted; but whether this is the primary reason why these creatures are gifted with venom is not so certain, seeing that in many species it probably comes very little into play for this purpose — e. g., in the sea snakes, in which the fangs are so short that the fish on which they live are scarcely scratched by them, and even in the great Ophiophagus, the snake-eating snake of India, whose natural diet consists of animals in which the circulation is so slow and vitality so sluggish as serpents that they are certainly swallowed before any poison could have time to work its effect upon them. In all probability the primary office of this remarkable fluid is to act as a digestive, it having been found by experiment that albumen, pieces of hard-boiled egg, etc., dissolve in this quite as readily as in the gastric juice of any flesh-eating animal. The writer has further established by his own experiments that small animals which have been submitted to the fangs of rattlesnakes, and other large viperine serpents are very much more quickly digested, not only by snakes, but by toads and other carnivorous reptiles, and even mammalia, than pieces of meat or animals of corresponding species which have not been so treated.

There can be very little doubt that this morbific fluid, this venom, is a product of a recent evolution. The venom gland, although large, is distinctly one of the salivary glands in structure, one of the racemose group, very little altered in appearance from that which secretes the ordinary saliva, the venom being in fact an abundant saliva, and containing some toxic element the nature of which has not yet been distinctly ascertained, in addition to the ordinary salivary products. There is probably no other instance in nature of the enormous disproportion of change of function when compared with change of structure as obtains in the venomous fluid of the gland of a poison-bearing snake, unless indeed it be the function of the brain of man when compared with that of animals almost equal to him in complexity of cerebral structure.

There remains, however, a third group of serpents, gifted with the power of killing their prey before deglutition. These, which number possibly 400 or 500 species (the number not being accurately ascertained owing to absence of observation of living specimens), may be termed the Constrictive Group; and although no such physical distinction can be drawn between these and the ordinary or Colubrine snakes on structural grounds, as is at once apparent between the latter and the venomous group, yet the process of feeding is so entirely different, as to suggest the feasibility of establishing such a difference, by careful dissection. With these snakes the prey is slain at the moment of the seizure, by constriction, by being wrapped within the folds of the body and crushed to death; and this process is so remarkable in its vigor and in its rapidity, that it is impossible to imagine the creatures destitute of specially developed, if not specially supplied, muscles for this purpose.

This group includes not only the great Anaconda of tropical America, the very much smaller Boas of that region, as well as the Tree Boas, and the Pythonoid snakes of Africa and the East Indies, but very many smaller species as well. The black snake of North America is indeed distinctively named Coluber Constrictor; but there are very many other species manifesting this peculiarity which have as yet obtained no such distintive recognition, such as the Blue Racer of the States, the Saw-marked snake of South America, and the largest of the European serpents, the beautiful four-rayed Elaphis of Italy and Greece, which occasionally attains a length of six feet, and is capable of swallowing a large rat.

It is just possible that this power of constriction may have been acquired recently, like the venom of the poison-bearing snakes. Unfortunately, paleontology affords no evidence upon this point. We know very little of the evolution of the Ophidia. Fossils are very scarce; and although some of them, such as the noted specimen from the London Clay, suggest serpents of large size, and therefore presumably constrictors, we know nothing beyond what is suggested by mere inference as to whether they were gifted with venom, or had this property of constricting their prey before swallowing.

If we examine the lateral and intercostal muscles of one of the large Pythonoid snakes, we shall find that although these are very highly developed, and have indeed in certain instances small tendenous slips attaching them to the ribs, which are not found in smaller species, they are precisely analogous to the ordinary intercostal muscles which obtain through the whole of this family.

In certain species, such as the Milk snakes of the Northern states, and the Mandarin snake of China, we may occasionally see, when they are dealing with prey rather too strong for them, a sort of attempt made at constriction, a rapid coiling and uncoiling of the body, as though to confuse the animal struggling within the grasp of the jaws and teeth. And it is perhaps not wholly unjustifiable to imagine that this power of constriction may originally have been acquired in this way; that serpents which had previously fed, as our ordinary Colubrine snakes do, upon frogs, lizards, and soft-bodied animals which they could kill by pressure of the jaws alone, found themselves, for some reason or other, reduced to catching the smaller mammalia, mice, moles, etc., and that in their endeavors to get these within the cavity of the mouth, they found it necessary to bring the body into play to effect the purpose which had hitherto been accomplished by the jaws alone. One may, however, express the hope that when larger materials are at hand for examination, in the shape of the grander Pythonoid snakes, and most especially of the great Water Boa, the Anaconda of Central America, that some more definite information on this point will be gleaned.

NOTES AND NEWS.

AT a recent meeting of the Canadian Institute, Mr. Andrew Elvins asked permission to add a sentence or two to his paper on the satellites of Jupiter, read at a former meeting. He said: "The period of each satellite as we pass outward from the planet is about double that of the one next inside itself, except in the case of Satellite I. Half its period would be about 21 hours, but there is no satellite having that period. Half of this 21-hour period is just where Professor Barnard's new satellite exists. Its period is between 11 and 12 hours. I therefore think that an undiscovered sixth satellite exists at 166,000 miles from Jupiter's centre, with a period of 21 hours."

—The faculty of the Museum of Comparative Zoölogy, Cambridge, Mass., will receive applications from candidates desiring to occupy the table at the Naples Zoölogical Station, which has been placed at its disposal from Oct. 1, 1893. The applicant must be (or have been recently) a student or instructor at some American university, preferably a person who has taken the degree of Ph.D. or S.D.; he must have published some creditable original investigation, and should be recommended as an able investigator by the professor under whom he has studied. Applicants will please forward to Professor Alexander Agassiz, Director of the Museum, before May 10, their recommendations and a statement of their qualifications and of the subject to which they hope to devote themselves. In order that the faculty may make the most satisfactory disposition of the table during the whole year, the applicants are requested to state the length of time they desire to remain at Naples, and also the earliest and latest dates within which they can avail themselves of the appointment. The faculty will, at its meeting in May, nominate to the Corporation of Harvard College for approval the incumbent or incumbents for the year 1893–94.

—The papers entered to be read at the April meeting of the National Academy of Sciences, are as follows: On the Systematic Relations of the Ophidia, E. D. Cope; Biographical Memoir of General Montgomery C. Meigs, H. L. Abbott; On the Nature of Certain Solutions, and on a New Means of Investigating Them, M. C. Lea; The Relations of Allied Branches of Biological Research to the Study of the Development of the Individual, and the Evolution of Groups, The Endosiphonoidea (Endoceras, etc.) Considered as a New Order of the Cephalopods, A New Type of Fossil Cophalopods, Results of Recent Researches upon Fossil Cephalopods of the Carboniferous, A. Hyatt; Biographical Memoir of Julius Erasmas Hilgard, E. W. Hilgard; Monograph of the Bombycine Moths of America, North of Mexico: Part I.—Notodontidæ, A. S. Packard; Intermediary Orbits, G. W. Hill; The Relations between the Statistics of Immigration and the Census Returns of the Foreign-born Population of the United States, Statistical Data for the Study of the Assimilation of Races and Nationalities in the United States, Richmond Mayo-Smith; Telegraphic Gravity Determinations, Comparison of Latitude Determinations at Waikiki, T. C. Mendenhall; A One-volt Standard Cell, H. S. Carhart (introduced by T. C. Mendenhall); Fundamental Standards of Length and Mass, T. C. Mendenhall; Peptonization in Gastric Digestion, R. H. Crittenden; Helen Kellar, Alexander Graham Bell; On a Potentiality of Internal Work in the Wind, On a Bolograph of the Infra-red Solar Spectrum, S. P. Langley; The Classification of the Gastropodous Mollusks, Theo. Gill. Presentation of the Draper Medal to Professor H. C. Vogel.

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Attention is called to the "Wants" column. It is invaluable to those who use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The "Exchange" column is likewise open.

OBSERVATIONS ON GOPHERS AND MOLES IN OREGON.

BY F. L. WASHBURN, STATE EXPERIMENT STATION, CORVALLIS, OREGON.

THE Zoölogical Department of the Oregon State Experiment Station has endeavored for two years to find some remedy for the pocket gopher and common mole found in this section. The first named, *Thomomys bulbivorus*, is a bad pest, and the decree has gone forth that our mole, *Scapanus townsendii*, is to be placed in the same category. Various traps purporting to catch gophers and moles are on the market, but few of them are reliable. Two, however, have been found to do very effective work in the case of both animals. Of poisons, powdered strychnine, introduced into pieces of potato an inch square and thrust down the burrows, has proved efficacious in the case of gophers in the absence of succulent root-crops. And small pieces of beef, poisoned with the same agent, have been placed in moles' burrows with occasionally good results, though nothing conclusive can be claimed for that now.

It is, however, of a few habits of the gopher, and more particularly of some interesting discoveries regarding the diet of the mole that we would here treat.

The pocket gopher has not a little intelligence. The horticulturist of this station reports finding a nest stored with potatoes, the tubers lying in layers, and each layer separated from the adjoining layers by more or less dried grass. The entrance to this nest, or at least one entrance, was from below, affording a perfect system of drainage quite desirable in this country of wet winters.

Again, it is a matter of frequent occurrence to see gopher mounds arranged in a straight line from that side of a field or garden upon which the gopher enters to some fruit-tree or potato, parsnip, or carrot patch, indicating that his main burrow beneath the surface has been pushed directly to these sources of food-supply. This main burrow, by the way, is from twelve to twenty inches below the surface, and has leading from it, at intervals, short branch burrows, which open on the surface of the ground and afford a means of getting rid of the soil excavated below. These branches are generally plugged with soil and their openings covered by a mound. The last one made, however, is often open, and the occupant of this underground retreat can frequently be seen protruding his head and disposing of the soil he has brought from the main burrow. As to the method of bringing out this soil opinions differ, some observers claiming that it is carried in the pockets, to some extent at least, and then thrown out of the pockets by movements of the fore-feet. Others deny this, asserting that the dirt is pushed before the animal and that the pockets are not used in this work. The writer has frequently secured specimens with forage in these pouches, but has never found them to contain earth, even in specimens killed in the act of excavating.

This rodent works during the night and is quite likely to be found digging early in the morning, again about noon, and again late in the afternoon. It is claimed that both the gopher and mole are more active, as regards digging, just before rain.

The nest of the pocket gopher is often found filled with camass bulbs, of which this animal is very fond, as much as a bushel of bulbs being reported as found in a single nest. When in the vicinity of gardens, however, more palatable food is found with which to store the larder.

The tender roots of young fruit-trees are, unfortunately, very tempting to these animals, and a dying cherry or apple can frequently be easily lifted from the ground, the root, gnawed completely through, showing the cause of its demise.

This leads to the subject of the mole's diet. Many, or most, of our scientists have united in defending the mole against charges of eating bulbs and other vegetable matter, and have stoutly asserted that the gnawed carrot, or parsnip, or crocus bulb, found in the course of the mole's burrow, was the work of one of the meadow mice. This is doubtless true. But that the mole occasionally, or possibly frequently, resorts to a vegetable diet must be acknowledged. A lady in Portland, Ore., quite sure that moles were eating her crocus bulbs, and feeling far from convinced of their innocence from the assertions of scientists, obtained three, which she kept in confinement. She found that they readily ate the following: Beef, mutton, pork, bread, wheat, pears, and peas. Unfortunately two of these pets were fed with worms taken from an old manure heap and died, showing symptoms of being poisoned. The description of these worms, as given me, answered to that of *Lumbricus fœtidus*, and it was undoubtedly that or an allied form which caused the trouble. Evidently this species of the Oligochetæ does not figure, naturally, on the mole's bill of fare. The writer witnessed the survivor eat peas greedily, running his sensitive snout from one end of the pod to the other and taking out every pea. This was convincing proof that the mole, under some circumstances at least, is not strictly carnivorous, and it is quite likely that he is frequently a malefactor as regards vegetables and roots. Personal examination of stomachs in specimens secured in March, 1892, revealed nothing but finely triturated earthworms, insects, and insect larvæ. In one captured in January, 1893, the stomach contained nothing but delicate, fibrous roots.

Amos W. Butler of Brookville, Ind., in speaking of moles, says: "I have never been satisfied that the mole in sandy soil is not very destructive to young pea sprouts just as they are emerging from the ground."

Both gophers and moles are active here during the winter season.

A word as to the breeding season may not be out of place. My diary states that on Feb. 28, 1892, a pregnant mole was captured containing three well-developed embryos, and two days later another was obtained with two embryos, apparently within a few days of birth. March 28, 1893, a pocket gopher was secured containing four young embryos. All this indicates an early date for the first litter. Probably more than one litter is produced. From specimens of Arvicolinæ secured it would appear that the breeding time of the field-mice is contemporaneous with that of the other two animals under discussion.

ONE OF THE GYPSUM CRYSTALS FROM THE CAVE AT SOUTH WASH, WAYNE CO., UTAH.

BY ALFRED J. MOSES, MINERALOGICAL LABORATORY, COLUMBIA COLLEGE, NEW YORK.

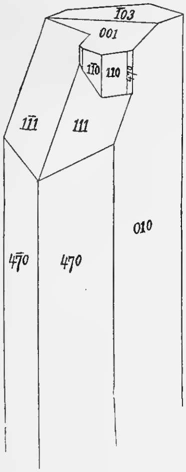
THE Deseret Museum of Salt Lake City sent last month to Columbia College two specimens from the remarkable deposit of Selenite, in Southern Utah, which was described in a recent issue of this paper.[1]

The larger of the two specimens was a portion of an evidently longer prism with very perfectly developed terminal planes. The specimen internally is colorless and glassy, but the surface is in part covered by a thin opaque layer. The weight of the specimen

[1] Feb. 17, 1893, pp. 85-86.

is 24½ pounds, its greatest length is 27 inches, its thickness (in direction of the ortho axis), is 4 inches, and its breadth (at right angles to the ortho axis) is six inches.

The angles of the crystal were taken with the hand goniometer. The most noticeable fact is that the unit prism of 111° 30′ occurs only on a curious prismatic extension, composed of this prism and a clino prism (470), which pierces the pyramidal plane 111 and extends upward about one inch to the basal plane common to this and the rest of the crystal as shown in the figure. The prism which occurs on the rest of the crystal has an angle of



approximately 79°, corresponding to the clino prism i—$\frac{7}{4}$ (470), and all its faces are striated vertically while those of the unit prism are smooth.

The other occurring forms are ι–$\bar{\iota}$ (010), –1 (111), $\frac{1}{3}$–$\bar{\iota}$ ($\bar{1}03$), and O (001). The cleavages parallel to (010), ($\bar{1}01$) and ($\bar{1}11$) were visible in the break at the lower end.

At least six phantom terminations can be seen apparently parallel to (001) and ($\bar{1}03$).

DISTANCE AND COLOR PERCEPTION BY INFANTS.

BY J. MARK BALDWIN, PRINCETON, N. J.

I UNDERTOOK at the beginning of my child H's 9th month to experiment with her with a view to arriving at the exact state of her color perception, employing the new method which I described and compared with other methods in a recent paper in this journal.[1] The method consisted in this instance in giving the infant a comfortable sitting posture, kept constant by a band passing around her chest and fastened securely to the back of her chair. Her arms were left bare and quite free in their movements. Pieces of paper of different colors were exposed before her, at varying distances, front, right, and left. This was regulated by a frame-work, consisting of a horizontal graded (in inches) rod, projecting from the back of the chair at a level with her shoulder and parallel with her arm when extended straight forward, and carrying on it another rod, also graded in inches, at right-angles to the first. This second rod was thus a horizontal line directly in front of the child, parallel with a line connecting her two shoulders, and so equally distant for both hands. This second rod was made to slide upon the first, so as to be adjusted at any desirable distance from the child. On this second rod the colors, etc., were placed in succession, the object being to excite the child to reach for the color.

So far from being distasteful to the infant, I found that with pleasant suggestions thrown about the experiments, the whole procedure gave her the most intense gratification, and the affair became her most pleasant daily occupation. After each sitting she was given a reward of some kind.

The accompanying tables give the results, both for color and distance, of 217 experiments. Of these 111 were with five colors and 106 with ordinary newspaper (chosen as a relatively neutral object, which would have no color value and no association to the infant). In the tables R stands for "refusal" (to reach out for the object), A for "acceptance" (and effort), N for the entire number of experiments with each color respectively, and n for the entire number with all the colors at each distance respectively. So $\frac{A}{N}$ = the proportion of responses or efforts for any color, and $\frac{R}{n}$ = the proportion of refusals for each distance.

Table I.

Distance, inches.	9	10	11	12	13	14	15	Totals.	Ratio $\frac{A}{N}$
	R. A.	R. A.	R. A.	R. A.	R. A.	R. A.	R. A.	R. A. N.	
Blue.	0—1	0—4	0—5	1—3	2—4	1—5	3—1	7—23—30	.76⅔
Red.	0—1	0—3	2—2	1—4	1—7	1—7	5—1	10—25—35	.71½
White.	0—0	0—0	0—0	0—1	0—5	1—1	3—0	4— 7—11	.63⅔
Green.	0—0	0—1	0—1	2—1	1—4	1—2	2—0	7— 9—16	.56⅓
Brown.	0—1	0—2	2—1	3—2	0—3	3—1	2—0	11—10—21	.47½
Totals.	0—3	0—10	4—9	7—11	4—23	7—16	15—2	37—74—111	.67
Ratio $\frac{R}{n}$	0	0	.33⅓	.39	.15	.30½	.89		

Table II.

Distance, inches.	9	10	11	12	13	14	15	Totals.	Ratio $\frac{A}{N}$
	R. A	R. A.	R. A.	R. A.	R. A.	R. A.	R. A.	R. A. N.	
Newspaper.				0—17	0—28	1—33	25—2	26— 80—106	.75½
Color.	0—3	0—10	4—9	7—11	4—23	7—16	15—2	37— 74—111	.67
Totals.	0—3	0—10	4—9	7—28	4—51	8—49	40—4	63—154—217	.71
Ratio $\frac{R}{n}$			.30½	.20	.07¼	.14	.91		

Color.—The results are evident in the tables (I. and II.), especially the columns marked "Ratio $\frac{A}{N}$" and "Ratio $\frac{R}{n}$." The colors range themselves in the order of attractiveness, i.e., blue, red, white, green, and brown. The difference between blue and red is very slight compared to that between any other two. This confirms Binet as against Preyer (who puts blue last), and also fails to confirm Preyer in putting brown before red and green. Brown to my child — as tested in this way — seemed to be about as neutral as could well be. White, on the other hand, was more attractive than green. I am sorry that my list does not include yellow. The newspaper was, at reaching distance (9 to 10 inches) and a little more (up to 14 inches), as attractive as the average of the colors, and even as much so as the red; but this is probably due to the fact that the newspaper experiments came after a good deal of practice in reaching after colors, and a more exact association between the stimulus and its distance. At 15 inches and over, accordingly, the newspaper was refused in more than 92 per cent of the cases, while blue was refused at that distance in only 75 per cent, and red in 84 per cent.

Distance.—In regard to the question of distance, the child persistently refused to reach for anything put 16 inches or more away from her. At 15 inches she refused 91 per cent of all the

[1] Science, April 21, 1893.

cases, 89 per cent of the color cases, and, as I have said, 92 per cent of the newspaper cases. At nearer distances we find the remarkable uniformity with which the *safe-distance* association works. At 14 inches only 14 per cent of all the cases were refused, and at 13 inches only about 8 per cent. The fact that there was a larger percentage of refusals at 11 and 12 inches than at 13 and 14 inches is seen from the table (I.) to be due to the influence of the brown, which was refused consistently when more than 10 inches away. The fact that there were no refusals to reach for anything exposed within reaching distance (10 inches) — other attractive objects being kept away — shows two things: (1) the very fine estimation visually of the distance represented by the arm-length, thus emphasizing the element of muscular sensation in the perception of distance generally; and (2) the great uniformity at this age of the phenomenon of "sensori-motor suggestion"[1] upon which this method of child study is based.

In regard to the relative use of the two hands in these and other experiments, — this is a topic to which I wish to devote another paper, giving details upon which certain conclusions (announced in an earlier note in this journal) are based.[2]

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 Convex Profile of Bad-Land Divides.

UNDER this caption Professor W. M. Davis, in *Science*, Oct. 28, 1892, discusses the "missing factor" in Gilbert's "Law of Divides," and concludes that it is "the creeping of the surface soil."

In my class-room lectures, and in a paper forwarded four months ago to the secretary of the Geological Society of America, but not yet published, I also have attempted an explanation of this missing factor. I mention this merely for the truth of history, not that I care much for the credit of priority, or fear the charge of plagiarism when my explanation appears. Its independent origin will be self-evident, because I have approached the problem in a very different way.

Both Professor Davis and Mr. Gilbert seem inclined to regard bad-land forms as something apart from land-sculpture in general — something which requires special explanation — while I have cited general laws and deduced these forms from them. My paper is entitled "Some Elements of Land-Sculpture: Water Curves, Weather Curves, and Structural Angles." Water curves are either horizontal, e.g., the serpentine course of a river, or vertical. The vertical water curve of erosion is concave upwards, e.g., the normal gradient of a stream excavating its channel in homogeneous material (b.c. Fig. 3); and the vertical water curve of deposition is convex upward, e.g., a *débris* fan, or alluvial cone.

All weather curves are convex upward. This fundamental law of the weather curve I have deduced theoretically in two ways, and that it is confirmed by observation almost goes without saying. An angular structural block, *A*, Fig. 1, is rounded by

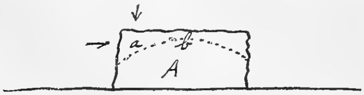


FIG. 1. — A structural block rounded by weathering. The dotted line is the weather curve, convex upward.

weathering, that is, its outline becomes a flowing curve, convex upward, like the dotted line in the figure, because the protruding angles are more exposed to attack, and at the same time the products of disintegration are in a position to be quickly removed.

[1] See my article on "Suggestion in Infancy," Science, xvii., 1891, p. 113; also my "Handbook of Psychology," Vol. II., pp. 297 ff.

[2] Science, xvi., 1890, p. 247.

The complex forces included under the general term weathering have a double advantage at *a* as compared with *b*, because the attack comes from two directions. Moreover, the removal of loosened particles, whether by falling raindrops, by winds, or by gravitation (one effect of which is creeping), proceeds many times faster at *a* than at *b*. By a similar but slightly modified process of reasoning, it may be shown that a sharp crest triangular in cross-section would be rounded also by weathering (c.f, La Noé and Margarie, Les Formes du Terraines).

Another method of deducing the upward convexity of weather curves is that which is based upon the law of slopes in relation to hardness. The harder the rock the steeper the slope, other things being equal. Let 1, 2, 3, and 4 (Fig. 2) denote strata which grade regularly downward in hardness, No. 1 being hardest of all. Then, if the products of disintegration are at once and completely

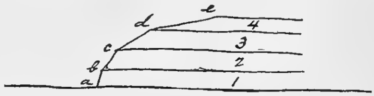


FIG. 2. — Convex slope formed by the weathering of rocks which regularly increase in hardness downwards.

removed, as, for instance, by a stream flowing at *a*, the hard rock, No. 1, will form a cliff *ab*, while *bc* will be less steep, *cd* still less steep, and *de* very gentle. Each element of the slope, e.g., *bc*, is a straight line in cross-section, but the general effect is that of a curve; and if the beds were very thin it would pass from a broken line to a true flowing curve, convex upward. Now we may conceive the series 1, 2, 3, 4 to have been originally homogeneous, and that weathering has softened the upper members. In that case the downward gradation in hardness would be by infinitessimal laminæ, and the resultant slope a typical weather curve.

Ordinarily, the convexity does not extend to the bottom, because the weather curve is there replaced by the vertical water curve of erosion. This combination of weather and water curves modifying structural blocks yields the form shown in Fig. 3, the

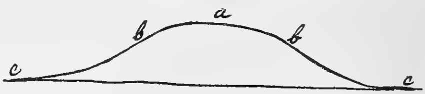


FIG. 3. — Cross-section of any ordinary ridge or hill.

most typical, as it is also the most familiar and universal of earth-forms. The upper part, *ab*, of each slope is a weather curve, convex upward, and the lower part, *bc*, is a water curve, concave upward. Bad-land divides are excellent examples of the general law, instead of being exceptions to it. The convex profile of the summit which puzzled Gilbert is simply the familiar and omnipresent weather curve. The only thing exceptional about it in the bad-lands is its narrowness and sharpness of curvature. That depends chiefly upon the early stage of the base-levelling in those regions, as I have shown in my forthcoming paper.

Creeping is a real factor in the rounding of divides, but is only one phase of the secondary process of transportation. Disintegration is the primary process. And in the subsequent movement of loosened particles, falling raindrops, gusts of rain driven aslant by winds, the winds themselves, the rolling and tumbling effects of gravitation as distinguished from the slow process of creeping — all these are active and efficient agents of removal. Their combined effects overshadow the results of creeping, especially on the bare, sharp ridges of the bad-lands. The clays are compact and firmly adherent. It is on gentle and turf-bound slopes that the slow process of creeping is relatively most effective.

Nor do I agree that the weather curve on the summit of bad-land ridges would be obliterated if the rainfall should increase. The effect of falling raindrops belongs to the category of weathering, and produces convex curves. It is only when the fallen drops gather into rills and begin to *flow* that the concave water curve of erosion begins to form. Hence increased rainfall would probably strengthen rather than obliterate the weather curve, especially when we consider the effect of increasing vegetation which would follow increased rainfall. L. E. HICKS.

Lincoln, Neb., Nov. 4, 1892.

The Moon's Atmosphere.

IN *Science* of Feb. 24, Sir Robert Ball makes application of the kinetic theory of gases to explain the absence of air from the moon. He observes that, although the mean molecular velocity of translation is less than that required by a body projected vertically from the moon to overcome the moon's attraction, "in the course of their movements, individual molecules frequently attain velocities very much in excess of the average pace," and would therefore be able to escape from the moon into space, and thus, in time, the whole atmosphere would be lost. I think a full consideration of the subject will not justify that conclusion, but that we shall be obliged to resort to some other physical laws to solve this old problem of speculation.

The kinetic theory requires all the molecules of a gas to have equal masses, equal energies, and hence equal mean velocities. This mean velocity for the hydrogen molecules at 0° C. is about 1,800 metres per second, while that of oxygen and nitrogen is about 450 metres per second, since the velocity is inversely proportional to the square root of the mass of the molecule. To overcome the moon's attraction a body must have a vertical velocity of about 2,200 metres per second. But it must be remarked that the escaping molecules, if there are such, are only those of the outer confines of the atmospheric envelope, where the mean free path of the molecules is relatively very great, as suggested with respect to the earth's atmosphere by H. Daniells ("Principles of Physics"), and the temperature of those regions is very low. If the temperature is about 68° absolute scale (—204° C.), as assumed by some authorities, the mean molecular velocity falls to about 225 metres a second, since the velocity varies as the square root of the absolute temperature. The vertical velocity, then, or the vertical component of the velocity must be about ten times the mean velocity to balance the force of gravitation, which is not probable.

Again, if the temperature is much lower than 68° absolute, approximating the absolute zero, and the molecular velocity always obeys the law before mentioned, the velocity also would approximate zero, and of course the molecules could not escape the attraction. It appears, then, to be largely a question of the temperature of the outer limits of an atmosphere. With this in view, let us compare results on planetary bodies of different size and stage of world life. As already suggested, with respect to the earth and moon, the earth's attraction at the surface is about five times that of the moon at its surface. This, *cœteris paribus*, would require about five times greater molecular velocity of its atmosphere to escape than for that of the moon. But, if we take into account the previous history of the two bodies, it is observed that the earth was highly heated for ages after the moon had become comparatively cool, and this must have rarefied and expelled its atmosphere to great heights, and maintained a temperature in those regions which, according to the proposition under discussion, would have caused the earth to lose its atmosphere. In general, it would follow that the major planets and larger satellites would lose their atmospheres more completely while cooling than the smaller ones, unless they have correspondingly greater quantities of volatile matter in their composition than the smaller ones. And such seems to be the result. Even Jupiter, whose attraction at the surface is 2.6 times that of the earth, is believed to have an atmosphere much less extensive proportionately than the earth. Mars offers a good example of a small planet with a copious atmosphere. Its attraction is only about twice that of the moon. Why has he not lost his atmosphere? If the application of the kinetic theory alone explains the loss of the moon's atmosphere, it would require Mars to have suffered the same fate before now. Possibly we are committing the error of the Greek philosophers in treating melecules as independent masses instead of regarding them as inter-dependent centres of activity whose phenomena, as a system, constitute the qualities of matter. I do not assume to offer a solution for this complex problem, but hope rather to encourage discussion which will call out all the principles of physical science applicable to it. W. H. HOWARD.

Adrian College, Adrian, Mich., April 15.

Note on the Crystalline Lens of the Eye.

MR. MCLOUTH's observation upon "A Peculiar Eye," as observed by him in "a domestic animal," given in *Science*, No. 531, would have been considerably enhanced in value had he recorded at the same time what that "domestic animal" was; whether it was an anserine fowl, as a duck or goose; or a gallinaceous one, as a hen, turkey, peacock, or guinea-fowl; or whether a carnivorous mammal, as a dog, or a cat; or an *Equus*, or a *Bos*, or a *Sus*, or an *Ovis*, or what not.

To the minds of some, the so-called "domestic animals" form a natural group, and even such an authority as Girard was so blind as once to propose a *special* classification for the domesticated mammals! It is not uninteresting to trace the origin of this idea, associated as it is in a way with the kindred one of man holding a place apart from the rest of organized beings.

It is only necessary to invite Mr. McLouth's attention here to the fact that the crystalline lens in the eye of man consists of *three* triangular segments, and their existence is easily demonstrated by immersion of the lens in strong alcohol, or by boiling it. The apices of these three segments are at the centre of the lens, in *front;* their bases in the circumference. Another structural feature of the lens is seen in the laminæ of which it is composed. The treatment just proposed demonstrates these also, consisting, as they do, of concentric layers, which are firm at the centre, but become softer as we approach the peripheral ones. Likewise, by thus treating the crystalline lens from the eye of a horse, we prove that it also divides into its concentric laminæ, and its *three* triangular segments. But whether this holds true in the case of all vertebrates has not, I think, been demonstrated. Very likely the crystalline lens of the "domestic animal" examined by Mr. McLouth had been submitted to a process which had a similar effect upon it as boiling or immersion in alcohol would have had, and simply exhibited its normal structure. From what I can gather from the communication of your correspondent in *Science* there was nothing abnormal about the lens of the eye he examined.

R. W. SHUFELDT.

Takoma, D.C., April 14.

The Aurora.

IN *Science* for April 7, at page 186, certain statements of mine in regard to auroral effects proceeding from the sun's eastern limb are called in question. It would have been much more satisfactory if these criticisms had given evidence of such familiarity with the subject as would be shown by the mention of even a single date on which it might be claimed that an aurora appeared in the absence of well-defined solar conditions of the character indicated. Except where specific mention is made of such individual instances, the writer proposes to refrain from discussion, which would readily become interminable as well as utterly inconclusive. Such results as those of Professor Ricco, recently announced in *Astronomy and Astro-Physics* and elsewhere, it is a pleasure to meet with and comment upon. He simply takes the case of the great magnetic storms of 1892, which were eleven in number, and studies the coincident solar conditions, especially with reference to the location of spot groups at the meridian. In seven out of the eleven instances he finds that there were such groups on the meridian, but that the magnetic effect, if it proceeded from them at all, was not felt for a varying period of from twenty-one to fifty-one hours subsequently. If, however, he had gone further and inquired what there was at the eastern limb on these dates, he would have found that there was a spot group in that location in every one of these instances without any exception whatever, and that these groups were located upon areas which were much disturbed at successive returns by rotation. Moreover, there was in these instances no appreciable retardation or variability of retardation, the magnetic storm being in progress on the very dates when the disturbed sections were in process of being brought into view by rotation. Perhaps the most striking illustration of the whole matter in a single instance is to be found in the history of a great disturbance upon the sun in January, 1886. Upon the 12th of that month spots suddenly began to form almost precisely at the meridian and about 10° south of the sun's equator. Upon the four days following, these spots became numerous, and some of them very large, covering an enormous area, extending finally from the meridian almost half-way to the western limb. It would seem that if magnetic effects ever proceed from the sun's meridian that this, above every other, should have been a case in point. But there was scarcely any disturbance whatever and no auroras were reported from any source. On Jan. 16 and 17 the magnets were entirely free from disturbance when this great spot-group was undergoing many rapid changes and was generally in the precise location to have a terrestrial magnetic effect according to the idea which Professor Ricco attempted to work out as above described. When, however, this area was at the eastern limb, from Jan. 7 to 11, although it had not yet developed spots and was the seat of groups of brilliant faculæ only, there was an entirely different state of affairs, a great magnetic storm being in progress and auroras being reported generally from localities in high latitudes. Thus it appears that it is not faculæ in general that produce such marked effects, but faculæ in the location of areas frequented more or less persistently by spots, etc. M. A. VEEDER.

Lyons, N.Y., April 14.

Where is the Litre?

I HAVE read Professor Mendenhall's contribution to *Science* of April 21 with surprise. I did not think it possible for so eminent a man to so entirely miss the point of any article he might condescend to read and criticise. Nor did I think it possible for so keen-witted a controversialist to so entirely forget his own argument as to admit and corroborate the very statements he set out to refute. Yet any reader of *Science* who may take the trouble to read the two articles written respectively by Professor Mendenhall and myself under the heading "Where is the Litre?" will see that both of the unlikely events in question have happened.

I invite my distinguished critic to re-peruse the paper he attacks, and to thus ascertain whether it contains any statements or contentions displaying "ignorance of the recognized principles of metrology," or whether it sets forth "certain conclusions which will generally be harmless on account of the very magnitude of their errors." If he can find any statements, contensions, or conclusions that appear to him to justify such descriptions, let him quote them in their *ipsissima verba*, and let him show in what manner they betray ignorance or error. I will then, in my turn, show the Professor to be mistaken.

This is no over-bold challenge. It is almost self-evident that Professor Mendenhall was unable to find any display of ignorance or any erroneous conclusion in my article; as, in that case, he would naturally have quoted the offending passages in justification of his severe remarks. But his only approach to quotation is worded as follows: "The sermonizing finish to the article, beginning with the sentence, 'In spite of the much lauded simplicity of metric measures,' etc., may, however, mislead a few readers whose ideas have been befogged by the perusal of the previous three pages." Such a reference is too loose, too indefinite, and too general to indicate what particular statements or conclusions are objected to; and the Professor's scornful allusion to easily-befogged readers of *Science* is, perhaps, too donnish.

And now, while leaving my critic to the digestion of my challenge, I may, without impropriety, quote some opinions that have reached me from other authorities.

1. The *Engineering News* of March 30, in an editorial reference to my paper, says: "Different enactments by legislative bodies, errors in measurement and in calculation, difference in weights between bodies weighed in air and weighed in vacuo, and difference in weights between water containing air and water freed from it have conspired to produce these variations. It is true these variations are all so small as not to affect the practical accuracy of any ordinary measurements; but for the exact work of physicists and chemists, and for some of the finer measurements of engineers, these variations are sufficient to affect the results. The moral which Mr. Emmens points is that the author of any paper or treatise claiming scientific accuracy, and dealing in quantities whose exact values may be in doubt, should preface his work with a statement of the constants adopted throughout the work. In a personal letter to us Mr. Emmens makes the further suggestion that the international congress of scientists and engineers at Chicago next summer will afford an excellent opportunity for defining anew the metric standards whose values have become most variable, thus restoring to the system the advantages of simplicity and freedom from ambiguity which it was originally intended to possess. It certainly gives good ground for criticism that in every school in the land pupils are taught that the litre is equal to the cubic decimetre, whereas, in reality, the litre is about 0.1 cubic inches larger than a cubic decimetre, the exact variation depending on what value is chosen for each."

2. Professor De Volson Wood, of the Stevens Institute, writes: "Your article in *Science*, 'Where is the Litre?' is such a model of courteous discussion that I thank you for it. The closing remarks contain sentiments I often advocate, but you have done it so much more completely and in all respects so much better than I could, that I appreciate it."

3. Mr. R. A. Hadfield, of the Hecla Steel Works, Sheffield, England, whose scientific reputation is world-wide, writes: "It appears to me you have touched the weak point of the Metric system, and it was only the other evening, at a lecture on this subject, that I was aware for the first time there was a difference between the litre and the cubic decimetre. No doubt many others are in the same way, and it would therefore be specially desirable to have some common understanding on this matter."

4. Mr. Latimer Clark, F.R.S., writes: "I will see the Board of Trade with your letters. They are as anxious as you or I can be to help in such a cause, and would do anything to promote it. The Chicago conference would afford a capital opportunity for raising the question, and I will do anything required if you will point out what you recommend. The difference between the litre and cubic decimetre is simply one of popular belief and teaching, and it arises from the French Bureau having decided to adopt the bulk of the kilogramme of water as the bulk of the litre. I may perhaps add that the Warden of the Standards here has written me that he acknowledges my dictionary as correctly setting forth the values they have adopted and are employing, and he adds that he recommends the book to all enquirers on the subject."

I refrain from adducing further evidence lest I should put Professor Mendenhall in the position of the dissentient juryman who complained that "he had never before, in the whole of his life, met with eleven such obstinate fellows."

STEPHEN H. EMMENS.

Youngwood, Pa., April, 25.

Sham Biology in America.

MR. CONWAY MACMILLAN has shown more enthusiasm than discretion in his recent article. He is writing in a good cause, namely, the elevation of botany to an equal rank with zoölogy in biological teaching in universities. Biology, however, is not the science of animals and of plants, as Mr. MacMillan maintains, it is rather the science of life; and I am not aware that biology is taught in any large institution in this country without taking advantage of the fact that certain laws and principles of life are, for purposes of practical study, far better shown in plants than in animals. Plant biology is therefore extensively taught upon the lines laid down by Huxley and Martin, and on such lines we simply select the organism which best demonstrates a certain principle. If the botanists of this country allow the zoölogists to take the lead as *biologists*, that is, in setting forth the fundamental principles of life from their observations upon animals, it will naturally follow that zoölogy will occupy the leading position in the universities. Mr. MacMillan's argument should therefore be directed to the botanists and not to the zoölogists, who are in no way responsible for the alleged one-sided state of biological education.

While Mr. MacMillan's enthusiasm is in a good cause, he has allowed it to run away with his discretion. Without sufficient reflection or inquiry, he has, unintentionally, I am sure, given an entirely wrong impression of the character of work done in several institutions; this is done under a very sensational title and in a style of questionable taste. As it is desirable that this impression should not spread, and as the arrangement of courses in Columbia is cited by Mr. MacMillan as a leading example of the manner in which botany is subordinated to zoölogy, let us see what the Columbia courses are, as announced in the circular of the faculty of pure science:—

17 *Courses in Botany*, in A. Department of Botany.	1. Elementary Botany. 2. Elementary Botany. 3. General Botany. 4. Vegetable Anatomy. Cells and Tissues. 5. Morphology and Determination of Flowering Plants. 6. Economic Botany. 7. Cryptogamic Botany. 8. Advanced Vegetable Anatomy. 9. Natural Orders of Flowering Plants. 10. Advanced Cryptogamic Botany. 11. Comparative Study of Tissue of Twelve Species. 12. Comparative Study of Plants from a Certain Area. 13. Critical Study of a Genus.
in B. Department of Geology.	1. Palæobotany. 2. Study of Flora of Certain Geological Horizons.
3 *Courses in Physiology*, in Department of Physiology.	1. General Physiology. Lower Animal Types. 2. Human Physiology. Man and Lower Animals 3. Laboratory Physiology.

There are altogether eleven courses in zoölogy under the Department of Biology, two of which, namely, "Elementary Biology" and "Cellular Biology" are taught in part from plants.

It does not appear that botany is ignored in this programme of biological courses of study in this institution. The fact that the botanical courses are not arranged under the Biological Department is a mere technicality of administration, which raises no confusion in the minds of students, any more than does the separation of the Department of Physiology, which is equally cognate to biology. The separation of these three departments is simply owing to the fact that botany and physiology were already well established when the trustees decided to found a distinct department in which biology would be taught especially as illustrated in animal types. HENRY F. OSBORN.

Columbia College, New York, April 13.

Cedar Waxwings,

Mr. Edwin M. Hasbrouck's "Presumably new fact relative to the Cedar Waxwings (Amp. Ced.)" in the issue of the 17th ult., is a very interesting discovery. The observations from which his conclusions were obtained, are familiar to modern ornithology, while his inductions are assuredly new to me. Whether they are accepted or not, his views of the importance of carefully studying the *first* plumages of birds will scarcely fail of universal acceptance. I have no criticisms, but wish to add an observation concerning the wax tips of the secondaries and retrices of the species which I am inclined to think will favor his conclusions.

I have made the ultimate anatomical structure of feathers a special study for many years, during which I have given those of the period before the first moulting special consideration, and have met with some extremely interesting things.

I have never been so fortunate as to meet with a wax tip while the young bird was still in the nest, but have occasionally seen them in very fresh subjects, or as early as the 25th of July. The development of the appendage, after it has commenced to appear, is very rapid indeed, resembling the process of the growth of the new antlers of a buck. I cannot yet state definitely the length of time, but from three to five days ordinarily, and doubtless sometimes a little more. In a work devoted to the Birds of Minnesota, I have made some references to my familiarity with the species, to which I might add many more notes, since that went out of my hands, that are even more in point, but suffice to say, the red wax is secreted in the ciliohamular portion of the barbules of the terminal barbs of the feather.

The rapidity of the development of the appendage is such that occasionally it results in doubling the whole series of barbs with their barbules, back upon the rachis of the feather, and reveals the fact that the horny material constituting the wax-like mass is filled from the tip, shaftward, as if in fact, as in appearance, it consists of genuine red sealing-wax, which has become so thickened or condensed as to cease flowing before quite reaching the point of union of the barb with the delicate, overladen rachis The naked portion of those barbs becomes an easy object of observation under low powers of the microscope, and under supremely good light and a higher magnification, the reflected portions of the barb with its barbules, and even the barbicels, may be seen resting upon the unreflected portion of the barbs and rachis. That there is some special condition very temporarily involved, that produces these decorations, there can be no doubt. I have never yet succeeded in seeing a wax-tip on a waxwing reared in captivity, excellent as has been my opportunity. Who next has something new about the Cedar Waxwing?

P. L. HATCH.

An Appeal to Naturalists.

MAY I appeal through your valued columns for the coöperation of the naturalists of the country? The following letter from Professor Kölliker of Würzburg is the occasion of my appeal:—

WÜRZBURG, April 4, 1893.

MY DEAR PROFESSOR MINOT:

May I ask you if you could procure for me some rare American forms of fishes and amphibians, preserved in Müller's fluid, so as to be investigated microscopically after Golgi's and Weigert's method? Larger animals should be cut transversely, so that the fluid can enter the spinal canal and act upon the spinal marrow. At the same time the head or body should be opened and the brain acted upon.

The list of my wishes is very large, but I shall be very glad, if I get only some of the animals mentioned. It includes, among the amphibia and reptiles, Amphiuma, Siren, Menobranchus, Menopoma, full-grown and larval, young alligators and tortoises; among the fishes, Lepidosteps, Amia, Spatularia, Scaphyrhynchus, full-grown and also very young. . . . I am working just now at the microscopic anatomy of the nervous system, and have begun to extend my investigations to the comparative part also. Unfortunately, specimens in spirit only are worth very little, and the only good methods are those of Golgi and Weigert. But even Golgi's is only useful on embryos and young animals, and you know that both these methods demand a previous preservation in Müller's fluid.

Believe me, etc.,

A. KÖLLIKER.

In view of Professor Kölliker's distinguished services to science, covering a period of over fifty years, and of his undiminished activity in research, every one must feel a wish to promote any investigation Professor Kölliker undertakes. In order to secure the material for which Professor Kölliker asks, I seek for contributions from my American colleagues. I request that all specimens may be sent to me at the Harvard Medical School, so as to be in my hands by May 30. All material thus obtained can be packed and forwarded to Professor Kölliker, together with the list of contributors.

The specimens should be kept in the Müller's fluid until they reach Würzburg. In order to secure a good result with the fluid, it must be used in large quantities, and should be changed every day for the first week, and twice during the second week. Müller's fluid will not penetrate hard tissues, such as bone, for more than a quarter of an inch, and soft tissues for more than three-fourths of an inch. On this account the pieces to be preserved must not be too large.

The best formula for Müller's fluid known to me is:—

Bichromate of potassium	2 per cent
Sulphate of sodium	2 " "
Water	96 " "

In practice it is convenient and sufficiently exact to dissolve two grammes of each salt in 1,000 cubic centimetres of water.

It will be, I am sure, a pleasure to many naturalists in America to learn of an opportunity of rendering a service to Professor Kölliker, to whom we all owe so much, and whose continued activity is perhaps the most remarkable instance of prolonged and fully sustained mental power in the whole history of biological science. We must all feel confident that any material placed at his disposal will be the means of securing important additions to knowledge. CHARLES SEDGWICK MINOT.

Harvard Medical School, Boston, Mass.

BOOK-REVIEWS.

The Meaning and the Method of Life: A Search for Religion in Biology. By GEORGE M. GOULD. New York, G. P. Putnam's Sons. $1.75.

THIS book is a result of the unsettled and transitional state of religious opinion. Feeling deeply the want of some religion, but dissatisfied with the religions of the past, Dr. Gould has sought in the phenomena revealed by his favorite science of biology the basis of a new theology and a new religion. His views are somewhat singular. He holds that matter is eternal and independent of God, who is the author of life and mind only, using matter as the material of the living bodies that he forms, but having otherwise no control over it. Hence God is a limited and conditioned being, and, though very wise and perfectly good, is very far from omnipotent. This theory is somewhat like one that had some prevalence in ancient times, which also regarded matter as eternal and the Creator as merely the workman who fashioned it; yet the doctrine of this book limits Him still more, since it confines Him strictly to the world of life, excluding Him entirely from the vastly larger field of inorganic matter. In this way Dr. Gould thinks that he accounts for the existence of evil, which is due to the limited power of the Creator, whose goodness is thus saved at the expense of his omnipotence. Every living thing is an incarnation of Divinity, and man especially so. Man's duty consists in promoting the growth and fulness of life everywhere, and especially the spiritualization of human life. On the question of immortality, Dr. Gould expresses no decided opinion, holding that God has not seen fit to reveal his design with regard to man's future, and believing that information concerning it would be of no use to us here if we had it.

Such is Dr. Gould's religion; but, though it may find some favor among other biologists, we doubt if it wins acceptance anywhere, for religions and philosophies that deny the Divine omnipotence have never proved congenial to the human mind, and never will. His theory of the universe and its Author is evidently due to a too exclusive study of one science to the neglect of other and wider views, a mode of investigation peculiarly dangerous in theology. But whatever may be thought of his positive doctrines, all true souls will symphathize with the sentiment expressed in his introductory chapter, that "the bravest, noblest attitude is that of unsatisfied longing, and the never stilled faith that light will come into all of our darkness, and that the riddle of our lives will be solved."

Beiträge zur Kenntniss der Baues und Lebens der Flechten. II. Die Syntrophie. VON DR. ARTHES MINKS. WIEN, 1893.

DR. MINKS of Stettin, Prussia, is, or should be, known to all who are interested in the Lichens, and the controversy with regard to them, as one of the strongest advocates of their autonomy, on grounds peculiarly his own. In various publications he has announced the result of arduous and long-continued investigations, which are at least worthy of serious consideration. They cannot be ignored, as is the fashion among those who adopt the ideas of the new school.

CALENDAR OF SOCIETIES.

Biological Society, Washington.

Apr. 22.—O. F. Cook, Notes on the Natural History of Liberia; J. N. Rose, Two New Trees of Economic Importance from Mexico; V. A. Moore, Observations on the Distribution and Specific Characters of the Streptococci Group of Bacteria; Erwin F. Smith, Peach Yellows and Plant Nutrition.

Geological Society, Washington.

Apr. 26.—The first half hour will be devoted to continuing the discussion concerning the Age of the Earth. Bailey Willis, Interpretation of Sedimentary Rocks; M. R. Campbell, The Influence of Post-Paleozoic Deformation on the Drainage of the Central Appalachians.

Academy of Sciences, Biological Section, New York.

Apr. 10.—H. F. Osborn, on "The Evolution of Teeth in Mammalia in Its Bearing upon the Problem of Phylogeny," reviewed the recent researches and theories of Kükenthal, Röse, and Tacker upon the formation and succession of the dental series in mammamalia, and pointed out that, especially in marsupials, cetaceans, and edentates (with other placentates), the existence of two series of teeth was now abundantly proven, as well as the fact that Homodynamous forms were derived from early Heterodont. He then showed that recent discoveries demonstrated that in marsupials teeth of the second series might be interposed in the first series—to explain the typical dentition of such forms of Didelphys. This transposition enables a comparison of dentition of marsupial with that of turassic mammalia $\left(= i, \frac{4}{4}, c, \frac{1}{1}, p, \frac{4}{4}, m, \frac{8}{8}\right)$. It was further noted that the triconodont type (as Amphilestes) was probably the hypothetical point of divergence of placental mammalia. As to the form of crowns, the theory (Kükenthal–Röse) that complex mammalian types were made by concresence of simple reptilian cusps was upon the evidence of the turassic mammalia shown untenable, as well as the converse theory that cetaceans have derived homodynamous form by the splitting of the cusps of triconodont. Bashford Dean, in "Contributions to the Anatomy of Dinichthys," correlated the parts of this Devon-Lower Carboniferous Arthrodiran to those of Coccos tens. Notes were made upon the (1) disposition and character of the lateral line organs, (2) pineal foramen, (3) nasal capsules, (4) dentary plates (homologies), (5) ginglymoid articulation of lateral shoulder plates, (6) character of shagreen, (7) probable disposition of paired and unpaired fins. N. L. Britton presented a "Note on the Genus Lechea." This genus of Cistineæ is entirely American, and, from the investigations of Mr. Wm. H. Leggett and Dr. Britton, appears to consist of about fourteen species.

Agassiz Scientific Society, Corvallis, Ore.

Apr. 13.—C. D. Thompson, Relation of Soils to Plant Growth.

Mar. 8.—Professor John M. Bloss, The Early Lives of Some of Our Scientists.

In the work now before us, Dr. Minks considers the question of the so-called parasitic Lichens. In 1880, in "Morphologisch-lichenographische Studien," II., he had said that "Lichen and parasite are two irreconcilable conceptions." In the present work he develops this idea and extends it to a considerable number of Lichens, the apothecia of which had been previously considered to belong to the thallus on which they are found, and applies the term "syntrophy" to all such. In a syntropic lichen, by careful microscopic investigation, the epiphytic apothecium is found to have a very delicate thalline tissue of its own, from which it derives sustenance independently of the foreign thallus on which it grows. Dr. Minks describes this relation in language almost as picturesque as that used by Schwendener, in a phrase which has become classic. The syntrophic apothecia, he says, "are guests, because they offer nothing to the host, but claim services from him without compensation. But they are not boarders, else they might properly be termed parasites, but only lodgers. They are tenants, who pay no rent, but share the lot of the landlord."

In application of this doctrine, Dr. Minks considers a number of genera and species of Lichens, which, from his point of view, are syntrophic. Prominent among these is the genus Pyxine, the apothecia of which are syntropic on species of Physicia. Others are the Caliciacei and the Gyalectacei, the latter being elevated to the dignity of a tribe, while the author follows Nylander in combining the Lecanorei and the Lecideei in one tribe, the Lecano-Lecideei.

It were much to be desired that some of our younger botanists and microscopists, instead of consuming time in tedious and often superficial attempts to determine species, and of accepting as a dogma the Schwendener theory, neglecting to study what has been said on both sides of the controversy, would make themselves familiar with the copious literature of the last few years, and apply themselves to the study of the morphology and physiology of the Lichens, which, from whatever point of view they are considered, are among the most remarkable products of the vegetable kingdom. They might be able, by patient labor and by not being in too much haste to arrive at conclusions, to make valuable contributions to the vexed controversy. W.

The Story of the Atlantic Telegraph. By HENRY M. FIELD. New York, Chas. Scribner's Sons.

THE story of difficulties overcome in the endeavor to accomplish a great work is always interesting, and the account here given of the laying of the Atlantic cable reads like a romance. As is natural, since the writer is a brother of his, the work of Mr. Cyrus W. Field is given the most prominence, but we notice with pleasure that the indomitable perseverance and courage of the financiers engaged in the undertaking is recognized as it should be.

The Voltaic Cell. By PARK BENJAMIN, LL.B., Ph.D. New York, John Wiley & Sons.

DR. PARK BENJAMIN has had long experience in collecting the material for encyclopedic treatises, and the reader of this book may be sure that nearly all that could be collected on the subject has been incorporated here. This, however, does not necessarily constitute a good book, and in the present case the material used in cementing together the vast quantity of contained information might have been improved upon had a little more care been spent on this part of the work. For instance, while the list of cuts of storage cells is a very complete one, the theory of the changes which go on during the charge and discharge is hardly touched upon. The book contains a large number of valuable tables of conductivities of solutions, heats of combination, etc. R. A. F.

THE Egypt Exploration Fund's new circular respecting the archæological survey of Egypt may be obtained from Dr. W. C. Winslow, 525 Beacon Street Boston.

Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

For sale or exchange—A Telescope (36 diameters, copper barrel)—for $20 cash or scientific books of that value. A. N. Somers, La Porte, Ind.

For sale—A complete set of the Reports of the Second Geological Survey of Pa., 1874-1893, including the Grand Atlas. Publisher's price over $115. Will sell for $50. Address F. D. Chester, Newark, Del.

The undersigned has skins of Pennsylvania and New Jersey birds, as well as other natural history specimens: which he wishes to exchange for marine, fresh water, and earthworms of the South and West. Correspondence with collectors desired. J. Percy Moore, School of Biology, University of Pennsylvania, Philadelphia.

For sale or exchange.—I have a Caligraph typewriter (No. 2) in perfect order and nearly new. It is in a heavy leather, plush-lined office case, the whole costing me about $100. I desire to obtain for it, either by sale or exchange, a new, No. 5 "Kodak" camera, with six double feather-weight plate-holders and the latest pattern of their tripod. The lens and pneumatic time-shutter must also be the same as those now sold with the last No. 5 Kodak. The price of what I desire in exchange is $78. Address. for particulars, P. O. Box 314, Takoma, District of Columbia.

For sale.—An Abbe binocular eye-piece for the microscope. Alfred C. Stokes, 527 Monmouth St., Trenton, N. J.

For sale or exchange.—One good long range Remington B. L. rifle, 44 calibre, also land and fresh water, and marine shells. Want shells, Safety, camera or printing press. A. H. Boies, Hudson, Mich.

Fine collection of microscopic slides for sale, or would exchange for first-class pneumatic bicycle. J. E. Whitney, Box 549, Rochester, N. Y.

For sale—A Zentmayer new model U. S. Army Hospital monocular stand. Price $110, will sell for $75. Address H. C. Wells, No. 151 Broadway, New York.

For sale—A complete set of the third series of the American Journal of Science (1870-1893) handsomely bound in single volumes in dark brown half morocco. Address G. H. Williams, 803 Cathedral Street, Baltimore, Md.

Wants.

WANTED, as principal of a flourishing technical school, a gentleman of education and experience who will be capable of supervising both mechanical and common school instruction. Special familiarity with some technical branch desirable. Address, giving age, qualifications, etc., J. B. Bloomingdale, Fifty-ninth street and Third avenue, N. Y.

THE undersigned desires specimens of North American Gallinae in the flesh for the study of their pterylosis. These species are especially desired: *Colinus ridgwayi, cyrtonyx montezumae, deudragapus franklini, lagopus welchi, tympanuchus cupido* and *pedioecetes phasianellus.* Any persons having alcoholic specimens which they are willing to loan or who can obtain specimens of any of the above are requested to communicate with Hubert Lyman Clark, 3922 Fifth Avenue, Pittsburgh, Pa.

A COMPETENT TEACHER of botany in college or university is open to engagement. Address L., Box 86, Rochester, Mich.

CAN any one inform me as to the age to which cats have lived? I have one twenty years old. Edward D. Webb, 132 W. Eighty-first St., New York.

WANTED — Second-hand. Foster's Physiology, Balfour's Comparative Embryology, Claus & Sedgwick's Zoology, Flower's Osteology of Mammalia, Vine's Physiology of Plants. Please state editions and prices asked and address Richard Lees Brampton, Ontario, Canada

WANTED.—American Journal of Conchology, seven volumes. Parties having these for sale will please address the undersigned, stating condition and price. R. Ellsworth Call, Louisville, Ky.

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WANTED.—By well-qualified and experienced science master and associate of the Royal School of Mines, London, aged 26 (at present in England), a mastership in technical college or university for any of the following subjects: Engineering sciences, geology and mineralogy, physics, chemistry and metallurgy, etc., etc. Can provide excellent references and credentials. Apply, J. G., 17 Sussex St., Rochdale, England.

SCIENCE

ELEVENTH YEAR.
VOL. XXI. No. 535.

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SINGLE COPIES, TEN CENTS.
$3.50 PER YEAR, IN ADVANCE.

CONTENTS.

Entered at the Post-Office of New York, N.Y., as Second-Class Mail Matter.

NEW METHOD OF PROTECTING BUILDINGS FROM LIGHTNING.

SPARE THE ROD AND SPOIL THE HOUSE!

Lightning Destroys. Shall it be Your House or a Pound of Copper?

PROTECTION FROM LIGHTNING.

What is the Problem?

In seeking a means of protection from lightning-discharges, we have in view two objects,—the one the prevention of damage to buildings, and the other the prevention of injury to life. In order to destroy a building in whole or in part, it is necessary that work should be done; that is, as physicists express it, energy is required. Just before the lightning-discharge takes place, the energy capable of doing the damage which we seek to prevent exists in the column of air extending from the cloud to the earth in some form that makes it capable of appearing as what we call electricity. We will therefore call it electrical energy. What this electrical energy is, it is not necessary for us to consider in this place; but that it exists there can be no doubt, as it manifests itself in the destruction of buildings. The problem that we have to deal with, therefore, is the conversion of this energy into some other form, and the accomplishment of this in such a way as shall result in the least injury to property and life.

Why Have the Old Rods Failed?

When lightning-rods were first proposed, the science of energetics was entirely undeveloped; that is to say, in the middle of the last century scientific men had not come to recognize the fact that the different forms of energy—heat, electricity, mechanical power, etc.—were convertible one into the other, and that each could produce just so much of each of the other forms, and no more. The doctrine of the conservation and correlation of energy was first clearly worked out in the early part of this century. There were, however, some facts known in regard to electricity a hundred and forty years ago; and among these were the attracting power of points for an electric spark, and the conducting power of metals. Lightning-rods were therefore introduced with the idea that the electricity existing in the lightning-discharge could be conveyed around the building which it was proposed to protect, and that the building would thus be saved.

The question as to dissipation of the energy involved was entirely ignored, naturally; and from that time to this, in spite of the best endeavors of those interested, lightning-rods constructed in accordance with Franklin's principle have not furnished satisfactory protection. The reason for this is apparent when it is considered that the electrical energy existing in the atmosphere before the discharge, or, more exactly, in the column of dielectric from the cloud to the earth, above referred to, reaches its maximum value on the surface of the conductors that chance to be within the column of dielectric; so that the greatest display of energy will be on the surface of the very lightning-rods that were meant to protect, and damage results, as so often proves to be the case.

It will be understood, of course, that this display of energy on the surface of the old lightning-rods is aided by their being more or less insulated from the earth, but in any event the very existence of such a mass of metal as an old lightning-rod can only tend to produce a disastrous dissipation of electrical energy upon its surface,—"to draw the lightning," as it is so commonly put.

Is there a Better Means of Protection?

Having cleared our minds, therefore, of any idea of conducting electricity, and keeping clearly in view the fact that in providing protection against lightning we must furnish some means by which the electrical energy may be harmlessly dissipated, the question arises, "Can an improved form be given to the rod, so that it shall aid in this dissipation?"

As the electrical energy involved manifests itself on the surface of conductors, the improved rod should be metallic; but, instead of making a large rod suppose that we make it comparatively small in size, so that the total amount of metal running from the top of the house to some point a little below the foundations shall not exceed one pound. Suppose, again, that we introduce numerous insulating joints in this rod. We shall then have a rod that experience shows will be readily destroyed—will be readily dissipated—when a discharge takes place; and it will be evident, that, so far as the electrical energy is consumed in doing this, there will be the less to do other damage.

The only point that remains to be proved as to the utility of such a rod is to show that the dissipation of such a conductor does not tend to injure other bodies in its immediate vicinity. On this point I can only say that I have found no case where such a conductor (for instance, a bell wire) has been dissipated, even if resting against a plastered wall, where there has been any material damage done to surrounding objects.

Of course, it is readily understood that such an explosion cannot take place in a confined space without the rupture of the walls (the wire cannot be boarded over); but in every case that I have found recorded this dissipation takes place just as gunpowder burns when spread on a board. The objects against which the conductor rests may be stained, but they are not shattered.

I would therefore make clear this distinction between the action of electrical energy when dissipated on the surface of a large conductor and when dissipated on the surface of a comparatively small or easily dissipated conductor. When dissipated on the surface of a large conductor,—a conductor so strong as to resist the explosive effect,—damage results to objects around. When dissipated on the surface of a small conductor, the conductor goes, but the other objects around are saved

A Typical Case of the Action of a Small Conductor.

Franklin, in a letter to Collinson read before the London Royal Society, Dec. 18, 1755, describing the partial destruction by lightning of a church-tower at Newbury, Mass., wrote, "Near the bell was fixed an iron hammer to strike the hours; and from the tail of the hammer a wire went down through a small gimlet-hole in the floor that the bell stood upon, and through a second floor in like manner; then horizontally under and near the plastered ceiling of that second floor, till it came near a plastered wall; then down by the side of that wall to a clock, which stood about twenty feet below the bell. The wire was not bigger than a common knitting needle. The spire was split all to pieces by the lightning, and the parts flung in all directions over the square in which the church stood, so that nothing remained above the bell. The lightning passed between the hammer and the clock in the above-mentioned wire, without hurting either of the floors, or having any effect upon them (except making the gimlet-holes, through which the wire passed, a little bigger), and without hurting the plastered wall, or any part of the building, so far as the aforesaid wire and the pendulum-wire of the clock extended; which latter wire was about the thickness of a goose-quill. From the end of the pendulum, down quite to the ground, the building was exceedingly rent and damaged. . . . No part of the aforementioned long, small wire, between the clock and the hammer, could be found, except about two inches that hung to the tail of the hammer, and about as much that was fastened to the clock; the rest being exploded, and its particles dissipated in smoke and air, as gunpowder is by common fire, and had only left a black smutty track on the plastering, three or four inches broad, darkest in the middle, and fainter towards the edges, all along the ceiling, under which it passed, and down the wall."

One hundred feet of the Hodges Patent Lightning Dispeller (made under patents of N. D. C. Hodges, Editor of *Science*) will be mailed, postpaid, to any address, on receipt of five dollars ($5).

Correspondence solicited. Agents wanted.

AMERICAN LIGHTNING PROTECTION CO.,
874 Broadway, New York City.

SCIENCE

NEW YORK, MAY 5, 1893.

THE SO-CALLED "SAP" OF TREES AND ITS MOVEMENTS.[1]

BY CHARLES R. BARNES, PROFESSOR OF BOTANY, UNIVERSITY OF WISCONSIN, MADISON, WIS.

THE subject which I have chosen to present to you this evening is not chosen so much on account of the information which I am able to impart as for the purpose of correcting a great deal of misinformation which is widely prevalent. Many false ideas as to the nature and movements of what is popularly known as the sap of trees are extant, and in a large number of cases these ideas are founded upon mistaken notions of the physiology of plants. Our own knowledge about many of these matters is yet exceedingly imperfect, and it is for that reason that many of my statements will of necessity be negative. The subject also is one which must have considerable interest for those who are so intimately engaged in cultivating fruit and shade trees as are the members of this society; and I take it that no fact in regard to the life and mode of working of the plants with which we are so constantly dealing will be entirely without interest.

What is Meant by "Sap?"

It will be necessary for us at the outset to gain some accurate idea, if possible, of what is meant by the word "sap." If we think for a moment of its various uses, we shall see that it is a word which designates not a fluid of definite composition, but one under which is included a great variety of watery solutions. The sugar-maker begins even before the snows have left the ground to collect from wounds in the trunk of the maple-trees a sweetish liquid which he calls "sap." After a considerable time the proportion of sugar which this liquid contains diminishes very greatly, and he then abandons his work because, as he says, the "sap" has become too poor. The man who has postponed pruning his grape-vines or trees to too late in the season finds that from the cut surfaces a watery substance is trickling which he calls "sap." But the sugar-maker will be unable to obtain either sugar or syrup from this fluid, which is, however, called by the same name as that from which he manufactures his sweets. When a boy, who is making a whistle, hammers the bark of the twig in the spring, he finds it easy to separate the bark, because, as he says, the surface of the wood is then slippery with "sap." The sap of the boy is widely different from the sap of the pruner and the sap of the sugar-maker.

Again, what we do not call sap may furnish us with some illustrations of the diversity of meanings of this term. We do not ordinarily speak of the "sap" of the apple, or of the "sap" of the grape, or of the "sap" of the orange, but call the fluids which these fruits contain "juice." And yet they are not more different in their composition from those fluids which we do call sap, than the three examples already mentioned are different from each other. We might therefore, in all reason, apply this word sap to the juices of fruits.

We popularly distinguish the older hard internal wood of the tree under the name of "heart wood," from the younger, softer, and lighter-colored external wood, which we call the "sap wood." To the fluids which saturate the sap wood we are constantly in the habit of applying the word "sap," but I have never heard it applied to the exactly similar fluids which saturate the heart wood. As far as the composition of these fluids is concerned, there is no reason why that in the heart wood should not equally well be designated as sap.

[1] An address delivered before the State Horticultural Society of Wisconsin, February, 1893; stenographically reported, and revised by the author.

What then are we to understand by the word "sap?" Evidently not a substance of any definite composition; but the word signifies only in the most general way the various watery fluids which are found in the plant. There is no reason indeed why these solutions should not be called *water*, for in many cases they are almost as pure as the water which we drink. In the chemist's sense, the water which we draw from our wells is a watery solution of various substances, and yet we do not designate it commonly by any other term than simply "water." In a similar manner, it is quite proper for us, and perhaps it would conduce to clearness of ideas, to designate the watery solutions in plants simply by the term "water," understanding it in its popular and not in its strictly chemical sense.

Movements of Water in Trees.

Let us turn now to the consideration of the movements which the water in trees exhibits. I shall confine my remarks to trees simply for the reason that they present the greatest variety of water movement, and at the same time furnish the greatest difficulties in the explanation of these movements. If, therefore, we understand the movement of water in trees, we shall be able readily to transfer these ideas to the movement of water in the smaller plants, although the statements applicable to the trees are not always applicable to the smaller plants, because of their greater simplicity; however, the greater includes the less.

The Evaporation Stream.

In the first place, there is need of a very considerable amount of water to supply the constant evaporation which is going on from the leaves of trees. Immense areas of delicate tissue are exposed to the dry air, and oftentimes to the hot sun, in the form of foliage, and from this foliage there is going off at such times large quantities of water in the form of vapor. The water needed to supply this evaporation must come from the soil, because it is not possible for the leaves to take in any water, not even when they are wet by the rains or by the dews. The water enters, not at the base of the trunk where the large roots are found, but only at the extremities of the finest rootlets. At these points the rootlets are clothed with a "nap" or "pile" of fine hairs. These root-hairs must not be confounded with the fine branches of the root, for it is only the finest branches which are covered with the close-set hairs. Consequently, it is only the youngest and most delicate parts of the root which allow the entrance of the water. But the water escapes from the leaves, and from the point of entrance to the point of exit is a far cry for the coursing droplets. How does it pass through this long space?

It is just here that our knowledge is most defective. We know a number of things that are true in regard to it, and we know a number of things which are not true in regard to it.

We know that it moves in the sap wood of the tree, and neither in the bark or in the heart wood. Many of you must have made observations which are sufficient to establish this point. You have, for instance, observed that the bark of trees might be peeled off for a considerable distance, and that the leaves would still retain their green color and their freshness. In many cases, indeed, the mere removal of the bark from the tree is not sufficient to bring about its death until several months, and in some trees not until several years, after the injury. Death, however, is inevitable sooner or later; but the fact that the leaves remain fresh for so long a time is evidence that the supply of water is not interfered with. Death ensues from a totally different cause, namely, from the starvation of the roots in a way which will be explained later.

Again, you must have observed that it is quite possible to have the entire heart wood of the tree removed, as is often done by decay and yet to have the leaves remain fresh and green for an indefinite time. In fact, the rotting-out of the heart wood scarcely interferes with the vitality of the tree, except as it renders it mechanically weaker, and consequently more liable to be overthrown by storms. If any further proof were needed, it is perfectly possible to show experimentally that the sap wood alone is engaged in the transfer of the water required for evaporation by cutting into it. A saw-cut which passes through the sap wood, but leaves the heart wood intact, brings about within a very short time the withering of the leaves. In some trees, indeed, a cut which severs only the outer youngest layers of the sap wood will produce the same effect, since in such trees *only* the youngest layers of the wood carry the water. By experiments on twigs it can be demonstrated that withering will occur even if the bark is almost completely uninjured.

We know the water to supply evaporation moves chiefly in the cavities of the elements of the wood. The wood of the tree is composed of a large number of *fibres*, that is, elongated cells pointed at both ends, and of *ducts*, that is, tubes of great length formed by the breaking together of rows of cells placed end to end. You can get an idea of the manner in which these ducts are formed by imagining a series of round pasteboard boxes piled one on top of another, after which the top and bottom of each is removed, so that, instead of a series of separate chambers, we have now a long tube. The fibres may be likened to a series of lead pencils, sharpened at each end, and placed in contact with each other, the points of the lower ones overlapping the next ones above and fitting in between them. In my illustration the cavity of the fibre would be represented by the lead, and it would be more accurate if we could conceive of the cavity as not extending entirely through the pencil, but stopping short of the point. Minute pits extend from the cavity of one of these fibres to the other, and the walls also of the long ducts are also marked by larger thin spots. It is in the cavities of these ducts and fibres that the water chiefly travels.

We do not know what part is taken in this ascent of the water by those peculiar elements of the wood which you know by the name of silver grain or the pith rays. You will remember these as the shining plates of tissue which extend from the centre of the wood toward the circumference. They are particularly prominent in the oak and show most when it is split "with the grain." It is probable that these cells have a great deal to do with the movement of water, but their exact *rôle* is not fully agreed upon.

We are in almost total ignorance at the present time as to the force by which the water is elevated through so many feet. There are trees in the gullies of Victoria, Australia, whose height exceeds 470 feet, and we must invoke some force which is able to raise water from the level of the soil to the level of the highest leaf. A year ago we thought we had a hypothesis which would account for this movement, but later researches have brought to light some facts which are at present totally irreconcilable with what was a most charming, and, at that time, a most satisfactory explanation, and we shall be obliged to abandon it unless the wine of the new knowledge can be held by the old bottles of theory.

At the time when our knowledge of capillarity was greatly extended by the celebrated researches of Jamin, it was thought that we had knowledge of a force adequate to account for the raising of water to these great heights. The fibres and ducts which I have described to you seemed to answer very perfectly the requirements of capillary attraction, and it was thought that this force, by reason of which water rises through narrow spaces, was the one sought. But the rise of water in capillary spaces is proportioned to the size of the opening; the smaller the opening, the higher will it rise. With the decrease of the calibre of the tubes, however, the friction increases enormously, and only small quantities will be able to be moved on account of the diminished size of the tubes. It was quickly seen that, in order to account for a rise of even a hundred feet, the tubes of the wood must be vastly smaller than they really are.

When it was found that the air in a plant is under a less pressure than that outside the plant, it was thought that the force had been discovered, and that atmospheric pressure furnished the explanation. Negative pressure, however, on the interior never reaches zero, and consequently cannot account for a rise of more than 33 feet.

Again, what was called root-pressure was invoked to explain the phenomena. It is found that water is absorbed at certain times so rapidly by the roots that it exists in the plant under considerable pressure, and it has been claimed that root-pressure, combined with the other forces already known, was adequate to account for the rise of water. But this, too, has failed us.

It is perhaps the greatest weakness of the last theory (that of Godlewski), which we have just had to abandon temporarily at least, that it depended for its explanation upon the indefinite and illusive "vitality" of certain portions of the plant. Godlewski's brilliant hypothesis, which ascribes to the activity of the living cells of the medullary rays the function of receiving from lower levels the water and passing it on to higher tissues through rhythmic variations in their osmotic power, due possibly to respiratory changes, may yet hold the clue which we are seeking. But when Strasburger jacketed a young tree for a distance of 35 feet, and kept it surrounded by hot water until all of the living cells in the tree trunk were unquestionably killed, and when under these circumstances the water-supply to the leaves was not interfered with, so that they remained green and fresh, we were obliged to conclude that the lifting of the water is not dependent upon the life of the tissues directly, but that it is evidently carried on by a physical process yet to be explained.

Before passing from this topic of the movement of water which supplies evaporation, I must allude to a very common and widespread idea,—at least I judge it to be widespread, because it is so frequently propounded by my students,—that "the sap goes down in winter and up in the spring." Just where the sap is supposed to go in winter is not exactly clear; since, if the roots are absorbing water in the fall when the evaporation is diminished, they are likely to have quite as much water as they can hold already. The conception, apparently, is that all of the water lodged in the trunk and spreading branches goes downward into these roots. It needs, however, only the most casual examination of trees in winter to discover that at this time they are almost saturated with water. The twigs of the hickory tree, for example, will be frozen on a cold day in winter, so that they are as brittle almost as glass, and one can snap off a twig half an inch in diameter as though it were an icicle. The same twig, when not frozen, on a mild day will be so tough that there will be no possibility of breaking it.

Again, if one cuts off a branch from a tree in winter and brings it into a warm room, he will quickly discover that water is oozing from the cut end, showing that the twigs are almost saturated with it. As a matter of fact, the water in trees increases from mid-summer or early fall to the beginning of growth in early spring. There is thus no necessity for any "going up" of the sap in spring until the leaves are expanded and the water with which the tree is already saturated begins to be evaporated from the foliage.

Bleeding.

A second movement of water in trees is that which occurs in the so-called "bleeding." The bleeding of trees occurs at different times of the year, either before growth has begun at all, or just as it is beginning. In the two cases the cause is quite different. We find a good example of both sorts of bleeding in the gathering of the sap by the sugar-maker. This gathering begins at the time when the ground is still frozen and the roots are almost or quite unable to absorb any water, but at a time when the air is warmed through the middle of the day by the increased heat of the sun. At first the expulsion of water from wounds made in the trunk is due to the expansion by heat of the air inside the smaller branches and twigs of the tree. This sets up at once a pressure upon the water, and this pressure is transmitted to all parts of the tree. The water with which the tree is filled is thereby forced out as soon as an opening is made for its escape. Later in the season, however, the roots begin their work of absorption, and there is then set up the so-called *root-pressure*, by reason of which the water is forced out at the same openings.

The latter sort of bleeding is necessarily delayed until growth is about to begin, and is checked as soon as the foliage is sufficiently expanded to begin evaporation.

A bleeding similar to the last takes place at the hood-like tips of grass leaves, where the skin is nearly always ruptured. The little drops of water which accumulate here are commonly mistaken for dew, but are merely droplets exuded from the interior of the leaf, because the falling temperature of the air toward evening has diminished the evaporation from the leaves, while the roots in the warm soil are still absorbing water, and consequently producing an internal pressure. The movement of water in these cases of bleeding, it will be seen, is necessarily toward the point of exit, which may be above or below the point at which the pressure arises.

Secretion of Nectar.

A third sort of movement of water is that which takes place in the nectaries of flowers and leaves. The flowers of our common linden, for example, secrete a considerable quantity of sweet fluid, which is sometimes miscalled "honey," but is properly known as nectar. Honey, by the way, is nectar after it has been digested by the bees. At certain points in the flower there are groups of cells whose special business it is to withdraw water from the parts below, and filter it through their outer walls, after having added to it the materials which make it sweet. The movement of water in this case is extremely limited.

The Transfer of Food.

The last movement of water of which I shall speak is of those solutions which contain the food of the plant. These materials are not those absorbed from the soil, or gathered directly from the air, but they are the substances which have been manufactured by the leaves out of the materials obtained from the soil and from the air. Since these foods are put together in the leaves, necessarily the movement of water containing them in solution must be in a different direction from that which supplies the evaporation. The materials thus manufactured in the leaves must be carried either to those parts which are growing or to those places in which they are to be stored for future use. It is manifest at the first glance, therefore, that the direction of the movement must be in general *inwards* from the leaves, and, since the roots require for their nutrition a considerable amount of these substances, there must be a very decided *downward* movement to supply them.

Now it is plain that these solutions of food must keep out of the way of those portions of the water which are chiefly to supply the evaporation from the leaves. We have seen that the latter travel in the sap wood. The food currents, however, travel almost exclusively in the inner parts of the bark. You will therefore understand why stripping off the bark, or even cutting it, ensures the death of the tree eventually, even though the leaves remain long unwithered, since the roots depend upon the food formed by the leaves, they perish when severed from their base of supplies.

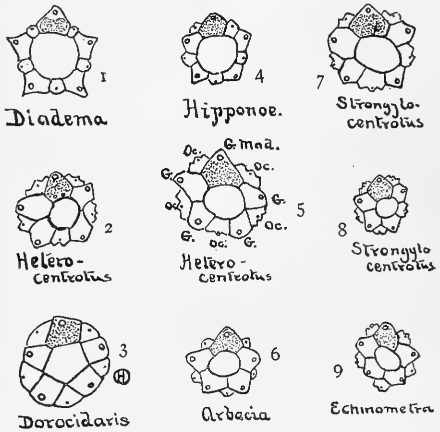
The movement of the evaporation stream is relatively rapid. The movement of this food current is relatively slow. We do know something of the mode of movement of these food currents. They are apparently brought about through the process known as diffusion, or osmosis, and are therefore necessarily slow. The cause of the movement is practically the same as that for the movement of oil in the lamp-wick, although it is by no means by the same method. The oil in the lamp-wick travels upward because at the top it is being destroyed *as oil* by reason of the heat of the flame. So the direction and existence of the current of water carrying food is because the various substances dissolved in the water are being altered at the place of growth or storage into new materials. The commonest of these food substances is sugar, and at the growing point of the stem, for example, the sugar is being constantly destroyed as sugar and is being converted into cellulose or protoplasm or some other material. So long as that alteration is going on, just so long will the sugar particles move toward that point.

But I must not impose further upon your patience. I have tried to sketch very briefly, and only in outline, the different movements which the water in the plant is undergoing. I have said nothing of the extreme variety of materials which may be found in this water in different plants, or even the variety found in the same plant at different times, but have endeavored merely to show you that there is going on constantly in the living tree a series of molecular and mass movements, of which too few people have any conception. To our imperfect knowledge let me hope that some of you may contribute facts which shall enable us some day to explain the many things which are now obscure.

NOTE ON THE SEA-URCHIN SKELETONS.

BY HENRY LESLIE OSBORN, PH.D., ST. PAUL, MINN.

IN looking through the drawings of students in the freshman class of the aboral ring in *Strongylocentrotus droebachiensis* from Portland, Maine, I came upon one which I at first supposed to be erroneous, but which, on comparison with the specimen, proved the specimen to be exceptional. The usual arrangement of the genital and ocular plates in this species is shown in Fig. 7 of our cut. The five genital plates and two of the oculars border upon the anal ring, the three remaining oculars being shut out by



the contiguity of the enlarged genitals. The arrangement found by exception is figured at 8. It consists in the exclusion of four oculars from the aboral ring, so that only one gets a share in forming the border. I seized the occasion to look into the cases of about fifty specimens which happened to be on hand in the laboratory, and found that the case of Fig. 8 occurred twice in that series and that all others were like Fig. 7, which is normal, and which I have observed in many more than fifty different specimens at different times. It is interesting to note that in Fig. 122 of Agassiz's "Seaside Studies," page 103, a drawing of a specimen of this species is given, in which three ocular plates border the aboral ring, and in which the plates are thus quite symmetrical. This must be of very exceptional occurrence, for I have never met it in the many specimens I have seen. I should be very glad to know if it has been at all generally observed.

In connection with the case of Strongylocentrotus, it is interesting to examine the aboral ring of other regular echinoids. In *Diadema* (Fig. 1) the ring is perfectly regular, with five genitals and five oculars of equal size; in *Arbacia* (Fig. 6) it is equally regular, but with five large genitals, which form a ring about the aboral area, and exclude from it wholly the five small genitals. In *Dorocidaris* (Fig. 3) the ring is nearly regular, four oculars barely reaching the ring, the fifth being shut out. In *Hipponoe* (Fig. 4) the case is nearly as in *Diadema*, one ocular, however, not reaching the ring. In *Echinometra* (Fig. 9), a very elongate urchin, but not elongate in the plane of the madreporic plate, the five oculars do not any of them reach the ring and the ring is very asymmetrical, being elongate in the major axis of the elliptical specimen. In *Heterocentrotus*, too (Figs. 2 and 5), the body is elliptical and the ring is asymmetrical, being longer in the direction of the major axis of the specimen. In this genus (one figured from the Bermudas [5] and one from the Philippine Islands [2]) there is only one ocular which borders the ring, a second barely reaching it in 5 or not really doing so in 2, this is not like the case presented in Fig. 8, the exceptional Strongylocentrotus, for there the lateral and not the median ocular is the one which surely borders the ring. A number of undetermined Echinoids at hand from the Pacific Ocean closely resemble Strongylocentrotus, and in them the aboral ring is like Fig. 7.

These comparisons have interesting morphological suggestions. *Dorocidaris* is a central form, as well as an early one palæozoölogically, in it the oculars are neither wholly on nor wholly excluded from the ring; from it *Diadema* is a departure toward and *Arbacia* from the ring, and all three of these are tolerably radial in symmetry. *Hipponoe* is a slight departure from the regular symmetry of the aboral ring, all the rest are not radial. The elongations of *Echinometra* and *Heterocentrotus* are in the same plane in each case, but the plane is not in the plane of the madreporic plate, as it is in the departures from radial and toward bilateral symmetry in clypeastrids and spatangids. The aboral pole of Strongylocentrotus is very much out of radial symmetry, though the shell in all other respects is very perfectly regularly radial. The meaning of the exceptional case presented in Fig. 8 might perhaps be understood as a reversion toward an ancestral form in which the oculars were all excluded from the aboral ring. I cannot think of any adequate physiological explanation of the relations of these bones in either Fig. 7 or Fig. 8.

Hamline Biological Laboratory, March 24, 1893.

THE CANALS OF MARS.

BY S. E. PEAL, SIBSAGAR, ASAM.

THE question as to the distribution of land and water on the planet Mars, and nature of the so-called "canals," is one on which there has of late been considerable speculation; and in the hope of throwing some little light on the subject, it may not be amiss to draw attention to a recent geological discovery relating to the distribution of land and water on our earth.

At no very distant period it was generally supposed that terrestrial continents and oceans had frequently — or at least occasionally — changed places, that oceanic islands, as a rule, were the summits of submerged or emerging ranges, the last relics, or forerunners, of extensive land masses. All this is now changed, and one of the most recent and important discoveries of modern geology is the fact that the great continental masses and deep ocean floors are permanent features of the earth's crust.

On p. 150, "Island Life," Mr. A. R. Wallace tells us that "there is the strongest cumulative evidence, almost amounting to demonstration, that for all known geological periods our continents and oceans have occupied the same general position they do now." And at p. 330, "during the whole period of geologic time, as indicated by the fossiliferous rocks, our continents and oceans have, speaking broadly, been permanent features of our earth's surface." Referring to ocean floors, Mr. J. Murray again says, "The results of many lines of investigation seem to show that in the abysmal regions we have the most permanent areas of the earth's surface." While M. Faye points out that "under the oceans the globe cools down more rapidly and to a greater depth than beneath the surface of the continents. At a depth of 4,000 metres the ocean will still have a temperature not remote from 0° C., while at a similar depth beneath the earth's crust the temperature would be not far from 150° C."

Last, Professor James Geikie, in his address to Section E, geography, of the British Association, says, "We must admit that the solid crust of the globe has always been subject to distortion, and this being so, we cannot doubt that the general trends of the world's coast-lines must have been modified from time to time by movements of the lithosphere. . . . It seems to be the general opinion that the configuration of the lithosphere is due to the sinking in and crumpling up of the crust on the cooling and contracting nucleus." "According to Professor Winchell the trends (of the great world ridges and troughs) may have been the result of primitive tidal action. He was of opinion that the transmeridianal progress of the tidal swell, in early incrustive times, on our planet, would give the forming crust structural characteristics and aptitudes trending north and south. The earliest wrinkles to come into existence, therefore, would be meridianal, or submeridianal, and such is certainly the prevalent direction of the most conspicuous earth features." "So far as geological research has gone, there is reason to believe that the elevated and depressed areas are of primeval antiquity — that they antedate the very oldest of the sedimentary formations. We may thus speak of the great world-ridges as regions of dominant elevation and of the profound oceanic troughs, as areas of more or less persistent depression."

The great areas of elevation and of persistent subsidence are very distinctly marked out on our earth by a meridianal-lobed arrangement, caused, as Professor G. H. Darwin thinks, by *tidal* rupture during early stages of crust formation. This great recent discovery is, therefore, one of the greatest importance to all seeking for the solution of the problem of the distribution of land and water on Mars.

Tested by our moon, and viewing the marea as "seas" now in some way solidified, the foregoing conclusions are borne out in the most remarkable manner on the hemisphere which is presented towards us.

From Walter to Cassini we have distinct evidence (of different kinds) of the existence along the prime meridian of a vast shoal or submerged continent lying north and south, bordered on the east by the series of marea, Nubium, O. Procellarum, and Imbrium, and on the west by Nectaris, Tranquilitatis, and Serenitatis, each series of three marea having a meridianal trend. Near the limb again, east and west, we see the well-known two series of vast walled plains, lying north and south, the great Sirsalis cleft, also north and south, 400 miles long, being a vast anticlinal surface-fracture.

That the persistent subsidence of ocean floors (an axiom in terrestrial geology) is also clearly seen in our moon, is well illustrated in the remarkable arrangement of the clefts in relation to the marea, viewed as areas of subsidence. In regard to this question, Mr. A. C. Ranyard in *Knowledge*, September, p. 173, says: "The evidence brought forward by Mr. Peal, with regard to the general subsidence of the great lunar marea seems to me conclusive." So that the two features of slow subsidence of ocean floors and meridianal arrangement of the land and sea areas due to primeval tidal rupture during crust formation, are seen on both globes of the earth-moon system.

But the arrangement of the land and sea areas on Mars is on a totally different plan, there is an entire absence of equatorial oceans, and of meridianally placed continents divided, as in our case and the moon, by wide troughs of subsidence. We see on that globe two vast polar oceans divided by a more or less continuous land girdle.

We may reasonably assume that on Mars the crust-formation began on the poles, and that, as time went on and further condensation took place, subsidence and formation of polar sea-basins would ensue, their floors, being the coldest and densest portion of the crust, persistently sinking in, would naturally cause the emergence of the equatorial land-girdle. The comparatively unbroken continuity of this latter would again be due to the absence of a large satellite causing tidal rupture: there would be no breaking-up of the emerging land girdle round the equator, during crust-formation, as in the earth-moon system.

Professor G. H. Darwin thinks that the effect of solar tides on Mars must be "inconsiderable," they might yet, however, be sufficient to cause and maintain a slight overspill from one polar ocean-basin into the other, as the northern or southern hemispheres were presented towards the sun.

During the equinoxes, also, for some months, twice a year, solar attraction would probably draw the water from each polar basin during the daytime through the lowest levels on to the equatorial land-girdle, the ebb taking place at night.

Persistently in operation from the very earliest periods, these two causes might well establish and maintain well-marked tidal channels, the so-called "canals," in fact, and in this way solve these enigmatical features. Their being open to the seas at each extremity is a powerful argument in favor of the above view.

With such an effectual and continuous circulation of the water from the polar basins, over the tropical areas, we may perhaps see the solution for the remarkable mildness of the climate on Mars and smallness of the polar caps. The thermal effects of our Gulf Stream would be produced not only at one spot, or even one pole, but all round; each polar basin would have currents of warmer water poured in daily.

The occasional duplicity of the canals may perhaps be due to a series of large islands, as seen so frequently in terrestrial rivers flowing through alluvial tracts. Viewed from a great elevation, our Brahmaputra would undoubtedly appear double for hundreds of miles, especially in the dry season when the large sand "churs" or islands fill the bed of the river, though even in the rains there are many, more or less permanent, of large size, such as our "Majulé," or middle ground, 130 miles in length by 10 or 20 wide, giving the appearance of a series of vast loops. The rule indeed is that this large river is seldom seen confined to one channel.

The remarkable feature of the whole case seems to be that so far there has been little or no reference to terrestrial experience when discussing the problem of the distribution of land and water on Mars. The great recent geological discoveries bearing on the subject appear to have been overlooked, but if the law of the permanent subsidence of ocean floors, now an axiom among geologists, and so clearly seen on our moon, applies to Mars, we can see more or less clearly that the coldest and densest portions of the Martian crust will be the floors of the two polar ocean basins, the slow, steady subsidence of which causes the emergence of the equatorial land-girdle; the comparative completeness of this, again, being due to the absence of a large satellite, to cause tidal rupture during formation.

Last, we seem to see an intelligible solution for the so-called "canals," as modified tide-channels, and even for their occasionally appearing double; the exceptionally effectual circulation of the water on the planet being the solution for the mildness of the climate.

NOTES AND NEWS.

THE expedition, equipped by the de Laincel fund for linguistic and paleographic research among the Maya remains of Mexico, under the charge of Dr. Hilborne T. Cresson of the Bureau of Ethnology, Washington, D. C., reached Mexico in January and proceeded to the Partedo de la Frontera near the Guatemalian border and from thence to the little-known region around the lake of Petin. While in this part of the country the guide died of malarial fever, and Dr. Cresson, accompanied by his Maya servant, continued the explorations until the season was too far advanced for further research. The expedition has been very successful in the collection of material which will aid in deciphering the Maya hieroglyphs and demonstrates a rich field for future study both paleographic and linguistic. It has been found that exact drawings made by the pencil the size of the original glyph or half-size, will be most serviceable for giving details which repeated trials of the camera failed to satisfactorily produce, as many of the minor components, which recent study has shown to be very important in the interpretation of the glyphs, are so delicate in execution and so worn by time that the impression is calculated to deceive the student. Moreover, the forests which surround the ruined Maya structures are very dense and a proper light for photography is impossible to be obtained, and even if enough space was cleared for light the cast shadows of the tablets themselves lead to erroneous lines, when the negative is printed. A comparison of photographs of paper squeezes, made by previous expeditions, shows that much of this work has been hurriedly done and the minor components more or less distorted by being carelessly removed, so making them almost useless for exact study.

— An important meeting of the Victoria Institute took place on April 17, at Adelphi Terrace, London, the president, Sir Gabriel Stokes, Bart., in the chair; after the election of several new members and associates, Major C. R. Conder, R.E., D.C.L., read a paper on "The Comparison of Asiatic Languages," in which he dealt with the ultimate relationship of the great divisions of Asiatic speech, forming the separate families called Aryan, Semitic, and Mongolic, and the affinities of the oldest monumental languages in the Akkadian and the Egyptian. After describing the accepted principles of internal comparison of languages in each group, Major Conder urged that the roots, to which philologists have referred all words in each family, run — in a large number of cases — through all these families, probably indicating a common source of language. He proceeded to draw results as to the primitive condition, and original home, of the Asiatics, and pointed out that Egyptian was grammatically to be classed with Semitic languages, and Akkadian with Mongolic speech. A comparative list of some 4,000 ancient words, from the languages in question, accompanied the paper, which was listened to throughout by a large and appreciative audience. The discussion was commenced by Professor Legge of Oxford University, who, referring to the work of his life as a student of Chinese for upwards of half a century, urged the value of such work as that done by Major Conder. In all his comparisons, he was possibly not prepared to agree, but that did not prevent him from recognizing the great value of what he had done, and the evidence afforded by such researches as to the primitive unity of the human race. Prof. Legge's remarks were followed by those of Mr. T. G. Pincher, the Akkadian scholar, Professor Koelle, Dr. Kenneth Macdonald, Professor Postgate, Principal R. Collins, and others. Captain F. Petrie, the honorary secretary, during the evening read an important communication from one of the members exploring in Egypt, in which some newly-discovered sculptures were described, these threw quite a new light on the mode of transporting immense masses of stone by water, which was in use among the Egyptians in the days of the Pharaohs.

— Professor S. S. Laurie's work on "John Amos Comenius" has been republished in this country by C. W. Bardeen of Syracuse, with a preface and a bibliographical appendix. Professor Laurie begins with a brief study of the Renascence and the Reformation in their relation to education, and then proceeds to an account of the checkered and roving life of the great Moravian bishop and educator. Then, taking up the leading works of Comenius, he endeavors to show what were the real contributions made by him to educational theory and practice. His principal merit, as Professor Laurie justly says, was in the method of teaching which he advocated, a method greatly in advance of that practised in his own time and similar in many respects to that followed by the best teachers of to-day. What the method was may be learned in detail from this book, where it is set forth at considerable length. He held that we ought to copy the methods of nature, and his works are filled with fanciful analogies between her operations and the labors and processes of the teacher. His own text-books, however, especially those for the study of languages, are often as unfit for their purpose as they could well be, and his whole method is of too formal a character, and is vitiated, as Professor Laurie remarks, by the belief that a man can be manufactured. Moreover, his idea of knowledge was too utilitarian, and he had no appreciation of philosophy or of art and the esthetic side of literature and life. Nevertheless, his method was a great improvement on that of his contemporaries, and his advocacy of milder discipline was equally commendable. He also advocated the Baconian study of nature; and in these days, when natural science and utilitarian studies have become prominent, and so much stress is laid upon right methods of teaching, it is not strange that his life and work have become objects of interest. Few persons, however, will care to study his own writings, and hence this book, which gives so full an account of them, will serve a useful purpose.

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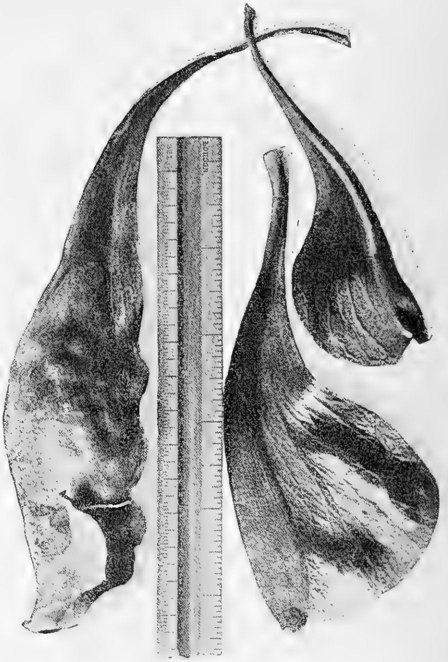
Attention is called to the "Wants" column. It is invaluable to those who use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The "Exchange" column is likewise open.

THE PRODUCT OF A CHANGED ENVIRONMENT.

BY GEORGE H. HUDSON, STATE NORMAL AND TRAINING SCHOOL, PLATTSBURGH, N. Y.

Toward the latter part of September or early in October 1891, a number of pitcher plants (*Sarracenia purpurea*, L) were sent to me from Wolf Pond, Franklin Co., N. Y., together with other bog plants, for our school Wardian case. This case is 120 cm. long, 51 cm. wide, 45 cm. deep, and stands before an east window where it does not get very much light, save on sunny mornings. We keep in this case many kinds of mosses, ferns, some fungi, and several small animals such as salamanders, toads, wood-frogs, young alligators, and different insect larvæ. This case also furnishes abundant material for microscopic study, such as rhizopods, infusorians, rotifers, etc. The pitcher plants were carefully set out in the east side of the case, and for several months the pitchers were kept filled with water, and were occasionally fed with flies and bits of meat. Later in the season the plants were neglected; the pitchers were not filled with water, nor was any kind of animal food given them. In the late spring there were two plants living. These plants had begun to increase the width of the leaf-like margin of their pitchers while the hoods and tubes themselves were suffering a marked change. These changes were intensified during the summer, and the result is shown by the reproduction of a photograph taken Nov. 5, 1892. This photograph shows an old and somewhat decayed phyllodium from one of the two plants, and, in contrast with it, one of the new phyllodia from each. These new phyllodia are bright green, without a trace of the usual coloring, serving to attract insects, save on the very edge of the aborted and flattened hood, where a faint border about 2 mm. deep may be noticed. Some of these hoods have not opened; the hairs which line others are in an immature and useless condition. The leaf-like margins of these curiously modified petioles, instead of being from one-fourth to one-third the width of the tube as in normal specimens, have become from three to four times the width of the now weak and flattened tube. The scale photographed with these phyllodia will show the extent of this modification. The scale shows inches on the left and centimetres on the right. Of the next older phyllodia the larger hoods have decayed, while the tube and its wing-like expansion are still in a healthy condition. This pitcher plant grows wild in Plattsburgh, and I have seen it in many places in the Adirondack region, but I have never noticed such wide margins in a state of nature. Was the change in our Wardian case made because of the absence of animal food, which made it necessary for the plant to look in other directions for its support? Was it made because of the absence of the influence of water in its tubes while it was forming these new phyllodia? Was the plant obliged to sacrifice its pitchers in order to extend its chlorophyl-bearing surface on account of the loss of light? The changes made, it will be noticed, were just those changes which would best bring it into harmony with its changed environment. Was this change made in response to the demands of the new environment, or were the changes but the reversion to an ancient type consequent simply on the diminished vitality of the plant? This curious change suggests many experiments which might easily be made to determine the extent to which certain lower organisms could vary in response to external stimuli, and thus be able to adapt themselves to unusual conditions in a changed or changing environment.

Early in the winter one of the little toads used to get into a large prostrate phyllodium, apparently to take a bath. We have noticed him a number of times sitting just within the hood with



his body partly in the water. The red, spotted salamanders crawl over the alligator and share the sunny portions of the case with him. Believing these bright-colored beings not fit for food, he has offered the little things no violence. One of the small garden toads did not fare so well but became a victim of a pair of jaws that broke his bones in their embrace.

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.

Variation in Native Ferns.

The wide variation in the forms of British ferns is well known to all who read the works of English florists, but there is less of this in America. Scolopendrium vulgare will have different auricles at the base, and sometimes is forked at the apex, but it varies little beyond this. I have found Woodsia ilvensis also with a forked apex, but this rare.

Aspidium acrostichoides is much more variable in many ways. Almost any woodland will present differing forms, as regards scales, form of frond, and division of pinnæ. Several times I have found fronds clearly pinnatifid in the lower part. This might be called an extreme form of the variety *Incisum*, and is most likely to occur late in the season, and in rich woods undergoing the process of clearing. The pinnæ are also sometimes once or twice forked at the apex, and there are other changes from the normal form.

Camptosorus rhizophyllus is less variable, but the basal auricles will sometimes be prolonged much like the apex, and will root like that. Specimens have thus been found giving origin to three young plants. I have one frond which is pinnate, but with a normal frond from the same root.

The Osmundas vary much. O. regalis has often a few sporangia on the otherwise sterile pinnæ, and O. Claytoniana has sometimes the same feature. I have a curious series of O. cinnamomea, where the so-called variety Frondosa is fertile at the base and apex, at the base alone, at the apex alone, or only in the centre. There are other oddities in this fern.

The Botrychiums vary much. The variations of B. virginianum are well known, and the variety *Gracile* is quite persistent in some places. B. matricariæfolium varies from age and location, but I have found many distinct and beautiful varieties of B. ternatum growing luxuriantly together. In one abundant plantation of B. simplex there are many strange forms, not altogether due to age. This is in sphagnum, and not far off I have found it growing in water.

The variation of Cystopteris fragilis is a never-failing perplexity to some. It seems so different, and yet so familiar. Often it has been mistaken for Woodsia obtusa.

Some ferns which I collected in Colorado were interesting to Professor Underwood from their local variation in a broad sense, being near the limits of their district. I collected Aspidium septentrionale on Cheyenne Mountain, but it was immature, yet those of the preceding year were smaller and quite unlike those I had from Europe.

On Skaneateles Lake, N.Y., I found small patches of Pellæa gracile growing on wet rocks; in one place, almost in the bed of a small waterfall. In other places there I found it on dry ledges, the roots and tufted fronds forming a dense mat. The station made quite a difference in the appearance.

W. M. BEAUCHAMP.

Baldwinsville, N.Y., April 14.

Singing of Birds.

IN answer to E. B. Titchener's inquiry regarding the relation of song to emotion in birds, the following is offered by one who has made careful observations in the language of over fifty well-known species.

That there is an expression of feeling in the notes of all of our birds no true lover of our feathered friends will attempt to deny. We are all most willing to admit the existence of a bond between them and us, and this relation, or assumption perhaps, we would not care to have dispelled. Nevertheless, although I am so anxious to invest these creatures, "favorites of creation," as Figuier so fondly calls them, with higher attributes of feeling and expression, it remains a fact, that their notes do not change in quality as a result of change in emotion. At least, this is so in so far as our ears are able to distinguish. Let us consider some cases.

A pair of robins will make a great outcry if their nest is molested, the excited notes of the male corresponding exactly to his cries when engaged in his vernal battles, or, later, when giving excited warning to its defenceless young when a marauding cat is at hand. If the eggs are taken, the pair quickly subsides, and the male will probably be singing the same evening; surely the next morning. Within a very few days a new nest is begun in the same neighborhood, the song continuing daily.

I have carefully noted the song of the warbling vireo, which is one of the few birds which sing while sitting upon its eggs. In one instance, after the set of four eggs was removed, the bird remaining near by, and uttering its querulous notes, I waited to observe. The male quickly returned to the empty nest, which it had recently left, and at once gushed forth in song. It may be that the song expressed much sorrow, or at least a complaint, but to me it was the same inspiring, ecstatic warble that I was accustomed to hear. I have robbed the nest of the scarlet tanager, rose-breasted grosbeak, wood thrush, hermit, and indigo bird, all beautiful singers, and then waited and listened, allowing ample time for the male to learn of the spoliation. In each instance the male quickly tuned up, and, to my idea, sang as sweetly as ever.

The expression of sentiment, or whatever we wish to call it, in the harsh *caw* of the common crow, or the single *cruk* of the raven, may mean as much, and probably does, as the tinkling melody issuing from the elfin winter wren. Then, too, the ever mournful, lonesome song of the wood pewee, or the solemn-sounding *hoo-hoo* of the great-horned owl, or the weird monotony of the whip-poor-will, undoubtedly answer the purpose equally with the sprightly notes of our little friend, the melodious, jingling song-sparrow. However, these notes and songs, although they may mean much to the birds, are, to our obtuse ears, ever the same.

MORRIS GIBBS, M.D.

Kalamazoo, Mich.

On a Supposed Climatic Variation in the Wing-Color of some Orthoptera.

I HAVE read with much interest the communication of Mr. Lawrence Bruner (p. 133) on the supposition that climatic differences may account for the different coloration — yellow, orange, red, blue — of the wings in some North American locusts, and, as the author requests other opinions, I will relate my experience in the Transvaal, where I made a considerable collection of orthoptera in the neighborhood of Pretoria.

Pretoria stands on the high table-land of the Transvaal; an almost treeless region, consisting of vast grassy plains well known by the name of "veld," with occasional hills or ranges of low mountains. In the dry winter season these plains are merely covered with a brown withered herbage; after the summer rains they are clothed with a more or less luxuriant crop of grasses and other plants. Consequently the conditions are very uniform throughout the area, but as I collected in the immediate neighborhood of the town of Pretoria, and during the summer season of 1890–91, the conditions of soil, climate, and altitude were absolutely identical.

My collection of orthoptera made at that time[1] affords evidence against the conclusions of Mr. Bruner, respecting the North American species, as the following list of some of my captures — a few conspicuous species — will show.

Species with orange-colored wings:—

Parga gracilis Burm.
Phymateus leprosus Fabr.

Species with yellow-colored wings:—

Catantops sulphureus Walk.
Oedaleus citrinus Sauss.
Oedaleus tenuicornis Schaur.

Species with red-colored wings:—

Phymateus squarrosus Linn.
Phymateus morbillosus Linn.
Zonocerus elegans Thurb.
Laphronota porosa Stål.
Acridium rubellum Serv.

Species with blue-colored wings:—

Oedaleus acutangulus Stål.

It will be observed that the same genera show different coloration, as *Phymateus*, orange and red; *Oedaleus*, yellow and blue.

The philosophical conception of the origin of these bright colors is very difficult. Of course they are purely non-protective, as species thus ornamented are most conspicuous objects when on flight; and even on the ground or elsewhere, where their folded wings and sombre or greenish hues assimilate them to their surroundings, they are easily found and greedily devoured by most birds. I found their remains in the crops of many birds. Even

[1] A complete list is given in the Natural History Appendix to my "Naturalist in the Transvaal."

in the Acciptres, species of *Falco* and *Cerchneis* were found gorged with them; the Secretary bird (*Serpentarius secretarius*) is an orthopteral glutton; bustards, especially the "Gom Paauw" (*Otis kori*) can apparently exist on them alone, while flocks of the common "Spreo" (*Spreo bicolor*) make vast inroads in the immense swarms of the smaller species.

Their survival in the struggle for existence would seem to have been almost entirely dependent on their entraordinary fecundity. Only species with great vitality and immense power of reproduction could withstand the requirements of this mighty avian banquet. The origin of the brightly colored wings cannot, however, be placed to the credit of abundant vitality, as some genera of large and active species exhibit brightly and also sombre and modestly colored wings. W. L. DISTANT.

Purley, Surrey, England, April 3, 1893.

A Puzzle for Future Archæologists.

NEAR Enon, in Clark County, Ohio, is a well-known artificial mound, commonly called "Prairie Knob," while the level tract on which it is situated is called "Knob Prairie." A former pupil of mine informed me that when he was a boy his grandfather sunk a shaft in the centre of the mound down to the underlying black soil, without finding any thing of consequence. The old gentleman was disappointed, not to say disgusted, to find this cherished landmark, which he had so long held in high esteem as the supposed receptacle of the regulation quantity of "Indian" relics, so utterly barren. He thereupon determined, in the generosity of his heart, that future explorers should not go unrewarded. He therefore deposited in the hole a miscellaneous collection of stone implements, pottery, shells, old bones, etc., such as he imagined a properly constructed mound ought to contain. This done, he carefully refilled the shaft, and restored the mound to its former appearance.

Imagine the sensation that such a find as this is likely to make when brought to light by some enterprising mound explorer of the twentieth century! CHARLES B. PALMER.

Columbus, Ohio.

Pre-Historic Remains in America.

NOTWITHSTANDING Dr. Brinton's protest in *Science*, April 14, I think most readers will agree that the language I quoted from his "Races and Peoples" (not "American Race") is clearly open to the incidental criticism offered. That the physical conditions of the American continent have been a potent agency in forming a distinct race, as he explains his language, is readily admitted. I also believe they have moulded the *heterogeneous* elements which peopled the continent from different quarters, at different eras, into a comparatively "homogeneous race," but it is difficult to understand the process of rendering "homogeneous" those already one in race and derivation.

If Dr. Brinton has failed to observe a marked difference between the Atlantic and Pacific types, I presume it is because he has not made the comparison with this thought in view, as it is certainly very apparent. His reference to the few shells and copper articles found in Tennessee and Georgia bearing Mexican and Central American designs is unfortunate for his position. He knows, or ought to know, that these are looked upon by all archæologists as puzzling objects because of their remarkable departure from the types of the Atlantic slope. This fact is, of itself, evidence of the general impression in the minds of archæologists of the differences between the art types of the two regions.

He asks, "Is he [Thomas] not aware that both the Nahuatl and Maya languages trace their affinities exclusively to the eastern and not the western water-shed?" Not claiming to be a linguist, I must present as my reply the words of one who is.

Dr. D. G. Brinton says, in his "Races and Peoples," p. 248: "All the higher civilizations are contained in the Pacific group, the Mexican really belonging to it by *derivation* and *original location.* Between the members of the Pacific and Atlantic groups there was very little communication at any period, the high Sierras walling them apart; but among the members of each Pacific and each Atlantic group the intercourse was constant and extensive. The Nahuas, for instance, spread down the Pacific from Sonora to the Straits of Panama; the Inca power stretched along the coast for two thousand miles; but neither of these reached into the Atlantic plains." Observe that he says "all the higher civilizations," which, of course, includes the Maya as well as Mexican people. Even in his later work he reiterates this opinion. In speaking of the groups into which he classifies the stocks, he remarks: "This arrangement is not one of convenience only, I attach a certain ethnographic importance to this classification. There is a distinct resemblance between the two Atlantic groups and an equally *distinct contrast between them and the Pacific groups*, extending to temperament, culture, and physical traits" ("American Race," p. 58). Now, when it is remembered that he classes the Mexicans, and, by the above-quoted language, the Mayas also, with the Pacific group, it would seem that, at the date the book referred to was published (1891), he was advocating precisely the same view as that advanced in my letter to *Science*, as he directly contrasts the Atlantic and Pacific groups as to temperament, *culture*, and physical traits, and holds that there was very little communication between the people of the two regions. He says further of the Mayas, that "So far no relationship has been detected with any northern stock," but is inclined to look to the Mississippi Valley for their priscan home.

If Dr. Brinton still holds the view indicated in the above quotations, which are from his most recent works, I cannot understand the position he takes in his note to *Science*, as the one is in direct conflict with the other. I have not appealed to the numerous statements in his older works which differ from the views indicated in *Science*, as it appears that in the light of new data, and for reasons satisfactory to himself, he has, since 1887, entirely changed his views in reference to the origin of the people of the American continent and the course of migration so far as affected thereby. (See "Myths of the New World," 2d ed., pp. 34–35, and Address at Meeting of A. A. A. S., Salem, 1887.)

I may remark, in closing this communication, that it is very singular the numerous resemblances between the customs and arts of the West Coast Indians and Pacific Islanders, which descend even to unusual designs, have no special significance and are disposed of with the single word "illusory," while the resemblances in a few designs on shells and copper, though unusual, are sufficient to warrant us in looking to the valley of the Mississippi for the priscan home of the Mayas. Distance has, of course, to be taken into consideration in deciding as to the signification of these resemblances. What I assert is that the types of the West Coast, including Mexico and Central America, taken as a whole, have a more marked resemblance to the customs and art of what we may call the Pacific region (especially the islands) than to those of the Atlantic slope. This indicates, at least, a culture influence affecting the inhabitants of the Pacific Coast not felt on the Atlantic slope. And no theory, which fails to give it more value than the mere coincident result of the "human psychical development" can abide the test of thorough examination.

CYRUS THOMAS.

The Lobatcheffsky Centenary

OCTOBER 22, 1893, a century will have passed since the birth of the famous Russian geometer, Lobatcheffsky. The world is just beginning to understand that, as mental ancestors of the modern cientific theory of man and the universe, only two take rank with him, Copernicus and Darwin. Until 1826 nothing had been published to overthrow the dogma that man has absolutely exact knowledge of "the space of experience." Lobatcheffsky showed that we can never know that any rectilineal triangle in "the space of experience" has its angle-sum exactly equal to a straight angle. As one result, geometrical axioms have disappeared for ever, and are replaced by *assumptions*. Thus he re-made not only mathematics, but kenlore. The Imperial University of Kasan is justly proud of its pupil, whom it speaks of as "encompassing it with an immortal splendor." It has organized a committee to raise a Lobatcheffsky fund to establish, in honor of his birthday, a prize, open to the world, for researches pertaining to non-Euclidean geometry. As a member of this committee, I will be glad to forward, in the name of the individual donor, any contribution toward this homage from all the enlightened world to one of the foremost names in "the pedigree of human thought."

GEORGE BRUCE HALSTED.

2407 Guadalupe Street, Austin, Texas.

Nesting of the Road-Runner.

THIS very peculiar long-tailed bird is common here throughout the year. It inhabits mainly the broad arroyos covered with chapparal thickets and scrub-oaks, as here is found its principal food, small snakes and lizards. The breeding season is from the middle of March to the last of July. The number of eggs laid varies in this locality from three to nine, though usually four to seven. The eggs are pure white, covered with a thick chalky coating which is often found partly scratched off.

The nests are built in thick chapparal bushes or scrub oaks, from two to five feet from the ground. They are composed of coarse sticks placed roughly across the supporting branches to the thickness of about two inches and a diameter of ten inches. Over this platform is placed a layer of sage leaves and twigs, forming a shallow, saucer-shaped depression. Then last, but invariably, is placed in the depression a small amount of dry horse-manure broken into small pieces. I do not know the reason of this last addition but it is nevertheless an invariable constituent of the Road-Runner's nest

The nest of the Burrowing Owl presents the same peculiarity, though with an apparent reason. The nest cavity of the Burrowing Owl is always partly filled with green horse-manure. In this case the decaying vegetable matter probably forms heat enough to carry on the incubation. But in regard to the Road-Runner's nest I do not see the necessity of the dry horse-manure.

I would be pleased to hear from any one who is acquainted with the nesting habits of the Road-Runner. JOE GRINNELL.

Pasadena, Cal.

Ad Ignorantiam.

THE calumniators of Professor Wright have been fully met, and an animus for their attack suggested. There are some critics remaining who have used an argument not found in logic,— that "ad ignorantiam," — with freedom, and, to the users, with telling effect. A few words as to this argument may not be inopportune.

A. can neither recognize the peculiarly shaped pinnacles on the top of a glacier from day to day, nor can he remember the names of the people who are introduced to him at the receptions to which he goes. B. can do both readily, and states his ability to do so. Thereupon C. jumps up and says that it is impossible to B. to speak the truth, as it is notorious that A. can do neither, and A. is an authority on all subjects. A. finds it impossible on Monday to stake out the surface of a slippery sidewalk, and publishes the fact. On Tuesday B. comes along with knit socks over his boots and makes that sidewalk look like a dress-maker's pin-cushion. When this fact is published, the ubiquitous C. springs up and tells how often the frame of A. subsided in the attempts, and therefore B. never did what he claims to have done.

A whole tribe of A's fail to find Truth at the bottom of the well—all old authorities to the contrary notwithstanding—and thereupon dogmatize to the effect that she is not there or, if there, is a palimpsest edition, introduced by ex-Olympian means. When B. shins down the rope and brings up the damp and coy dame, he is met by shrieks of C., to the effect that he carried her down in his pocket, because all the A's., aided by the strongest microscopes, could not locate her within seven rows of apple trees of the place.

It may strike people as rather funny for men who have said that certain things do not exist, to prove that they do not exist by failing to find them. It is not their business to find them, or, rather, it would seriously hurt their business to find them. They cannot adduce their ignorance or inability against the knowledge and power of others who have done what they have failed to do, and what they wished to fail to do.

The writer does not think many of the questions as fully settled as they might be; but he does not propose to believe a man because he poses as an ignoramus.

EDWARD H. WILLIAMS, JR.

Bethlehem, Penn., April 14.

Color in Flowers.

IN reply to the inquiry on p. 179 will say that the preservation of colors in flower is fully explained in Professor Bailey's "Horticultural Rule Book." F. H. PLUMB.

Springfield, Mass., April 20.

BOOK-REVIEWS.

Idle Days in Patagonia. By W. H. HUDSON. New York, D. Appleton & Co. VIII. 256 p. 8°.

THE author of "The Naturalist in La Plata," reviewed on a previous occasion in these columns, has given us in the present volume another interesting book. At first sight the title seems somewhat misleading, inasmuch as the author met with an accident a few days after his arrival in the country and was confined to the house for a considerable period. As, however, he says the book would probably never have been written if the original intentions in visiting the country had been carried out, we may consider the accident a lucky one. His "Idle Days" gave him ample time for thought, and in this as in the previous volume we have many original ideas. The most of the time was spent in the valley of the Black River, and in his chapter upon the valley we note a fact that may be of interest at the present time in view of the controversy going on in relation to palæolithic man in America. In wandering along the banks of the stream he found many arrowheads on the ancient village sites. They were of two widely different kinds, "the large and rudely fashioned, resembling the palæolithic arrowheads of Europe, and the highly-finished, or neolithic arrowheads of various forms and sizes, but in most specimens an inch and a half to two inches long. Here there were the remains of the two great periods of the Stone Age, the last of which continued down till the discovery and colonization of the country by Europeans. The weapons and other objects of the latter period were the most abundant, and occurred in the valley: the ruder and more ancient weapons were found on the hillsides, in places where the river cuts into the plateau. The site where I picked up the largest number had been buried to a depth of seven or eight feet; only where the water after heavy rains had washed great masses of sand and gravel way, the arrowheads with other weapons and implements had been exposed. These deeply buried settlements were doubtless very ancient."

He found that to the inhabitants of the valley, the river was all in all. Beyond its banks spread the gray, desolate desert; within the valley's bounds were light and life. Just as all things were mirrored in its waters, so was the stream reflected in the minds of the people. "Even the European colonists," says he, "have not been unaffected psychologically by the peculiar conditions they live in, and by the river on which thay are dependent. When first I became cognizant of this feeling, which was very soon, I was disposed to laugh a little at the very large place 'the river' occupied in all men's minds, but after a few months of life on its banks it was hardly less to me than to others, and I experienced a kind of shame when I recalled my former want of reverence, as if I had made a jest of something sacred. Nor to this day can I think of the Patagonian river merely as one of the rivers I know. Other streams, by comparison, seem vulgar, with no higher purpose than to water man and beast, or to serve, like canals, as a means of transport." So powerfully did the river impress the native minds that they became incapable of imagining any place to be habitable without it.

In one chapter we have an account of the habits of several breeds of dogs. A Scotch collie was found to take kindly to the wild life in the desert and soon became the leader of the ordinary dogs. But four pure-breed grayhounds, when tired of moping about the house, would take to the desert and course on their own account, returning, however, in a couple of days gaunt, thin and lame. Having been well fed and recovering their spirits and strength, they would again betake themselves to the desert, to return again to their master's house, worn and thin. These hounds, if left to themselves, would have soon perished, while the collie would have been successful in the struggle for existence.

Anything but a pleasing picture is drawn of the struggle the new settler has with Nature in his new home. Animals, birds, insects, and even inanimate forces are all arrayed against him, but the author considers even the severity of the struggle conducive to the well-being of the individual concerned. "The man," he says, "who finishes his course by a fall from his horse, or is swept away and drowned when fording a swollen stream, has, in most cases, spent a happier life than he who dies of apoplexy in the counting-house or dining room; or who, finding that end which seemed so infinitely beautiful to Leigh Hunt (which to me seems so unutterably hateful), drops his white face upon the open book before him. Certainly he has been less world-weary, and has never been heard to whine and snivel about the vanity of all things."

An interesting account of leaf-cutting ants is given (pp. 138–142), and the bird-music of South America is stoutly defended and favorably compared to that of Europe. He says: "The bird language of the English wood or orchard, made up in most part of melodious tones, may be compared to a band composed entirely of small wind instruments with a limited range of sound and which produces no storms of noise, eccentric flights and violent contrasts, nor anything to startle a listener — a sweet but somewhat tame performance. The South American forest has more the character of an orchestra, in which a countless number of varied instruments take part in a performance in which there are many noisy discords, while the tender, spiritual tones heard at intervals seem, by contrast, infinitely sweet and precious."

Two of the chapters deal with "Sight in Savages" and "Eyes." These have many points of interest which cannot be referred to in detail here. The one on the "Plains of Patagonia" deals with that peculiar topic of why certain scenes, inherently not pleasing or attractive, withal impress themselves upon the mind with wonderful vividness and are always recalled with pleasure. The plains are not possessed of great scenic attractions, for "Everywhere through the light, gray mould, gray as ashes and formed by the ashes of myriads of dead trees, where the wind had blown on it, or the rain had washed it away, the underlying yellow sand appeared, and the old ocean-polished pebbles, dull red, and gray, and green, and yellow." From an elevation "On every side it stretched away in great undulations: but the undulations were wild and irregular; the hills were rounded and cone-shaped, they were solitary and in groups and ranges; some sloped gently, others were ridge-like and stretched away in league-long terraces, with other terraces beyond, and all alike were clothed in the gray everlasting thorny vegetation." There is, also, a striking lack of animal life. "All day the silence seemed grateful, it was very perfect, very profound. There were no insects, and the only bird-sound — a feeble chirp of alarm emitted by a small skulking wren-like species — was not heard oftener than two or three times an hour. The only sounds as I rode were the muffled hoof-strokes of my horse, scratching of twigs against my boat or saddle flap, and the low panting of the dog. And it seemed to be a relief to escape even from these sounds when I dismounted and sat down: for in a few moments the dog would stretch his head out on his paws and go to sleep, and then there would be no sound, not even the rustle of a leaf. For unless the wind blows strong there is no fluttering motion and no whisper in the small stiff undeciduous leaves, and the bushes stand unmoving as if carved out of stone." Day after day he was drawn to these dreary wastes and the peculiar state of mind seemingly induced by them is thus described: "During those solitary days it was a rare thing for any thought to cross my mind: animal forms did not cross my vision or bird-voices assail my hearing more rarely. In that novel state of mind I was in, thought had become impossible. Elsewhere I had always been able to think most freely on horseback; and on the pampas, even in the most lonely places, my mind was always most active when I travelled at a swinging gallop. This was doubtless habit; but now, with a horse under me, I had become incapable of reflection; my mind had suddenly transformed itself from a thinking machine into a machine for some other unknown purpose. To think was like setting in motion a noisy engine in my brain and there was something there which bade me be still, and I was forced to obey. My state was one of suspense and watchfulness; yet I had no expectation of meeting with an adventure and felt as free from apprehension as I feel now when sitting in a room in London. The change in me was just as great and wonderful as if I had changed my identity for that of another man or animal; but at the time I was powerless to wonder at or speculate about it; the state seemed familiar rather than strange, and although accompanied by a strong feeling of elation, I did not know it — did not know that something had come between me and my intellect — until I lost it and returned to my former self — to thinking, and the old insipid existence."

The peculiar state of mind here described the author attributes to a reversion to a primitive and savage mental condition, a state of intense watchfulness and alertness, but without the exercise of any of the higher mental faculties. He believes that man still retains much of the savage in him and this is brought out in wild and desert places, in times of great danger and under many adverse circumstances. This, like many other questions, touched upon or discussed, is food for thought for the reader.

JOSEPH F. JAMES

Washington, D. C.

The Coal-Tar Colors, with Especial Reference to Their Injurious Qualities and the Restriction of Their Use: A Sanitary and Medico-Legal Investigation. By THEODORE WEYL. Translated, with permission of the author, by Henry Leffmann, M.D., Ph.D. Philadelphia, P. Blakiston, Son, & Co.

THE coal-tar colors having replaced the vegetable products in all branches of dyeing, a study of their sanitary relations becomes of great interest, and the more particularly, too, because of their rapidly extending application in the coloration of foods and of articles of daily household use. The call for active legislation in these matters has become imperative, but the exact legal status of the new colors has not yet been clearly defined, nor has their physiological action been sufficiently demonstrated. The civilized governments have passed laws regulating the sale and use of certain coal-tar colors, but, in correspondence with the imperfect knowledge we have as yet attained in this branch of science, these legal statutes proved inadequate and failed in their purpose. To determine by direct experiment the physiological action of the colors in question, and thus to provide a basis for a new and better legislation, was the work undertaken by Dr. Weyl, and this little book upon the sanitary relations of the coal-tar colors, translated from the German by Dr. Leffmann, is the published account of these same experiments, together with much else of importance and interest. The book is somewhat technical, but this need deter no one from its perusal, for, as Dr. Leffmann remarks in his preface, "the essential matter is so distinctly set forth that the chemical portion may be passed by those who are unable to comprehend it." There is no portion that may not be read with profit by all, the technicalities are well masked behind good English, and, thanks to Dr. Leffmann, we have a book of live interest from beginning to end. Reviewing the book critically, we have but one fault to find, and that with the arrangement. It will suffice to name the parts in their order as follows: Translator's Preface, Preface, Contents, Introduction, General Part, to page 34, Appendix, pages 35–60, Special Part, pages 61–148, Appendix, Index. This seems to us an original system of book-making, but, after all, change the *names* of the parts, and we have everything in proper place.

Beginning the book with the General Part, we have a few pages on the preparation of the coal-tar colors, their classification, nomenclature, commercial forms, uses, etc. The so-called poisonous colors are then discussed, and the arsenical nature of many of the earlier manufactures is pointed out. Fuchsine, for example, thanks to its contained arsenic, was long regarded as poisonous, until being produced in a state of purity, its entire harmlessness was demonstrated. There is a general review of the laws regulating the use of poisonous colors, and then, *verbatim*, the enactments of Germany under date of July 5, 1887. In 1888 there were appended to the said enactments regulations as to the examination of colors, fabrics, fruit jellies, liquids, etc., for arsenic and tin, and these Dr. Weyl has given in full. The methods are interesting and exact, though not original. The laws of other countries than Germany are given in some detail, and then we pass to the experimental part, the method to be followed being first described. As it was out of the question to test all, or even the greater portion, of the numberless coal-tar derivatives, Dr. Weyl selected such as were suspicious or had already been regarded as poisonous and endeavored to take those in most general use. Of the nitroso colors, we have dinitrosoresorcinol and naphthol green, B. The nitro colors include picric acid, saffron-substitute, Martins' yellow, naphthol yellow S, brilliant yellow, and aurantia, and of these only the sulphonated colors, naphthol yellow, and Martin's yellow were found to be harmless. The azo- colors are discussed at some length from both a technical and toxicological standpoint, but of the twenty-three colors examined only two, menatil yellow, and orange II., produced distinctly poisonous effects when administered by the stomach. Many, however, developed a slight albuminuria, and one at least was plainly poisonous when introduced into the subcutaneous cellular tissue.

It is highly gratifying to remark the comparative harmlessness of by far the greater number of the coal-tar colors, and even in those colors which are indicated as poisonous such large does are necessary in order to produce toxic effect as to render accidental poisoning from the same a practical impossibility.

Much honor is due Dr. Leffmann for his part in giving to the English-reading public this book, the first on the subject in our language,— but the hearty reception it has met with from chemist, medico-legal expert, and medical practitioner alike, bespeaks sufficiently its worth and opportune appearance.

CHARLES PLATT.

Alternating Currents. By FREDERICK BEDELL, Ph.D., and A. C. CREHORE, Ph.D., Instructors in Physics, Cornell University. New York, W. J. Johnson Co.

THE Johnson Co. is to be congratulated upon the appearance and make-up of this volume. The large, clear print, good paper, and well-drawn figures, make it one of the best books, from a mechanical standpoint, which has ever been published. On careful examination there does not appear to be a single misprint, or a single error in the mathematical formulæ, in marked contrast to the slipshod English and errata which disfigure almost every page of Fleming's book. No less are the authors to be congratulated on their work, for this book will probably be for years a standard text-book on the subject. Whatever one may find to criticise, it will not be the manner in which the subject is treated, nor mistakes in the treatment.

The subject is developed in a logical and simple manner. In Part I., which contains the analytical methods, we have, after an introduction on the elementary notions of the magnetic field, current flow, and harmonic motion, the general equation for circuits with resistance and self-induction; then the solution to this equation, and its application to the different cases possible. The constants of the equation are determined in each case, and curves plotted from actual values of the resistance and self-induction. Next in order come the general equations for circuits with capacity and resistance, and circuits with resistance, capacity, and self-induction. These are treated in the same manner. All possible cases are considered, the constants determined and curves drawn to illustrate the solutions.

Chapters xii. and xiii. treat of circuits with distributed capacity and self-induction, a subject of the utmost importance in these days of long-distance telephoning and telegraphy.

Part II. contains the graphical treatment. The analytical results obtained in Part I. are made use of as a foundation for the graphical methods. In addition to the cases considered in Part I. we have cases of circuits, in series and parallel, containing different voltages, resistances, self-inductions and capacities, and the results of variations of the latter in such circuits. At the end of the book is given a table of mechanical and electrical analogies, amplified from that previously given by other writers. The consistent notation used throughout the book gives an added pleasure to its perusal.

There are some things omitted which might have been treated of with advantage. For instance, though the graphical solution of problems concerning divided circuits is given, the analytical is not. If Lord Rayleigh's method were the only one known, there might be a reason for this, but those who are readers of *La Lumiere Electrique* and *L'Electrician*, will call to mind various neat and simple methods of treating the subject, and the latter is too important, practically, to be able to do without any thing which can add to our information.

We understand that the authors have underway a volume on alternating circuits containing iron. With Kennelly's and Steinmetz's laws, we may expect from the analytical treatment much that is new and important with regard to the best size and dimensions of transformers for given efficiency and output, etc.

This work has been adopted as a text-book by a number of American universities, Cornell, Purdue, University of California, and others. R. A. F.

Comparative Philology of the Old and New Worlds with Reference to Archaic Speech. By R. P. GREG, F.S.A., F.G.S., etc. 1 Vol. LXXII. 355 p. Royal 8°. London, Kegan Paul, Trench, Trübner & Co., 1893.

IT it a painful duty for a reviewer to take up a work which is honest in intention and laborious in execution, but hopelessly deficient in method; and such is the one before us. To issue its considerably more than four hundred large pages must have cost the author a great deal of work and of money; yet for all scientific purposes the results he reaches must be estimated as scarcely above zero.

The judgment may seem harsh, but let us see what he sets out to prove and what methods he adopts. He writes to support the hypothesis of an original unity of language, of an original common tongue, an archaic speech of great simplicity, composed of differentiated emotional and imitative utterances, fragments of which can be traced in all the languages of the world, bringing them, therefore, into a genetic relationship. To prove this, he devotes over 350 pages to "Tables of Accordances," lists of words which he believes to be from the same root in the most diverse tongues. The hypothesis is by no means a novel one, nor does he claim it as such, but perhaps it has not before been urged with such abundance of illustration.

Whatever one thinks of the hypothesis, all will agree that a competent knowledge of linguistics should be asked in its supporters, if they claim a hearing before the scientific public; and just here Mr. Greg is strangely deficient. His introduction begins with a survey of American languages, and as these figure largely in the tables, they will serve as a test of his work in general.

His authorities at once awake astonishment. Ignatius Donnelly's "Atlantis," the second-hand reports of Bancroft, Canon Cook, Hyde Clark, and Bradford, the tracts of Professor Campbell, and Vincente Lopez, and a few unimportant and defective vocabularies, such as these of Marcoy and Parry, are the books that figure most prominently in his "list of authorities" What he has learned from them is on a par with their value. He speaks (p. x.) of "the ancient Nahua and Aztec languages of Mexico," unaware that these words are merely different names for the same language. On the same page he refers to the "Californian" language, as if any such existed; and attributes to Schoolcraft (instead of Lieber) the term *holophrastic*, as applied to American idioms Who "Dr. Daniel Whitney, the well-known American philologist," may be, will certainly puzzle readers, as he is surely not known on this side of the Atlantic.

When it comes to the tables of accordances, all American languages are conveniently divided into northern, central, and southern. It would go hard with a student if in this broad field he could not find a word somewhat analogous in meaning to any other word in any other language; particularly were the student satisfied as easily as Mr. Greg. For instance, among his "accordances" there are plenty of instances of analogies like the following: Accadian, *shuku*, wheat, American, *mays*, maize; Accadian, *ka*, life, American, *ak*, water; Hebrew, *ben*, son, American, *hua*, son; Thibetan, *sna*, to breathe, American, *cenka*, noise; Indo-Chinese, *petan*, bird, American, *pa-hue*, to fly, etc. Thousands of his "accordances" are no closer than these.

But the unscientific spirit of the book is only too painfully apparent throughout. All such mere phonetic similarities, even where they are real and close, are of absolutely no value and prove nothing whatever concerning the relationship of linguistic stocks. This can only be demonstrated by studying the history and growth of a language, tracing its development and the influences to which it has been subjected, ascertaining the evolution of its grammatical forms and categories, separating the original elements from grafts and accretions, and confining comparisons to the former exclusively, and then only in the forms which existed at the earliest ascertainable epoch. Any such method as that adopted by Mr. Greg, in which these elements of linguistic growth are omitted, and even in which identity of alphabetic value is not attempted, is wholly valueless; and it is most unfortunate that all writers on linguistics have not been educated to recognize this fundamental principle of research.

An Atlas of Astronomy. By SIR ROBERT STAWELL BALL, LL.D., F.R.S. New York, D. Appleton & Co.

IN this work Sir Robert Ball has added a handy companion to his "Star Land." The atlas contains a series of seventy-two plates explanatory of the sun, moon, major planets, and fixed stars. The object of the atlas is to put into a convenient form, for the amateur astronomer, those data that will interest in a study of the evening sky. The author has in the introduction given the usual definitions of the coördinates of the position of a heavenly body as seen projected upon the celestial vault. A very neat explanation of the manner in which the orbit of a binary star is computed, is given, and as the process is so simple young astronomers will find in the construction of the orbits of the hundreds of binary stars very interesting instruction. The lunar maps, although upon a small scale, are very complete, giving as they do a representation of some part of the moon's surface throughout the whole lunation. A good selection of telescopic objects, such as interesting double stars, nebulæ, and rich star clusters is also given. The name of each object and its position in the sky are given as well as a short explanatory note describing the object. We note that some of the explanations given by the author are a little abstruse. On page 2, in describing the path of a planet, the words ellipse and orbit seemed to be woefully mixed up, so that it is difficult to follow the meaning of the author. For example, we have the statement that "the line *PA* through the two foci is the axis major of the ellipse. This is immediately followed by the statement that "it is bisected in *O* at the centre of the orbit." An orbit and an ellipse are not the same by any means, and should never be considered as such. A few lines following we have the statement that "the point *P*, nearest the sun, is the perihelion of the orbit. We certainly fail to see the truth of that statement. We should say that *P* was the perihelion point of the object moving in the orbit. The same criticism applies to the point of aphelion. Again, we must question the statement that "the time that the planet takes to go around its orbit is the periodic time." We were not aware that a planet went around its orbit. If it does, what is the name of the path in which the planet itself is moving? Upon the whole, the work has been neatly arranged, and the publishers have made it attractive both in style of printing and in neatness of binding. We would recommend the book to those who are seeking for some popular work that has in a handy form the interesting points in astronomy.

G. A. H.

CALENDAR OF SOCIETIES.

Philosophical Society, Washington.

Apr. 29.—Cleveland Abbe, Measurements of the Growth of Plants with the Auxanometer; Henry Farquhar, The Price of Silver; M. H. Doolittle, Is there an Objective Reality?

Appalachian Mountain Club, Boston.

Apr. 29.—Charles M. Skinner, Across British America.

May 3.—Lemuel C. Barnes, Mount Hermon in April; Charles C. Hall, The Shawangunk Mountains.

Society of Natural History, Boston.

May 3.—R. T. Jackson, Notes on the Development of Palms.

Royal Meteorological Society, London, England.

Apr. 19.—"The Direction of the Wind over the British Isles, 1876–80," by Mr. F. C. Bayard, F.R. Met. Soc. This is a reduction on a uniform plan of the observations made twice a day, mostly at 9 A.M. and 9 P.M., at seventy stations during the lustrum 1876–80, and the results are given in tables of monthly and yearly percentages. "Notes on Two Photographs of Lightning Taken at Sydney Observatory, Dec. 7, 1892," by Mr. H. C. Russell, F.R.S. These photographs were taken with a ½-plate view-lens, mounted in a whole-plate camera, and, as a matter of course, there is some distortion at the edges. Both photographs show the gas-lights in the streets as white specks, the specks being circular in the centre and crescent-shaped in other parts of the plate, owing to distortion. The lightning-flashes are also distorted. Mr. Russell believes that this distortion may account for the so-called "ribbon" flashes which are seen in many photographs of lightning. He has also made some measurements of the length and distance of the flashes, and of the intensity of the light. "Notes on Lightning-Discharges in the Neighborhood of Bristol, 1892," by Dr. E. H. Cook. The author gives some particulars concerning two trees in Tyntesfield Park which were struck by lightning, one on June 1 and the other on July 18, and also some notes concerning a flagstaff on the summit of Brandon Hill, which was struck on Oct. 6. "Constructive Errors in Some Hygrometers," by Mr. W. W. Midgley, F.R.Met.Soc. The author, in making an investigation into the hygrometrical condition of a number of cotton mills in the Bolton district, found that the mounting of the thermometers and the position of the water receptacle did not by any means conform to the regulations of the Royal Meteorological Society, and were so arranged that they gave the humidity results much too high. The "Cotton Factories Act" of 1889 prescribes the maximum weight of vapor per cubic foot of air at certain temperatures; and the author points out that, if the instruments for determining the amount present in the mills have an error of 20 per cent against the interests of the manufacturer, it is necessary that the makers of the mill hygrometers should adopt the Royal Meteorological Society's pattern for the purpose.

Introduction à la Théorie des Explosifs. Par M. E. SARRAU, Ingénieur en Chef des Poudres et Salpêtres, Membre de l'Institut. Paris, Gauthier-Villars et Fils, 1893. 115 p.

To the student of applied and theoretical mathematics this work of the eminent M. E. Sarrau will be most welcome. Unfortunately our language can boast of but little on the mathematical theory of explosives, and that little mainly in isolated chapters, imperfect and confusing in their briefness. Whether this be due to lack of interest or of study, we do not care to discuss, but certain it is that for a clear exposition of mathematical thought we must turn to writers of other lands than ours. From France we have received many of our best works, and it is with pleasure that we now announce this new work from the hand of M. E. Sarrau. A prefatory note, four lines in all, states the author's intention to bring forward such mechanical, thermo-chemical, and thermo-dynamical problems as are necessary to a comprehensive theory of explosives. Throughout, the discussion, which is both analytical and general, is so clearly and concisely accomplished as to be quite within the understanding of any student versed in differential and integral calculus. The first chapter treats analytically of the mechanical principles, including work, kinetic energy, and potential energy. In the second chapter are established the general laws of gases, Mariotte's, Gay-Lussac's, the law of specific volume, the hypotheses of Avogadro and of Ampere, the molecular and atomic weights, the melecular volumes, and the chemical formulæ. In this same chapter are studied the laws governing the specific heat of gases and the laws of gaseous mixtures. Chapter III. treats of the thermo-dynamics of perfect gases, and Chapter IV. of the general principles of thermo-dynamics, including thermal phenomena, equivalence, and the principle of Carnot-Clausius. Chapter V. is devoted to liquids, the law of compression, and the equations of Van der Waals and Clausius. The preceding theories and principles are applied in Chapter VI. to the various transformations, first without change in physical state and then with change from one state (of pressure, temperature, and volume) into another. The nature of heat is discussed in Chapter VII., and the heat theory of chemical reactions in Chapter VIII. Chapter IX., the last in the book, contains a study of dissociation, theoretical and practical. It will be noticed that the author has confined himself strictly to his outline as planned, and the work is, as the title indicates, merely an introduction to the further and advanced study of explosives, but it is such an introduction as comes from a master hand, and is suggestive of latent power and of the ability to pursue the demonstration to its completion. C. P.

THERE will be given at the gallery of the Boston Art Club, under the auspices of the Appalachian Mountain Club, from the 6th to the 24th of May, a remarkable exhibition of mountain photographs by Vittorio Sella of Biella, Italy. In addition to the exhibit (327 subjects) to which the "diploma of honor and large gold medal" has just been awarded at the competitive exhibition in Turin, it is expected that nearly two hundred other subjects will be represented, making it the most extensive exhibition of Sella's work ever given. The collection will fully represent the mountains of Dauphiny, Switzerland, the Tyrol, Sicily, and the Caucasus.

—Lieutenant Peary of the United States Navy, during his coming expedition to northernmost Greenland, will record observations of the aurora, upon a plan that will enable comparisons to be made in detail with records from other localities. The plan is already in operation, upon an international basis, and the results are proving to be important. Numerous observers widely distributed are desirable, and, inasmuch as even those who have no special technical knowledge may make entries that will be of value, any who feel so disposed may coöperate. Further information and supplies of blanks may be obtained from M. A. Veeder, Lyons, New York, U.S.A., who will be glad to receive, also, any records of observations of the aurora whatever, for purposes of comparison.

Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

Texas Plants. I will collect sets of plants represented in this region of Texas, either for sale or for exchange for literature, for anyone who will send me an order in time to fill it. Plants to be ready complete in November. C. F. Maxwell, Box 174, Dublin, Texas.

For sale or exchange—A Telescope (36 diameters, copper barrel)—for $20 cash or scientific books of that value. A. N. Somers, La Porte, Ind.

For sale—A complete set of the Reports of the Second Geological Survey of Pa., 1874–1893, including the Grand Atlas. Publisher's price over $115. Will sell for $50. Address F. D. Chester, Newark, Del.

The undersigned has skins of Pennsylvania and New Jersey birds, as well as other natural history specimens: which he wishes to exchange for marine, fresh water, and earthworms of the South and West. Correspondence with collectors desired. J. Percy Moore, School of Biology, University of Pennsylvania, Philadelphia.

For sale or exchange.—I have a Caligraph typewriter (No. 2) in perfect order and nearly new. It is in a heavy leather, plush-lined office case, the whole costing me about $100. I desire to obtain for it, either by sale or exchange, a new, No. 5 "Kodak" camera, with six double feather-weight plate-holders and the latest pattern of their tripod. The lens and pneumatic time-shutter must also be the same as those now sold with the last No. 5 Kodak. The price of what I desire in exchange is $78. Address. for particulars, P. O. Box 314, Takoma, District of Columbia.

For sale.—An Abbe binocular eye-piece for the microscope. Alfred C. Stokes, 527 Monmouth St., Trenton, N. J.

For sale or exchange.—One good long range Remington B. L. rifle, 44 calibre, also land and fresh water, and marine shells. Want shells, Safety, camera or printing press. A. H. Boies, Hudson, Mich.

Fine collection of microscopic slides for sale, or would exchange for first-class pneumatic bicycle. J. E. Whitney, Box 549, Rochester, N. Y.

For sale—A Zentmayer new model U. S. Army Hospital monocular stand. Price $110, will sell for $75. Address H. C. Wells, No. 151 Broadway, New York.

Wants

WANTED, as principal of a flourishing technical school, a gentleman of education and experience who will be capable of supervising both mechanical and common school instruction. Special familiarity with some technical branch desirable. Address, giving age, qualifications, etc., J. B. Bloomingdale, Fifty-ninth street and Third avenue, N. Y.

WANTED.—A young man as assistant in our microscopical department. Queen & Co., Philadelphia.

THE undersigned desires specimens of North American Gallinae in the flesh for the study of their pterylosis. These species are especially desired: *Colinus ridgwayi, cyrtonyx montezumae, deudragapus franklini, lagopus welchi, tympanuchus cupido* and *pedioecetes phasianellus.* Any persons having alcoholic specimens which they are willing to loan or who can obtain specimens of any of the above are requested to communicate with Hubert Lyman Clark, 3922 Fifth Avenue, Pittsburgh, Pa.

A COMPETENT TEACHER of botany in college or university is open to engagement. Address L., Box 86, Rochester, Mich.

CAN any one inform me as to the age to which cats have lived? I have one twenty years old. Edward D. Webb, 132 W. Eighty-first St., New York.

WANTED — Second-hand. Foster's Physiology, Balfour's Comparative Embryology, Claus & Sedgwick's Zoology, Flower's Osteology of Mammalia, Vine's Physiology of Plants. Please state editions and prices asked and address Richard Lees Brampton, Ontario, Canada

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First inserted June 19, 1891. No response to date.

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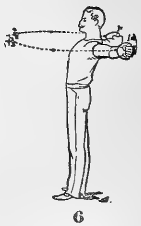

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SCIENCE

NEW YORK, MAY 12, 1893.

MOUNT ORIZABA OR CITLALTEPETL.

BY J. T. SCOVELL, TERRE HAUTE, IND.

THE central portion of Mexico is a plateau from 3,000 to 8,000 feet in elevation. About 19° north of the equator a broad belt of this plateau is composed of volcanic formations, which culminate in the snow-clad peaks of Citlaltepetl, Popocatepetl, and Ixtaccihuatl.

Citlaltepetl stands on the eastern margin of the plateau, about 80 miles from the coast, its eastern slope rising from the Gulf, the others from the plateau.

Orizaba, the name of a city on the eastern slope, is the name by which the mountain is best known to foreigners, but, seen from a distance, rising far above all surrounding peaks, with its crown of glistening snow, the Indian name of Citlaltepetl, star mountain, seems singularly appropriate.

Popocatepetl, smoking mountain, and Ixtaccihuatl, woman in white, rise from the plateau about 100 miles west of Citlaltepetl. These old volcanoes, with Mount St. Elias in Alaska, are the culminating points of North America.

rises considerably higher than its rivals further west. Dr. Franz Kaska, using mercurial barometers, made the elevation 18,270 feet. Professor A. Heilprin, using an aneroid barometer, adjusted by a mercurial, and estimating his station as 120 feet below the true summit, made the elevation 18,205 feet. My aneroid made the elevation estimated at 120 feet only 86 feet. Making this correction, the elevation would be 18,171 feet. Mr. O. G. Bunsen, C.E., of the University of Texas, and the writer, using railway levels to 8,313 feet, carried a line of spirit levels up to 14,000 feet, then using our aneroid barometer, made the elevation 18,179 feet. In April, 1892, by triangulation from the 13,000 feet level of Bunsen and Scovell, I made the total elevation 18,314 feet. These results, arrived at by different methods, seem closely confirmatory. Popocatepetl is about 700 feet lower than Citlaltepetl, and Ixtaccihuatl is about 700 feet lower than Popocatepetl. In a paper before the National Geographical Society, Dr. Mendenhall gave the elevation of Mt. St. Elias as 18,010 ft., so that Mt. Orizaba seems to be the highest elevation in North America.

Climate of Glaciers.

In this region the summer is a wet season and the winter a dry one. In the sunshine it is generally hot, summer or winter, even on the upper slopes, but in the shade or at night it is usually cool

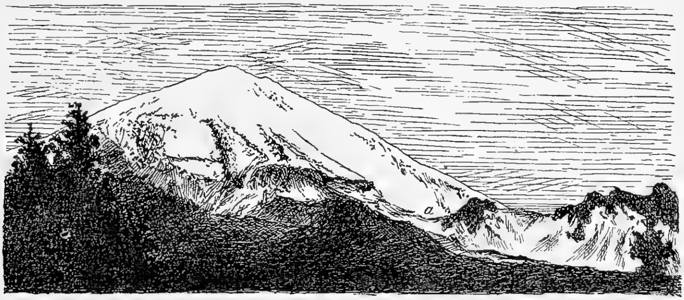

FIG. 1.—Southwestern slope of Citlaltepetl, taken Aug. 3, 1891, from the 13,800 ft. level. *a*. The 16,000 ft. level where the horses are left. See moraine just above and to the left of *a*. Tree in foreground is just above the cave.

Citlaltepetl, situated just within the northern boundary of the torrid zone, rising from tropical waters to polar snows, presents within narrow limits an epitome of the earth. On the slopes of this mountain may be found every variety of surface and every kind of climate, they produce all classes of vegetation and afford a congenial home for all sorts of animal life. This region, with its wonderful variety of scenery and its myriad forms of life, is of special interest to the student of science, whatever his department.

In July, 1891, a party consisting of W. S. Blatchley of Terre Haute, Ind., entomologist; Henry E. Seaton, now of Cambridge, Mass., botanist; A. J. Woolman of South Bend, Ind., icthyologist; U. O. Cox of Mankato, Minn., ornithologist; and the writer, visited the eastern slope of Citlaltepetl, making interesting collections of the varied forms of life which abound in that region. We found some forms new to science, found some familiar forms in unexpected localities, saw many interesting things, making the trip an interesting and valuable one to us. The different members of the party have published, or are preparing to publish, accounts of the work done in their several departments.

There is considerable discrepancy among observers as to the elevation of these Mexican mountains, until recently Popocatepetl has been considered the highest elevation, but determinations made within the last three or four years show that Citlaltepetl

and pleasant, anywhere between 4,000 feet and 10,000 feet. In summer the northeasterly winds seem to prevail, as shown by the fact that a tract of country, about 50 miles wide, to the southwest of Citlaltepetl, was dry and dusty, receiving only an occasional shower, while on either side of this region it rained almost every afternoon. The explanation seems to be that the winds from the northeast, losing their moisture on the mountain, flow over the region to the southwest as dry winds. Above the elevation of 12,500 feet, there were evidences of westerly winds, as leaning trees, drifting sands, more abundant vegetation on the eastern side of rocks, etc. But the winds most noticeable, summer and winter, were cold winds down the mountain at night, and warmer winds up the mountain by day. There is in general no rainfall during the winter, during the summer it is scanty from the coast up to 1,500 feet, then plenty of moisture to the summit, except on the southwest above 8,000 feet. The rains on the lower slopes are represented by snows on the upper slopes, but, while it rains almost every afternoon below, the snows above are less frequent, sometimes eight or ten days passing without a storm. But snow falls often enough during the summer to keep the peak covered down to about the 14,000 feet level, forming a distinct snow-line. If for a few days no snow falls, the old snow melts, and the snow-line rises, while an exceptional storm may carry the snow down to 11,000 feet or below, but 14,000 feet seems to be about the average level of the snow-line. As the dry season comes on, the snowfall gradually ceases, and the snow that has accumulated during the summer rapidly disappears under the heat of a tropical sun; rocky ridges and loose sands appear on the south and east, while an extensive glacier is disclosed on the north and west. (See Figs. 1 and 5 for summer views and 2 and 4 for winter views.) The glacier on the southwest extends downward to about 16,250 feet, narrow tongues of ice reaching 400 or 500 feet further downward, while on the north the main body descends nearly to 15,000 feet. In April, 1892, near the close of

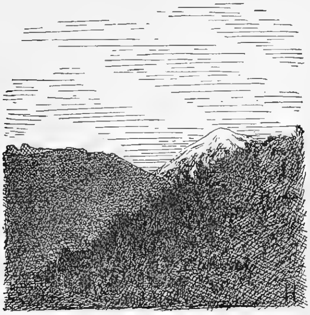

FIG. 2.—The Peak from the 13,000 ft. level on the southwest. Sierra Colorado on the left.

the dry season, the snow had disappeared and the ice had retreated some distance, leaving a valley or series of basins between the glacier and the crest of the moraine. In these basins were streams and ponds of water, small bodies of ice and broken rocks. (See Fig. 2 and Fig. 3.) The moraine is from 100 to 300 feet high on its outer face, and from nothing to 15 feet on its inner slope. The moraine is as steep as loose rocks will stand, but the rocks composing it are by no means loose, they are bound together with ice. The ice is continually melting from the outer face of the moraine, but the mass is practically constant, apparently supplied by water from the melting glacier above. Water from the glacier sinks slowly into the moraine, becoming ice again, later it melts from the face of the moraine and is absorbed by the rocks and sands below, without forming streams. I only saw one instance of a stream across the moraine, and it soon disappeared in the porous rocks. Dry drainage channels indicate that sometimes there is water enough to form streams, but, in general, there are no streams above 12,000 feet, and those below are few and small, on account of scanty rainfall and porous rocks. While it snows frequently during the summer, the total amount does not seem to be very great. The slope on the north is more gradual, so that the glacier on the north, measured along the slope, is about five miles long, while on the west it is not more than two miles in length (see Fig. 6). The width is from eight to ten miles, and the thickness or depth from 10 feet to 50 feet. I found no polished bowlders or striated rocks, the only evidences of motion were occasional crevasses, and the interval between the moraine and the ice. During the summer the glacier probably advances to the moraine, and both are generally covered with snow; but, on a photograph of the peak (Fig 1), taken from an elevation of about 13,700 feet, Aug. 2, 1891, after eight days without snow, the moraine can be seen as a sort of terrace across the slope of the mountain, while on July 28 and 29 it was entirely hidden by snow. The fact of the glacier on the west and north seems to indicate that more snow falls on those slopes, and that the moisture from which it is formed comes from the west. But the moisture might come from the Gulf, and the snow formed on the east be carried to the western slopes by the wind. From the storms I saw, I judged that the snow was somewhat equally distributed over the mountain, whether the storm was westerly or easterly, and that the glacier on the north, and the naked rocks and sands on the south, were due to the fact that more snow melted on the south, rather than that more snow fell on the north.

At first I felt sure that the glacier had its source in the Pacific Ocean, but the more I investigated the matter the more I inclined to the view that much of it might have come from the Gulf. These glaciers are not very extensive as glaciers go, but they present many interesting features for study, and they are easily accessible, one can ride to the foot of the moraine with little danger or fatigue. And there is little or no danger attending the exploration of the glacier, beyond the physiological effects of the great elevation. From January to the middle of April the glacier may be seen at its best. No danger from snow-slides or avalanches, and crevasses are not numerous or extensive, and can be easily avoided. In summer, there are occasional snow-slides on the western slope, and after 10 A.M. on a clear day there might be some danger from snow-covered crevasses, but earlier the frozen snow forms a safe bridge over any crevasse there may be in the glaciers of the Star Mountain.

Ice is quarried from the glacier for domestic use in the surrounding towns. The ice is taken out and dragged to the foot of the moraine and there loaded on burros or horses for transportation to the lower slopes.

Geology.

Citlaltepetl, Sierra Negra, and Sierra Colorado are the culminating domes of a great mass of volcanic rocks, which forms part of the eastern boundary of the famous valley of Mexico. This mass, about 50 miles in diameter, at the 8,000 feet level, rises by gentle slopes to the 13,000 feet level, above which rise the peaks mentioned. Citlaltepetl, the highest, is somewhat cone-shaped, but Sierra Colorado and Sierra Negra are ridges trending east and west, each about a mile in length. Sierra Negra is about five miles, a little west of south, from Citlaltepetl, and Sierra Colorado is about three miles southwest of the same peak.

From the summit of these peaks down to the 8,000 feet level trachytes, basalts, and scoriaceous rocks seem to make up the bulk of the mass. Then there are 200 or 300 feet of Cretaceous limestone in nearly horizontal strata, then from 800 to 1,000 feet of Jurassic limestone, whose crumpled and folded strata remind one of the folded rocks of Arkansas, thence down to about 4,000 feet there are several alternations of basaltic rocks and Carboniferous limestones. Near the city of Orizaba the limestone is thick-bedded and associated with beds of quite good marble and beds of the famous Mexican onyx. Below the city there are Devonian limestones, then Carboniferous strata to about 2,500 feet, then Cretaceous to the coast sands, about 25 miles from the Gulf. I noticed the succession of rocks, but did not attempt to identify the limestones, I name them as identified by Mr. Hugo Finck of Cordoba.

FIG. 3.—Detail at the foot of the glacier seen in Fig. 2.

According to Mr. Finck, the different phazes of the Glacial, Champlain, and Terrace periods are well marked in this portion of Mexico. The valleys of Orizaba and Cordoba were occupied by glacial lakes, over whose beds were deposited several hundred feet of drift materials, and the bowlder-strewn region below the valley of Cordoba seems to indicate glacial or iceberg action.

The interstratification of volcanic and carboniferous rocks seems to indicate that this region has been a centre of volcanic energy for several geological ages, and that the limestones mentioned occupy a comparatively narrow space along the slopes of a great core of volcanic materials.

Of the peaks mentioned, Sierra Colorado has an elevation of about 14,000 feet, has a smooth, uniform outline, and the greater portions of its outcropping rocks are of a reddish color. I saw no indications of a crater. Sierra Negra has an elevation of about 15,000 feet, with a uniform surface, broken only on the south, where the old crater was situated. The southern or outer wall of the crater has been broken away, leaving the inner or northern wall as an abrupt and rugged section of the otherwise uniform slopes of the mountain (see Fig. 4). The outcropping rocks are dark basalts.

Seen from a distance, especially when covered with snow, Citlaltepetl seems quite symmetrical, but in winter, or on careful inspection in summer, great ridges of rock may be seen leading up, like giant ribs, from all directions quite to the summit, giving the peak a rugged, restless appearance, so different from the restful outlines of its less elevated neighbors.

The crater of Citlaltepetl occupies the whole summit. It is somewhat elliptical in form, measuring about 800 feet from north to south by 600 feet from east to west, with a depth of between 400 and 500 feet. The rim is nearly horizontal, the difference in level between the highest and lowest points being about 80 feet. The peak is a little steeper on the south than on the north, the slope being between 30° and 35° on the south, increasing toward the summit. Inside the crater, for 30 or 40 feet, the slope is about the same as outside, then about 200 feet of vertical walls, then a sharp talus to the centre, as shown in the annexed cross-section.

The hot sunshine by day and the intense cold at night, with plenty of moisture during a large part of the year, result in rapid disintegration of the crater walls, cutting down the rim, and filling up at the bottom with fragments from the crumbling walls, so that, geologically speaking, the crater can exist but a short time. The ruggedness of the slopes and the fact of a crater seem to indicate that the peak is of recent formation. Citlaltepetl has a hot top, at least great areas of the rim of the crater are hot, giving off steam and gases, and large quantities of rocky material at the summit are the product of this fumarole action. Sulphur is found in considerable quantities in the rim of the crater, but I saw no evidence of sulphur gases, or anything to indicate that sulphur is being deposited at the present time. While there are many evidences of the recent formation of this cone, there is no evidence of an eruption of lava within historic times.

Between 8,000 and 12,000 feet we saw no rocks, the mountain was covered with a thick mantle of finely pulverized volcanic material, in some cases a black sand, then a sand of lighter color, again it is clay, that frequently appears like rock, called by the Mexicans tepetate. This deposit rises to about the same elevation on Popocatepetl, and to a height of 10,000 feet on the Toluca Mountains west of the City of Mexico, and in some places it constitutes the rim of the valley of Mexico. The proposed tunnel for the drainage of the City and valley of Mexico is deing dug through the tepetate, where it is at least 300 feet deep. The material was evidently deposited from water. It seems to indicate a more extensive lake system than now exists, and possibly more rapid erosion. The presence of this deposit at such high elevations seems to indicate an upheaval of some 3,000 or 4,000 feet within comparatively recent times. The geology of this region may not be very complicated, but fossils are not abundant, and there will doubtless be many conflicting opinions among geologists in regard to many of the geological features, yet the geologist who works up the region carefully will find an immense amount of very interesting geological material within a very limited area.

Life on Citlaltepetl.

With its base in the torrid zone and its summit in the region of perpetual snow, the eastern slope of Citlaltepetl produces almost every variety of vegetable and animal life. From the coast up to the 1,500 feet level, the country is practically a steppe; rainfall limited, soil sterile, vegetation scanty except along the streams; birds, insects, lizards, etc., abundant with but few mammals.

FIG. 4.—The Peak, seen over Sierra Negra, looking north from Esperanza during the dry season.

The region between 1,500 and 6,000 feet is the life-centre of this slope, the rainfall is abundant, and the soil, composed of the debris of volcanic and limestone rocks, is exceptionally fertile, producing a vegetation of great variety and luxuriance. The forest trees are seldom large but they exist in great variety, bearing ferns, orchids, bromelias, and other plants in great profusion on their trunks and branches; and every spot not shaded by the forest is crowded with a rank growth of herbaceous plants.

While in Cordoba, at an elevation of about 3,000 feet, I met a man from New York, who was buying Mexican lumber for the inside finish of his house, and went with him out to a little sawmill, where there was not more than 2,000 feet of lumber in stock. From this small quantity 65 pieces were selected, and, on counting, the man found he had 41 different kinds, all valuable as finishing lumber. It is said that there are as many as 100 different kinds of trees on the slopes of Orizaba that are valuable for lumber, besides many that furnish valuable dyes, oils, or gums.

This region is also famous for its orchids and ferns. I saw in Cordoba a collection of 70 species of native orchids, and was told that the collection did not contain nearly all the species of the region. Mr. Hugo Finck has sent to the Kew gardens from this region more than 50 species of ferns that were new to science. In this region are cultivated cotton, sugar-cane, coffee, pineapples, bananas, oranges, lemons, wheat, corn, potatoes, and many other interesting and valuable plants. The markets of Orizaba, at an elevation of 4,000 feet, displayed the most extensive variety of vegetables and fruits I ever saw in one collection. In this region there were many beautiful birds with some mammals and reptiles, but after plants insects were perhaps the most interesting zoölogical features of the locality. Professor Blatchley, in eleven days' collecting, took 160 species of moths, 145 of butterflies, 125 of coleoptera, 60 of hemiptera, and 40 of orthoptera. Other kinds of insects were numerous, but no collections were made of them. (See *Entomological News*, May, 1892.) From 6,000 feet upward the character of the different kinds of life changes rapidly, and the numbers of individuals and the variety of species are greatly diminished. In two days' collecting at 8,000 feet, Professor Blatchley only took about a dozen species of butterflies and beetles, and other forms of life seem to diminish in numbers quite as rapidly as the insects. Birds were an exception, for they were as numerous and varied at 8,000 feet as below.

Above 7,000 feet the different forms of life were more like those of the northern zones. There were oaks and elders, mustards, plantains, chickweeds, dock, violets, and familiar ferns; sparrows, meadow-larks, blackbirds, crows, woodpeckers and humming-birds were common along with many unfamiliar forms. But while vegetation was abundant, there were no forests similar to those so common in the temperate zone.

were much more abundant in the summer, though more orchids and some other plants bloom in the dry season. So that whether one visits Citlaltepetl in summer or winter he will find the plant-life interesting and well worthy of consideration. Insects were found up to the 14,000 feet level, and I saw two white butterflies at the summit, but the number of species found above 9,000 feet were very few. Between 8,000 and 9,000 feet there were some familiar birds, but above and below these levels most of the birds were peculiar to the locality. There seemed to be several species of humming-birds and many others with highly colored plumage, but we heard no songs more beautiful than we hear in temperate zones.

Sparrows were common up to 14,000 feet, and I heard one while on the summit, but whether he made his home there or was only a visitor like myself, I could not tell. Woodpeckers were busy about the trees between 13,000 and 14,000 feet, and several other birds were seen and heard at that elevation.

The rainy season was not favorable for collecting birds, but Professor Cox secured some very interesting specimens. Among reptiles, lizards were the most common, and they seemed just as lively near the 14,000 feet level as on the coast sands. Salamanders were found near the 14,000 feet level and at other localities on the slope, toads and tree-toads were seen, and collections of snakes were seen, but no live ones were taken by the party. Lizards are much more abundant in the dry season. I took more in three hours one day in April than the whole party saw in fifteen days in July. We saw rabbits, had mice in camp at 12,000 feet, saw evidences of moles, ground squirrels, and

FIG. 5.—Same as Fig. 4, but taken during the wet season.

Pines are common from 6,000 feet upward, but the forests of pine and spruce begin at about 9,000 feet, thinning out above 12,000 feet, so that the forest scarcely reaches 13,000 feet, although in some localities trees are found up to the 14,000 feet level. Above 13,000 feet a species of juniper spreads out over the rocks so that at a distance it appeared like moss. Along the slopes above 13,000 feet there were mustards, compositæ, castelleias, and a few other plants with two grasses, but no ranunculaceæ, claytonias, willows, or other water-loving vegetation so common on the high slopes of the Rocky Mountains in Colorado. The distribution is different in the two localities, on Orizaba individuals of the same species are seldom in groups, while on the other mountains great areas are often covered by one species. Near the 14,000 feet level, at the foot of a cliff looking east and south, where there was an indication of moisture, we found 14 or 15 species of plants, some of which had not been seen elsewhere above 12,000 feet. Only four species extended to any distance above this cliff, they were a castelleia and a draba, both nearly stemless, and scattering bunches of two grasses, probably an agrostis and a bromus, and these were passed at about 15,500 feet. The oaks stopped abruptly just above 9,000 feet. The ylite tree, whose thick bark furnishes a valuable dye, stopped as abruptly just above the 11,000 feet level.

A thistle, with a large white blossom, was seen only above 13,000 feet. Others again, as the castelleia, had a wide range, gradually diminishing in size as the elevation increased. Between 8,000 and 9,000 feet there were nearly as many flowers in April as in July, while above 9,000 feet in July we found nearly 75 species, but in April scarcely a half-dozen were found, of which the castelleia and draba were two. In the regions below, flowers

other burrowing animals. Saw tracks of antelope and coyotes above 14,000 feet, but mammalian life did not seem to be abundant. Fish are abundant in the streams of the dry season, but during the wet season the streams are muddy torrents, containing but few fish and it is almost impossible to do successful fishing in such rapid streams, so that but few species were taken. Those taken were interesting, some of which are probably new to science. The predominant forms of life were plants, insects, and birds.

Professor Seaton collected over 500 species of shrubs and herbs between the 3,000 feet and 14,000 feet levels, and made many interesting observations as to the distribution of plant-life within those limits and the families and genera most abundantly represented by the flora of Orizaba (see Proceedings Indiana Academy of Science, 1891). The vegetation, insect-life, and birds were all we expected, but serpents, tarantulas, scorpions, centipedes, and the like, so common in pictures of tropical life, were seldom seen. We found the zoölogical altitude zones somewhat like the latitude zones, but with interesting variations, the details of which will be brought out fully in the reports from the different members of the party.

The Ascent.

The ascent of Citlaltepetl is neither difficult nor dangerous. Leaving Vera Cruz or the City of Mexico by the morning train, one reaches San Andres early in the afternoon, then by tramway, about six miles, to Chalchicomula, a little town of some 3,000 people situated on the western slope of the mountain at an elevation of about 8,300 feet. At this place, guides and horses may be engaged and other preparations made for continuing the ascent next day. A ride of six or seven hours over a steep but fairly good road for horses takes one to a cave, at an elevation of about 13,700 feet, where camp is usually made for the second night. The work of the third day is severe, and preparations should be made for a good breakfast and an early start. These items must be looked after by the tourist himself, as the guides are in no hurry, and an ordinary Mexican breakfast would not do for an American or Englishman who has a day of hard work before him. Starting early on the third morning, one rides to the foot of the moraine, near the 16,000 feet level, above which the slope is too steep for horses and the real work of the ascent begins. (See *A*, Fig. 1.)

The ascent from this point is made along a ridge which forms the eastern boundary of the glacier. In the dry season the tourist climbs slowly upward over rock and ice without danger, except such as may arise from severe exertion in the rarified air of such great elevations.

In the wet season the rocks and ice are more or less thickly covered with snow, which necessitates precautions not called for during the dry season. The eyes should be protected by colored glasses, and the face by a thick veil from the heat and light reflected from the snow, and the feet should be wrapped in coarse cloth to protect them from cold and to prevent slipping on the crusted snow. The chief guide leads the party, cutting steps in the snow for himself and followers. One might miss his footing and slide to his death on the rocks below, but the danger is not great if the instructions and example of the guide are followed carefully. It requires considerable exertion to climb steep slopes at low elevations, but when the elevation is so great that nearly or quite half the air is below, the least exertion is exhausting.

FIG. 6.—The Peak from the west at the 10,000 ft. level.

The lungs can get oxygen enough to supply the system when at rest, and one may ride from the sea-level to the 16,000 feet level without discomfort from light air. Above 16,000 feet, one not accustomed to the air of such elevations can climb but a few feet before sinking down in utter exhaustion, gasping for breath, with palpitating heart, oppressed brain, and possibly a qualmy stomach. After a brief rest the unpleasant symptoms pass away, then a little climb, then a rest, and so upward, the climbs getting shorter and the rests longer till at length the summit is reached. Some can climb faster than others; a good rule is to climb so far as possible without opening the mouth to breathe, then rest. On the average, one does well to climb 500 feet an hour.

Edward Whymper speaks of a "mountain sickness" which affected him and his assistants while exploring among the high Andes. Some of us had a little nausea, but we did not attribute it to the rarified air, and Mr. Bunsen had a severe headache while on the summit, which passed away soon after the descent began, but none of the party was affected with the mountain sickness of Whymper. No other locality on the globe affords such a full and comprehensive panoramic view as does the eastern slope of Citlaltepetl, whether seen from shipboard some 20 or 30 miles at sea, or from the summit of the mountain. The view from the summit is clearest during the forenoons of the wet season when the air is free from dust and usually clear. During the dry season a dust or haze pervades the air to an elevation of 9,000 or 10,000 feet so that objects below that elevation cannot be distinctly seen.

The descent is made to the cave or to Chalchicomula for the night, and Vera Cruz may be reached on the fourth day; thus practically making a journey from the tropical to the polar region and return in four days. Nowhere else on the earth can this be done as easily, quickly, and safely as on the eastern slope of Citlaltepetl, the Star Mountain of North America.

NOTES AND NEWS.

IN October, 1891, Thomas George Hodgkins, Esq., of Setauket, New York, made a donation to the Smithsonian Institution, the income from a part of which was to be devoted "to the increase and diffusion of more exact knowledge in regard to the nature and properties of atmospheric air in connection with the welfare of man." With the intent of furthering the donor's wishes, the Smithsonian Institution now announces the following prizes to be awarded on or after July 1, 1894, should satisfactory papers be offered in competition: 1. A prize of $10,000 for a treatise embodying some new and important discovery in regard to the nature or properties of atmospheric air. These properties may be considered in their bearing upon any or all of the sciences — e.g., not only in regard to meteorology, but in connection with hygiene, or with any department whatever of biological or physical knowledge. 2. A prize of $2,000 for the most satisfactory essay upon (*a*) The known properties of atmospheric air considered in their relationships to research in every department of natural science, and the importance of a study of the atmosphere considered in view of these relationships. (*b*) The proper direction of future research in connection with the imperfections of our knowledge of atmospheric air, and of the connections of that knowledge with other sciences. The essay, as a whole, should tend to indicate the path best calculated to lead to worthy results in connection with the future administration of the Hodgkins foundation. 3. A prize of $1,000 for the best popular treatise upon atmospheric air, its properties and relationships (including those to hygiene, physical and mental). This essay need not exceed 20,000 words in length; it should be written in simple language, and be suitable for publication for popular instruction. 4. A medal will be established, under the name of the Hodgkins Medal of the Smithsonian Institution, which will be awarded annually or biennially, for important contributions to our knowledge of the nature and properties of atmospheric air, or for practical applications of our existing knowledge of them to the welfare of mankind. This medal will be of gold, and will be accompanied by a duplicate impression in silver or bronze. The treatises may be written in English, French, German, or Italian, and should be sent to the Secretary of the Smithsonian Institution, Washington, before July 1, 1894, except those in competition for the first prize, the sending of which may be delayed until Dec. 31, 1894. A principal motive for offering these prizes is to call attention to the Hodgkins Fund, and the purposes for which it exists. Suggestions and recommendations in regard to the most effective application of this fund are invited. It is probable that special grants of money may be made to specialists engaged in original investigation upon atmospheric air and its properties. Applications for grants of this nature should have the indorsement of some recognized academy of sciences, or other institution of learning, and should be accompanied by evidences of the capacity of the applicant, in the form of at least one memoir already published by him, based upon original investigation. To prevent misapprehension of the founder's wishes, it is repeated that the discoveries or applications proper to be brought to the consideration of the committee of award, may be in the field of any science or any art without restriction; provided only that they have to do with "the nature and properties of atmospheric air in connection with the welfare of man." Information of any kind desired by persons intending to become competitors will be furnished on application. All communications in regard to the Hodgkins Fund, the Hodgkins Prizes, the Hodgkins Medals, and the Hodgkins Fund publications, or applications for grants of money, should be addressed to S. P. Langley, Secretary of the Smithsonian Institution, Washington, U.S.A.

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THE VALUE OF A WATER ANALYSIS.

BY W. P. MASON, RENSSELAER POLYTECHNIC INSTITUTE, TROY, N. Y.

A GREAT deal of popular misconception exists upon the subject of the analysis of potable water, and it is commonly supposed that such an examination may be looked upon from practically the same point of view as the analysis of an iron ore. That this belief is founded on fallacy may, however, be readily shown. When an iron ore is submitted for analysis, the chemist determines and reports upon the percentages of iron, phosphorus, sulphur, etc., found therein; and at that point his duties usually cease, inasmuch as the ironmaster is ordinarily capable of interpreting the analysis for himself. Even should the analyst be called upon for an opinion as to the quality of the ore, the well-known properties of the several constituents make such a task an easy one, and, assuming the sample to have been fairly selected, the opinion may be written without any inquiry as to the nature of the local surroundings whence the ore was taken.

A water analysis, on the other hand, is really not an analysis at all, properly so-called, but is a series of experiments undertaken with a view to assist the judgment in determining the potability of the supply. The methods of conducting these experiments are largely influenced by the individual preferences of the analyst, and are far from being uniform or always capable of comparison, thus often introducing elements of confusion where two or more chemists are employed to analyze the same water. Some of the substances reported, "albuminoid ammonia," for instance, do not exist ready formed in the water at all, and are but the imperfect experimental measures of the objectionable organic constituents, which our present lack of knowledge prevents our estimating directly.

Thus the numerical results of a water analysis are not only unintelligible to the general public but are not always capable of interpretation by a chemist, unless he be acquainted with the surroundings of the spot whence the sample was drawn, and be posted as to the analytical methods employed.

It is very common for water to be sent for analysis, with the request that an opinion be returned as to its suitability for potable uses, while at the same time all information as to its source is not only unfurnished but is intentionally withheld, with a view of rendering the desired report unprejudiced in character.

Such action is not only a reflection upon the moral quality of the chemist, but it seriously hampers him in his efforts to formulate an opinion from the analytical results.

For instance, a large quantity of common salt is a cause for suspicion when found in drinking water, not because of any poisonous property attaching to the salt itself, but because it is usually difficult to explain its presence in quantity except upon the supposition of the infiltration of sewage; yet an amount of salt sufficient to condemn the water from a shallow well in the Hudson valley, could be passed as unobjectionable if found in a deep-well water from near Syracuse, N. Y.

We thus see how important it is for the chemist to be fully acquainted with the history of the water he is to examine, in order that he may compare his results in "chlorine" with the "normal chlorine" of the section whence the sample is taken. A knowledge of the history of the water is no less important in order to interpret the remaining items of a water analysis. Some time since a water was sent from Florida to this laboratory for examination, and was found to contain 1.18 parts "free ammonia" per million.

Much "free ammonia" commonly points to comtamination from animal sources, and had it not been known that the water in question was derived from the melting of artificial ice made by the ammonia process, the enormous quantity of ammonia found would have condemned it beyond a peradventure. As it was, the water was pronounced pure, the other items of the analysis having been found unobjectionable.

Analytical results which would condemn a surface-water are unobjectionable for water from an artesian well, for the reason that in the latter case high figures in "free ammonia," "chlorine," or "nitrates" are capable of an explanation other than that of sewage infiltration. Even though such water should have, at a previous period, come in contact with objectionable organic waste material, yet the intervening length of time and great distance of underground flow would have furnished abundant opportunity for thorough oxidation and purification.

"Deep" samples taken from the same lake, at the same spot and depth, will greatly vary in analytical results if the temperature of the water at the several dates of sampling should be markedly different, owing to the disturbing influence of vertical currents.

Again, suppose it is desired to determine whether or not the water of a large stream is so contaminated with up-stream sewage as to be unfit for a town supply. An analysis of the water taken from the site of the proposed in-take would very probably be valueless, because the enormous dilution to which the admitted sewage would have been subjected would remove from the analytical results everything of an absolute character. Examinations of any real value in such cases should always be of a comparative nature. Samples should be taken above and below the point of contamination and again at the proposed in-take. If the difference between the first and second samples, which is a measure of the pollution, be maintained, or nearly so, at the point of in-take, then the water should be condemned no matter how completely the analytical results fall within the limits of the so-called standards of organic purity.

Thus it is that a chemist must be in full possession of all the facts concerning the water which he is asked to examine, in order that his opinion as to its purity may be based upon the entire breadth of his passed experience, for in no branch of chemical work is experience and good judgment better exercised than in the interpretation of a water analysis.

As Nichols has well said, "It is a great mistake to suppose that the proper way to consult a chemist is to send a sample of water in a sealed vessel with no hint as to its source. On the contrary, the chemist should know as much as possible as to the history and source of the water and, if possible, should take the samples himself."

In the taking of samples for so important a matter as a town supply, the chemist should unquestionably personally superintend their collection; but, for individual outlying waters, printed instructions have to be frequently depended upon. Those issued from this laboratory are as follows:—

DIRECTIONS FOR TAKING A WATER SAMPLE.

Large glass-stopper bottles are best for sampling, but as they are seldom at hand, a two-gallon, new demijohn should be employed, fitted with a new soft cork. Be careful to notice that no packing straw or other foreign substance yet remains in the demijohn, and thoroughly rinse it with the water to be sampled. Do not attempt to scour the interior of the neck by rubbing with either fingers or cloth. After thorough rinsing, fill the vessel to overflowing so as to displace the air, and then completely empty it.

If the water is to be taken from a tap, let enough run to waste to empty the local lateral before sampling; if from a pump, pump enough to empty all the pump connections; if from a stream or lake, take the sample some distance from the shore, and plunge the sampling vessel a foot and a half below the surface during filling, so as to avoid surface scum.

In every case fill the demijohn nearly full, leaving but a small space to allow for possible expansion, and cork securely. Under no circumstances place sealing-wax upon the cork, but tie a piece of cloth firmly over the neck to hold the cork in place. The ends of the string may be afterwards sealed if necessary.

Bear in mind, throughout, that water analysis deals with material present in very minute quantity, and that the least carelessness in collecting the sample must vitiate the results. Give the date of taking the sample, as full a description as possible of the soil through which the water flows, together with the immediate sources of possible contamination.

STARFISHES OF THE INDIAN OCEAN.

BY DR. R. W. SHUFELDT, TAKOMA, D. C.

BIOLOGICAL work of a very excellent character has within the last few years been accomplished in the Indian Seas through those employed on board H. M. Indian Marine Survey steamer, "Investigator," Commander C. F. Oldham, R.N., commanding. Much of this success is due to the labors of Mr. A. Alcock, Surgeon-Captain, I.M.S., and late Naturalist to the Survey.

Mr. Alcock has recently sent me from Calcutta a copy of his work, entitled "An Account of the Collection of Deep-Sea Asteroidea," from the region just mentioned — it being an extract from the *Annals and Magazine of Natural History* (Ser. 6, Vol. XI.) for February, 1893. From it, it would appear, that since the year 1885 many parts of the Indian Ocean, in waters varying from 100 fathoms to 1,000 fathoms and over, have been very profitably dredged by the naturalists of the "Investigator."

Mr. Alcock remarks, "A large collection of littoral and shallow-water forms [of starfishes] has also been made, but these are not here considered. If it be thought objectionable to have separated the deep-water from the shallow-water forms, it may be urged in justification that within the limits of Indian seas, so far as our experience at present goes, there is no instance of the two sections overlapping, and on another ground, that almost nothing has been published, and nothing else is promised, about the extremely interesting Asteroidea of the deeper waters of India. Of the basins into which these waters may conveniently be divided, the Bay of Bengal proper—the basin best explored by the dredge so far — gives us the smallest number of unknown species. Beyond the limits of the 30-fathom line it would seem as if the overwhelmingly muddy bottom of the bay presented conditions specially unfavorable to the existence of starfishes; and after passing this limit we usually dredge nothing until we reach true bathybial conditions in the middle of the bay" (pp. 73, 74).

On the Andaman side, however, in 561 fathoms of water, they met with *Brisinga*, and opposite to the Kistna and Godávari Deltas, in 500 to 700 fathoms, where the bottom was of a hardening clay, *Flabellum (japonicum* and *laciniatum*), *Bathyactis*, *Phormosoma*, and Spatangoids, *Pentagonaster*, again appeared. In the middle of the bay, with a bottom of accumulating *Globigerina*-ooze, the well-nigh cosmopolitan forms of *Pararchaster*, *Dytaster*, *Porcellanaster*, *Styracaster*, *Hyphalaster*, *Paragonaster*, *Zoroaster*, *Marsipaster*, *Hymenaster*, and *Freyella* rewarded the efforts of the dredger.

Peculiarly favorable to starfish-life is the enclosed basin of the Andaman Sea, which thus far, however, has only been examined up to 600 fathoms. Of twenty-one species here collected, no less than sixteen were new to science, including three very remarkable generic types. Eighteen species were dredged in the Laccadive Sea, and other very interesting localities were examined. Little, however, was added to our knowledge of the life-habits of the deep-sea starfishes, though "like some of the common reef-forms they must sometimes live in swarms, as, for instance, *Zoroaster carinatus*, of which over a score have been taken at one haul, *Pontaster hispidus*, of which about fifty have been dredged at the same time, and *Nymphaster florifer*, of which a 150 have come up on the tangle-bar."

The food of these deep-sea types seems mainly to be mollusks, prawns, and amphipods, and in some cases they gorge themselves with *Globigerina*-ooze. "A curious case of symbiosis, which has been observed too often to be a merely accidental association, occurs between *Dictyaster xenophilus* and an annelid."

Mr. Alcock's work forms a brochure of about fifty pages, with some good figures on plates, and throughout the whole he has followed the classification of Mr. Sladen, now well-known to the students of the Asteroidea, through their reading of it in those classical volumes, the "Challenger Reports," to which it was contributed.

THE USE OF POISONS AS FUNGICIDES AND INSECTICIDES.

BY L. R. TAFT, AGRICULTURAL COLLEGE, MICH.

ALTHOUGH copper sulphate has been used for many years for the destruction of the smut spores of wheat and oats, it is only about ten years since it was first employed upon fruit and similar crops as a fungicide, and for fully one-half of this period it was only used in an experimental way.

Its effects have proven so beneficial, however, that the fruit-growers, of the State of Michigan alone, will this year use several tons in combatting the various diseases that infest their crops.

The amount in time and materials expended in the use of fungicides in the United States must then reach many thousands of dollars, and it is very desirable that as much light as possible be secured upon the time and number of the applications that are necessary to obtain the best results, as well as upon the mixtures that will be most effective and economical. It has been clearly shown by many experiments that, to be most effective, the applications must be made early in the season, before the disease has obtained a foothold; but, as the number of sprayings required to hold the disease in check will depend upon such conditions as character of crop, season, and location, and the prevalence of the disease, it is doubtful if anything more than a general rule can be given, and this must be modified to suit the conditions.

Experiments have demonstrated that very small amounts of the salts of copper will destroy the spores of fungi, and have shown that the original formulæ for most of the fungicides were deficient in water, or, in other words, the mixtures were unnecessarily concentrated. Although, as now used, the strength has been greatly decreased, the limit has by no means been reached. The amount of copper sulphate in Bordeaux-mixture has been reduced from sixteen to six pounds for twenty-two gallons of water, and the experiments of the writer tend to show that for many diseases one or two pounds are fully as beneficial.

Two or three years ago most writers recommended some form of ammoniacal solution of copper carbonate, but, after a thorough trial, most fruit-growers have come to consider Bordeaux-mixture preferable to any of the ammonia-containing mixtures. The ammonia solutions were commended as being cheaper and easier to apply, but, in fact, the Bordeaux-mixture of the same strength is much less expensive; if properly strained it is not likely to clog the pump or nozzles; it is less easily washed from the plants; and it is not only less likely to injure the foliage, but it allows the arsenites to be used at the same time, thus forming a combined fungicide and insecticide, and the lime also prevents all injury from the arsenic.

For these reasons the Bordeaux-mixture is preferable, and its use should be commended.

This lime-mixture covers the plants with a sort of whitewash, and, although this is in one way objectionable, in another, from the consumers' standpoint at least, it is preferable to some of the clear solutions, which, although they contain fully as much poison, are not very noticeable upon the plants.

Fruits sprayed within a few days of the time of gathering would in one case not be saleable, and in the other, although they might have upon their surface a sufficient amount of poison to produce injurious effects, would seem above suspicion.

The results obtained from spraying various fruits with a combined fungicide and insecticide in 1892 convinced the writer that too great care cannot be taken in the use of these poisons upon all crops, any exposed portions of which are edible, and that in no case should they be used within one month of the time of ripening, while an interval of six weeks to two months will be preferable. The fruits experimented upon were strawberries, raspberries, currants, gooseberries, cherries, and pears. The experiment was conducted in the same manner with all of the fruits, and when ripe they were analyzed and tested for arsenic and sulphate of copper. The spraying was done about as in ordinary practical work, except that it was rather more thorough, the amount used being perhaps double that generally employed. Except that the raspberry and strawberry retained rather more of the poison, the results were quite similar, and those obtained with two of the fruits will answer for all.

Gooseberries sprayed June 18, 29, July 8, and 22 with Bordeaux-mixture (copper sulphate, 2 pounds; lime, 1½ pounds; water, 32 gallons) and London purple (1 pound to 200 gallons), using one-half gallon of the mixture to a very thick, full row two rods long. One pound of fruit gathered Aug. 2 gave, on analysis, .0365 grains of arsenic and .355 grains of copper sulphate. In making the analysis, the fruit was first washed in ten per cent hydrochloric acid, and the amounts of arsenic and copper sulphate thus abstracted were, respectively, .0203 grains and .208 grains, after which there remained of each .0162 grains and .147 grains.

Fruit from another row that had been sprayed in a similar manner, except that the Bordeaux-mixture was made from the usual formula (copper sulphate, 6 pounds; lime, 4 pounds; water, 32 gallons), gave of arsenic .0723 grains, and of copper sulphate .62 grains, from one pound. In each case the last spraying was eleven days previous to the date of picking.

The pears were sprayed with the same mixture as the first lot of gooseberries, on June 15, July 7, 21, and Aug. 7, and were gathered and analyzed Sept. 6, or thirty days after the last application. The result from one pound of fruit gave, of arsenic .0089 grains, and of copper sulphate .0745 grains.

The above analyses were made under the direction of Dr. R. C. Kedzie, chemist of the Michigan State Experiment Station.

Attention is called to the fact that only about one-fifth as much copper sulphate was found upon the pears thirty days after spraying as upon the gooseberries gathered eleven days after receiving the last application, also that with a weak solution as compared with a strong one, the amount both of copper and arsenic remaining upon the fruit was reduced in about the same ratio as the strength of the mixture used.

This certainly emphasizes the advice previously given, (1) to use a solution as weak as will secure freedom from disease, and (2) cease spraying with all poisons at least one month before the fruit ripens.

LIGHTHOUSE ILLUMINANTS.

BY WM. P. ANDERSON, CHIEF ENGINEER OF MARINE DEPARTMENT, OTTAWA, CANADA.

IN *Science* for Feb. 6, 1885, a sketch was given of the progress of lighthouse illumination in Great Britain and Ireland, together with a short description of the strongest lights and apparatus utilized up to that time. Since that article appeared the conflict between the advocates of electricity, mineral oil, and gas, respectively, has not decreased, nor has any settlement satisfactory to all parties yet been reached. The matter has on several occasions been brought before the Imperial Parliament, and in February last some further correspondence on the subject was laid before the House of Commons.

A consideration of some of the points lately elicited will be an interesting addition to Mr. Kenward's notes on lighthouse apparatus in *Science* for April 21 last.

The lighthouses of the United Kingdom are under divided control: the English lights are managed by the Trinity House, the Scotch lights by a board of commissioners, and the Irish lights by a separate commission; all under the general direction of the Government Board of Trade, and each anxious to maintain lights of the highest efficiency, almost regardless of cost.

The English authorities, from the observations made in 1885, are satisfied of the superiority of electric arc-lights where the highest possible power is required, and consider oil-lights the cheapest and most easily managed for ordinary purposes. The Scotch commissioners endorse this view of the case; but the Irish board seems to favor the use of illuminating gas.

The chief opposition to the decision of the English Trinity House appears to be instigated by Mr. John R. Wigham of Dublin, the inventor of the gas system. He claims that he did not get fair play in the trials of 1885, because a rule was adopted restricting the size of the lenses and lanterns within limits that prevented him from obtaining the best results from his gas-lights. Since that time he further claims that by enriching common gas with hydrocarbon a greater amount of light can be obtained from it than from the richest cannel-coal gas. Actual experiments have shown that cannel-coal gas has an illuminating power of 28 candles, nearly double that of ordinary Newcastle-coal gas, 16 candles. By passing the ordinary gas through the vapor of solid napthaline, or albo-carbon, a perfectly safe and inexpensive material, it is enriched with hydrocarbon to such an extent as to give double the illuminating power of cannel gas. He also suggests, as an improvement in lighthouse illumination, placing lenses so as to form a quadrilateral or trilateral figure, which would permit the use of lenses of much larger illuminating surface and of much longer focal distances than is possible with the 6, 8 or even 16-sided lenticular apparatus heretofore used, thereby immensely increasing the illuminating power of the lighthouses.

Mr. Wigham has had a lens of long focus made, with a bulls-eye or central portion 19 inches in diameter, and two concentric rings, one 4 and the other 4½ inches wide, giving a total diameter of 36 inches, all in one piece. This is surrounded by a belt of prisms 2 feet 10 inches wide, consisting of ten rings, outside of which is a third portion consisting of eight rings of totally reflecting prisms, partially surrounding the second portion, so as to complete a lens about 10 feet 10 inches wide by about 8 feet high. In the focus of this lens is placed an "intensity" burner composed of 148 fish-tail jets, grouped to burn the enriched gas, which, when lighted, forms a solid flame of 14 inches diameter by 6 inches high. The illuminating power of the burner is calculated to be about 8,500 candles, which should give an actual intensity of light through the lenses of about 2,300,000 candles. Experiments made with this apparatus showed splendid results at a distance of 6½ miles. In full moonlight the beam cast a strong shadow, and was very large and dazzlingly bright, reducing a neighboring first-order fixed light to what seemed by comparison a remote and feeble glimmer.

The case for and against gas as a lighthouse illuminant seems to be as follows: Its advantages are facility in increasing or decreasing the power of the light to suit the various states of the atmosphere, and also speed and sharpness in eclipsing lights by cutting off the supply of gas, and thus occulting them while at the same time saving the illuminant; as well as the fact that where gas is used for illumination it can be utilized at a minute's notice to operate a gas-engine in connection with a mechanical fog-alarm, while with any other source of power delay must occur in putting the fog-alarm into operation. It is further claimed that the large size of the gas-flame, giving an unusual number of extra-focal rays, has a better effect in illuminating a large area of fog, and consequently makes the light more readily visible.

The weak points of gas are the difficulty of manufacturing it at some isolated stations, and also the necessarily large size of the flame, which involves the use of very large lenses, and a long focus, to prevent a wasteful distribution of extra-focal light.

The arrangement of illuminating apparatus proposed by Mr. Wigham for a most powerful light is a battery of four giant lenses, surrounding a central burner, intensified by having similar lenses with additional burners arranged one over the other in three tiers, or "in triform." To accommodate such an apparatus would require a lantern with glazing at least 20 feet in diameter by 24 feet high. The lenses alone would cost £8,400, an expenditure which would only be justified by the necessity for an exceptionally powerful light.

Mr. D. A. Stevenson, Engineer to the Northern Lighthouse Board, in a report on electric light as an illuminant, claims that the complaints against the penetration of this light in fogs are not well founded, and that many criticisms of its power are due to prejudice, partly owing to the persistent way in which it is decried as a lighthouse illuminant by certain writers to the press, partly from a misunderstanding of the fact that, being very rich in the most refrangible rays of the spectrum, that is, very white, it suffers a greater percentage of diminution in passing through fog than oil or gas light, which is redder, but nevertheless, owing to its enormously greater initial power, the electric light is always a better penetrator of fog than the others. He claims that sailors, on their ordinary courses, are never in a position to form an opinion of the subject that is worth anything, because they cannot see different lights in the same conditions of atmosphere. He adduces observations made by keepers in his service on each other's lights, which go to prove that the electric light is in all cases the more powerful. These are observations from one station burning an oil light to another electrically lighted, and the reverse Three pairs of such stations are instanced; in every case the electric light being visible in fog that totally obscured the oil lamp.

THE COLLECTION OF FOSSIL MAMMALS IN THE AMERICAN MUSEUM OF NATURAL HISTORY, NEW YORK.

BY HENRY F. OSBORN, COLUMBIA COLLEGE, NEW YORK CITY.

THE third expedition from the Museum is now in the field, and the collections of fossil mammals made under the direction of Dr. J. L. Wortman during the summers of 1891 and 1892, are being rapidly prepared for exhibition upon the geological floor of the museum. The first year's work was in the Wahsatch beds of the Big Horn Mountains, a country which had been very thoroughly explored for Professor Cope. This yielded rather disappointing results, although exceptionally fine material of *Coryphodon* was procured, including very considerable portions of the skeleton, which will soon be mounted for exhibition in the museum. The most unique discovery in this horizon was the skull of *Palæonictis*, an ancient carnivore which has hitherto been represented only by two lower jaws found in the Suessonian of France, the horizon contemporary with the Wahsatch.

Early in 1892 Dr. Wortman, accompanied by Mr. Peterson, who had been for several years on the U. S. Geological Survey, started into the Puerco or basal Eocene beds of northern New Mexico, and by the most energetic and careful search in fields which had also been explored for Professor Cope, succeeded in procuring a very valuable collection of these Lower Eocene types. Among the most unique specimens of this series are the upper and lower jaws of *Polymastodon*, a large-sized successor of the ancient *Plagiaulax* of the Middle Jurassic beds. Another discovery was the skull of *Pantolambda*, an ancestor of Coryphodon. Altogether nearly five hundred specimens were shipped East from this tour. The party then went into the Laramie, in search of the Triceratops, but were unsuccessful. They secured later in this horizon a large collection of the minute teeth of the Cretaceous mammals, which is paralleled only by that in the U. S. Geological Survey collection.

The richest results obtained thus far, however, are from the White River Miocene of South Dakota. Here the beds are 800 feet thick, and a thorough exploration was made from the bottom series in which the huge *Titanotherium* is found, to the top in which the new forms *Protoceras*, *Artionyx* and *Aceratherium tridactylum* were found. These top beds were practically a discovery, for nothing has been recorded from this stratum before, excepting the skull of a female *Protoceras*, which is in the U. S. Geological Survey collection. The male *Protoceras* presents four pairs of protuberances upon the skull, the most exceptional being the large vertical plates upon the maxillaries. This White River Miocene is the classic ground of Leidy's memoirs, but in these and by far the greater part of the literature of this horizon, the animals only of the so-called "Oreodon" stratum have been described, together with the forms from the lower "Titanotherium" stratum. This has been due to the fact that these strata at once attract the ordinary collector by the profusion of bones which are washed out from them. An intervening stratum between the "Oreodon" and "Titanotherium" layer, appears, also, to have been generally overlooked, because of its unpromising exterior. Mr. S. Garman, collecting for the Museum of Comparative Zoölogy, some years ago secured one specimen of the very unique Rhinoceros-like form, *Metamynodon*, the type specimen and the only one which has hitherto been known. Dr. Wortman directed his attention, therefore, especially to the location of this stratum, and succeeded in finding a seam about thirty feet in thickness, which proves to be especially characterized by abundant remains of Metamynodon. The party secured four or five skulls, and one nearly complete skeleton. This animal is distinguished by huge canine tusks in the anterior portion of the head, which give it an appearance quite different from that of the rhinoceros; in fact, the skull and skeleton are entirely peculiar, and unlike any perissodactyl which has been found hitherto. Yet this animal flourished in the midst of large herds of true rhinoceroses, for the diligent search made by the museum party has resulted in the discovery of a whole series of hornless rhinoceroses, from the bottom of these beds to the top. They increase gradually in size, and in the evolution of the teeth, in the loss of the lateral fifth toe in the fore foot, and reach a culminating point in the new species, *Aceratherium tridactylum*. As the name indicates, this species is mainly characterized by the presence of but three toes in the fore foot. It is represented in the museum collection by one of the most remarkable specimens which has ever been found. This is a complete skeleton from the tip of the nose to the tip of the tail, lacking only the fore limb of the left side, and a few of the ribs and sternal bones. It is over seven feet long and four feet high, and has been mounted upon a large panel of sandstone and plaster, giving the impression that it has been simply hewn out of the matrix. The animal appears to be of about the same size and proportions as *Ceratorhinus* or the rhinoceros of Sumatra; in fact it has very nearly the same proportions and form, except that it lacks the small horns upon the nasals and frontals. Among American species its affinities are with the *Aphelops megalodus* Cope of the top of the Miocene.

A third specimen of note is the hind foot of *Artionyx*. As Leidy called Oreodon a ruminating hog, so this animal might be called a clawed hog, for the foot closely resembles that of the pig or peccary, until we reach the phalanges, which have articulations and large terminal claws somewhat similar to those seen in the bears, while the ankle-joint is of the artiodactyl type, and the four toes are set in pairs on either side of the median line, there being also the rudiment of a fifth. The name given this fossil refers to its combination of the artiodactyl and unguiculate character. This is possibly a relative of the clawed Ungulate — *Chalicotherium* — which presents such a remarkable combination of characters, and is now known to have been distributed over North America, Europe, and Asia, during Miocene times. The contrast between these two types is very striking; for while Artionyx combines an artiodactyl foot with uncleft claws, Chalicotherium combines a perissodactyl foot with cleft claws. One of the most interesting problems of the future will be clearing up the relations between these two forms and their relations to other groups.

CURRENT NOTES ON ANTHROPOLOGY.—XXVII.

[*Edited by D. G. Brinton, M.D., LL.D.*]

Theories in Criminal Anthropology.

Two articles which appeared almost simultaneously in February last present with sharpness and brevity the conflicting views of the two leading schools of criminal anthropology.

One is by Dr. Sorel, in the *Revue Scientifique*. It is a warm defence of the doctrines so strenuously urged by Professor Lombroso, and which were substantially repudiated at the Congress of Brussels last year (see *Science*, Nov. 18). Sorel maintains that the opponents of Lombroso did not understand his assertions, and that they confused the discussion by introducing speculative questions as to the abstract nature of crime, quite out of place in a study in natural history; and much more to the same effect.

The other paper is by the late Professor Meynert, and is printed in the Mittheilungen of the Vienna Anthropological Society. It is principally occupied with a refutation of Lombroso's assertion that genius is a pathological development, or the result of such; but also attacks his theory of crime as attributable to a degeneration of the brain and a reversion to an atavistic condition of the race. Several serious errors in Lombroso's method of handling statistics are pointed out; as, for instance, his neglect of the fact that the depraved physique of the criminal is owing to his unhygienic surroundings, and to attribute his criminality to such physique is to confuse concomitant with cause. Again, in comparing criminals with wild beasts, he confounds the methods of natural history with that of judicial procedure.

A careful reading of the two articles will prove entertaining.

A Chemical Test of the Antiquity of Bones.

The effort was made by M. Adolphe Carnot last summer, in a paper read before the Academie des Sciences, Paris, to establish a chemical measure of the antiquity of bones. He claimed that this is shown by the amount of fluorine they contain. Its relative proportion increases as the bones are older. Representing the maximum by 1, modern bones show but .06, those from the old quaternary strata .35, those from the tertiary .64. Hence, when human and other bones are found in the same strata, and the question whether they should be assigned to the same age arises, analysis is claimed to offer a solution; and M. Emile Rivière had recourse to it as the crucial test in the disputed age of some human bones found along with those of extinct species in the gravels near Billancourt, on the Seine; proving, he believed, that the human bones were intrusive and late.

It seems to me, however, that this test, which I learn about from an abstract in the Journal de l'Alliance Scientifique, March 15, is open to some serious risks.

Not only do the inorganic constituents of bone differ largely in the different osseous tissues of the same skeleton, but they notoriously vary greatly at the different epochs of life. According to the analyses of Heintz, the fluoride of calcium in the average femur of an adult is about 3.5 of its inorganic constituents. Where the proportion in ancient bones differs notably from that in modern, how can we decide what part of it is owing to post mortem changes conditioned on the quality of the soil, the amount of percolation, the length of exposure before inhumation, and the like incidents? While it would be most desirable to have at hand a positive chemical test of antiquity, we must hesitate to accept as conclusive one which seems exposed to be influenced by these precarious conditions.

Cave-Hunting on the Mediterranean.

Shortly after leaving the French frontier on the road which leads from Marseilles to Genoa, the track penetrates by a tunnel the Baoussé Roussé, or Red Rocks, the sea front of which is perforated with natural caverns looking out on the blue Mediterranean. They have furnished rich mines for the archæologist, as they were selected by the earliest of the human race who dwelt there as favorite resting-places for both the living and the dead. Fresh discoveries were made in one of the grottoes in February, 1892, of which a note will be found in *Science*, July 26, 1892, giving the opinion of J. Vaughan Jennings. A still more elaborate study was made by Dr. Verneau, which appears in *L'Anthropologie*, 1892, No. 5. His conclusions, briefly, are that the three skeletons found side by side were an interment dating from a period intermediate between the quaternary and the neolithic epochs; but it had been made in strata containing traces of an older and different industry, which could properly be called quaternary.

Some excavations of MM. Fournier and Rivière, published in *Le Naturaliste*, Feb. 15, 1893, revealed a station of the Magdalenian epoch in a rock-shelter at La Corbière, near Marseilles, and probably as ancient a relic-bearing stratum as has been found in that district.

A curious fact about it was that at the remotest corner of the small grotto was a skeleton, the bones in place, but with no signs of interment, and no funerary objects. Evidently the corpse had been left to decay where the man breathed his last. Either he lived alone, or the others had deserted the grotto on his death. The authors refer to another such instance in another shelter. Probably in these, we see the signs of that horror of death which is one of the earliest prompters of the religious instinct. Tribes are known to history who deserted the dwelling and the corpse within it, when the owner died.

Researches in Early Aryan Ethnology.

One of the most earnest students of the early Aryan tribes is Professor Wilhelm Tomaschek, of the University of Vienna. In a late number of the Mittheilungen of the Anthropological Society of that city he discusses with profound erudition the relationship of the ancient Illyrians and Thracians.

In its first paragraphs he declares himself a believer that the primitive Aryan speech developed itself in Europe, wholly uninfluenced by either Semitic, Coptic, or other affiliations. From an extended comparison of the relics of ancient Illyrian and Thracian — principally proper names — he reaches the conclusion that the east European group of Aryan tongues should be divided into two sub-groups, the one including the Thracian, Phrygian, and Armenian, the analogies of which are with the Celtic and Italic dialects of western Europe; the other comprising the Slavic and Illyrian idioms, whose analogies are with the Lithuanian of the Baltic. The modern Albanian is a true descendant of the Illyrian, though it has suffered much decay, and also presents a number of non-Aryan radicals, which, the author ventures to suggest, survived from the pre-Aryan Ligurian speech of the locality. The Veneti of northern, and the Iapyges of southern Italy belonged without doubt to the Illyrian stock. The Thracian language itself, a pure Indo-Germanic tongue, became entirely extinct; but the author announces the near publication of a work in which he has collected all known relics of it as preserved in epitaphs, inscriptions, and proper names.

The Study of Folk-Tales.

A valuable addition to the science of folk-lore is a work just published by the English Folk-lore Society, from the pen of Miss Marian R. Cox. It is a volume of 535 pages, a monograph on the tale of Cinderella, giving 345 variants, with abundant notes and discussions of analogous narratives from all parts of the world and all periods of history.

An introduction is contributed to the work by Mr. Andrew Lang, in which he endeavors to present what he now believes to be the true explanation of such analogies, carefully refuting various opinions on the subject which he is generally believed to have endorsed. Mr. Lang was once president of the Folk-lore Society, and though he has announced that he has given up that for more lucrative pursuits, his opinions are much respected. He is generally understood to have explained such analogies by the convenient word "chance," and to have been the adherent, if not the parent, of the "casual" theory in folk-lore. Certainly he has denied all definite meaning and all real content in primitive myths. He now modifies these positions by explaining the analogies as based on "the universally human," or else on "common customs." He believes in transmission, "where the incidents are numerous and the sequence exact"; which, indeed, is the only resource for one who, like Mr. Lang, can see nothing in native myths but "the obscene or puerile stories of savages."

Fortunately, we are not driven to take refuge in such vague phrases to explain the striking parallelisms of human thought and expression in tribes far apart in space and time. The scientific explanation of them is based on two factors; first, the fixed laws of the human imagination; and second, the objective reality of the sequences which are symbolically set forth in the narratives. The late story is often the ancient nature myth, decked out by personification and poetry, but still true to those sequences which objectively are ever and everywhere realities.

As for the imagination, what is it but a faculty operating under laws as rigid as those of physics? As the distinguished ethnographer, Von Hellwald, remarks: "In spite of the endless multiplicity of forms, yet often one and the same or very nearly allied forms recur in localities widely asunder, and this seems to occur most frequently in forms which are peculiarly strange and artificial. We are almost forced to accept the discouraging suggestion of Peschel, that the human faculty of thought is a mere mechanism, which under a given stimulus is always forced to perform the same motion."

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.

A Physiological Effect of Cave Visiting.

Dr. Hovey's interesting account of a visit to the Mammoth Cave in March, published in *Science* for April 7, 1893, recalled a recent conversation with my father, Dr. C. Fayette Taylor, on the subject of the cave, which he visited in July, 1860. He was particularly struck with, and vividly describes, the physiological effects experienced on emerging from the cave. He made the usual long trip with some fifteen companions, reaching upper air after a stay of about twelve hours under ground. On emerging the sense of smell was intensified to such an extraordinary degree, that most common objects, such as trees, plants, animals, and even people had strong individual odors, mostly unpleasant; about half the party were strongly nauseated and vomited. One tree could easily be distinguished from another by its characteristic odor. This effect lasted about half an hour and then passed off. The guides told him that this was a usual experience. Dr. Hovey alludes to this effect of a sojourn in the cave in a lecture published in the Bulletin of the American Geographical Society, March 31, 1891, in the following words: "By contrast with the pure oxygenated air of the cave, the odors of the outside world, of the trees, grass, weeds, and flowers, are strangely intensified and for many delicate natures overpowering." In a letter dated April 11, 1893, Dr. Hovey says: "I have always, or generally, been accustomed to rest at the entrance on emerging, for the reason that neglecting this precaution is apt to be followed by disagreeable consequences. I have know visitors to suffer from nausea and headaches by reason of a too sudden change from the peculiarly pure air of the cave to that of the outside world. The sense of smell is greatly intensified in almost every case."

I judge that this intensification of olfactory perceptions is due to the rarity of olfactory stimuli in the cave; on emergence, in accordance with a physiological law, the perceptive powers for these particular stimuli, having rested, are intensified, so that odors too delicate to make an impression under ordinary circumstances are powerfully felt. By the constant repetition of the ordinary olfactory stimuli this effect passes off, and soon only the stronger odors are registered in consciousness. In other words, consciousness is mainly concerned with the registration of the contrast between the stimulus of the moment and a background of fused and undifferentiated impressions. Ordinarily, sensations are increased by more intense stimulation, but they may also be increased, as in the illustration just given, by varying the background so as to bring ordinary stimuli into stronger relief. That a similar effect has been intensified by heredity is illustrated by Dr. Hovey's remarks on the auditory sensitiveness of the cave fauna. He says in the lecture already referred to: "The tiny [blind] fish are colorless, having cartilage instead of bones, are viviparous, and are so sensitive that if a grain of sand should fall on the water they would dart away with rapidity. Blind crawfish are also found here, whitish, semi-transparent, with remarkably long antennæ and more delicate in every way than those found in outside streams. These also are highly sensitive and not easily captured."

This agrees with an observation of Professor Cope, quoted in the "Standard Natural History," Vol. III., p. 173. He says the Amblyopses, when swimming near the surface, as is their habit, are "easily taken by the hand or net, if perfect silence be preserved, for they are unconscious of the presence of an enemy, except through the sense of hearing. This sense is, however, evidently very acute, for, at any noise, they turn suddenly downward and hide beneath stones, etc., at the bottom."

Henry Ling Taylor, M.D.

New York.

Pre-Historic Remains in America.

If Professor Thomas, in *Science*, May 5, had really desired to inform readers what my conclusion was in reference to the original home of the Mexican or Uto-Aztecan stock, he would have quoted, not various fragments from earlier studies, but the following from "The American Race," p. 121: "That very careful student, Mr. George Gibbs, from a review of all the indications, reached the conclusion that the whole group came originally from the east of the Rocky Mountain chain, and that the home of its ancestral horde was somewhere between these mountains and the Great Lakes. This is an opinion I have also reached from an independent study of the subject, and I believe it is as near as we can get to the birthplace of this important stock."

What I said of the Mayas was: "The uniform assertion of their legends is that the ancestors of the stock came from a more northern latitude, *following down the shore of the Gulf of Mexico.*"

If Professor Thomas can controvert either of these propositions, I shall be glad to change my views to his.

As for his assertion that I "ought to know" that the shells and copper ornaments found in Tennessee and Georgia "are looked upon by all archæologists as puzzling objects because of their remarkable departure from the types of the Atlantic slope," I certainly know nothing of the kind, nor does Professor Thomas. Only last summer that most competent archæologist, Dr. E. Seler, published an article to show that these very objects are so little of a departure from historic Atlantic types that the theory of a relationship to Maya art is in his opinion unnecessary (see *Science*, Nov. 4, 1892).

If Professor Thomas had made himself acquainted with the current literature of American archæology, he would not have risked such a statement. D. G. Brinton.

Philadelphia, May 8.

Tornadoes.

About five o'clock of the evening of April 24, a peculiar wavy appearance was noticed in the clouds, which were moving north. Every few minutes one or more miniature tornadoes would appear. The little funnels would last twenty or thirty seconds, others formed only to be destroyed shortly afterwards.

The whole time was about fifteen minutes, when the upper layers of clouds became more or less mingled with the lower layers. The barometer had been falling all day. The same evening there were two destructive tornadoes in Missouri and a heavy wind-storm at Paxton, Ind. E. M. Danglade.

Vevay, Ind., April 29.

Pivotal Sounds in Recollection.

In 1884 I published the statement that in the endeavor to recall some forgotten word or name that a remarkable tendency existed to substitute another word or name having, somewhere in its construction, a letter corresponding to one in the desired word or name. For example, Cavendish suggests itself, or rather may do so, when one is trying to recollect Van Antwerp, and so on; the V being the pivot upon which both names revolve, apparently, in the memory. In addition to this I find, at least in my own experience, an inclination to swing these memory efforts around the R sound more frequently than with other instances; for example, for many years, and slightly to this day, I hesitate in naming Dearborn and Randolph Streets. Of course, any one living upon either of these streets would soon overcome such confusion through one name appearing oftener than the other in use.

The knowledge of this disposition has enabled me sometimes to recover the proper word by taking other words with the same "pivotal" letter, or sound, regardless of their sequence in spelling the word sought. S. V. Clevenger.

Supt. Ill. East. Hospital for Insane.

Singing of Birds.

In reply to a query by E. B. Titchener (*Science*, April 7) with regard to the expression of emotions in the singing of birds, I have a few notes. A song-sparrow, *Melospiza melodia*, with a broken leg past mending, was kept in our house in a cage about a year and a half, fed, bathed, otherwise cared for and occasionally allowed the freedom of a room. A happier, merrier fellow, I never saw. He sang early and late, nearly the year round, moped a few days and died. The taxidermist said he was much wasted in flesh, and had lived as long as he could. He was kept as comfortable as possible, and his song seemed purely an expression of happiness. Mary B. Moody.

Fair Haven Heights, New Haven, Conn., May 2.

Photographs of Botanists.

Your botanical subscribers and readers most likely will be interested in the collection of photographs of about 150 American botanists and a small number of foreign botanists, that Michigan State Agricultural College is displaying in the Departments of Liberal Arts at the Columbian Exposition.

I hope still others of the "fraternity" will be willing to add a cabinet-sized picture of themselves to a supplementary list, to gratify their friends. W. J. Beal.

Agricultural College P. O., Mich., May 3.

BOOK-REVIEWS.

Coal-Pits and Pitmen: A Short History of the Coal Trade and the Legislation Affecting It. By R. Nelson Boyd. London, Whittaker & Co., 1892. 256 p. 12°.

In this volume, which is an enlarged edition of a paper published under the title of "Coal-Mine Inspection: Its History and Results," the author has gathered a great number of facts relative to the subject. In one chapter he gives an account of the condition of the colliery population during the last century, which is not a pleasing one. The harsh methods of treatment led to many strikes and great destruction of property. The men were at first practically slaves, but an act of Parliament passed in 1775 and another in 1799 did away with the system of bondage, although with little benefit to the men at first. Subsequent acts have mitigated the rigors of their condition and protected them from the rapacity of mine owners and overseers.

The history of the coal trade is treated of in considerable detail, and mention is made of early explosions and means of ascertaining the presence of fire-damp. The early machinery, of a very primitive character compared with modern appliances, is also described. The investigations of one of the various Parliamentary committees show the condition of the colliery operatives in 1833. In referring to this subject, Mr. Boyd states that, "The children were frequently beaten by the men for whom they worked; so much so, that 'they seldom slept with a whole skin.' Besides this, their backs were cut with knocking against the roof and sides of the roadway, and their feet and legs covered with sores and gatherings, owing to the water. The children, boys and girls, earned their wages by drawing the coals in tubs along the galleries by means of a belt and a chain passing between the legs. Many girls were thus employed, and after a time became crooked and deformed. From the nature of the work they soon became as rough and uncouth as the men and boys, fighting and swearing like them."

Considerable attention is given to colliery explosions, and the

CALENDAR OF SOCIETIES.

New Mexico Society for the Advancement of Science, Las Cruces, N.M.

April 6.—F. C. Barker, The English Form of Government; C. H. Tyler Townsend, Life Zones of the Organ Mountains in Southern New Mexico.

Anthropological Society, Washington.

May 2.—Henry Gannett, Estimates of Wealth; Wm. T. Harris, The Great Benefit to the Public of the Estimates of Wealth; Anita Newcomb McGee, Transmission of Congenital Deformity; J. D. McGuire, The Evolution of Stone Working.

May 9.—J. N. B. Hewitt, Common Errors in Regard to Indian Language; H. E. Warner, Primitive Belief in a Future State: a Comparative Study; F. A. Seely, The Pivot Point in Modern History: Andrew Palaeologus at Barcelona; Thomas Wilson, Fourth Centenary of the Discovery of America, at Madrid, 1892.

Geological Society, Washington.

May 10.—Walter H. Weed, The Post-Laramie Beds of Montana; J. S. Diller, The Tertiary Revolution in the Topography of the Pacific Slope.

Philosophical Society, Washington.

May 13.—E. D. Preston, Remarks on the Method of Reducing the Waikiki Observations for Changes of Latitude—Results; F. H. Cushing, Ancient Pueblo Arches; Cleveland Abbe, The Formation of Rain; G. K. Gilbert, The Average Temperature of the Earth.

long list of these is simply appalling. In spite of many improvements, their number even in recent years is not small, although, as a rule, they are not quite as disastrous as they formerly were. Over 5,500 lives have been lost from this cause in England alone between 1850 and 1889. We can commend the volume to all persons interested in the subject of coal and coal mining.

J. F. J.

The Student's Handbook of Physical Geology. By A. J. JUKES-BROWNE. Second edition, revised. London and New York, G. Bell. 1892. 666 p. 8°.

THE breadth of view expressed in the preparation of this excellent text-book makes it more worthy of notice in an American journal than most English books are. Its illustrations, both verbal and graphic, are of course largely British; but so many examples are taken from other parts of the world that its insular origin is not prominent. Its various chapters suggest many of the newer points of geological view; its style is simple and easy, inciting the student to further reading than the end of his lesson. As is the case with most text-books on geology, the treatment of the chapters on sedimentary deposits and on the origin of valleys shows clearly how much later an understanding was reached in the latter than in the former subject. Stratified rocks are described as if they were manifestly the product of aqueous deposition; but the origin of valleys by aqueous denudation is carefully argued out, with a series of proofs. Buried valleys are mentioned before drowned valleys, although the natural order of occurrence is the other way. From my own greater interest in physiographic geology than in the other chapters of the book, my attention is naturally directed towards that part of the subject, especially as the author recognizes it as a primary division of the science. While its treatment is greatly in advance of that which it commonly receives, it still leaves something to be desired before it shall be commensurate with its importance and with the treatment of other equally important but more attentively considered chapters. For example, it is implied that anticlines are normally transformed into valleys, and synclines into mountains; while it is easily shown that this transformation is not dependent on the attitude but on the hardness of the beds involved in the folding and on their relation to base-level. Again, in the discussion of sub-aerial denudation and the origin of valleys, no reference is made to the completion of the task of valley-making in the base-levelling of the region; and plains of denudation are referred to only as a product of marine erosion.

The book may be warmly recommended for the reference shelves of school libraries, and until the variety of American text-books of geology is increased, it will doubtless share with Geikie's smaller "Geology" a place in our schools and colleges.

W. M. D.

ONE of the largest cases in the Century Company's room at the World's Fair is devoted to an exhibit of "how a dictionary is made." Beginning with a copy of the very earliest English dictionary, Bullokar's "English Expositor," printed in London in 1616, a half-dozen of the important dictionaries of the past are shown, up to Bailey's, Johnson's, and the Imperial, the latter of which was the basis of the Century Dictionary. The exhibit includes a copy of the edition of Bailey's which was the first to include cuts, or "engraven schemes," as they are called on the title-page. In order to picture the growth of the language, especially in scientific lines, each book is open at the words beginning with "micro," of which in the first dictionary there is but one word, "microcosmus," while in the Century there are eight pages of the compounds of "micro." These eight pages, from the first manuscript, through the various proofs (showing additions and corrections) up to the finished dictionary, form the exhibit, with the addition of plates, original pictures, engravings on wood, and the manuscript and proofs of the word "take." With the latter are the quotations and definitions, used and unused, handed in by readers. The entry under "take" occupies about twelve columns in the dictionary, but it will be seen that not more than half of the material gathered was finally used.

Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

I have 500 microscopic slides to exchange in lots to suit. Want Kodak, first-class field-glass or scientific books. A. C. Gruhlke, Waterloo, Ind.

Texas Plants. I will collect sets of plants represented in this region of Texas, either for sale or for exchange for literature, for anyone who will send me an order in time to fill it. Plants to be ready complete in November. C. F. Maxwell, Box 174, Dublin, Texas.

For sale or exchange—A Telescope (36 diameters, copper barrel)—for $20 cash or scientific books of that value. A. N. Somers, La Porte, Ind.

For sale—A complete set of the Reports of the Second Geological Survey of Pa., 1874–1893, including the Grand Atlas. Publisher's price over $115. Will sell for $50. Address F. D. Chester, Newark, Del.

The undersigned has skins of Pennsylvania and New Jersey birds, as well as other natural history specimens: which he wishes to exchange for marine, fresh water, and earthworms of the South and West. Correspondence with collectors desired. J. Percy Moore, School of Biology, University of Pennsylvania, Philadelphia.

For sale or exchange.—I have a Caligraph typewriter (No. 2) in perfect order and nearly new. It is in a heavy leather, plush-lined office case, the whole costing me about $100. I desire to obtain for it, either by sale or exchange, a new, No. 5 "Kodak" camera, with six double feather-weight plate-holders and the latest pattern of their tripod. The lens and pneumatic time-shutter must also be the same as those now sold with the last No. 5 Kodak. The price of what I desire in exchange is $78. Address, for particulars, P. O. Box 314, Takoma, District of Columbia.

For sale.—An Abbe binocular eye-piece for the microscope. Alfred C. Stokes, 527 Monmouth St., Trenton, N. J.

For sale or exchange.—One good long range Remington B. L. rifle, 44 calibre, also land and fresh water, and marine shells. Want shells, Safety, camera or printing press. A. H. Boies, Hudson, Mich.

For sale—A Zentmayer new model U. S. Army Hospital monocular stand. Price $110, will sell for $75. Address H. C. Wells, No. 151 Broadway, New York.

Wants.

WANTED, as principal of a flourishing technical school, a gentleman of education and experience who will be capable of supervising both mechanical and common school instruction. Special familiarity with some technical branch desirable. Address, giving age, qualifications, etc., J. B. Bloomingdale, Fifty-ninth street and Third avenue, N. Y.

WANTED.—A young man as assistant in our microscopical department. Queen & Co., Philadelphia.

THE undersigned desires specimens of North American Gallinae in the flesh for the study of their pterylosis. These species are especially desired: *Colinus ridgwayi, cyrtonyx montezumae, deudragapus franklini, lagopus welchi, tympanuchus cupido* and *pedioecetes phasianellus.* Any persons having alcoholic specimens which they are willing to loan or who can obtain specimens of any of the above are requested to communicate with Hubert Lyman Clark, 3922 Fifth Avenue, Pittsburgh, Pa.

A COMPETENT TEACHER of botany in college or university is open to engagement. Address L., Box 86, Rochester, Mich.

CAN any one inform me as to the age to which cats have lived? I have one twenty years old. Edward D. Webb, 132 W. Eighty-first St., New York.

WANTED — Second-hand. Foster's Physiology, Balfour's Comparative Embryology, Claus & Sedgwick's Zoology, Flower's Osteology of Mammalia, Vine's Physiology of Plants. Please state editions and prices asked and address Richard Lees Brampton, Ontario, Canada

A GRADUATE ENGINEER will give instruction evenings in geometry, trigonometry and surveying, mechanics, physics, mechanical drawing and general engineering construction. Five years' experience in field and editorial work on engineering journal. References furnished. C. S. H., 102 Tribune Building, New York.

SCIENCE

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SCIENCE

NEW YORK, MAY 19, 1893.

MR. HOLMES'S CRITICISM UPON THE EVIDENCE OF GLACIAL MAN.

BY G. FREDERICK WRIGHT, OBERLIN, OHIO.

MR. HOLMES has now concluded his series of reviews of the evidence of glacial man in America, having treated of the evidence from Trenton, N.J., and of that from Madisonville and Newcomerstown, Ohio, in the first two numbers of *The Journal of Geology*, published at Chicago, and of the Little Falls evidence, in Minnesota, in the April number of *The American Geologist*. It is, therefore, an appropriate time to make some remarks upon his criticisms. This I will do with as much freedom from prejudice as possible, and I think I am in position to be as free from bias as one can well be; for all along I have been in a strait betwixt two, being under pressure from my theological predilections to discredit the evidence, and accepting it at first with much misgiving.

A calm review of the case in the light of Mr. Holmes's criticism seems to make it probable that we have been mistaken about the character of Miss Babbitt's discoveries at Little Falls. Mr. Holmes seems fairly to establish the probability that the discoveries there made were either in the surface deposits or in a talus of the bank which had fallen down from the surface. But I will leave this for further discussion by those who are more familiar with the ground.

In case of the discoveries at Trenton, N.J., however, his criticisms fall far short of discrediting the abundant evidence that had been presented by other investigators, and this I say with what I believe to be pretty full knowledge of the facts and conditions connected with the discoveries—knowledge which I have derived from numerous personal investigations upon the spot and from frequent conferences with persons who have from time to time reported discoveries. But, as the discussion of this evidence in detail will more properly fall to some others who have more immediate cognizance of the facts, I will do nothing more here than simply to express the convictions of my mind after repeatedly reviewing the evidence on the spot since his criticisms.

The last paper of Mr. Holmes, however, treats of the reported discoveries in Ohio, whose discussion more properly falls upon me. The two discoveries upon which most reliance has been made in Ohio are that by Dr. Metz, at Madisonville, in the glacial terrace of the Little Miami River, and that of Mr. Mills, at Newcomerstown, in the glacial terrace of the Tuscarawas. Mr. Holmes urges two objections to the glacial age of the implement discovered by Dr. Metz at Madisonville, and with him I understand Mr. Leverett to agree. The implement was found some distance back from the margin of the terrace, where the material was finer than that facing the river, and occurred eight feet below the surface of the loam, in the upper part of the gravel. Mr. Leverett suggests that this loam may have been deposited later than the main part of the terrace. I do not, however, understand him to have any direct evidence of this, but simply to suggest it as a possibility. I am confident, however, that it is nothing more than a bare possibility, and that any separation of that portion of the terrace from that nearer the river is in the highest degree improbable. The glacial terrace is continuous from the river to Dr. Metz's house, and, according to the laws of the formation of such terraces, the finer material would be deposited back from the main stream in exactly the manner in which it is deposited there. We may therefore reject that supposition with a very great degree of confidence.

Second, Mr. Holmes and Mr. Leverett suggest that this implement may have worked down eight feet through the loam and into the gravel by the agency of upturned trees, or of the rotting tap-roots of oak trees. Professor Chamberlin has suggested to Mr. Leverett that probably fifty generations of trees had grown upon this spot. But it is difficult to see how the number of the generations of trees growing upon the spot would materially affect the question. The most that Mr. Holmes claimed in reference to the Little Falls locality was that implements might have worked down by the upturning of trees three or four feet into the surface soil. But fifty disturbances of the soil to a depth of three or four feet would not have the effect of one disturbance of eight feet. To go half-way fifty times does not produce the effect of going the whole of the way once. The supposition of the implement's having worked down through a tap-root as it decayed seems to rest upon so slight a probability that it is scarcely worthy of consideration. The necessity of resorting to such hypotheses to explain away each item of proof in detail will impress most reasonable minds with the extreme difficulty of resisting the evidence presented in favor of glacial man in America.

With reference to the Newcomerstown implement, there can really be no better answer to Mr. Holmes's criticisms than to reproduce, with a few critical remarks, two paragraphs in which he unconsciously reveals the attitude of mind with which he has approached the question. The paragraphs are taken from his article in the second number of *The Journal of Geology*, pp. 158–159, in the midst of which there are injected two beautiful fancy sketches, illustrating how he supposed the banks might have appeared when the implement was discovered. Here are the paragraphs:—

"Professor Wright is entirely satisfied with the results of his efforts to corroborate the statements of the collector. He has examined and re-examined Mr. Mills, receiving every assurance of the verity of the find, but, after all, he really secures no additional assurance and can receive no fully satisfactory assurance that Mr. Mills was not in error. Professor Wright has visited and photographed the site, and will speedily prepare a plate for publication, for just what purpose, however, it is rather hard to see, since the nature of the gravels is not disputed, and a volume of photographs will not give additional weight to the proofs. A photograph made of the tree after the bird has flown will not help in determining the bird. No more will observations on Mr. Mills's moral character, his education, or business reputation diminish the danger of error. The specimen may not have been found in place, notwithstanding all possible verification, and it may be a reject, notwithstanding its resemblance to foreign types, and Professor Wright may be wrong in urging his conclusions upon the public, notwithstanding his painstaking efforts to secure all possible affirmative testimony.

"It is nowhere stated that Mr. Mills actually picked the specimen out of the gravels; it was probably loose when he discovered it, but, even if he could say that it was fixed in the gravel mass, the necessity of questioning the find would still exist. All the authentication Professor Wright can possibly secure will not enable him to determine whether Mr. Mills struck with his walking-stick a small mass of the gravel in place at a depth of sixteen feet, or whether he was dealing with a mass which had slid with its inclusions of modern relics from the surface to a depth of sixteen feet."

In a former communication to *Science* (Feb. 3, 1893), I had promised to publish a more detailed account of this discovery, accompanied with a photograph of the bank. It is to this that Mr. Holmes refers. The promised publication appeared in the *Popular Science Monthly* for May, simultaneously with the article by Mr. Holmes in *The Journal of Geology*. Doubtless it will strike the reading public rather strangely to have Mr. Holmes speak so slightingly of the value of a photograph of the bank showing it as it actually was soon after the discovery, when he has himself given two fancy sketches, representing an impossible condition of things, to inform us how he thinks it might have been. The photograph of the bank taken by Mr. Mills, within six months of the time of his discovery, exhibits its face intact, and is a part of the evidence presented as to what was the actual condition of the gravel when the discovery was made. The haste with which Mr. Holmes has plunged into this discussion is shown by his statement on a previous page that Mr. Mills had "published nothing save through Professor Wright." The report of the Western Reserve Historical Society referred to by Mr. Holmes is entitled a report "by Mr. Mills and Professor Wright," and the specific account of the discovery is given in Mr. Mills's own words, in which he says that when a space of about six feet in length by two in height fell down, it exposed the specimen to view. It is true that that statement is not so explicit as it should have been, and I have given, in the *Popular Science Monthly*, the fuller details as given to us upon the spot, and as repeated two or three times to me in correspondence, namely, that the implement was seen by him projecting from the face of the gravel bank after the fall of gravel before referred to, and when the edges of the strata of gravel were all visible and undisturbed, and that he took it out with his own hands; or, if you want to avoid all error, that he worked it loose with his walking-cane until it fell out at his feet, when he took it up, made his notes upon it, and put it in his collection. Mr. Mills is as capable of drawing a section of the bank as Mr. Holmes is, and that he has done, but most readers will prefer to see a photograph, in which there is no danger of the incorporation of fanciful elements.

In view of all that Mr. Holmes has said of the importance of expert testimony, it is difficult to see, also, why he should say that observations upon Mr. Mills's moral character, education, and business reputation may not diminish the danger of error in such a case; for how else can you determine the value of an expert's testimony? If there is doubt about his moral character, that of course vitiates the evidence in a high degree. So, also, if there is doubt about his ability to discern the difference between disturbed and undisturbed gravel in such a situation, that would largely vitiate the observations. But Mr. Mills's education and habits of observation are such that his evidence in so clear a case as this is, is as good as that of any expert could be. What does Mr. Holmes suppose led Judge Baldwin and the other members of the Western Reserve Historical Society to incur the trouble and expense of going down to Newcomerstown, except it was to inform themselves of the capacity of Mr. Mills to bear testimony to the very points at issue? Of course, we cannot force conviction upon the minds of the public, but we can get the facts of the situation and the conditions under which the evidence was given with all possible clearness before them. If any portion of the reading public chances to be in the attitude of mind in which Mr. Holmes asserts he is in when he says he does not care for a photograph of the bank, and does not care to know anything about the moral character and education of the witness, and that he is sure that Professor Wright cannot possibly secure a proper authentication of the facts, it will be a difficult matter to overcome the prejudice with which the subject is approached. But the number who are biassed to such an extent and are the subjects of such "invincible ignorance" is, I presume, not numerous.

Of course, I do not deny that there are things so improbable that they could not be established by any amount of human testimony. It is more likely that the senses should be deceived in some cases than that the things which seem to happen should really occur. But this is not a case of that sort. The existence of glacial man is not a highly improbable thing, and this evidence of Mr. Mills is in analogy with a vast amount of other evidence leading to a similar conclusion. There is nothing in the character of the implement, in the conditions under which it is reported to have been found, or in the testimony presented, to raise any serious suspicion of error. The fact that Mr. Mills was not specially impressed by the importance of the discovery at the time is not at all surprising, since his thought had been little directed to the phase of the subject involved in his discovery. He had in his collection thousands of other implements found upon the surface, and, after making note of the circumstances connected with the finding of this, it was laid with them.

In conclusion, I would simply add that in procuring, as I have done during the past season, some sections of the gravel in undisturbed condition for the exhibit at Chicago, I have had ample opportunity to study its behavior, both when it is in place and when it is in a recently formed talus, and, in reply to Mr. Holmes's assertion that it is *impossible* to tell whether Mr. Mills found this in the undisturbed strata or in the talus, I would say that the observer who could not tell the difference would be one whose testimony was utterly unworthy of consideration. While I am about it, also, I might as well refer to the fact that there is a slight discrepancy, which may attract the attention of some, both in my own and in Mr. Mills's statements about the depth at which the implement was found. In "Man and the Glacial Period" I say, that it was *sixteen* feet. In my original report upon it, I say *fifteen* feet. In the more specific details given in the *Popular Science Monthly* I say *fourteen and three-fourths* feet, and Mr. Mills has sometimes spoken of it as fifteen feet and sometimes as fourteen and three-fourths feet. It is easy enough to see why both of us should say fifteen feet, for that is a round number, but not so easy to see why in one place I should have said sixteen feet. But the discrepancy is not one that materially affects the evidence. I presume, therefore, that my error arose from the principle of assimilation with which we are so familiar in the textual criticism of the New Testament. In the appendix to the third edition of my "Ice Age in North America," I give it as fifteen feet. But in writing the paragraphs in the later book, I had just had occasion to speak of one of Dr. Abbott's discoveries which was sixteen feet below the surface, and the close association of the two in my mind doubtless led to the substitution, and, since there was nothing specially dependent upon it, the discrepancy being so slight, my attention was not aroused through all the subsequent proof-readings.

PHARMACEUTICAL EDUCATION.

BY HENRY KRAEMER, COLLEGE OF PHARMACY OF THE CITY OF NEW YORK.

DURING the past year a number of papers have appeared in *Science* demonstrating the "onward march" of institutions of the highest learning, as well as that of professional and technical schools in America. The one cry to be heard all along the line is to raise the standard. The requirements for a preliminary education have been markedly increased and the courses of studies materially lengthened both as to the number of hours required per week and the years of study. In our colleges of pharmacy there have been a similar awakening and a desire to extend the course from two to three years. It may be well, however, at this point to state for the benefit of those who are unfamiliar with the requirements of our best colleges of pharmacy, that before a diploma is granted the student must have been engaged in the drug business for a period not less than three and one-half or four years. This means practically an apprenticeship of six years, although a great many students find it necessary to work in drugstores while attending colleges.

The teachers of pharmacy have for a number of years been discussing ways by which students will be compelled to devote all of their time to college work during the sessions of study. Yet while they claim that students should not be employed as clerks in the stores and at the same time attend college, the employers are opposed to the students devoting so much of their time to college work during the winter session. There has been more or less of a compromise, but nevertheless colleges of pharmacy are raising their standard as are the other schools of learning, and it is very probable that, in a few years, three solid sessions of undivided work as well as fours years' apprenticeship will be required before a candidate shall receive his or her degree.

The position of the pharmacist is a peculiar one. He, in the majority of cases, does not make his living by means of his actual business in medicines and prescriptions. He finds it necessary to carry a line of goods known as "druggists' sundries" and "patent medicines." These, and more especially the latter, he would give up if he could, but the line of competition is so great and the public still expect the pharmacist to carry any-and-everything to suit their convenience, that it seems only practicable to a very few to abandon these in their business. The public also expect the pharmacist to know something of everything, and whatever it be, whether ills or troubles or discomforts of any kind, they run to him. I remember, when attending college, one of the professors, who was a practising pharmacist for a number of years in one of the best localities in a large city, telling us that one night he was hastily summoned by a neighbor to his house, where, in the midst of a splendid reception, the gas had suddenly gone out, and, not knowing what to do, they sent for the pharmicist. He went, and being of a practical mind and true to the instincts of his discomforted neighbor, he remedied the trouble. This simply illustrates the very close relations of the pharmacist to the public.

Now, as soon as the public will expect the pharmacist to deal in medicines only and all other articles related to the art of medicine, then the pharmacist as a business man (which he must be) will confine himself to the labors of his profession. And as soon as he can confine himself solely to the art of pharmacy as taught in our colleges, there will be no question of an extended curriculum of studies, as complete as that of any institution of learning. Then we shall have laboratories fully equipped in the particular kind of analytical and chemical apparatus which he needs for the assay of drugs and in their examination for purity. Likewise will the course in microscopical work be so extended that the pharmacist will make such analyses, for the busy physician, as the examination of urinary sediments and other discharges, such as sputum for tubercle-bacilli, etc. Indeed, it is in these two fields that the advanced work in pharmacy is tending, and accurate results will only be attained by thorough instruction in chemical and microscopical manipulation. There must be such a blending of chemical and botanical instruction that the pharmacist, while not a specialist as a chemist or a botanist, yet indeed is a specialist with regards to the practical application of these sciences as an aid to the physician in his healing art and in the preparation of pure medicines of definite and authorized strength. This condition of specialization will come, for pharmacists are marching onward in the line of progress; and it is only a question of a few years, when the host of young men, graduating by the hundreds from our colleges of pharmacy, and who are thirsting to apply their teachings and make their living in this practical application, will unite and raise the standard of their business to the profession which it is theirs to make it.

THE TELL EL-AMARNA TABLETS.

BY THE REV. THOMAS HARRISON, F.R.G.S., MEMBER OF THE SOCIETY OF BIBLICAL ARCHÆOLOGY, AND SENIOR LECTURER TO THE PALESTINE EXPLORATION FUND, STAPLEHURST, KENT, ENGLAND.

THE Tell el-Amarna tablets, after some years of patient study on the part of experts, are now known to consist for the most part of a political correspondence of great interest and importance between kings, governors, and officers, who formed their plans, struggled with their difficulties, fought their battles, and made their exit from the worry and work of life 3,370 years ago. These letters are inscribed on brick tablets, and, as a rule, occupy both sides of the tablet. With two exceptions, which are from Hittite princes and in their language, the letters are written in an ancient form of the cuneiform script. They were found in the year 1887 by an Egyptian peasant woman amid the ruins of the palace of Amenophis IV., or Khu-en-Aten, at a place now known as Tell el-Amarna, midway between Minieh and Assiout, on the eastern bank of the Nile, about 180 miles by river south of Cairo. The tablets number 320. The writers of the letters from Palestine (178 in number) are Amorites, Phœnicians, Philistines, and others, and they are addressed to the Pharaoh of Egypt and certain of his officials. At the time of this correspondence (about 1480 B.C.) the power of Egypt was waning and Egyptian garrisons were being withdrawn from Palestine in face of successful attacks by the kings of Armenia, Nii, Shinar, with the Hittites of Merash and Kadesh on the north, and of equally successful attacks by the *Abiri* (Hebrews) on the south. The letters state that the Abiri came from the desert and Mount Seir. Major Conder affirms that "the date of the letters is exactly that which is to be derived from the Bible (I. Kings vi., 1) for the Hebrew invasion, according to the Hebrew and Vulgate text, and it agrees with the fact that the Egyptian conquests made by the XVIII. dynasty (1700 to 1600 B.C.) had been lost when the XIX. dynasty acceded." It is certainly very interesting to find in the letters the names of Japhia (Josh. x., 3, one of the kings killed by Joshua) and most probably that of Adonizedek, king of Jerusalem; while the name of a king of Hazor is read as Jabin (Josh. xi., 1). It is also pointed out that the name of the captain of Jabin's host is, Egyptian, Sisera or Ses-Ra, meaning servant of Ra.

In most of the letters from the kings of the cities of Phœnicia and Northern and Southern Palestine the appeal is ever one for Egyptian troops to enable them to hold their cities for the Pharaoh, to whom they seem to have appealed in vain. The earlier letters of brave Ribadda, the king of Gebal (now Jubeil, north of Beyrout), usually begin with the following salutation, which is given as a specimen of such salutations at that time, "Ribadda of the city of Gebal of his Lord, the King of many lands, the prosperous king, Baalath of Gebal, she hath given power to the King my Lord. At the feet of the King my Lord, my Sun seven times seven times I bow."

The salutation of the later letters becomes shorter and less ceremonious, as Ribadda felt that he was being left to his fate. Here is one of his appeals for help: "I have been hard pushed. Help speedily O King my Lord. . . . Soldiers and chariots, and you will strengthen the chief city of the King my Lord."

And what can be more pathetic than this, coming from that same brave heart, which has now for more than 3,300 years ceased to trouble itself about chariots and men of war and Pharaohs who could not or would not come to his aid.

"And will not my Lord hear the message of his servant? Men of the city of Gebal, and my child, and a wife whom I loved, this son of war, the son of Abdasherah has seized; and we have made a gathering, we have searched; and I cannot hear a word spoken about them. I am doing my duty to the King my Lord, and once more, despatch thou men of garrison, men of war, for thy servant, and will you not defend the city of the King my Lord?"

On May 14, 1892, a cuneiform tablet was found by Mr. Bliss while excavating at the old Amorite city of Lachish, in Judea, in which the name Zimridi twice occurs. From the Tell el-Amarna tablets we learn that Zimridi was governor of Lachish, and, moreover, in a tablet from the king of Jerusalem to Amenophis IV., we are informed of the death of Zimridi at the hands of the servants of the Pharaoh just named.

Many matters of great interest in connection with these tablets can find no mention within the limits of this paper. It may be added, however, that the topographical value of these letters is very great; and also that the evidence which they afford as to the Hebrew conquest of Palestine under Joshua is in favor of the Bible chronology (Acts xiii., 20; I. Kings vi., 1) and against that of Dr. Brugsch and Bunsen.

SOME CONFLICTING ESTIMATES OF DISTANCE.

BY ARTHUR E. BOSTWICK, PH.D., MONTCLAIR, N. J.

ACCORDING to all authorities with which I am familiar, a small, regular pattern, if looked at squintingly, so that the horopter is nearer the eye than the pattern, but at such a distance that adjacent corresponding parts of the latter overlap and coalesce, should appear closer to the observer, and if looked at in like manner, but so that the horopter is farther from the eye than the pattern, it should appear farther away. This seems natural, for, in each case, the image on the retina being unblurred, the point to which the axes of the eyes converge should be taken as the distance of the object. In this case, the angle actually subtended by the pattern remaining the same, the mind should infer, in the first case, that the pattern has grown smaller, and, in the second case, that it has grown larger.

The writer of this note has never been able to make things appear to him in this way. When the horopter is nearer than the object, the pattern, though it appears smaller, seems also distinctly more distant, and when the horopter is farther away, the pattern seems larger and nearer. When one has learned the trick of causing the adjacent parts of the pattern to overlap and coalesce perfectly, the experiment may be tried as often as one likes, and I have tried it often and under many different conditions, always with the same result. Of course, care has always been taken to make sure of the point at which the axes of the eyes converge, either by converging them at first on the tip of the finger and then removing it, or by moving the finger to and fro in the field of vision after the eyes have become fixed, the separate images becoming closer together or farther apart, according as the finger approaches or recedes from the horopter. The fact is, as is well known, that an estimate of an object's distance is always an inference from various data furnished by the eye, as the visual angle, the position of the horopter, and the muscular movement in each separate eye necessary to effect accommodation. For distant objects the last mentioned fails, and aërial perspective comes in to aid; but for objects that can be used in this experiment the three factors mentioned are those on which the eye relies. The conditions in the experiment being unique, the data obtained are discordant, and it is not wonderful that different persons, under the circumstances, disagree in their estimates of the distance of the pattern.

Take the case where the eyes are squinted. The pattern being seen clearly, and no accommodation being necessary, each eye separately infers that the object has remained stationary. The horopter having advanced, the two eyes jointly agree that the pattern is nearer. But, if it is nearer, the angle it subtends remaining the same, the pattern must be actually smaller. But, on the contrary, no accommodation for bringing it nearer has been necessary, so, if it is smaller, that must be an apparent effect due to its having moved back. The conclusion to which one comes must be influenced by the relative weight that he is unconsciously accustomed to give to the different data on which his estimate of a distance is ordinarily based. And having interpreted the phenomena in one particular way at first sight, this becomes habit, and what may have been determined by chance the first time one tries the experiment becomes a settled thing. Often as I have tried it, however, I am always conscious of a queer feeling of surprise as the pattern comes out clearly before me — a feeling that all is not quite right, due, of course, to the unconscious clashing of these contradictory data. I may add that in my own case, and I suppose in that of others, in monocular vision an object appears distant or near as the eye is fixed respectively on something nearer than it or something beyond it. As accommodation is associated always with concentration of the axes of the eyes, it is doubtless impossible to accommodate the focus perfectly to the pattern while the horopter is in a different plane, hence, as in the case of the writer, this may tip the balance in favor of his peculiar way of inferring from the clashing data.

NOTES AND NEWS.

ALL lovers of ferns will be glad to learn that an association for the study of these plants by correspondence has been formed. The work will be made as easy as possible for beginners, and all who are interested in ferns are invited to join. Applications should be made to the secretary, Miss A. May Walter, 516 Spruce Street, Scranton, Penn., or to Willard N. Clute, Binghamton, N.Y.

— Professor Daniel G. Brinton, M.D., LL.D., of Philadelphia, received on May 10 the further honorary degree of "Doctor of Science" from the University of Pennsylvania. His works are numerous, and have been principally upon linguistics, ethnology, and American archæology.

— The Chicago Academy of Sciences has undertaken the collection of views from all localities in Illinois, and adjacent parts of Indiana, Michigan, and Wisconsin, for the purpose of bringing together, where they may be accessible to all scientific workers, a complete series illustrative of the geological and natural history features of the region. The value of such a collection is apparent, and the Academy believes that, in the interest of science, it may reasonably expect the coöperation of all who may be in a position to assist in the work. While all views are acceptable, those illustrating the following features are especially desired: geology, topography, land, water, and forest scenes, farm life, public buildings, neighborhood characteristics, and, in general, anything characteristic or unique in the study of nature or man. In sending views, please observe the following directions: 1. Send photographs unmounted. 2. Send with each a careful description of (*a*) the locality, (*b*) objects shown, (*c*) direction of view, (*d*) by whom taken. Number descriptions and views to correspond.

— A meeting of the Victoria Institute was held at Adelphi Terrace on May 1, at which an address by Professor Maspero, embodying the results of his investigations during the past ten years as regards the places in Southern Palestine claimed, according to the Karnac records, to have been captured by the Egyptians in the campaign under Sheshonq (Shishak) against Rehoboam. M. Maspero pointed out the great help that the recent survey of Palestine had been in determining the localities referred to, and specially referred to the fact that the Egyptian letters, rigorously transcribed in Hebrew letters, gave almost everywhere the regular Hebrew forms in the Bible, "without change or correction." The paper was admirably read in the author's absence by Mr. Theo. G. Pinches of the British Museum, who afterwards added some remarks. The discussion was continued by several members, including Major Conder, R.E., who contributed many interesting details. During the discussion reference was made to the great interest taken in the question by the late Canon Liddon, who, on the occasion of Professor Maspero's former paper being read, pointed out that the identity of form of the words in the Egyptian and Biblical records pointed to the antiquity of the latter.

— Morris Phillips & Co. have issued a new edition for 1893 of "Abroad and at Home." This book is a guide of an unusual character, giving much information in regard to hotels, boarding-houses, restaurants, etc., of considerable value to those who intend to go abroad, or who intend to travel in this country. Last year, the book first appeared early in the summer, and during the three summer months three editions were called for. This year, new matter has been inserted descriptive of Atlantic City, Niagara Falls, the St. Lawrence, Adirondacks, and Saratoga Springs, and a summer-resort guide giving information regarding the leading hotels. A specially prepared chapter on Chicago also appears.

— "Miss Helen Keller, who may be regarded as the most remarkable person in this country when her natural deficiencies [blind and deaf] are compared with her graces and gifts," says *The Evening Star* of Washington, of May 11, "is now a guest at the house of Mr. Alexander Graham Bell in this city. Last evening a number of well-known gentlemen were also his guests and had an opportunity to see how extraordinary is the intelligence of this young lady and how more marvelous is her power of expression, not only by manual signs, but also by distinct and agreeable oral utterances. Among the guests was Senator Sherman. Professor Bell said to Helen: 'This is the birthday of Senator Sherman and we are going to drink his health. We want you to propose a toast. Do you know what that means?' As this was a new idea to the young lady it was explained to her. 'We want you to propose a sentiment in honor of this birthday,' said Professor Bell. Helen looked puzzled or thoughtful for a minute and then said slowly and with a sympathetic emphasis: 'I propose his health, happiness and prosperity. May he be as helpful to his country in the future as he has been in the past, and may he be blessed with all good things in this life and in the beautiful life to come.' During the evening the quickness and fitness of her answers to Professor Newcomb and other scientific gentlemen surprised everybody. So did her accurate repetition of Longfellow's Psalm of Life, and so did her keen enjoyment of stories told to her and of the conundrums with which she puzzled the friends who were talking with her. Her story is wonderful, and the skill of her teacher, Miss Sullivan, is admirable in the highest degree. Besides Senator Sherman the following gentlemen were present: Senator Morrill, Professor Langley, Professor Gilbert, Maj. Powell, Professor D. C. Bell, President Gilman of Baltimore, the Danish minister, Mr. G. G. Hubbard, Mr. Pollok, President Gallaudet, Professor Newcomb, and Col. Britton. Here is what Miss Keller got off on one of the justices of the Supreme Court, who called to pay his respects on the occasion of the reception given her by Mrs Graham Bell the other evening: Helen asked the justice: 'Do you know my friend, Judge Holmes?' 'No, dear, he lives in Boston.' To which she replied, smilingly: 'Oh, I thought you knew him, because you see you are brothers-in-law.' The justice took in the bon mot and laughed heartily."

— The institution which was founded and endowed a few years ago in Washington by Professor Alexander Graham Bell for the increase and diffusion of knowledge relating to the deaf, and which has heretofore had its headquarters at 1334 Q Street, is to have a new home. The institution is styled the Volta Bureau, and in the past two or three years has issued a number of valuable books upon the education of the deaf. The work of the bureau has increased to such an extent that it has been found necessary to provide new quarters. To this end, ground was broken, within a few days, on the northeast corner of 35th and Q Streets for the proposed building, which has been planned and upon which work will be commenced at once. The first sod was turned by Miss Helen Keller, the remarkable deaf and blind girl whose history and wonderful development is known to readers of *Science* through recent publications. The ceremony of breaking ground was also participated in by Miss Elsie May Bell, Miss Marian Hubbard Bell and Master Douglass McCurdy.

— Rarely has a more interesting and beautiful memorial been raised to the memory of a man devoted to science than the monument lately unveiled in New York City to the eminent ornithologist, J. J. Audubon. The ceremonies took place on the afternoon and evening of April 26, and consisted of the dedication of the monument in Trinity Cemetery at 3.30, and a public meeting and addresses at 8.30 in the hall of the American Museum of Natural History, the principle address being a noble eulogy on Audubon, by Professor D. G. Elliott, president of the American Ornithologists' Union. The movement has been carried out chiefly through the agency of the New York Academy of Sciences. It was begun six years ago, and originated with Professor Thomas Egleston of the Columbia College School of Mines, who noticed the fact that Audubon's remains had lain for over thirty years in an obscure vault in a remote corner of Trinity Cemetery, almost unmarked, and wholly undistinguished by any proper memorial. He enlisted the interest of a few scientific friends, and the coöperation of the trustees of the cemetery, who offered an excellent site, free of expense. The matter was brought before the American Association for the Advancement of Science during its meeting in New York in that summer (1887) by Professor Daniel S. Martin, but no formal action was taken. At the opening of the meetings of the New York Academy of Sciences, in the autumn, Professor Martin again presented the subject, and a committee was appointed, consisting of Professor Egleston, as chairman, and Drs. N. L. Britton and D. S. Martin. To the labors of this committee, and especially of its chairman and secretary, Professors Egleston and Britton, the result now attained is due. Other societies were invited to coöperate, and have done so to some extent, especially the American Ornithologists' Union and the Agassiz Association. The amount sought was $10,000. About five hundred persons have subscribed in varying amounts, the whole exceeding the sum proposed, while the monument has cost somewhat less. A balance of over $1,000 will remain, which is to be held in trust permanently, as an "Audubon Memorial Fund," by the Academy of Sciences, and used for the publication of important scientific memoirs on subjects kindred to those of Audubon's studies and pursuits. The monument itself is a noble and striking work. It stands on a beautiful knoll, close to the 152d Street entrance, facing the point where Audubon Avenue is to be opened through to that street from the north, and close to the old estate, Aububon Park, where the great ornithologist passed his later years. It consists of a Runic cross, some fifteen feet high from the base, and is richly carved with appropriate designs, this treatment being possible, historically and æsthetically, upon the Runic cross. The nearly cubical base bears on its front a medallion head of Audubon; on the back an inscription of the manner of its erection, through the New York Academy of Sciences; and on the two sides designs of the hunter's and artist's outfit, respectively, with flowers particularly noted or described by Audubon. The shaft and arms of the cross are elegantly carved in scroll-work, interwoven with a series of birds

THE AUDUBON MONUMENT.

and animals of characteristic North American species, on the front and back respectively. The whole is unique and impressive, carefully studied in both its scientific and artistic details, and singularly happy and appropriate in conception. The material (Hudson River blue-stone) lends itself admirably to the work thus wrought, and the whole rests on a substantial granite base. The spot is beautifully laid out and kept by the trustees of the cemetery, and the whole enterprise reflects great credit on those who have planned and executed it, and is an honor to the Academy of Sciences, an ornament to the city, and a fitting tribute to the memory of the great ornithologist.

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THE CHICAGO ACADEMY OF SCIENCES.

BY C. H. GORDON, EVANSTON, ILL.

The arrangements recently completed whereby the Chicago Academy of Sciences receives from Mathew Laflin of Chicago $75,000 for the construction of a building at Lincoln Park, revives interest in an association formerly among the foremost of similar associations in this country. In giving a brief résumé of the history of the Academy, the treatment will naturally follow the order suggested by its financial experiences, as follows: (1) period of organization and early struggle — 1856–64; (2) prosperity — 1864–81; (3) decline — 1881–91; (4) revival — 1891.

1. *Organization.* The Chicago Academy of Natural Sciences, as it was at first termed, was established in 1856, chiefly through the efforts of Robert Kennicott, then a young man of indomitable perseverance and rare scientific attainments. There were fourteen charter members as follows: J. B. Blaney, M.D., N. S. Davis, M.D., J. W. Freer, M.D., C. Helmuth, M.D., E. W. Andrews, M.D., H. A. Johnson, M.D., H. Parker, M.D., F. Scammon, M.D., Robert Kennicott, George A. Mariner, Samuel Stone, R. K. Swift, J. D. Webster, H. W. Zimmerman. The energy, loyalty, and ability of these men, some of whom are still active in the affairs of the Academy, gave the impetus which contributed largely to the success attending it in subsequent years. Steps were taken very early to establish a museum, and rooms were secured in what was then known as Dearborn Seminary on Wabash Avenue, north of Washington Street. Subscriptions to the amount of $1,500 were obtained, and Robert Kennicott, who had contributed his fine collection of zoölogical specimens, was installed as Director. Contributions were also made by Dr. Andrews and others until many thousand specimens were accumulated.

The financial crash of 1857 ruined most of the subscribers to the original fund, and left the Academy nearly destitute of means for meeting its necessary expenses. In 1859 it was incorporated under the laws of Illinois and its name changed to its present form. The Civil War breaking out soon afterwards completely absorbed public attention, and took from its ranks the more active members for service in the army. Deprived of public attention, destitute of money, and stripped of working members, the whole enterprise seemed likely to be extinguished.

In 1859, Robert Kennicott departed for Arctic America in charge of an exploring expedition under the auspices of the Smithsonian Institution. He returned from Alaska in 1862, richly laden with specimens in all departments of natural history. The results of this exploration were considered to be second to no other similar expedition on record. As the expenses of the expedition were borne by the Smithsonian Institution, the collections all went to Washington with the understanding, however, that a full series of the specimens was to be presented to any institution, otherwise eligible to such donation, that Mr. Kennicott might designate, provided suitable provisions were made for their reception and preservation. Naturally, Mr. Kennicott designated the Chicago Academy of Sciences as the recipient of such donation. During the year 1863 the collections were studied and arranged, and early in 1864 word was received stating that the duplicate series awaited the action of the Academy.

2. *Period of Prosperity.* The opportunity thus offered was eagerly seized by the active spirits remaining, and steps taken to secure the requisite funds. On Feb. 22, 1864, an informal meeting of interested gentlemen was held, at which Professor Agassiz, who was opportunely visiting Chicago, was present. Professor Agassiz testified to the great value of the collection and gave added impulse to the movement.

A reorganization of the Academy followed, and a corresponding act of incorporation obtained in 1865. A change in the constitution was effected by which life memberships of $500 each were established. During the summer of 1864, an active canvass was made resulting in 125 subscribers to life-memberships, making a total of $62,500. Of this, $50,000 was to be held as a permanent endowment. The following year the collections were received and deposited temporarily in the Metropolitan Building, at the corner of Randolph and LaSalle Streets.

The selection of a lot and the erection of a building next engaged the attention of the Academy, and it was only after much effort and discussion that a location was decided upon; but finally in January, 1867, the trustees reported the purchase of a lot on Wabash Avenue, just north of VanBuren Street, and the construction of a fireproof building thereon soon after began. Here began the contention, sometimes characterized by good humor, sometimes by acrimony, and renewed from time to time through the following years between the Academy and its trustees. The cause of this contention lay in the constitution, which was defective in two points; first, in delegating to the trustees, a body of its own creation, sole authority in certain lines, thus depriving itself of supervision over the acts of its servants; and, second, of failure to define clearly the powers and limits of the two bodies. A dual government was thus inaugurated, destined to bring trouble and perplexity into the affairs of the Academy. The assets of the Academy at this time, as shown by the financial report, were $72,000, with an annual income of $6,500. In the meantime, however, it had suffered two irreparable losses. The first was in the death of Robert Kennicott, which occurred at Nulato, a Russian port on the Yukon river, May 13, 1866, while on a second expedition to the far north, which had been undertaken the previous summer. Following close upon this (June 7th) came a disastrous fire in the building containing the collections, by which a large portion were destroyed and the remainder badly damaged.

Notwithstanding these misfortunes, however, the years from 1864 to 1871 may be reckoned as the most prosperous years in the history of the Academy. Active investigations of much importance were being carried on in various lines, and the work of its active members attracted the attention of leading scientists throughout the country, many of whom were enrolled as corresponding members. In meteorology, observations were conducted under the special supervision of Dr. Joseph Henry of the Smithsonian Institution. The work in natural history, which had received special impetus from the labors of Robert Kennicott, was continued under the able leadership of Dr. Stimpson, and the growth of the museum was remarkable. The library was in constant receipt of books and the publications of scientific associations of all countries, while the papers presented to the Academy were of a high order of merit.

The Academy building was completed in 1867, and the associaciation immediately entered upon the publication of its proceedings, the first part of volume one being issued in 1868 and the second in 1870.

On the departure of Mr. Kennicott, in 1865, Dr. W. S. Stimpson, a young man of rare scholarship and excellent scientific attainments was called to act as director of the museum, to which duties were added those of the secretaryship. The Academy was especially fortunate in having at the helm in its early career two men of such eminent ability, energy, and devotion as Kennicott and Stimpson. During these years Dr. Stimpson was actively engaged in investigations, the published results of which would have made his a well-known name among the scientists of the world.

In 1871 came the great fire destroying the Academy's building with all it contained, sweeping away all the results of Dr. Stimpson's life-work, as well as swallowing up in the general ruin the private fortunes of the most active supporters of the Academy. The loss of his papers was a severe blow to Dr. Stimpson, from which he never recovered. After the fire he was taken to Florida, where he died the following May.

At the beginning of 1872, the assets of the Academy, exclusive of the lot, were $23,000, $10,000 of which represented the insurance on the burned building. No money was available for building, but it was decided to borrow and erect on the lot two buildings, one for the museum and one for rental. The courage and hopefulness thus evinced was but a part of that characterizing Chicago after the fire, and, as in the case of many a private interest, the too sanguine view was but the prelude to further disaster. The buildings were completed in 1873, involving a financial burden of $80,000, afterward increased to $100,000.

In the general depression of business following the fire, the income of the Academy was insufficient to meet expenses and interest, until in 1881 the mortgage was foreclosed and the society was homeless.

During this time, however, the scientific work was carried forward with commendable zeal and success. The records show the interest to have been well sustained and the papers meritorious, while the museum prospered notwithstanding the financial stress.

3. *Decline.* Following the loss of the property, interest flagged, hope died out, and for ten years it became a bare struggle for existence. The museum building was retained by rental for two years, after which the collections were transferred to the Exposition Building, where they remained for several years under the care of the curator, J. W. Velie. The meetings were desultory and not well sustained. Two series of valuable bulletins were issued, however, during this period.

4. *Revival.*— In 1891 it was decided by the city authorities that the old Exposition Building should be removed. This revived the question of the disposition of the collections. A proposition involving its transfer to Chicago University was not favorably received by many of the members, when an opportune benefactor appeared in the person of Mathew Laflin, and settled its location at Lincoln Park. This agreement contains a provision by which the commissioners of the park are to add $25,000 toward the erection of the building and to bear all the running expenses, including salary of curator and assistants to an amount not exceeding $5,000 annually. The final arrangements were completed April 1, 1893, since which plans have been accepted and the construction will soon be under way.

Within these two years interest in the Academy has greatly revived, many new members have been enrolled, and active investigations set on foot along many different lines. Sections have been formed in astronomy, microscopy, chemistry, and other lines of work.

The disposal of the museum frees the Academy from a heavy burden, thus making the income available for publications which are to be renewed at once.

One of the enterprises now engaging the attention of the Academy is a geological and natural history survey of Chicago and vicinity. This will include the preparation of a topographic map of the area on a scale of about one and one-half inches to the mile, with contour intervals of five feet, and accompanying reports upon the geology, paleontology, zoölogy, botany, and archæology of the district. The work is in charge of a board of managers, and is being prosecuted as actively as possible. In the preparation of papers many noted scientists both in and out of Chicago are giving assistance.

In connection with this work the board has also undertaken the collection of views from all parts of Illinois and adjacent parts of Indiana, Michigan, and Wisconsin, illustrating interesting features of geology, topography, and other points of interest. These will be mounted, classified, and deposited in the Academy building, where they will be accessible to all who may wish to consult them.

The president of the Academy is Dr. S. H. Peabody, ex-president of Illinois University and superintendent of the Liberal Arts exhibit at the World's Fair. Dr. Peabody has been an active worker in the Academy for many years.

The present hopeful outlook for the society must be attributed in large measure to the untiring zeal and energy of its efficient secretary, Professor W. K. Higley. Among those identified with more or less of the history of the Academy the following are still among its loyal supporters: Dr. E. W. Andrews, G. C. Walker, E. W. Blatchford, B. W. Thomas, B. F. Culver, C. M. Higginson, Professor G. W. Hough, Dr. N. S. Davis, S. W. Burnham, S. H. Peabody, and others. Prominent in the past but no longer appearing on the active roll are the names of Professor M. Delafontaine, E. Colbert, J. D. Caton, Professor H. H. Babcock, ex-Gov. Wm. Bross, J. H. Rauch, J. W. Foster, and others.

IS IT A SCIENCE?

BY WILLIAM L. SCRUGGS, ATLANTA, GA.

In the current discussions of international questions we often encounter the words commonwealth, state, and nation in the alternate form, as if they were synonymous and convertible terms. Now, a commonwealth may be a state or a nation, or both; a state or a nation may be a commonwealth. But the term nation implies the unity of a people of the same race, descent, and language under one government; whereas a state may be composed of people of diverse origin united under one government of whatever form; whilst a commonwealth is the unity of a people under a free or representative government.

Again, we have the commonly accepted statement that "states or nations are bodies politic or societies of men united together for the purpose of promoting their mutual safety and advantage by the joint efforts of their united strength." This is Vattel's definition, derived from Cicero. But states and nations are not equivalent terms, nor are "societies of men united together for the purpose of promoting their mutual safety and advantage" necessarily either "states or nations." The old Hudson Bay Company was such a "society of men united," but it was neither a nation, state, or commonwealth. Pirates and robbers are so united, but they have none of the essential elements of statehood. The political bodies corporate in the United States, the people of which constitute our national government, are literally within Vattel's definition; but they are neither "states" nor "nations" in the strict legal sense. They have a local police system or automatic government, but none of the elements of sovereignty or nationality. The very form of their local autonomy is prescribed by a superior power; they can have no diplomatic relations even between themselves, much less with foreign powers; they cannot declare war or enter into public treaties; they cannot establish post-offices and post-roads; they cannot levy and collect import duties; their very local legislation must conform to that of an external and paramount authority; and their citizens are such only by reason of the fact that they are citizens of the United States. Hence, so far from being "sovereign." these political bodies corporate are not even "states" in any just sense. They would be more properly denominated dependencies, provinces, or commonwealths.

Again, conforming to custom, we are in the habit of speaking of "the law of nations," when it is manifest there is no such thing. Law is a rule of conduct prescribed by some superior power able to enforce obedience. But sovereign states acknowledge no superior; all are equal. They recognize no common paramount authority; nor have they established any common magistracy to interpret and apply rules for the regulation of their reciprocal relations. They have no common code illustrated by judicial decisions. True, there is an established *usage* or custom in the intercourse of nations which by common consent has the moral force of law; the real meaning of which is, that there are certain forms of public *opinion* which nations, no less than individuals, cannot very well afford to disregard, although the duties thus imposed are enforced by moral sanction only. The old

Romans called this *jus enter gentes,* the French denominate it *droit des gens*, the Spaniards call it *derecho de gentes*, and we, for lack of a more specific term, call it international law. But *law* it is not; and, besides, if we admit the term at all, "law of nations" and "international law" are certainly not equivalents. The one implies an impossible condition of things, the other, though more approximately correct, would be more accurately described as international ethics or morality.

Furthermore, we are in the habit of describing what we call "international law" as "the natural law of individuals applied to nations," and when we are asked what this "natural law of individuals" may be, we reply readily that it is "the law of nature applied to moral actions," and that it consists of "rules which are common to all mankind," quite independent of the accidents of time, place, and circumstance. Now, this is little else than mere words without any definite import, for in reality there are not, and never have been, any such "rules." There is not a single, universal, fixed "rule" of human conduct which all men of all ages and countries have recognized in practice; there is no uniform moral code, written or unwritten, which peoples of all countries have even professed to obey.

But, we are told, there are certain "principles of justice, discoverable by right reason and established by usage," which *ought* to regulate the mutual relations of nations. But who shall accurately define "justice," and who shall give us an authentic standard of "right reason?" Public opinion in each sovereign state establishes a criterion of justice which rises no higher than the intellectual development or civilization of the people of that particular state; and what the people of one may consider "right reason" is often deemed wrong reason by those of another. Thus some regard all moral distinctions as merely conventional, others believe moral distinctions to have been "written in the heart of man by the finger of God." Most Christian peoples believe, or at least believe they believe, there is "a positive law, audible in conscience, which enjoins certain actions and forbids others," according to their respective suitableness or repugnance to the social nature of man. Others believe that conscience itself is merely the result of education and environment, consequently that there cannot be, in the very nature of the case, any positive moral standard. No matter how it originated, I presume that most people will agree that what we call "conscience" is nothing more than that faculty of the mind which takes cognizance of its own thoughts; that, even in the most latitudinal sense, the term can imply no more than a moral standard of action in the mind, and that this standard is always relative, that is, high or low, according to the degree of intellectual development.

We are in the habit of evading the consequences of these propositions by assuming, first, that moral distinctions have had eternal existence in the mind of the Creator, which never changes; and, second, that to Christian peoples *only* have been revealed the will of God. This would limit what we call "international law" to Europeans and their descendants on this continent; and it, moreover, assumes as a fact that, in our international relations we are governed by rules which, in their very nature, are unchangeable, which is absurb. For, reason about it as we may, we cannot get rid of the fact that our standard of morality is progressive, and therefore ever changing. There is always an advance from lower to higher conceptions of humanity and justice, and corresponding changes in public sentiment as to what is right and expedient in our international relations. The general concensus of the Christian world touching the abstract propositions of right and wrong is not what it was even one short century ago, and a century hence it will not be exactly what it is to-day. The time was when the most enlightened nations, including the one through which was derived our form of religion, spared neither age nor sex in battle. Later on, they spared non-combatants, but put all prisoners to death. Further on, the lives of prisoners were spared, but they were reduced to slavery. As civilization advanced, prisoners of war were ransomed by the payment of money or its equivalent. Finally, they were put on parole and regularly exchanged. Not many centuries ago, Christian nations went to war for the avowed purposes of conquest and selfish aggrandisement. After this, war was still held to be justifiable if waged for the declared purpose of opening new avenues of trade. Later on, war could be justified only on grounds of reasonable apprehension for national safety, or for the vindication of national honor. Perhaps the time is not very remote when Christian peoples will realize that there is a higher method of settling international disputes than that adopted by the ants and beetles, and then the principle of arbitration will be universally accepted.

Hitherto, what we call our international law has been deemed inapplicable to pagan nations and savage tribes, and in our dealings with both we have not always been governed by our own rules of justice. Our apology for this has been the assumption that such peoples are not themselves governed by the rules of justice which we acknowledge. But, if we are subject to a system of ethics which we profess to believe of divine origin, is not that, of itself, an all-sufficient reason for not departing from it in our dealings with other than professedly Christian peoples? It would seem that, if we are more than a community of hypocrites, our relations with the indigenous peoples of this continent ought to have taught us this wholesome lesson long ago.

To sum up, then, our so-called international law is but public opinion sanctioned by usage among those who call themselves Christians. But this public opinion necessarily changes with the progressive stages of intellectual development. Therefore it is not, and cannot be, a "fixed rule" of conduct in the reciprocal relations of nations. We err in calling it a "science," because our conceptions of its fundamental principles are neither clearly defined nor easily referable to known facts. And we err in limiting its application to so-called Christian nations, because we thereby contradict our professions and impair confidence in our sincerity.

BRITISH STONE CIRCLES.—II. STONEHENGE.[1]

BY A. L. LEWIS, LONDON, ENGLAND.

If the circles at Abury (or Avebury) claim the first notice on account of their great superiority in size above all others, Stonehenge naturally, and for many reasons, takes the next place to them. Stonehenge is eighteen miles south of Abury; the nearest town to it is Amesbury (three miles), but as Amesbury is not on any line of railway, Salisbury (Great Western or South Western railways) is the most convenient place from which to visit it; the distance is eight miles, six by road and two across the plain after leaving the road, and there is now no refreshment house on the way. The British entrenched hill, on which the Roman, Saxon, and Norman city stood, and which, under the title of Old Sarum, returned representatives to Parliament till 1832, at which time it was uninhabited, will attract notice, and may be visited either in going or returning.

The outer circle at Stonehenge is 100 feet in diameter, and if it were ever completed (which is a point in dispute) consisted of 30 stones, averaging $13\frac{1}{2}$ feet in height; they were roughly squared and had two knobs or bosses worked on the top of each, and they were connected by smaller stones, each of which had a hole at each end, made to fit on the knobs of the upright stones on which it rested; these arrangements are found in no other circle, and are of themselves sufficient to render Stonehenge perfectly unique. One stone of this circle, still standing in its place, is shorter and slighter than the others, and this has led to doubts as to whether the outer circle were ever complete. Inside the outer circle were, first, a circle of small stones, the original number of which is uncertain, and, second, inside these five trilithons or groups of three stones, two upright and one connecting their tops, these capstones, like those of the outer circle, were kept in their places by holes fitting on knobs cut on the tops of the uprights, but while each upright of the outer circle had two knobs, and the chain of capstones was continuous, the uprights of the trilithons had but one knob each, and each pair of uprights with its capstone was separate from its neighbor; these trilithons were arranged in the form of a horseshoe, the highest (of which the uprights were 22 feet above ground) being in the centre, and the opening of the horseshoe, which is 44 feet wide, being toward the northeast. Inside this horseshoe of trilithons was a horseshoe

[1] No. I, Abury, appeared in No. 529, March 24.

of smaller stones, originally perhaps 19 in number, and from 6 to 9 feet high, the highest being in the middle, and inside these, and in front of the highest trilithon, is a flat stone, about 17 feet long and 3 wide, which is commonly called the altar stone, though, if sacrifices were ever offered there it would have been much more convenient to have had a smaller but higher altar standing upon this slab. There is a small stone lying inside the small inner horseshoe, which has two hollows and seems therefore to have been intended to rest upon two small upright stones, but no stones suitable for its support now exist, and it is possible that this stone may have stood on two small stones on the slab already mentioned, and may have been the actual altar. It has, however, been thought that it was the capstone of a small trilithon which stood in the middle of the open side of the horseshoe formed by the large trilithons, but there is no evidence as to its original position or use or as to the former existence of any small trilithon.

The smaller stones or bluestones as they are called were brought from a great distance — Devonshire, Wales, or Ireland — but the larger stones forming the outer circle and the great trilithons were obtained from the surrounding plain. Nine of the inner bluestones and nineteen of the outer ones remain, some standing and some fallen; twenty-four of the stones of the outer circle are represented by standing or fallen stones (including fragments), and six of its lintels or cross-stones are still in position; of the trilithons two are complete and the other three are more or less ruined, though all the stones of which they consisted are there, some broken, some entire.

The circles are surrounded by a slight ditch and bank, 300 feet in diameter, from which an avenue defined by earthen banks leads in a northeasterly direction for about 1800 feet, when it divides into two branches, the most northerly of which leads towards a space enclosed by earthen banks and called by Stukeley the "Cursus." Just inside the ditch and bank are two barrows, on opposite sides of the circles, and so placed that a line from one to the other passes through the centre of the circles. There are also two single stones near the inner circumference of the ditch placed like the barrows on opposite sides of the circles and so that a line from one to the other passes through the centre of the circles. At the point where the avenue joins the ditch there is a large stone lying flat, and nearly 100 feet along the avenue stands a rough stone, called the "Friar's Heel," in such a position that anyone standing on the flat stone called the "altar," already mentioned, may see the sun rise over its tip, or nearly so, on Midsummer morning, a fact which is generally verified by several people every year. It has been said that the flat stone between the Friar's Heel and the circles formerly stood upright, and hid the former from the latter, and that the coincidence as to the sunrise was therefore not intentional; but if the flat stone ever were upright the sun would have appeared to rise over it, and if neither stone existed the whole arrangement of the circles and avenue would still direct attention to the northeast or midsummer sunrise quarter.

Stonehenge has been attributed to various peoples, ranging from Atlanteans of 10,000 B.C., to Danes of the ninth century of our era, and numerous suggestions have been made as to its object. Two or three archæologists of late years have endeavored to show that it is merely the skeleton of a vast tower of dry or uncemented masonry, and the visitor must form his own idea as to the probability of this view. Burials would seem to have taken place in the centre, as bones and iron armor were dug up there in 1620, but this does not show that burial was the only or even the chief object for which the circles were constructed. Perhaps the view that best fits all the facts is that a circle or circles with avenue and outlying stones so arranged as to make it suitable for sun-worship existed here in very early times, and that long afterwards, in the dark period between the Roman rule and the Saxon domination, certain murdered Britons were buried in the circles, which were restored and re-arranged as a monument to their memory. Stonehenge, while it has much in common with the other British circles, has also so many points of difference from them, that it seems as though it must have had a special history of its own.

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.

What is Biology?

ORIGINATING from the time of the appearance of Dr. Campbell's book[1] on biological instruction, a discussion is for the present time being held. Professor C. MacMillan opened this discussion in some very interesting articles,[2] the main feature of these being a sharp criticism of the way in which biological science has been and is taught in the colleges and universities. Mr. Francis H. Herrick[3] has tried to save the reputation of the biological departments in pleno. As the question of a clear and logical definition of the term biology meets with some of my own considerations, I should like to make a few remarks on this side of the point; the position of botanical science in the scientific institutions being merely a question of power laid in the hands of the director or professor of such institutions, I shall leave this in better hands.

It would be well, indeed, if we could get a logical definition of biology, and if we could succeed in removing from the text-books the old definition that "biology is the science of living things." Doing this, we would avoid much confusion, especially among the students — and there are many of them yet — who think that the physiological science is still a well established branch of natural science, and not merely a subdivision of a more or less heterogene "biology."

LaMarck used, first of all, the word biology, and, afterwards, from 1802 to 1822, G. R. Treviranus wrote a very remarkable book,[4] defining biology as the philosophy of living nature. Singularly, the idea of the range of living nature has, in the course of time, been limited, instead of broadened; so we see how the scientists of old times saw, in the fire, a manifestation of life. Oken, in his "System der Biologie," adopted the definition of Treviranus, while the second and third quarters of this century created physiological schools that fought against the "natural philosophers," and brought forth an experimental physiology.

When the profound thinking of Ch. Darwin (not especially of all his pupils and successors) caused a world-wide sensation, and cast new light upon natural history, the term became rather limited instead of broadened, and, in fact, from an evolutionary standpoint, we cannot, as has been done,[5] regard biology as "the science of living things." Biology has grown up with the teachings of Darwin, it is closely connected with evolutionary ideas, and, logically, appears to us in view of these teachings; therefore, we must frame our definitions in accordance therewith.

Huxley's view of the matter was taken up, and has been followed ever since, though now and then it has been modified. One of these modifications appears in a very reputable text-book,[6] biology being defined as "the science which treats of the properties of matter in the living state;" physiology, however, is "the science of action and function, essentially dynamical." I am sure that we could point out many instances of action and function that would never be classified under the heading of physiology or even biology, nay, "*general* biology." On the other hand, I doubt if physiological science is really characterized by the word dynamical; in other words, if "physiological action and function" necessarily presupposes something "dynamical."

1 John P. Campbell, "Biological Teaching in the Colleges of the United States," Bureau of Education, Circular of Information, No. 9, 1891.

2 Botanical Gaz., xvi., p. 301, 1892 (see also pp. 260 and 336). Science, April 7, 1893, p. 184.

3 Science, April 21, 1893, p. 220.

4 Biologie oder Philosophie der lebenden Natur., Vol. 1-6, 1802-1822.

5 Huxley, "On the Study of Biology (Lectures on Evolution)." See "Humboldt Library," No. 36, 1882, p. 37.

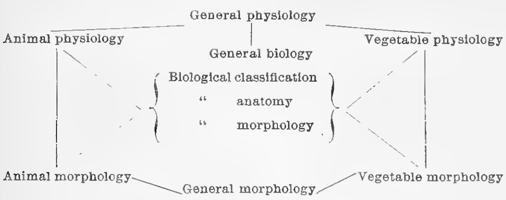
6 Sedgwick and Wilson, "General Biology," New York, 1886, pp. 7-9.

We have had for several hundred years the term physiology, which is the science of the life-phenomena.[1] There is no reason why we should not retain this name, and use it as it has been used ever since the revival of science in the sixteenth century. Biology is of later origin, it was born with evolution, and it is merely a branch of the all-embracing physiology. Biology does not consist of the entire sum of life-phenomena; it is the branch of physiology which treats of *the mutual relationship of the forms of organized matter, especially in view of the theories of adaptations and of natural selection.*

I wish to confine my remarks as far as possible to vegetable biology, and here I shall invite your attention to a very important paper by F. Delpino,[2] who regards biology as the main basis of Darwinism, and points out its importance for the theories of plant metamorphosis. With reference to the latter, we find that Warming[3] will admit only the "definition of metamorphosis" into the biology. Goebel[4] explains the state of affairs very logically in these words: "Biology regards the parts of the plants as if they were not limbs (in morphological sense), but organs, or tools," and thus he mentions one feature of biological investigation, namely, the study of correlation.

If physiology is placed at the head of natural science, and biology in its proper place as a branch thereof, we shall be able to see more distinctly how to reach the ideal, namely, the comparative physiology of animals and plants, for which so much material has been accumulated that we are able to grasp many important features of *life in general.* I have repeatedly[5] referred to this fact, but it will be admitted that the *fundamental definitions* must be logical and not ridiculous.

How biology, in the true and limited sense, branches out into the other departments of botany, I have shown in the following table. We have two features of living things: form and function, and, accordingly, the morphology and the physiology. The table shows



how we get a biological classification, or a comparative systematic botany, or zoölogy. Biological morphology is practically a morphology which deals with adaptations of the different forms to certain ends and comparatively regarded. Biological anatomy is teaching the structural adaptations in animals and plants from a comparative standpoint.

To apply biological characters and features to the systematic part of either zoölogy or botany will tend to make the registration of species and forms more valuable to physiology.

Probably it seems trifling to write quite elaborately about a question of definition. If, however, our fundamental definitions shall be not merely *adaptations for the extension of private power and influence*, we must consider them well. This is not only a question of logical consideration, but of scientific principles.

J. CHRISTIAN BAY.

Missouri Botanical Garden, April 27.

[1] See J. von Sachs, "Vorlesungen über Pflanzenphysiologie," 1887, p. 3.

[2] Fondamenti di Biologia vegetale, I.; Prolegomeni (Revista di Filosofia scientifica, Milano., I., 1880, No. 1, pp. 58–90). See Botanisches Centralblatt, vol. ix., 1882, pp. 333-335.

[3] Warming, in Meddelelser fra den bot. Forening i Kjoebenhavn, I., 192.

[4] Pflanzenbiologische Schilderungen, I., 1889, Introduction.

[5] See Science, March 24, 1893, p. 162; Bot. Gazette, xvii., 1893, p. 105; Biologisches Centralblatt, xiii., 1893, p. 38.

Epidemic Forms of Mental or Nervous Diseases or Disorders.

IT is very desirable that certain data should be gathered on "epidemic forms of mental or nervous diseases or disorders.' As an example of what is meant, I would instance "The Children's Crusade," which occurred in Europe; the persecution of certain individuals supposed to be possessed of witches in New England, and chorea, or St. Vitus's dance, occurring among school children; panic is another form very common, especially at the present day.

Could any of the readers of *Science* furnish me with any information of occurrences which have come under their notice or which they may have read about? They are certainly very common, for one reads of them very often in the daily papers. If some of your "live" readers would consider this subject seriously, and send so full reports as possible, they would not only be doing a personal favor, but would certainly be contributing toward an interesting and important collection of scientific facts.

JAMES WOOD, M.D.

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Color of Flowers.

I HAVE just seen Miss Neal's question in your issue of March 31, 1893, as to how to preserve the colors of flowers when pressing them. If some of your readers have not already sent a better recipe, the following may be found useful.

Immerse the stem of the fresh plant in a solution of 31 grains of alum, 4 of nitre, and 186 of water for a day or two, until the liquid is absorbed, then press the plant in the usual way, sift some dry sand over the flower, and submit to a gentle heat for about twenty hours.

I have found this process pretty successful. A. B STEELE.

Edinburgh, Scotland, April 28.

The Aurora.

IN my contribution to *Science*, April 7, on the above subject, no mention was made (as required by Dr. Veeder in his reply in the issue of April 28) of a particular instance of want of coincidence between auroral display and solar disturbance at the eastern limb, for the following reasons: First, because I have, so far, considered each phenomenon as being dissociated, or rather not connected in the manner stated; second, because I do *not* think it possible to point out such a want of coincidence with the very liberal limits of time evidently comprised in the term "eastern limb" by the advocates of this theory; and, third, amidst the bewildering number of instances, which must occur between even dissociated phenomena of such frequent occurrence, even when the limit spoken of is of reasonably brief duration, it is possible (most probable) that coincidence will be mistaken for cause. That this coincidence is not so great as claimed, seemed to me to be indicated by the results mentioned as obtained by Greenwich, as also by the same conclusion arrived at by Professor Ricco, as mentioned by Dr. Veeder; surely this is a fair assumption to make, if discussion of the same or similar records give results so widely different?

Personally, I do not wish to take any part in this discussion. Dr. Veeder's theory has constantly appeared in the press and by pamphlet without any attempted refutation; believing it to be founded on false premises, I have felt called upon to act as censor, failing any one else.

Granted a very large number of coincidences between auroral displays and the position of a disturbed area at the eastern limb of the sun; if Dr. Veeder will place a limit of, say, twenty-four hours for the term "eastern limb," and consider occurrences beyond this as not being coincidences, I believe he will find that there are as many auroras (I should be inclined with this limit to say, very many more) which occur without this particular solar source of energy as with it. Again, allowing *any* interpretation of the term "eastern limb," and, applying the same interpretation rigorously throughout, I think it will be found that the proportion of coincidences will increase from the minimum sunspot period to the maximum, and that this coincidence will vary directly as the number of sun-spots visible. Now, if there were an intimate connection between the two classes of phenomena, the appearance of an area of great solar disturbance at the eastern limb, as is occasionally the case at the time of a minimum, should give *very* marked auroral displays, whereas it is quite certain that the coincidence is not so marked at these times (where the element of "chance" is reduced) as at the time of a maximum; is this not so?

Auroras are, or are not, an effect of sun-spots on the sun's eastern limb. I spent fourteen months in Hudson's Strait, and, to my knowledge, during the auroral season from 50 to 75 per cent of our clear nights (and clear nights were a peculiarity of the latitude in winter) had auroral displays. Assuming two such solar areas as required constantly on the sun, and representing the term "eastern limb" by twenty-four hours, we have a vastly larger number of auroras unaccounted for than this theory accounts for.

Quoting from Dr. Veeder's letter to *Science*, April 28, he says: "When, however, this area was at the eastern limb, from Jan. 7–11, *although it had not yet developed spots*, and was the seat of brilliant faculæ only, . . . great magnetic storms" were "in progress and auroras . . . reported in high latitudes."

I never saw, nor do I expect to see, the eastern or western limb of the sun when faculæ are visible to ordinary powers, when they were not more distinctly "brilliant" there than elsewhere. If this condition can be taken as a fulfilment of this theory, it is evident that the theory is beyond argument.

This quotation furnishes the required instance "in which an aurora appeared in the absence of *well-defined* solar conditions," for, according to the evidence supplied, "a great magnetic storm" was in progress from Jan. 7–11, whereas I feel certain that Dr. Veeder cannot claim that an area represented by *five* days' solar rotation (Jan. 7–11) could possess (in fact, his words show it did *not* possess) *well-defined* solar conditions of the nature required.

Sun-spots [1] have been a special object of study at this observatory since its institution. It is safe to say that something is known of their nature and origin, but that it is as nothing to that which remains to be investigated. It is possible to allow fanciful attributes to this little-known agency, which will account for any theory we may be pleased to conceive, but, treated in accordance with any known dynamical law, there seems to be no way of accounting for the peculiar action of this force, which is not equally applicable to its position at the western limb. It seems evident, from the nature of a sun-spot's formation, that the force employed is exerted in a vertical direction; it would be reasonable to expect that the resulting maximum effect should be evident, if at all, in the same direction; not horizontally, as this theory requires.

Assuming the solar force to be an "electro-magnetic" one, any resulting auroral development should bear a fixed relation to the line joining the source of energy with the earth's centre and the plane of rotation of the earth. If this is a fact, it is quite evident that points widely differing in longitude on the earth's surface will experience similar effects, as the earth's diurnal motion brings them successively under this influence, after a time-interval almost infinitely less than that represented by the difference of longitude of the two points considered. No one will surely claim that this is even approximately the case.

Again, "cosmical dust and debris" is not conclusively present in the "zodiacal light." Even accounting for the origin of the zodiacal light in this way, it is observationally evident that the rest of interplanetary space is not so filled, for this light is only visible as an appendage to the sun, in certain fixed directions; elsewhere the absence of the light proves that this "dust and debris" is not symmetrically disposed about the sun. Admitting, for the sake of argument, that interplanetary space was filled with this dust and debris, the lapsed æons of planetary existence with the countless orbital revolutions of the planets themselves must have swept out, as the masses of the planets must have aggregated to themselves, the last vestige of such dust and debris, leaving vast intervals without this assumed conducting material.

[1] "Sun-Spots: Their Maximum and Minimum Periods and Zones of Greatest Frequency." Read before the Royal Astronomical Society, April 13, 1882.

I should be pleased, and I think it would be a matter of more than personal interest, if Dr. Veeder has the time, in what I know to be a very busy life (setting the "limit" I have suggested), if he would, from out the fund of information in his possession, see how far the element of "chance" enters into this question, not admitting too much of the suppositional when sun-spots fail at the required period by the substitution of "faculæ," and at the same time show a comparison of coincidences through a semi-period, at least, of solar activity. W. A. ASHE.

The Quebec Observatory, May 6.

BOOK-REVIEWS.

The Earth's History. An Introduction to Modern Geology. By R. D. ROBERTS. New York, Chas. Scribner's Sons. 1893. Maps and illustrations. 270 p. 12°. $1.50.

THIS volume is one of a series now being published in England by Murrray and in this country by the Scribners, as an outcome of the popular University Extension movement. The prospectus states that "the aim of these manuals is to educate rather than inform. In their preparation, details will be avoided except when they illustrate the working of general laws and the development of general principles; while the historical evolution of both the literary and scientific subjects as well as their philosophical significance will be kept in view."

The author of the present volume has been successful in carrying out this plan, for without being detailed he presents the broader aspects of the science in a familiar and pleasing manner. In the chapter on the "Agents of Destruction," he refers particularly to the Grand Cañon region, where the phenomena of denudation are shown on such a magnificent scale. This is followed by chapters on the extent of the destructive operations in Nature, and these, in turn, by other chapters on the construction of land. The constructive agents are grouped under the three heads of deposition, movements of the crust, and addition by extrusion from the interior. There are interesting accounts of shallow-water deposition, of calcareous deposits, such as coral reefs, and of deep-sea deposits. The author does not commit himself in regard to the origin of atolls, referring to Darwin's theory of subsidence, but not discussing others that have been advanced. An interesting account is given of the formation of Monte Nuovo in 1538 and of the destruction of Krakatoa in 1883.

The last part is devoted to the "Evolution of Land Areas," and we have here the application to geological phenomena of the principles enunciated in the first parts. Two chapters deal with the evolution of the British Islands. Altogether the volume gives an excellent exposition of geological phenomena and must serve as a useful compend to all who desire a knowledge of the principles without having to wade through a mass of details concerning the subject. For these details other volumes must be consulted. JOSEPH F. JAMES.

Washington, D. C., May 3.

Public Health Problems. By JOHN F. J. SYKES. The Contemporary Science Series. New York, Charles Scribner's Sons. 8°.

THE multiplication of books relating to public health may perhaps in itself be encouraging, but the fact that the quality in no way keeps pace with the quantity is quite the reverse. The book before us covers a wide field — from "heredity" to "dwelling-houses" — but conveys, whether rightly or wrongly, the impression of being in the main the result of a "cram." The chapter on heredity, for example, opens with this remarkable statement, "The Darwinian theory of natural selection has given prominence to two schools of evolutionists, the one attributing evolution solely to selection, and the other, whilst not denying the effects of selection, valuing — perhaps over-valuing — the effects of heredity" (p. 8). If the reader be fairly conversant with modern biological literature and be in a somewhat cynical mood he will at least derive some amusement from the rest of that chapter.

It is perhaps unfair to single out the chapter on heredity for especial criticism since the subject is rather remote from the author's main theme. We regret, however, to be obliged to point to the fact that the succeeding chapters are sprinkled with far too many misstatements and that the whole book is marred by a loose and slovenly style. We do not wish to imply that the book does not contain many valuable and interesting facts, but the general lack of precision of statement is painfully evident in a passage like this: "It has been hitherto [sic] held that putrefaction was a chemical action only, but recent researches have shown that numberless microbes are concerned in the process, and without these micro-organisms organic bodies retain their form" (p. 69).

Debatable questions are dismissed in a rather summary fashion, e. g., "There can be no doubt whatever that sewer gas may produce sore throat, diarrhœa, and typhoid fever. . . ." (p. 65). "the germs of disease may be easily carried into the air from refuse and fæcal accumulations" (p. 75).

As for the style, "Koch demonstrated the presence of cholera bacilli in the water of Indian ponds or tanks, probably harboring and multiplying in the banks" (p. 76.). "However urgent those specially familiar with the deteriorating influences at work may regard the remedies applicable, yet they can never secure their adoption without the consensus of the opinions of others" (p. 355).

The author in a measure, however, disarms criticism by his unimpeachable statement in the conclusion (p. 344), "Errors of omission and of commission may be readily found in all human work . . ."

Report on the Brown Coal and Lignite of Texas. By E. T. DUMBLE, State Geologist. Austin. 1892. 243 p. Plates. 8°.

THIS volume is one of the numerous ones that have recently appeared on the work of the survey. It contains a very full account of the origin, formation, and composition of the Brown coal and of its use as fuel. Many details are given of the geology of these deposits in Texas, and comparisons are made between these and the European lignites. A strong argument is made for the use of the Brown coals in Texas, and the results of the investigations made by the author in Europe and in Texas may be summed up about as follows:—

Brown coal and lignite of good quality are capable of replacing bituminous coal for all household, industrial, and metallurgical processes.

Texas has an abundant supply which is so situated as to permit its being mined and delivered for use at a far less cost than bituminous coal.

The raw coal can be used in stoves and grates, under locomotive boilers, in iron smelting, lime burning, etc. It may be used for the manufacture of gas for lighting or heating. It can be made into artificial fuel by "briquetting" with coal-tar, pitch, etc., and then used like ordinary coal. Certain varieties, if charred, will form a coke with coking coal and coal-tar pitch, which can be used for locomotive engines and other similar purposes.

These facts are of great importance to a country like Texas, where wood is practically absent, and where the ordinary soft coals and anthracite are nearly unknown. There seems no reason why similar deposits of lignite in other States west of the Mississippi River should not be utilized. JOSEPH F. JAMES.

Manual of Machine Drawing and Design. By D. A. LOW and A. W. BEVIS. London and New York, Longmans, Green & Co. 1893. VI. 375 p. 8°. $2.50.

THIS excellent work is designed for the use of engineers and their apprentices, and for students in technical schools, and is admirably adapted to its purpose. It is more a drawing-room

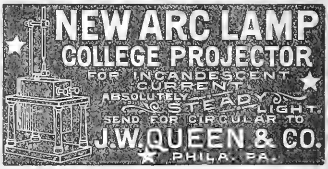
CALENDAR OF SOCIETIES.

Anthropological Society, Washington.

May 16.—Annual address by the president of the society, Dr. James C. Welling, The Last Town Election in Pompeii: An Archæological Study of Roman Municipal Government based on Pompeian Wall Inscriptions.

Society of Natural History, Boston.

May 17.—Clarence J. Blake, Out of Darkness into Light; or, The Education of a Blind Deaf-Mute. Mr. Anagnos kindly consented to allow Miss Thayer of the Kindergarten for the Blind, and her pupil, Willie Robin, to be present.



text-book than one for class-room use, such as Unwin's. It includes less mathematical discussion than samples of good designing, graphically presented. It is full of excellent "dimensioned" illustrations of a great variety of machinery, and especially of machine-tools and steam-engines. It gives a large number of rules and tables of proportions of parts of machinery taken from the standard treatises and from the note-books of skilled designers. In many cases the methods of computing sizes and proportions are given. The drawings have all been prepared from working drawings, and especially with a view to their use for this purpose. Standard and successful practice is thus laid before the young student, or practitioner; and the art of proportioning is thus not only acquired, but the novice is, at the same time, made familiar with the best designs of his seniors. A combination of this work with that of Professor Unwin would seem likely to make an ideal course; the one being used in the drawing-room, the other in the class-room in conjunction with lectures. For the ambitious apprentice, no better plan could be recommended than a similar course of private reading and practice.

The Philosophy of Individuality, or the One and the Many. By ANTOINETTE BROWN BLACKWELL. New York, G. P. Putnam's Sons. $3.

THIS work is a new attempt to solve the problem of the universe. It is by no means easy reading, the style being at once verbose and obscure, and the same thought is often repeated again and again, without ever being made clear. The fundamental doctrine of the book is a new theory of matter, namely, that matter is not a substance at all, but merely a complex of motions; or, in the author's own words, that "matter is literally composed of aggregated and coöperative modes of motion." Even an atom is regarded as nothing but a combination of balanced and correlated motions: "Our atom of matter, then, is a unit of motions with innate energy enough to achieve vastly more than has yet been required of it by physical evolution." This view is expounded and illustrated through several chapters, and the endeavor is made to show how the theory applies to what we commonly call substances, and to distinguish between these "complexes of motion" and the "free motions" of heat, light, electricity, and gravitation. The theory is admitted to be nothing but a hypothesis, and we fear that it will always remain so.

Passing now from the realm of matter to that of mind, the author presents a theory of mind and consciousness similar to that of Herbert Spencer, that mind and matter are merely two aspects of one underlying reality. It differs, however, quite radically from Spencer's view in regarding life and consciousness as attributes not of an organized body only, but of each individual atom: "The rhythmic atom is *alive* with the high possibilities of ever-growing sensibility and actual knowledge." The objection that there is no evidence of life or feeling in inorganic matter, Mrs. Blackwell endeavors to meet by the theory of "potential mind" and "nascent feeling," phrases which are made to do duty instead of arguments and proofs. The grand difficulty with such a theory is to account for personality; for, if every atom is sentient by itself, it would seem that I must have as many minds as there are atoms in my body, and Mrs. Blackwell is by no means successful in removing the difficulty. "We assume," she says, "that the one commanding ego in each higher organism is exclusively but one individual unity!" but, notwithstanding her exclamation point, there is no warrant in her theory for such an assumption

Such are the fundamental doctrines on which the author seeks to found a rational theology and a belief in the immortality of the soul, but we find little in her arguments that is convincing or satisfactory. The whole theory is hypothetical; and, while we recognize the earnest purpose of her book, we cannot think that she has added anything important to our knowledge of nature or of man.

SCIENCE

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SCIENCE

NEW YORK, MAY 26, 1893.

THE NORIAN ROCKS OF CANADA.

BY ANDREW C. LAWSON, GEOLOGICAL LABORATORY, UNIVERSITY OF CALIFORNIA, BERKELEY, CAL.

AN important advance has been made in American Archæan geology by the publication of a memoir, by Professor Frank D. Adams of McGill College, Montreal, on the Norian or Upper Laurentian rocks of Canada.[1] The memoir embraces the results of five seasons' field-work conducted for the Geological Survey of Canada, together with an exhaustive and masterly petrographical investigation in the laboratory of the rocks constituting the Norian terranes. A full bibliography of these interesting rocks accompanies the memoir; also a map showing their known distribution, and a table of twenty-four analyses compiled from various sources. Thoroughness of research and soundness of judgment characterize the work throughout, and all interested in Archæan geology or in general petrography will welcome it as one of the sure steps which make for scientific progress. The author introduces his subject by a general presentation of the geology of the Laurentian, in which he gives a brief history of the growth of our knowledge and theories of these rocks, and discusses their nomenclature, both geological and petrographical. In the latter connection he takes occasion to describe, in general terms, the rock of which the Norian is prevalently constituted, and makes good its claim to be regarded as a distinct petrographical type, the characteristic of which is the preponderance of plagioclase to the partial or entire exclusion of the ferro-magnesian silicates. For this type of rock he uses the term "anorthosite," in accordance with a usage which has long been in vogue among the Canadian geologists. The anorthosites are regarded as belonging to the gabbro family, constituting one extreme of a graded series, of which pyroxmites and other granular rocks, rich in ferro-magnesian minerals to the practical exclusion of feldspar, are the other extreme, while common gabbros, norites, etc., are the middle members.

The various regions occupied by these anorthosites are described, and the petrographical and geological relations are discussed in detail. Their occurrence appears to have a peculiar relation to the so-called Archæan nucleus of the continent. So far as explorations serve us, they are only known to occur on the periphery of this great "Canadian shield," along the line of the St. Lawrence drainage, from near its source to the Straits of Belle Isle, and again farther north on the Labrador coast. Professor Adams calls attention to the seeming analogy between this distribution and the modern distribution of volcanoes on the periphery of the continents. The more important anorthosite occurrences, both by reason of their great extent and for the greater study that has been given to them, are (1) the Morin region, north of Montreal, and (2) the Saguenay region, which is the largest known. The Morin mass has a diameter of 37 miles and an area of 990 square miles. It is surrounded by the gneisses, crystalline limestones, quartzites, etc., of the Grenville series (Laurentian), through which it is clearly eruptive. It holds included in it blocks of the surrounding gneiss, and the latter is traversed by apophyses from the main mass; and at places distinct evidence of an alteration contact-zone is observable. The deeply-worn surface of both the Laurentian and the invading anorthosite is overlaid by flat-lying, unaltered Cambrian strata. The age of the anorthosite is thus limited as Post-Laurentian and Pre-Cambrian. Petrographically, the anorthosites are characterized by a peculiar cataclastic structure, which is most pronounced where the rock mass evinces a schistose or foliated structure. This cataclastic structure is ascribed to pressure acting on the mass and deforming it while it was yet deep in the crust of the earth, and at a temperature probably near its fusion point. In addition to the dominant mineral plagioclase, many other rock-forming minerals occur, such as augite, hypasthene, ilmenite, hornblende, biotite, etc. Most of these play a very subordinate *rôle* as normal constituents, while others are rare accessories; and some are secondary or decomposition products. Certain layers of anorthosite occur intercalated with the gneiss and crystalline limestone of the surrounding Grenville series. These layers of apparently interbedded anorthosites vary in thickness from one yard to several hundred yards, and in length from half a mile to eight miles. The apparent interbedding is due to the intrusion of the anorthosite within the strata of the Grenville series.

The Saguenay region presents a mass of anorthosite of still more extensive proportions, occupying not less than 5,800 square miles. The anorthosite of this region is essentially similar to that of the Morin region, both petrographically and in its relations to the Laurentian and to the base of the Palæozoic. There are, however, some differences in petrographical detail; one of these being the common occurrence of olivine among the ferro-magnesian constituents. The Laurentian gneiss of the surrounding region corresponds rather to the Ottawa gneiss than to the Grenville series, and the contact phenomena on the periphery of the anorthosite mass are more confused than in the case of the Morin mass.

The other localities where occurrences of anorthosite rocks are described are: In Labrador, in Newfoundland, on the north side of the St. Lawrence River and Gulf, in the State of New York, and on the east coast of Lake Huron. To this list of localities the present writer may be permitted to add northern New Jersey and the northwest coast of Lake Superior, where he has met with anorthosite rocks in the field.

Among the more important results of Professor Adams's work may be mentioned: —

1. The clear recognition, as plutonic eruptive formations, of rock masses, which, being petrographically and geologically units, have each an enormous extent. The Morin mass occupies nearly 1,000 square miles, and the Saguenay mass is at least about six times that area. For such irruptive plutonic masses Suess has proposed the designation "batholite," and it will facilitate discussion to use that term. The recognition and description of the laccolitic type of mountain structure by Gilbert has been prolific of very fruitful results. The fact that there are such structures was so ably presented by their discoverer that it was warmly and generally received, and it has become an important element in discussions of orogeny. The fact that there *are* batholites has not had so vigorous a presentation, and the important *rôle* which they play in the structure of the earth's crust has not received the recognition which its importance merits. The able account which Professor Adams gives us of these great batholites of anorthosite is, therefore, a welcome addition to our knowledge of such structures. The present writer has elsewhere endeavored to call attention to the existence of granitic batholites in the Rainy Lake region, which are comparable in size to the anorthosite areas of Quebec; and it seems to him that a more general appreciation of the importance of these great igneous masses would simplify and advance our discussion of certain problems of tectonic geology, metamorphism, and geognosy. If it be a fact, as Professor Adams and the writer have shown for two distinct regions, that immense masses of igneous matter, ranging from 25 to 100 miles or more in diameter, have invaded the crust from below, dis-

[1] Ueber das Norian oder Ober-Laurentian von Canada. Stuttgart, 1893.

placing or absorbing it, is it not likely that such an event has occurred frequently and in many different parts of the earth's crust? Geological records are, indeed, full of suggestions that such is the case. Yet in all our current discussions of orogeny, epeirogeny, and regional metamorphism, how little is this factor in the problem considered? What part has the development of the Nova Scotia batholite played in the folding of the Cambrian strata of that province, in their metamorphism and in their becoming charged with gold? What part has the development of the great batholite of the Sierra Nevada played in the folding and metamorphism of the earlier Mesozoic rocks of California and in their becoming charged with gold? What part has the development of the great British Columbian batholite played in the folding and metamorphism of the earlier Mesozoic strata of the west coast of that province? Was the British Columbian batholite synchronous in its development with the Sierra Nevada batholite? Are they separate and distinct affairs, or are they simply geographically separate manifestations of one stupendous process of crust development? In either case has not the exposure by denudation of these great batholites and their intrusive relations to the surrounding terranes practically reproduced the conditions which we find in the Archæan terranes of the Canadian plateau? These are a few of the questions which can only be profitably discussed when the batholite is recognized as a much larger element in tectonic geology than the dyke, the neck, the boss, the sill, or the laccolite. Batholites abound. Why should they not be recognized?

2. A second important result is the immense simplification which is effected in Archæan geology in the Canadian territory, where most questions of that ilk must find their final solution. Hitherto the Norian rocks have been classed as part of a supposed system of metamorphic sedimentary strata known as the Laurentian. This system was divided by Logan into an upper and a lower division, the latter being sub-divided into two parts, viz.: the Grenville series and the Ottawa gneiss, so that his scheme stood thus: —

Norian series =		Upper Laurentian
Grenville series = Upper division	}	of Lower Laurention.
Ottawa gneiss = Lower division	}	

The recognition of the irruptive character and post-Grenville age of the Norian rocks is a great gain, and reduces the Laurentian system to two members. The simplification thus effected suggests to the present writer still other possibilities in the same direction. It seems probable that the Grenville is a profoundly metamorphosed series of sedimentary strata. Its bedded character and the fact of its being composed of strata of limestone, quartzite, iron ore, graphite, etc., in addition to the gneisses, favor this view. The Ottawa gneiss, on the other hand, has a very different character. There are no beds of limestone, or quartzite, or iron ore, or graphite. The mass of the formation is eminently granite, with gneissic foliation, which in some cases is well defined, and in others vague or almost absent. What is the relation of the Ottawa gneiss to the Grenville series? The former would be recognized by any petrographer as a granite — a plutonic igneous rock. Professor Adams recognizes the geological identity of the Ottawa gneiss with the Laurentian gneiss and granite which the writer has described as invading the upper division of the Archæan complex (Ontarian system) in the region northwest of Lake Superior. There the igneous irruptive and batholitic character of the granites and gneisses (= Ottawa gneiss) and its invasion of the Upper Archæan rocks is unequivocally demonstrated by evidence which has been abundantly adduced elsewhere. Does the Ottawa gneiss of the Ottawa valley bear a similar batholitic and intrusive relation to the Grenville series? From what the writer knows of the region, it seems to him eminently probable that such will be found to be the case. This hypothesis is favored somewhat by certain harmonious analogies which it would establish between the Archæan complex in the Lake Superior region and the region of the Lower Ottawa. Generally, the Archæan complex throughout Canada, omitting the Norian, is composed of two great divisions. The lower division seems generally to have the petrographical characters of the Ottawa gneiss. The upper division is usually recognizable as an assemblage of metamorphic, sedimentary, or mixed sedimentary and volcanic strata. Part of this upper division has usually been referred to as Huronian, but, according to several authorities, this term was originally applied to a post-Archæan series on the north shore of Lake Huron; and there is some confusion attending its use. Even when applied to Archæan rocks, the term has embraced only a portion of the upper division of the complex. In western Ontario, this upper division includes at least one other group besides that which has usually been called Huronian. The writer has elsewhere proposed the term "Ontarian system," as a comprehensive designation to embrace the whole of the upper division of the Archæan in western Ontario. Now it seems to the writer that the Grenville series in Quebec occupies the same stratigraphical position in the Archæan complex as does the Ontarian system (embracing Contchiching and Keewatin [Huronian?]) in western Ontario. Admitting, for the sake of clearly stating the hypothesis, that the Grenville series is the equivalent of Ontarian system, or any part of it, we would have the following parallelism: —

	Western Ontario and Minnesota.	*Eastern Ontario.*	*Quebec.*
In order of superposition.	Ontarian system	Hastings series	Grenville series
	Laurentian system	Ottawa gneiss	Ottawa gneiss
	[1] Carltonian { Anorthosites of Minnesota		Norian
In order of chronological sequence; an irruptive rock being of later age than the formations which it invades.	[1] Carltonian { Anorthosites of Minnesota		Norian
	Laurentian System { Batholitic Granites and Gneisses	Ottawa gneiss	Ottawa gneiss
	Ontarian System	Hastings series	Grenville series

[1] See Bulletin, No. 8, Geolog. and Nat. Hist. Surv. of Minnesota.

If this hypothetical correlation should ever be established, it would then seem that different names and different stratigraphical positions had been given to groups of strata geologically equivalent because of their petrographical dissimilarity. The Grenville series is characterized by limestones and quartzites, with little or no volcanic admixture. In the Ontarian system of western Ontario sedimentary rocks, in a more or less metamorphic state, are common enough; but there is a scarcity of crystalline limestones and quartzites, and altered forms of volcanic rocks abound. This petrographical dissimilarity, however, in no way militates against their geological correlation. It is interesting to note in this connection that the Hastings series, which is geographically between the Quebec region and the Lake Superior region, is intermediate in petrographical character between its suggested equivalents on either side. By some authors it has been correlated with the Grenville series, and by others with the Huronian (Archæan).

GEOLOGY OF TUCUMCARI, NEW MEXICO.

BY W. F. CUMMINS, TEXAS GEOLOGICAL SURVEY, AUSTIN, TEXAS.

IN 1852, Professor Jules Marcou, as United States geologist, made a trip across the country with the engineers who were sent out to survey a railroad route from Fort Smith, Ark., to the Pacific Ocean, near the thirty-second parallel. On that trip he passed through the Tucumcari region, and published a description and section of Pyramid Mountain, one of the representative buttes. From the strata at Pyramid Mountain and other places in the vicinity he collected a number of invertebrate fossils, two of which he figured and described as *Ostrea marshii* and *Gryphæa dilatata*, var. Tucumcari, Marcou, and referred them to the same species as those described in Europe under the same names.

The collections of Professor Marcou were placed, by order of the Secretary of War, in the hands of W. P. Blake for description. He differed entirely with Professor Marcou in his identification of the fossils, and referred them to *Ostrea subovata*, Shumard, and *Gryphæa pitcheri*, Morton. Others engaged in the controversy, which became very personal and bitter, and the wounds made by the lances of the combatants have not healed to this day.

The locality was not visited again by any geologist until, in 1888, Mr. R. T. Hill visited the place and made a short stay. He visited it again in 1891 and made further observations. In 1891 I visited the locality, made many sections of the hills in the vicinity, and collected a large number of fossils. This comprises all the geological work in that immediate vicinity, so far as I am informed.

Professor Marcou referred the strata to the Triassic and Jurassic, basing his conclusions as to the Jurassic upon the fossils found there, which he claimed were identical with those found only in the Jurassic of Europe. There is now no dispute about the correctness of his reference of the lower beds to the Triassic.

After Mr. R. T. Hill's first visit he published a paper, in which he said he was inclined to confirm Marcou's reference of the upper beds to the Jurassic. After his second visit he again confirmed Marcou's reference; but upon further consideration he concluded that the beds belonged to the Cretaceous.

When I visited the place I took time to collect fossils and study the stratigraphy and lithological character of the several parts of the formation, and the result was that I could not agree with either of my predecessors I found evidence there of the existence of the Triassic, Cretaceous, and Tertiary. Since the publication of Mr. Hill's latest paper there is no disagreement between us. Professor Marcou still contends for the correctness of his reference of the upper beds to the Jurassic.

The evidence of the Cretaceous age of the middle part of the strata is based upon the fossils I found in the beds, associated with those from the same place found by Marcou. The following is a list of the fossils found by me:—

Gryphæa dilatata, var. Tucumcari Marcou; *Ostrea marshii*, as determined by Marcou; but in reality *Ostrea subovata*, Shumard; *Gryphæa pitcheri*, Morton; *Exogyra texana*, Roemer; *Ostrea quadriplicata*, Shumard; *Trigonia emoryi*, Con.; *Cardium hillanium*, Sow.; *Cytheria leonensis*, Con.; and a single leaf of a dycotyledonous plant, which I described and figured under the name *Sterculia Drakei.*

It will be apparent to everyone acquainted with the fossils of the Cretaceous that those enumerated belong only to Cretaceous strata, and, if taken from the beds of the Tucumcari region and correctly determined, the conclusion that the beds are Cretaceous would be inevitable.

Professor Marcou, therefore, seeing this, in reviewing my publication, endeavors to avoid the conclusion by saying that either the determinations of the fossils found by me were incorrect or that they did not come from that locality, and suggests that the labels on my packages were loosely put on and became mixed with collections made elsewhere; and on this flimsy subterfuge (to give it no harder name) still insists on the correctness of his reference to the Jurassic.

A simple recital of the circumstances attending the collection, shipment, and determination of the fossils under consideration will be sufficient to satisfy any reasonable mind on both doubted points, especially in the absence of any motive for deception on my part. The facts are that for more than a month prior to the collection of the fossils in dispute we had not collected a single one from any Cretaceous bed, and every package previously collected had been shipped to the museum. Those collected at Tucumcari were shipped overland to Las Vegas, New Mexico, were delayed there for several months, and did not arrive at Austin until every package collected from other localities had been opened and put in the cases. When the boxes containing the Tucumcari collections arrived, instead of opening them as the other collections were in the storage room, I had them taken to my private room, opened them myself, and put them in a separate case, where they are now with the labels originally placed on them in the field. There were at least fifty packages of these fossils, and each package had two labels attached, so that it is utterly impossible for them to have become mixed by accident or carelessness.

Again, myself and my assistants discussed the fossils in the field as we picked them up, and our note-books show that we then determined them as they are now designated. There can therefore be no reason for supposing that the fossils did not come from that locality, notwithstanding Professor Marcou says that he did not find such fossils there, as if that fact could justify him in saying another explorer did not. Marcou travelled rapidly through the country, made a section at one place, and devoted six hours to the examination of the strata at that precise locality, while I travelled at my leisure, and took all the time necessary to collect the fossils.

He says he has seen the collection of Professor A. Hyatt made in that vicinity, and that there are none of the fossils enumerated by me in his collection. Professor Hyatt has never said that he collected fossils from that locality, and so far as I know he never did; but even if he had, would that be a reason for concluding that another person could not find other fossils? Professor Hyatt has written no paper on that region.

As to the correctness of the determination of the fossils, I took every precaution to prevent any mistake in this matter. I did not wholly rely upon my own judgment, but, after opening up my collection, I made up small suits and sent them to various parties for determination, without giving them the location from which they were collected, but simply asking for specific determinations, and without repeating what others had said, or even giving my own conclusions, and there was unanimous agreement as to all the species I have published.

It will thus be seen that I have taken extraordinary care to be certain of my facts before publishing them and my conclusions drawn therefrom.

The evidence of the Jurassic age of the beds relied upon by Professor Marcou is based upon two species found in the beds, described by him as heretofore mentioned. One of them he calls *Gryphæa dilatata*, var. Tucumcari Marcou, and the other *Ostrea marshii.*

After making my collection at Tucumcari, I sent to Europe for samples of the *Gryphæa dilatata* from the type localities and compared them with Marcou's variety collected by me. The best that can be said is that it may be a variety of the original type.

The samples collected by me of what I suppose was his *Ostrea marshii* are not *O. marshii*, but *O. subovata* of Shumard. We have hundreds of specimens of *O. subovata* in the museum, collected from well-known Cretaceous horizons, and upon comparison with them the specimens from Tucumcari are found to agree in every essential particular. Therefore the proof of the Jurassic age of the beds is narrowed down to one fossil, and that only a variety of the form found in the Jurassic of Europe, and which has not been reported from any of the well-known Jurassic horizons in North America. This will certainly not be considered sufficient to establish the Jurassic age of the beds when there is associated with it the other forms enumerated which are certainly Cretaceous.

I placed a great deal of stress upon the fact of having found in these beds a dicotyledonous leaf, as proving the Cretaceous age of the beds, for the reason that, so far as I know, no dicotyledons have been reported from any strata lower than the Cretaceous, in either North America or Europe. It is true that they have been reported from beds which some geologists held to be Jurassic, but which by others were referred to the Cretaceous upon the very ground that they contained dicotyledons.

Professor A. Hyatt has been quoted as expressing the opinion in private that the beds at Tucumcari were Jurassic, but in a recent article he deprecates such a use of his opinions privately expressed, says it was unauthorized, and asserts that he has no opinion on the subject.

THE MARINE BIOLOGICAL STATION OF THE UNIVERSITY OF TEXAS.

BY CHARLES L. EDWARDS, AUSTIN, TEXAS.

ANY ONE familiar with the brilliant pioneer work of Agassiz, Pourtalès, and Brooks, together with their students, upon the fauna of the Gulf of Mexico and neighboring waters will gladly welcome the inauguration of any movement, however modest, to continue the exploration of the American Mediterranean. The successive summer expeditions sent from the Johns Hopkins University since 1887, to the different Bahama Islands and Jamaica, and the proposed establishment of the Columbus Station on the latter islands, are familiar to all interested in the work of this field.

An important part of the School of Biology, created in January, 1892, by the regents of the University of Texas, both as regards instruction and research, is the Gulf of Mexico Station. As a result of a brief preliminary survey which, at the request of the regents, I made last summer at Aransas Pass and Galveston, several facts of prime importance in locating a biological station became apparent. The low Texas coast is bordered by exceedingly shallow bays, from two to ten miles wide, cut off from the Gulf of Mexico by a very narrow sand-formation. This almost continuous stretch of sand, raised unevenly by innumerable dunes formed by the wind, is broken at eight places by narrow channels into seven islands, and at three other points partially unites with the mainland to form extended peninsulas. Its gulf shore is unindented, while, on the other hand, its bay shore-line is quite irregular. Vast areas of the bays are exposed at low tide, forming mud-flats; while even in Corpus Christi and Matagorda Bays the depth does not exceed fifteen feet. Since the mean tide is less than half a foot at most places in the bays, an advantageous location for a biological station must be contiguous to the free waters of the Gulf. Even then one must go from along most of the outer shore five miles to seaward, in order to reach the ten-fathom curve. Directly off the entrance to Galveston Bay this depth of less than ten fathoms extends for a distance of thirty miles.

Reaching from near the mouth of the Rio Grande along the extreme southern Texas coast for one hundred and fifty miles northward is Padre Island. The bay which it cuts off, Laguna Madre, is for the most part a vast mud flat, and the Padre itself is inaccessible. Farther north, at either Aransas Pass, where Corpus Christi and Aransas Bays empty into the Gulf, or at Pass Cavallo, the entrance to Matagorda Bay, would be, with a suitable building, an excellent location for the station. The entrance to Galveston Bay, while in some respects not having the natural advantages of the other two locations, yet is much more accessible. Here is a highly desirable building, which cost some $15,000, soon to be vacated by the Quarantine Department. Since this building belongs to the State, the Galveston location was recommended and a bill was introduced in the 23d legislature of Texas to set aside the present officers' quarters of the Quarantine Department at Galveston for the purposes of the Marine Biological Station of the University of Texas. With a further item of $5,000 for equipment, the bill was favorably reported from the Committee on Grounds and Buildings of the House, but, owing to the large number of measures having precedence, this bill, unfortunately, was not considered.

Besides a building fully supplied with the necessary aquaria, microscopes, reagents, etc., for laboratory study, and boats of light draught for work in the shallow water, it is planned, after the idea of Dohrn[1] for the Naples Station, to equip a seaworthy steamer as a floating station for deep-sea collection and observation in the waters of the Gulf of Mexico. For the wonderful possibilities of this field no addition to the eloquent testimony of A. Agassiz,[2] in his description of the work on the "Blake" expeditions, need be added.

As Professor Whitman has so ably demonstrated in building up a national station for marine biology at Woods Holl, it is in generous coöperation that the science can best be advanced. The University of Texas extends a welcome to any investigator in the various lines of biology who may desire access to the fauna and flora of the Gulf of Mexico. Going from the various stations established by Agassiz, Brooks, Whitman, and others on the Atlantic and Gulf coasts, to that of Jordan on the Pacific, the investigator may have the enviable advantage of studying a special group of animals or plants under the most diverse geographical conditions. Once this migration, which in Germany is so enlarging and helpful to the student, is made possible among our biological stations, the great advantages are too apparent to need mention.

Since the best conditions are not this summer available for work on the Texas coast, it has been decided to hold the first session of the marine station in the Bemini Islands, Bahamas. Applications for admission will be received until June 20. Of investigators no special fee is required. For students not attending the University of Texas, there is a laboratory fee of $25. The necessary expenses for the session, including transportation from Austin and return, will approximate $100.

[1] Bericht über die Zoölogische Station während der Jahre 1885–1892. Mittheil. Zoöl. Station, Neapel, 10 Bd., 1893, pp. 633–674.

[2] Three Cruises of the "Blake," Boston, 1888.

METHOD OF MAKING A SANITARY INVESTIGATION OF A RIVER.

BY CHAS. C. BROWN, C. E., UNION COLLEGE, SCHENECTADY, N. Y.

THE following programme of the investigation of the Hudson River and its tributaries has been worked out in our labors for the State Board of Health during the last four years, and may be of interest. The work commenced with an inspection of the shores of the river to determine the causes of certain nuisances which existed along its banks and to determine the method of abating these nuisances. These consisted principally of marshes or badly drained pools with some areas partly covered with water at high stages but open to the sun at low water. A few nuisances arising from the deposit of garbage or the discharge of sewage were also found. All these were evident on inspection and it was possible to abate most of them with little difficulty. In any other case the same would usually be true, except where there are large areas of bottom land which are overflowed by high water and are not well situated for drainage after the high stage is passed. It is possible, however, except in the largest watersheds, to drain much the largest part of such lands. There is no question that such bad conditions as are often found have a decided effect upon the salubrity of the neighboring lands, and that sooner or later treatment of the problem of drainage must begin and be carried through as rapidly as funds will permit.

Where a river is also used as a source of water supply, a much more detailed study must be made of its condition and possibilities. In the case of the Hudson, a study of the geology of the watershed was made to show what the inorganic chemical impurities of the water might be, the result being very favorable to the purity of the water, as much of the area is covered by the oldest formations and supplies but little inorganic matter of any sort. The southern tributaries of the Mohawk bring in more such matter, in the form of lime from the Helderberg and neighboring formations, than any other part of the watershed. Where the surface soil is made up of disintegrated rock it may have a beneficial effect upon the water by acting as a filter to remove some of the organic matter, or it may have a deleterious effect by adding much alkaline matter to water percolating through it. In some cases this may be so serious as to prevent the use of water from some parts of the watershed for water supply. In connection with this study of the geology goes the study of the organic pollution from vegetable sources, since much of this comes from marshes and swamps whose existence is due to the arrangement of the geological strata. There are cases where the amount of such pollution is excessive. It is probable, however, that there are very few cases where the swamps cannot be drained and thus greatly improve the condition of the water. Much of the water from the upper Hudson and the northern tributaries of the Mohawk shows the effect of this sort of pollution, but the dilution from comparatively unpolluted sources is great enough to reduce it far below the objectionable point.

After this study, which usually is not very important, comes the study of the pollution by the population on the watershed. In investigating the purity of an established supply or in selecting a new one this is the most important study of all. We have approached the subject in three ways, keeping the results obtained by the three processes in juxtaposition so that they can be used as mutually explanatory. The first and, after sufficient experience in judging effects, the best method is by actual inspection of the polluting matters discharged into the stream. These matters consist of sewage, garbage, drainage from fertilized fields and other sources of animal matter, and the discharges from manufacturing establishments, some of which are chemicals and some putrescible organic refuse. In connection with this inspection goes a determination of the amount of water flowing in the stream at the point of entrance of the polluting matter A careful study of the relative amounts of polluted liquid and river water with the proper consideration of the amount of pollution the water already contains and the character of the new supply will give a very clear idea of the condition of the resulting mixture. Detailed study by chemical and biological methods of typical conditions will bring one to the ability to determine by the inspection the probable condition of new streams in approximately the same circumstances. In our own case, having but little material obtained under the conditions existing in this country, it was necessary to make this detailed examination.

The second process applied to the river was, therefore, the chemical analysis of numerous samples from various places. It is usually considered that the elements determined in water analysis which denote the amounts of organic matter in different forms and the amount of salt are the important ones. The list included, therefore, albuminoid and free ammonia, nitrogen as nitrates and as nitrites, chlorine, oxygen absorbed, as well as the total solids, loss on ignition, color, appearance, and odor at 100° F.

The third process applied was the biological analysis of samples of water taken from the same places as those for chemical analysis and from many more. The biological analysis may be made with reference to the number and kinds of algæ, infusoria, and other microscopic animal and vegetable life, with reference to the numbers of bacteria, and with reference to the numbers of such species of bacteria as can be recognized as coming from sources which are dangerous or suspicious. All of these determinations will be valuable in deriving a basis for a definite opinion as to the character of the water. A few experiments were made under the first head, and it was found that as regards flowing water in rivers this determination was not as necessary as others, moreover, the methods of such analysis have been well elaborated by others, therefore it was left until a more convenient season. It is advisable to make this analysis on some samples, however, to secure the fullest knowledge possible.

As regards the determination of numbers of bacteria, it was soon discovered that in order to determine the relative amounts of pollution in the rivers at different places, it is necessary to reduce the possible sources of addition to the numbers of bacteria to a minimum. This is done by waiting until a rainless period, or nearly so, has intervened of sufficient duration to reduce the river to its low stage and then give time for the collection of the samples. It is possible by taking this precaution to show the increase in pollution by showing the increase in numbers of bacteria below the source of pollution. Rains, sharp and heavy or long continued, will wash into the stream much matter from the soil, abounding in bacteria, so that the indications of pollution from the constant flow of sewers and the like will be greatly obscured. It is probable, also, that it would be difficult to compare the purity of two streams in widely different parts of the country or in different conditions as to soil, slopes, area of watershed, etc., on account of the consequent variations in numbers from what may be called outside sources. Our work has shown the possibility of determining the relative increase in pollution in the flow of a single river when proper precautions are taken. One point to be mentioned is that where sewage is discharged into still water much of it will settle at once and samples taken near the surface will not show the full effect of its presence.

The numbers of bacteria, at least unless determined without the greatest care to eliminate all disturbing conditions, are not therefore so important an indication as is desired. It is well known that there are species of bacteria which exist under their most favorable conditions in the intestines of men and the higher animals, but will live for a greater or less length of time in other places. If the numbers of such bacteria could be ascertained the relative amount of pollution from such suspicious sources would be best determined. On the recommendation of Dr. Theobald Smith of Washington we tried the method of determining the numbers of *Bacillus coli communis*, by the fermentation-tube method. This bacillus was selected because it is the most common in fecal matter and its growth in such media as ordinary river water at ordinary temperatures is believed to be very slow, if there is any. The method suggested by Dr. Smith was found to be easily applicable in practice, and some very valuable results have been secured. It promises to be a most valuable aid in determining the pollution of water, and is especially valuable because it gives the statement of the serious part of the pollution, while all the other methods mentioned are open to the objection that they may reject a water which has a large amount of pollution not from dangerous sources, the water being, therefore, comparatively clean; while they may, on the other hand, pass as good water which shows but little actual amount, that amount being of a very suspicious nature.

Many of the results of the investigation whose programme is presented above, are given in the last four reports (tenth to thirteenth) of the N. Y. State Board of Health. A paper by Dr. Smith, giving the argument for the selection of *B. coli communis* as the index of pollution, is given in the thirteenth report. The investigation is not yet finished, and other reports will follow until sufficient data have been obtained to warrant a definite statement as to the condition of the water at the intakes of the various waterworks which draw from the lower parts of the rivers under investigation. It is believed that the work done shows that the programme given above is the proper one to follow, and this statement of it is therefore made with the hope that it will be found useful in other cases. Considerable experience in selecting conditions and in collecting samples and data will be necessary to make one expert in drawing definite conclusions from the results obtained.

NOTES AND NEWS.

THE museums and laboratories of the University of Pennsylvania are represented by a very considerable exhibit at the Columbian Exposition. The University has erected an inclosure on the space assigned to it in the gallery of the Liberal Arts Building, for which the design was contributed by the University School of Architecture. This forms the exhibit of that school. Within this space are contained various collections from the different departments. Notably the Veterinary and Biological. The latter contributes a psychological laboratory, which will be in operation during the summer. The Department of Archæology has sent collections from three of its sections, American, Babylonian, and Egyptian, which are now installed in the gallery. The large collection of games and religious objects contained in the Oriental Section of the Museum will be shown in the Anthropological Building.

— The present interest in subjects connected with the study of sociology has led Professor Charles R. Henderson of the University of Chicago to prepare "An Introduction to the Study of the Dependent, Defective, and Delinquent Classes," which will be published about June 1 by D. C. Heath & Co., Boston and Chicago.

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Attention is called to the "Wants" column. It is invaluable to those who use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The "Exchange" column is likewise open.

SIGN LANGUAGE IN PRINT.

BY FREDERICK STARR, UNIVERSITY OF CHICAGO, CHICAGO, ILL.

MY attention has lately been called to a matter which seems to me of some interest. It is well known to all readers of *Science* that gesture language is a common means of communication between our different Indian tribes.

Mr. Lewis Hadley of Chicago is at present engaged upon a plan for reducing the sign language to print. The purpose of the work is benevolent and religious, the idea being to bring religious instruction to the old Indians. It is well known that old Indians will never learn to read our language. It is believed by Mr. Hadley and his friends that they will quickly learn a printed sign language. Of course, all these old men make constant use of gestures and signs; and, if they take kindly to the printed gestures, there is no question that considerable progress might be made.

Mr. Hadley has had difficulties to contend with in carrying out his work. He has been hampered by the lack of funds and by the novelty of the undertaking. In his first experiment he cut the dies for printing himself, and the resulting impressions were black designs with the figures in white lines, and the result was exceedingly ugly. He has since then simplified the designs and made them in the form of ordinary type, and has now an extensive font of several thousand types, which will be used in printing cards and tracts for the instruction of the Indians.

There are two points to be considered in reference to this plan: First, its feasibility; second, its methodology.

There are three questions that arise in reference to feasibility:—

1. Is there a universal sign language among the Indians?
2. Can the signs be represented by type?
3. Will the Indians care to learn it?

1. As regards the first of these, Mallery says that there is no absolutely fixed sign language in general use among the Indians. While this is true, it is also true that all Indians gesture, and the gestures are so natural and so self-expressive that there is no question that natural signs, although new, would be generally understood.

2. There is, of course, a difficulty in representing the gestures by type so that they can be readily recognized. This difficulty all who have attempted to work in the subject of gesture language realize. Mr. Hadley has changed the forms of his type repeatedly. He has produced finally what appears to be simple, plain, and easily understood characters. Many of these may have to be still further changed, but in large measure they meet the requirements.

3. There is a very serious question as to the favorable reception of this printed gesture language by the Indians themselves. It is, however, in a certain sense picture-writing, and picture-writing is natural to the North American. Mr. Hadley is doing, on a large scale and at one stroke, what the Indians have begun to do in many cases. Colonel Mallery has shown in his papers the close relationship between gesture language and pictography. The picture character is often only an attempt to represent a gesture. This being so, it may be possible that a kind reception will be given by the Indians to the printed sign language.

As to the method of introducing the printed sign language into use, Mr. Hadley has devised a game of cards, which, he believes, will help greatly in the work of teaching. Each card has upon its face, in unusually large type, a gesture. Upon its back is printed the English equivalent for the gesture. The game to be played with these cards is based upon certain gambling games, already quite familiar to the Indian, and success in the game depends upon the Indian giving the English word for the sign represented. All games of an instructive kind are more or less of a nuisance, but it is not impossible that these cards may be successful in the way they are intended. Besides the game of cards, the purpose of which is really to teach the speaking and reading of English through the printed sign language, a considerable number of texts, mainly of a religious character, are to be issued. It is expected that an Indian who has a story or a passage printed in the sign character will himself make the signs represented, and by making the signs he will gain the idea to be conveyed.

Every text of the sign type has under it the English equivalent words. In order to convey an idea what this test is like, I present herewith a line of the text as it appears in print.

It will be seen that, quite apart from its religious and educational purpose, this matter is one of scientific importance, and we shall watch with interest how far it may succeed.

CURRENT NOTES ON ANTHROPOLOGY.—XXVIII.

[*Edited by D. G. Brinton, M.D., LL.D., D.Sc.*]

The Present Position of the Hittite Question.

THE ethnic position of the Hittites has been a perplexing question for many years. It seems to have been answered in a certain degree by the recent excavations of Von Luschan at Sindjirli, which is in ancient Hittite territory. Halévy has shown that the two stelas brought from there to Berlin are in a Phœnician dialect. The Hittites of the Bible were, therefore, probably Semites.

Unfortunately, this solution leaves the real problem untouched; for it is now clearly established by Belck, Lehmann, and others, that the mysterious syllabic inscriptions and bas-reliefs at Pterie, Nymphi, and about Lake Van, were not by the Semitic Hittites, but by a wholly different people, who called themselves Chaldi, after their chief divinity, Chaldis. To their land they gave the name Biaina (Urartu,—Ararat, in Assyrian), and their chief city Van, their own name of which was Tuspa-na, was founded about 833, B.C., by their early king, Menuas.

The confusion partly arose from the fact that the Semitic Hittites, previously tributary to the Assyrian monarchs, were subjugated by the Chaldi king, Argistis I., about 800, B.C.; and, further, that at the fall of the Chaldic kingdom, about the close of the seventh century, B.C., many of the Chaldic people were driven southward into Cilicia and its neighborhood.

The question therefore remains, Who were the Chaldi? The prevailing theory has been that their language had Mongolian or Turkish affinities; but Professor Sayce has pretty clearly shown that it had regular declensions, a nominative ending in *s*, an accusative in *n*, oblique cases in terminal vowels, and an adjective which followed the noun and agreed with it in these respects. This is not at all analogous to any Mongolian or Turanian language, and, if correct, disproves the theory.

A bolder one is advanced, not entirely for the first time, by M. Salomon Reinach in the *Révue Archéologique* for January. He maintains that the migration of the Chaldi, or supposed Hittites, was from west to east; that they were emigrants from European lands; and, in fact, were none other than a part of our old friends, the Pelasgi of Greece; and these in turn were of the same stock as the Tyrseni and Etrusci of Italy. Sufficient can be said for the theory to make it worth further and serious investigation.

An older theory, to wit, that the true Hittite is an Indo-European language most akin to the modern Armenian and probably its ancestor, has been revived with considerable force by Professor Peter Jensen of the University of Marburg. His article is in the *Sunday-School Times*, March 25 and April 1. His criticism on Peiser's theory, that it is allied to the Turkish, is severe and merited. With reference to the puzzling and complex questions suggested by the inscriptions and ethnic types presented by the ancient monuments of Cilicia, he proposes the theory that the rulers of this district were at one time Semites or strongly semitized, while the mass of the population was of Indo-European blood. His opinion of his predecessors' studies is briefly summed up in these words, with reference to those of Sayce, Conder, Peiser, Ball, and Wright: "All are without foundation, and their results are destitute of value"!

A Linguistic Map of Guatemala.

Dr. Karl Sapper of Coban, Guatemala, has published in the first number of *Petermann's Mittheilungen* for the current year a map showing the present distribution of the native languages in Guatemala, accompanying it with a carefully prepared article on the dialects and culture conditions of the descendants of the aborigenes of that country. Much of it is from his own studies, much of it from the excellent works of Dr. Stoll. He does not seem to be aware of the publication by me of the material collected by Dr. Berendt on the Xinca, the Pipil of Acasaguastlan, and other dialects. He falls into the rather serious error, which I pointed out in a paper published by the Congress of Americanists (session of 1890), of locating a language of the Mixe group in Guatemala, though he adds that no signs of it now exist. It never was there. He fails to solve the only real obscurity which remains in the linguistics of Guatemala, that is, the identification of the Popoluca located by the historian Juarros at Conguaco, in the partido of Guazacapan, which was *not* Xinca.

The language of Yupiltepec he considers a dialect of the Zinca, and brings into closer relationship the Chorti and the Chol. His expressions about the dialect of the Cajaboneros are not clear; in one sentence he speaks of their tongue as containing elements fundamentally diverse, "ursprünglich fremd," to the Kekchi; and in another refers to this element as perhaps Chol, which is merely another Maya dialect.

While Dr. Sapper' work is open to these slight criticisms, it is in the main worthy of the highest praise.

The Earliest Extension of the Iron Age.

In these notes (*Science*, March 10) I referred to some recent studies on the early Iron Age in central Europe. The question still remains, When and how did the art of working iron reach those localities? Two valuable papers of late publication have interesting suggestions touching this point. One is on "Le Premier Age du Fer au Caucase," by M. Ernest Chantre, who for twenty years has travelled, studied and excavated in the Caucasus; the other by M. Louis Siret, scarcely less distinguished for his archæological campaigns in Spain. Some remarkable coincidences are pointed out by both.

M. Chantre finds that the most ancient sepulchres in Lower Chaldea which contain iron are shown by their funerary contents to be contemporaneous with the third and fourth dynasties of Egypt, at which period occur the first signs of this industry on the Nile. At the lowest, this would place them 2500 years, B.C. The knowledge of the metal reached the southern and central vales of the Caucasus about 1500 B.C., through the extension of a "Semito-Kushite" people, who were the ancestors of the modern Ossetes. They were distinctly non-Aryans, and the art of working iron was not introduced by them into Europe. Later on, about the seventh century, B.C., their culture was deeply modified by irruptions of Mongolic hordes from the East. (All this in spite of the fact that the modern Ossetes speak an Aryan tongue!)

The proof of this early Semitic influence is found in the identity of art-motives, decorations and methods, and especially in the numerous traces of the worship of the goddess Ishtar, the Astarte of the Phœnicians. In the Caucasus, as elsewhere, her favorite symbol, the dove, is constantly met with in ancient tombs; as is also that of the hand, employed in her rites as the symbol of adoration and peace.

It is true, as M. Chantre remarks, that in every station of the earliest iron age in Europe, from Greece to Scandinavia, we find figurines of birds, evidently sacred, and all to be traced to the dove of Astarte. They are proofs of what impressed M. Siret so much in his study of the earliest civilization of the Iberian Peninsula,—"the worship of a female deity represented under various symbols." He also, in his article in *L'Anthropologie*, 1892, No. 4, is forced by the results of his own excavations to assign this civilization to the daring early navigators of Semitic blood, to the Phœnicians, sailing from the far east of the Mediterranean, rounding the rocky shores of Spain in search of tin from the Cassiterrides, or amber from the far-off shores of the Baltic. The first signs of iron there follow without a break on a highly developed bronze period; and its earliest discovered use was as rivets to fasten together plates of bronze. This indicates peaceable introduction and artistic growth, not the result of violence and conquest. The merchant, not the warrior, was the civilizer.

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.

Sham Biology.

THE article "On the Emergence of a Sham Biology in America" undoubtedly brought much joy to many botanists. Some of us know from experience that many American botanists are never so much tickled as when some one has gotten them to believe a zoölogist is hopping about a botanist and is worrying over the adhesive soil the botanist is trying to shake from his own trousers and boots onto those of the zoölogist.

Feeling himself above any and all of the charges made in the interesting tirade, the present writer has concluded he ought to at least make an attempt to show how strong a position his colleague had taken. No chuckling botanist can have any rational ground for gleefully pointing to me as a zoölogist badly hurt. Notwithstanding this, I am fully aware of the fact that nothing others may do or say is too minute to impel some people to strike their breasts, pour out eloquent prayers of thanks, and then go their way rejoicing over the capital they imagine can be made out of the sins of others. I also must say I am not at all sure of what my fellow-zoölogists will think of me for daring to answer for others. The unqualified and sweeping statements in several places technically include me, and this fact I offer as an excuse for attempting to indicate to botanists that the "sham biology" article is not so impregnable a piece of scientific work as I know many think it is.

It may be well to forestall possible taunting thrusts by stating that I have never desired to give a course in "general biology;" that I never attempted to plan, nor even thought of planning, a course in general biology to be given under my supervision, though the opportunity to do so was before me when I came to the University of Minnesota as an instructor. I have always insisted, and now insist, upon the independence, the autonomy of the two sub-departments of biology. My whole record stands as a proof of this, and therefore it cannot be said with justice that I belong to a class often called "sore-heads."

Let us at the outset agree to take the figures of speech for what they evidently were intended to illustrate, and not try to divert the real issues by seizing an opportunity to nag our immaculate brethren in botany.

We can all agree to the definition of biology. If it is to be improved at all, it must be made more general by saying it is the science of matter in the living condition. The writer tells us the word is "still to be defined as by Lamarck and Treviranus—both distinguished botanists." Will the botanists observe the innocent way in which their extremely generous champion here puts forth a foreclosed first mortgage claim on these distinguished naturalists'—*biologists*. And this, too, so imperturbably in a discussion whose whole tenor is to brand good zoölogists as out-and-out usurpers. Now couple the definition of biology and this innocent act with "I have not at present time to discuss the fundamental absurdity of courses in 'general biology'—as if it were possible to plunge boldly into comparative study of plants and animals before one has studied plants and animals themselves." Who is now sure of what the writer means by "biology" and "general biology?" Is general biology a bold plunge into the comparative study of plants and animals before one has studied plants and animals themselves? Perhaps it is. Perhaps it is not. I have yet to learn of the fool board of trustees that is paying some fool instructor attempting to compare things without considering the things being compared. Is a course in "general botany" a course in the comparative anatomy of plants? Is a course in "general biology" to be proper only when it brings the student to the level of the genius who has been studying plants and animals for thirty or more years? If it is, then away with your "fundamental absurdity" of long courses and short courses and three-month courses in botany—as if a man could plunge into comparative study of plants without studying plants themselves! The figure of following "analytical statics" "up by geometry and the calculus" is not at all to the point, and it indicates an altogether different conception of the term biology and the phrase "general biology" than one would be led to expect in view of the explanation previously given. It might be well for our iconoclast to consider the definition of biology above suggested, and to ask, What is a plant and what is an animal? I do not mean to imply that in biology we are not to call living things plants or animals and that the student is not to study the things under these names. At present it is impracticable to do otherwise. But I do mean to imply that it is possible to teach the general laws and principles of biology in connection with the study of both plants and animals themselves. Moreover, I maintain that it is beyond rational objection that the student who studies well a *Pteris* and a *Lumbricus* (I use the words to indicate centres) has a broader and better foundation of facts for the great generalizations of biology than the student who studies only a *Pteris* and a *Ranunculus*. We will admit without discussion that the end to be attained by a general course is both informational and disciplinary. Now I cannot admit that the methods of study in botany are inherently different from those of zoölogy. Botany and zoölogy are coördinates of biology, and the methods in either must be biological methods. So far as the discipline is concerned, therefore, a term devoted to the study of plants and one to the study of animals will give as good results as two terms devoted to the study of plants, and certainly more information of value in every-day life will be gained in the former course. I cannot see why this could not be true even if sixteen full weeks, for example, were given up to the study of animals and only eight to plants. (The botanists have my permission to exchange the number of weeks.)

After all, it is not evident to me that the "fundamental absurdity" has caused this spasmodic cough of the botanist. The thorn seems rather to be—some zoölogists are conducting courses in "general biology," and, naturally enough, ultimately guide the more interested students into zoölogical lines of work. The botanists have confessed that they can't conduct such courses because they become so one-sided as to be unable to see that there is anything good in zoölogy they can't duplicate in botany—they become, if I may venture a figure, soles with their eyes on the under side, and then they croak about "fundamental absurdities." Is jealousy loose among our botanists? It is safe to say if the botanists had this monopoly of courses in general biology they would not talk about "fundamental absurdities."

It seems to me that the paragraph containing the words "fundamental absurdity" is full of extravagant utterance. Is it not absurd to say a zoölogist can't write anything worth reading upon the anatomy of Pteris? Is it not absurd to say only specialists can write anything of value? Either such statements are wild and not sufficiently guarded or the botanists are all fools. And even fools sometimes write things worth quoting. Certainly the zoölogists do not at present believe the work of all botanists is unreliable and should never be referred to.

I now come to the specific charges. Without giving any reasons, I must beg to differ entirely with the writer as to the question of the phylogeny and ontogeny of the "sham biology."

The matter of the inadvertent use of the term biology is, of course, to be regretted. But it ought to mollify the iconoclast somewhat to see that the nomenclature in our university organization is nowhere logical and consistent throughout.

I am not sure that I know just what is meant by saying Johns Hopkins University would have a better influence with "an honest naming of the zoölogical courses that *were* provided for." I was not aware they had ever been named otherwise. Certainly the zoölogical courses were named in accordance with their subject matter when I was at Johns Hopkins University. And I can assure the writer it will take more than brilliant rhetoric and insinuations to make me believe conditions have so changed as to have necessitated a dishonest naming of the zoölogical courses. I cannot speak positively about the present course in general biology at this noble university, and will therefore speak only of what existed several years ago. The course in general biology, as I got it, was by no means as one-sided as any botanist would have made it. A general biology course that will develop enough love for botany in a student to make the purchase of such expensive books as Sachs and various monographs a pleasure, and that will develop a respect for botany and admiration of botanists and their magnificent work, such as I know has been developed at Johns Hopkins, should hardly be dubbed "sham biology," simply because zoölogists were in power. If the facts gotten were wrong, it was the botanist's own fault; for the references were to the recognized authorities. If the course leaned any way in my case, it was toward botany. I have heard it said the course took up two or three plants and a dozen or more animals—that a few weeks were given to plants and months to animals. That certainly is not a true picture of the course I was privileged to get. And why must a course, in order to be above the shadow of a "sham biology," consider just as many plants as animals? Must just as much time be devoted to plants as to animals? It seems to me no weaker principle could be adopted. If the botanist directs such a course of study, he will naturally illustrate more with plants. The zoölogist will naturally use animals more frequently. But this does not necessarily produce a sham biology. I admit a decided zoölogist or a decided botanist will always be in danger of curtailing the sister-science too much, and a course laid out by the one or the other may, naturally enough, not be altogether satisfactory to the colleague "not in it." Such facts do not touch the possibility of botanist and zoölogist conjointly formulating a course in general biology. If the principles, laws, and generalizations to be impressed upon the student be taken as the guide, and the two kingdoms of living things be viewed as the store-houses of facts, a true general biology becomes a possibility. Why should a school of biology, organized with a professor of zoölogy at its head be any more a school of sham biology than a university with an ichthyologist as president be a sham university?

The "always insular capabilities of the Johns Hopkins biologist for blatant philistinism in regard to things botanical" would be an unpardonable fling in view of what men the writer's previous statements would make it cover were it not for the fact that it appears to have been written with the ghost of that pamphlet (which I had supposed dead, because of its absolute flatness) dancing before him in "cool effrontery." But even the pachydermatous zoölogists can appreciate moderation; and it is no weakness to keep one's just appreciation of an evil that does exist in some places under the influence of reason.

There are many things in the way of criticism and explanation yet to be said, but I will close by pointing out what an influence botany seems to be having on my estimable colleague. Some zoölogists divide the organs of animals into the vegetative organs, their functions being those common to plants and animals, and the organs of animal function, their functions being characteristic of animals. My genial associate must have learned this fact from some one and makes a desperate effort to use it in classifying the sub-sciences of biology by trying to limit zoölogy to the vegetative organs of animals, and relegating the animal functions to psychology, which is held "coördinate with zoölogy rather than as one of its sub-divisions." I may be wrong, but that effort looks like a bid for a vote.

To guard against any misapprehension on the part of those not acquainted with the actual attitude of the departments of botany and animal biology toward each other at the University of Minnesota, I must say that Professor MacMillan and myself are not at loggerheads here, but that we do and always have pulled together for the equal advancement of both botany and animal biology. The adjustment of our courses is not the result of a compromise, but the individual and united recognition of facts and conditions. We are not competitors, and there is no likelihood that we shall become such. HENRY F. NACHTRIEB.

Professor of Animal Biology, University of Minnesota.

April 18.

On Methods of Defending the Existence of a Sham Biology in America.

Two recent papers in *Science* deserve a little attention at this time, for they serve as examples of the kaleidoscopic movements by which "biologists" hope to defend themselves against the clearly stated charges of incorrect use of terminology which have been brought against them. It will not be permitted to these wanderers from the path of orthographic rectitude to conceal their retreat under cover of a sea of ink. The discretion, good taste, enthusiasm of the writer, are not the subjects of the discussion and will not be discussed by him. No shuffling to alien positions can be admitted as an answer to the definite impeachment which has been brought against courses in zoölogy masquerading under the erroneous name of biology.

Although the briefer, the article by Mr. H. F. Osborn [1] of Columbia College should, from the acknowledged ability of its writer and its air of gentlemanly candor, be given first consideration. Mr. Osborn is under such manifest misapprehension, however, that it will be necessary first of all to correct him and indicate to him just the point at issue. He says "the arrangement of courses in Columbia is cited by Mr. MacMillan as a leading example of the manner in which botany is subordinated to zoölogy." Since absolutely nothing was said in my article about the subordination of botany to zoölogy at Columbia or anywhere else, I am naturally interested to learn by what higher criticism, textual or literary, Mr. Osborn has arrived at such an unexpected result. In my former paper it is written, "At Columbia College it is apparent that the subject of botany, since it stands by itself under its own organization, is supposed at least by the 'biologists' of that institution to be quite without the pale of their own science." It is my evident and distinct purpose here to charge, not subordination, but misuse of terminology. Indeed, if there were any "subordination" at Columbia, I should think it would be of the zoölogical courses staggering as they are under the weight of a false nomenclature.

In his note, Mr. Osborn cites a number of botanical and zoölogical courses at Columbia and then uses the word "biological" correctly in the sentence, "It does not appear that botany is ignored in this programme of *biological* courses in this institution." Immediately afterward he uses the word incorrectly when he says, "the fact that the botanical courses are not arranged under the *Biological* Department is a mere technicality of administration." A "Biological Department" without botanical courses is, however, something more than a "technicality"; it is a sham. Mr. Osborn is, of course, at liberty to have his department separated as he will; it is no affair of ours, — but why should he permit such a line as this from the circular of information,[2] "Biology (Zoölogy) . . . Professor Osborn"? Why does he appear as defining the word biology as zoölogy? I am sure it must be for some better reason than the anxiety to use a high-sounding word, even though that word be used incorrectly.

Having thus indicated to Mr. Osborn the errors into which a probably hasty perusal of my former article has led him, I may now note his principal defensive movement. He says, "Biology, however, is not the science of animals and of plants, as Mr. MacMillan maintains, it is rather the science of life." Therefore, "those who set forth the fundamental principles of life are biologists,"—a fair paraphrase, I trust, of Mr. Osborn's argument. This is so unexpected a point of view to be taken by one of the leading animal morphologists of America that it is indeed difficult to collect one's self for a reply. The venerable style of talk about "life," I supposed, was extinct in scientific circles, unless one includes the metaphysicians. "Life," I had supposed, was an abstraction from certain observed phenomena of a group of things known as plants and animals. I presume Mr. Osborn does not use the word as does the Boston University in its Year Book,[3] where Group IX. in Courses of Instruction is "Chemistry, *Biology*, and Geology," and Group X. is "*Life*, Personal Development, and Expression." I did not suppose that the statement that "biology was the science of living things" could possibly find objection in such a quarter as Columbia College. Here at Minnesota we are busily studying living things, but if Mr. Osborn is studying "life," he is evidently on another plane altogether. Long ago, one used to hear of "vital force" and "life," but I supposed we now believed that the best way to learn about life was to study living things. If it is true that the zoölogists are going in for the study of "life" under the belief that biology is not the science of living things, I wish them God-speed on a perilous, if ancient, voyage. And if this really is the modern view of "biology," I yield me a captive to Mr. Osborn's convincing argument and beg to withdraw among those botanists who believe that botany is the science of the living things, plants, and will certainly, if I know them, be glad to leave the study of "life" open to the zoölogist-"biologist," who rules out living things as irrelevant to his science.

Let me, in closing, call the attention of Mr. Osborn to the fact that I am unaware of any one-sided state of true biological education in America. There is nothing one-sided about it in Harvard University. It is the sham biology that is one-sided, and for this the zoölogists are responsible in large measure, therefore the epistle is addressed to them. I recall now but one institution which names its botanical courses, a "department of biology." And this department is manned by a Johns Hopkins doctor of philosophy, from whom one might unfortunately expect the one-sided view.

The paper by Mr. Francis H. Herrick,[4] entitled "On the Teaching of Biology," requires some elucidation and correction that I may venture to give. Notwithstanding its characterization of my former article as "thoroughly bad," I take pleasure in acknowledging its own uncommon excellence. Any defense of the sham biology is sufficiently difficult, and while the air of righteous enthusiasm was accurately enough predicted it was scarcely realized with what vigor the plaintiff's attorney would be afforded the treatment sanctioned in such cases by all the traditions of the bar.

Aside from its entertaining personal character, the contribution by Mr. Herrick appears to seek the establishment of the following points: (1) The study of biology is not two disciplines, but one discipline; (2) biological science is not to be set over against physical science, but is to be included in it; (3) zoölogy, when presented under the name of biology, is not a sham biology, but a "restricted biology"; (4) the better fundamental division of biology is into general morphology and general physiology, not into botany and zoölogy. Stated thus, with such condensation as is necessary for clearness, it is hoped that the exact meaning of Mr. Herrick is preserved. These four points, only the third of which seems to have direct bearing on the question at issue, may now receive their proper attention.

[1] Science, Vol. XXI., p, 234. New York.

[2] Columbia College Circular of Information, 1893, Pt. VII., p. 4. New York.

[3] Boston University Year Book, Vol. XX., p. 66, 1893. Boston.

[4] Science, Vol. XXI., p. 220. New York.

(1) There is certainly a unity in the science of biology. This unity is not, however, zoölogy. Breadth of view demands rather a recognition of the true unity, and for such recognition the writer is contending. "Good observation" will convince Mr. Herrick that one who writes "Biology is either a superficial smattering of natural history facts and methods — and in this case not of any value — or a strong, uniform presentation of the facts of botany and zoölogy — and in this case a very different thing from a sham biology which is principally, or all, zoölogy" —doubtless appreciates the breadth of biological science almost, if not quite, as clearly as he would if contending that zoölogy alone may pass current for biology. For such higher unity of biology it is a duty to contend against any or all disintegrating views that may arise from the misfortune of a narrow education.

(2) Mr. Herrick laments the inadequacy of my early training along biological lines and, indeed, charges me in so many words with having been myself a student at Johns Hopkins University. As principal evidence of an indwelling incapacity he adduces my setting biological science over against physical science. He writes, regretfully reminiscent: "a student who had followed this general biological course with a fair degree of success, would have learned that 'biological science is not to be set over against physical science in the broadest sense,' but that in this broadest sense biology is a physical science coördinate with chemistry and physics." In this connection the following quotation may be noted. It is from Dr. C. O. Whitman, an acknowledged leader, I believe, in American zoölogy: "The term biology is so frequently used with latitudinarian disregard of its etymological significance that it becomes necessary to recall its original meaning. . . . As still used by the best authorities, the term is a very comprehensive one, denoting not one science or the fragment of a science, but a multitude of sciences embracing the entire organic world in contradistinction to the inorganic or physical world. From this broad standpoint all the natural sciences fall into two great groups, known as the biological and the physical."[1] Doubtless, no italicizing will be required to impress Mr. Herrick with the ripeness of the harvest that awaits his discriminating mission-effort among his biological colleagues. With his fine solicitude for those whose "early comprehensive training" has not sufficed to distinguish clearly between physical and biological science, he will scarcely permit himself to overlook so distressing a failure in Dr. Whitman to conform with the standard of absolute correctness. The writer, however, must continue to believe that a grouping of natural sciences into physical and biological sciences is not altogether unproductive of right thinking and ventures to commend, as a useful discipline, to Mr. Herrick, the reading of Dr. Whitman's programme not only on account of the value of its definition of biology and the general breadth of its views, but also because a uniform line of defence will be highly advantageous for all who find themselves, whether by necessity or by choice, enlisted under the flag of the sham biology.

(3) While the term "restricted biology" is an ingenious suggestion for such courses in zoölogy as are offered at Columbia College and Johns Hopkins University under the inappropriate name of biology, it is not clear that the old-established word "zoölogy" is not better. It is scarcely so vague and has the merit of brevity. If either of these institutions should gracefully announce a "department of Restricted Biology" and should confer degrees upon "doctors of philosophy in restricted biology" it would certainly indicate the dawn of ethical development if not the noon-tide of philological precision. And if such a consummation lies near the heart of Mr. Herrick he shall not wander farther without my sympathy. But, unfortunately, one must here note the crucial and deplorable fact; these institutions do not employ the term "restricted biology," but use instead the broader term, biology, for their zoölogical courses. Since a part of anything posing valiantly as the whole is universally recognized as a sham, it is hardly possible in such a case for the sham biology to escape its just characterization.

(4) It is unreasonable, of course, to ask that an American "biologist" should be familiar with the literature of plant-morphology from Hofmeister to Guignard, Strasburger and Treub. But those humbler botanists that have followed the progress of recent investigation in this field would realize how distant seems to be the day when general homologies between higher plants and higher animals may be demonstrated with certainty. The established fact that between sporophytic plant-embryos and gamozoan animal-embryos there exist few known homologies-in-general must give pause to ambitious talk about a "general morphology." Such a general morphology would certainly demand a basis of general phylogenetic and ontogenetic comparison. It is true that in cytology, and especially in nuclear dynamics there may be read, for the future, possibilities of a general morphology. Chromatomeres may indeed be always homologous as well as analogous, broadly speaking. But to-day "general morphology," as a science, does not exist. I should be glad to learn the title of some compendium of general morphology. I should be pleased to hear the name of some living or deceased investigator who could, in the broad sense of Mr. Herrick's division, be termed a "general morphologist." The fundamental division of biology into two sub-sciences, one of which, at least, does not exist as such, seems scarcely so productive of good as the time-honored division into botany and zoölogy. Between plant-physiology and plant-morphology there are innumerable series of contact-points. Between plant-morphology and animal-morphology there are few. Until, therefore, we may claim a far wider knowledge of the facts of morphology and physiology—at least in the field of botany — it will be difficult for Mr. Herrick to impose his divisions of biology to suit the terms of his argument.

In the second place, the science of biology is clearly not principally a method or discipline as Mr. Herrick seems to think it is; it is, also, and primarily, an orderly group of facts about an orderly group of things. These things are living things. The primary division must therefore be along the line of mass, not along the line of method. Living things conveniently divide with great exactness — although not absolutely, as Mr. Herrick acutely indicates — into plants and animals. Biology, therefore, divides conveniently into botany and zoölogy. A particular method is the essence of morphology, but plants-in-the-aggregate are the essence of botany. Biology is, primarily, a group of facts about a group of things, not a group of facts about a group of methods of studying things. Plants (for example) are things, not methods, and therefore the fundamental division of biology into botany and zoölogy is more logical than its division into morphology and physiology. It thus appears not only that the divisions of biology urged by Mr. Herrick have never existed and do not exist now, but also that logically they should not exist as primary divisions but only as secondary. Finally, even if they did and should exist, the classification would not help the sham biology. For the union of a sham "general morphology" and a sham "general physiology" would probably result in a sham biology, and a "general morphology" which upon criticism reveals itself as the special morphology of animals is evidently a sham morphology.

It is a source of regret to the writer that anyone should suppose that he would "stigmatize" any university or any honorable graduate of a university. His function is purely indicative, and, while he agrees with Mr. Herrick that the truth about the state of affairs in certain curricula and the state of culture in certain graduates is so melancholy that perhaps even so strong a word as "offensive" may rightly be applied, he must disclaim any connection with such a condition beyond that of an interested spectator, grieved that able young men should be dwarfed in their conceptions of the great field of biology through acceptance of a sham in place of the truth. He has the kindest of feelings for such young men and a warm sympathy for institutions straining every nerve in an unequal struggle with others of greater wealth and breadth. But he cannot permit his sympathy and kindly feeling to withhold him from the task of pointing out to those who may profit, perhaps, the impossibility as well as the undesirability of further acceptance of shams for realities. If words mean anything, zoölogy and biology are not synonymous, and it is hoped that no false pride will prevent the zoölogists from

[1] Programme of Courses in Biology, 1892–93, p. 6. Chicago.

joining the botanists in the development of an accurate nomenclature. For while some sneer at nomenclature as a trivial matter and of no importance, it must be remembered that nomenclature is the expression of ideas, and ideas are of much importance. CONWAY MACMILLAN.

University of Minnesota.

Photographs of Scientific Men.

A NOTE in your recent issue having to do with a request for the photographs of American botanists suggests that an appeal made through the columns of *Science* is likely to aid a collection made by myself. Some six or seven years ago, finding great difficulty in procuring the portraits of American scientists, I began gathering the photographs of the members of the National Academy of Science, and last year deposited in the Smithsonian Institution a collection of mounted portraits (with mounted autograph letters) of every member of our academy save two. This collection forms part, I believe, of the exhibit of the Smithsonian Institution at the Chicago Columbian Exhibition. The two portraits which are needed to make the set entirely complete are those of John Henry Alexander (1812–1867) of St. James College, Maryland, and later of the U. S. Coast Survey, and Jonathan Homer Lane (1819–1880), long connected with the U. S. Coast Survey and the U. S. Patent Office. I should be glad to obtain photographs of the two scientists or to make arrangements for the copying of any likeness of them known to exist.

MARCUS BENJAMIN.

640 Madison Avenue, New York City, May 18, 1893.

The Palæolithic Man in Ohio.

IN the second number of *The Journal of Geology*, Mr. Wm. H. Holmes has resumed his polemic against the evidence of the existence of palæolithic man in North America with a long article upon "Traces of Glacial Man in Ohio." Like his previous article upon the Trenton finds, this, too, is characterized by the kind of reasoning, which a correspondent of *Science* has called the argument *ad ignorantiam*, i. e., because he has failed to find palæolithic implements in a certain locality, therefore no one else has ever found them there. The present article, however, exhibits also a striking example of what might be called "the argument by monopoly." Mr. Holmes produces two fanciful cuts to show how the top of a gravel pit might have slid down so as to bury Indian relics coming from the surface; but he cannot see any sense in Professor Wright's preparing a plate to show precisely where in the same gravel-pit Mr. Mills actually found the object in dispute.

But the great difficulty about Mr. Holmes's discussion of this subject is that he has no correct appreciation of what a palæolithic implement really looks like. This is not to be wondered at when we reflect that his studies in "archæology" have been limited to investigations of the subject of "native art." He says "close analogies of form between Indian rejects and some varieties of European palæolithic objects are too common to permit the attachment of much value to this feature of this or any other similar find." Accordingly he proceeds to prepare a plate containing, besides the object discovered by Mr. Mills, of which he gives as good a copy as he can have made, four unfinished Indian celts found by him fifty miles away. Of these objects he says, "they correspond very closely in material and appearance with the New Comerstown specimen, as will be apparent from an examination of the plate. The figures are presented without identification in order that the student may, by an effort to distinguish them, convince himself of the similarity of the supposed paleolith to the quarry-shop rejects of the region."

Now I undertake to assert that any competent student of prehistoric archæology who has studied the subject in the Old World, where palæolithic implements have been found in large numbers, will have no difficulty in discriminating upon Mr. Holmes's plate between the true palæolithic implement and the four unfinished Indian celts placed beside it. All plates, however, fail to give a fair representation of solid objects like these, from the necessity of the case. They must be handled to be understood. The four unfinished celts resemble those previously figured by Mr. Holmes in describing the objects he discovered near Washington, where I have myself found similar objects several years ago. I repeat here, what I have said in another place, "no trained archæologist would hesitate for a moment to pronounce that the objects figured in the article entitled 'A Quarry Workshop' (*American Anthropologist*, Vol. III., plate 4) do not bear the slightest resemblance to real palæolithic implements."

I conclude this note with what I have already urged to the readers of *Science*, that "only a jury of the acknowledged prehistoric archæologists of the world is competent to pronounce judgment upon this question." HENRY W. HAYNES.

Boston, May 13, 1893.

BOOK-REVIEWS.

Mineral Resources of the United States. 1891. By DAVID T. DAY. Washington, D. C., Department of the Interior, Government Printing Office. 1893. 630 p.

IT is somewhat unfortunate that these volumes cannot be more promptly produced, the late date of their issue impairing materially the value of the statistics contained. But in spite of this they are always welcome, and together—the present volume being the eighth in the series—they form a valuable component of every library. The arrangement is the same as in previous issues, and we find the familiar names of Birkinbine, Kirchoff, Weeks, Parker, and others under their respective specialties. Mr. Parker's statistical article on coal is exhaustive, occupying nearly 200 pages in all, and is supplemented by the articles on coke, petroleum, and natural gas by Mr. J. D. Weeks. Mr. Wm. C. Day continues his paper on stone from the "Resources" for 1889–90. An admirable and much-needed division appears upon the clay materials of the United States, written by Mr. Robert T Hill, and as this is in some respects the feature of the present volume an outline may not be out of place. Beginning with descriptive remarks, Mr. Hill passes on to the commercial classification, the origin and natural classification, residual or rock kaolins, and sedimentary or bedded clays. The sedimentary clays of the geological formations are given in natural sequence. The accessory minerals used in the clay industries are described and then the occurrence of clay materials by States.

Other interesting articles are those on natural and artificial cements, by Spencer B. Newberry, both descriptive and statistical, on precious stones by the expert, Mr. Geo. F. Kunz, and Mr. Packard's descriptive article on aluminum, the last including several pages on bauxite, with analyses and a sketch of the development in the South. Alabama, Georgia, and Arkansas are mentioned as containing the mineral, but Tennessee with its good promise, Virginia, and North and South Carolina are not spoken of.

An unfortunate slip of the binder has placed pages 49–64 inclusive between pages 32 and 33, but in other respects the book is all that can be wished for. C. P.

William Gilbert of Colchester, On the Loadstone and Magnetic Bodies. A translation by P. Fleury Mottelay. New York, John Wiley & Sons.

A RATHER acrimonious discussion between Professor S. P. Thompson and Messrs. Wiley & Sons has attracted even more attention to this book than it would otherwise have received. It will be remembered that the Gilbert Club was formed in England a few years ago, and that one of the objects of their existence was the publication by subscription of Gilbert's works. Professor Thompson was one of the committee on publication, and the matter seems to have been left mostly to him. From various causes, one of which was possibly the fact that the latter is translating and editing a number of books on his own account, the publication of the Gilbert Club has been delayed. Previous to the determination of the club to undertake the publication of Gilbert's work, Mr. Mottelay had been seized with the same idea, and, as neither he nor his publishers were in any way infringing on the rights of the Gilbert Club, the work has recently been issued. This aroused the ire of Professor Thompson, who, not being able to find any fault with Mr. Mottelay, wrote a number of rather bitter letters to the various technical papers, in which he spoke very disparagingly of Messrs. Wiley & Sons, and their conduct in publishing a book he had intended to publish himself. He was so evidently in the wrong, however, that most of the papers refused to allow him space on the subject, and united in defence of the publishers of the book, and Professor Thompson himself has probably by this time seen his mistake.

Of the book itself there is nothing but praise to be said. Mr. Mottelay is a worker of no mean reputation in this line of work, and his notes are always interesting and instructive. The translation seems to have been well done, so far as can be judged by comparing a few passages of the original which have appeared with the book. Mr. Mottelay's acquaintance with the vocabulary of the Schoolmen is of great use to him in the work, not that Gilbert was a schoolman, very far from it, but the language of philosophy had only begun to get rid of their marks (if indeed it is entirely free to this day).

On reading the book, we are struck with the sturdy self-confidence of the man, Gilbert of Colchester. He was right, and he knew it. A little bit of this is due possibly to the age he wrote in, but even more, it seems, to the man. Fearless he is in drawing conclusions, and he does not hesitate to dispute the evidence of others when it does not agree with his theory. Yet in one instance only does he appear to have been mistaken, i.e., in his proposed method of finding longitude by the inclination of the compass, which he proposed under the idea that the inclination was constant.

A few extracts from the work will give a good idea of the man and his work.

Before doing so, we may mention the fact that Lord Bacon thought that Gilbert had carried his theory a little too far, and had said that Gilbert had "endeavored to build a ship out of materials not sufficient to form the rowing-pins of a boat."

Page 2. "But lest the story of the loadstone should be jejune, and too brief, to this one sole property then known were appended certain figments and falsehoods, which in early times no less than nowadays were by precocious sciolists and copyists dealt out to mankind to be swallowed. For example, they asserted that a loadstone rubbed with garlick does not attract iron nor when it is in presence of a diamond. The like of this is found in Pliny and in Ptolemy's "Quadripartitum," and errors have steadily been spread abroad and been accepted — even as evil and noxious plants ever have the most noxious growth — down to our day, being propagated in the writings of many authors, who, to the end that their volumes might grow to the desired bulk, do write and copy all sorts about ever so many things about which they know naught in the light of experience. Such fables about the loadstone even Georgius Agricola, a man that has deserved well indeed of letters, has inserted as truthful history in his books, 'De Natura Fossilium,' putting his trust in others' writings."

Page 102. "In Baptista Porta's opinion there seems to be a mixture of stone and iron, i.e., ferruginous stone, or stony iron. 'The stone,' he says, 'is not changed into iron so as to lose its own nature, nor is the iron merged in the stone, but that it retains its own essence; and while each strives to overcome each, from the struggle results the attraction of the iron. In the mass of the loadstone there is more stone than iron; therefore the iron, lest it should be dependent on (subdued by) the stone, craves the strength and company of iron, to the end that what it cannot procure of itself it may obtain by the help of the other. The loadstone does not attract stones, because it has no need of them; and if one loadstone attracts another, it is not for the sake of the stone, but of the iron shut up in the stone.' As though the iron in a loadstone were a distinct body, and not blended with one another like all other metals in their ores. And it is a height of absurdity to speak of these substances thus confounded together as warring with each other, and quarreling, and calling out from the battle for forces to come to their aid. Now iron itself when

CALENDAR OF SOCIETIES.

Agassiz Scientific Society, Corvallis, Ore.

May 10. — Professor Dumont Lotz, Food Adulterants.

Biological Society, Washington.

May 20. — V. A. Moore, the Distribution of Pathogenic Bacteria in the Upper Air Passages of Domesticated Animals; C. V. Riley, Some Further Notes on Yucca Pollination; B. W. Evermann, The Ichthyologic Features of the Black Hills; W. H. Dall, New Forms of Fossils from the Old Miocene of the Gulf States; C. Hart Merriam, Biology in our Colleges; C. Hart Merriam, Facts of General Biological Interest Resulting from a Study of the Kangaroo Rats.

Geological Society, Washington.

May 24. — Whitman Cross, On the Occurrence and Characteristics of Laccolitic Rocks; Walter H. Weed, The Northern Peaks of the Crazy Mountains, Montana.

touched with loadstone seizes iron with not less force than loadstone itself. These fights, seditions, conspiracies, in a stone, as though it were nursing quarrels as an occasion for calling in auxiliary forces, are the maunderings of a babbling hag, rather than the devices of an accomplished prestigiator."

So much for his attacks on the older philosophy. As an example of his own reasoning, we may give the following: "What is it that produces this movement? (speaking of the attraction of electrified bodies). The body itself circumscribed by its contour? Or is it something imperceptible for us, flowing out of the substance into the ambient air? And, if it is an effluvium, does the effluvium set the air in current, and is the current then followed by the bodies? Or is it the bodies themselves that are directly drawn up? But, if the amber attracts the body itself, then suppose the body itself is clean and free from adhesions, what need is there of friction? Nor does the force come from the lustre proceeding from the rubbed and polished electric, for the vincentina, the diamond, and pure glass attract when they are rough, but not so strongly nor so readily; because they are not so readily cleaned from the extraneous moisture settled on the surface, nor are they subjected all over to such an equal degree of friction as to be resolved into effluvia. Nor does the sun, with its shining and its rays, which are of vast importance in nature, attract bodies thus, and yet the common run of philosophers think that liquids are attracted by the sun, whereas only the denser humors are resolved into rarer, (and) into vapor and air; and thus, through the motion given them by diffusion, they ascend to the upper regions, or, being attenuated exhalations, they are lifted by the heavier air. Neither does it seem that the electric attraction is, by the effluvia, rarefying the air so that the bodies, impelled by the denser air, move towards the source of rarefaction. If that were so, then hot bodies and flaming bodies would attract other bodies, but no lightest straw, no rotating pointer is drawn toward a flame. If there is afflux and appulsion of air, how can a minute diamond, the size of a chick-pea, pull to itself so much air as to sweep in a corpuscle of relatively considerable length, the air being pulled toward the diamond only from round a small part of one or other end? Beside, the attracting body must move more slowly or stand still before coming into contact, especially if the attracting body be a broad flat piece of amber, on account of the heaping-up of air on the surface, and its rebounding after collision. And if the effluvia go out rare and return dense, as with vapors, then the body would begin to move towards the electric a little after the beginning of the application, yet when rubbed electrics are suddenly applied to a versorium, instantly the pointer turns, and the nearer it is to the electric, the quicker is the attraction. . . . In addition to the attraction of bodies, electrics hold them for a considerable time, hence it is probable that amber exhales something peculiar which attracts the bodies themselves, and not the air. It plainly attracts the body itself in the case of a spherical drop of water standing on a dry surface; for a piece of amber held at a suitable distance pulls towards itself the nearest particles and draws them into a cone, were they drawn by the air the whole drop would come toward the amber."

Page 273. "The variation in the Indian Ocean all the way to Goa and the Moluccas is noted by the Portuguese, but they are mistaken in many points, for they follow the first observers who set down the variations for sundry places, ascertained by the use of unfit instruments, or by inaccurate observations, or by conjecture. Thus in the island of Brandö they make the compass vary 22 degrees to the northwest. Now, in no region, in no place on the earth that has not a higher latitude than that, is the variation so much as 22 degrees; in fact the variation on the island is trifling, so when they say that in Mozambique the compass varies to the northwest one point, they are in error, even though the compass they use be that of Portugal, for without a doubt the needle varies in Mozambique to the southwest one-quarter of a point or more."

The book is handsomely bound, and will form a valuable addition to the electrical library. R. A. F.

Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

Minerals for exchange—John Holl, Rollo, Wilmington, Delaware.

For sale or exchange.—Johnson's Universal Cyclopædia, 8 vols., ed. 1888. Binding, half-morocco. Will sell cheap for cash or would exchange for typewriter. Address W. J. McKom, Mason, Mich.

I have 500 microscopic slides to exchange in lots to suit. Want Kodak, first-class field-glass or scientific books. A. C. Gruhlke, Waterloo, Ind.

Texas Plants. I will collect sets of plants represented in this region of Texas, either for sale or for exchange for literature, for anyone who will send me an order in time to fill it. Plants to be ready complete in November. C. F. Maxwell, Box 174, Dublin, Texas.

For sale or exchange—A Telescope (36 diameters, copper barrel)—for $20 cash or scientific books of that value. A. N. Somers, La Porte, Ind.

For sale—A complete set of the Reports of the Second Geological Survey of Pa., 1874–1893, including the Grand Atlas. Publisher's price over $115. Will sell for $50. Address F. D. Chester, Newark, Del.

The undersigned has skins of Pennsylvania and New Jersey birds, as well as other natural history specimens: which he wishes to exchange for marine, fresh water, and earthworms of the South and West. Correspondence with collectors desired. J. Percy Moore, School of Biology, University of Pennsylvania, Philadelphia.

For sale or exchange.—I have a Caligraph typewriter (No. 2) in perfect order and nearly new. It is in a heavy leather, plush-lined office case, the whole costing me about $100. I desire to obtain for it, either by sale or exchange, a new, No. 5 "Kodak" camera, with six double feather-weight plate-holders and the latest pattern of their tripod. The lens and pneumatic time-shutter must also be the same as those now sold with the last No. 5 Kodak. The price of what I desire in exchange is $78. Address, for particulars, P. O. Box 314, Takoma, District of Columbia.

For sale.—An Abbe binocular eye-piece for the microscope. Alfred C. Stokes, 527 Monmouth St., Trenton, N. J.

For sale or exchange.—One good long range Remington B. L. rifle, 44 calibre, also land and fresh water, and marine shells. Want shells, Safety, camera or printing press. A. H. Boies, Hudson, Mich.

Wants.

WANTED.—Second-hand copy of Ehrenberg's Radiolaria, Berlin. 1875. Selected diatom slides, cash or both in exchange. D. C. Lewis, M.D. Skaneateles, N. Y.

WANTED, as principal of a flourishing technical school, a gentleman of education and experience who will be capable of supervising both mechanical and common school instruction. Special familiarity with some technical branch desirable. Address, giving age, qualifications, etc., J. B. Bloomingdale, Fifty-ninth street and Third avenue, N. Y.

WANTED.—A young man as assistant in our microscopical department. Queen & Co., Philadelphia.

THE undersigned desires specimens of North American Gallinae in the flesh for the study of their pterylosis. These species are especially desired: *Colinus ridgwayi, cyrtonyx montezumae, deudragapus franklini, lagopus welchi, tympanuchus cupido* and *pedioecetes phasianellus*. Any persons having alcoholic specimens which they are willing to loan or who can obtain specimens of any of the above are requested to communicate with Hubert Lyman Clark, 3922 Fifth Avenue, Pittsburgh, Pa.

A COMPETENT TEACHER of botany in college or university is open to engagement. Address L., Box 86, Rochester, Mich.

CAN any one inform me as to the age to which cats have lived? I have one twenty years old. Edward D. Webb, 132 W. Eighty-first St., New York.

WANTED—Second-hand. Foster's Physiology, Balfour's Comparative Embryology, Claus & Sedgwick's Zoology, Flower's Osteology of Mammalia, Vine's Physiology of Plants. Please state editions and prices asked and address Richard Lees Brampton, Ontario, Canada

A GRADUATE ENGINEER will give instruction evenings in geometry, trigonometry and surveying, mechanics, physics, mechanical drawing and general engineering construction. Five years' experience in field and editorial work on engineering journal. References furnished. C. S. H., 102 Tribune Building, New York.

SCIENCE

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SCIENCE

NEW YORK, JUNE 2, 1893.

THE LATEST DISCOVERIES IN CHALDÆA.

BY THE MARQUIS DE NADAILLAC, PARIS, FRANCE.

ALL we knew of Chaldæa was a very few years ago completely legendary. Thanks to our scientists and to their discoveries, every day brings us new and important facts. Inscriptions allow us to learn the names of kings six thousand years old. Sculptures disclose to us their faces, their weapons, their vestments, the soldiers who followed them, the horses who bear them. We may be allowed to say, with a just pride, that the nineteenth century, now so near its close, deserves well of science, and that never have the progresses in every branch of human knowledge been either so numerous or so wonderful.

In September last, Mr. Maspero presented to the Académie des Inscriptions[1] the photograph of a Chaldæan sculpture dedicated by King Naramsin, who reigned in Babylonia and in the northern parts of Chaldæa some 3800 years before our era. The sculpture actually in the Museum of Constantinople is in a very mutilated condition but shows, nevertheless, a masterly execution and can well be compared with the celebrated diorite statues excavated at Tello a few years ago. Centuries must certainly have elapsed before men could have acquired such an art and attained the civilization it allows us to presume. Centuries are not less necessary to arrive at the agglomeration of population required for the execution of similar monuments and the establishment of a kingly power. It is difficult even to imagine the vast past we must embrace.

The last researches have brought to light numerous works of a more rude and primitive art which tend to confirm our opinion. Amongst the discoveries lately reported by that eminent Assyrian archæologist, Mr. Léon Heuzey,[2] those of Mr. de Sarzec at Tello are the most important, and date certainly from older times than King Naramsin. The discovery of three new fragments have completed the celebrated *stèle des vautours*, so named on account of the vultures which hover above the scenes depicted, and certainly one of the oldest and most remarkable works of the Chaldæan artists.

The monument presents on both sides figurative scenes. It was ordered, as the inscription runs, by E-anna-dou, King of Sirpoula,[3] son of A-Kourgal and grandson of Our-Nina, one of the oldest Chaldæan kings as yet known to us. One of the most remarkable scenes which it has been possible to reconstruct represents a funeral after a battle or a thanksgiving after a victory. The number of the human bodies shows the severity of the fight. An ox is provided for the sacrifice to be offered to the presiding deity. Other fragments show E-anna-dou at the head of his soldiers, disposed in six ranks, armed with pikes and carrying large rectangular shields of a very peculiar form.

The king himself is represented in the act of piercing with his lance a defeated enemy, or on his chariot charging the flying crowd. This last scene is unluckily much damaged. In both of them the king carries in his left hand a long lance. The chariot is a set of panels curiously put together; on the front seat are deposited a battle-axe and a quiver full of arrows. The wheels unluckily have been destroyed. E-anna-dou wears a kaunakes rolled round his legs and a woollen cloak thrown over his shoulders and chest. He has no beard but an abundant flow of hair partly brought up on his neck and partly hanging on his back. In all the scenes in which the king appears, besides his lance he carries in his right hand a crooked weapon of a very peculiar form, which Mr. Heuzey compares to the weapon or mark of dignity carried by the head of the Asiatic tribe Amou in the celebrated picture in an Egyptian tomb of the XII dynasty at Beni-Hassan near Minieh. It is obvious that such a fact is worth noticing. Detail we must not omit in all these Chaldæan scenes, the ground is strewn with human bodies symmetrically arranged, head against head, as a carpet for the warlike and savage king.

The other side of the stela presents sculptures presumed to be of a mythological character. A man or a god of gigantic size is the principal actor; his bulky head, his powerful frame, his broad shoulders, form a striking contrast with the figures already described. His hair is in the same style as the hair of King E-anna-dou, and is maintained by a large head-band; but what distinguishes him from the royal figure is a flowing beard plaited in the Assyrian fashion. The body is naked, the middle part alone is covered by some sort of cloth, of which little remains. In his hands the giant carries a massive club and a curious instrument, the use of which is difficult to guess; it figures an eagle whose claws rest upon a lion's head, the heraldic figuration of the town of Sirpoula, says Mr. Heuzey, to whom we leave the responsibility of the assertion.[4]

Under the principal personage is a very striking scene. Near the arm which carries the eagle is an immense net in which crawl in all possible attitudes, trying to escape through the meshes, a number of naked men whose features recall those of the captives under the feet of E-anna-dou and who evidently belong to the same race. We cannot tell the meaning of this scene; it brings to mind the curses of the prophet Habakkuk, comparing the defeated populations to fishes which the Chaldæan conqueror carries off in his net.[5]

With these sculptures were found fragmentary inscriptions difficult to decipher. We can, nevertheless, read "Isbanki," with its principal town, "Ner-ki-an," as a country subdued by King E-anna-dou. We find also mentioned two Chaldæan towns, Our and Erech, which are named in Genesis. They were, therefore, in existence and in communication with Sirpoula in those very ancient days, certainly many centuries before Moses.

E-anna-dou, we have said, was the grandson of Our-Nina, already king of Sirpoula. We are in possession of numerous antiquities which can leave no doubt of the existence of Our-Nina, bricks from an edifice probably a temple erected by him,[6] tablets with inscriptions,[7] other tablets with animal figures, small bronze statues, the fragments of an onyx vase, and sculptures which show the king amongst his court and family.

One of the fragments thus discovered shows a double procession marching towards a man placed alone in the middle. The name of Our-Nina, twice repeated, leaves no doubt as to the personage so figured. The king is naked with only a kaunakes rolled round his loins. His hair is closely shaven, and he carries upon his head a large basket similar to those carried by the slaves in the *stèle des vautours* and very similar also to the couffe in use with the Arabs till this day. In his devotion, the king is carrying the materials for the building of a temple. An inscription recently deciphered puts the fact above discussion. It reads thus: "Nina-Our, son of Nini-hal-dou, son of Gour-sar, the temple of

[1] Bul., Sept. 30, 1892.

[2] Bul. Acad. des Inscriptions, 12 Aout et 21 Octobre, 1892.

[3] Assyriologists look upon Sirpoula as the same town as Lagash, mentioned in some cylinders also found by Mr. de Sarzec.

[4] The giant himself, according to our eminent Orientalist, is the god or hero Isdoubar, figured in a very old relievo discovered a few years ago by Mr. de Sarzec.

[5] "They take up all of them with the angle, they catch them in their net and gather them in their drag. Therefore they rejoice and are glad. Shall they therefore empty their net and not spare continually to slay the nations?"—Habakkuk, C. IV., 15-17.

[6] Découvertes en Chaldée, Pl. xxxi., Fig. 1.

[7] Revue d'Assyriologie, T. II., p. 147.

the god Nin-ghir-sou has erected." We read, also, in another inscription behind the head of the king: "Nina-Our, King of Sirpoula," and a little above his knees: "from Magan in the mountain quantities of wood he has ordered," and after the last personage figured: "the temple of the goddess Nina he has erected."[1] We are evidently in presence of a very pious prince, and we know from other sources that he erected or repaired a certain number of temples dedicated to his gods. All the figures and inscriptions show a most primitive art, inferior to that dating from the days of E-anna-dou. They are nevertheless of high value as historical and genealogical records.

The importance of these discoveries cannot be overrated. That importance resides not only in the insight they bring on the customs, wars, and religions of nations whose very names were unknown but a few years ago, but also in the greater antiquity we must now accept for the origin of man himself. The dates given for the creation must be amended, as we know now with certitude, that in those days men and nations already existed in numbers, towns were built, monuments were erected, arts were flourishing, kingdoms already powerful were in existence, and we find both in Asia and in Africa traces of a civilization which centuries alone could have reared and maintained.

AN EXPERIMENTAL BASIS FOR LITERARY CRITICISM.

BY CONWAY MAC MILLAN, UNIVERSITY OF MINNESOTA, MINNEAPOLIS, MINN.

THE volume, entitled "Analytics of Literature," just published by Ginn & Co. of Boston, seems to the writer so epoch-making a work that he takes advantage of the courtesy extended by *Science* to direct the attention of scientific men in general and biological students in particular to the new and brilliant application in it of the familiar methods of research which they have themselves used in other departments of investigation. The truth is that there is the emergence of a new science — the science of experimental criticism, or, if one likes, the science of style-morphology, embryology, and physiology. It is a most noteworthy volume, and though unpretentious and perhaps marred by departures here and there from the strict scientific method, it will take its place with such a work as that of Fechner, in which he brought recalcitrant psychology under the laws of empiricism, and banished the intuitional and closet-metaphysician in the ratio in which he introduced the laboratory method of psycho-physics and the experimental psychologist of the school of Wundt. No more far-reaching scientific work has been done in America than the reduction, in this book by Dr. L. A. Sherman, of so mysterious a matter as literary style to the basis of a department of experimental science. But after an acquaintance with the method and an application of it, during the ten years past in which it has been laboriously and carefully developed by its originator, I have no hesitation in pronouncing the work an extraordinary and inspiring advancement of biological methods into a field where, oddly enough, they have not before been employed.

The new point of view is simply this: style is an institution. It may be considered apart from the message which the writer wishes to convey. Under such an analysis style is found to obey the laws of other institutions or organisms. It is a matter of evolution. It is in any case a structure of which the phylogeny and the ontogeny may be calculated. In the child, one may study the ontogeny of a style, and of children's phases of sentence development the author of "Analytics of Literature" gives some valuable examples. And in the literature of the English-speaking peoples there is a vast storehouse of palæontological material from the study of which, after comparison with the ontogenetic development, it is possible to determine some of the laws of style-evolution. Thus a foundation for a style taxonomics is laid and one finds that, as one should expect, all the well-known laws of heredity in general and of progress, degeneration, variation, reversion, or atavism, persistence of type and modification of type in particular, apply to literary styles precisely as to organisms. It becomes possible to determine a style, not in the old intuitional manner of literary art as indicated in Sainte Beauve, Arnold, or Lessing, but in the precise manner of the zoölogical monograph. It becomes possible to establish genera, species, tribes, orders, if one will, of literary style, and the whole matter of literary criticism at one touch passes over into the domain of natural science, just as music so passed in the thought of Schopenhauer and Wagner, metaphysics under the hand of Wundt, biology by the genius of Aristotle, Bacon, and Darwin.

The genesis of such a work must be of interest. As indicated in the preface it was a development, not an inspiration. The first published paper that pointed out the objective method in criticism, so far as known to the writer, was that of Sherman in the *University Studies*, October, 1888.[2] Here the matter of enquiry was the changing length of the sentence in English prose and a number of statistics were presented. It was shown that there has been a progressive shortening of the sentence from early pre-Elizabethan prose to the present. Some data are added here by way of illustration. They are taken from both the *Studies* article and from the recent volume.

Average number of words to the sentence in various English writers, computed from prose, on the basis of five hundred sentences.

Chaucer.	40. +	Browne,	33.40
Thomas More,	52. +	Fuller,	32.80
Lyly,	52.22	Addison,	37.90
Roger Aschman,	42. +	"Junius,"	31.90
Sidney,	50 65	DeQuincy,	32.28
Fabyan,	63 02	Matthew Arnold,	37.
Spenser,	49.82	Lowell,	38.
Hooker,	44. +	Pater,	36 5
Bacon,	22.	Macauley,	22 45
Dryden,	45.26	Channing.	25.73
Bunyan,	37.50	Emerson,	20.58
Milton,	60.80	Bartol,	15.97

These averages once established may be tried in other parts of the works of any author and will be found practically constant. For example, in Macauley's "Essays" the average length of the sentence is 23.+. Testing by the "History of England" it was found that in this the average of the 41,579 periods counted is 23.43 words per sentence. Thus it can be shown in any author who has acquired a style that five hundred or a thousand sentences taken at random will establish a sentence-norm for that author and from this norm the variation will be slight. Disparities, too, are greatest in more ancient styles, indicating their less complete organization. For example, in Chaucer the average of Melibœus is 48.+, while that of the Parsonn's tale is 36.+, an almost unparalleled discrepancy.

It is possible, then, for any author to plot a curve of sentence-length, and when this is done the surprising fact stands forth that the average is brought about by "evening-up" a comparatively large number of sentences only a little shorter than the mean with a comparatively few sentences excessively long. Since the long sentence is clearly shown by palæontologic investigations to be the older type in any literature, it appears that in modern stylists, even, there is an atavistic tendency, and this is capable of beautiful and instinctive comparison with the persistent styles of low type that can be picked up anywhere — in newspaper-advertisements or in cheap novels — where, if there is an independent style at all, it will be one of older and lower organization.

Even in the preliminary analysis of sentence-length, singular and unintelligible facts have been discovered that demand further investigation before their import can be known. DeQuincy is peculiar in the number of prime-sentences, those in which the number of words is indivisible by any quantity but the number itself and unity. Curious lapses into ancient manner in moderns and astonishing forecasts of modern manner by ancients

[1] Amiaud, "Records of the Past," T. I., p. 64; Jensen, "Keilenschriftliche Bibliothek," T. III., p. 10.

[2] On the Sentence-length in English Prose, pp. 119–130.

—as. for instance, in the case of Bacon—attract one's attention. But space permits of no extended indication of these points.

Next to the shortening of the sentence. a decrease in predication is a striking fact in the evolution of the prose-style. While in earlier writers the per cent of simple sentences is small, it rises rythmically to a high average in modern stylists. The following examples will illustrate:—

	Per cent Simple Sentences.		Per cent Simple Sentences.
Chaucer,	.4	Shaftesbury,	27
Spenser,	11	DeQuincy,	14
Hall,	7	Macauley,	39
Sidney,	10	Channing,	31
Hooker,	12	Newman,	16
Barrow,	15	Emerson,	37
Addison,	12	Lowell,	23
Bacon,	19	Grant,	31
Bunyan,	10	Everett,	32
Bolingbroke,	13	Bartol,	45

The laws of shortening and simplifying the English prose-sentence may be derived from comparative morphological studies in styles, or better by the assistance of ontogenetic or embryological work. The latter method is called in by Dr. Sherman in the series of comparisons between the style of speech of the child and the literary styles in the phylogenetic series. The stages in either case are found to be (a) monosyllabic exclamation, (b) predication, (c) co-ordination of predications, (d) sub-ordination of some predications to others, (e) suppression of less important predications. Examples from the early lispings of childhood are brought side by side with others selected from the field of English prose and a statistical enquiry, most subtle and ingenious, is instituted into the various percentages of illative, temporal, causal interior and exterior conjunctions in different writers. The result, simple as it seems when once fairly grasped, is no less splendid an achievement of the biological method in its new application. There is in the child. as it learns to talk, a recapitulation of the phases through which the English writers of prose have passed in their development of the modern style. The record of the palæontological series tallies with that of the embryological and one can explain the changes from the earlier styles to the later by the same laws that one sees at work in the child as he learns the art of speaking and of writing.

In the discussion of poetry the same scientific method may be used, and its employment is indicated, but somewhat less fully, by Dr. Sherman. In the portion of the "Analytics of Literature," which is particularly devoted to the poetic side of English, the most notable discovery is doubtless the law of intensification, through which, when associations were few, the poetic idea demanded a whole sentence for its vehicle, as in Chaucer; but as the association value of words increases the poetic idea can be carried by clauses, as in Shakespeare, by phrases as in Keats or Shelley, and, finally, by single words, as excellently illustrated in Browning. This discovery is made the basis of a scientific analysis of different poetic styles and the results obtained while new are of the deepest value. Things before mysterious and the subjects of vatic utterance by the various critics, become suddenly transferable to the solid ground of experiment and calculation. Poetry is no longer presented to one as something to be intuitionally appreciated but as an object of experience and of analysis after the ordinary methods.

On the whole, it is not possible to commend too highly this new departure in a field which has long lain in darkness, awaiting the light of science to make its laws and phenomena generally apparent. The adoption of such clear-cut, substantial, experimental foundation in rhetorical courses in colleges and schools cannot but be of the highest utility. It is evident, furthermore, that a vast untried territory is now discovered to those who wish to engage in useful research. It becomes apparent how halting and poor is former critical method when one notes what tremendous conquests of unknown facts are possible through this single pioneer work. The study of literature—after the usual objection and objurgation from those not yet in sympathy with the unifying power of the scientific method—promises to take its place not as an art, but as a science of the biological series. Too much praise can hardly be laid upon the writer of this work which so definitely hands literary criticism over into the hands of scientifically-minded men.

IN REGARD TO COLOR-BLINDNESS AMONG INDIANS.

BY LUCIEN I. BLAKE AND W. S. FRANKLIN, PHYSICAL LABORATORY, UNIVERSITY OF KANSAS, LAWRENCE, KAN.

THE fact that blindness to certain colors exists among civilized people, is well established; also the percentage of cases to be found among males has been determined with considerable probability for the races of Europe and America. There has been much diversity in methods of testing, and the results of many reported determinations might well be called in question. Still, it is probably not far from the truth that four out of every hundred males are more or less deficient in color-sense. Of females, there have been reported (B. J. Jeffries, M.D., 'Color-Blindness," p. 85) as examined in Europe and America, 39,828, and of these only 60 were color-blind, or two-tenths per cent. Of both males and females, 156,732 have been tested, and of these 5,417, or 3.52 per cent, were color-blind. These statistical facts have naturally excited interest and discussion. If so large a number as four out of every hundred are unable to distinguish colors, there arises, of course, a practical question, important to the railroads, marine, etc.

The gravity of this fact is already recognized more or less in all countries, by the test-examinations for color-blindness among employés. But there is in these statistics also much of interest to scientists.

Most cases of color-blindness are found to be congenital and are incurable. Many have been produced by disease, some by violent concussions in accidents, and some by excessive use of tobacco and alcohol. Temporary blindness to violet may be induced by santonine. From these facts several interesting questions have suggested themselves to us. If color-blindness follows the laws of heredity, is it on the increase or decrease? Further, is it a product of civilization? The first of these queries can be answered only by statistical data extending over long periods of time. The second naturally suggests a comparison: first, of the color-sense of civilized nations among themselves; and second, of civilized with uncivilized peoples.

Of tests on native tribes, we can find but two recorded—those of Dr. Favre on some tribes in Algiers, and those of Dr. Fox on 150 American Indians, but where we do not know.

First, for the comparison of civilized tribes among themselves we have calculated the following percentages from tables reported by Dr. Jeffries:—

Countries.	No. Examined.	Per Cent Color-Blind.
Austria	5,250	3.79
Denmark	5,840	3.74
Belgium	8,106	4.13
Holland	2,300	1.43
Finland	1,200	5.00
Norway	205	4.88
Sweden	32,504	3.73
Switzerland	3,024	5.36
Germany	6,344	4.12
Russia	12,830	3.30
Italy	2,065	2.32
England	16,431	3.75
United States	44,844	3.64

Average per cent, 3.76.

No great reliance can be placed upon these results. The numbers examined are too small, the methods of testing not uniform or equally reliable. However, the probabilities of error are almost equally distributed, so that the conclusion is fairly well established, even without great accuracy of data, that among civilized nations color-blindness is almost equally common.

Second. Among uncivilized people Dr. Favre's results from Algiers, already alluded to, show 414 examined, and only 2.6 per cent color-blind.

Dr. Fox reports 161 young Indians in the United States tested, and only 1.81 per cent are color-blind.

These percentages. so low compared with those for civilized people, suggested to us the thought that color-bindness may be a product of civilization, and these have led to our own tests, here reported.

At the Haskel Institute, at Lawrence, Kansas, are several hundred Indians, representing many tribes. These we have recently tested by Holmgren's method, with Berlin worsteds. 418 have been examined — 285 males and 133 females — only three cases of color-blindness exist, or only $\frac{7}{10}$ of 1 per cent. These were males, and all full-blooded Indians. The tribes were Pottawatomie, Pawnee, and Cheyenne. Of these two had defective color sense for red and one for green.

The Indians were almost evenly divided as full-bloods and half-breeds. It seemed to us that the half-breeds showed more instances of blunted color-sense than the full-bloods. This was evidenced in more frequent and prolonged hesitation among them in comparing the colors, than among the full-bloods. If this be confirmed by more extended examinations, it would, in conjunction with the low percentages obtained as above, be a strong argument for the theory proposed by us, that defective color vision is in some way the product of civilization. To this conclusion, our tests, at least, seem to point. The data are too meagre at present to propose any explanation why defective color-vision comes with civilization. It is not accidental that nearly every case of color-blindness is for red, fewer for green, and seldom one for violet.

What is the meaning, that the defects are thus limited at present, at least, to the lower end of the spectrum? The Helmholtz-Young theory of color perception will locate the affection in the layer of rods and cones responding to the first of the three primary sensations of color. But why this special layer is, with few exceptions, the only one affected, has at present no explanation. Also why the percentage among females is so small, has no explanation.

The law of heredity indicates increased sensitiveness in those nerves which are subjected to special use through many generations. It seems reasonable to look for an explanation of the more perfect color-sense in females, to this fact,— but whether this law of heredity will increase the percentage among males cannot be foretold without an enormous increase of data.

The theory here proposed is that defective color-sense is a product of civilization with the use of tobacco as a possible factor. The non-use of tobacco would explain also the low percentage of color-blindness among females. This theory leads to the thought of increase of color-blindness in males in the future generations.

THE VERTICAL SCRIPT.

BY W. H. METZLER, BOSTON, MASS

I PRESUME that most of the people of this country were taught to write the slanting script, according to a code of rules such as that given by Spencer, DeGraff, and others. It would be interesting to know what proportion have continued consciously or unconsciously to observe those rules, and what proportion have forsaken them for a position of body, pen, and book more suited for rapidity and ease, and no doubt in many cases better from an hygienic standpoint.

Little observation will be required to convince us that there are but few who observe the rules they were taught.

Some years ago I had charge of about one hundred and twenty-five pupils in writing, who had been taught the slanting script according to Spencer's rules. After using that method a short time, I became convinced that the collapsed position which very many assumed was due to the methods. When allowed to write as they pleased, about 5 per cent of them observed all the rules, about 70 per cent observed part of them, but not all, and the remainder apparently observed none of them. Those pupils placing the book directly in front, with about an equal amount of both forearms on the desk, sat most erect and wrote a script varying but little from the vertical, and those turning the right side, placing the right forearm on the desk parallel to its edge, sat least erect. Observing this, and my own experience having taught me that with paper directly in front I must sit more erect, could write faster, and with a good deal more ease than with it at the right, I directed the pupils to place their writing-books in front of them, and found beneficial results follow in that the body was kept more erect and the writing on the whole much improved. At that time I had not heard of what is now known as the *vertical script*.

It, together with the many evils resulting from the methods so commonly used in this country, was first prominently brought to my mind while attending a course of lectures given by Dr. W. H. Burnham at Clark University in 1891–92. The substance of which is contained in an invaluable paper published in the *Pedagogical Seminary*, Vol. II., No. 1. Dr. Burnham has made a thorough study of the subject of school hygiene, and his paper gives, besides a comprehensive bibliography, the opinions and conclusions of the best writers and investigators of different countries, and should be in the hands of all teachers and school boards.

The following are some of the rules given by Schubert for writing the vertical script: —

1. Straight-central position of the tablet or copy-book. 2. Two-thirds of both forearms should rest on the desk in symmetrical positions, meeting at right-angles and forming an angle of 45° with the edge of the desk. The elbows should be about a hand's-breadth from the body. 3. The hand should rest on the outer edge of the nail of the little finger. The index finger should form a slightly convex bow. 4. The pen-holder should be long and grasped not too near the pen. Its upper part should not rest against the index finger, but on the middle of the hand between the thumb and index finger, and should point towards the elbow rather than towards the shoulder or breast. 5. The arm as it moves toward the right in writing should be moved as a whole, so that all positions that it occupies will be parallel. 6. After each line the paper should be correspondingly raised, so that a proper distance between the point of the pen and the edge of the desk be preserved. 7. The lines should be short. 8. The lines joining the eyes and the shoulders should be horizontal, and the eyes from 30 to 35 centimetres from the paper.

Since hearing the lectures and reading the paper, I have made some observations to ascertain whether those placing the book directly in front of them sit more erect than those who place it at the right, and though I did not find many who used the straight-central position, yet I found that a larger per cent of those placing the book in front sit erect than of those placing it at the right.

Since so many evils are due to poor methods in writing, it would seem that the subject should receive far more attention than has heretofore been given to it, and the vertical script be given a thorough test at least. The fact that no two hand-writings, like no two faces, are exactly alike would indicate that, after a few general instructions to secure a healthful position of the body, no complex set of rules should be given. Each person will then develop that particular hand-writing most suited to him.

DISINFECTANTS AND DISINFECTION.

BY DAVID BEVAN, M.D., PHILADELPHIA, PA.

THE question of disinfectants and disinfection has come to be of as great practical importance as it is of scientific interest. The term disinfectant is by the laity, and to some extent by the medical profession, grossly misused in being considered as synonymous with antiseptic and deodorant, since science has so ably demonstrated the nature of the contagium in infectious and contagious diseases, only such agents as are capable of destroying the contagium are to be designated as disinfectants. An antiseptic retards or prevents the development and pullulation of the organism; but the organism is not destroyed. A deodorant merely destroys odors, but does not necessarily have any effect whatever upon the organism.

The cholera scare of last summer inaugurated a season of apprehension and an unprecedented demand for disinfectants.

The universal cry for a disinfectant has given birth to a number of compounds, the virtues (?) of which are only equalled by the number and chemical incompatibilities of their ingredients. These compounds are often efficient deodorants; sometimes they are antiseptics, but never are they disinfectants.

To consummate the process of disinfection, there are two essential requirements, 1st, that the substance to be acted upon is infected; 2d, that the agent employed is a disinfectant. During an epidemic or in sporadic cases of infectious diseases, the efficiency and adaptability of a given disinfectant will depend greatly upon the nature of the substance to be disinfected and also as to the presence or absence of spores.

The various subjects for disinfection will now be considered and under each the most available and efficient disinfectant.

Excreta, Sputum, or other Discharges. By far the best disinfectant that we possess is the bichloride of mercury; as in solution of 1: 1000 it destroys anthrax spores after a few minutes' exposure. In using this salt three precautions are to be observed: 1st, its extremely poisonous character; 2d, its corrosive action on all of the common metals, and 3d, the facility with which it combines with albumen to form an insoluble, inactive compound.

That the dangers arising from poisoning may be reduced to a minimum, it will be found expedient, 1st, to color the tablets or solution, whichever it may be, with one of the aniline dyes, and further to keep the salt in a peculiarly-shaped bottle, conspicuously labelled. In reference to the second precautionary measure, it should never be used except in metallic vessels. To prevent the salt combining with albumen acidulate the solution. In Wilson's "Hygiene" we find the following formulæ recommended by the Local Government Board of Great Britain. Dissolve half an ounce of corrosive sublimate, one fluid ounce of hydrochloric acid, and five grains of commercial aniline blue in three gallons of water.

Carbolic acid, in a five per cent solution, as a disinfectant for excreta, though very popular, is, taken all in all, extremely unsatisfactory. Upon adding such solution to a quantity of excreta, the additional dilution and the presence of large quantities of organic material decidedly interfere with its action and render it unreliable. If carbolic acid be used, it should be according to the following formula:—

Carbolic acid	10 parts.
Glycerine	10 "
Water qs.	100 "
Mix.	

Expose the excreta to an equal quantity of this solution for at least six hours.

Chloride of lime has been highly recommended in a solution containing four per cent of available chlorine. Although the chloride of lime will not destroy the more highly resisting forms of contagium, e. g., anthrax spores, it is a most excellent agent for disinfecting the stools of cholera Asiatica and typhoid fever. To make the above strength solution, dissolve six ounces of the best chloride of lime, containing twenty-five per cent of available chlorine, in one gallon of water.

Underclothing, bed linen, towels, napkins, etc., if of little value should be destroyed by fire, otherwise, we may expose them, in a suitable apparatus, to flowing steam for fifteen minutes or resort to boiling for twenty minutes or immersing in a 1: 1000 solution of the bichloride of mercury.

For disinfecting the hands, we recommend one of the following methods:

The nails should be short and clean.

The hands are thoroughly washed for several minutes with soap and water, the water being as warm as can be comfortably borne, and being changed frequently. Use a brush which has been sterilized by steam. The excess of soap is washed off with clean warm water. The hands are immersed for one or two minutes in a warm saturated solution of permanganate of potash, and are rubbed over thoroughly with a sterilized swab. Then place the hands in a warm saturated solution of oxalic acid until they are completely decolorized. Wash the hands with a sterilized salt solution of water. Immerse the hands for two minutes in a 1: 500 solution of the bichloride of mercury.

The above method is used by Welch of the Johns Hopkins Hospital.

Professor Keen, of the Jefferson Medical College Hospital, uses the following method:—

The hands are washed with soap and warm water; the nails, being cleaned and trimmed with a knife, are then scoured with a sterilized brush. All loose skin about the nails is removed. The hands are again washed in warm water but without soap. Immerse the hands in alcohol for two minutes and briskly rub one over the other. They are then immersed in a 1: 1000 solution of the bichloride of mercury. This latter method is a most excellent one. The writer has tested the skin and nails of the hands, after being sterilized as above directed, and also the cat-gut and silk, which were handled by the professor or his assistants, with almost invariably negative results.

To disinfect the general surface of the body, wash with a 1: 2000 solution of the bichloride of mercury and then bathe in warm water.

Should a person die of an infectious or contagious disease, the body must be cleaned and disinfected before removing from the isolation quarters. To disinfect the body, first wash it in clean water and then wrap in a sheet thoroughly saturated with a 1: 500 solution of the bichloride of mercury.

So long as the source of infection remains, there is a continuous reproduction of the poison. It is impossible to disinfect a room during its occupancy by a person suffering with an infectious or contagious disease, by liberating gaseous disinfectants, as any such agents of sufficient potency will kill the patient. However, the wall, furniture, etc., may be washed with a 1: 2000 solution of the bichloride of mercury and then with warm water. In such instances, the greatest reliance is to be placed upon cleanliness and ventilation. If these two provisions be thoroughly carried out, offensive odors will be abolished, or prevented from accumulating in such force as to be disagreeably perceptible. Ventilation should never be effected through another room or hallway, but communication established and maintained with the outside air and in such a manner as not to create draughts. As soon as the infectious nature of the disease is determined, the patient should be isolated. The room should contain as few articles as possible. All upholstered furniture and drapery should be removed and their places supplied by wooden articles and simple muslin or linen curtains. The attendant or attendants upon the sick should not be permitted to associate with other persons or to leave the isolated portion of the dwelling without first disinfecting their person and putting on clean clothes from the skin out. The sending of unnecessary articles into the room, such as extra napkins, towels, etc., should be strictly interdicted. Everything coming from the sick quarters should be disinfected by one of the methods already indicated.

To disinfect the vacated room or rooms, first disinfect and remove all the furniture, etc., then close all cracks and crevices about the windows and doors, leaving one door open; place in the room a tub in which there are about three inches of water. In the centre of the tub place a large shallow pan, preferably of iron, containing two pounds of sulphur for every one thousand cubic feet of air space in the room. Set fire to the sulphur and drop into the tub, about the sulphur pan, several very hot bricks. Quickly leave the room, close the door and all crevices about it. The infected quarters are now air-tight, sulphurous acid gas is being generated, so also is steam, which will facilitate the action of the gas and secure better penetration. The room is to remain thus for twenty-four hours, then to be ventilated freely; the surfaces washed with a 1: 1000 solution of the bichloride of mercury and lastly with warm water.

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IRON AND ALUMINIUM IN BONE BLACK: THEIR QUANTITATIVE DETERMINATION.

BY DR. F. G. WIECHMANN, COLUMBIA COLLEGE, NEW YORK.

THE determination of iron and aluminium in bone black has thus far been commonly effected by the so-called ammonia-acetate method, which, until quite recently, has also been the favorite method employed for the determination of the constituents mentioned in mineral phosphates.

As this method, however, is open to serious objections, it was decided to test its accuracy, and to compare the results obtained with those yielded, respectively, by the method of E. Glaser,[1] and by the combination of Glaser's method with that of A. Stutzer,[2] first suggested by R. Jones[3] for the analysis of fertilizers.

For valuable analytical work performed in this connection, the writer's thanks are due his assistant, Mr. E. C. Brainerd.

The schemes of analysis used in this investigation are minutely given in the following directions:—

Method I. Acetate of ammonia process.

(This method is based on the solubility of calcium phosphate in acetic acid, and on the insolubility of the phosphates of iron and aluminium in this medium.)

1. Powder sample. 2. Dry thoroughly. 3. Weigh out 3.0 grammes. 4. Dissolve in distilled water + 25 cubic centimetres HCl (conc.), boiling gently for one hour. 5. Filter. 6. Wash residue on filter, until the wash-water no longer reacts for Cl with $Ag\ NO_3$. 7. Add excess of $Ba\ Cl_2$, boil till $Ba\ SO_4$ is granular. 8. Filter. 9. Wash the $Ba\ SO_4$ on the filter till no more reaction for Cl with $Ag\ NO_3$. 10. To filtrate and wash-waters combined add NH_4OH, until the precipitate formed begins to appear permanent. 11. Then add acetic acid to pronounced acid reaction, and boil. 12. Filter. 13. Wash the precipitate well. 14. Dry, ignite, weigh. 15. Regard the precipitate as $Fe\ PO_4 + Al\ PO_4$, calculate to $Fe_2\ O_3 + Al_2\ O_3$, and so report.

Method II. Glaser's method.

(In this process the calcium is removed from an alcoholic solution by means of sulphuric acid before the precipitation of the iron and aluminium is effected.)

1. Powder sample. 2. Dry thoroughly. 3. Weigh out 5 0 grammes. 4. Dissolve in distilled H_2O + 30 cubic centimetres H Cl (conc.) + 10 cubic centimetres $H\ NO_3$ (conc.). 5. Make the solution up to 500 cubic centimetres with distilled water. 6. Filter. 7. Of the filtrate take 100 cubic centimetres (equal to 1.00 gramme), place in a 250 cubic centimetre flask, add 25 cubic centimetres $H_2\ SO_4$ (conc.). Shake frequently, and allow to stand for five minutes. 8. Add absolute ethyl alcohol, cool, fill up to the mark with alcohol, and shake well. 9. As volume contraction will take place, fill up to the mark repeatedly with alcohol, and shake each time. Continue this filling up to the mark until no more contraction takes place. 10. Allow the solution to stand for 12 hours. 11. Filter. 12. Of the filtrate take 100 cubic centimetres (= 0.4 gramme), place in a large platinum dish on a water-bath, and heat until all the alcohol is removed. 13. Wash the remaining solution into a beaker with 50 cubic centimetres of distilled water. 14. Heat to boiling, and then remove the flame. 15. Add NH_4OH very carefully to alkaline reaction. 16. Boil until the ammonia is completely expelled. 17. Filter. 18. Wash the precipitate thoroughly with boiling, distilled water. 19. Dry, incinerate, weigh. 20. Regard the precipitate as $Fe\ PO_4 + Al\ PO_4$, calculate to $Fe_2\ O_3 + Al_2\ O_3$, and so report.

Method III. Combination of the methods of Glaser and Stutzer.

(Stutzer's method consists essentially in precipitating the iron and aluminium, principally as phosphates, in a solution of ammonium acetate; treating this precipitate with a solution of ammonium molybdate, to remove the phosphoric acid as phospho-ammonium molybdate; filtering out this precipitate, and in the resulting filtrate precipitating the iron and aluminium as hydrates, by ammonium hydrate; drying and igniting this precipitate, weighing it as $Fe_2\ O_3 + Al_2\ O_3$, and reporting it as such.)

The following scheme, it is believed, offers all the advantages of both the Glaser and the Stutzer methods.

Proceed exactly as in Method II. up to and inclusive of section No. 18.

Then continue as follows:—

1. Place filter and contents in a beaker which contains 150 cubic centimetres molybdic solution,[4] at a temperature of about 40° C. 2. Keep the mixture at a temperature of about 65° C. for from 12 to 15 hours. 3. Filter out the precipitate. 4. Wash the precipitate thoroughly with $NH_4\ NO_3$ solution (1:10). 5. To filtrate add $NH_4\ OH$ till it is well alkaline. 6. Heat for 2 or 3 hours over a gentle flame, replacing any loss by evaporation by the addition of water and ammonic hydrate. 7. Filter out the precipitate. 8. Dissolve this precipitate from the filter with H Cl. 9. Precipitate with NH_4OH, and boil out all free ammonia. 10. Filter. 11. Wash precipitate, dry, incinerate, and weigh. 12. Regard the precipitate as $Fe_2\ O_3 + Al_2\ O_3$, and so report.

The mixture on which these three methods were tested consisted of:—

Tri-calcic phosphate...	20.00 grammes
Aluminium sulphate..............	0.10 "
Ferrous sulphate..................	0.10 "

These amounts of the sulphates of iron and aluminium corresponded to 0.67 per cent of $Fe_2\ O_3 + Al_2\ O_3$, as was ascertained by analysis.

The mixture was dissolved in $H_2\ O + H\ Cl$, and made up to 500 cubic centimetres. 24.75 cubic centimetres of this solution contain 1.00 gramme of the dry substance.

In Method I., used 3.00 grammes; in Method II., 1.00 gramme; in Method III., used 1.00 gramme of the "dry substance" for analysis.

Results of Analysis.

		Method. I.	Method. II.	Method. III.
		Per cent.	Per cent.	Per cent.
$Fe_2\ O_3 + Al_2\ O_3$	Present	0.67	0.67	0.67
	Found	0.56	0.60	0.63

Method I. has evidently yielded the least satisfactory result.

[1] Zeitschrift für Angewandte Chemie, 1889, p. 636.

[2] Zeitschrift für Angewandte Chemie, 1890, p. 43.

[3] Chemiker Zeitung, 1890, p. 269.

[4] Dissolve 100 grammes molybdic acid in 40 grammes, or 417 cubic centimetres of ammonium hydrate (sp. gr. 0.96), and pour the solution thus obtained into 1500 grammes, or 1250 cubic centimetres, of nitric acid (sp. gr. 1.20). Keep in a warm place for several days, decant the solution from any sediment, and preserve in glass-stoppered vessel.

Examining into its merits, it is readily seen that this method, as previously stated, is open to several serious objections: phosphate of aluminium is quite soluble in an excess of acetic acid; the precipitate of the phosphates of iron and aluminium is very apt to carry with it some of the calcium salt; the precipitate of the iron and aluminium obtained is not necessarily pure normal ortho-phosphate; and, finally, there is a great risk of introducing an error in calculating the combined phosphates of iron and aluminium over to the sesqui oxides.

The molecular masses of the compounds concerned are:—

$$\begin{aligned} Fe\,PO_4 &= 151 \\ Al\,PO_4 &= 122 \\ Fe_2\,O_3 &= 160 \\ Al_2\,O_3 &= 102 \end{aligned}$$

If the constituents, the iron and the aluminium phosphates, occur in the precipitate in the proportion of their respective molecular masses, i.e., 151 : 122, no error will be committed in assigning to this precipitate of the mixed phosphates the formulæ, ($Fe\,PO_4 + Al\,PO_4$), and calculating to $Fe_2\,O_3$, as is shown by the following example. Assume the composition of the precipitate to be:—

$$\begin{aligned} Fe\,PO_4 &= 0.151 \\ Al\,PO_4 &= 0.122 \\ \hline Fe\,PO_4 + Al\,PO_4 &= 0.273 \end{aligned}$$

Calculating the combined phosphates over to the combined oxides:—

$$2\,(Fe\,PO_4 + Al\,PO_4) : (Fe_2\,O_3 + Al_2\,O_3) :: .273 : x$$
$$546 \quad : \quad 262 \quad :: .273 : x$$
$$x = 0.131$$
$$\text{i.e., } (Fe_2\,O_3 + Al_2\,O_3) = 0.131$$

Calculating the $Fe\,PO_4$ and the $Al\,PO_4$ separately over to their respective oxide, and then adding them:—

$$2\,Fe\,PO_4 : Fe_2\,O_3 :: 0.151 : x$$
$$302 \quad : \quad 160 \quad :: 0.151 : x$$
$$x = 0.080 \quad Fe_2\,O_3$$
$$2\,Al\,PO_4 : Al_2\,O_3 :: 0.122 : x$$
$$244 \quad : \quad 102 \quad :: 0.122 : x$$
$$x = 0.051 \quad Al_2\,O_3$$

$$\begin{aligned} &0.0800\ Fe_2\,O_3 \\ &0.0510\ Al_2\,O_3 \\ \hline &0.1310\ Fe_2\,O_3 + Al_2\,O_3, \end{aligned}$$

which is identical with the value previously obtained. If, however, the iron phosphate and the aluminium phosphate are present in a proportion different from the one assumed in the above example, the result obtained by calculating their combined weight to combined oxides is wrong. It will be too high or too low, accordingly as the iron, aluminium, or the phosphate predominates.

Example.—Assume that the combined phosphates weighed exactly the same as before = 0.273 gramme; but assume the composition of the precipitate to be:—

$$\begin{aligned} Fe\,PO_4 &= 0.219 \\ Al\,PO_4 &= 0\ 054 \\ \hline & \quad 0.273 \end{aligned}$$

Calculating the combined phosphates over to the combined oxides, of course the same result as previously found will be obtained, namely, that

$$\underset{(Fe\,PO_4\,+\,Al\,PO_4)}{0.273} = \underset{(Fe_2\,O_3\,+\,Al_2\,O_3)}{0.131}$$

But calculating the $Fe\,PO_4$ and the $Al\,PO_4$ separately to their respective oxide, there is found:—

$$\begin{aligned} &Fe\,PO_4 \quad 0.219 = 0.1160 \quad Fe_2\,O_3 \\ &Al\,PO_4 \quad 0.054 = 0.0225 \quad Al_2\,O_3 \\ &\left.\begin{matrix} Fe\,PO_4 \\ + \\ Al\,PO_4 \end{matrix}\right\} 0.273 = 0.1385 \left\{\begin{matrix} Fe_2\,O_3 \\ + \\ Al_2\,O_3 \end{matrix}\right. \end{aligned}$$

a higher result than obtained above.

If the composition of the same weight of the combined phosphates of iron and aluminium be assumed to consist of

$$\begin{aligned} Fe\,PO_4 &\ 0.054 \\ Al\,PO_4 &\ 0.219 \\ \hline &\ 0.273 \end{aligned}$$

there will result as before:—

$$\underset{(Fe\,PO_4\,+\,Al\,PO_4)}{0.273} = \underset{(Fe_2\,O_3\,+\,Al_2\,O_3)}{0.131}$$

But,

$$\begin{aligned} &Fe\,PO_4 \quad 0.054 = 0\ 0286 \quad Fe_2\,O_3 \\ &Al\,PO_4 \quad 0.219 = 0.0915 \quad Al_2\,O_3 \\ &\left.\begin{matrix} Fe\,PO_4 \\ + \\ Al\,PO_4 \end{matrix}\right\} 0.273 = 0.1201 \left\{\begin{matrix} Fe_2\,O_3 \\ + \\ Al_2\,O_3 \end{matrix}\right. \end{aligned}$$

a value considerably lower than obtained by the other method of calculation.

Method II. makes a much better showing than the preceding method. The chief objection to it, is the error involved in weighing the iron and the aluminium as phosphates and calculating them to the oxides, as explained above.

This difficulty, however, could be obviated in the following manner:—

Proceed with the analysis exactly as directed, and weigh the iron and the aluminium as phosphates; then dissolve in $H_2\,SO_4$; reduce the iron by means of zinc and platinum in a $H_2\,SO_4$ solution; titrate with standardized $K_2\,Mn_2\,O_8$ solution, and record the iron as $Fe_2\,O_3$; calculate this to iron phosphate, $Fe\,PO_4$; subtract this value from the weight of the combined phosphates, and then calculate the remainder, the $Al\,PO_4$ to $Al_2\,O_3$.

Method III. has certainly yielded the most satisfactory result, for the difference between the amount of the iron and the aluminium oxides present and determined is only 0.04 per cent, a difference corresponding to less than two-tenths of a milligramme in the actual weight of the precipitate, $Fe_2\,O_3 + Al_2\,O_3$, in this experiment.

The feature which serves as the special endorsement of this method is the fact that the constituents sought are reported in the very form in which they are weighed, and that thus the introduction of errors by calculation is excluded.

In order to test the working of these three methods in actual practice they were applied to the analysis of four samples of bone black.

The results obtained follow:—

Sample.	Method I.	Method II.	Method III.
1	0 65	0.47	0.47
2	0.50	0.44	0.54
3	0.58	0.41	0.46
4	0.43	0.36	0.38

OSTEOLOGICAL NOTES.

BY DANIEL DENISON SLADE, M.C.Z., CAMBRIDGE, MASS.

THE jugal arch in the order of the Cetacea presents some singular modifications. In the Delphinoidea, the squamosal, frontal, and jugal enter into its composition. The squamosal sends forward a large, bulky process which nearly meets the descending post-orbital process of the frontal. The jugal is an irregular flat bone, covered by the maxilla, and sends back from its anterior and internal border a long and very slender process, curved slightly downwards, to articulate with the short, obtuse process of the squamosal, thereby forming the lower boundary of the orbit.

So far as the relations of the squamosal and jugal are concerned, the portion of the arch thus formed is a counterpart of that of the horse; although the union of the two bones is much more complete in the latter animal. The jugal in the horse is relatively a much larger bone, and sends back a well-developed process which underlies that of the squamosal, with which it is joined by a nearly horizontal suture, thus forming a strong suborbital bony wall.

In the Delphinoidea, the delicate character of the suborbital process of the jugal, and its union with the squamosal, render it difficult at first sight to determine its relation to the arch, and yet, when compared with that of the horse, its homological character cannot be disputed.

In the Balænoidea, much the same conditions are presented, except that the suborbital process of the jugal is both stronger and more curved. The small capacity of the temporal region, as well as the limited extent of the arch in the Cetacea, are correlated with the modifications presented by the mandible, in which the condylar surface is small, and looks directly backwards. There is no ascending ramus, and the coronoid process is quite rudimentary, — all of which conditions are in direct relation to the nature of the food, and absence of the masticatory movements.

The jugal arch in the Sirenia is enormously developed, being composed of the squamosal and the jugal. The former of these is much thickened and presents upon its external face a smooth convex surface.

In the Manatus, this process of the squamosal rests loosely upon the process of the malar, which, underlying it, extends back as far as the glenoid, having first formed a rim which is both suborbital and post-orbital, besides sending a broad plate downwards and backwards, thereby greatly increasing the vertical breadth. The orbital fossa is separated almost completely from the temporal by a bony partition.

The surface for the muscular attachments, both of the temporal and masseter, are extensive, while the pterygoid plates and groove are relatively enlarged. The vertical curvature of the arch is great, but the horizontal is inconsiderable. The ascending ramus of the mandible is broad, compressed, with rounded angle, and surmounted by an obliquely-placed small convex condyle, much raised above the molar series. The coronary surface is broad, directed forwards, and but slightly elevated above the condyle.

In the Dugong (Halicore), the jugal arch is much less massive; there is no post-orbital process from the jugal, and consequently no separation of the orbital and temporal fossæ by a bony orbit. The coronoid process of the mandible looks backward.

Although the horizontal curvature of the arch is very slight in both genera of the Sirenia, the temporal fossæ are deepened and extended — conditions owing to the walls of the cranium being compressed in a lateral direction, which materially increases the extent of surface for muscular attachment and development.

In the order Edentata, the jugal arch also offers unusual modifications. In the Myrmecophagidæ it is very incomplete, being composed of the proximal end of the jugal, articulating with a narrow projecting process of the maxilla, and a very rudimentary fragment of the squamosal. These separate portions, however, do not meet. In fact, they are widely separated. No boundary exists between the orbital and temporal fossæ, the latter being comparatively shallow. The glenoid fossa is a shallow cavity running antero-posteriorly, and well adapted to the pointed, backward projecting condyles of the mandible, whose long, straight horizontal rami present neither coronoid process nor angle. In Cycloturus, the mandible is somewhat arched, and presents a well-marked angular process, as well as coronoid surface slightly recurved.

In the Bradypodidæ, containing the two species Bradypus and Cholœpus, the arch is imperfect, consisting of the jugal, which is narrow at its articulation with the lacrymal and maxilla, but which, widening out into a broad, compressed plate, terminates posteriorly in two processes, the upper pointing backwards and upwards, while the lower looks downwards and backwards. The straight process of the squamosal, although fairly developed, fails to meet either of those of the jugal. There is a post-orbital process of the frontal, which is best marked in Cholœpus. The glenoid is shallow and narrow from side to side. The mandible, widest in Cholœpus, develops a rounded convex condylar surface, well raised up from the dental series, while the coronoid surface is large and recurved. The rounded angular process projects backwards to a considerable extent. The symphysis in both species is solidified, while in Cholœpus it projects forwards into a spout-like process. The temporal surface for muscular attachment is large, as are also the pterygoid plates.

In the Dasypodidæ, the arch is complete, and in its formation the jugal largely enters. This bone extends from the lacrymal and frontal to the process of the squamosal, the anterior third of which it underlies. There is no post-orbital process of the frontal. The glenoid presents a broad, slightly convex, transverse surface. The pterygoids are small. The mandible has a high ascending ramus, the condyle is transverse and high above the alveoli, while the coronoid surface is large and the angle broad and projecting.

In the Manidæ, the jugal arch is incomplete, owing to the absence of the malar, which, if present would occupy almost the exact centre of the arch,—the length of the squamosal process, and that of the maxillary, being nearly equal on either side. The temporal and orbital fossæ form one depression in the side of the skull. The rami of the mandible are slender and straight and without teeth, angle, or coronoid process. The condyle is not raised above the level of the remainder of the ramus.

In the Orycteropidæ the jugal arch is complete. The horizontal curvature is very slight. The post-orbital process is well developed. The mandible rises high posteriorly, with a coronoid slightly recurved, and with an ascending pointed process on the angular edge below the condyle.

In the Marsupialia, the jugal arch is always complete, and composed of the jugal, resting on the maxilla, and squamosal, the first extending from the lacrymal anteriorly to the glenoid fossa posteriorly, of which it forms the external wall. The process of the squamosal passes above the jugal, being united to it by an almost horizontal suture. The horizontal and vertical curvatures of the arch are considerable, and the space for both temporal and masseter muscular insertions is extensive, and the various ridges and crests are extensive, especially in the families of the Dasyuridæ and Didelphyidæ. The post-orbital of the frontal is present as a rule, although in most forms inconsiderably developed. The ascending ramus of the mandible is less elevated than in several of the orders of the Mammalia. The condyle is but little raised above the molar series. The masseteric fossa is extremely projected at its lower external border. The mandible has, with one exception, an inverted border to the angle.

In the Monetremata, the Echidnidæ possess an arch in which the squamosal is compressed, and sends forward a slender, straight process to join the corresponding slight, shaft-like process of the jugal. The horizontal curvature of the arch is extremely small.

In the Ornithorynchidæ, the arch is made up of the malar resting upon a process of the maxilla, which, passing straight backwards, unites with the squamosal process that rises far back on the sides of the cranium. While the mandible of the Echidna has but the rudiments of the parts which usually enter into its formation, that of the Ornithorynchus is more fully developed, in relation to the attachment of the horny teeth.

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 Mean Distance of the Earth.

THE interlinear readings to Sir Robert S. Ball's "The Course of an Ice Age" which Miss Hayes gives in *Science* for April 28 have been read and studied with much grateful appreciation by some readers of that book who find the higher mathematics rather slippery ground to walk on without help. On behalf of a group of such readers, I wish to say a few words on the interlinear reading given for the first selection from Sir Robert's book.

The passage is: "There can be no doubt that when the eccentricity is at its highest point the earth is, on the whole, rather nearer the sun, because, while the major axis of the ellipse is unaltered. the minor axis is least." Miss H. says: "This is equivalent to saying that the mean distance of the earth from the sun is a function of the eccentricity of the earth's orbit, . . ." and then she proceeds to find an expression for this mean distance, first from the standpoint of geology, and, second, from a consideration of the kinematical element of velocity. The result in the first case is that

$$r' \text{ (mean distance)} = a \sqrt{1-e^2},$$

and in the second case that

$$r' = a \sqrt[4]{1-e^2}.$$

From what is said in introducing the second case it appears as if the kinematical result were only an "as it were" mean distance, and not the actual average of all the different distances. If this were so, this part of the article would scarcely supply an interlinear reading for the passage from Ball, for it seems evident that he means the real average distance and not a virtual average. The geometrical result should give the real average, but does it — I mean does Miss H.'s geometrical result give it? This makes it equal to the semi-axis minor, but that surely cannot be true. Of course, it is quite true, and, as Miss H. says, it is easily shown that

$$\frac{1}{\pi}\int_0^\pi r\,d\theta = a\sqrt{1-e^2},$$

but she does not show how it is shown that the mean distance

$$= \frac{1}{\pi}\int_0^\pi r\,d\theta.$$

As an assumption it does not seem to be convincingly reasonable.

The assumption made in the kinematical discussion seems much more reasonable. It is that the mean distance is the radius of a circle, in the circumference of which a point travels with the same areal velocity as that of the earth in its orbit. If the idea of velocity be dropped, we shall get back from kinematics into geometry, and the same assumption will give us for mean distance the radius of a circle whose area is equal to that of the given e lipse.

Thus $$\pi r_0^2 = \pi a b$$

and $$\therefore\ r_0 = \sqrt{ab} = a\sqrt[4]{1-e^2}.$$

This is the same as Miss H.'s kinematical result, and, like it, agrees with the dynamical result in her equation (4).

ALICE PORTER.

Yarmouth, N.S., May 15.

A Beautiful Spectacle.

I GIVE below a description of a phenomenon seen here on the evening of May 9 and wish you or some of your readers could tell me if it is rare or common, and what is the cause or its relation to other phenomena

On Tuesday evening, May 9, between 9.15 and 9.45 (north latitude 44°, west longitude 66°, but *time* is 60°), we were treated to a curious and beautiful spectacle. Right across the sky from west to east stretched a magnificent arch of luminous radiance. On the west it seemed to spring from a solid mass of black cloud which extended along the whole northwest horizon. Its width was nearly uniform from the western base up beyond the summit, and measured about two degrees. The summit was among the stars of Berenice's Hair, and was 15 to 20 degrees south of the zenith. The eastern branch narrowed as it neared the horizon, and tapered off to a point before quite completing the semi-circle. The color was fairly uniform throughout, and of a grayish or pale-bluish white, some say "yellowish." Except for the cloud-mass in the northwest the sky was beautifully clear, and the brighter stars along each side of the arch seemed to shine out with unusual brilliancy and sparkle. Those covered by the arch were not obscured, but twinkled through it as through a transparent veil. To some observers the summit seemed for a time to move very slowly a little farther south, and near the time of breaking up there were narrow, dark rifts crossing it obliquely; but, on the whole, the entire structure stood remarkably steady, without any of the swaying, or shooting, or shimmering, or wavering motion generally seen in auroras. There had been some auroral outbursts about half an hour earlier, and this phenomenon was probably connected with them. Whatever it was due to, it was a splendid sight — such a sight as the rings of Saturn must be as seen from the surface of that planet — and it was much admired by all who saw it. It broke up and melted away before 10, and in another quarter of an hour the sky was clouded all over.

ALICE PORTER.

Yarmouth, N.S., May 12.

A Fall of Colored Snow.

ON Jan 8 1892, between one and five o'clock P.M., there fell about one inch of colored snow throughout the northern half of La Porte County, Ind.

Mixed with the snow was a large percentage of mineral and vegetable matter giving the snow a reddish-brown hue. Every flake of snow had a particle of this matter, that served as its nucleus, from which the mass became granular. The mass was moist enough to form a crust within twelve hours.

At the time it fell there were six inches of clean snow very evenly distributed over the surface, probably not any surface bare within fifty miles of the above-named area. This old snow was quite compact.

During the next twenty-four hours following the fall of colored snow about four inches of clean snow fell on top of it, and became a crust within a few days, thus embedding the colored snow between two compact strata of ordinary snow, by which it was kept free from contamination for about a month. During that time several persons procured samples of it for examination.

The meteorological conditions at the time of its fall were: Wind from west-southwest; all clouds moved in same direction. Temperature about zero at 8 A.M. Jan. 8, 12 to 3 P.M., rising; 8 P.M., zero. Thermometer stood at zero Jan. 9. At Chicago from 4 until 4 30 there was light snow, too light to measure. At Grand Haven, Mich., it snowed almost continuously from Jan. 5 to 10; and on Jan. 8, thermometer fell from 18 to 8 above zero (the coldest of the season); while at Chicago it went down as low as 5 below from 12 above zero. That station reports a high-pressure area for the whole northwest country, weather cold and clear. This area closely followed an area of low-pressure, which was central over Upper Lake Michigan during the morning of Jan. 8, moving rapidly northwestward during the succeeding twenty-four hours, general snow marking its passage. The Chicago observing station records wind from west to northwest Jan. 8–9.

Having had my attention called to some of these facts by an article in a local newspaper by Honorable G. H. Teeter of Rolling Prairie, Ind., I began to collect samples, and procured one from that gentleman. I sought to make a survey of the area covered by its fall, but was unable to locate bounds in any direction, although I traced it over an area 25 by 45 miles.

To avoid uncertainty in an analysis of the matter, I drove several miles into the country with Professor F. M. Watters, then science teacher in La Porte High School, to procure samples of it that should not be affected by dust from chimneys and railroads.

I made three analyses of it, besides carefully examining it under the microscope, using both low and high powers. Meanwhile, Mr. Teeter procured an analysis by Professor H. A. Huston, chemist of Indiana Agricultural Experiment Station, at Purdue University, Lafayette, Ind., as follows: —

"Loss on ignition (water and other volatile matter)	15.04
Silica	65.64
Alumina and oxide of iron	15 50
Lime	2.19
Magnesia	1.38
Phosphoric anhydride	.10
Oxide of titanium and undetermined	.15
Total	100.00"

Professor Huston adds: "The composition of the material is such that one is led to believe it to be of volcanic origin, as it approximates very closely to some of the analyses of lava from the

Pacific islands, and from Iceland. I am, therefore, inclined to believe it is a volcanic product."

My analyses approximated Professor Huston's very closely, though I found mica scales, a trace of sulphur, in one analysis, with nearly one per cent vegetable matter in my first sample analyzed (this one procured from Mr. Teeter).

In precipitating the matter by melting the snow, the heavier portions fell to the bottom, and unless care was used the larger portion of the vegetable matter would be lost through being poured off. I found the coarser grains of silica (white sand) to be water-worn and scratched. Lime particles adhered to the sand-grains, just as one finds on the shores of lakes or rivers. Of the vegetable matter, I found the seed of a wild pea (Lathysus ochrolencus), growing abundantly all over the Northwest. This seed, to make sure of no mistake I planted and grew the vine to maturity. Among wood-fibres identified were poplar (Populus tremuloides), pine (Pinus strobus), and casex (Casex tenella).

Now as to the source of this matter. It is plainly terrestrial; and, as the whole area traversed by the winds that carried it were covered with snow at the time, it is evident that it could not have been raised east of Lake Michigan. Its constituent elements preclude all possibility of its being meteoric or volcanic matter.

The fact that the sample analyzed by Professor Huston closely approximates certain volcanic samples can easily be accounted for on the ground that the precipitated mass was not homogeneous, and what was sent him could only have represented a portion of the mass, as another portion of it, sent to me by Mr. Teeter, out of the same lot, contained one per cent of vegetable matter, mica scales, and three small copper pyrites (yielding sulphur on ignition).

Every element of this matter is met with in abundance throughout all portions of the Northwest, and nowhere else do we find all of them on the surface. I conclude that this volume of matter must have been raised somewhere northwest, being carried southeastward until it encountered the area of high-pressure that extended north of Chicago, and deflected in its course and fell within the area mentioned above.

Can anyone throw more light on the subject?

A. N. SOMERS.

La Porte, Ind., March 21.

The Aurora.

I HAD thought that no matter what Professor Ashe might say in regard to my note printed in *Science* for April 28, I would refrain from further comment. Inasmuch, however, as he in effect demands that something further be said, as appears in the closing paragraph of his note printed in *Science* for May 19, p. 277, I presume that I have no option but to comply. The point to which he asks special attention is as to the element of "chance" affecting the conclusions at which I have arrived respecting the location upon the sun of the seat of the activities originating the aurora in any given instance. The manner in which he puts this inquiry, as well as the general drift of his criticism, shows that I have failed to make myself understood in spite of very persistent efforts in the various notes and papers which he mentions, and which certainly, therefore, must have been taken into consideration in the comments made in the letter above mentioned. This being the case it will be necessary to begin at the beginning and state the heads of the argument by which my conclusions have been reached, so that if there is any flaw in the reasoning its precise location may appear and so that it may be explained also once again what are the precise conclusions for which I have been contending. The substance of the argument, stated in a few propositions as briefly as possible, is as follows: The agreement between the curves, representing the frequency of auroras, magnetic storms and sunspots is exact, and the nature of these phenomena is such that there can be no doubt whatever that the aurora owes its origin to a special form of solar activity. This proposition can be controverted successfully only by denying that there is such agreement as is claimed of the curves mentioned, or by advancing some alternative explanation of their connection with each other which will leave solar activities out of the question. Until this is done, this proposition must stand, the evidence in its favor being adequate and there being no evidence pointing in a different direction. The solar origin of the aurora being thus established, its manifest periodicity at intervals of $27\frac{1}{4}$ days must be explained in accordance with its solar origin. If this can be done, the proof of such origin will incidentally be greatly strengthened. Now this period is totally indistinguishable from that of a synodic revolution of the sun — giving every evidence of being absolutely the same. This being the case we are able to formulate proposition number two to the effect that there is a periodicity of the aurora corresponding to the time of the rotation of the sun as seen from the earth. Here again the evidence is adequate and there is no evidence pointing to any other possible explanation. These two propositions being established there follows another, from which there is in the very nature of the case no possibility whatever of escape, and which is to the effect that whatever it is upon the sun which is capable of producing the aurora, it has this power during a very limited portion only of each revolution, which portion always remains the same during succeeding revolutions relative to the position of the earth in its orbit, otherwise the periodicity described could not exist. It remains only to identify the point whence the auroral effect proceeds. The period of auroral recurrence and that required for the completion of a synodic revolution of the sun as determined from the average rate of motion of spots being identical, there is no other way than to study the appearance of the sun at times of auroral recurrence in order to learn whether such recurrence is attended by any characteristic features. Thus it is found that no matter what appears elsewhere on the sun at such times there are always at the eastern limb areas on which spots are frequent and persistent. Thus the evidence is adequate that there must be something in that location in such cases which is responsible both for the sunspots and the aurora, and there is no evidence pointing in any different direction. On the contrary, the manner in which magnetic storms begin and the exactness of the periodicity manifest in their times of beginning are such as are totally inconsistent with any other explanation than that the originating impulse is brought to bear by coming into range suddenly around the sun's limb. But be this as it may, such behavior corresponds precisely with what is known in regard to the operation of electro-magnetic induction in which very precise arrangements of lines of force and development of poles in certain directions in the case of rotating bodies, or otherwise, are the rule, and there is no correspondence whatever to the mode of action of any other force of which we have knowledge. Thus at no point throughout the research, as above outlined, has there appeared to be even the slightest "chance" for an alternative hypothesis. The evidence in favor of each proposition stated has been adequate and all in one direction, and moreover, taken together it is cumulative; each point strengthening the others and nowhere developing any inconsistencies. Professor Ashe is mistaken in stating that there has been "no attempted refutation." I have letters and articles by the score from persons who started in with vehemence, some of them many years ago, but who have gradually become very respectful, finally being brought to a realizing sense, that it is facts and not a personality against which they had been contending.

M. A. VEEDER.

Worms in the Brain of a Bird.

APRIL 7th, 1890, two common Bitterns (*Boturus mugitans*) were brought to me to be mounted. One of them was still alive but did not seem to be just natural, seemed to lack what we might call bird intelligence, and was smaller than the other and poor in flesh. This bird was given to one of my pupils in taxidermy, Miss Bernice Pike, to mount. When the head had been skinned and was ready to sever from the neck, which was done by cutting through the skull, the brain-cavity was found to contain a mass of thread-worms, occupying about one-third of the brain cavity. These were seemingly like the ordinary Gordius or Hair Snake, about the size of a Gordius that is three inches long, and coiled in a mass in the upper posterior part of the brain, and extending some down into the spinal canal. As near as I could say without removing them, they occupied the subarachnoid space, and had absorbed much of the cerebrum and the upper part of the cerebellum, the outlines of the Arbor Vitae being plainly visible. I have never seen anything like this in any other bird, nor have I seen any account of worms being found parasitic in the brain before. G. H. FRENCH.

Carbondale, Ill.

Epidemic Forms of Mental or Nervous Diseases or Disorders.

IN response to the inquiry concerning "epidemic forms of mental or nervous diseases or disorders," in the issue of May 19, I send the following account of an incident which "came to pass" under my own eyes. Several years ago our next-door neighbor's little girl, perhaps five or six years old, met with an accident which rendered it necessary that she use a crutch. Another little girl of about the same age, who lived in the adjoining house, seeing the little lame girl with her crutch, obtained a stick which she used as a crutch, hopping and limping, just as she saw the little lame girl doing. At first no attention was paid to this childish fancy, this imitation, this "playing being lame." After some days had elapsed, however, and this play became so constant as to be annoying, the stick was taken away, and the little girl told to put her foot to the floor. She screamed and cried and insisted most strenuously that she could not put her foot to the floor: she could not stand upon it, etc. I cannot say how long she persisted in thinking she was lame, but shall never forget how real her apparent affliction was to her, nor her screams of pain when she declared she could not stand without her "crutch" MRS. W. A. KELLERMAN.

Columbus, O.

The Winter of 17.9.

"IN the famous winter of 1709 thousands of families perished in their houses, the Arabic Sea was frozen over, and even the Mediterranean."

The above is found in a foot-note on page 39 of Professor Meech's article on the intensity of the heat and light of the sun at different latitudes, published in one of the "Smithsonian Contributions to Knowledge." It seems incredible The "Arabic Sea," I take it, is what we call the Arabian Sea, or at least some one of the bodies of water which border on the Arabian peninsula. No information is given as to where the "thousands of families perished with cold in their houses."

The Arabian Sea measures about 500 miles across its narrowest part. Can it be possible that it was frozen over? or the Red Sea? or the Persian Gulf? or the Gulf of Oman? Has the Mediterranean Sea been frozen over either in historic, or even in glacial times?

The statement in Professor Meech's paper is made unequivocally, as if speaking of a well-known and well-established fact. It is not put in quotation marks, nor is any authority cited.

A few weeks ago I wrote to Professor Langley, secretary of the Smithsonian, asking for any information which he might be able to give me To-day I received the following reply:—

"I beg to say that I do not know the original source from which his particular statements were derived, but that the winter in question was one of exceptional severity is fully attested by well-authenticated records.

"Under the article 'Temperatur,' in Gehler's 'Physikalisches Wörterbuch,' it is stated that at Paris the thermometer fell many times to $-19°$ C.; that in the Kleiner Belt of the Danish seas the ice extended so far from the coast that the end of it could not be seen from the highest towers; and that the cold must have been especially intense in southern Germany and Italy, since the Adriatic Sea was wholly covered with ice.

"Additional records of this severe winter will probably be found in the memoirs of E. Brückner, who has been making an extended study of variations in climate."

There is nothing in the fact that at Paris the thermometer fell to $-19°$ C., or $3\frac{1}{2}°$ below our zero, nor that in the Danish waters ice formed far out from the coast to compare in any way with the freezing over of "the Arabic Sea or the Mediterranean!"

The fact—if it be a fact—that the Adriatic Sea was wholly covered with ice would indeed be very remarkable. but even that was a small affair (indicating a moderate climatic aberration) in comparison with a temperature so abnormally low as to freeze over so large a body as the Mediterranean, or one so large and so exceptionally warm as the Arabic Sea.

Can any reader of *Science*, or any one else, throw any light on this subject? C. B. WARRING.

Animal Effigies.

CAN you or any of your readers furnish me with a reference or references as to large numbers of small animal effigies of pottery found together in any mound of the United States?

C. B. M.

BOOK-REVIEWS.

The Moon's Face: A Study of the Origin of its Features. By G. K. GILBERT. 52 p. Washington, April, 1893.

THE present paper, although delivered as the presidential address before the Philosophical Society of Washington last December, has only recently been distributed in its complete form. Mr. Gilbert is well known as a geologist and a student of topographic form, and in this paper he has carried his studies away from things terrestrial and turned his eyes and his attention for a time to things celestial. The observations upon which the paper is based were made during three months of the past year, eighteen nights being available for the work, and the $26\frac{1}{2}$-inch refractor of the U. S. Naval Observatory being the instrument employed. Numerous laboratory experiments were also carried on, and the literature relating to lunar topics was searched. The craters, as the most conspicuous features of the moon's face, are mainly dealt with in the paper, and after a description of their characteristics and a statement of the various theories advanced to account for them, the author advances a theory of his own. The volcanic theory is one held by many writers, but a comparison of terrestrial and lunar craters, even when the differences in condition are considered, led Mr. Gilbert to reject the hypothesis as untenable. The "bubble" theory, advocated by Robert Hooke in his *Micrographia*, in 1667, is mentioned, but as Mr. Gilbert had not seen the book the theory is not discussed in any detail. It may not be amiss to devote a few words to it here.

Hooke describes the features of the craters as he saw them through his telescope, and gives an illustration of some of them. Except as regards detail and the characteristic central hill shown in Mr. Gilbert's figures, those given by Hooke are very similar. In describing the craters he says: "These seem to me to have been the effects of some motions within the body of the moon, analogous to our earthquakes, by the eruption of which, as it has thrown up a brim or ridge round about higher than the ambient surface of the moon, so has it left a hole or depression in the middle. proportionally lower." He also mentions, what is of more interest, that he had made several experiments to ascertain, if possible, the origin of the pits. "The first was with a very soft and well-tempered mixture of tobacco-pipe clay and water, into which, if I let fall any heavy body, as a bullet, it would throw up a mixture round the place, which for a while would make a representation not unlike these of the moon, but considering the state and condition of the moon, there seems not any probability to imagine that it should proceed from any cause analogous to this; for it would be difficult to imagine whence those bodies should come; and next, how the substance of the moon should be so soft; but if a bubble be blown under the surface of it, and suffered to rise and break; or if a bullet or other body sunk in it be pulled out from it, these departing bodies leave an impression on the surface of the mixture exactly like those of the moon, save that these also quickly subside and vanish. But the second and most notable representation was what I observed in a pot of boiling alabaster, for then that powder being by the eruption of vapors reduced to a kind of fluid consistence, if, whilst it boils, it be gently removed beside the fire, the alabaster presently ceasing to boil, the whole surface, especially that where some of the last bubbles have risen, will appear all over covered with small pits exactly shaped like these of the moon, and by holding a lighted candle in a large dark room, in divers positions to this surface, you may exactly represent all the phenomena of these pits in the moon, according as they are more or less inlighted by the sun." He then goes on to advocate the second theory, and concludes finally that the craters had their origin similar to those formed in the alabaster.

A "tidal" theory, which supposes a time when a thin crust concealed a liquid beneath, which was moved by the action of tides in such a manner as to produce craters, is also examined and rejected by Mr. Gilbert. So also is a "snow" theory, and then are considered the "meteoric" theories, which suppose the pits to have been caused in some way by the impact of extra-lunar bodies. As we have seen, this theory was considered and rejected by Hooke in 1667, but others have not seen the same difficulties that he did. Mr. Gilbert advances the following theory:

"It is my hypothesis that before our moon came into existence the earth was surrounded by a ring similar to the Saturnian ring: that the small bodies constituting this ring afterward gradually coalesced, gathering first around a large number of nuclei, and finally all uniting in a single sphere — the moon. Under this hypothesis the lunar craters are the scars produced by the collision of those minor aggregations, or moonlets, which last surrendered their individuality."

This hypothesis was tested in numerous ways, and it was found

CALENDAR OF SOCIETIES.

Philosophical Society, Washington.

May 27. — S. P. Langley, On Recent Observations in the Infra-red Spectrum; G. K. Gilbert, The Average Temperature of the Earth; Cleveland Abbe, The Formation of Rain.

Chemical Society, Washington.

Apr. 13.— Subject for discussion: Organization as a Section of the American Chemical Society; G. L. Spencer, A New Drying Oven. The walls of the oven are made double and the space between them filled with a non-conducting substance. The bottom of the oven is also made double, the outer wall being made of Russia iron and the inner of copper. The space between is filled with air This oven has lately been devised in Dr. Peale's laboratory by Dr. G. L. Spencer. The drying bulbs are made in the shape of a flask with rounded bottom. The content of the flask varies from 150 to 200 cubic centimetres. From six to eight of these drying flasks are connected *en batterie* with the pump. If a current of hydrogen is to be introduced into the drying flask, it is easily accomplished by passing a very small glass tube through the cork, joined to another tube by a rubber connection immediately below the cork. The inner tube should pass nearly to the bottom of the flask, passing through a wash bottle containing caustic soda, and then through a sulfuric acid bulb. The speed of the current, which need not be very great, is controlled by a stop or pinch-cock. Any of the sample which may touch the inner tube during the intumesence, caused by desiccation, remains thereon and is weighed at the end with the tube, which is detached and left in the drying bulb. H. W. Wiley, A New Lamp for Securing a Constant Monochromatic Flame. This lamp was devised to secure a constant uniform coloration for polarimetric observations. It consists essentially of two wheels with platinum gauze perimetres and spokes, driven by a clock-work and mounted as shown in the figure. The sodium salt, chlorid or bromid, is saturated in solution, is placed on the porcelain crucibles to such a depth that the rims of the platinum wheels dip beneath the surface as they revolve. By means of the crossed bands the wheels are made to revolve in opposite directions, as indicated by the arrows. The solution of the salt, which is taken up by the platinum net-work of the rim of the wheel, thus has time to become perfectly dry before it enters the flame, and the sputtering, which a moist salt would produce, is avoided. At every instant, by this arrangement, a minute fresh portion of salt is introduced into the flame, with the result of making a perfectly uniform light, which can be used for hours without any perceptible variation. The polariscope should be so directed toward the flame as to bring into the field of vision its most luminous part. The platinum wheels are adjustable, and should be so arranged as to produce between them an unbroken yellow flame. H. B. McDonnell, A Filter for Fine Precipitates. To prepare the ordinary Gooch filter for the retention of fine precipitates, the writer adds a little powdered asbestos on top of the ordinary asbestos felt in the bottom of the perforated crucible. The fine asbestos can be purchased from dealers in chemicals, and should be an impalpable powder. It is prepared for use by treating with acid, to remove all soluble matter, and washed a few times by decantation. It is kept in water, in which it is suspended, by agitation, just before use. A filter prepared in this manner will perfectly retain barium sulphate, even when it is precipitated cold and filtered at once.

May 11. — The society amended the constitution and by-laws so as to conform it to the requirements of the constitution of the American Chemical Society, in order to become a local section of that society. Oma Carr, The Predominant Organic Acid in Acid Juices. A tenacious, difficultly soluble incrustation forming upon the tubes of the multiple-effect evaporator at the Medicine Lodge Sugar Works, Medicine Lodge, Kansas, was examined, first with regard to the practical problems connected with its removal, and, second, with regard to its composition, particularly the organic acid in combination with the magnesium and calcium of the scale. The incrustation contained a high percentage of organic matter — 54.4 per cent, of which 43.5 per cent was carbon, or 23.7 per cent on the original material. Sulfuric and acetic acid digestions of the scale were made, the magnesium salt of the prevalent organic acid isolated and converted into a repeatedly purified lead salt. Combustion of these salts rendered results concordant with the theoretical composition of tri-plumbic citrate. Aqueous solution of the acid gave reactions confirmatory of the combustions. Inasmuch as the scale may be taken as an index of the predominant acid combined with the magnesium of the scale, the assumption is plausible that the predominant acid is citric, and not aconitic, as has been commonly supposed. H. W. Wiley, On the Estimation of Levulose in Honey. The principal methods of estimating levulose in the mixtures heretofore practised are those which consist in the destruction of some of the ingredients in the mixture and the estimation of the remaining one, or the method of Wiechmann, which consists in the estimation of the polarizing and reducing power before and after inversion. Neither of these methods can be applied to honey, which contains other optically active bodies besides cane-sugar, levulose, and dextrose. The method presented rests on the principle of the change in the specific rotatory power of honey, due to temperature; the other optically active bodies present remaining practically unchanged, as far as specific rotatory power is concerned, with changes of temperature. Polarizations of many samples of honey were made at intervals of 10 from 0 to 88. The temperature at 88 was chosen as the maximum temperature, because at that temperature a pure invert sugar, composed of equal parts of levulose and dextrose, becomes optically inactive. In other words, the specific rotatory power of levulose at 88 is the same as that of dextrose. A chart was shown giving a graphic representation of the changes in rotatory power, due to temperature. The chart shows that from 20 to 88 the changes are practically equal for either increments or decrements of temperature. From 20 to 0 there is a slight curve, showing a small deficiency in rotatory power at 0 from that which would be calculated from the rate of change from 88 to 20. A table was shown giving the results of the calculation of the per cents of levulose in various samples of honey by this method, which were very satisfactory.

to agree with most of the features of the moon. We cannot, however, enter into further details or explanations that are given of various other objects, interesting though they be. We can only quote one of the concluding paragraphs, as this gives some idea of the difference in conclusions which result from the study when compared with those of other authors. He says: "This sketch of the life of our nearest neighbor has but little in common with the accounts of other biographers. To her has been ascribed a fiery youth, after the manner of the sun, a middle life of dissipation, like Jupiter and Saturn, a hardening and wrinkling old age, toward which the earth is tending, and finally, the end of change — death. If the record of her scarred face has now been read aright, all that remains of the old narrative is the *denouement*: the moon is dead." JOSEPH F. JAMES.

The Mineral Industry. Its Statistics, Technology, and Trade, in the United States and other countries from the earliest times to the end of 1892. Vol. I. Edited by Richard P. Rothwell. New York, The Scientific Publishing Company. 1893. 628 p.

IN the years of 1874–75 and '76 *The Engineering and Mining Journal* of New York published the first complete reports of the coal production of the United States, and in 1889 as special government agent for the census, the editor of the journal, Mr. Richard P. Rothwell, collected the statistics of gold and silver. The scope was gradually extended until in January, 1892, a magnificent volume of statistics was given to the world and universal encomium heaped upon the journal and its staff for their wonderful work. Indeed such was the unstinted praise accorded it we can but wonder what language will be used for the present volume, no longer a supplementary number in journal form, but a handsome library volume of 628 pages. It is the intention to make this the first of a series to embrace within a few years the statistics and technology of the mineral interests of the world, in many cases going back historically to the earliest times and always carried down to the date of publication. Such a vast undertaking has heretofore been considered impracticable for private enterprise and has been relegated to the unlimited resources of governments, it is worthy to note, however, that simultaneously with the issue of this volume containing all statistics accurately and systematically recorded for 1892, there appears the government publication of similar character for the year 1891. The introduction to this latter volume, by the way, speaks of "the impossibility of concluding a complete canvas of the products of huge industries like coal, iron ores, and building stone without a considerable delay after the close of the year reviewed," and yet the *Engineering and Mining Journal*, depending entirely upon personal courtesy and confidence for its success, has accomplished this feat so impossible to the expensive machinery of government. The journal is fortunate in possessing a large and carefully trained staff, and in being in communication with experts in all branches of industry the world over, but more than this is needed, and much praise is due to all connected with the enterprise. Especially is praise due to Mrs. Sophia Braeunlich, that able financier and business manager of *The Engineering and Mining Journal*, and to Mr. Richard P. Rothwell, editor of both journal and "statistics."

To attempt even a running review of this work would be out of the question, the table of contents alone occupying ten pages of small print. Suffice it to say that without exception the articles therein contained are written by men pre-eminently fitted for their best treatment, and in all cases by experts in each particular line. Among the contributors are: Dr. George Lunge, Dr. Francis Wyatt, author of "The Phosphates of America"; E. O. Leech, Director of the U. S. Mint; Professor J. F. Kemp, George F. Kunz, J. Langeloth, Dr. Thomas M. Chatard, Richard E. Chism, H. O. Hofman, Emile Delecroix, and many others of equal fame. They have been well chosen, and we congratulate them upon the part they have played in this most admirable work. C. P,

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Exchanges

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

The Rev. A. C. Waghorne, New Harbor, Newfoundland, wishes to sell collections of Newfoundland and Labrador plants, all named by competent botanists. He is going on a missionary journey along the Labrador coast, from the middle of July till October, and in return for much needed aid towards (Episcopal) Church purposes in that region, will be glad to be of service to any botanists who may write to him. Letters posted in the U. S. up to July 1 will reach him at the above address, and if posted later will be forwarded.

For sale.—J. D. Dana's Report on Crustacea of the U. S. Exploring Expedition under Charles Wilkes. Text and plates well bound in three volumes, half morocco, $75. Samuel Henshaw, Boston Society of Natural History, Boston, Mass.

For exchange—I wish to exchange cabinet skins of Californian birds or mammals for any book on the following list, books if second-hand to be in good order. Manual of Vertebrates, fifth edition, D. S. Jordan; Nests and Eggs of North American Birds, Oliver Davie; Marine Mammals of the West Coast of North America, C. M. Scammon; The United States and Mexican Boundary Survey, Vol. II., Zoology, S. F. Baird. F. Stephens, Witch Creek, San Diego Co., Cal.

Minerals for exchange—John Holl. Rollo, Wilmington, Delaware.

For sale or exchange.—Johnson's Universal Cyclopædia, 8 vols., ed. 1888. Binding, half-morocco. Will sell cheap for cash or would exchange for typewriter. Address W. J. McKom, Mason, Mich.

I have 500 microscopic slides to exchange in lots to suit. Want Kodak, first-class field-glass or scientific books. A. C. Gruhlke, Waterloo, Ind.

Texas Plants. I will collect sets of plants represented in this region of Texas, either for sale or for exchange for literature, for anyone who will send me an order in time to fill it. Plants to be ready complete in November. C. F. Maxwell, Box 174, Dublin, Texas.

For sale or exchange—A Telescope (36 diameters, copper barrel)—for $20 cash or scientific books of that value. A. N. Somers, La Porte, Ind.

For sale—A complete set of the Reports of the Second Geological Survey of Pa., 1874–1893, including the Grand Atlas. Publisher's price over $115. Will sell for $50. Address F. D. Chester, Newark, Del.

Wants

WANTED. — Second-hand copy of Ehrenberg's Radiolaria, Berlin. 1875. Selected diatom slides, cash or both in exchange. D. C. Lewis, M.D. Skaneateles, N. Y.

WANTED, as principal of a flourishing technical school, a gentleman of education and experience who will be capable of supervising both mechanical and common school instruction. Special familiarity with some technical branch desirable. Address, giving age, qualifications, etc., J. B. Bloomingdale, Fifty-ninth street and Third avenue, N. Y.

WANTED.—A young man as assistant in our microscopical department. Queen & Co., Philadelphia.

THE undersigned desires specimens of North American Gallinae in the flesh for the study of their pterylosis. These species are especially desired: *Colinus ridgwayi, cyrtonyx montezumae, deudragapus franklini, lagopus welchi, tympanuchus cupido* and *pedioecetes phasianellus*. Any persons having alcoholic specimens which they are willing to loan or who can obtain specimens of any of the above are requested to communicate with Hubert Lyman Clark, 3922 Fifth Avenue, Pittsburgh, Pa.

A COMPETENT TEACHER of botany in college or university is open to engagement. Address L., Box 86, Rochester, Mich.

CAN any one inform me as to the age to which cats have lived? I have one twenty years old. Edward D. Webb, 132 W. Eighty-first St., New York.

WANTED — Second-hand. Foster's Physiology, Balfour's Comparative Embryology, Claus & Sedgwick's Zoology, Flower's Osteology of Mammalia, Vine's Physiology of Plants. Please state editions and prices asked and address Richard Lees Brampton, Ontario, Canada

A GRADUATE ENGINEER will give instruction evenings in geometry, trigonometry and surveying, mechanics, physics, mechanical drawing and general engineering construction. Five years' experience in field and editorial work on engineering journal. References furnished. C. S. H., 102 Tribune Building, New York.

SCIENCE

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SCIENCE

NEW YORK, JUNE 9, 1893.

NOTE ON THE "AGE OF THE EARTH."[1]

BY W. J. MCGEE, U. S. GEOLOGICAL SURVEY, WASHINGTON, D.C.

ROUGHLY classified, there are four principal ways of estimating the duration of geologic time, of which two are geologic and two non-geologic. The first of these is based on sedimentation, and the second on erosion; the third is based on terrestrial temperature and supposed rate of cooling, and may be designated physical; the fourth method rests on inferences as to the cooling of the sun and other cosmic changes and conditions, and may be called astronomical.

The sedimentation estimate, as commonly applied, depends on an erosion estimate for its unit; usually it is assumed that the rate of degradation of the land is a foot in 3,000 to 7,000 years, and that in the long run the rate of sedimentation on the sea-bottoms of the globe is the same. The unit rate commonly accepted is that determined by Humphreys and Abbot from measurements of the matter transported by the Mississippi River, or one foot in 6,000 years = 1 mile in about 30,000,000 years. In earlier decades the aggregate thickness of sediments was usually placed at a few miles only, but probably no modern geologist acquainted with the results of researches in the Algonkian rocks of the Lake Superior and other regions would venture to estimate the total thickness at less than 50 miles; and this rate and thickness of sediments would indicate a period of 1,500,000,000 years for the deposition of the stratified rocks of the earth. It is probable that geologic process was more active in the earlier ages than at present; on the other hand, the deposition of the stratified rocks represents only the closing episode in the history of the earth — ages must have been required for the antecedent cooling and encrusting of the planet before the transfer of materials by hydric agency began.

Until recently the erosion estimate has seldom been applied, except as a unit for the sedimentation estimate; and even now it is applicable only to the later eons without the introduction of so many unknown quantities as to vitiate its results. Perhaps the most favorably conditioned region for the use of this method thus far studied is that found in eastern United States. Here rate-units have been determined from the measurements of recession of Niagara Falls and other cataracts, and these have been corroborated by measurements of the recession of Saint Anthony and other cataracts in the Mississippi Valley. These rate-measures and the measures of the volume of material removed from the gorges since the disappearance of the latest ice-sheet of the Pleistocene yield a fairly consistent chronometer for the post-glacial period, giving a value ranging from a few thousand to as many as 50,000 years. Toward the margin of the glaciated area in eastern United States there is another series of gorges, of which that of the Potomac, between Great Falls and Georgetown, may be taken as the type, representing erosion since the close of the Columbia period, or since the end of the first ice-invasion of the Pleistocene. A number of these gorges have been studied by Chamberlin, Salisbury, Gilbert, and others, as well as by the writer; and all geologists familiar with them are agreed that, if the post-glacial erosion is represented by unity, the post-Columbia erosion must be represented by two figures. These post-Columbia gorges themselves are excavated in the bottoms of many times longer and wider gorges recently shown to have been cut since the deposition of the Lafayette formation — e.g., the post-Columbia gorge of the Potomac is not over 12 miles long, a quarter of a mile in mean width, and 100 feet in mean depth, or $\frac{1}{17}$ of a cubic mile in content; while the post-Lafayette gorge of the upper river with its principal tributaries may fairly be put at 600 miles in aggregate length, 1 mile in mean width, and 250 feet in mean depth, or 30 cubic miles in content, or more than 500 times greater than the post-Columbia gorge.

The recent estimates for the post-glacial period derived from the Niagara and other gorges are shorter than of old, ranging from 5,000 to 10,000 years; it is not conservative to estimate the post-Columbia gorges at less than 20 or 30 times as old (assuming erosion to be uniform), or, say, 200,000 years. Now the post-Lafayette period is represented not only by the gorges trenching the Piedmont and Appalachian regions, but also by the widespread ravining and hill-sculpturing of the pre-Lafayette base-level peneplain of these regions, by great estuaries hundreds of feet deep and scores of miles broad traversing the coastal plain, and by the entire removal of two-thirds of the volume of the Lafayette formation (nearly all along and north of the Potomac) and extensive degradation of subjacent rocks, i.e., by erosion fully 500 times greater in the gorges and many thousand times greater over the general surface than the post-Columbia erosion; and it is accordingly hardly conservative to estimate this period at less that 20, 50, or 100 times longer than the post-Columbia period, or say 5,000,000 or 10,000,000 years. In this estimate allowance is made for the discrepancy between the figures based directly on relative erosion and those derived from the Humphreys and Abbot coefficient (1,000,000 or 2,000,000 years for the 200 or 300 feet of post-Lafayette degradation). It is, of course, to be remembered that erosion, *per se*, does not give a trustworthy time-measure, since stream-work is a function of declivity rather than time; but in eastern United States the physiographic conditions affecting stream-declivity have varied so little as to render the erosion-rate here exceptionally uniform.

In brief, while the erosion estimate of geologic time is subject to a large probable error, even to a considerable "factor of safety," the phenomena of eastern United States indicate an enormous lapse of time, probably reaching into millions of years, since the deposition of the late Neocene-Lafayette formation; yet this is one of the latest episodes in the development of the continent.

Combining the erosion estimate and the sedimentation estimate by employing the former so far as applicable and then using sediment ratios beyond, with a "factor of safety" beginning at 4 for the last and shortest period and raised to successively higher powers with each successive period and age counted backward into the past, the following values are obtained[2]:—

Period or Age.	Mean Estimate.	"F. of S."	Minimum Estimate.	Maximum Estimate.
Post-glacial period...	7,000	4	1,175	28,000
Post-Columbia.......	200,000	16	12,5 0	3,2 0,000
Post-Lafayette....	10,000,000	64	156,000	640,000,000
Cenozoic (including Lafayette)........	90,000,000	64	1,406,000	5,760,000,000
Mesozoic	300,000,000	256	1,172,0 0	76,800,000,000
Paleozoic............	2,400,000,000	1024	2,343,000	2,457,600,000,000
Age of the Earth.....	6,000,000,000		10,000,000	5,000,000,000,000

[1] The meeting of the Geological Society of Washington, held April 12, 1893, was devoted to a symposium on the age of the earth, based chiefly on the recent article on that subject by Mr. Clarence King (Amer. Jour. Sci., vol. xlv., 1893, pp. 1-20); and these paragraphs are a revised abstract of remarks made on that occasion.

[2] American Anthropologist, vol. v., 1892, pp. 329-340. Through a simple and evident arithmetic error in this paper, Dana's ratios of 1, 3, and 12 for the Cenozoic, Mesozoic, and post-Cambrian Paleozoic are computed as 1, 3, and 36.

These general estimates are indefinite, and the minima, mean, and maxima are alike unworthy of final acceptance; but they stand for a real problem and not a merely ideal one, and represent actual conditions of the known earth; and, so far as the science of geology is concerned, the maximum estimate is quite as probable as the minimum, while the mean is much more probable than either.

As commonly made, the physical and astronomical estimates of the age of the earth are based on the assumption that the planet is (1) homogeneous, and (2) simple in structure. Thus the cooling of the earth would appear to be assumed analogous to that of a heated spheroid immersed in an ocean, and cooling at a rate determined by relative temperatures of spheroid and water, i.e., at a progressively decreasing rate. Now the actual planet is (1) heterogeneous and (2) complex in structure; and it may be questioned whether sufficient allowance is made for these facts in the non-geologic estimates.

By reason of terrestrial heterogeneity, the temperature of the earth's surface is not directly dependent on the relative temperatures of the terrestrial interior and surrounding space, but is chiefly determined by a complex and wonderfully efficient mechanism for collecting and conserving solar heat, in which the atmosphere and the liquid envelope play important *rôles*. Most geologists and physicists are of opinion that glacial periods might be explained by geographic changes, and hesitate to adopt such a theory only because of the dearth of positive evidence, or the existence of negative evidence, of such changes; and it is commonly recognized that, other conditions of sun and earth remaining unchanged, the earth might be materially chilled or warmed if the land and sea were disposed in zonal or meridional belts in such manner as to cut off or facilitate aqueous and aërial circulation. There is, indeed, reason for supposing that if the earth with its present mean interior temperature were divested of its heat-conserving mantles of air and water it would become a frozen planet. Thus, whatever may have been the case in the pre-geologic stages of planetary development, the present temperature of the external earth, and so its rate of cooling, depends on the sun rather than on the proper heat of the planet; and if (as is probable) the aggregate quantity of air and water enveloping the planet is diminishing, the efficiency of the terrestrial mechanism for conserving solar energy must have been even greater during the earlier ages of geologic development than now.

Again, the earth is complex in chemic constitution, and, moreover, it is probable, if not certain, that this complexity is correlated with temperature. If the course of terrestrial development, as commonly recognized, could be reversed for a time, the constitution of the earth-crust would be materially modified; as the temperature rose through a few degrees, the oxidation and fermentation of certain substances would doubtless be accelerated; with a few dozen degrees increase, life would be destroyed and the highly complex compounds manifesting that form of energy would be broken up; with a few hundred degrees rise, the coals would be consumed and the carbonaceous shales and limestones would be transformed, and these changes would be accompanied by profuse development of energy in the form of heat; and with a few thousand degrees increase in temperature, most of the compounds of the earth-crust would be modified or destroyed and the elements separated or re-combined in simpler forms. Consideration of the effects which would necessarily follow reversing the mechanism of planetary development indicates that the history of planetary growth is one of chemic differentiation coupled with molecular degradation, in which at least such molecular undulations as those of light and heat have progressively decreased in vigor. Moreover, this law appears to pervade the cosmos. It is probable that, as long since suggested by Kirkwood, the temperature of the cosmic bodies varies directly, while their chemic complexity (as determined by the spectroscope) varies inversely with their volume; and the meteorites, which give some indication of the constitution of other parts of the solar system, if not of still more distant portions of the cosmos, are made up chiefly of elements common to the earth, yet are united in frequently distinct and usually simpler compounds. Thus the phenomena of the planet, of the cosmos in general, and of meteorites appear to express a law of inverse relation between chemic constitution and temperature, i e., a law of chemic differentiation accompanying molecular degradation; and this law is in accord with the results of some of the latest researches concerning the ultimate relations of matter and energy. It follows that an aged planet like the earth must have stored up within it a vast amount of latent molecular energy; and incidentally that the law of cooling based on bodies of simple constitution is inapplicable. So it may be questioned whether the simple law of cooling, supposed to indicate the age of the earth, is more trustworthy than would be a formula for the volume-temperature relations of H_2O, derived from laboratory experiments on ice when extended to a body of the same substance passing through the gaseous, liquid, and solid conditions; or whether the simple law of cooling deduced largely from laboratory experiments conducted under circumscribed conditions are much more applicable to the highly complex earth than to the body of a hibernating animal.

In short, the geologic estimates of the age of the earth are based on direct observation under actual conditions so fully known, that, although certain factors are variable, all may be safely assumed to be known; while the factors involved in the non-geologic estimates — surface and sub-surface temperatures, thickness of the earth-crust, properties and conditions of rocks, etc. — must be furnished by the geologist, so that, at the best, such estimates represent nothing more than the grist ground from a mathematical mill; and, moreover, it usually happens that unknown factors are introduced to give texture to the product, but which, at the same time, so far adulterate the grist as seriously to affect its value. The geologic estimates concerning the age of the earth are based on real processes and actually observed conditions in such manner as practically to eliminate inaccuracies growing out of complex and unknown factors, and are thus strictly pertinent to the case; while the non-geologic estimates are based on ideal conditions immeasurably simpler than those actually attending a planet, and thus, interesting and instructive as they are in the abstract way, have very little to do with the concrete case.

It is significant that the discussion of geologic process by students who are not geologists is commonly trammeled in two diametrically opposite ways. The student of the "exact" sciences is seldom willing to grant so high a degree of mobility in the terrestrial crust as is required by the geologist to explain current continent movements, and is given to rejecting or ignoring the evidence of such movements; while, on the other hand, he is the first to reject as excessive the time-estimates of the geologist based in part on, and in complete harmony with, these observed movements. This mental habit, growing out of the methods and postulates employed in certain lines of study, is constantly to be borne in mind in weighing non-geologic opinion concerning the rate of geologic process, just as the opposite tendency on the part of geologic study is to be guarded against.

THE STANDARD COLOR SCHEME.

BY J. H. PILLSBURY, NORTHAMPTON, MASS.

In *Science* for Feb. 26, 1892, I gave a brief account of a color scheme, first proposed by myself in 1880, and set forth in more elaborate form in a paper read before a meeting of the Society of American Naturalists held in Boston, Dec. 31, 1890. During the present year, through the courtesy of the Department of Physics of Wesleyan University, of whose laboratory and apparatus I was allowed free use, the standards previously selected by the consensus of a number of color experts have been located by wave-lengths and, as far as possible, also by the prominent absorption lines of the solar spectrum. Since there are vibrations of an infinite variety of wave-length, any number of standards might be selected, but it is not, of course, desirable to select a larger number than the eye can readily distinguish. Six colors are clearly recognized by every normal eye in the solar spectrum, and this number has been chosen for the scheme of standard colors, as being both convenient and practical. These colors are red, orange, yellow, green, blue, and violet. For the area of the solar spectrum taken for measurement, which we will call the unit of the color, a patch of the spectrum obtained by a diffraction grating, representing a range of fifty ten-millionths of a millimetre in wave-length, was selected. This gives an area of color of convenient size for comparison, and one which appears quite homogeneous to the eye, even in those parts of the spectrum where the change is most rapid. The wave-lengths here given represent the centre of the area selected. The location of the standards with relation to the absorption lines of the spectrum, where such a location was possible, will give a convenient means of ascertaining the position of the standards I have selected without recourse to the elaborate method required in the use of the goniometer.

The Standard Spectrum Colors.

Color.	Wave Length.	Location by Prominent Solar Lines.
Red	6587	Above the "C" line.
Orange	6085	Between lines 6123 and 6066.
Yellow	5793	Between lines 5816 and 5763.
Green	5164	Between lines 5189 and 5139.
Blue	4695	No prominent lines.
Violet	4210	No prominent lines.

To obtain the intermediate hues, which it may seem desirable to introduce between these standards, these should be combined in inverse proportion to what the artists call the "value" of the colors. This is not, perhaps, easily determined, yet its approximate measure can be ascertained with sufficient accuracy for this purpose. These, however, are of much less consequence than the standards. Using Maxwell discs in these standards, the following formulæ will serve to illustrate, viz.: —

Orange Red R 70, O 30, Red Orange R 41, O 59.

Here the orange, having what the artist calls a higher "value," is used in a smaller proportion than the red. The same will be true in producing the tints and shades of any color. The amount of white or black to be used must be determined by the value of the color.

It has been urged in objection to the spectrum colors that they are not the colors of nature. In reply to this objection, it should be said that nature has no other colors than those of the spectrum. With these, however, are combined more or less of white light and shadow, producing the beautiful effects which so charm us in the landscape as it is spread out before us. For purposes of instruction, a series of what we may call "broken" colors is valuable. These are mixtures of the standards with both white and black, in given proportions. The amount of white and black must be determined, as in the case of tints and shades, by the value of the color. For advanced educational purposes, these broken colors are valuable, but should not be used until the student is well grounded in the knowledge and use of the standards.

The adoption of this scheme for practical purposes is also a subject of interest. By the use of the Maxwell discs, made in these standard colors, it is possible to determine the components of any color with which one may meet. The formula for such an analysis will enable anyone, by means of a similar set of discs, to reproduce the color with perfect ease. New combinations of color may also be produced with equal facility. In cases of experimentation, to ascertain what combinations of color would be harmonious, this is a great saving of time, labor, and cost.

The use of such terms as vermilion, emerald green, ultramarine, and other similar terms to express the results of analysis, is impracticable in the extreme, on account of the variability in the use of the terms.

Discs made in these standards are manufactured and can be furnished at a moderate price. These discs are at present made in pigments, which are excellent reproductions of the spectrum hues. Some of them, however, can be produced in the brilliancy required only by the use of analine colors, and these are not permanent when exposed to the light. For this reason they must be carefully protected when not in use and have to be frequently renewed.

MISSOURI OFFICIAL GEOLOGICAL REPORTS.

BY F. A. SAMPSON, SEDALIA, MO.

THE late publications of the geological survey of Missouri contain lists of the reports of the survey, which lists are not complete and give but a part of the official geological reports of the State; the four below mentioned should clearly be added to the list.

By an act of the legislature of the State approved Feb. 11, 1839, a Board of Internal Improvements was organized to have supervision and control over all State roads, railroads, slackwater navigation, or canals. The act provided for the appointment of a chief engineer, who should cause to be compiled "a large and correct map of the State" showing in "a correct and minute manner" the geographical, topographical, and geological features of the State. In his office should be kept "all reports of engineers, geologists, and other scientific persons, either contributed by individuals or ordered by the State." A supplemental act, dated two days later, provided for surveys of four rivers, the Osage, the North Grand, the Salt, and the Merrimac, and one railroad route, that from St. Louis to the Iron Mountain.

The members of the board were appointed by the Governor, and these were assigned as commissioners of the above five routes. A State engineer and a geologist of the Osage River survey were also appointed. The journal of the board and the reports of the chief-engineer and geologist are among the scarcest of the publications of the State. I know of no copy in the State except my own. It was published in the appendix to the Senate journal of the eleventh General Assembly, 1840-41, and probably in the House journal also, but I have never seen any copy of the latter, I know of no copy of either journal in any library in the State. The report I have I found in the tower of a court-house in Central Missouri, the journal part having been torn away, but leaving the appendix complete. The title of the report is "Report of a Geological Reconnoissance of that part of the State of Missouri adjacent to the Osage River, made to William H. Morrell, Chief-Engineer of the State, by order of the Board of Internal Improvement, by Henry King, M.D., Geologist, President of the Western Academy of Natural Sciences, etc., etc.," pp. 506-525.

Professor Swallow made a report on the southwestern branch, in obedience to the act of March 3, 1857, which required the State geologist to make a thorough survey along the lines of all railroads aided by the State, and to report in detail to the president and directors "all the mineral, agricultural, and other resources which may affect the value or income of the road under their direction." But one such report was published, but this is as much one of the official reports of the survey as any other of Swallow's reports. Its title is "Geological Report of the Country along the line of the Southwestern Branch of the Pacific Railroad, State of Missouri. By G. C. Swallow, State Geologist. To which is prefixed a Memoir of the Pacific Railroad. St. Louis: Printed by George Knapp & Co., 1859." 93 pp., plates and geological map of southwest Missouri.

Another edition of this report somewhat fuller was published in New York by the Pacific Railroad Company, but I have not seen a copy of it.

The third omitted report is a short one, but it could not have been omitted on account of that fact, as Swallow's third report is still shorter. It is entitled as follows: "Report to the Board of Curators concerning the Transfer of the Geological Survey to the School of Mines, and the work executed during the year. By Charles P. Williams, Ph.D., Director Missouri School of Mines and Acting State Geologist."

When preparing the bibliography of the geology of Missouri I found this report in the catalogue of the Missouri State University for 1876, pp. 213-216, a publication in which one would not look for a geological report, but for some years the catalogues of the university contained many papers of merit, as addresses and lectures, contributions from the laboratories of the University and of the School of Mines, Bulletins of the Agricultural College, etc.

The latest of these omitted reports was found by me since the publication of the Missouri bibliography, in the House journal of the adjourned session of the twenty-sixth General Assembly, 1871–72, pp. 226–290, and I think it was not published except in that journal. Its title is "Report of Progress of the State Geological Survey, from August 30, 1870, to March 13, 1872. By J. G. Norwood, State Geologist, pro tem., State University, Columbia, March 13, 1872."

CHARACTER IN ANIMALS.

BY W. C. BARRETT, M.D., D.D.S., BUFFALO, N. Y.

MAN too often looks upon the lower orders as possessed of nothing but selfish instincts and impulses, and as being moved by nothing but animal appetites. He becomes a tyrant over them, and never for a moment dreams that they can comprehend his meanness and injustice. A little more of observation would remove this impression. Who that has been in close contact with any class of animals but can call to mind instances of the exercise of gratitude, real benevolence and magnanimity, that would do honor to the noblest human beings?

This is not confined to domestic animals, nor can the exhibition of special traits be attributed to their association with man. It is a truth which no observer will deny, that some are quite incapable of affectionate impulses. They seem to have sufficient intelligence, but like some men they are utterly and entirely selfish, while others are even morose and vindictive. There is as distinct and characteristic an individuality in their natures as in that of human creatures. It is an interesting exercise to study these personal peculiarities even in wild animals, and to detect the human traits which distinguish each. Birds that seek the companionship of man exhibit a wide variation in individuality. It is not difficult to obtain the confidence and trusting faith of some robins, for instance, while others are ever suspicious and distrustful.

I was once possessed of a common red squirrel, that was caught when but a few days old, and which had the most charming personal characteristics imaginable. I never saw in any human being a stronger and more marked individuality than this animal possessed. It was as playful as a young kitten, and delighted in the attentions of anyone of whom it was fond. It was as affectionate and as demonstrative as ever I saw a young child. It had withal a merry, playful mischievousness, that while it was at times vexatious, made it seem almost human. It was allowed to run about the rooms at will, and it found the most constant delight in entangling a piece of knitting or other work, and, when detected, in attaining some inaccessible height, then indulging in a chuckling kind of chatter. The chess table could never be set out with the animal at liberty, but that when the players became absorbed in the game and had forgotten all else, Jennie would suddenly alight upon the table, scattering rooks and pawns in every direction, and instantly disappearing up a curtain or into some nook in the book-cases. This would be repeated as often as the players forgot their surroundings, until it became necessary to catch her and shut her up in her cage. One could not lie down upon a couch, with a newspaper which he was reading held aloft over the head, but that like a lightning flash Jennie would light upon the paper or book, and instantly scramble away to some safe place, where she would absolutely chuckle at the success of the scheme. She never gnawed the furniture but once, for she never forgot the punishment which this brought.

She was subject to likes and dislikes, and every visitor who entered the room was carefully scrutinized. If it was a lady who was looked upon with favor, her hair was pretty sure to be pulled down by the demonstrations of affection, and out of a seeming pure love for good-natured mischief. If, on the other hand, the visitor was looked upon with distrust, he could never get near the animal. It loved to fondle those who were its favorites, and exhibited the utmost affection for them. Indeed, its attentions sometimes became too intrusive for comfort.

One unlucky day an accident deprived poor Jennie of her life, and I obtained another, caught at quite as early an age, and always treated with the same kindness and care. I had expected another such charming pet, but there was no more similarity in disposition than there might be between two utterly diverse children. The second animal was morose, sullen, vindictive, in every way disagreeable. The first one would never under any circumstances attempt to bite, while the second was at least always threatening it, and forever scolding and chattering, until at last I gladly gave it freedom in the woods and obtained a successor.

This one was unlike either of the others. It was not playful or affectionate, nor was it perverse and churlish. It was a complete exemplification of the miser, and its whole character was absorbed in its acquisitiveness. It was ever hunting for nuts and other things which struck its fancy, usually articles of food, which it carried away to a secret place in a closet. Occasionally these were taken out by some member of the family and placed in another room, for the purpose of watching the seeming exultation with which the squirrel made their discovery, and the enjoyment it appeared to take in carrying them away and again hiding them. It would run back and forth with such extreme assiduity that it would tire itself out and drop panting upon the floor, only after a few moments' rest to recommence the task. If the newly-found treasure were suddenly removed during its absence, there would seem to be the most poignant disappointment. The animal would for a time search anxiously for the vanished wealth, and then in succession visit the members of the family who were present, and seem to beseech its return, as if knowing that we were responsible for its loss.

There was never a moment during the day which was not spent in searching for something to add to its hidden possessions, or in arranging and rearranging its store. The animal, like some men, was so utterly absorbed in its avariciousness, that it had no time to devote to anything else. All affection was lost in its sordid nature. It had no special dislike for or fear of human beings, yet it sought solitude, apparently to enjoy the contemplation of its accumulations. It was unsocial, simply because of its covetousness. No human mind ever exhibited a meaner avariciousness, or a more parsimonious stinginess. It would suffer for lack of food, rather than take one nut from its great possessions. Its most salient characteristics were so disagreeable to witness that I finally gave the animal away, and after several other attempts gave up in despair that attempt to find another such cheerful, engaging, affectionate, trusting pet as the first one, being fully convinced that such characteristics are as rare among squirrels as they are among men and women.

CURRENT NOTES ON ANTHROPOLOGY.—XXIX.

[*Edited by D. G. Brinton, M.D., LL.D., D.Sc.*]

Modifying Agents of Skull-Form.

IT looks now as if Broca, however eminent in many branches of anthropology, was no wiser as a prophet than others of that genus when he ventured this prediction: "The day will come when the characteristics of all the races and their subdivisions will be so well known, that the study of a series of skulls will be sufficient to determine their origin."

It is in pursuit of the realization of this dream that craniologists have labored ever since, with the result that they are farther from the goal than ever. Now, the wiser among them are turning their attention rather to the history of the development of the skull and its parts, both in the individual and, comparatively, in the realm of animal life, and not endeavoring to use it as a standard for the classification of races and peoples. It is found that in certain instances the shape of the same skull varies materially with the age of the individual; that the tendency to reversion to one or the other type in the parents is by no means equal in all cases; that there are marked correlations with greater strength, viability, and sexual life, which give one or the other form an advantage in a given *milieu* above its associate; that the prevailing type of a geographical province seems to exert an influence without direct intermixture of blood; that social planes, which mean different modes of life and nourriture, exert an influence; and so on. This is the newer science of craniology, more complex, indeed, but far more promising than the old study of dry bones alone.

Ethnic Ideals of Physical Beauty.

The *vis superba formæ*, the "proud strength of beauty," has never yet been sufficiently acknowledged as a formative principle in the evolution of racial and national types. Through conscious cultivation and sexual selection every individual strives more or less to possess and propagate those traits which the national imagination conceives as the comeliest. In a recent thesis, Dr. Loubier tells us from a wide reading of the French poets of the twelfth and thirteenth centuries what they portray as the ideal of manly beauty. It is this: tall, broad-shouldered, deep chest, slender figure, foot arched, skin white, hair blonde, quick eyes, high color, red lips. Evidently this is the High German type rather than that of the modern French; but the poets drew their heroes only from the nobles, and not from the common herd.

Some years ago, in an article on "The Cradle of the Semites," I had occasion to study the ideals of male and female beauty shadowed forth in the erotic composition known as the "Song of of Songs," or the "Song of Solomon," in the Old Testament. It dates from about 250 B.C. There the male is portrayed as "white and ruddy," his hair black and curly, his eyes gray ("like doves washed with milk"), his stature tall. He describes his bride as "fair all over, without a spot," slender, "like a palm tree" (not fat, as modern Oriental beauties), her hair "as a flock of goats," that is, wavy and light-brown, probably, her lips red, "like a thread of scarlet." The interesting feature in both these descriptions is that they point much more to the blonde than to the brunette type as that which hovered before the imagination of the sons and daughters of Israel as the realization of their amorous dreams.

The Easternmost Wave of the Early Aryan Migrations.

The Khmers of Cambodia have long been regarded as an isolated people of mixed blood and uncertain affinities. In a meritorious work published in Germany this year, Schurtz's "Kathecismus der Völkerkunde," the author refers to them as the probable aboriginal inhabitants of Cambodia. On the other hand, in the Mémoires of the Society of Anthropology of Paris, Dr. Maurel of the French Marine has a very able article, based on original observation, much of it anthropometric, going to show that the ancestors of these Khmers were the leaders of the easternmost wave of migration of the Aryan or Indo-European stock.

That they came from Hindostan and brought with them the Aryan culture of that country is proved by the stately ruins of their temples around Ang-kok, whose walls are decorated with bas-reliefs of scenes from the Ramayana. Their arrival was probably about the third or fourth century of the Christian era, and their route apparently was from the delta of the Ganges across lower Birmah and Siam. It is likely that even at this time most of their followers were non-Aryan and the leaders rarely of pure blood. In later generations they received a large infusion of Mongolian admixture from the tribes they found in Cambodia, who belonged to that race.

These conclusions are borne out by a close anthropologic study of the existing population and of the history and archæology of the country. If correct, they show that the mighty Aryan stock, wandering from its pristine seat in western Europe, reached in its eastern wanderings almost to the shores of the Pacific, on the China Sea.

The Evolution of the Idea of God.

Last year a book was published in both French and English by Professor G. D'Alviella, under the title, "The Idea of God as Illustrated by Anthropology and History," and it received a careful handling by the distinguished Professor Reville in the Proceedings of the Musée Guimet. From these two excellent sources we may take the last word as to the genesis of the notion of Deity, as understood by scientific minds.

It arises first from the association of the idea of personal life with that of motion; for instance, the swaying of the tree to the primitive man is as certain a proof of personal life as the flying of a bird. By extension of this, and later through dreams, memories of the dead, and casual associations of motionless objects with motion (as a rock in the midst of a rapid), arose spiritism or animism, to which these writers apply the general name "polydemonism." In this stage there is no Pantheon, no hierarchy of the gods, no idealized generalizations of divine powers.

This appears in the next stage, which is "polytheism," in which the mind of man seeks to coördinate the visible powers of nature, and to explain one by the other, thus subsuming a group under one abstraction, which becomes to him a personified idealized force. This is the epoch of mythology, which is at once an imaginary history and a tentative philosophy of the unseen agencies in nature.

The ultimate stage, monotheism, has various origins, depending on the ethnic psychology of the people among whom it arises. It may be an exaltation of the national god through national pride, so that he shall be "God of Gods and Lord of Lords," as seems to have been the case with the Israelites; or it may arise from concentrated devotion to one divinity to the mental exclusion of others, as in the so-called "henotheism" of ancient Egypt; or, again, in nations of uncommon speculative insight, it may be a purely logical deduction, as among the ancient Greeks. Most of the so-called monotheisms are in reality only "monolatries;" that is, there is worship of but one god, though many divine powers are recognized as existing.

The important point is urged, especially by M. Reville, that this sequence of development is not historical; it is not even ethnic; but strictly anthropologic; that is, the whole of the sequence exists contemporaneously and in the same locality with its highest member. Alongside of the pure speculations of Plato were the puerilities of paganism; and in modern Christian communities there are far more polydemonists and polytheists than monotheists, in the scientific sense of that term.

Both writers reach the opinion that the religious sentiment is not a passing phase of human mental evolution, but a permanent trait; and that, though all existing cults and creeds may pass away, it will only be to give place to nobler ideals of humanity and loftier conceptions of divinity.

NOTES AND NEWS.

A SERIES of international congresses, under the auspices of the World's Congress Auxiliary, and the authority of the Government of the United States, will be held in Chicago during the progress of the World's Columbian Exposition. The Congress of Anthropology will begin on Monday, Aug. 28, and will continue until Saturday evening, Sept. 2, 1893. It is requested that the title and abstract of any paper to be offered to the Congress be forwarded as early as possible to the secretary of the Local Committee, with a statement of the time required for its reading, in order that the Congress, at its organization, may have the material for the arrangement of the programme for the week. The committees of the International Anthropological Congress are: Local Committee of Arrangements, F. W. Putnam, chairman, C. Staniland Wake, secretary, Edward E. Ayer, James W. Ellsworth, H. W. Beckwith, and Frederick Starr; Executive Committee, Daniel G. Brinton, president; Franz Boas, secretary; W. H. Holmes, representative of the American Association for the Advancement of Science; W. W. Newell, representative of American Folk Lore Society; Otis T. Mason, representative of Anthropological Society of Washington; Alice C. Fletcher, representative of the Women's Anthropological Society of America; Louis A. LaGarde, representative of United States Army Medical Museum; and the presidents and secretaries of the Sections of the Congress. Address all communications to Professor C. Staniland Wake, Local Secretary, Department of Ethnology, World's Columbian Exposition, Chicago.

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Attention is called to the "Wants" column. It is invaluable to those who use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The "Exchange" column is likewise open.

THE FORMATION AND DEFORMATION OF MINNESOTA LAKES.

BY C. W. HALL, MINNEAPOLIS, MINNESOTA.

IN respect to inland waters Minnesota shows conditions which are exceptional in North America. Exclusive of that portion of Lake Superior within her boundary, the State has 5,700 square miles of lakes varying from a few acres to the size of Red Lake, which has 340,000 acres in its area. These lakes are conveniently divided into three classes.

First, rock-bound lakes. These bodies of water occur chiefly in the northeastern portion of the State. They occupy the troughs in the crustal folds that have contorted the surface, or the depressions where excessive faults have broken and considerably tilted the strata. These, as a rule, are long, narrow, and deep. The water is clear and sparkling, abounding in fish, and remarkably free from the various forms of plant growth peculiar to shallow waters.

Second, silted-river lakes. These lakes occur in rivers where rapid streams have brought down a deposit into the channels of the more sluggish ones, the silting debris being so course that the slower current fails to transport it. Thus slower streams are choked up and the water set back for miles. Such lakes are Lake Pepin, formed where the sluggish Mississippi is dammed by the debris brought into its channel by the more rapid Chippewa River, Lake Saint Croix, Lake Lac qui Parle, and many others.

Third, glacial lakes. If this group could be minutely subdivided, there would be seen several types of lake formation. Those of the Lake Agassiz type, where one portion of the shore consisted of a wall of ice, have long since disappeared and left scattered pools of varying sizes, occupying the depressions in the generally level surface of the old lake bottom. Going outside of the State for an illustration, we may name Lake Winnipeg as the largest pool now remaining in the bottom of glacial Lake Agassiz. Many other lakes are scattered over the level portions of the State, occupying the depressions in the drift-sheet where this was laid down evenly through the steady and uniform movement of the ice, or through the silting effect of waters due to the melting of the ice border.

But by far the greatest number of lakes in Minnesota are those occupying the depressions in the unevenly distributed morainic matter deposited during glacial times. Portions of the State are thickly studded with these beautiful sheets of clear water. The region between Minneapolis and the Red River valley is appropriately called the Lake Park Region, as lakes occur here in vast numbers. Wright County, nearest to Minneapolis, contains 259 lakes, Kandiyohi County contains 286, and Ottertail County, well up towards the borders of glacial Lake Agassiz, holds the banner over 430 lakes. Passing northeastward from the Lake Park Region towards Ontario, past the head-waters of the Mississippi, and across the upper streams of the St. Lawrence River basin, we pass gradually from the region of moraines to the region of thin glacial deposits and constant and tumultuous rock-exposures, carrying large numbers of lakes of the first type.

It has been frequently estimated that Minnesota contains 10,000 lakes. To one passing over the State through the region named this does not seem an exageration. There must be several thousand lakes from one mile in length upwards to the very largest.

While glacial lakes show many varieties of form due to the position of tongues, branches, and subdivisions of the moraines, they are generally circular in outline. The deepest portion is at the centre. Their shores show but few successive beaches to indicate erosion at their outlets and consequent drainage, or great variation in the amount of rainfall during the last few thousand years. They were all evidently formed in the same general manner, by the washing down of fine silt from the high land into the bottom, thus gradually filling the interstices in the gravels and sands beneath them, making water-tight bottoms to hold the water. Probably the lakes were small at first, and enlarged gradually as this deposition of fine silt extended their borders until the lowest point in the margin was reached and an outlet drained away the excess of water. When this outlet was reached the conditions of formation ceased, and the conditions of deformation became manifest. Material was constantly washing in from the high lands around through the melting snow of successive springs and the heavy showers and rain-storms of the summer months; it was also brought by streams flowing into the lakes from every direction, and formed *in situ* by the vigorous growth of aquatic vegetation. In the shallower lakes this last cause of deformation works with great rapidity. After the ice disappears in the spring under the warm sun of this latitude the water very rapidly rises in temperature to 70° or 75°, a favorable temperature for vigorous vegetable growth, and thus plant-forms which can get foothold upon the lake bottom will develop a vast amount of plant debris. Already hundreds of the shallow, small lakes of the State have disappeared, and rich, productive hay-meadows have taken their place. This will be the fate of thousands more within the coming century. On every hand we hear old settlers speak of large lakes once affording superb hunting ground for wild geese, ducks, and other water fowl and excellent fishing, as now either hay-meadows or extensive marshes soon to be fitted for hay production by a further lifting of the surface above the level of the outlet through this rapid accumulation of the vegetable mold.

The lakes of Minnesota afford some of the most attractive summer resorts to be found in our northern States; as already famous can be mentioned Minnetonka, White Bear, the Chisago Lakes, and Waconia. The list can be indefinitely extended. Their climatic influence is very marked. The amount of heat stored up during the summer, if calculated by its mechanical equivalent, is enormous. With 43° as the average increment in temperature, 10 feet as an average depth of 5,700 square miles of water surface, we have nearly 11 cubic miles of water. Since each cubic foot of this water receives 1,250,000 foot pounds of heat, which must be given off during the autumn months as these lakes gradually settle down to the freezing-point of winter, the amount of heat thus made available for our autumn weather reaches quintillions of foot pounds. This warmth is a break against early autumn frosts. The south side of Lake Minnetonka has most productive vineyards and fruit gardens, while the northern side is liable to early frosts.

Other writers have called attention to the distribution of fresh-water lakes. They are almost wholly confined to the glacial regions of our globe. Northern Europe and northern central North America, with other isolated portions of the globe, are the only places where many bodies of fresh water are found. It remains to be noted that within these glaciated regions the oldest portions are already comparatively free from lakes. The southern border of the glacial area of the United States is almost wholly devoid of them. The vast prairies of Ohio, Indiana, and Illinois have but few, yet there are vast agricultural tracts within these States which show deep and rich accumulations of vegetable mould, evidence of former aquatic plants. Doubtless there were once thousands of lakes within these States, but the silting-in of mineral debris and the growth of vegetation have filled them up, and thus altered the whole face of the country, a result soon to be very marked within Wisconsin, Minnesota, and the Dakotas. Indeed, the years can almost be counted when glacial lakes within these States will be rare indeed.

The question how long a period of time has elapsed since the retreat of the glacial ice-sheet from the central portion of the North American continent cannot be here discussed. Yet, by way of suggestion, it may be said that, if the filling-up of glacial lake basins be a chronometer useful in measuring geological time, the rate at which these lake basins are now filling up and disappearing, and the fact that they have already disappeared from the southern portions of the glaciated area, are strong presumptive evidence that the ice of the glacial period lingered longest in the region between Lake Michigan and the Missouri River to the north of the 44th parallel, and that here the time since its disappearance has been comparatively brief; indeed, that the estimates of Gilbert, Wright, Winchell, Upham, and others are long enough to explain every phenomenon save, possibly, that of the redistribution of plants.

The two remaining lake types are more permanent. Rivers will continue to be silted and their currents choked so long as two streams of varying transporting power merge into one. Rock basins will continue to hold water so long as the conditions of erosion are unfavorable through the obdurate resistance of crystalline rocks, and plant growth is discouraged through the lack of soil, as is now the case around the margins of the rock-basin lakes of northeastern Minnesota.

THE ROYAL SOCIETY OF CANADA.

THE twelfth annual meeting of the Royal Society of Canada was held at Ottawa, Canada, during the week beginning May 22, and terminated its sessions on Thursday evening, the 25th.

The meeting opened under the presidency of Dr. J. G. Bourinot, C.M.G., clerk of the House of Commons, etc. The meeting was very well attended by fellows and delegates.

The society divides itself into four sections, as follows: I. French Literature and History; II., English Literature and History; III., Mathematical and Chemical Sciences; IV., Geology and Biology.

Amongst the papers which interest the readers of *Science* most were those of Sections III. and IV., besides the "Science Lecture" given to the public under the auspices of the Royal Society in the Assembly Hall of the Provincial Normal School.

The president's inaugural address dealt with "Our Intellectual Strength and Weakness," which received most favorable comment

The public science lecture was delivered by Dr. Ramsay Wright, professor of biology and histology in Toronto University. His subject was, "The Natural History of Cholera." In a masterly manner Professor Wright treated his subject, and described this minute microscopic plant through all its phases and life-history in a simple, clear, and practical manner, throwing a flood of light and giving an amount of information of great value.

In Section IV. Mr. Whiteaves gave the presidential address, in the course of which he summed up the result of researches in the Cretaceous formations of Canada. In the course of his address Mr. Whiteaves showed that in Canada no less than 600 species of fossils were known from the Cretaceous rocks of the Northwest Territories, of the Rocky Mountain region, and of the coast and islands of British Columbia. Of these some 450 were marine invertebrates, mostly shells, and characterized the two divisions into which the Cretaceous system was divided in Canada, viz, the Earlier and Later Cretaceous.

Sir William Dawson had described or identified no less than 115 species of plants from the Nanaimo, Queen Charlotte Islands, and British Columbia Cretaceous basins. Mr. Whiteaves himself had devoted his attention to the invertebrate and vertebrate faunas (*partim*), whilst Professor Cope had in his hands a number of the deinosaurian remains which characterize certain horizons of the Cretaceous in the Prairie region of the Northwest.

Then came Sir William Dawson's paper, entitled "Additional Notes on Cretaceous Plants from Port McNeill, British Columbia." The collection made by Dr. G. M. Dawson at this place was cursorily noticed in a note printed in the Transactions of this Society (1888, p. 71, Sec. IV.). As the collection is large and the specimens unusually perfect, and some of the species are new and very interesting, it has been thought desirable to prepare more detailed descriptions, more especially as these plants belong to either a station or a horizon somewhat distinct from those so familiar in the coal-fields of Nanaimo and Comox on the other side of Vancouver Island.

This paper was followed by another from Mr. Whiteaves, "Description of Some New Species of Fossils from the Trenton Limestone of Manitoba." This was a continuation of two others on the same subject which have already appeared in the Society's Transactions. It contained descriptions and illustrations of several species of Cephalopoda and of one rugose coral.

Dr. Ells then read a most interesting contribution on the geology underlying Northumberland Straits: "The Geology of the Proposed Tunnel under the Northumberland Strait between New Brunswick and Prince Edward Island." The paper discusses briefly the several geological formations which border on that portion of the Gulf of St. Lawrence adjacent to the Strait, with reference more particularly to the several members of the Carboniferous system, the rocks of which have a very considerable development in this area. The proposed tunnel, according to its present location, will traverse these between Cape Tormentine, in New Brunswick, and Carleton Point, in Prince Edward Island, and the description of the strata which will probably be encountered is given, as shown by the series of bore-holes put down during the past season along the line of the principal route.

Dr. Ells was elected fellow of the Royal Society at its last meeting, and has, through his numerous papers and writings on the geology of Canada, contributed much new information regarding the economic minerals, as well as some of the most intricate problems of geology, chiefly in New Brunswick and Quebec.

Mr. Lawrence M. Lambe contributed his second paper on "Sponges from the Pacific Coast of Canada." The paper describes the sponges collected by Dr. G. M. Dawson in the vicinity of Vancouver Island and the Queen Charlotte Islands. There are, all told, about twenty species, seventeen of which are siliceous.

Mr. W. Hague Harrington read a paper on the "Canadian Uroceridæ," in which descriptions, synoptical tables, and lists, together with remarks on the occurrence, distribution, etc., of the species, are given, whilst the Rev. G. W. Taylor of Victoria, B.C., presented "A List of the Land and Fresh-Water Mollusca of Canada, with Notes on their Distribution."

Mr. G. F. Matthew of St. John, New Brunswick, so well known for his valuable papers on Cambrian geology and palæontology, was to the fore with three papers or contributions:—

(*a*) "Illustrations of the Fauna of the St. John Group, No. VIII.," contains descriptions of new species from Band *b* of Division 1, and Band *b*, Division 3; also forms from Division 1 *b*.

(*b*) "On Some Remarkable Organisms of the Silurian and Devonian Rocks of Southern New Brunswick, No. 2." A paper on certain species of the above formations was read before the Royal Society in 1888. The present article contains descriptions of a few others, all of which are from the well-known plant beds of Lancaster, near St. John. These were found by Mr. W. J. Wilson, now of the Geological Survey of Canada. 1. The wing of an insect of the genus Homothetus. 2. A new species of scorpion. This species is of Silurian (Upper) type; the thoracic shield, which is unusually narrow, is the only part certainly known. 3. A new land shell; it resembles *Strophites grandæva* of Sir Wm. Dawson, but is larger and proportionately more slender. 4. A millipede, a minute species, belonging to the division Chilopoda; of which the body is not complete.

(*c*) "Traces of the Ordovician System on the Atlantic Coast." This system has not heretofore been recognized by its fossils on the Atlantic Coast of America, except at St. John, where the oldest part of it (Arenig horizon) is folded in with the Cambrian rocks at St. John. We now recognize it at two other points, viz., Conception Bay, Newfoundland, and Bras d'Or Lake, Cape Breton.

In both these localities the fossiliferous beds consist of gray sandstones holding a fauna of European facies, several of the species being common to these and the Russian deposits. Overlying the Ordovician strata at Conception Bay are the sandstones of Great Bell Island, which, as they contain Brachiopods of the Trimerellid family, are probably Silurian (Upper). The fossiliferous Ordovician deposits in New Brunswick and Maine are either fine, dark shales and slates or siliceous mud-rocks, and indicate the probable existence of one or more deep-water sounds in Ordovician times partly shut off from the ocean over the area of the Acadian provinces.

Mr. G. U. Hay contributed an interesting paper, which received hearty endorsement and well-merited comments, on the "History and Present State of Botany in New Brunswick." The subjects dealt with in this paper are: 1. The History of Botany in New Brunswick, referring briefly to the explorations made by botanists in this and the neighboring provinces in the first half of the present century, noting the discoveries of plants made and the partial or fragmentary lists, chiefly of forest trees. Of these lists, probably one of the earliest and fullest is that by Sir James E. Alexander, preserved in his second volume on "L'Acadie," and comprising over one hundred species of plants, shrubs, and trees collected between Petitcodiac and Boiestown. 2. The Present State of Botany in New Brunswick, showing that a fairly complete survey of the Phenogamous flora of the province has been made, with the result that lists aggregate between nine hundred and a thousand species of flowering plants. A beginning has also been made in the cryptogamous flora by the preparation of short lists of mosses, lichens, and algæ. The economic importance of a wider and more general study of this subject is urged, especially with regard to agriculture and forestry; the collection of information on the time of flowering of plants and the ripening of fruits each year at many stations throughout the Province; more attention paid to the medicinal plants found in the Province, and more general and systematic attempts made for the extermination of weeds.

Three more geological papers were on the programme, but, on account of the gentlemen who were to read them being absent or engaged in the work of other sections, they were "read by title." These are as follows: "Note on the Gold-Bearing Ore of the Crawford Mine, in Peterborough County, Ont.," by Professor E. J. Chapman, LL.D. "Notes sur le forage d'un puits artésien dans le quartier du Palais, Québec," par l'Abbé J. C. K. Laflamme. Ce puits a été creusé dans un terrain dont l'horizon géologique ne semble pas encore absolument établi. L'examen des échantillons qui en ont été tirés pourrait être de nature à jeter un peu de lumière sur ce problème de stratigraphie. "Note sur la valeur de l'ouvrage de J. Cornuti sur les plantes du Canada," par l'Abbé J. C. K. Laflamme. L'auteur, dans sa monographie sur M. Sarrazin, avait déjà donné des détails capables de servir à l'histoire des sciences au Canada. En examinant l'ouvrage de Cornuti, publié longtemps avant les travaux de Sarrazin, il espère ajouter une page à peu près inconnue au plus grand nombre de nos botanistes. L'ouvrage de Cornuti a été imprimé à Paris en 1634. Par conséquent, dès le commencement de la colonie, on y a toujours prêté le plus vif intérêt au développement des sciences, et les travaux qui ont été faits sur ce point ont une valeur réelle.

Altogether the meetings were a great success. Dr. George M. Dawson, C.M.G., etc., was elected president of the Royal Society, and Dr. Bourinot honorary secretary. HENRY M. AMI.

THE PLACE OF THE LABORATORY IN TEACHING PHYSICS.

BY A. D. COLE, DENISON UNIVERSITY, GRANVILLE, OHIO.

THE use of laboratory methods in teaching physics has become almost universal in American colleges. But it may well be questioned whether the usual plan followed is the best one. Most colleges require elementary physics as a condition for admission, but this preparation is usually obtained in schools where no opportunity for systematic laboratory work is given, and the student enters college completely ignorant of laboratory manipulation, sometimes indeed without having even seen his teacher perform experiments. In his sophomore or junior year he takes a lecture course in general physics, and at the same time or the year following, a laboratory course. This latter course, however, is often elective, so that many students graduate from college without any laboratory knowledge of physics whatever.

But suppose one does take the laboratory course, is it well adapted to his needs? It often has very little connection with the lecture course, and is conducted by a different instructor. The student begins his work with no training in the accurate use of either hand or eye. Yet his first problem is often a difficult one, involving the skilful handling of complicated apparatus. He does not understand his instrument, turns to reference manuals, but finds their explanations are too general to help, or refer to a form of apparatus differing from his. The instructor cannot help him at once, as he is busy with some other bewildered student. He waits awhile, appeals to a neighbor in vain for help, finally makes a desperate start and at once succeeds in getting his instrument completely out of adjustment. When the instructor does finally get to him, it perhaps takes all the time that can be spared him to get the apparatus adjusted for a fresh start. Two hours have passed, and almost nothing accomplished. This is no fancy sketch. The writer has seen just such cases repeatedly, in several of the best-known laboratories in the country. Is it any wonder that students so misused find physics "hard" and uninteresting? The trouble is not with their work, but they have not been prepared to do it. Yet they can be prepared and with no greater expenditure of time.

Instead of giving the student ninety to a hundred and eighty experimental lectures as a preliminary to such work, give him about half that number, and in place of the other half let him demonstrate for himself the principal facts of physics in a series of about seventy-five measurements, requiring only moderate precision, in order that he may have time for a sufficient number of experiments to fairly illustrate his lecture course. Let him do this while the lectures are in progress, not after they are finished. Let the lectures be given say twice a week, on the other three days of the week give one hour to laboratory practice, and a half hour to recitation on the work of the day and the lecture of the preceding day. Keep laboratory work and lectures in close connection. The ideal method is to have all the class work simultaneously on the same subject — one connected with the lecture of the preceding day — and to conclude with the half hour of recitation.

Of course it is impracticable to duplicate apparatus to such an extent as to carry this system out perfectly, but if the class is divided into pairs for work, and each pair be provided with their own set of the instruments of frequent use, such as metric rule, hand-balances, dividers, test-tubes, etc., a considerable number of the earlier simple measurements can be carried on by all simultaneously. Thus a few glass tubes with the articles named above, will enable a whole class to study the laws of capillarity together, with an approximate verification of the law of diameters.

Where but few duplications of apparatus are possible, five or six different experiments may be going on together. To prevent confusion and loss of time, the apparatus necessary for each is placed by itself, and with it a paper describing briefly the method to be followed and giving references to books kept in the laboratory for the purpose. Each paper may be designated by a number, and each working pair is assigned one of these numbers. A class (or division) of twenty can thus get to work in one minute. For example, suppose the class is just beginning the consideration of specific gravity. Various methods of determining it have been described in the lecture of the day before. The ten working pairs are sent to the desks, where there are sets of apparatus illustrating five different methods, each duplicated once. One is arranged for finding the specific gravity of glass by the hydrostatic balance, another for that of lead by Nicholson's hydrometer, a third for that of quartz by the specific gravity flask, a fourth for alcohol by Jolly's balance, a fifth for mercury by Hare's communicating tubes. On the next laboratory day, each division takes a second one of the five methods, and so on till each has had them all. Lecture and laboratory exercise have helped each other. Each one understands the subject and is prepared to enjoy and profit by the more careful measurement of specific gravity with delicate balance and corrections for variation of temperature and pressure from standard conditions, that awaits him in his term or two of advanced practical work. Such a course prepares him fully for the higher grade of work, so that neither inherent difficulties or imperfect explanation can now be a bar to progress.

It must be admitted that the method presented involves some additional effort on the part of the instructor, but there is abundant compensation in the superior results obtained. If space permitted, I would add something concerning methods of securing at small expense the duplication of apparatus necessary to keep the laboratory studies in close connection with lecture and classroom work, but that would better be reserved for another occasion.

DISCOVERY OF ANCIENT ARGILLITE QUARRIES ON THE DELAWARE.

BY HENRY C. MERCER, DOYLESTOWN, PA.

THE discussion of the Trenton gravel specimens has forced several important questions upon our attention. Where did the argillite come from with which the chipped objects were made? Granted that much of it was found in the river-bed in the shape of boulders and erratic blocks, whence had this material been transported by the river?

To learn that modern Indians on the Delaware quarried jasper and in the process of blade-making strewed the quarry site with "wasters," resembling in form the Trenton specimens, was to ask whether they also quarried argillite.

We had found argillite "turtle-backs" on the surface at the camp-sites of Gilmer's Island, Gallows Run, Ridges Island, and Lower Black's Eddy on the Delaware, but they lacked the final and convincing association with the quarry to prove their pedigree, and we still sought the whereabouts of the ancient pits, the refuse heaps, and the "rejects" or blocked-out implements which were to repeat in the now famous blue stone, the story of the inchoate blades of jasper.

The way towards an answer to one of the vital questions that concerns the antiquity of man in the Delaware valley was opened on May 22, by the discovery by me of a series of seven or eight depressions surrounded by masses of argillite chips (a quarry in fact with all the surface characteristics of Macungie, Vera Cruz, and Durham, in America, or Grimes Graves, or Spiennes, in Europe) on the steep north slope of the hillside at Point Pleasant, Bucks County, Pennsylvania, on the right bank of Gaddis' Run, about one-quarter mile above its mouth and half a mile from the well-known Indian camp-site at Lower Black's Eddy. The work of carefully clearing out one of the depressions and trenching its refuse heap was begun yesterday afternoon and will occupy an indefinite time.

Notched in the slope whose angle is about 35 degrees, the depression, one of eight or nine others, fronts a solid ledge of argillite (an outcrop of the large vein here traversed and exposed by Gaddis' Run, and twice tapped near by, by modern quarries as the purest source of the material).

Its largest diameter is about thirty feet, its depth five, and breadth eight. The trench begun across its narrowest width, penetrating for three feet through loose yellow mould, has shown as yet nothing of importance beyond two bits of charcoal and broken (quartzite pebble) hammer-stones at a depth of one and one-half feet. Another excavation about three feet in diameter has entered the mass of refuse for four feet without reaching its bottom, and discovered at various points thirty-three "turtle-backs," twenty-five broken bases or points, and four hammer-stones. On the surface about the other pits I gathered in a few hours twenty "turtlebacks," six ends or points, and fourteen hammer-stones.

With the work of penetrating to the bottom of the refuse, and studying the ancient quarrying process scarcely begun, I have hardly had time to more than think of the important questions suggested: Who made and worked the quarry? Will it show a successive series of occupations? Can it be connected with the village site at Lower Black's Eddy? What shall we say of these rudely chipped forms? Are they "wasters" and do they of all "wasters" yet heard of, resemble the Trenton specimens?

We are twenty-five miles above Trenton and at the largest and purest outcrop of argillite on the right river bank above that place.[1] The bed of Gaddis' Run and the river-shore below its mouth are thickly strewn with argillite blocks and water-worn boulders — a pathway, in fact, littered with blade material, extending, from the ledge above referred to, to the Indian camp half a mile distant. While the significance of this has been obscured by chipped fragments from the modern quarries fallen into the stream, and the stone dressing that has accompanied the building of a dam, two bridges, and a canal aqueduct, there can be little doubt that the inhabitants of the village often went no farther than a few hundred yards along these beaches for their material.

But too much hangs upon the further examination of this site and the neighboring camp, now at last unfolded to the student in its fuller significance, to warrant a premature word.

[1] On Dark Hollow Run (below New Hope) I found a small vein of it nearly two miles from the river. The blue slate in Pidcock's Creek, on the south slope of Bowman's Hill and at the Harvey and Van Hart quarries below Taylor'sville, lacks the conchoidal fracture.

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.

Science Work at the Avalon Summer Assembly.

I HAVE just received a little blue pamphlet containing the announcement of the new summer school at Avalon, New Jersey, and an extremely interesting and suggestive address on "Science Teaching in the Schools," by Dr. Charles Dolley, its president. Copies of this, I have been told, may be had by writing to Mr. Charles Adamson, secretary, 119 S. 4th Street, Philadelphia, Pa. The objects and methods of this new school are so new and attractive that it certainly marks the beginning of a new era in the teaching of science and art in our common schools.

The keynote of Dr. Dolley's address is struck by the following sentences in speaking of the proper method of educating the coming generation: "They begin by moulding little birds' nests of clay, or constructing cones and cylinders, cubes, and octagons out of paper, without ever having examined a bird's nest, other than that of the sparrow under the eaves, and knowing absolutely nothing of the interest to be found in a prism of quartz, a snowflake, or an icicle. They have been taught of the distribution of whales and camels and all sorts of exotic varieties, but are absolutely ignorant and blind to the wonders of nature to be found at their very doors; wonders requiring no text-books, no costly instruments, but which may be investigated by means as simple and inexpensive as the key and kite string of Franklin."

How few the teachers, let me add, who have the slightest inkling as to the wonderful history written in the chalk or slate they daily use!

Missions and philanthropic societies do good work in this world, but much is wasted. "What is needed," says Dr. Dolley, "is a sanitary missionary in every home, and this we can secure by training the children, by awakening in their minds a desire for something better, for more sunshine, more flowers, a wider horizon and more wholesome surroundings." How few the housekeepers who know the slightest whit about the yeast they use, the mother and flowers of vinegar, the moulds on jellies, the cause of rancid butter, or the nature of contagion! "The tritest things of our mortal experience are the most mysterious." There is enough of interest in a mucilage bottle to keep a man studying a lifetime.

The above quotation is the tenor of that admirable article which ought to be read by every person interested in education and the welfare of his children and country.

Children are born with a love for nature which usually later leaves them. They are of an inquisitive turn of mind and admire flowers and birds while very young, but this is soon smothered instead of being fostered by text-book work and the common method of poking facts into reluctant brains in an ill-ventilated schoolroom by a teacher who knows all about the recent advances in pedagogy but nothing about the subject he is teaching.

The training of the senses of observation, of the faculty of reflection, and of the using of the hand, constitutes an education. One thus trained can get ten times more out of life than the bookworms who feed on second-hand facts.

The students — and the very youngest students — must be reached, and the manner of reaching them is through the teachers. For this purpose the new summer school at Avalon has been established. The work in natural history will be mostly out-of-doors. The students will go with the teachers out among the dunes, in boats about the bays and thoroughfares, among the marshes and along the shore, gathering plants and animals for study. The students in art will sketch right out among the bright-colored sand dunes and study the richness of color which characterizes those beaches and marshes by the sea.

Dr. Charles Dolley is the president and leading founder of this institution. He was formerly Professor of General Biology in the University of Pennsylvania. He was long a student and near friend of Dr. Joseph Leidy, whom, in his character and broadness of views, he resembles. He is a close student of nature, versed in both languages and science, and possessed of such personal magnetism and pleasing manners that he is always surrounded by many friends and admirers who are always helped by his never-failing enthusiasm and encouragement. A better person could not have been chosen for such a position.

The place selected for this summer school is a good one. Plenty of good material for science work is near at hand. It is the only beach on the coast where beautiful forests of red cedar and holly are still standing. There is a long even beach, unexcelled for bathing and carriage and bicycle riding. There are high white sand dunes and beyond vast stretches of salt marshes intersected by many bays, thoroughfares, and salt ponds. On the mainland there are forests of pines and many beautiful plants peculiar to the "low pine barrens of south Jersey."

For a sum not exceeding $50, including all expenses, a teacher can spend five weeks pleasantly and profitably at the seashore, not only bathing and enjoying the salt sea air and the other pleasures of such a result but breathing in a kind of knowledge which they will relish and impart to those under them and thus help to bring about this change in the manner of common school education for which many are hoping. JOHN GIFFORD.

Swarthmore, Pa.

Early Man in Minnesota.

IN *The American Geologist* (April, 1893) Mr. Wm. H. Holmes has published another long article, this time endeavoring to show that there is no evidence whatever of the existence of early man in Minnesota. The article is very prettily illustrated with fanciful sketches, which Mr. Holmes's practice as an artist makes him to evolve from his inner consciousness, and which he employs in all his writings in place of arguments in support of his theory of the non-existence of palæolithic man in North America.

He says that "Besides the investigations of Professor Winchell and Miss Babbitt, no work has been done upon the archæology of this region, although other writers, notably Mr. Warren Upham, Professor G. F. Wright, and Mr. Henry W. Haynes, taking for granted the correctness of all the original observations and conclusions, have ventured to enlarge upon the material published."

The only "venturing" I have done has been to express to the late Miss Babbitt, who sent to me for examination a large quantity of pieces of quartz collected by herself, the conviction that these fragments were of artificial and not of natural origin. As Mr. Holmes calls them Indian refuse, I can scarcely be charged with very hazardous venturing. Miss Babbitt wrote to me that she had discovered them in undisturbed deposits of glacial origin. If this is true, as I have no reason to doubt it is, certain objects among them presenting the palæolithic type must of necessity be true palæolithic implements, and not, like many of a similar type that have been found on the surface, be of doubtful origin. Every one knows that such objects are also sometimes found in Indian shell-heaps and village sites. Accordingly, whether any particular object can be positively identified as a true palæolithic implement or not depends upon the conditions of its occurrence. That is a question for geologists to answer, and if they pronounce the site to be of glacial origin the probability is very great that similar objects found in the immediate vicinity are also palæolithic implements. This is the state of the question with respect to the objects found in the Trenton gravels. How does this reasoning apply to the so-called Babbitt quartzes? The glacial man in fashioning palæolithic implements must have produced a great many splinters and fragments, just as the Indian did in producing his implements. If any particular locality offers only one available material to work with, the refuse of palæolithic man and of the Indian must be precisely alike. I understand this to be true of the out-cropping of veins of quartz in the slate in the neighborhood of Little Falls, Minnesota. No one doubts that Indian relics are found in that vicinity, as is always the case at all good fishing sites like that. But Indian implements and palæolithic implements are very different in appearance, and no skilled archæologist will mistake one for the other. I have seen palæolithic implements that Miss Babbitt said she had found in undisturbed glacial deposits. This is positive, undisputed testimony. What has Mr. Holmes to say in answer to it? I will quote his words precisely: "My investigations have shown that the glacial quartzes were probably not originally included in the loam but rather that they were introduced into it in post-glacial times, and that they were rude because mere shop refuse, the period of occupation thus, in all probability, corresponding to that of our historic aborigines." This may be very convincing to some people, but to my mind it is not quite satisfactory. Professor Winchell says that the quartz fragments are to be found over a very extensive region, "up and down the river an unknown distance," and extending "downward three or four feet" in "hard-pan drift containing boulders." That is something quite different from "loam," the word persistently employed by Mr. Holmes in speaking of the fragments. Loam is defined by Webster as meaning "a mixture of clay and sand with organic matter to which its fertility is chiefly due." If this is the nature of "hard-pan drift, containing boulders," I am incapable of understanding ordinary language. Professor Winchell's words can only be understood of undisturbed glacial deposits. But Mr. Holmes says "there is nothing in the conditions and phenomena of the site that will enable us to say whether the beginning of the quartz-working dates back one hundred or one thousand years." He reaches this sweeping conclusion by imagining that Indian refuse from the surface has been introduced into this "hard-pan" by sinking through the decaying substance of the roots of large trees that have been uprooted by a tornado. Let me quote his own words: "The explanation thus furnished of the distribution of the worked quartzes of this locality through the glacial deposits to the depth of four feet or more is so satisfactory that no other theories are called for, and little further discussion seems necessary." To my mind this explanation is just as satisfactory, and no more so, than Mr. Holmes's former explanation that "most of the so-called gravel implements of Europe are doubtless the rejects of manufacture."

Mr. Holmes first draws pretty pictures, and then draws from them the conclusion that "the record may be so altered in the period of a generation as to be read ten thousand years instead of fifty. Such is the magic of Nature's transformations, and such are the pitfalls set for unwary explorers." Miss Babbitt, the "unwary explorer" in the present instance, is no longer living to defend herself from such assumption as this, but I think all lovers of justice will feel that this is a pretty weak answer to her positive assertions. Mr. Holmes continues: "The mistakes made by Miss Babbitt are precisely such as others have made through taking up investigations in the geologic department of archæology without adequate knowledge either of the processes and phenomena of geology, or of the arts and habits of our aboriginal peoples." I had supposed that such crass ignorance as this was confined, in Mr. Holmes's judgment, to myself; but it seems that there are others falling under a like condemnation. How fortunate it is for the rising generation that Mr. Wm. H. Holmes has appeared to set them quite right in regard to the prehistoric archæology of North America. HENRY W. HAYNES.

Boston, May 22.

Preliminary Note on Eggs of Cottus Richardsoni.

ONE finds in scientific literature so little relating to the habits of even some of our best-known fishes, that reliable information on piscine life-histories is much to be desired.

The little miller's thumb (*Cottus Richardsoni*, Agassiz) was found breeding plentifully in a large spring near Philadelphia on Apr. 29, and a fine lot of material for future embryological study procured.

The places selected for oviposition were invariably the fountain-heads of small, lateral springs which emptied into the main body of water, and where the water was freshest and coldest. No eggs were found at more than a few feet distance from a spring-head. In two or three cases the streams were so tiny that the fishes must have been forced almost to squirm along to the nesting-place. The greatest number, however, were found where a powerful current flowed from beneath an overhanging rock.

A passage is forced beneath a stone, which may be a mere pebble or a large boulder, and a small, shallow chamber hollowed out of the underlying soil, unless the stone be so supported that a natural chamber is formed beneath it. This accommodates the fishes during egg-laying and impregnation; and later serves the male as a resting-place. To the under side of the stone, which forms the roof of the chamber, the eggs are attached, not singly and in small clusters arranged in a single layer, as is the case with Batrachus and other fishes of similar habit, but in an irregular, coherent mass, in which the eggs are often piled up five or six deep, but in most cases are only two or three. The eggs, while very firmly coherent, are loosely arranged, giving the mass a very porous structure, which permits a free flow of fresh water. This is the more necessary since the eggs deeper in the mass are sometimes the first to hatch, when they frequently escape through the passages between the more superficial ones, the collapse of their own egg-membranes making additional room for those which follow. The number of eggs, and consequently the size and shape of the masses, varies, the eggs numbering from 120 to 500. In most cases all of the eggs in a mass were of approximately the same age; but several times, eggs in two or three stages of development were found together, the deepest, of course, being the most advanced. In the cases of most fishes, as is well known, the eggs all hatch, under favorable conditions, at the same time. Whether the several lots are deposited by different females, or whether the eggs are matured in several batches, and the female returns to complete oviposition, I cannot say.

The eggs when newly deposited are of a delicate, translucent, pink color. They average one-tenth of an inch in diameter, being large for the fishes' size, but are quite variable, and are often misshapen by contact with their fellows. As usual, the axis of the embryo passes through the lowermost pole, the dark, widely separated eyes being prominent objects on this side of the egg. There is no regularity about the direction of the embryonic axis, which, in the different eggs of a mass, is found to point in every direction.

Some of the eggs hatched while being conveyed home, and the young lived several days in an aquarium jar. They are very active little creatures, darting about in a most lively manner, often swimming to the surface and then sinking to the bottom, where they rest for a moment, before undertaking another excursion. This activity is exhibited from the time of hatching. When first hatched they are nearly a quarter of an inch long and far advanced in development. The pigmentation is very slight, there being no prominent aggregations of chromatophores anywhere except in the eyes, which are densely pigmented. Branched pigment cells are scattered sparingly, especially on the dorsum of the head.

In every case the eggs were attended by the males, which showed no disposition to desert their posts, but remained motionless, trusting to their protective coloration for concealment.

Several of these males, which were thrown alive into a satchel, seemed to suffer no inconvenience whatever through their absence from water for three hours, but were at once active when placed in a dish. J. PERCY MOORE.

Gophers and Moles.

IN the course of his interesting "Observations on Gophers and Moles" in your issue of April 28, Mr. F. L. Washburn makes mention of two moles which were fatally poisoned by eating worms taken from an old manure heap. I presume that the Oligocheta there identified as *Lumbricus fœtidus* are equivalent or closely allied to those known to fishermen on this side of the water as "brandlings" (*Allolobophora fœtida*), commonly found under manure and readily distinguished from the common earthworm (*Lumbricus terrestris*) by their display of brilliant red rings; and if this be so, I can add my testimony — founded on disastrous personal experience — that the unsavory annelid is toxic also to reptiles. This is somewhat remarkable, seeing that it is devoured with impunity by fish and amphibians. During a severe and prolonged frost, six or seven winters ago, when frogs and common earthworms were not to be obtained, I incautiously tendered a number of these brandlings to certain colubrine snakes, wild with hunger from enforced abstinence after casting their sloughs; they included several mocassins (*Tropidonotus fasciatus*), a Bordeaux snake (*Coronella girondica*), two garters (*Tropidonotus ordinatus*), two or three specimens of *Zamenis atrovirens*, and a whole brood of little Japanese vibakaris (*Tropidonotus vibakari*), born in my vivarium and the sole representatives of the species in Europe. The result was that within a very few minutes the whole lot, as well as a couple of South African slowworms and a large apodous lizard, the "glass snake" so-called (*Pseudopus pallasi*), were in violent convulsions; and although by prompt and vigorous measures I forced them to disgorge and got them all into hot baths as speedily as possible, I lost eight out of my forty-three vibakaris, the Bordeaux snake, both slow-worms, one dark green, and one garter— hideous evidence of the baleful virulence of the *fœtidæ* and the lamentable lack of instinctive discrimination on the part of the reptiles. Evidently the conspicuous coloration of this worm is not to be added to the list of protectives, since the creatures to which it is most exposed, frogs, toads, etc., prey on it with avidity. Serpents, as a rule, will not take worms unless they have been "taught" to do so — such tuition, however, being quite practicable. ARTHUR STRADLING, C.M.Z.S., etc.

Watford, England.

BOOK-REVIEWS.

Destructive Distillation. A Manualette of the Paraffin, Coal Tar, Rosin Oil, Petroleum, and Kindred Industries. By EDMUND J. MILLS, D.Sc. (London), F.R.S. Fourth edition. London, Gurney & Jackson. 200 p. 8°.

HISTORICALLY an ancient industry, this branch of scientific investigation has always proved most absorbing and as early as the sixteenth and seventeenth centuries upon it was concentrated the whole attention of the laboratory. Heat was considered in the medium of a reagent and in the retorts of the alchemists vegetable, animal, and mineral matter was subjected to "analysis." The above work by Dr. Mills, now in its fourth edition, with improvements including the results of much additional research, is founded upon a course of lectures delivered in Anderson College, Glasgow, and is illustrated by actual inspection of many of the processes referred to. Since the appearance of the first edition, in 1887, the book has found its way into the hands of every technical student and every chemist the world over. Dr. Mills has become a recognized "authority" upon the subject, deserving and receiving the highest praise for his patient, earnest research. The main sections of the book are indicated by the title, the kindred industries embracing Wood Tar, Asphalt, Ozokinite, Peat, Lignite, Bone Oil, Fixed Oils, Cellulose, etc. Appendix A provides a description of the six principal types of shale retorts, each being illustrated by a figure, and appendix B furnishes a complete bibliography of destructive distillation in its modern development. A neat summary describes the application of heat to cellulose and kindred bodies as leading to cumulative resolution, the process being in principle the same whether performed by nature or by human contrivance. At a high temperature the liquid distillate is characteristically "aromatic"; at a low temperature "fatty." In either case the persistence of the n C_8 group can be freely traced throughout. Inasmuch as a chemical equivalent for much of the "temperature" can be found in "time," petroleum may appear in rocks never actually igneous; and we can understand the occurrence of degraded hydrides, such as turpentine and other "aromatic" compounds in living trees.

C. P.

Poole Brothers' Celestial Handbook and Planisphere. Compiled and edited by Jules A. Colas. Chicago, Poole Brothers.

THE above publication is made up of two parts, the Handbook and the Planisphere.

The planisphere consists of a stiff, circular cardboard, about twenty inches in diameter, upon which has been engraved all the principal constellations that can be seen from the North Pole to 50° south declination. Fastened to the circular disc is a frame made of the same substance, and formed so as to project the horizon upon the sky, and also to assist in noting the days of the year. The planisphere is exceedingly handy, as the explanations printed upon it suffice for finding the approximate time at which any celestial body rises, culminates, or sets. In the hands of the learner of the constellations the planisphere is a great improvement upon the ordinary star-maps.

The handbook, which serves as a companion to the planisphere, contains in a neat form references to the principal constellations, the interesting double stars, the same being neatly illustrated, and the brighter nebulæ and star clusters. Short notes are given which contain the names, magnitudes, distances, and colors of the doubles. Tables are also to be found, giving the names of the bright, fixed stars, the principal binaries, colored stars, and those having a parallax. These are followed by short sketches of the phenomena of shooting stars, the principal periodic comets, and those that have an interesting history, and, last, the principal planets.

As Mr. Colas has simply compiled the remarks in the handbook, it is possible for one to find certain statements that may be questionable. He has probably fallen into pitfalls by following too closely some of the writings of Flammarion. For example, the statement that the earth and moon as seen from Sirius would appear as a spot is exceedingly misleading. A simple calculation would show that from the boundary of the solar system, that is, from Neptune, the moon as seen from that point would never depart more than 18 seconds of arc from the earth.

We note that Arcturus and Alpha Bootis are mentioned as if they were two distinct stars. This is probably a slip of the pen, as well as the statement that the constellation Cassiopeia can be seen every day.

The author has carried his book well down to date, as mention is made of Barnard's discovery of the fifth satellite of Jupiter, and Anderson's discovery of the new star in Aurigæ.

In our opinion the statement quoted from Flammarion's "Les Etoiles," that Baron Dembowski observed the yellow companion to 15 Lyncis covering the blue one by one-fourth of the former's diameter, is exceedingly doubtful.

A star, as seen in the most powerful telescope, is a point of light, never a disc, and such statements as the above are, to say the least, misleading.

The compiler has, in a note on Neptune, raised the question of priority of the discovery of the position in which the outermost planet would be found.

CALENDAR OF SOCIETIES.

Agassiz Scientific Society, Corvallis, Ore.

May 10.—Dumont Lotz, Food Adulterants.

May 31.—Wallis Nash, Darwin's Life and Works.

We would say that perhaps Mr. Colas was led by sentiment to make the unqualified statement that all the glory of the great mathematical computation, for that it was, rests with Le Verrier. The statement, however, is very questionable.

In glancing through the pages of the handbook, we can but admire the neatness with which the text has been arranged, and the selection of interesting objects gives a field of wide range.

We would recommend these two publications to the ambitious learner of the starry heavens as fit companions for his watches.

G. A. H.

Missouri Botanical Garden. Fourth Annual Report. St. Louis, 1893. Plates. 226 p.

THIS is a handsomely gotten up volume, in which we find several papers of interest. The report of the director, Dr. William Trelease, shows a flourishing condition of affairs. The herbarium now contains some 203,000 specimens, and the library over 11,000 volumes and pamphlets. The valuable library of Dr. Lewis Sturtevant was received as a donation, and included many manuscript notes. The only condition was that he retain the books during his lifetime or for so long a time as he needed them. The third annual flower sermon and the proceedings at the third annual banquet are reported in full. In the latter we find many interesting statements respecting Mr. Henry Shaw, the founder of the garden. There are also two scientific papers: "List of Plants Collected in the Bahamas, Jamaica, and Grand Cayman," by A. H. Hitchcock, and "Further Studies of Yuccas and Their Pollination," by Wm. Trelease.

Professor Hitchcock discusses in his introductory remarks various principles of nomenclature, finally taking 1753 as the starting point, in accordance with the recommendation of the Botanical Club of the A. A. A. S. The double citation plan is followed, the original author of the name being placed in parenthesis, the name of the author of the combination coming last. The original spelling of the specific name has been followed, except in those cases where typographical errors were clearly apparent. Notes are given on many of the species and several new ones are described. The relationship of the flora to the surrounding region is also discussed, and notes are given upon geographical distribution. Dr. Trelease's paper gives descriptions of the various species of *Yucca*, and mentions the mode in which some of them are fertilized. He agrees with Professor Riley that fertilization takes place through the intervention of species of *Pronuba.* He considers *Yucca whipplei* to belong to the genus *Hespero-Yucca*, the common Spanish Bayonet of San Bernardino region being considered as var. *graminifolia.* This variety is fertilized by a new form of *Pronuba*, described as *P. maculata*, var. *aterrima.*

J. F. J.

THE lists of expectant graduates of Sibley College, Cornell University, in mechanical engineering, are just published by the registrar. The total number of candidates for the first degree is just one hundred; for the second degree, twelve come up, and are already, in most cases, through their examinations. Two or three of the first-degree men may fail; but the total will exceed one hundred. The graduating class, for the whole university, inclusive of its law school and special courses of four years' length, will be considerably above three hundred. There are about two hundred graduate students on the catalogue, a large proportion of whom take their degrees this year. Of these, many take the first degrees in Sibley College, where the custom of going through the regular "general courses" before entering the professional school is rapidly gaining ground, and is greatly encouraged by the authorities — where the student can afford the time and the expense.

—The Contemporary Publishing Co. have just issued an important work on the subject which to-day most attracts the attention of geographers: "The Arctic Problem and Narrative of the Peary Relief Expedition," by Professor Angelo Heilprin, the leader of the expedition.

SCIENCE

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SCIENCE

NEW YORK, JUNE 16, 1893.

ORGANIC COLOR.

BY F. T. MOTT, F.R.G.S., LEICESTER, ENGLAND.

THE colors of plants and animals may be due (1) to diffraction and interference from striated surfaces, as in some iridescent feathers and shells; or (2) to pigments whose function seems to be especially to give color; or (3) to the molecular structure of the tissues themselves.

The first of these causes is not a physiological phenomenon. It can be equally well exhibited by artificial means. The second and third are phenomena connected with the most fundamental elements of organic life.

The colors of tissues or pigments depend, of course, upon the portion of white light which is reflected from them. The white light of the sun falling upon an object is in all cases partly absorbed and partly reflected. This "light" is merely a series of undulations or ripples running through the ether, the ripples being of various sizes; and the color of the object depends altogether upon the quantity and the size of the ripples which are stopped or absorbed. The cause of the differences in color among various organic objects must lie in the varying power of absorption possessed by the tissues. The petals of a pink rose absorb the undulations of medium size, and reflect both the larger and smaller ones; while the petals of a scarlet geranium absorb the small undulations, reflecting only the medium and larger ones. But why should this difference exist in the absorbing power of the flowers?

Here is the *crux*, which is still a crux in spite of the much-lauded hypothesis of insect selection. Insect selection may account for something, but there is much more for the explanation of which it is quite unavailable. The supposed all-sufficiency of this hypothesis is completely answered by the fact that insect selection cannot come into play at all in the production of color until the plant has already shown its power to produce that particular color quite independently of insects. It is evident that the color is due not to insects, but to some inherent capacity of the plant, and that a plant which could produce a small pink petal could equally well produce a large one under suitable conditions. The utmost that can be claimed for insect selection is that it may accelerate the production of the large and showy petals by giving to the plants which show their tendency to produce such petals better opportunities of developing that tendency. The crucial question remains, What is it which gives to a plant this tendency to produce colored petals?

There are two fundamental laws of nature which, in the attempts to solve this question, have not been sufficiently regarded, viz., (1) the law of the concentrating wave, and (2) the law of sympathetic vibration. By studying the action of these laws, it seems possible to carry the solution at least one step further back from the position in which it now stands.

We are familiar with the simple wave of oscillation, as in the pendulum; and also with the wave of undulation, as in sound, light, and the spreading rings upon the surface of water; but the wave of concentration is less familiar to us, and has been less carefully studied.

All organisms are illustrations of this particular wave-form. As the kinetic energy of the swinging pendulum increases as it approaches the centre of its curve, and then again diminishes, so the energy of the organism increases as it approaches the climax of its life, and then diminishes. But, while the swing of the pendulum is a simple process, involving merely the alternate change of form of a given amount of energy, the growth of an organism is almost infinitely complex. The accumulation of energy which this growth represents takes place slowly and intermittently by the drawing-in of outlying units to a centre, the energies of these units being aggregated and assimilated to the forms of the original germ. This accumulation of energy continues, if not violently interrupted, until a certain definite degree of concentration is attained, after which a gradual dispersion of the energy sets in until the organism dies of old age. All organic individuals have a limited period of life, and pass through a similar series of periodic changes, gradually attaining a climax of concentrated energy, which is afterwards gradually dissipated. The ascending and descending phases of the wave are rarely equal, but probably always equivalent. One may be long and slow, the other short and rapid, or *vice versa*. The variations in this equivalence may be almost infinite.

It is clear that this law of the concentrating wave controls the life-history of every individual. It is not so demonstrable, but is, nevertheless, highly probable that the same law controls the development of species, genus, order, and class, and is, in fact, the fundamental law of the unfolding universe, at all events, of the organic phase of it.

What is the relation of this law to organic color?

Let us assume the general correctness of the molecular theory as the nearest approach which science has yet made to an explanation of the structure of matter.

Throughout all substances in which the temperature is above the point of absolute zero, as in all living organic substances, the molecules must be in continual motion. But, being linked together by powerful "affinities" (whatever these may be) into definite groups, and in all solid bodies packed very closely together, their motions cannot be altogether free. There must be certain directions in which they can move, and certain others in which they cannot move; and the group of motions possible to them will differ in each compound chemical substance. There will be a definite group of motions possible to the molecules of albumen, another to those of starch, etc. There will also be a definite but more complex group of motions possible to the totality of molecules which make up each kind of tissue, as skin, muscle, bone, nerve, etc., and a still more complex group possible to those which make up each organic individual. This characteristic grouping of possible motions may be called *molecular rhythm*.

One of the specific functions of organized matter is that of assimilation and growth. There is no organism which can assimilate matter of every kind. The power of selecting food depends upon the molecular rhythm of the organism. Substances whose molecules have possible motions which harmonize with those of the feeding organism can be assimilated and made to add their energies to the stock of that individual, causing it to "grow" in size and in accumulated energy. Substances whose molecular rhythm will not allow of such assimilation are not available as food for that organism.

The organism continues to feed, to assimilate, and to grow, in accordance with the law of the concentrating wave up to a certain point, but there is a limit which it cannot pass. Having reached that limit, concentration ceases and dispersion sets in. What determines that limit?

The phenomenon of growth is not a passing function, not a mere reception and assimilation of energy. Every organism is more or less active and parts with energy as constantly as it receives it. Growth is the result of the balance between these two processes. As long as the organism assimilates more energy than it dissipates by its various activities, growth continues.

The limiting-point is that epoch in life at which assimilation becomes so small as to be overtaken by the dispersion. But what diminishes the power of assimilation?

The molecular rhythm of any organism is necessarily of limited compass. In the embryo only the main chords are struck; the molecular motions are comparatively free, and some amount of modification is then possible. But, as the intervals are filled up by the harmonious motions of the assimilated molecules, the rhythm becomes fuller and, at the same time, more fixed, till a point is reached at which little more permanent assimilation is possible. This is the climacteric of the life of that organism. The limit is determined by the original outlines of the molecular rhythm in the embryo, slightly modified by the amount and the quality of the food assimilated during growth.

We have now to consider the action of the law of sympathetic vibration on the growing organism.

A tightly stretched wire may be made to give out a musical note by sounding near to it a certain note on a violin. The group of vibrations produced in the violin is communicated to the air, and the air communicates it to the stretched wire. It can only do this if the condition of the wire is such as to make that group of vibrations possible to it. If only some part of the group is possible to it, it may take up that part and reject the rest. This is a case of molar, not of molecular, vibration, but the same law operates in the case of the molecular vibrations of radiant heat and light, which are communicated through the ether. Such of these vibrations as are possible to some of the molecules of the substance receiving them will be absorbed, while the rest will be transmitted or reflected. The energy of those vibrations which have been absorbed will go to increase the amplitude of the vibrations by which they have been assimilated. They do not, as in the case of food, introduce fresh molecules with harmonic vibrations which occupy vacancies in the established rhythmical system, but they give increased force to certain existing vibrations, and thus alter the balance of forces in the established system. The vibrations so reinforced will acquire a more or less controlling influence throughout the total group, and there will be a tendency among the vibrations of less energy to fall gradually into the swing of these controlling vibrations. Thus the molecular rhythm of the total group may become modified within certain narrow limits by the light in which the organism lives, if that influence is continued for a sufficiently long period, as well as by the food which it assimilates.

It is evident that the tendency of the action of these two laws — the law of the concentrating wave and the law of sympathetic vibration — must be to enhance the energy of the original molecular rhythm of the embryo, to make it fuller, richer, more definite, and less capable of further modification as it approaches its climacteric, while, at the same time, it is simplified and cleared of a number of vibrations differing only slightly from the controlling ones, by the bringing of these gradually into unison.

The bearing of these results upon organic color remains to be discussed.

It is a well-known physiological rule, with possibly a few exceptions, that organisms in their embryonic and early stages are less brightly colored than they afterwards become. This is plainly seen in the young of birds. Germinating seeds are nearly always of a dull white, which means that about an equal, though small, quantity of every vibration in the sunlight is able to be absorbed. The color of the first leaves is the primary green, indicating that the plant, while able to absorb a larger proportion of the white light, is becoming less able to absorb the vibrations of medium size. As the foliage becomes developed, varied tints of green, with some reds and dull purples, are reflected, showing that there is a still less varied capacity for absorbing. Finally, in the blossom of the higher orders of plants, of which the color is nearly always some shade of the brilliant secondary hues, scarlet, orange, yellow, blue, or rose, it is evident that a great simplification of the molecular rhythm has taken place, so that the absorption is chiefly confined to one group of the light-vibrations, while large proportions of the other two are reflected.

This process of simplification in the molecular rhythm, as the concentrating organic wave approaches its climacteric, may be traced in various phases of vegetable and animal life. In a flowering tree or shrub there are three great systems of structure, viz., the stem and branches, the foliage, and the inflorescence; and these represent three stages of advancing development and vitalization. The color reflected by the system which stands lowest in the scale, the stem and branches, is generally dull green, brown, or plum, indicating that nearly all the light which reaches them is absorbed. In the uniform green of the foliage a certain amount of simplification is shown; and in the brilliance of the inflorescence we see the greatest simplification attainable by that species.

Those families of plants in each great class which have the lowest organization display, as a rule, the least color. The Coniferæ represent the earliest type of existing trees, and are nearly all sombre in coloring. The Amentiferæ stand next, and, though brighter in foliage and much more varied, do not attain to colored blossom. The numerous orders of "flowering" trees and shrubs are of most recent origin, and represent the highest phase of development in the great concentrating wave of vegetable life. The fern form is a very ancient one, and has never, even to the present day, developed much in the way of color. But, if it be true that the recent Monocotyledons are derived from the ferns, they may represent the simplified condition of the fern wave, and among them are many of our most brilliant flowers. The Fungus form must probably be an ancient one also, and among recent Fungi many brilliant hues are developed, which can have no connection with insect choice.

In the large class of birds, the Ratitæ (ostrich, emeu, rhea, cassowary, and apteryx) are the nearest to the reptilian type, and are all dull in color, with the exception of the head of the cassowary, which may have a special explanation. The gulls and albatrosses, which seem to stand next in order of development, are brighter in plumage, but with very little trace of the secondary hues. Brilliant color is almost confined to the more recent insessores.

Among mammals, the early types of elephant, rhinoceros, hippopotamus, hog, etc., are quite without color, while the Carnivora and the Ruminantia, more characteristic of the present epoch, have developed some warmer tints. The Mammalia, as a whole, however, constitute the most recent of the great classes, and it has not yet reached the stage of brilliant coloring. On the other hand, the Mollusca are extremely ancient, and among existing Mollusks a large number display the most brilliant coloring in all the secondary hues.

The final result of the foregoing argument is that the gradual development of organic color is a physiological necessity; that brilliant coloration is a mark of the maturity of some organic force-wave, in which the molecular rhythm has reached its maximum simplification; and that the effect of insect selection in the development of colored flowers is comparatively small.

The very curious appearances of mimicry, which are often supposed to be protective, but of which a large proportion seem to have no such function, may probably be attributed to sympathetic communication of the vibratory motions, which must be passing through the ether in all directions in the neighborhood of organic life.

An animal which spends its life in proximity to the brown bark of trees will be under the influence of the molecular rhythm of such bark, and may have its own molecular rhythm gradually modified by sympathetic action, or it may entirely resist such modification according to its fundamental molecular structure.

The possibility of sympathetic modification of weaker vibrations by a more energetic one, with which they are nearly synchronous, is clearly suggested by the action of a sensitive flame, which, while it is unaffected by vibrations which are palpably discordant, shows itself sensitive to such as are nearly, but not quite, in unison with it. An organism differs from a flame, as from a fixed string or a tuning-fork, in the fact that it is constantly growing, and the added molecules supply material which may be easily amenable to modification.

If the hypothesis here described should be found to explain satisfactorily the phenomena of organic color, the corollaries to be deduced from it will be far-reaching and of much interest, and will apply to beauty of form as well as to brilliance of color.

The world in its early stages must have been sombre and un-lovely. The forests of the coal period, when the great Sycopods were at their climax, may have exhibited some brighter greens, with tendencies towards yellow or glaucous tints; the shells of the Amonites, in the Liassic seas, may have been colored to some extent; but the great concentrating wave of organic life in its progress towards an unknown climacteric must yield an ever-increasing glory of color and form to the surface of this planet.

The beauty of summer as we know it now, though it has never been paralleled in the past, will be as nothing to the blaze of brilliance which shall mark the summers of the future.

MINOR PHONETIC ELEMENTS OF MAYA HIEROGLYPHS.

BY HILBORNE T. CRESSON, M.D., PHILADELPHIA.

THE Maya graphic system, the earliest steps of which began as picture-writing, was the natural outcome of a desire to record knowledge made by a people who may be classed as the most intelligent and civilized of the American race. The language they used, monosyllabic and rich in homophones, is in fact quite as unique as the development attained in their graphic art in its progression from thought-writing to a certain degree of sound-writing, which has been denominated ikonomatic (D. G. Brinton; "Essays of an Americanist"), "writing not by things but the sound of the names of things." Scientific research has shown that there is less reason than formerly to doubt Landa's suggestion, and that of more recent authorities, in regard to its phoneticism, which is without doubt of a higher standard than has hitherto been supposed.

Dr. Cyrus Thomas has well said in a recent article (*Science*, Vol. XX., No. 505, pp. 197–201) that ". . . although we may know the chief phonetic element of each part of a compound character, we cannot interpret the whole. This will undoubtedly be true unless there are indications of the minor elements." Want of complete lexicons containing words that correspond to the archaic language of the hieratic and demotic script is also a difficulty which must be considered in the work of interpretation — yet with the almost insurmountable obstacles that exist, it may be said that progress has been made in the work. A study of archaic symbols and ideographs has been made in order to determine, if possible, how certain elements used in the Maya graphic art have been derived — in most cases, we think, from the animate and inanimate forms of nature, or from things invented by man for his necessities. In these researches we must not overlook the superstitious offices of early people in symbolizing ideas — basketry, pottery, and rock-scratchings affording many valuable hints of the changes from the nature-derived elements to the more conventionalized, used ideographically or as phonetic elements, be they chief or minor elements. The figures employed may have been in many cases mere conventionalities, but there is evidence in the work of the Maya scribes that the motives of this convention are based upon primitive realism — for they were but simple-minded children of nature, keen observers of her endless variety of forms, quick to adopt the motive she suggested where it could be utilized to serve a desired purpose.

That these assertions are not the outcome of mere theory we shall give, presently, a list from which we think have been derived what we deem to be phonetic elements of the Maya glyphs. Nature is the source from which have been derived the phonetic elements used by us in endeavors to interpret the Maya glyphs, and it may be said that the results are encouraging; the ideographic suggestion and the chief phonetic element having been obtained, recourse can be made to the "minor phonetic elements" — one analysis being a check to the other.

We have progressed far enough to feel sure that the Maya graphic system is based upon picture-writing, a necessary outcome in the progression of all graphic systems, from thought-writing to sound-writing. The majority of the glyphs, as we find them, whether hieratic or demotic, are associated with ideographs, many of these having combined with them phonetic elements which appear as glyphs or component parts of other glyphs — be they single glyphs or component parts of compound glyphs. An excellent example of an ideo-phonetic design is that of Hun Cimil, the god of the Maya hades (see plate C, Codex Peresianus, or Codex Cortesianus, p. 16). It will be remarked by consulting this first-named Codex, de Rosny's edition, that the abdomen of this figure is composed of the day-signs of Landa, the elements composing which, according to the analyses that we have made, are phonetic. In fact, we have found that sixteen out of twenty of the Maya day-signs, and many of their variants, are phonetic. We firmly believe that they will all prove to be phonetic when future study shall have demonstrated more exact methods of analysis. Between the legs are phonetic elements and the ideo-phonetic head (of a cayman?) to the right of the knee of the figure is connected with the glyph of Cimi. Around the ankles are designs that appear in the Codices, at times, as glyphs. The majority of the components of the head, arms, ornaments of the wrists, and the implements held in the left hand also appear as phonetic elements in Maya script. It is for this reason that the term "ideo-phonetic" has been used for the drawings, as they are composites conveying ideographic suggestions — the ideograph itself being intermingled at times or composed of phonetic elements that appear, as we have said, as the component parts of other glyphs. (See figure of Hun Cimil, pp. 53, 15, Cortesianus; p. 14, Tro., also, ibid, 3, 29, 14, 34.) Hundreds of other examples might be quoted, but as they abound throughout the Maya graphic system this will not be necessary.

The following list will indicate the animate and inanimate forms of nature and inventions of man which, it is thought, suggested certain phonetio elements of the Maya graphic system, viz.: —

Sky	Animals	Head	Huts
Sun	Birds	Eyes	Houses
Wind	Fish	Nose	Idols
Water	Reptiles	Jaws	Implements of war and of the chase
Lightning	Insects	Mouth	
Earth	Appendages of animals, birds, insects, and crustaceans, etc.	Teeth	Clothing
Fire		Ears	Ornaments
		Tongues	Pottery
		Arms	Colors
		Hands	Grinding-stones, etc
		Feet	Tortillas
		Thighs, etc.	Maize
			Honey, etc.

At a future time examples will be given of analyses of the Maya glyphs, the ideographic suggestion of the glyph, if any, and the drawings which accompany them, together with minor phonetic elements being considered. To give examples in this article, already longer than intended, will be impossible. The results obtained from the list to which we have assigned certain phonetic values, and used in interpretation, are encouraging — proving to our own satisfaction that minor phonetic elements undoubtedly exist in the Maya graphic system. These minor elements have in many cases been considered as meaningless decorations, component parts of ideographs. Many of the minor elements are so combined together that they are difficult to trace. Errors and omissions of the Maya scribes at times increase these difficulties and require especial study and aptitude for such analyses.

The colors, red, yellow, and black, seem to be used in the Peresianus, with phonetic and ideographic value (see plates xxiii. and xxiv.), and are combined at times with the minor elements of the glyphs. It is probable that colors are also used with a certain ideographic and phonetic value in the other Codices. Interesting combinations are to be remarked in the connection of the consonants with the vowel sounds, Landa suggests *ma*, *me*, *mo*, for the phonetic value of a certain glyph, and this method of assigning several phonetic values to a glyph is quite common; determinatives in many cases being used to indicate the value intended. Where these determinatives are wanting it is necessary to try the principal phonetic element through the vowel sounds. The principal phonetic value is, however, generally given by the minor elements of the glyph.

If the system and list of phonetic values adopted by the writer in the interpretation of the Maya glyphs, be correct, it suggests a higher standard of phoneticism than can well be accorded to a people, who, though the most highly civilized of the American races, were, we are to suppose, but an Indian people. Judging by the testimony of the minor phonetic elements there was more method and arrangement in these than we can expect from a Maya — Indian — scribe, and for this reason the writer is prone to condemn his own work, yet repeated trials with the phonetic list arranged by him have given such good results that he is of the opinion that with careful research some good results may accrue that will be of value to students of Maya and its paleography.

It may be added, in conclusion, that the glyph known to Maya paleographers as that of "The God with the Old Man's Face," has been analyzed — its minor elements suggesting that it is that of Hoobuil-Kanil-Bacub. The suggestion given by the minor elements is "Ho-ka-n-ba-ka." The association of this glyph with "The Bee-Keeper's Narrative" of the Troano, lends a strong probability that the interpretation is a correct one, and that a former analysis attempted was erroneous.

This article is intended to be suggestive. The writer holds himself in readiness to modify any of the statements made, if the contrary be proven, or he finds in the progress of his researches that new evidence obtained proves former suggestions to be erroneous, thus only can we diminish the field of error and enlarge that of the truth.

Mexico, Jan. 30.

PRELIMINARY NOTE ON THE DISTRIBUTION OF PLACE-NAMES IN THE NORTHERN HIGHLANDS OF SCOTLAND.

BY JOHN GUNN, ACTING SECRETARY, ROYAL PHYSICAL SOCIETY.

NOTHING, at the present day, exhibits in a stronger light the effects of the Scandinavian occupation of the Northern Highlands of Scotland than the frequent occurrence of Norse place-names. And this, it must be remembered, in spite of the fact that the invaders were never permanently able to establish their own tongue as the language of the country, except in the Orkney and Shetland Islands (which form no part of the Highlands) and perhaps in certain areas in the Hebrides. The Celts have always had a wonderful power of assimilating to themselves alien races which come among them, and although subdued and ruled over by the vikings and their posterity down to the present time, caused their conquerors to adopt their language, dress, laws, and customs. Yet the number of places named by the Norsemen and still retaining these names is very remarkable.

As to general distribution, these names are more numerous along the coasts than inland. The vikings did not care to settle far from the sea, where impassable mountains and thick forests, inhabited by a warlike and hostile people, hindered convenient access to the sea. Thus as we retire from the sea-shore the place-names assume a more and more distinctly Celtic character. But even in places where the Scandinavian nomenclature more persistently prevails it is interesting to note how only the larger areas and more striking features of the landscape bear Scandinavian names. A parish, with its streams, estates, local districts, and large farms may bear names derived from the Norse, but those of crofts, burns, pools in the rivers, boulders, etc., have, as a general rule, purely Gaelic designations, many of them, doubtless, dating from a much later period than that of the Norse occupation. In this connection it is somewhat curious to observe how few mountains bear Scandinavian designations; forming bold features in the scenery, most of them must have been well known to the vikings, whose names, if they ever named many of them, have come down to us in so very few instances.

Good examples of the facts above stated may be gleaned from the topography of the county of Caithness, as there the vikings found a surer and more permanent footing than on any other part of the mainland of Scotland. The name, Caithness, is itself compound, but was undoubtedly given by the Scandinavians, and signifies "the headland of the Calaibh," the last-mentioned word being the name of the Celtic tribe which owned the district, and resisted, although unavailingly, the invasion and partial conquest of their ancient possessions. Caithness is divided into ten civil parishes, viz., Thurso, Olrig, Dunnet, Canisbay, Bowes, Wick, Watten, Halkirk, Latheron, and Reay. All these are of Norse origin except the two last mentioned, and all, with the exception of Halkirk, have sea-coasts. Latheron and Reay are Gaelic, and these districts, along with the western portion of Halkirk, were the places in which the aborigines were left to dwell in comparative peace. Yet here, all along the coasts, we find numerous Norse derivatives, such as Skail, Lylester, Forse (occurring also in the form Forso), Berriedale, and many others. In the western Halkirk area, which lies far from the sea, we can only remember two Norse names, viz., Glutt and Rumsdale. In the Scandinavian area, however, we discover the aboriginal element to be remarkably strong. The Gael was, and is, naturally facile in topography, and gave a name to almost every object, natural and artificial, which came under his notice in a fairly permanent form. A constant pool of water, a boulder of peculiar color or somewhat uncommon shape or size, a corner of waste land, a ditch — all were named. He frequently added a word from his own vocabulary to a Scandinavian root, using oftenest *Ach* (a field) or *Bal* (a town or farm) in this connection. Thus, we get such compound forms as Achalipster, Achkipster, in which examples we have, in conjunction, the Gaelic *ach* and the Scandinavian *ster*, both words having the same meaning, and making the names tautological.

These remarks are merely intended as an introduction to a more particular examination of a subject of particular interest and of sufficient importance to have induced Sir Charles Wilson, Director of the Ordnance Survey, to request the coöperation of the Scottish Geographical Society in revising the place-names for new issues of the Survey maps. The council of the society thereupon nominated a committee to undertake the work; and this committee, under the presidency of Dr. James Burgess, C.I.E., is now engaged in an examination of all the place-names in the Highlands, and, where there is any doubt, authoritatively fixing the correct form of spelling.

NOTE UPON THE ABSORPTION OF SULPHUR BY CHARCOAL.

BY WILLIAM P. BLAKE, SHULLSBURG, WISCONSIN.

IN tearing down some heaps of pyritic zinc ores, where heap-roasting to expel the sulphur from the pyrite had been attempted, a part of the wood used as fuel was found at the bottom of the heap not only carbonized, but portions of it, such as small limbs of trees, and looking like ordinary charcoal, were saturated with sulphur. The original form of the wood and its structure, its grain-rings of growth, bark, etc., seemed to be perfectly retained, but the weight and solidity of the masses at once showed that some change had taken place, and this change it was easy to prove was due to the presence of a large amount of sulphur penetrating every part.

The fragments of this sulphurized carbon are hard and brittle, and break most readily at right-angles to the length of the original tree-limbs. The color is very nearly that of ordinary charcoal, but lacks the lustrous black, having instead a grayish-black shade, and when the compound is cut or scratched with a knife, it exhibits a sub-metallic lustre. Specific gravity 1.60.

In the May number of the *American Journal of Science* Professor W. G. Mixter[1] describes the deportment of charcoal with the halogens, nitrogen, sulphur, and oxygen. He points out the extreme difficulty in obtaining fairly pure amorphous carbon, it so tenaciously holds such elements either occluded in its pores or in combination. His experiments were conducted upon three varieties of amorphous carbon, viz., sugar charcoal, lamp-black, and gas carbon. He found that charcoal after exposure to chlorine retains a considerable quantity at high temperatures; one experiment upon heated lamp-black showing an absorption and retention of from 14.3 to 15.5 per cent, while gas carbon, ignited in chlorine and allowed to cool in a current of dry nitrogen, failed to absorb chlorine. He concurs with other recent writers on this subject that carbon and chlorine do not unite directly, but states that chlorine does combine with carbon at high temperatures when hydrogen is present in the carbon, the hydrogen being apparently replaced by chlorine; for, while gas

[1] Amer. Jour. Sci., Third Series, xlv., No. 269, May, 1893, p. 263.

carbon containing 0.035 per cent of hydrogen does not take up chlorine, sugar charcoal, with 0.07 per cent, does take it up.

The experiments with charcoal and sulphur showed the absorption of from 20 to nearly 47 per cent in charcoal containing much hydrogen and oxygen, while nearly pure amorphous carbon takes up but little sulphur. Professor Mixter regards the sulphur as chemically combined with the carbon, in his experiments, and cites Berzelius in support of this view.

THE EARTH AS AN ELECTRICAL CONDUCTOR.

BY A. F. MCKISSICK, ALABAMA POLYTECHNIC INSTITUTE, AUBURN, ALA.

STEINHILL, at Munich in 1837, was the first to discover that the earth might be used instead of a return wire, contact being made to the earth at the two ends by means of metal plates sunk in the ground. He discovered this while experimenting on the Nürnberg-Fürther railroad for the purpose of determining whether the track could be used for telegraphic purposes. He noticed that the current passed from one rail to the other and the idea to use the ground as a return circuit occurred to him, which he afterwards found to be perfectly feasible. The earth is almost universally used as the return circuit in telephone and telegraph lines. While it is true that in the former a complete metallic circuit is sometimes found, it is not on account of the failure of the earth to conduct the current but for the purpose of diminishing the induction, caused by the presence of electric light and power circuits.

The earth-plates are made of zinc or copper and are sunk in moist earth, in a spring, or in the bed of a river. It has been generally considered that the earth offers no resistance at all as its cross-section is so large, although its specific resistance may be very high. While the resistance of the earth may be neglected when we have to deal with telephone and telegraph circuits, we must consider its resistance when it is to be used for conducting currents of large volume.

The element of danger to life and property forbids its use as a return in commercial lighting and motor circuits.

In street railway circuits, however, the earth is used partly as a return. It has been found that the earth alone, as a general rule, offers too much resistance, so that it is now almost the universal custom to use in connection with the earth the rails bonded together and also a bare copper wire. I had occasion during the past year to notice very closely this resistance in installing a motor at the experiment station of the A. and M. College of Alabama. I had expected to use the earth as a return, but owing to the very high resistance had to abandon this idea. It was with the idea of finding out how much the resistance of the earth near this motor was, that the following experiments were made.

An earth-pit was dug six feet deep, eight feet long, and two feet wide, at each end of the line running from generator at college to motor at experiment station. This line is by measurement three thousand feet long. A plate of copper, seven by two feet, and a plate of tin of same dimensions, soldered to a No. 0000 B and S wire were used as the earth-plate at each end. The plates were packed firmly with charcoal and iron filings and the pit filled with old iron. The water rose in one of the pits to a depth of two feet. With all connections soldered, the resistance measured by a Wheatstone bridge was found to be 102 ohms. Supposing the earth connection was not a good one at each end of the line, an additional earth connection at each end was made by sinking a large piece of iron in a well. With this additional connection there was no appreciable difference in the resistance. Connections to the earth were then made at different distances from the college by connecting one end of a wire to the overhead wire, the other end soldered and tied to a piece of iron six feet long, driven down flush with the ground. These distances were respectively 500, 1,000, 1,500, 2,000, and 2,500 feet from the college. These connections were made at different times, always removing an earth connection when its resistance had been measured. The resistances in the same order were 307, 567, 153, 707, and 217 ohms. The comparatively small resistances of stations 3 and 5 are probably explained by the fact that they were located near branches (small streams).

From these results we may conclude that the resistance of the earth is a very unknown quantity, and the assumption that the resistance of the earth can be neglected in any soil is an unsafe one when the object in view is to transmit currents without very much loss.

A VALUABLE FLORIDA DEPOSIT.

BY THOS. R. BAKER, PH.D.

THERE occurs near Bartow, Fla., and at other points as far south as Haines City a geological deposit which has recently been found to be very valuable as a material for covering the sandy side-walks and streets of Florida towns. It is popularly known in South Florida by the name "clay," but consists essentially of sand, clay, and oxide of iron, the proportions of which, determined by chemical analysis, are given in the following table:—

	Per cent.
Moisture	4.20
Silica	69 03
Aluminum silicate	18.21
Iron oxide	8.53
Calcium carbonate	Trace.

Geologically considered, the deposit is a sandstone rock, and, although it has to be quarried from its bed, it almost completely disintegrates in the quarrying, and needs no further preparation to fit it for the use to which it is applied. It is of a reddish color, due to the presence of oxide of iron.

The material is simply spread over the side-walk or street to which it is to be applied to the depth of several inches, and then sprinkled with water, and rolled with a heavy roller. After being walked upon and driven over for a short time it becomes very compact, and fully as hard as it is in its native bed.

The most valuable constituent of this material, when used as a covering for roads, is undoubtedly the oxide of iron, which acts as a cement, rendering the material capable of becoming compact and hard. That the iron serves this purpose was verified by removing it from the compound, and subjecting the mixture of the remaining constituents to tests that had been applied to the original material.

The adaptation of this deposit to the improvement of roads was first brought to notice by the South Florida and other railroad companies, who used it for the improvement of railroad crossings, drive-ways about stations, etc , and the first extensive use made of it for streets and side-walks was by the city of Orlando about a year ago. It has given excellent satisfaction in Orlando, nothing having been done for the place for years that has so improved it. It has been the means of converting streets so sandy that travel over them was very slow and difficult into drive-ways over which travel is easy and pleasant. Now, on Orlando streets, vehicles and horses' hoofs have the familiar rattle and thud that are heard when driving over a macadamized road. It is the opinion of those who have studied the subject that geological deposits like the one here described are of very rare occurrence.

NOTES AND NEWS.

THE next meeting of the Australian Association for the Advancement of Science will be held in Adelaide, South Australia, commencing on September 25th, 1893. The meeting in Adelaide will be presided over by Ralph Tate, F.L.S., F.G.S., professor of natural science at the University of Adelaide. At the time fixed for the meeting, South Australia will be at its best. There is no better time at which to visit Australia than when spring is merging into summer. To naturalists, this time of year is specially attractive, and these may be reminded that at the meeting of the Association they will come into contact with men of like tastes from all parts of Australia. Should visitors wish to prolong their trip, they will do well to visit during the months of October and November the principal objects of interest in the mainland, and in December, January, and February to pass on to New Zealand and Tasmania.

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Attention is called to the "Wants" column. It is invaluable to those who use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The "Exchange" column is likewise open.

REPORT ON THE MEMORIAL PRESENTED TO THE SMITHSONIAN INSTITUTION REGARDING AN AMERICAN TABLE AT THE NAPLES ZOÖLOGICAL STATION.[1]

To the Biologists of the United States: I have the honor to submit the following report on the Memorial which was circulated last winter, petitioning the Smithsonian Institution to support a table at the Naples Station.

Thirteen copies of the Memorial were sent out. Twelve of these, bearing the signatures of nearly two hundred working biologists, representing about eighty universities, colleges, and scientific institutions, were returned to me, and were presented in person to Professor S. P. Langley, secretary of the Smithsonian Institution.

In reply to the Memorial, the following letter was received:—

Smithsonian Institution,
Washington, April 7, 1893.

Dear Sir:

I have given careful consideration to the petitions and papers presented by you, and I have decided, in behalf of the Smithsonian Institution, to rent a table at the Naples Zoölogical Station for three years, and have already taken steps to secure it.

I shall be glad to be able to learn the opinions of the representative biologists of the United States in regard to the best administration of this table, and I shall esteem it a favor if, through your mediation, an advisory committee of four persons may be formed; one to be nominated by the president of the National Academy of Sciences, one by the president of the American Society of Naturalists, one by the president of the Americal Morphological Society, and one by the president of the Association of American Anatomists, with the understanding that I may, if need arise, feel at liberty to ask their counsel in regard to the regulations for the use of the table, or as to the merits of applicants for it.

The table will be known as The Smithsonian Table. Publications resulting from its use will bear the name of the Smithsonian Institution, and such of them as are of sufficient importance will probably be printed in the "Smithsonian Contributions to Knowledge."

While the exact conditions will be determined later, I may say, subject to better advices, that it seems to me now that applications for the use of the table should be made to the secretary of the institution, who will probably desire to feel authorized to consult the above-mentioned committee concerning them, whenever, in his judgment, occasion arises for doing so.

If this meets your approval, will you kindly communicate to the president of each of the societies named my request, that he nominate a member of the advisory committee in question?

Very respectfully yours,
S. P. Langley,
Secretary.

Dr. C. W. Stiles.

In accordance with this letter, I communicated with the gentlemen designated and forwarded the following nominations, made by them, to Secretary Langley:—

Major John S. Billings, M.D., U.S.A., nominated by Professor O. C. Marsh, president of the National Academy of Sciences.

E. B. Wilson, Ph.D., professor of zoölogy, Columbia University, nominated by Professor Chittenden, president of the Society of American Naturalists.

C. W. Stiles, Ph.D., zoölogist, Bureau of Animal Industry, U. S. Department of Agriculture, nominated by Professor C. O. Whitman, president of the American Morphological Society.

John A. Ryder, Ph.D., professor of embryology, University of Pennsylvania, nominated by Professor Allen, president of the Association of American Anatomists.

In regard to these nominations, the secretary of the Smithsonian Institution has addressed the following letters to me:—

June 5, 1893.

My dear Sir:

I am pleased to receive your letter of 2d instant, in reference to the appointment of members of the advisory committee, with whom I may feel at liberty to consult, concerning appointments for the Smithsonian Table at the Naples Zoölogical Station.

As I understand you, Doctor J. S. Billings, U.S.A., of Washington, is nominated by the National Academy of Sciences; Professor E. B. Wilson of Columbia University, New York, to represent the American Society of Naturalists; you to represent the American Morphological Society; and Professor John A. Ryder of the University of Pennsylvania to represent the Association of American Anatomists.

I am glad to accept the nomination of these gentlemen, and in each case to appoint the nominee a member of the committee; and since you do not name the chairman, I beg that you will, acting provisionally as such, make this statement to each of the gentlemen in question.

I would suggest that it would much facilitate the business in hand, if the chairman of the committee should be a resident of Washington, and be so far authorized to speak for the committee, that he need not consult its individual members on every separate application. I am, sir,

Very respectfully yours,
S. P. Langley,
Secretary.

June 8, 1893.

Dear Sir:

I wish to add to my letter written two days ago the statement that I have decided to designate Dr. Billings chairman of the Advisory Committee on the Smithsonian Table at the Naples Station, and yourself as secretary.

Please communicate this fact when you write to the several members of the committee announcing their formal appointment.

Yours respectfully,
S. P. Langley,
Secretary.

Dr. C. W. Stiles,
U. S. Department of Agriculture, Washington.

Professor Langley has also sent me the following announcement for publication, and a copy of the contract between the Smithsonian and Professor Dohrn, which is here published for the benefit of those who contemplate a trip to Naples.

The secretary of the Smithsonian Institution announces that the Institution has secured a table at the Naples Zoölogical Station for the use of American investigators. Applications for the use of this table will be received at any time, and should be accompanied by credentials indicating that the candidate is qualified to carry on original investigation in some field for which especial facilities are offered at the Naples Station. These credentials should be accompanied by a statement of the history of the candi-

[1] Scientific journals throughout the country please copy this report in full or abstract it, so that it may reach every working biologist.—C. W. S.

date as a student and investigator, together with a list of such original papers as may have been published by him. The application should be also accompanied by a statement of the character of the investigation which the candidate desires to pursue, and the dates between which he wishes to occupy the table.

Appointments will be made by the secretary of the Smithsonian for a specific period, and, in the consideration of the claims of the candidates, the Secretary will probably avail himself of the counsel of an advisory committee of four, representing the National Academy of Sciences, the Society of American Naturalists, the American Morphological Society, and the Association of American Anatomists.

Persons who may occupy the Smithsonian table are expected to make a report at the end of their term of occupation, or every three months in case of long residence at the station. It is expected that due credit will be given to the Smithsonian Institution in any publication resulting from studies carried on at its table, and the "Smithsonian Contributions to Knowledge" will probably be available for the publication of at least a part of the papers resulting from the Naples investigations.

All correspondence should be addressed to S. P. Langley, Secretary of the Smithsonian Institution, Washington, D.C.

STAZIONE ZOÖLOGICA DI NAPOLI.

Entre la "Smithsonian Institution," Washington, et le Professeur Dr. Antoine Dohrn, Directeur de la Station Zoölogique de Naples, a été établi le suivant

CONTRAT.

1. Monsieur le Docteur A. Dohrn met à la dispcsition de la Smithsonian Institution une table d'étude dans les laboratoires de la Station Zoölogique à Naples, aux conditions suivantes et contre l'indemnité qualifieé dans l'article 11 de ce contrat.

2. La table doit être prête à être occupée par le savant nommé par la Smithsonian Institution, dans le terme de huit jours aprés que l'Administration aura été avisée de son arrivée.

3. La table doit être munie des objets énumerés ci-dessous:—

(*a*) Des principaux réactifs chimiques,

(*b*) Des instruments necessaires à la technique anatomique et microscopique,

(*c*) Des accessoires pour le dessin.

Les laboratoires seront dûment pourvus d'instruments et d'appareils plus compliqués qui sont devenus d'usage, pourtant ceux-là se trouveront au nombre de deux ou trois exemplaires, et l'on est tenu de s'en servir en commun.

La Station ne pourvoit pas les tables d'instruments optiques, puisqu'il s'entend que ceux qui viennent y travailler en possèdent de leur propre choix.

4. La table possède un nombre suffisant de petits aquariums pourvus d'eau de mer courante, et devant servir aux expérience que le savant se trouvera dans la nécessité d'entreprendre.

5. Les animaux qui feront l'objet d'étude seront renouvelés aussi souvent que possible et selon que le savant en demandera. On pourra en outre avoir du matériel preparé et conservé selon les méthodes voulues, afin de pouvoir continuer les études commencés à Naples.

6. Le grand Aquarium annexé à la Station Zoölogique sera ouvert gratis à l'occupant de la table, soit pour en jouir, soit pour y faire des études sur les moeurs des animaux.

7. La Bibliothèque de la Station Zoölogique est accessible à l'occupant de la table, dans une salle contiguë aux laboratoires, et peut servir de salle de lecture et à la compilation des manuscrits.

8. Les laboratoires seront ouverts à sept heures du matin en été, et à huit heures en hiver. Dans des cas exceptionnels on poura s'accorder avec l'Administration pour d'autres arrangements, pourtant les employés ne seront pas tenus de tenir les laboratoires prêts avant l'heure indiquée. Depuis le 20 Juin jusqu'au 20 Août les laboratoires seront fermés.

9. L'occupant de la table aura le droit de prendre part aux expéditions de pêche que feront les embarcations de la Station, ainsi que de se faire enseigner les diverses methodes en usage.

10. Les degats commis par l'occupant de la table sur les instruments et utensiles resteront à la charge de d'Administration de l'Institut autant qu'ils ne depasseront pas la sommes de 20 francs.

11. Le présent Contrat aura la durée de trois ans. et la Smithsonian Institution s'engage à payer à Monsieur le Docteur Antoine Dohrn, Directeur de la Station Zoölogique, annuellement et par anticipation la somme de francs 2500 en or (deux mille cinq cents francs en or) pour la table louée dans les laboratoires de la Station Zoölogique.

Signé en double exemplaire.

Washington, June 9, 1893.	Naples, 16 May, 1893.
S. P. LANGLEY,	PROFESSEUR DR ANTON DOHRN,
Secretary of the Smithsonian Institution.	Directeur de la Station Zoölogique de Naples.

In conclusion, I wish to express my obligations to the signers of the petition for their prompt and hearty support in this matter, which is of great interest to us all.

The Smithsonian Institution has now placed a table at our disposal, and in so doing has rendered to the professional biologists of the country a service which should be appreciated by all, and which will be especially appreciated by those of us who, on account of the non-existence of an American table for many years prior to the establishment of the Davis table, have either been debarred from the Naples Station or have worked there only at the courtesy of foreign institutions or by personal favor of Professor Dohrn. Let us now show our appreciation of Professor Langley's action by seeing that the table is occupied the entire time. I would respectfully suggest that those contemplating making application for the use of the table should do so at as early a date as possible, so that ample time will be given for correspondence and for arranging a proper distribution of the table so that all worthy applicants may be given an opportunity to spend a few months at the station.

Respectfully submitted,

C. W. STILES.

ASSOCIATION OF COLORS WITH SOUNDS.

BY B. F. UNDERWOOD, CHICAGO, ILL.

A BLOW on the head often gives rise to luminous sensations (for luminousness is a sensation and not, as is popularly supposed, a thing per se) and, under the influence of the shock, the person seems to see a multitude of sparks. Describing the effect of a fall on the ice, boys say it made them "see stars." Frequently there is great variety and brilliancy of colors thus seen. Vibrations which, affecting the auditory nerve, produce the sensation of sound, in some cases have the power of causing the sensation of luminousness. Indeed, there are persons who, whenever they hear a sound, also perceive a color, one sound corresponding with red, another with blue, another with green, etc. Dr. Nussbaumer, of Vienna, relates that when a child, in playing one day with his brother, he struck a fork against a glass to hear the ringing, and while he heard the sound, he discerned colors. He says that when he stopped his ears, he could tell by the colors how loud was the sound produced by the contact of the fork with the glass. Very much the same were the experiences of the brother. The doctor relates the observations of a medical student in Zurich, to whom notes of music were translated by certain fixed colors, the high notes by clear, the low ones by dull colors.

M. Pedrona, an opthalmologist of Nantes, states that he had a friend who was accustomed to the simultaneous perception of sounds and colors, but he avoided speaking of it, not wishing to be thought strange or to be an object of curiosity or a subject of discussion. At one time a number of persons were repeating a slang expression, which occurred in some popular story, "That is as fine as a yellow dog," applying it in a jocular manner to all kinds of things and actions. One of the company said of another person, "Have you noticed his voice? It is as fine as a yellow dog." M. Pedrona's friend replied seriously and with emphasis, "His voice is not yellow; it is pure red." The downright earnestness with which the remark was made, caused the whole company to laugh outright. "What," said they, "a red voice? What do you mean?" The gentleman had to explain the peculiar faculty which he possessed of seeing the color of voices. When he had done this each person present desired to be informed of the color of his own voice. The voices were characterized as blue, red, green, etc. The joke was on a young man who happened to have a yellow voice.

M. Pedrona says that his friend had perfect sight and hearing and that he was in the best of health. With him a luminous impression seemed to be made before he experienced the sonorous impression. So keen was the chromatic sensitiveness that he knew whether the sound was blue, red, yellow, or of other color, before he could judge of its quality and intensity. He differed in one respect from the Zurich student — he did not perceive a change of color with every modification of tone. A sharp note was only brighter, while the flat one was duller than the natural. The same piece of music played upon different instruments produced different sensations. A melody played on a clarionet was red and on a piano blue. The color was intense in proportion to the energy of the sound. The colored appearances of the sound were perceived on the vibrating body, for instance, on the strings of the guitar or over the keys of the piano. "The seat of color," said the person who experienced these impressions, "appears to me to be principally where the sound is made, above the person who is singing. The impression is the same if I do not see any one. There is no sensation in the eye, for I think of the same color with my eyes shut. It is the same when the sound comes from the street through the walls and partitions. When I hear a choir of several voices, a host of colors seem to shine like little points over the choristers; I do not see them but I am impelled to look toward them and sometimes, while looking toward them, I am surprised not to see them."

This association of colors with sounds is more common than has hitherto been thought by the few persons who have called attention to the phenomena. It has been assumed that the experiences were hallucinations. It is more probable that they result from some connection between the auditory and visual nervous fibres. It is now known that there are motor nerve-centres which perform particular functions, and it will probably be found that near the acoustic centres are also chromatic centres, and that, in such cases as have been described above, they echo to each other. The fibres of the nerve of hearing may thus produce vibrations at different periods of the chromatic fibres.

According to the doctrine of evolution all the other senses have come slowly into existence as so many modifications of feeling. Indeed, hearing and sight, as well as taste, are modes of feeling. Differentiation of feeling has, in the evolutionary processes, corresponded with the differentiation of physical structure. In the lowest forms of life there are no developed and defined parts like the organs of hearing, sight, smell, and none such as in the higher animals make possible variety and sensitiveness through touch alone. "The spider's touch, how exquisitely fine," exclaims Pope. What a difference in the sensation of touch between the speck of living jelly, homogeneous so far as it appears to the eye, and a man with his differentiated structure, his several senses through which

> Soft silence, and the night
> Become the teachers of sweet harmony.

THE GULL LAKE BIOLOGICAL STATION OF THE UNIVERSITY OF MINNESOTA.

BY CONWAY MACMILLAN, UNIVERSITY OF MINNESOTA, MINNEAPOLIS, MINN.

THE establishment, during the present season, of an inland biological station, marks a new epoch in Americal biological instruction. While several excellent marine stations have already been organized both upon the Atlantic and Pacific coasts, and most recently upon the Gulf of Mexico, up to the present time — so far as known to the writer — there has been no inland station provided for the free use of American investigators. The great need of such a station, well equipped for every kind of biological work, has long been pressing, and it is now hoped that a foundation has been secured upon which to build as broadly as possible for the best interests of American biology. The establishment by individual enterprise of such a private laboratory as the well-known one at Milwaukee, has served to accentuate the need of an inland station, access to which might be general. The University of Minnesota proposes now to offer such a station, and a party of twenty, representing at least four different institutions, begin work early in June. The station is situated upon one of the deep bays of Gull Lake, in Cass County, Minnesota. This lake is about eighteen miles from Brainerd, and lies in the pine-belt of central Minnesota. It is an attractive sheet of water, about twelve miles in length and four miles in width, with irregular coast-line, and surrounded by hills, meadows, marshes, promontories, swamps and smaller lakes. With a great diversity of conditions in its vicinity and in its own waters, it is an excellent spot for general inland biological work. Its situation, too, as one of the innumerable lakes which form the general reservoir in which the great central river of the continent takes its rise, adds an interest to its study. As a region for the investigation of the various problems of isolation it is peculiarly fine. Many of the hundreds of lakes in Cass county were originally united, but are now separated from each other by permanent divides. In such waters, comparative study of the plankton, pelagic and limnetic groups of organisms can not but be productive of new and important results. Both zöologically and botanically, Gull Lake and its tributary country promise a rich field of investigation.

The laboratory buildings form a cluster of cottages on the brow of an abrupt hill, lying toward the east. The cottages number five, and in addition there is a larger building, two stories in height, with kitchen and dining room and sleeping apartments. These buildings have been placed at the disposal of the biological departments of the University through the courtesy of the Northern Mill Company of Minneapolis. Until recently, they formed a supply camp and headquarters for the company while it was cutting timber in the vicinity of the west shores of Gull Lake. The cottages are neatly plastered and papered, and form an altogether admirable series of buildings for a summer station. From Brainerd, the laboratories are reached by the Brainerd and Northern Minnesota Railway, the officials of which have assisted much in the development of the plan of establishment.

Apparatus of all necessary sorts has been shipped from the University, and the investigators in the station will be given every facility in the power of the University to pursue their work under favorable and inspiring conditions. Boats have been put upon the lakes, and a steamer belonging to the Northern Mill Company has been placed at the disposal of the station for extended trips about Gull Lake itself. Dredges, nets, seines, collecting apparatus of all sorts, both aquatic and terrestrial, have been shipped to the station, and are in constant use. Abundant opportunity for collection may be secured, and those who desire are permitted to give their principal attention to such work, while others are engaged more particularly upon lines of special research.

The direction of the laboratory is under the professors of botany and animal biology in the University of Minnesota, and thus broadly organized there is no danger that the name will be a misnomer for a special zöological or botanical station. The plan of establishment contemplates the largest and most modern development, and equipment for work in experimental embryology, oecology, plankton study, etc., will be freely provided, as it is demanded.

To the botanists and zöologists of America it is not necessary to explain or defend the establishment of such a station. Modelled, as it is, somewhat upon the lines of its old world predecessor, Plön, it hopes by its connection with one of the state universities to offer its advantages to a constantly increasing circle of investigators, at a cost much below that which might be possible for any private institution of kindred nature. While still in an inchoate condition, when the ultimate possibilities and expectations are considered, it will begin with a relatively large corps of workers, under conditions highly favorable for a successful continuance. It will, during the first season, from June 1st to September 1st, welcome any serious student who may come to its doors. While its accommodations are not unlimited, it can care for such as give due announcement of their coming, and the directors will be glad to enter into correspondence with those contemplating a visit. The address of mail should be as follows: Stony Brook Landing, care of Northern Mill Co., Brainerd, Minn.

NOTES ON THE POLLINATION OF PLUMS.

BY L. H. PAMMEL, AMES, IOWA.

SOME years ago while making a few random examinations of the cultivated DeSoto Plum (*Prunus Americana*, Marshall) I found to my surprise that the flowers were not all perfect, although described as such. Many flowers have since been examined and I have never failed, in some individuals at least, to find this character well pronounced.

In all cases examined the suppression was in the direction of the pistil. The stamens in all cases were well developed. In these imperfect flowers the pistil is short, scarcely as long as the calyx tube. In the Rollingstone the pistil is entirely absent in many cases.

To see how generally the pistils were rudimentary, a number of counts were made on branches selected at random on several trees.

First Tree.

	Perfect.	Imperfect.
First branch,	10	2
Second "	14	4
Third "	17	5
Fourth "	7	2

Second Tree.

	Perfect.	Imperfect.
First branch,	15	2
Second "	10	6

Third Tree.

	Perfect.	Imperfect.
First branch,	0	36
Second "	16	0

These imperfect flowers also occur in the Pottawattamie, but not so commonly as the Rollingstone and DeSoto. I thought at first that these imperfect flowers might be due to the improvement of the variety under cultivation, but on examining some seedlings along an old fence I found that imperfect flowers also occurred. Of the enormous number of perfect flowers produced on a single tree a small percentage only develop into plums. They are undoubtedly in many cases fertilized but for want of nutrition fail to mature.

The flowers of *Prunus Americana*, in absence of cross pollination, are undoubtedly close pollinated. To test the matter of close fertilization, about 150 flowers were covered with paper bags. Of these fifty set. Between twenty-five and thirty flowers were castrated and pollen applied from other flowers of the same plant with the result that one-third set. Considering the circumstances under which they were made it is a fairly good showing.

I was much interested this spring to notice that some forms of *Prunus domestica* (Moldavka Plum) are proterogynous. The pistil in some cases protrudes while the flowers are still more or less closed. In other forms of *Prunus domestica* grown on the college grounds the pistil matures simultaneously with the stamens. This latter condition agrees with Hermann Müller's[1] observations, who says of *Prunus domestica*, *P. avium* and *P. cerasus*, "anthers and stigmas ripen simultaneously and spread apart out of the flower." *Prunus padus*, L, and *P. spinosa*, L, according to the same authority, are proterogynous.

The *Rosaceæ* constitute an interesting order of plants, although many of them show adaptations for cross-pollination, they may, at the same time, in absence of cross-pollination, be self-pollinated, not, however, in all cases. Strawberry growers are only too familiar with the failure that results when only one variety is set out.

This tendency to separation of sexes is well marked in widely separated orders and has been admirably discussed by Darwin,[2] who says: "There is much difficulty in understanding why hermaphrodite plants should ever have been rendered diœcious." "We can, however, see that if a species were subjected to unfavorable conditions from severe competition with other plants, or from any other cause, the production of the male and female elements and the maturation of the ovules by the same individual might prove too great a strain on its powers, and the separation of the sexes would then be highly beneficial." As stated in a previous paragraph, many plum flowers are staminate in function as the fruit never develops. This being the case, it would seem an advantage for the pistil to become abortive and in some cases entirely suppressed. May it not be a step in a direction to prevent self-fertilization, which seems to occur quite commonly in some members of this order, or is it the direct action of climate as Darwin thought to be the case in the strawberry?

[1] "Fertilization of Flowers," English translation, p. 222.

[2] "The Different Forms of Flowers on Plants of the Same Species," p. 278, D. Appleton & Co., New York.

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.

Total Heat Received by a Planet.

IT may be as well to call attention to the shortest method of treating what seems to be the principal point at issue in the articles on "Sun-Heat and Orbital Eccentricity" and on "The Mean Distance of the Earth" in recent issues of *Science*.

We have simply for the amount of heat, dh, received by any planet in our system by radiation from the sun, in the infinitesimal time dt, on a definite area, say a square foot, of its vertically exposed surface,

$$dh = \frac{c}{r^2}\, dt,$$

in which c is a constant depending on the absolute radiation of the sun, which we suppose to be always the same.

But we have

$$dt = \frac{r_2\, d\theta}{k\, \sqrt{p}},$$

in which dt is expressed in terms of the day, $d\theta$ in the usual way, so that $180° = \pi$; k being the Gaussian constant, depending on the mass or absolute attractive force of the sun, and p, the semi-parameter, $= a\,(1 - e^2)$.

Strictly, we must understand by k the Gaussian constant $0.017202 +$ multiplied by $\sqrt{1 + \mu}$, in which

$$\mu = \frac{\textit{mass of planet}}{\textit{mass of sun}}.$$

We have then

$$dh = \frac{c}{k\, \sqrt{p}} d\theta,$$

and for the total heat received by radiation on the definite area in one revolution,

$$\frac{2\, c\, \pi}{k} \cdot \frac{1}{\sqrt{p}}.$$

Now the major axis being supposed constant, $\sqrt{p}$ is proportional to the minor axis. If then the eccentricity varies in a planetary orbit, the major axis remaining constant, the quantity of heat received by the planet in one revolution by radiation from the sun is inversely as the minor axis, if the size and mass of the planet and the mass and absolute radiation of the sun remain unchanged.

REV. GEO. M. SEARLE.

Catholic University, Washington, D.C.

A Peculiar Occurrence of Beeswax.

AMONG the heterogenous collections of materials that are continually arriving at the National Museum for the purpose of identification, there were received some weeks ago, from Portland, Oregon, samples of a material closely resembling, if not identical with beeswax. Such it would have unhesitatingly been pronounced but for certain stated conditions relating to its mode of occurrence.

The material as received is in the form of (1) nodular, somewhat rounded masses, the largest perhaps the size of a goose egg; (2) in elongated cylindrical forms sometimes incompletely perforated, longitudinally, and (3) as rounded grains forming one of the constituents of a loosely coherent, silicious sandstone. The material is of a grayish color on the outer surface, indicating oxidation, but interiorly it has all the characteristics of genuine beeswax, both as regards physical conditions, color, smell, fusing point, and conduct towards chemical reagents.

In the letter accompanying, the wax is said to be found in masses of all sizes up to 250 pounds weight; that it occurs imbedded in the sand, being found while digging clams at low tide, and at a depth of 20 feet below the surface when digging wells. The material has been traced for a distance of 30 miles up the river.

Tradition has it that many hundred years ago a foreign vessel, (some say a Chinese junk) laden with wax, was wrecked off this coast. This at first thought seems plausible, but aside from the difficulty of accounting for the presence in these waters and at that date, of a vessel loaded with wax, it seems scarcely credible that the material could have been brought, in a single cargo, in such quantities, nor buried so deeply over so large an area. In a fragment of the sandstone above alluded to, the wax occurs in disseminated grains less than half the size of a pin's head and in such abundance that when ignited the stone falls away to a loose gray silicious sand. My correspondent states that the material has been mined by the whites for ever 20 years, but not to any great extent excepting the last 8 or 10 years, during which time many hundred tons have been shipped to San Francisco and Portland, and sold at the rate of 18 cents per pound.

Concerning the accuracy of the account as above given the present writer knows nothing. It is here given in the hope of gaining more information on the subject.

GEORGE P. MERRILL.

U. S. National Museum, Washington, D.C., June 9.

Books for Children.

WILL some specialists in natural history recommend some really satisfactory cheap books suitable for the guidance of children, ten years of age, in their rambles through the fields and woods? Most of the cheap books that I have seen do not give the necessary details for identifying specimens, and yet the naming of what is seen or collected is necessary for arousing enthusiasm in studying the forms of life. Some of the topics which I am inquiring about are as follows:—

The naming of free birds from their size, plumage, song, and habits; and the place and manner of constructing nests and habits of nesting. The naming of trees and shrubs from their bark and leaves. The naming of weeds and flowers found growing wild in the east-central part of the United States. The naming of land-snails, beetles, butterflies, and moths, and their habits.

Perhaps the Agassiz associations have made out lists of the specimens to be found in the various regions of the United States. If this has been done, I have not happened to see any notice of it.

In this connection, I wish to mention the work done by my own teacher in a suburban school at Cincinnati more than twenty years ago. The superintendent of the school, Mr. A. G. Weatherby, afterwards a professor in the Cincinnati University, was an indefatigable collector in various departments of natural history, and his enthusiasm was communicated to his pupils so strongly that there was hardly a boy in his school-room who had not a collection of local moths, land-snail shells, and fresh-water clam-shells. We had them all properly prepared and Mr. Weatherby named them for us; but we learned the localities in which different species were to be found through the broad experience of our teacher, and not from books. In fact, although many of our class of boys had almost complete sets of local snail-shells, and all named, yet I doubt if any of us ever looked into a work on conchology. I do not know whether any of Mr. Weatherby's early pupils have since become professional naturalists, as a result of his teachings, but I do know that the collecting excursions made under his direction were most beneficial as a means of sharpening our powers of observation, and added immensely to the happiness of boyhood.

I am sure that many readers of *Science* will be glad to get information such as I have asked for, as very few parents are able to help their children in classifying and naming the "finds" that they are continually bringing in from the fields.

FRANK WALDO.

Princeton, N.J., June 5.

Worms in the Brain of a Bird.

IN your issue of June 2 is a communication "Relative to Worms in the Brain of a Bird."

Your correspondent will find, by consulting "Fresh-Water Shell Mounds of the St. John's River, Florida," by Professor Jeffries Wyman, page 7, foot-note, an account of a parasitical worm commonly found in the brain of the "snake bird," or water turkey. CLARENCE B. MOORE.

Philadelphia, June 6.

Note on a Supposed New Endogenous Tree from the Carboniferous.

IN the May number of the *American Geologist* (Vol. XI., 1893, pp. 285, 286, Pl. VI.) I find a short paper by Mr. H. Herzer on "A New Tree from the Carboniferous Rocks of Monroe County, Ohio," in which he describes, under the name of *Winchellina fascina*, a new genus and species. The discovery of a new genus of plants in the Carboniferous, a formation of which the flora is now so very well known, is of itself of considerable interest, but when we learn that it was an endogenous tree the interest deepens, and the discovery, if true, would be the most important addition to our knowledge of the ancestors of this great group of plants that has been made in many years.

The Carboniferous has been called the age of ferns, from the great abundance and high state of development enjoyed by this class of plants in this part of the Paleozoic system. Several supposed endogens have been reported from the Paleozoic, but they have sooner or later been shown to belong to other vegetable classes, and at the present time there is not a single form accepted by paleobotanists as belonging to this age. In fact it is not until well up into the Mesozoic that undoubted endogens made their appearance. This is, of course, negative evidence, but it is so strong that it requires the most positive and convincing evidence to prove their earlier ancestry.

The literature relating to the internal structure of plants of the Paleozoic is now very extensive, and from a careful study of this it appears almost beyond question that the supposed new endogenous tree is a fern-stem of a well-known type. I have not seen the original trunk or sections cut from it, but, judging from the somewhat imperfect description and figures, it is impossible to see any differences of importance between *Winchellina fascina* and *Psaronius cottæ* corda[1] from the Permian of Saxony. It also approaches very closely to *Tubiculites* (*Psaronius*) *relaxato-maximus*[2] Grand'Eury, a fern-stem from the Carboniferous of central France. The cell-bundles described by Mr. Herzer are quite unlike those of any monocotyledon with which I am familiar, but agree well with those described for fern-stems from the older rocks. The reference of this plant to the ferns is also quite in accord with facts that have long been known, for Dr. Newberry recorded the genus *Psaronius* as occurring "in great abundance" in the Carboniferous rocks of Ohio more than forty years ago.[3]

The genus *Psaronius* is a somewhat comprehensive one, and a number of more or less satisfactory genera have recently been separated out of it by Williamson, Renault, Zeiller and others, and it is possible that when the fossil under discussion is more

[1] Stenzel, Ueber die Staarsteine, Jena 1854, p. 867, Pl. xxxv., Fig. 1.

[2] Flore Carbonifère du Dépt. de la Loire. Mem. l'Acad. d. Sci., xxiv., 1877, p. 102, Pl. x., Figs. 3, 4.

[3] Annals of Science, No. 8, Feb. 1, 1853, p. 97.

thoroughly studied it will be found to belong to one of these recently differentiated genera. F. H. KNOWLTON.

U. S. National Museum, Washington, D. C.

Mean Values.

MISS PORTER'S kindly criticism (*Science*, June 2) of one point in the article, "Sun-Heat and Orbital Eccentricity" (*Science*, Apr. 28), gives occasion to say a work in regard to mean values. Since the mean value of n quantities is the arithmetic mean of their sum, it would appear at first glance as if the term were a perfectly definite one; but if the quantities to be averaged are successive values of a function of some variable, then clearly their magnitudes depend not only on the nature of the function, but also on the law of variation of the fundamental. Thus, suppose we have the isotherm, $pv = c$, and wish to know the average pressure between the volumes $v = v_1$ and $v = v_2$. It is necessary to make some assumption in regard to the variation of v. If its increments are supposed equal, we understand by the "mean value" of the pressure the average of the pressures corresponding to the values of v. If the volume is assumed to depend in turn on some other variable in such a manner that the abscissa-increments are not equal, the mean value will now be the average of the new series of pressure-ordinates corresponding to the series of values of v arising under the second assumption. Evidently these two means will in general be unequal, but one is just as properly the "real average" as the other. The formula for mean value may be derived by a method even simpler than the usual analytical one as given by Williamson and Todhunter. Let it be required to find the mean value of y where $y = f(x)$ and x is an equicrescent variable. If $y = f(x)$ be treated as a curve referred to rectangular axes, $\int_a^b f(x)\,dx$ is the expression for the area, A, bounded by the X-axis, two ordinates, and the portion of the curve intercepted between the bounding ordinates. Let $A = A'$, where A' is a rectangle whose base equals the base of A. Then the altitude of A' is the average of the ordinates in A. For let

$$\frac{y_1 + y_2 + \ldots\ldots y_n}{n} = y_0,$$

the average of the series of ordinates.

Then $\quad y_1 + y_2 + \ldots\ldots = y_0 + y_0 + \ldots\ldots$ on to n terms.

Multiplying by $\triangle x$ and summing,

$$\Sigma\,(y_1 + y_2 + \ldots.)\,\triangle x = \Sigma\,(y_0 + y_0 + \ldots.)\,\triangle x;$$

or, making n indefinitely large,

$$\int_a^b y\,dx = y_0 \int_a^b dx = y_0\,(b - a).$$

But $$\int_a^b y\,dx = A, \text{ hence } y_0\,(b - a) = A',$$

and, since $b - a$ is the base of the rectangle, A', y_0 is it saltitude.

For example, let it be required to find the mean pressure between the volumes v_1 and v_2. If the isotherm is $pv = c$, the area, A, in this case becomes

$$\int_{v_1}^{v_2} \frac{c}{v}\,dv = c \log\left(\frac{v_2}{v_1}\right);$$

its base is $v_2 - v_1$, hence the mean pressure is

$$\frac{c}{v_2 - v_1} \log\left(\frac{v_2}{v_1}\right).$$

This conception of mean values may be readily employed when a curve is expressed in polar coördinates. If $r = f(\theta)$, let x be written for θ and y for r. The Cartesian equation thus arising furnishes a curve which sustains peculiar relations to the original polar curve. The radii-vectores are taken out of their fan-shaped arrangement and placed equi-distant and parallel, with their extremities on a common line, the X-axis. The pole may be viewed as having developed into this axis, whilst a circle of unit radius with pole as centre has developed into a straight line parallel to the axis, the radii-vectores keeping their normal position with respect to the circle. In finding the mean value of the radius-vector of an ellipse, $d\theta$ being constant, the figure A has three rectilinear sides: $x = 0$, $x = \pi$, and the X-axis. Its fourth side is the curve,

$$y = \frac{a\,(1 - e^2)}{1 + e \cos x}.$$

The base of the figure is π; hence the mean value is

$$\frac{1}{\pi}\int_0^\pi \frac{a\,(1 - e^2)}{1 + e \cos x}\,dx = a\,\sqrt{1 - e^2}.$$

It will be seen that the area-method serves only when the ordinates are equally distributed throughout the area A. In the dynamical problem of the earth's mean distance from the sun it is not θ (or x) which is the equicrescent variable, but t, the time. A must therefore be taken equal to

$$\int_{t_1}^{t_2} r\,dt,$$

for which $r = f(t)$ must be given; but, as is well known, the equation expressing the relation between r and t is transcendental and cannot be written in the form $r = f(t)$. Recourse must therefore be had to other devices for finding the mean distance when the problem is rendered kinematical by taking Kepler's second law into account. ELLEN HAYES.

Wellesley, Mass.

Iron and Aluminium in Bone Black.

WILL you kindly, in your next issue, print the following corrections to my article on "Iron and Aluminium in Bone Black," which has just reached me.

Page 300, first column. In twentieth line (from the bottom of page), after the word "permanent," insert, *and boil.* In nineteenth line (from bottom of page) remove the first two words: "and boil."

In twelfth line (from bottom of page) insert a decimal point between 5 and 0 at end of this line, for the figure must read 5.0 and not 50 grammes.

Page 301, first column. In twentieth line (from bottom of page) transpose after "iron." Instead of "aluminium, or the phosphate" then should stand: or the aluminium phosphate predominates.

J. G. WIECHMANN.

New York, June 7.

Estimated Distance of Phantoms.

IN *Science* of May 19, p. 269, Mr. Bostwick mentions the familiar experiment of binocular combination of regular patterns, such as a tessellated pavement or figured wall-paper, by means of ocular convergence, and states that in his case, although the figures of the phantom thus formed appear smaller, yet contrary to the statements of all other writers they do not appear nearer but farther off than the real object. This seems to me inexplicable if the phantom is really distinct.

As I have very unusual facility in making such binocular combinations, I will very briefly describe an experiment of this kind. I stand now looking down on the tesselated oil-cloth covering the floor of the library. By ocular convergence I slide the two images of the floor over one another in such wise as to combine contiguous figures. After perhaps a brief interval of indistinctness, the pattern appears with perfect clearness at half the distance of the floor and the figures of the pattern of half the real size. The sense of reality is just as perfect as in the case of a real floor at that distance. It seems to me as if I could rap it with my knuckle. Taking now this phantom as a real object, by greater convergence the plane can be brought up higher and higher, until by extreme convergence it is brought within three inches of the root of the nose and seen there with the greatest distinctness in exquisite miniature, the figures being only one-quarter inch in diameter. By relaxing the convergence a little, the phantom-plane may be dropped and caught on lower and lower levels until it falls to its real place on the floor. The combination beyond the plane of the object, and therefore with figures enlarged, is also easy if the figures are small, but never quite so easy as combination on the nearer side.

These phenomena are as easy to me as any ordinary act of sight. No device of any kind, such as use of pencil or finger to fix the point of sight is at all necessary. I can watch the double images approach, combine, pass over, combine with the next figure, etc., with the greatest ease and certainty. Moreover, the sense of reality and of exact distance is as perfect as that of any other object.

In young normal eyes great difficulty is often experienced in getting this perfect perception of distance because the phantom is not perfectly clear. The reason is this: The two adjustments of the eyes, the axial and the focal, are invariably associated in every act of sight. Therefore, in the experiment the eyes are accommodated to the point of ocular convergence, i.e., the distance of the phantom. But the light comes from a greater distance, viz., from a real object—the floor. The retinal image, therefore, is not distinct and the figures are blurred. I no longer, now, suffer from this difficulty, because I have become presbyopic, and have, therefore, lost the power of accommodation. The clearness of the phantom is perfect almost immediately. When I was younger, there was always a considerable interval before the phantom became clear. The clearing up was the result of a dissociation of these two consensual adjustments. While the axial adjustment remained adapted for the distance of the phantom, the focal adjustment (accommodation) was changed to the distance of the real object. Now this dissociation of two adjustments so invariably associated in every act of sight, is difficult for most, and impossible for many persons. But until this dissociation is effected and the phanton becomes perfectly clear, the sense of reality, and especially the perception of distance, will be imperfect and vacillating. The use of glasses adapted to distinct vision at the distance of some one of the possible phantoms will make that particular phantom clear.

Now this clear perception of the distance of a phantom, nearer and smaller in proportion to the degree of ocular convergence, is, of course, not peculiar to me. All writers on the subject record the same result. All my pupils who succeed at all in binocular combinations get the same result. I am sure I have tried hundreds, I might almost say thousands, and always with the same result. This result is, therefore, normal and in complete accord with the laws of vision. For near objects, there are two modes of estimating distance, viz., by axial convergence (binocular perspective) and by accommodation (focal perspective). Now, of these two, the former is by far the more exact, and therefore takes control of judgment of distance if the two are not in accord. This is proved by naked-eye combination of ordinary stereoscopic pictures by ocular convergence. In such cases, we have the phenomenon of inverse perspective. The judgment of relative distance by axial convergence completely reverses the real relative position of objects. Binocular perspective overrides every other form of perspective, whether focal, or mathematical, or aërial, and comes out victorious in spite of the absurdity or even impossibility of its results.[1]

Now, in the case of phantoms, axial convergence fixes the distance. But this fixes also the size; for the apparent size of anything is a product of the retinal image multiplied by the estimated distance. The size of the figures will be small in proportion to the nearness of the phantom. This is in exact accord with the laws of vision. But Mr. Bostwick says, that in his case the figures seem smaller and yet more distant than the real object. He explains this, if I understand him aright, by the fact that in the dissociation of the axial and focal adjustments, while most persons follow the axial, he follows the focal adjustment, in estimating distance. Near objects require greater accommodation; but there is no such accommodation in this case, therefore the objects judged by this test will not seem nearer. But, again, since

[1] If anyone is specially interested in this subject, he will find it fully treated in my little volume, entitled "Sight," volume 31 of the International Scientific Series.

CALENDAR OF SOCIETIES.

Appalachian Mountain Club, Boston.

June 14. — Miss Lucy A. Putnam, An Ascent of Adam's Peak, Ceylon; Henry Lambert, Forests and Forestry in America and Europe.

they seem smaller, and since distant objects also seem smaller, they will seem more distant.

There are two objections to this explanation. 1. The accommodation is for the distance of the real object, as is proved by the distinctness. Why, then, should the object seem farther? 2. Again, distant objects seem smaller only because their retinal images are smaller; but this is not so in the case under consideration.

In justification of his view, Mr. Bostwick says that "in monocular vision an object appears distant or near according as the eye is fixed respectively on something nearer than it or something beyond it." I am familiar with the fact here referred to, but in this case the appearance of greater or less distance is so imperfect that it can hardly be called estimate. It may seem farther or nearer almost at will. It is a matter of fancy, not a sober certainty of rational judgment. In fact, there is no ground for forming any judgment.

Although Mr. Bostwick speaks of his estimate of the distance of the phantom as "distinct," yet I cannot but think that, for want of complete dissociation of the axial and focal adjustments the image is not quite sharp; and that, if he got the same sharp, realistic image which I get, he would see the distance as I see it. Of course, there is no disputing about how things seem to different observers any more than there is about tastes; but nevertheless, there are some things which are normal and reducible to intelligible law, and some not. Mr. Bostwick's case may be abnormal, but I think probably not. I well know how illusive binocular phenomena are. He will, I am sure, pardon me for thinking that with more practice in experiments of this kind he will come to see things as others see them.

JOSEPH LECONTE.

Berkeley, Cal., May 27.

A Rain of Fishes.

DURING a recent thunder-storm at Winter Park, Fla., a number of fish fell with the rain. They were sunfish from two to four inches long. It is supposed that they were taken up by a water-spout from Lake Virginia, and carried westward by the strong wind that was blowing at the time. The distance from the lake to the place where they fell is about a mile.

THOMAS R. BAKER.

AMONG THE PUBLISHERS.

MACMILLAN & Co have published a brief biography of the late English anatomist, William Kitchen Parker, written by his son, T. Jeffery Parker. It begins with an account of his birth and early life on his father's farm, and then of his schooling and his apprenticeship, first to an apothecary and afterwards to a surgeon. With his strong inclination for biological studies, it was natural that he should choose medicine as his profession; but it is evident, as indeed his biographer admits, that he had no great love for his profession and only moderate success in the practice of it His prime interests in life, apart from his family, were two things not often found in conjunction at the present day, science and Wesleyan religion; and he seems to have been equally devoted to both and to have found no incongruity between the two. In biology he was largely self-taught; but a few discerning friends saw that he was capable of important original work, and assisted him in the prosecution of such work. He became a member of the Zoölogical Society and afterwards a fellow of the Royal Society; but the position that proved the most useful to him was the Hunterian professorship of anatomy and physiology in the Royal College of Surgeons, because it not only gave him the opportunity to lecture on his favorite subjects, but also added to his otherwise moderate income. His principal scientific work, his researches on the skull, is described at some length in this book, and there are briefer notices of his other studies and a bibliography of all his published works. His principal fault as a scientific writer, his son thinks, was his complicated style; his topics being arranged in a disorderly way and his sentences hastily constructed. Yet biologists will doubtless echo the words of the Royal Society that he was "an unworldly seeker after truth . . . whose beneficent influence will ever be felt in a wide-spreading and advancing science."

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This Company also owns Letters-Patent No. 463,569, granted to Emile Berliner, November 17, 1891, for a combined Telegraph and Telephone, and controls Letters-Patent No. 474,231, granted to Thomas A. Edison, May 3, 1892, for a Speaking Telegraph, which cover fundamental inventions and embrace all forms of microphone transmitters and of carbon telephones.

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First inserted June 19, 1891. No response to date.

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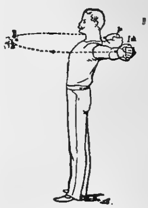

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SCIENCE

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SCIENCE

NEW YORK, JUNE 23, 1893.

A STUDY ON PLANT FECUNDATION.

BY H. J. WEBBER, SUBTROPICAL LABORATORY, EUSTIS, FLA.

THE phenomena of fecundation in obscure plants are in themselves probably uninteresting to the general reader. In so far, however, as they bear on the problem of heredity, which has been popularized by the works of Weismann and others, they become of interest to a wide circle of readers and thinkers. This extended interest makes it desirable that the results of important studies should be brought to general notice.

In the study of fecundation in plants, the most important works which have appeared for several years are the studies of Guignard[1] and Treub.[2] These have been summarized in the *Botanical Gazette* and *American Naturalist.*

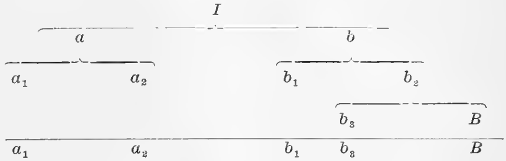
Shortly after the publication of Guignard's studies there appeared a study by Klebahn[3] on the "Fructification of Œdogonium Boscii," an alga of which numerous relatives occur in our American ponds. The article is in no sense revolutionary, yet contains much of interest and value.

A discussion of the occurrence of polar bodies in plants occupies a large part of the paper. This was true also of Guignard's paper mentioned above. Zoölogists found polar bodies to be a very general, if not universal, accompaniment of the animal egg, and they came to be looked upon as having an important *rôle* in the process of fecundation. Botanists now, it appears, discovered that in order to make the theories correspond they must find polar bodies in plants. Following this apparent necessity, for years, every fragment of protoplasm, every small cell or nucleus, anywhere in the region of the egg-cell, for which no other use could be positively affirmed, has been diligently pointed out as probably having the function of polar bodies. To clear up this probable rubbish must now occupy a great share of the attention of botanists writing on related subjects.

Klebahn's study was completed before the publication of Guignard's article demonstrating the presence of attractive spheres (asters) in plants, hence this interesting feature in fecundation is not mentioned.

In the male filament the nucleus lies in the upper end of the cell, the cap end, where the ring and the disunion in the membrane form. In mitosis the upper cell, forming the antheridium, receives only a small amount of protoplasm with the nucleus. After this mitosis the lower nucleus returns to the resting stage, still remaining in its old position, and shortly passes to a new mitosis. This *rôle* is continued till the number (four or five) of antheridium cells are formed. The sterile remainder of the mother cell, after the last mitosis, remains as the lower cell of the series. The protoplast of each antheridium cell divides into two portions, which become the antherozoids. The nuclei of the antherozoids are smaller than those of the vegetative or female cells and have no apparent nucleolus.

In the formation of the oogone a cell of the female filament divides into two daughter cells, an under (a) and an upper (b). These two daughter cells divide again, producing four cells (a_1, a_2, b_1, b_2). The upper one of these (b_2) is the oogonium mother cell. This divides again, and the upper daughter cell of this division is the oogonium (B), and the lower the supporting cell (Stützzelle or Trägerin — b_3). To make this more intelligible, the author's diagram is inserted here:—



In most cases four sterile cells accompany each oogone, but occasionally the cell (a_2) becomes also an oogonium mother cell, the division of which forms an oogonium, A, and its supporting cell (a_3). In this case only two sterile cells accompany each oogone.

The nucleus of the oogonium (B) and of the oogonium mother cell (b_2) are of about the same size and constitution as the nuclei of the vegetative cells. The difference between the nuclei of the oogonium and of the sterile accompanying cells, (b_1) and (b_3), is of especial interest. In the latter the nuclei are much smaller and the nucleolus is always absent. The author especially endeavored to count the number of chromatin bands passing to each nucleus, hoping to obtain some light on Weismann's theoretic reducing division supposed to occur in the formation of the polar bodies. Unfortunately it was found impossible to be sure of the number.

After the oogone has taken its definite form, an opening forms in the upper part for the entrance of the antherozoids. The opening, however, remains closed by an especially developed membrane until the protoplasm of the oogonium draws together into the mature egg-cell ready for fecundation. The nucleus of the oogone meanwhile lies in the upper part of the egg-cell near the point of activity, without, however, dividing or in any evident way giving off substance. The closing membrane now disappears, leaving the way open for the entrance of the antherozoids. Of the numerous cases examined, in no place was anything observed indicating a separation or throwing off of any part of the protoplasm or nucleus. On the contrary, the closing membrane is still present when the protoplast of the oogonium draws together. Nothing in the opening process of the oogonium of this species can be analogized to the formation of polar bodies, and in no stage in the course of fecundation and maturation of the oogone is there anything similar developed.

The antherozoids, passing through the opening in the wall of the oogonium, approach the egg, one fusing with it. Before fusing with the egg-nucleus the male nucleus enlarges from about four to six μ. No other change in structure is noticeable. After the fusion of the nuclei has taken place, the fecundated nucleus is still easy to recognize; the coarser chromatin elements of the male nucleus still forming a well differentiated group. Very soon, however, these marks disappear, the male chromatin becoming distributed till it is wholly unrecognizable. The nucleus of the egg is finally only slightly more strongly granular than the unfecundated. Many antherozoids enter the oogone cavity, but only one enters the egg. Eleven were counted in one case in an oogonium cavity.

The author reviews at considerable length the probable cases of polar bodies in plants, mentioned in literature. The conclusion reached is that at least they do not possess the importance and necessary *rôle* in plants that is assigned to them in the animal kingdom.

It may be possible that in *Œdogonium boscii* the two cells, b_1 and b_3, accompanying the oogonium are to be considered the equivalents of polar bodies. These, with the oogone (B), are de-

[1] Léon Guignard, "Nouvelles Etudes sur la Fecondation," Ann. des Sci. Naturelles Bot., xiv. (1891), pp. 163-288.

[2] M. Treub, "Sur les Casuarinées et leur place dans le Système Naturel," Ann. du Jardin Bot. de Buitenzorg, x., pp. 145-231.

[3] H. Klebahn, "Studien über Zygoten. II., Die Befruchtung von Œdogonium Boscii," Pringsheims Jahr. fur wissenschaftliche Botanik.," Bd. xxiv., pp. 235-267, 1 Taf.

rived from the divisions of the primary mother cell (b). They contain but a small quantity of cytoplasm, and are destined for no further development. After holding the oogonium in place for a time they become disintegrated. The similarity of the nuclei of these cells, in particular the supporting cell b_3, to the male nuclei is further very marked. One could easily believe that the nuclear mass which they separate from the egg nucleus becomes replaced by the sperm nucleus. Both cells, b and b_3, are, however, not present in all *Œdogoniums.* The supporting cell (b_3) is the only one constantly present, and this is frequently richer in contents, and in one case gives rise to an oogonium.

In the formation of the Antheridia, there remains a sterile nucleus, the one below the chain of antheria cells, from which these were abstricted. Here alse the similarity to polar bodies is manifest, but, as Strasburger has pointed out, it necessitates that a part of the male branch be compared to a polar body. The author concludes that a morphological conformity is not shown in either case. The process in *Œdogonium* may be brought in harmony with the theories of fecundation dependent upon polar bodies, but nothing is thus gained.

The results of this portion of the study can be summarized as follows: Genuine polar-body formation is not present in *Œdogonium.* On the other hand, the supposition is not impossible that the two accompanying cells (b_1 and b_3) are the physiological equivalents of polar bodies.

Of the minutia of nuclear fusion in fecundation much remains to be determined. The study lacks the fullness and roundness shown in the work of Guignard. Yet much is added to our knowledge, and our attention is turned to an interesting and promising group of plants for study.

CLIMATE AND THE VARIATION OF SLUGS.

BY T. D. A. COCKERELL, LAS CRUCES, NEW MEXICO.

THE slugs, or naked land-mollusca,— nacktshnacken, they say in Germany,— are found in nearly every part of the world. Many of the species are extremely variable in color and markings, and these variations, as might be expected, usually have a smaller area of distribution than the species to which they belong. Furthermore, as I propose to show in the present article, climate seems to have a marked influence on the variation of these animals, so that the same kind of variety may appear, at two distant spots, under similar environment.

Facts of this kind have been taken, by those who believe in the inheritance of acquired characters, as valuable evidence in their favor. I do not think, however, that they are so valuable in this connection as some have supposed. To cite a well-known example, the white color of many mammals and birds in the Arctic regions is undoubtedly correlated with a cold climate, but it is so very easy to see where natural selection comes in, that scarcely anyone would adduce this instance as proof of the direct influence of climate. So it may be in more obscure cases, where environment seems to directly modify species, that we have not yet found out the way in which natural selection is acting.

In order to be perfectly clear, I will give some examples in as few words as possible, numbering them separately, so that they may be taken one by one, and considered on their merits. I will also attempt to classify them under different headings, according to the kind of environment.

(*A*) *Influence of Altitude.*

1. *Limax marginatus*, Müller. This species is widely distributed in Europe. Its ordinary color is gray, with more or less longitudinal banding. In 1882 Lessona and Pollonera described a nearly black variety from high altitudes in Italy, calling it var. *rupicola.* In 1886 the Rev. A. H. Delap sent me two individuals of this variety from the top of the Reeks, County Waterford, Ireland, 2,300 feet above sea-level. They were at the very summit, miles away from any trees. However, about 100 feet lower down an example of the normal form of the species was obtained.

In this instance it can hardly be doubted that these dark forms originated independently on the Italian and Irish mountains, similar environment producing a similar effect.

2. *Limax maximus*, L. The normal colors are gray with black spots and streaks. A blackish variety (v. *nubigenus*, Bourguignat) is found in the Pyrenees.

(*B*) *Influence of Latitude.*

3. *Parmacella valenciennii*, W. and Van B. Extends from south France to Morocco. In the northernmost part of its range it is reddish-brown, without markings. In the Spanish peninsula the mantle becomes spotted with black (var. *punctulata*, Ckll.), and at Gibraltar and Tangiers the slug is conspicuously marked with black (var. *maculata*, Ckll.). But, curiously, at both the last localities there appears a variety, well marked with black, but dark-olive instead of reddish (var. *olivacea*, Ckll.). It is noteworthy that he varieties on both sides of the Straits of Gibraltar are alike. The var. *olivacea* resembles in color *P. olivieri*, Cuvier, from the Caucasus, at least as represented by an example in the British Museum.

4. *Ariolimax columbianus*, Gould. A large slug found in the Pacific coast region of North America, as far north as British Columbia. In California there is a sub-species, *californicus*, Cooper, identical in color with *columbianus.* From British Columbia to California the slug has two forms, one with, the other without, black spots, the ground-color in each being reddish-brown. In British Columbia there is a variety (*niger*, Ckll.) which is entirely black. In Costa Rica the species reappears as a sub-species, *costaricensis*, Ckll.; dark olivaceous in color. Thus on different continents two slugs, *Parmacella* and *Ariolimax*, each normally rufous, develop an olivaceous variety at the southernmost point of their range.

(*C*) *Influence of Moisture.*

5. *Arion ater*, Linné. This is a large slug common in northern and central Europe. Typically black, it varies to reddish, yellowish, white, brown, and gray, presenting also some beautiful varieties resulting from combinations of these colors. In England one may find specimens of several different colors in the same locality; but Dr. Leach noticed, as early as 1820, that the whitish and pale yellowish forms were specially to be observed in chalky districts. In Scotland, dark varieties prevail. But on the continent, where the climate is drier, is a brick-red form (var. *lamarckii*, Kal.) not to be observed on the British Islands at all. This red variety is so common and conspicuous in various localities in central Europe as to attract the attention of tourists and others who are not usually given to observing slugs.

At Chislehurst, in England, I found intensely black specimens in damp places.

It is possible that the black variety of *Ariolimax* from British Columbia, noticed above, may have some connection with the moist climate of that country.

(*D*) *Influence of Insular Conditions.*

6. *Agriolimax agrestis*, Linné. The common gray garden-slug of Europe, often mottled with dark-gray or black. There is a black variety found in England (var. *niger*, Morel.), and also above the zone of cultivation in the Azores, but not in continental Europe. There is also a very dark variety (*panormitanus*, Less. and Poll.) found in Sicily, and, according to Dr. Simroth, also in Crete. These examples of insular melanism may have to do with the influence of moisture.

7. *Ariolimax columbianus*, Gould. Specimens found by Mr. Hemphill on Sta. Cruz Island, off California, were paler than the type, being uniform light-straw color (var. *straminea*, Hemph.).

8. *Amalia gagates*, Draparnaud. As its name indicates, this slug is typically black, but in England it is nearly always lead-gray (var. or subsp. *plumbea*, Moq.) or brownish, very rarely black. In Sicily there is a large black form (var. *similis*, Ckll.), closely related to the great black sub-species *mediterranea*, Ckll., of Algeria. Here, as with *Parmacella*, we see similar or identical varieties on opposite sides of the Mediterranean. In Madeira, there is a dark-brown variety (var. *maderensis*, Ckll.). In Bermuda, where the species has no doubt been introduced, it is of the typical form. In Ascension and St. Helena are closely-related forms allied to subsp. *plumbea*, and another allied variety (var. *tristensis*, Ckll.) is found both on Tristan d'Acunha and Juan Fernandez.

It is difficult to see how the species can have got to St. Helena, Tristan d'Acunba, and Juan Fernandez if it was not carried there by man; yet it already shows some divergence from the type, and the specimens from the two latter islands, though they are so far apart, are alike. This is not extraordinary if we assume that like climatic conditions produce like effects, since the two islands are both far out in the ocean, at about the same parallel.

The problem becomes complicated, however, when we find *Amalia gagates* reappearing on the Pacific coast of North America, apparently quite native, though separated by long distances from other localities for the species. This Pacific form generally goes under the name *hewstoni,* given by Dr. Cooper, but I have examined authentic examples, and am convinced it is only *gagates.* Nor is this all, for in Australia and New Zealand are species of *Amalia* so very near *gagates* that some recent students have merged them in it. I have examined *A. antipodarum*, Gray, *A. emarginata*, Hutton, and *A. fuliginosa*, Gould, from New Zealand. *A. emarginata* I consider certainly a form of *antipodarum*, but this and *fuliginosa* appear to me to be valid species. They very much resemble *gagates* in structure, it is true, but, if they are really the descendants of imported slugs, the amount of modification they have undergone is remarkable. *A. fuliginosa* is in the British Museum also from the "Polynesian Islands"—exact locality not stated. There is also an *Amalia* in the Sandwich Islands, evidently very near to *gagates*, but whether identical with it or an endemic form cannot be ascertained in the absence of specimens.

Thus it is seen that *Amalia gagates* and its allies present to us some curious problems, which can only be solved by the collection of specimens from many localities, and their very careful comparison. Because the slug was described from and abounds in Europe, it does not therefore appear certain that specimens found in distant localities, closely resembling *gagates*, are descended from imported examples. We have often good reason for believing that this is their origin, but there is none of the certainty that we feel in regard to other species now found at the antipodes. Quite a similar example is afforded by *Agriolimax loevis* and its allies, which seem certainly native in very widely-separated places. It seems that *A. gagates* and *A. loevis* are very ancient species, surviving in those places where the climate suits them.

A STUDY IN POLARIZATION.—PRELIMINARY NOTE.

BY JOHN DANIEL, VANDERBILT UNIVERSITY, NASHVILLE, TENN.

USING a voltameter with platinum electrodes, separated by a glass partition bored in the centre with a hole two centimetres in diameter, over which was sealed a smaller glass plate bored with a hole one and one-half centimetres in diameter, this smaller hole being covered by metal plates of various thicknesses sealed tight over it, a study has been made of the polarization phenomena upon these thin metal partitions in different electrolytes and under various conditions as to thickness of partition, current strength, temperature, etc.

Without now going into details of the apparatus, methods, and results, the following summarized statement may be interesting:—

1. The polarization on a gold-leaf partition in good-conducting H_2SO_4 is zero, or too small to detect with our apparatus, for the range of current used.

2. The "critical thickness" in good-conducting solutions of H_2SO_4, $CuSO_4$, and NaCl is *greater* than .00009 millimetres for gold; .00015 millimetres for platinum; and .0005 millimetres for aluminum, under the above conditions. It is *less* than .0004 millimetres for gold; .002 millimetres for platinum; and .002 millimetres for silver.

3. The "upper critical limit" of thickness under these conditions seems to be about .004 millimetres, rather less than No. 3 gold.

4. Tables I., II., and III. all point to the conclusion that between "critical limits" of thickness the polarization for a given current increases with the thickness.

5. Table II., showing relation of polarization to current, expresses two interesting facts: (*a*) that the polarization on "thick" plates is about the same, in this voltameter, for all currents between .2 ampere and, say, .01 ampere, provided time enough be allowed in each case for the current to become constant, i.e., between the upper limit of current, at which the development of gas is so profuse as by mechanical obstruction and irregular escape to interfere, and the lower limit, at which the formation of gas is no faster than it can be dissipated. (*b*) Quite different is the case for "thin" plates, where, within the limits of current and thickness prescribed, the polarization is dependent upon the current and gives for each thickness a different curve, or rather straight line, for they are all straight lines converging to the origin, and differing only in *slope.* The current strength at which the polarization on very thin plates would reach a maximum is far above that used, being, perhaps, expressed in amperes instead of tenths and hundredths.

By *thick* plates are defined those above the "upper critical limit;" by *thin* plates, those below this limit of thickness.

6. Inspection of Table III., which gives the time-change of the polarization, will show a similar distinction between "thick" plates and "thin" plates, as was noted in the last paragraph, viz., that for thick plates the change is considerable and continues slowly for hours; for thin plates, the change of polarization with time is both less pronounced and extends over much less time.

7. It was noted, especially in the case of $CuSO_4$ as electrolyte, that there was polarization on gold-leaf if the gold exposed came in contact with the solution some distance beyond the edge of the hole in the glass plate to which it was sealed; thus in $CuSO_4$, for the stronger currents used, there was a symmetrical deposit of Cu, decreasing in thickness from the outside toward the centre, and vanishing at a small distance from the edge of the hole, this distance being less the stronger the current. If only one corner was left exposed, the Cu was deposited there. This phenomenon was further tested by bending a thick strip of aluminum, 4 centimetres long, into the shape of a narrow U, and simply hanging this U in the *open* hole of the glass partition, in $CuSO_4$, and closing the circuit on the voltameter; the two ends of the metal strip being thus in contact with the $CuSO_4$ on opposite sides of the glass two centimetres from the edge of the opening, there was decided deposit of Cu on one end and escape of oxygen from the other end.

8. In $CuSO_4$, all the plates except those below the critical thickness were destroyed by oxidation. No. 1 silver was destroyed is less than one minute. Of course, gold and silver above the critical thickness could not be used in NaCl, because of chemical action, though the thinnest plates were quite unaffected. Only the No. 7 gold was tested in KOH, as it dissolved the sealing-wax.

9. Thick plates of gold were strongly oxidized in H_2SO_4, especially with strong currents. Thin gold plates were apparently only oxidized under action of strong or long-continued currents. Compare Tables II. and III. Silver was even more easily oxidized than gold. Aluminum was so intensely oxidized by the current that no satisfactory measurements could be made for this metal, though the thin foil was unaffected.

10. With H_2SO_4 as electrolyte, after a thick plate of pure gold had been used as partition for the time-change of Table III., the end cathode was found to be gilded. A thick Pt plate being then substituted for the gold in the same solution for the results of No. 1 Pt in Table III., the Pt partition was found, on removal, to be gilded. The polarization for No. 1 Pt in this case was somewhat less than for the same Pt after both it and the end electrodes were thoroughly cleansed, the electrodes re-platinized, and fresh solution made.

11. The polarization in $CuSO_4$, using Cu electrodes, reached a maximum almost immediately and remained very constant. The maximum polarization for thick Pt in $CuSO_4$ was hardly 75 per cent of that for the same in H_2SO_4. In NaCl the polarization became constant very quickly also, but its value was decidedly greater, especially on thin plates, than in H_2SO_4; though the same distinctive behavior of thick and thin plates was maintained.

12. In H_2SO_4 of different concentrations the maximum polarization for a partition was of the same order of magnitude; but its value for very weak currents was decidedly greater in weak solu-

tions than for the same current in stronger solutions, up to 30 per cent. This shows itself especially with thin plates, and also in the shorter time required for thick plates to reach a maximum polarization with weak currents. The greater change in temperature and the greater change in concentration of weak solutions may account for this.

For currents between 0.1 and 0.2 ampere, the polarization on the end electrodes was:—

For H_2SO_4, 1.84;

" NaCl, 1.98;

" $CuSO_4$, 0.00, with Cu electrodes, though, if the current density was too great or the time long, the anode would oxidize and become irregular. C. Fromme, in a paper, "Ueber das Maximum der galvanischen Polarisation von Platinelektroden in Schwefelsäure" (*Annalen d. Physik u. Chemie*, Band XXXIII., s. 80–126), states that the maximum polarization varies both with the concentration and the relative size of the electrodes, the extreme limits being given as 1.45 to 4.31 volts—the minimum polarization coinciding with maximum conductivity. His method for measuring polarization was somewhat similar to that used in this work. As bearing upon "the change of polarization with time," I would refer especially to the investigation of Dr. E. Root upon this subject, discussed by Professor von Helmholtz, *Wisch. Abh.*, Vol.I., page 835. These experiments by Dr. Root seem to prove clearly that the liberated ions penetrate deeply into the electrode, even when liberated upon but one side of it, as in this case. I take great pleasure in expressing here my thanks and deep obligation to Professor A. Kundt and Dr. L. Arons for their kind sympathy and direction in this work.

Using $CuSO_4$ on one side of the partition, and H_2SO_4 on the other side, careful determinations have developed the curious fact that, although there is no visible development of ions (neither Cu nor O) at the gold-leaf partition, yet the Cu does not pass through the gold-leaf with the current, but H appears on the cathode instead, provided the current density at the partition be not greater than about .2 ampere per square centimetre.

The "critical current-density" at which the ions just begin to appear visibly on a gold-leaf partition varies for different electrolytes between the limit of 5.7 amperes for 30 per cent H_2SO_4 and sensibly zero for lead acetate.

This "critical current-density" is proportioned to the conductivity of the electrolyte. It therefore also has a decided positive temperature co-efficient.

ON THE FORMATION OF ALUMINUM SULPHATE IN THE SHALES THROWN FROM COAL-MINES.

BY M. H. LOCKWOOD, ASSISTANT IN THE DEPARTMENT OF GEOLOGY AND MINERALOGY, MISSOURI STATE UNIVERSITY.

My attention was recently called to a white crystaline formation found on and between the layers of a red-colored shale that is much used for walks in Columbia, Mo., and is obtained from the old waste heaps of coal-mines in the vicinity. Upon examination I found it to consist of aluminum sulphate, which is readily soluble in water, and has an alum-like taste. Occasionally some iron sulphate is present. The question arose as to how the aluminum sulphate was formed in between, and on, the layers of the shale.

For the purpose of studying the formation, I visited the Reece mine at Henry Station, on the Wabash railroad, and there collected the following waste materials as thrown from the mine, viz., fire-clay taken from below the coal, clay-parting from a layer about six inches from the bottom of the coal seam, iron pyrites mixed with coal from spots throughout the coal seam, clay containing iron pyrites and carbonaceous matter from just above the coal, and a blue argillaceous shale from above the coal.

The waste materials thrown from the mine, and exposed to the air and moisture, go through the process of slacking or burning, and it is during this process that the aluminum sulphate is formed. I also collected specimens from the burned and from the burning heaps about the mine.

Upon examination of the fresh specimens I found that the fire-clay contained no free aluminum compound that would form aluminum sulphate after the slacking or oxidation of the heaps. The clay-parting and other specimens containing iron pyrites and carbonaceous matter, will oxidize so rapidly when exposed to the air that the mass takes fire and we have iron sulphate and sulphuric acid formed. The sulphuric acid combines with the aluminum in the shales and clays about it, forming aluminum sulphate which crystalizes on the surface.

The shale from above the coal contains some simple compound of aluminum (probably the hydrate), and a considerable quantity of free sulphur. The presence of the aluminum was shown by the cobalt-nitrate test, and, also, when some of the shale was boiled with hydrochloric acid and filtered, the solution gave a white precipitate of aluminum hydrate upon the addition of ammonium hydrate.

Some pieces of the shale contained so much free sulphur that they would burn, upon ignition, with a blue flame, giving off fumes of sulphur dioxide. When some of the powdered shale was leached with carbon dioxide, and the solution evaporated, a residue of sulphur was obtained. These tests indicate that the sulphur and aluminum thoroughly penetrate the shale. When the heaps burn the sulphur becomes highly oxidized, and combines with the aluminum, forming aluminum sulphate within the shale. Heat drives the aluminum sulphate to the surfaces, hence it will crystalize between the layers and on the surfaces of the shale.

Free sulphur is found deposited in a crust at the top of the burning heaps. This shows that there is an excess of free sulphur in the waste materials.

The red color of the shale is due to the red oxide of iron formed when the water is driven off by the heat.

CURRENT NOTES ON ANTHROPOLOGY.—XXX.

[*Edited by D. G. Brinton, M.D., LL.D., D.Sc.*]

Prehistoric Ethnography of Northeastern Africa.

There are two very learned and suggestive articles in the *Beiträge zur Assyriologie*, Bd. II., Heft II., 1892, which may be combined to present the latest substantial opinions on the relations and sequence of linquistic stocks in the valley of the Nile and the lands adjacent. The one is by Franz Pastorius, on the Hamitic languages of East Africa; the other on the relations of the Semitic and Old Egyptian languages, by Fritz Hommel. In what I present on the latter theme, I have also had the advantage of a paper read before the Oriental Club of Philadelphia, by the able egyptologist, Professor W. Max Müller.

Scarcely any question in early ethnography could be more important. It touches directly on the origin of the two oldest civilizations of the world,—the Egyptian and the Babylonian. According to Hommel, the Old Egyptian of the Pyramid Texts, and the Old Babylonian (Semitic) tongues agree so closely in grammar, in sequence of words, in phonetics, and in lexicography, that their near relationship or their common origin must be admitted. Professor Müller informs me that in the Egyptian of the Rammesside epoch at least sixty per cent of the words in use were clearly Semitic. These relations are, however, distinctly not with the western Semites, but directly between the eastern Semitic (Babylonian) and the Old Egyptian. Hommel very pertinently adds that this by no means justifies the conclusion that the original home, *die ursprüngliche Heimat*, of the common stock was in the valley of the Euphrates; it might just as well have been on the Nile.

Some strength is given to the latter possibility by his comparisons of the Old Egyptian with the Berber dialects. He finds that the lexicon of these latter is Old Lybian, but that their grammar and syntax are very closely related to the Old Egyptian. There is no doubt but that the characteristic forms of the perfect and imperfect tenses were at one time common to the Berber, the Old Egyptian and the Semitic tongues. Besides these, as pointed out by Pastorius, the Hamitic (or Berber) dialects had in common with the original Semitic the personal pronouns, the feminine in *t*, and a number of minor structural elements. He is convinced that the East African Hamites (sometimes called Kushites) have been dwellers on the upper tributaries of the Nile, in Abyssinia, for many thousand years. Of their dialects, the Somali and Galla are much the most profoundly semitized, the Saho and Afar (Danakil) much less so. All these dialects stand in close relationship to the geographical features of the country, showing that they originated on the spot. They have both influenced, and been influenced by, the Ambaric (Ethiopic) Semitic stock, and to some extent by the Soudanese tongues. *Pari passu* with the language, the blood of the tribes has suffered from this intermixture.

The extreme interest of these conclusions cannot but impress all Semitic and Egyptologic students.

Fossil Human Remains in South America.

The critical scrutiny of the evidence of paleolithic man in North America, which has lately occupied considerable attention, has perhaps been pushed too far. When, as in the Ohio field, discoveries have been made which cannot be gainsaid, it is scarcely fair to prefer every conceivable explanation of them to the simplest one — that the articles were originally deposited where found.

Meanwhile, in South America, some interesting facts are communicated by Mr. Roth, of Buenos Ayres, to Professor Kollmann, and published by him in the Mittheilungen aus dem Anatomischen Institut, at Basel. Mr. Roth was the discoverer of the skull of "Pontimelo," which, by the way, he informs us is a typographical error for "Fontizuelos." This skull, together with some other human bones, was found under the carapace of a glyptodon of extinct species, and Mr. Roth argues that the man and the animal were contemporaries. He does not seem to have contemplated the possibility that men of later times may have found the carapace, and with it piously covered the remains of one of their dead. He asserts, however, that Döring, Burmeister, Ameghino, Moreno, and other leading geologists of the Argentine Republic, have acknowledged the contemporaneity of man and the glyptodon.

Roth cites a number of instances where human remains have been found in the upper Pampas formation. In 1887, he unearthed for the first time some in the middle Pampas strata; and, in the same, both he and others have found numerous pieces of pottery, an artificial shell-heap, and occasional silex points of human workmanship. He insists that there is no room for doubt that whenever the so-called "Pampeano Intermediar" was deposited, man was then living there. This time, if Ihering is right (see my note in *Science*, April 14), was in Pliocene (tertiary) days.

Professor Kollmann brings this into connection with other early finds in South America, and reaches the conclusion: "That the discoveries of ancient human remains in America prove that the various American races inhabited their continent quite as remote in time as did those of Europe and Asia, their respective localities;" which expression leads to the inference that he is a polygenist, or, else, seeks the cradle of the species outside these three continents.

The Ethnic Origin of the Jews.

In spite of the persistency of the typical Jewish physiognomy, it is proved by history that the Jews are far from a pure Semitic strain. They lived among and constantly intermarried with the Canaanites, Amorites, Philistines and true Hittites, none of whom were of Semitic blood; they bought Greek concubines, called in the Bible "pilegesh"; and, in turn, the males of many of the tribes around them, lured by the ever famous and still maintained beauty of the Jewish maidens, were quite willing to vow, "Thy people shall be my people, and thy God my God." In the Talmud these are called "proselytes of the King's table," and they were accorded honorable positions.

Such conversions by no means ceased with the destruction of Jerusalem by Titus. In the eighth century, Bulan, Prince of the Chasars, with all his people, embraced Judaism, and the repeated edicts in medieval time forbidding marriages between Christians and Jews can only be explained because such unions led the former to the faith of the latter.

At present, in all parts of the world, the prevailing anatomical type of the Jew is that of the brunette, with curly dark hair, dark eyes, often olive complexion, the skull long — dolichocephalic — the face rather narrow. This holds good for about ninety per cent of them; but nearly everywhere the remaining ten per cent— in Germany, over eleven per cent — are blondes, with light hair and eyes and round skulls — brachycephalic. In a much smaller percentage, the type is characteristically Mongolian, especially in the women, and about an equal number present negroid features. These aberrations from the ethnic type must be regarded as reversions through heredity to some of the numerous non-Semitic strains, which have, as above intimated, from time to time modified the pure current of Hebraic blood. That in spite of the number and extent of these admixtures the type has been preserved on the whole with such fidelity from the earliest Babylonian epoch, is a remarkable lesson in anthropology. An interesting discussion of the whole question by Von Luschan, Virchow and Alsberg may be found in the *Correspondenz-Blatt der Deutschen Gesellschaft für Anthropologie*, October, 1892. It effectually disposes of the absurd theory of Professor Gerland, of Strasburg, that the Semitic stock is a derivitive from the African negro — a theory which can only be explained by an anomalous degree of anti-Semitism obscuring his intellectual faculties.

NOTES AND NEWS.

A MEMBER of the Anthropological Society of Washington has placed in the hands of the treasurer of the society a sum of money to be awarded in prizes for the clearest statements of the elements that go to make up the most useful citizen of the United States, regardless of occupation. The donation has been accepted and the society has provided for the award of the following prizes during the present year (1893) under the following conditions: Two prizes will be awarded for the best essays on the subject specified above, viz.: A first prize of $150 for the best essay, and a second prize of $75 for the second best essay among those found worthy by the commissioners of award. These prizes are open to all competitors in all countries. Essays offered in competition for the prizes shall not exceed 3,000 words in length, and all essays offered shall thereby become the property of the Anthropological Society of Washington, the design being to publish the essays, at the discretion of the Board of Managers, in the official organ of the society, the *American Anthropologist*, giving due credit to the several authors. Each essay should bear a pseudonym or number, and should be accompanied by a sealed envelope bearing the same pseudonym or number, and containing the name and address of the competitor; and the identity of competitors shall not in any way be made known to the Commissioners of Award. Essays must be typewritten or printed, and must be submitted not later than November 1, 1893. While it is not proposed by the society to limit the scope of the discussion, and while each essay will be considered on its merits by the Commissioners of Award, it is suggested, in view of the character of the society and the wishes of the donor of the prize fund, that the treatment be scientific, and that the potential citizen be considered (1) from the point of view of anthropology in general, including heredity, anthropometry, viability, physiological psychology, etc.; (2) from the point of view of personal characteristics and habits, such as care of the body, mental traits, manual skill, sense training and specialization, and all-around manhood; and (3) from the ethical point of view, including self-control, humanity, domesticity, charity, prudence, energy, *esprit de corps*, patriotism, etc. The essays offered in competition for the Citizenship Prizes of the Anthropological Society of Washington will be submitted, on or about November 2, 1893, to five commissioners of award, including, it is proposed, one anthropologist, one jurist, one statesman, one educator, and one other not yet specified, all of national reputation, of whom at least one and not more than two shall be members of the society; and the award shall be made in accordance with the findings of these commissioners. Essays submitted in competition for the prizes should be delivered not later than November 1, 1893, to the secretary of the Board of Managers of the Society, Mr. Weston Flint, No. 1101 K street, N. W., Washington, D. C., to whom all correspondence relating to the prizes should be addressed.

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THE SEDIMENT OF THE POTOMAC RIVER.

BY CYRUS C. BABB, U. S. GEOLOGICAL SURVEY.

THE United States Geological Survey in May, 1891, established a gauging station on the Potomac River at Chain Bridge, D.C., for the measurement of the discharge of the river at that place. From that date to the present time daily readings of the height of the river have been maintained, which, taken in connection with the measurements of discharge, makes it possible to compute the daily discharges of the river at this place. A detailed account of the methods and results of this branch of the work may be found in a paper by the writer in the Transactions of the American Society of Civil Engineers, No. 537, Vol. 27, and entitled "The Hydrography of the Potomac River Basin." This article deals with the discharge of the river and its relation to the rainfall in the basin. It is also stated that measurements of the amount of sediment transported by the river were being made. The results are now available and are here given for publication.

Daily heights of the Potomac River at Great Falls, about sixteen miles above the City of Washington, have been kept since 1878 by the officials of the Washington aqueduct, together with a daily record termed "condition of water." Owing to the fact, however, that the dam across the river at Great Falls was not completed until 1886, the two sets of records previous to this latter date are valueless for discussion.

The results of this article are based in part upon the records of "condition of water," which are made as follows: A horizontal metallic tube, 36 inches long and with glass ends, is filled with water, and the distance at which a ball immersed therein can be seen from one end is noted. The observations vary from 1 inch in very muddy water to 36 inches, which is considered as clear. Samples of the river water at Great Falls were collected and were sent in to the main office in Washington, where they were analyzed quantitatively in so far as the determination of the ratio of the weight of the contained sediment to the total weight of the sample. At the same time the "condition of water" was recorded. Fifty-five samples were analyzed, with condition of water ranging from 1 to 36 inches. These quantities were plotted on cross-section paper, with condition of water as abscissæ and ratios of sediment to water as ordinates. Through the points thus obtained a smooth curve was drawn, from which a table was constructed, giving for each inch of condition of water the corresponding ratio of sediment. In order to obtain the total amount of sediment transported by the river for any length of time the discharge of the river for that period must be multiplied by the average ratio of the sediment to the water for the same period.

Simultaneous gauge readings of the height of the river at Great Falls and at Chain Bridge were maintained for a year and a half. From these observations a table was constructed, giving for the gauge height at one place the corresponding gauge height at the other. From the fact that no large tributary enters the river between these two points, the daily discharges at Great Falls may be computed from the table of gauge relations and from the daily discharges of the river at Chain Bridge. The daily ratio of sediment to water was found from the daily record of condition of water and the rating table of ratios and condition of water. Knowing then the daily discharge of the river in cubic feet per second and the daily ratio, it is simply a matter of multiplication of second-feet times the ratio times the weight of one cubic foot of water to obtain the weight of the total amount of sediment passing down the river per second. In this way the daily amounts of sediment from 1886 to 1891, inclusive, have been computed.

In considering the value of these figures, it would seem at first sight that the above method of measurement for condition of water was crude to base scientific results upon. The observations are not made for that purpose, but are more for the benefit of the fishermen in the vicinity of Washington. They have the advantage of being simple and inexpensive and can be maintained by an inexperienced observer. Another very important fact in their favor for this river is that, owing to the absence of lakes and extensive swamps throughout the basin, such as are found in the glacial region further north or the swampy regions of the extreme south, the coloring matter of this river is almost wholly due to mineral sediments and very little to vegetable deposits. It would be more accurate if daily samples of the water could be analyzed, but it would be expensive and would require a long time-interval before the results would be of value. There is a six years' record of condition of water, or over 2,000 observations. From a series of measurements certain average values for this record have been computed. Any one observation may depart greatly from this average, but when considered in connection with the total number of observations the effect of its departure from the mean is inappreciable.

The lowest record is 36 inches. In some cases the ball is just able to be seen at this mark; in others distant objects are plainly visible. There is here an arbitrary limit for the curve, which ought to extend considerably below this point, but taken in connection with the rest of the range, and especially with the upper part, where the ratios are large, the weight of this lower end is small. Errors will also arise depending upon the cloudiness of the day. However, errors due to this method of sediment measurement are not cumulative, but may be either plus or minus, and in a large number of observations tend to equalize each other.

It is therefore considered that the results are sufficiently accurate for all ordinary purposes.

The following facts are brought out. The average annual discharge of the Potomac River from a drainage area of 11,043 square miles is 20,160 second-feet, varying from 2,000 second-feet in time of low water up to 470,000 second-feet during the great flood of 1889. The total annual amount of sediment transported is 5,557,250 tons, or 353 pounds per second, and distributed through the six years from 1886 to 1891 as follows: 1886, 4,283,000 tons; 1887, 2,372,800 tons; 1888, 4,996,800 tons; 1889, 10,142,600 tons; 1890, 5,994,000 tons; and 1891, 5,544,300 tons. The average daily amount varied from 1 pound to 21,900 pounds per second. It is found from these figures that the average annual amount of sediment is to the weight of the annual discharge of water as 1 to 3,575. Assuming that one cubic foot of sediment weighs 100 pounds, this average amount of sediment would cover one square mile 3.98 feet in depth, and if spread over the drainage area would cover it 0.0043 inches in depth. At this latter rate it would take the river 2,770 years to erode one foot from the drainage area.

These results appear in the following table, together with similar data compiled for several other large rivers. The first column gives the name of the river; second, its drainage area in square miles; third, the average annual discharge of the river in cubic feet per second. The fourth column gives the total amount of sediment, in tons, annually transported by the river; fifth, the ratio of the weight of this sediment to the weight of the water annually discharged; the sixth, the height of a column in feet having a base of one square mile that the sediment would cover; the seventh, the depth in inches that the drainage area would be covered if this total amount of sediment should be spread over it; and the last column the authority for the data. The discharge and drainage areas for the Rhone, Po, Danube, and Uruguay are taken from a paper by John Murray in the *Scottish Geographical Magazine* for February, 1887. The drainage area of the Nile was measured by planimeter from the best maps obtainable.

Discharge and Sediment of Large Rivers.

River.	Drainage Area, square miles.	Mean Annual Discharge, second-feet.	Sediment.				Authority.
			Total Annual Tons.	Ratio by Weight.	Height column 1 square mile base, feet.	Depth over Drainage Area, inches.	
Potomac	11,(43	20,16	5,557,250	1:3575	4.0	.00433	
Mississippi [1]	1,214,000	610,000	406,250,000	1:1500	291 4	.00288	Humphreys and Abbot
Rio Grande [2]	30,000	1,700	3,830,000	1:291	2.8	.00110	U. S. Geological Survey
Uruguay	[3] 150,000	[3] 150,000	14,782,500	[5] 1:10,000	10.6	.00085	J J. Revy
Rhone	[3] 34,800	[3] 65,850	[4] 36,000,000	1:1775	31.1	.01071	J. Barois
Po	[3] 27,100	[3] 62,200	[4] 67,000,000	1:900	59.0	.01139	"
Danube	[3] 320,300	[3] 315,2 0	[4] 108,00 ,000	1:2880	93.2	.00354	"
Nile	1,100,000	[7] 113,000	[4] 54,000,000	1:2050	38.8	.00042	"
Irrawaddy [6]	125,000	475,000	291,430,000	1:1610	209.0	.02005	R. Gordon

GLACIATION IN PENNSYLVANIA.

BY EDWARD H. WILLIAMS, JR., LEHIGH UNIVERSITY, SO. BETHLEHEM, PA.

Owing to the difference of opinion regarding glaciation in this vicinity, I have taken the subject for the out-door work of the post-graduates in the mining course, during the past few months, as their geological survey, and I make now a preliminary statement of what has been found, without theorizing upon it in any way, as the work is to be continued and extended to adjoining regions.

The Lehigh University is situated on the north slope of what is called The South Mountain, or the Durham and Reading Hills, immediately back of South Bethlehem, Pa. The crest of the same varies from 665 to over 900 feet above tide, at the point mentioned. This is above the reach of glacial deposits by floating ice. To the north lies the great valley bounded by the Blue Ridge, and just north of this is a lower ridge of Oriskany sandstone in a vertical position. The nearest portion of this sandstone is therefore beyond the Blue Ridge. As the rocks of this ridge are mainly barren, while the Oriskany sandstone carries the usual fossils, this formation was taken as a test-rock, owing to the fact that the rock called Potsdam sandstone sometimes weathers so as to greatly resemble rocks of other formations.

Professor Salisbury stated that he had found glaciated stones 500 feet above the Lehigh River, on the mountain back of the University, and adduced that fact to refute the statement of Professor Wright, that the ice failed to come as far south as Bethlehem. The height of the point noted was proof that the specimen had not been brought by water, and that the ice-sheet had extended across the great valley. From this was deduced the idea that there had been two periods of glaciation, and that the one marked by the terminal moraine north of us, was the later of the two.

[1] "Report upon the Physics and Hydraulics of the Mississippi River," by A. A. Humphreys and H. L. Abbot, Philadelphia, 1861, p. 149.

[2] "Eleventh Annual Report U. S. Geological Survey," Washington, 1891, Part II., p. 57.

[3] Scottish Geographical Magazine, February, 1887, p. 76.

[4] "Irrigation in Egypt," by J. Barois, translated by Major A. M. Miller, Washington, 1889, p. 18.

[5] "Hydraulics of Great Rivers," by J. J. Revy, New York, 1874, p. 135.

[6] "Report on the Irrawaddy River," by R. Gordon, Rangoon, 1880, Part III. p. 25.

[7] Special Consular Reports, House of Representatives, 51st Congress, 2d Session, Ex. Doc. 45, Part I., p. 259.

To the south of that part of the mountain back of the University, lies a land-locked valley, so that there was no drainage southward, except at high levels, during the time when the bowlder clay was deposited and therefore there would be no current to divert icebergs into that valley and cause a universal distribution of that clay, as there is to the north of the South Mountain.

The preliminary work shows that Oriskany pebbles and bowlders are found at all altitudes over this mountain, and the great majority of the smaller ones lie in a clay, which may be due to the decomposition of the gneiss of the mountain; but which exists on the top of the highest part of the ridge. These have been traced into the Saucon Valley to the south as far as a line running from Friedensville to the second railroad cut south of Bingen. South of that line we find the clays from the subjacent limestones generally free from foreign stones, as far south as Centre Valley, the southern part of the survey. North of that line we have found four lines of glaciated material. In the valleys these run across all the formations from gneiss to limestone, in lines generally parallel, and with a freedom of glaciated material except in the lowest parts, where ice may have been present. Oriskany bowlders are found of considerable size, and in some parts abundantly. Only one of these lines has been traced fully, and that runs from the north of Bingen and at an elevation of about 300 A. T., across the Saucon Creek and Valley, and, passing south of Seidersville, has been followed to the summit of a hill west of the latter place, and at an elevation of 720 A. T.

It is comparatively easy to trace these lines, as the farms are provided with wooden and wire fences, except where these lines exist, and there the fences are of stone heaps, and the soil is stony. Digging under these lines shows that they are resting on rock in some cases, and on soil in others.

It may be said that the ice went over the South Mountain. In this case there has since been a great disintegration of the gneiss, as the cuttings for the South Bethlehem reservoir show 25 feet of rotten rock in some places. It may be said that these are evidences of a older glaciation; but this older intrusion followed exactly the lines of the later one, as can be seen by running a line from the points in New Jersey noted by Professor Salisbury (Pattenburg, etc.) to Seidersville, Pa., so that the advocates of a single period can say that this was a sudden intrusion for a short period followed by rapid retreat for twenty miles.

This work is not sufficiently extended to furnish data for theorizing, and it will be extended in the future; but attention is called to the fact that here exists a good field for observation, as the rocks of the country (gneiss quartzite and limestone) cause intrusive rocks from the Blue Ridge to be very prominent.

INFLUENCE OF PARASITES ON OTHER INSECTS.

BY G. C. DAVIS, AGRICULTURAL COLLEGE, MICH.

From a philantrophic standpoint, it seems cruel to see one class of insects preying upon another. The eager female parasite is so vigilant in her search that one would think a subject of her search could not escape till it had reached maturity; yet strategy, mimicry, offensive odor, hairy and other coverings, and many other peculiar and interesting methods of protection help to shield and protect the invader from its insidious foe till out of danger. In watching the ups and downs of the two from year to year, about the only effect that is noticeable is that the parasite generally holds the balance of power, though usually the balance is well equipoised.

Viewed from an economic and practical side, the practice loses its cruel aspect and is encouraged and fostered in many ways, as it means an inexpensive control of many of our common pests. There is little doubt but parasites do much more good than we are wont to give them credit for. In a large share of the cases of parasitism, about so many individuals of a species are parasitized each season, and the number left remains too small to produce serious damage. On the other hand, if the species had no parasite to contend with, it would soon be numerous enough to be a dreaded pest.

Very often certain species do appear in greatly increased numbers, and cause widespread consternation. No doubt climatic and other influences have much to do with these sudden up-risings, as we find species that are known to be parasitized but very little, which fluctuate in numbers greatly with different seasons. All the effect, then, cannot be attributed to parasites.

The difference between the work of parasites and other influences, is quite marked and distinct in certain channels, and can be easily traced. The tendency of parasites is to increase or decrease in numbers as the host is numerous or scarce. A few years ago the wheat aphis was so numerous over the wheat plants that it threatened to destroy the whole crop in this region. Presently certain of the aphis looked brown and swelled, which told plainly that the parasites were there too. The wheat grew and headed. Still the aphis increased by the thousands daily, and the parasites increased in numbers also. Then there came a time when the parasites were in the majority, and, before the wheat-heads had ripened, a live aphis was a scarce and hard thing to find. The next year the wheat aphis was not common, and what did appear were disposed of early by the parasites.

Sometimes the work of the parasites is not as prompt as the instance just cited. For illustration, the oak army worm, *Edema albifrons*, was never known to be numerous enough to greatly injure the oak till two years ago, when the species came in such numbers as to strip whole forests of their foliage. Of the several hundred caterpillars and pupæ collected, only one pupa was parasitized. Last year the trees were again stripped by countless numbers as the year before, but from the pupæ collected, about every one in ten was parasitized. Probably this year the caterpillars will be less numerous, and by next will be scarse, because of the work of parasites.

An ideal parasite is one that would keep its host in such camplete subjection that no outbreak would occur, and the numbers not great enough to do any harm. While the effect of parasitism is not ideal in every respect, it nevertheless is a boon to economic entomology, and has already been used to good advantage, by introducing many foreign parasites that are known to work on certain species. As we become still more familiar with these parasites and their hosts, much more good, through parasitic species will undoubtedly result.

EARTHQUAKES IN AUSTRALASIA.—II.

BY GEORGE HOGBEN, M.A., SECRETARY OF THE SEISMOLOGICAL COMMITTEE OF THE AUSTRALASIAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

In my former communication I explained the nature of the work that the Seismological Committee of the A. A. A. S. proposes to do anent earthquakes. In the present contribution I shall endeavor to sketch briefly what has already been done for New Zealand earthquakes.

The committee has published two reports, 1891 and 1892. The former was drawn up by Sir James Hector, F.R.S., and deals with New Zealand earthquakes to the end of the year 1890. It contains a list of earthquakes (537) felt in New Zealand from the earliest settled times, and gives interesting details concerning the somewhat severe shocks of October, 1848, and January, 1855. The last-named is notable as being one of the few in any country in which movement of the land has been actually observed by skilled observers on the spot. Captain Drury, R.N., was engaged at the time on the nautical survey of the New Zealand coast, and, being in the neighborhood of the land raised, was able by actual re-measurement to confirm the general impression. "An area of 4,600 miles was estimated to have been raised from one foot to nine feet, the greatest elevation being on the west side of the Wairarapa Valley, the vicinity of Porroria Harbor not being affected, and the west side of Cloudy Bay, north of Blenheim, having actually been depressed to the extent of five feet." (Transactions, A. A. A. S., 1891, p. 522). The elevation has been permanent. The same report (1891) contained a map by Sir James Hector, showing the seismic areas, principal fault and earthquake-rents in the islands of New Zealand. The division into seismic areas is not, however, based upon the determination of the earthquake origins, and, in the opinion of the present writer, is on that account misleading. At the same meeting of the Australasian Association (Jan., 1891), I read a paper on New Zealand Earthquakes, which contained a somewhat larger list (775) than the report of the committee, together with two maps and a diagram showing curves of monthly seismic frequency, the New Zealand curve based upon the records of 745 shocks being compared with Mallet's curves for the Northern and Southern Hemispheres — 5,879 and 223 earthquakes, respectively — (See Milne on Earthquakes, p. 256). The record for New Zealand shows a maximum of frequency in September, with smaller maxima in January and March, and minima in April and October, November, December. The inclusion of these facts might modify Mallet's curve for the Southern Hemisphere, but it does not appear that they point to any connection between earthquake-frequency and the season of the year.

One of the maps exhibited showed, by shading, the earthquake-frequency of the shocks in various parts of New Zealand, the region most effected being a portion of Cook Strait, included in the triangle Wellington, Blenheim, Wanganui; the next shade of frequency includes Christchurch, the next, Nelson. There is an isolated district of local earthquakes round Rotorua and Tarawera. On my other map were marked the epicentra of 35 earthquakes for which the data were sufficient to ascertain them with any degree of probability, and I have since been able to determine more or less exactly the origins of a few of the earthquakes of 1891–1892. The two chief sources are situated —(1) 10 miles north of Lake Sumner, or about 80 miles north-northwest of Christchurch. Hence proceeded the shock which on the 1st of September, 1888, threw down the upper portion of the spire of Christchurch cathedral. To the same origin I am able definitely to assign 10 other shocks, and probably many more belong to it. (2) 45-50 miles north-northwest of Wellington, in Cook Strait. This and some other origins near it are accountable for most of the New Zealand shocks, the average intensity being very low, III.—IV., on the Rossi-Forel scale.

The method used for finding the origins has been, in general, founded on the observed times of the shock at the several places at which it was felt, with the help of the isoseismals, when the effects were sufficiently definite to assign the degree of intensity on the Rossi-Forel scale.

One somewhat striking point in connection with all the recent earthquakes in New Zealand, is the low velocity of propagation they possess (less, with one exception, than 20 miles a minute). At first this made me doubt the correctness of the calculations, but the large number of shocks for which the velocity can now be approximately ascertained renders the results tolerably certain. In the solitary exception (an earthquake of the present year, which I am still investigating) the velocity is probably between 45 and 55 miles per minute. The depth of the origin has not been found in many cases, but in those for which the solution of the equations is most satisfactory, the depth is in each case about 24 or 25 miles below the surface.

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.

Some Recently Discovered Trilobites with Appendages.

THE past winter the Geological Department of Columbia College came into possession of some extremely interesting specimens of *Triarthrus Beckii*, which were discovered by Mr. W. S. Valiant, now of Rutgers College, in the Utica shales at Rome, N.Y. They were entrusted to W. D. Matthew, our fellow in geology, for complete description, and Mr. Matthew's paper, recently read before the New York Academy of Sciences, will appear in the Transactions of the Academy for May. Owing to the unavoidable delay in their issue, and because the subject is such an interesting one, this preliminary announcement is made. The Trilobites possess two undoubted antennæ that come out together from under the front-central portion of the head-shield, and project forward. They are jointed and entirely analogous to the antennæ of living crustaceans in structure. In full or in stumps, they have been identified on upwards of fifty individuals, some twenty of which belong to Columbia College. On one specimen, where the cheeks have been broken away, Mr. Matthew has detected comb-like structures, which we suppose to be gills. Leg-like appendages are well preserved, opposite the divisions of the body.

At the posterior end of the pygidium, tetson-like appendages can be distinguished, which are of great interest, and which are regarded as perhaps indicating an ability in the animal to propel itself backwards, as does the lobster, although its ordinary motion would be forwards, by means of its legs. Mr. Matthew brings out some other interesting facts and deductions, which will be illustrated by drawings in the full paper. J. F. KEMP.

Columbia College, May 26.

Cedar Waxwing.

IN view of the articles published in your paper during the past few months regarding the plumage of the cedar waxwing (*Ampelis cedrorum*), it may be of interest to call attention to a paper published in the "Transactions of the Norfolk and Norwich Naturalists' Society," Vol. III., pp. 326-344 (read Nov. 2, 1881), by Henry Stevenson, in which there is a very full discussion of the plumage of the allied Bohemian waxwing (*Ampelis garrulus*).

The presence of the wax-like tips in nestling birds is here recorded, and several captures of young in this plumage are referred to; the first nestling secured with red tips to the wing-feathers seems to have been taken by one of Mr. Wolley's collectors in Finnish Lapland in 1856. WITMER STONE.

Academy of Natural Sciences of Philadelphia.

Native Lead.

IT may be of interest to mineralogists to note a new locality for native lead, which occurs near Saric, Sonora, Mexico, about 35 miles south of this place.

The metal occurs in thin scales; and pellets, like small shot, have been reported, but I have not seen them. The scales seem to approach a rectangular form, and have been found nearly an inch long.

The gangue rock is evidently a pyrocene, of pale-green color, streak yellowish. The accompanying minerals are iron oxides, with traces of manganese, and carbonate of lime.

C. W. KEMPTON.

Oro Blanco, Arizona, June 8.

The Ancient Egyptian Language.

IT is the growing opinion of scholars that the ancient Egyptian language has more intimate Semitic relationship than has been generally admitted. The grammatical construction of Egyptian is distinctly Semitic; the pronouns, prepositions, and other particles are traceable for the most part to Semitic roots; the Semitic system of pronominal suffixes is often used. Benfey sought to establish this affinity by various considerations, grammatical and lexicographical, and the conclusion to which he came was that the Semites are only one branch of a great family, which includes not only the Egyptian, but also the other languages of Africa. De Rougé, Ebers, and Brugsch have declared their belief in the descent of the Egyptian from the same stock as Semitic. Dr. Fritz Hommel, in his recently-published brochure, "*Der babylonische Ursprung der ägyptischen Kultur*" (Munich, 1892), brings forward many proofs showing the Semitic origin of the Egyptian language and writing. He not only specifies a number of identical words, but shows the grammatical relations of the two languages. He also puts side by side some thirty-five characters which resemble each other in the two languages, both in form and signification, and even in sound. Dr. Hommel maintains that Egyptian culture originated in Babylonia.

In this connection we may mention the interesting fact that the Egyptian documents recently discovered in Palestine, rigorously transcribed in Hebrew characters, gave almost everywhere the regular Hebrew forms in the Bible, without change or correction. CHAS. H. S. DAVIS.

Meriden, Conn.

Funnel-Shaped Clouds.

DURING the afternoon of May 17 there appeared not far northeast of Colorado Springs numerous cloud-masses resembling incipient thunder-storms. They were not so large as ordinary thunder-storms. From a cumulus mass depended the fringes that mark the storm-cloud, but they were unusually long as seen in profile at a distance. Only a little rain fell from any of them, and none from most of them. From the centres of several of them also depended funnels or narrow cones. In one case this column reached fully one-fourth of the angular distance to the ground, the others nearly as far. The columns changed their form somewhat, but I could not discover any marked swaying or writhing, perhaps owing to the fact that those observed were at a distance. At the time the surface winds were light and variable, but the following days have been marked by very violent winds. These were nearer the tornado than I ever before saw in Colorado. G. H. STONE.

Colorado Springs, Col.

Glaciers in the United States.

AT this season of the year many scientists are preparing to visit and study the glaciers of Switzerland, that country being the Mecca of geologists who are converts to the glacial theories. I desire to call the attention of the readers of *Science* to the fact, that here in Pierce County, Wash., we have a system of glaciers surrounding Mount Tacoma, beside which those of Mt. Blanc are insignificant, both in area and distribution.

The glaciers of Mount Tacoma are eighteen in number, and are arranged in radial lines from the central dome of the mountain, which is 14,450 feet in altitude. As this mass rises from the sea-level, it is the most conspicuous peak in the United States. The limit of perpetual snow on the spurs is 4,000 feet, while the glaciers and snow-fields that lie in the cradles extend as low as 2,700 feet. With care, the glaciers and spurs are not over-dangerous travelling. The scenery is superb, and well repays the many campers who yearly seek the mountain slopes for health and recreation. About fifty persons have attained the summit, including two ladies. The glacialist may there study moraines, terminal, medial, and lateral, and make observations on the flow of ice to his heart's content.

If any of your readers desire further information upon this subject, it may be obtained gratis by addressing

FRED. G. PLUMMER,
Secretary Washington Alpine Club, Tacoma, Wash.

Tacoma, Wash., June 1.

Binocular Vision.

Professor LeConte's remarks on my note about binocular vision seem to call for a word or two in addition from me. Of course I should not have troubled the readers of *Science* with my ways of looking at things, had I not known that they were unusual, and quite at variance with everything accessible to me on the subject, including Professor LeConte's own excellent little book, to which he makes reference, and had I not also been quite certain of the subjective part of the phenomenon. It is now about ten years since I noticed it first. Though a student of physics, I had not then read enough of physiological optics to have met with any thing on this subject, hence I had not been told what I must expect to see — a fact that I have no doubt is responsible for my unhappy deviation from established rules. Since that time I have tried the experiment under every available set of conditions — almost whenever I have found myself looking at any kind of a pattern. I have tried it with perfectly flat decoration, relief, and actual net-work, such as the bottom of a cane-seated chair, or a coarse wire-cloth, always with the same result. The illusion is quite complete; I seem to be looking at an actual pattern. The use of a material point of regard, as the tip of the finger, was not, as Professor LeConte seems to have understood me, to aid in properly fixing the axes of the eyes, but simply to make sure on which side of the actual pattern the horopter lay — the all-important fact in the experiment. I may add, that in my case the coalescence of the images is easier with a more distant than with a nearer point of regard — contrary to Professor LeConte's experience. It seems to me that it would be valuable to secure some additional evidence as to the way in which the phenomenon strikes a person who has had no previous knowledge of its existence, say by using a stereoscope without lenses, fixing the distance of a point in monocular vision and then suddenly introducing a pattern, the observer being simply asked to estimate its distance. In closing, let me say that I lay no stress on my remarks in explanation of my own case. It really is more or less of a mystery, but it surely need not remain so. The abnormal eyes of Dalton did great things for the theory of color vision, and indeed it is from the abnormal more than from the normal cases that fruitful trains of thought are apt to take their rise. I esteem myself fortunate to have interested Professor LeConte, and I hope that this is by no means the last thought that he will give to the matter.

ARTHUR E. BOSTWICK.

Montclair, N.J., June 19.

A Night-Singing Cat-Bird.

PERHAPS it is not a rare occurrence, but I never heard of such a thing before, and I give the incident for what it is worth. A few evenings since I heard a cat-bird sing for nearly an hour just before midnight. The weather was mild, with not enough moonlight to cast a shadow. The bird's song was somewhat intermittent and scarcely so rapturous as his usual sunset or sunrise singing. In the intervals there occasionally came one or two of the mewing utterances characteristic of the bird.

A. STEVENSON.

Arthur, Ontario.

Is it a Paleolith?

A STONE axe has just been found in a field about eight miles northeast of this place, which very nearly proves (if not quite so) that man existed during, or prior to, the glacial period in North America. It was found by A. A. Newlin, on the summit-level in this (Parke) county, Indiana, on the south side of Sugar Creek. It is $6\frac{7}{10}$ inches long, $2\frac{3}{4}$ inches wide on the blade, $4\frac{3}{10}$ inches wide at the groove (or eye of our steel axes), $3\frac{1}{2}$ inches wide at the "back," or "poll," and is $1\frac{6}{10}$ inches thick, and, I am confident, was, when first made, nearly two inches thick.

One side is ground flat, and by glacial action, without any doubt. By that grind the groove was planed almost out on that side, and has been re-cut or filed out by some Indian long ages after the Indian who first fashioned the axe. The striations run from edge to poll, and the axe was moving edge forward, as the striations indicate, for they are deeper cut toward the edge, and weaker, become shallower and less distinct, toward the poll.

The opposite, or convex, side of the axe has been striated just enough to produce a distinct plane, which inclines to (or from) the flat side about eleven degrees.

The poll, the ends (as timber men call that part of the axe nearest to and farthest from the hand when using), the present convex side, and the grooves around the ends show the deepest and oldest weather-pits. Then the glaciated, flat side shows the next oldest weathering. Next, the newly-deepened groove on the flat side, and, also, a little deepening of the groove on the convex side, where the grinding had made the groove somewhat shallow, show the next oldest weathering; and, last, the smooth, whetted edge shows very little weather wear.

This axe was found about one hundred miles north of the southern boundary of the glacial drift on the Wabash River. I have found eleven places in the county where the rock, in place, is strongly and clearly glaciated, and three places have been found by other parties. The erratic bowlders which are striated on one to five sides are countless (to say nothing of those not marked), and I have examined them and studied them a great deal, and think I am not a bad judge of their comparative exposures and decompositions. As a result of my experience and judgment, I am strongly inclined to believe that this axe was made before, or during the glacier. That it was lost, or in some way fell into the sweep of the glacier and was ground flat on one side and striated a little on the other. That, after the glacier had receded, it was found, repaired, sharpened, and used till the steel tomahawk took its place, when it was cast aside. I feel confident that experienced archæologists will so decide.

JNO. T. CAMPBELL.

Rockville, Indiana.

Cloud Formation.

I wish to call the attention of meteorologists to a rather peculiar phenomenon witnessed by me several times last winter.

The slough between King's River and the San Joaquin, overflows in seasons of high water, causing dense growths of tule (*Scirpus lacustris*, or round tule, and *Typha latifolia*, or flat tule), often ten feet high. The buccaroes of the large stock ranches burn the dead matter in winter, to clear the land that the stock may get the young feed.

On Jan. 28, at 3.30 p. m., I noticed one of these fires. The wind was northwest, slight, and quite warm; the weather had been showery for a few days previous, but, saving a few clouds of the cirrus type, the sky was clear. The fire was not extensive, but made a dense smoke which rose in a nearly perpendicular column, nearly 2,000 feet, when it met a counter current of air from over the Coast Range, as evidenced by its drifting abruptly away to the northeast.

All this is a natural result of the topography of the country; but what arrested my attention was a cloud of the cumulo-stratus type, resting on the top of the column at the point of flexure, like a cap. It did not appear to drift away, nor did it grow larger or diminish, save that from its base it gave off a cloud of the nimbus type, that mixed with the smoke and gradually increased and extended, till, at about 10 o'clock p. m., it extended across the northern horizon, like a dense rain cloud. Meanwhile, other clouds began forming at sunset, and it rained before morning.

On Jan. 29, it cleared away, another fire was started, the smoke rose in a column to the same altitude, struck the current, and drifted away, no cloud forming. The same thing happened on the 31st. On Feb. 1, the apparent conditions were the same, save a few clouds came in from the coast, but were soon dissipated. In the afternoon I saw the fire start, and watched it. The smoke rose as before, and struck the upper current of air. Immediately a cloud formed. In less than half a minute it had reached its usual size, as large as the column, which it seemed to cap. It was a dusty day, so the column was often broken. I saw it blown from under the cloud, and a new one form three times in about five minutes.

I now noticed that, whereas the smoke drifted down the wind, with its upper surface a horizontal plane, the liberated clouds ascended into the wind in the manner of a kite. Once outside the influence of the smoke, they were dissipated like the rest of the clouds. General showers prevailed throughout the valley for the next three days.

Reasoning from my limited knowledge of physics, I might think the cloud was caused by a column of heated and vapor-laden air rising with the smoke, and being cooled by coming in contact with the upper current, causing its vapor molecules to agglomerate into cloud particles; but, for various reasons, I think this inadequate. I have since seen the fires several times, with a southerly wind, which generally brings our rain, but no cloud formed.

I have seen a theory advanced that vapor molecules need some solid nucleus to start the process of agglomeration. Can any one tell if this be so, and, if it is so, the rank that carbon takes as a condenser?

I would also like to know why no cloud formed save in a "chronic" state of the weather; and finally, why did the liberated clouds float into the wind in opposition to all known physical laws?

I cannot help but think that had a meteorologist been on the spot, he would have been able to throw light on the subject of cloud formation and precipitation. ALVAH A. EATON.

Riverdale, Cal., May 20.

Birds that Sing in the Night.

I WAS somewhat surprised that the writers under the above caption in the Dec. 2 and 16 Nos. of *Science* omitted some of the most familiar night-singers of the Atlantic seaboard of the latitude of New England. While never having heard some of those mentioned, I have often heard the field-sparrow, *Shizella pusilla*, break forth into rapturous song by night, especially if the moon be shining, at the nesting period.

Another of the most common night-singers is that songster of songsters, the prince of the thrushes, the Wilson's thrush, or "Marten," *Turdus fuscescens*. During late May, June, and early July he prolongs his vespers till nine or ten o'clock, and often breaks forth at intervals throughout the rest of the night.

Another songster is the cuckoo, whether the yellow or blue-billed, I know not. He generally sings in the low ground, and is popularly supposed to foretell rain. "Oft in the stilly night," while the moon was playing hide and seek with the clouds, and a thin mist was creeping slowly over the landscape, have I heard the "rain-bird's" voice come weirdly from the swamps. At first low and indistinct, perhaps owing to the inequalities of the atmosphere, a few steps may suffice to place one so it is heard with startling distinctness. At such times the sweetness of his voice is enhanced, and, as the clear, liquid notes swell on the stillness, we forget to quote Shakespeare: "The nightengale if she should sing by day, would be thought no better a musician than the wren," but rather burst into the rapturous quotation of a later poet:—

"O cuckoo! shall I call the bird
Or but a wandering voice!"

Then we forget the songster's ill-repute as an egg-thief, forget his benefits to the agriculturist, and love to feel the author of this melody is of supramundane origin, and not of earth, earthy. If a few birds' eggs mixed with a diet of tent caterpillars will make such a voice, let him have them, by all means. I heartily believe the bluejay is author of most of the mischief laid to his door, as I have seen him take both eggs and young of the smaller birds.

The horned lark, *Otocoris a rubeá*, is the most common night-singer in California, at least the valley. ALVAH A. EATON.

Riverdale, California.

Books for Children.

MR. FRANK WALDO, in *Science* for June 16, asks for lists of books that will enable children of ten to call by name the natural objects they meet in their rambles.

He says that those books which he has seen do not give the "necessary details." Therein lies the difficulty with children of ten years of age. As soon as the necessary details are given so many scientific words have of necessity been used that the results are beyond the comprehension of the clientage to which he proposes to cater. Those whom he wishes to reach, need just what he himself states at the end of his letter he was so fortunate as to have, viz., a personal guide and instructor.

The best book. for children, about flowers, with which I am acquainted, is Gray's, "How Plants Grow." Bright children of 12, if properly instructed, could use it in the woods and fields and find out, without the presence of a teacher at the time, the name of any of the larger and more interesting of the flowers, excluding, of course, the golden rods, daisies and other compositæ.

In the correspondent's state, New Jersey, there are several hundred species of birds, and many of them have nests and eggs so nearly alike, that by them even oölogists cannot tell the species with certainty. Most birds give several different notes, some an extensive range; nearly all sing differently at different times of the year. The bird book asked for — one that will enable a person of ten or any other age to name "free birds" without a teacher — is an impossibility.

The best book on birds, is probably Coues's "Key to North American Birds," but it could not be used by children under 15.

French's "Butterflies of the Eastern United States," is probably rudimentary enough for children of 12–14, provided a little preliminary work were done by an older person who understands the vocabulary used by the author. My own "Trees of the Northern United States" deals only with the leaves, bark, and occasionally the fruit, and contains as few scientific words as possible, and those are all defined with added illustrations whenever at all necessary. This fact, and its containing an accurate picture of the leaf of each species, ought to enable even those of ten to use the book. Binney's "Land and Fresh Water Shells of North America" contains illustrations of all the species, and, as far as these will enable one to name shells, ought to be all right for children. I attempted in my "Mollusks of the Atlantic Coast" to make an easy book to be used by children of 14 or more in naming the shells of the shore.

The beetles are too numerous in species for any book, large or small, expensive or otherwise, to enable children or even grown people to name all or even a majority of them. The moths are also very numerous in species and so far no one book, cheap or high priced, names them all. AUSTIN C. APGAR.

Trenton, N. J.

Teaching of Biology.

THE recent discussion in the pages of *Science* as to the methods of teaching biology now in vogue in this country, has brought out much that is of interest to all who seek to present that subject in a fair and unbiased manner to their students. Ignoring the controversial phase, which too many of the letters have shown, is there not, after all, the question yet remaining—How shall biology be taught?

Circumstances are alike at no two colleges in this country; differences of courses, students, surroundings and many other factors make it necessary that each teacher should solve the problem for himself. But in a large number of institutions the plan of study is such that unless a student elects to specialize in biological lines he will receive but one, or at most two, terms of training in natural history.

In such a case what is the best plan to adopt? A course in botany will give the student a slight acquaintance with some of the flowering plants only if the course be the one usually given in such cases. On the other hand, a course in zoölogy would leave the student with no knowledge of plants and but little of animals. He will receive no farther work in either line during his course. What will be the best for *him* in his life after leaving college?

After much consideration, the writer has sought to find a way between the two horns of the above dilemma by seeking to present fundamental principles, illustrating and demonstrating them by examples taken from either the animal or vegetable kingdom, as might be most advantageous. In this work the form itself has received far less stress than the principle which it illustrated, and the bearing it might have on the question whether the course was more botanical than zoölogical was not for a moment considered.

It was found convenient to begin by assuming that, in a degree, animals and plants are machines for the transformation and utilization of energy; adding to this, during the course, a consideration of the problems which must be successfully met to ensure existence and comparative study of the various ways in which these problems are solved.

The result of this course has been to encourage me to continue along these lines, reserving for psychology, which follows, the task of checking any tendency to regard living things as machines only. Looking over the ground covered, I find that nearly two-thirds of the examples chosen to illustrate the various principles were from the vegetable kingdom. Surely, whatever else it may be, this is not a course in zoölogy masquerading under the guise of one in biology. Whether it agrees with the original meaning of the word "biology" or not, I care but little, for many other words of our language are very far to-day from their original significance, while a study of the principles shown by "matter in its living state," is certainly not very far from the significance of the words from which "biology" is derived.

H. T. FERNALD.

State College, Pa., June 9.

BOOK-REVIEWS.

Fifteenth Annual Report of the State Board of Health of the State of Connecticut, for the Seven Months Ending June 30, 1892. New Haven, 1893.

IN addition to the customary features, the statistics upon the health of towns and the reports of local boards of health, this volume contains three papers under the heading "Miscellaneous." The first of these is by Dr. Herbert E. Smith, upon "Connecticut River Water as a Source of Typhoid Fever at Hartford." The unusual number of cases of typhoid fever in Hartford in the winter of 1891–92 led to an investigation by Dr. Smith, under the auspices of the State Board of Health. Dr. Smith has fixed the responsibility for the outbreak upon the water of the Connecticut River, which was supplied to citizens of Hartford during a large part of the months of October, November, and December. The period when the disease specially prevailed "corresponds with the time when cases originating in the use of the river-water must have appeared." From the evidence adduced by Dr. Smith there seems every reason to believe that typhoid germs, carried to the consumer by the river-water, were the cause of some fifteen deaths that otherwise might not have occurred. Dr. Smith suggests that the germs might have come from the sewers of Springfield, twenty-five miles up stream, and this seems certainly a likely supposition, if we are to judge from the history of similar epidemics.

The paper which follows this is by Dr. Charles J. Foote, upon "The Filtration of Water." It relates some experiments upon the Pasteur-Chamberlain filter, certain of which seem to indicate that defective packing around the porcelain may lead to the appearance of germs in the filtrate. As a result, partly of his own, partly of others' investigations, Dr. Foote concludes with the following practical advice: "We may conclude then, first, that the porcelain cylinder of a Pasteur filter should be cleansed once a week at least by boiling in water for an hour. A simple washing is not sufficient, since, when the porcelain is replaced, the living bacteria still remain in its pores and come through into the filtrate as soon as the water is turned on; second, that the filter should not be put in a position where it is near a range or any other apparatus giving out much heat, but put in a cold place; third, that a properly-constructed filter should be obtained, so that there is no chance of a leak around the packing."

The third paper is entitled, "Abstracts from a paper on the Duty of Public Disinfection following Acute Infectious Diseases," and is a remarkable production, as the following pregnant sentences may suggest: "One afternoon last December I sat upon the deck of the revenue cutter, 'Lot Morrill,' with the secretary of our State Board of Health." . . . "Smallpox has been lashed to hell by the agent vaccination, and like a whimpering hound is held securely in leash." . . . "Arms, arts, literature, science, all have their rewards, but not one of them surpasses in the magnificence of its gifts those of which the god-like science, medicine, is capable."

The Archæan Formation of the Abukuma Plateau. By B. KOTO. Journal of the College of Science, Imperial University, Japan. Vol. V., 1893. Plates.

THIS article of nearly one hundred pages and six well-executed plates shows us that Japan is not behind the western countries in scientific studies. Except a few cabalistic signs on the cover, and a few more on one page at the end of the article and a foot-note or two, there is nothing to indicate that it might not have come from an English or American printing office. Yet the whole of it, plates and text, is the product of a Japanese office. It is also a striking example of the extended use of the English tongue when a paper of this character appears in our language instead of French or German.

The paper is the result of several years of study by the author, and shows great industry and research. The Archæan is divided into an upper and lower division. In the former are placed the Gozaisho and Takanuki series, and in the latter the Laurentian. The Gozaisho series is estimated to have a thickness of 10,000 metres. The Takanuki series was found too distorted to estimate the thickness. Many details of sections are given, and there is also an extended discussion of the origin of the various rocks, and of their lithological characters. The paper is worthy the attention of students of the Archæan formations.

JOSEPH F. JAMES.

May 5.

The Microscopical Examination of Potable Water. By GEO. W. RAFTER. New York, D. Van Nostrand Company. 160 p. 18°. 50 cts.

THIS little book, forming No. 103 of the Van Nostrand Science Series, fully bears out the character of preceding volumes, and, like them, will be well received by all. The plain language used puts it at once within the limits of beginners in microscopical water analysis, and, at the same time, it affords a handy *résumé* of work done for the use of the professional student. Care has evidently been taken by the author to give as completely as possible "the state of the art" to the date of issue, and in this particular the book is in advance of some of its more pretentious rivals.

It is divided into two parts, the first treating the subject qualitatively, the second quantitatively, while appended is a bibliography of water examinations, both from the chemical and biological standpoint. These two fields of research, chemical and biological, are each given their proper place throughout the volume, and, more fairly than is usually the case, the author believes them of equal value when in conjunction with a study of environment, and equally valueless when used alone. It has become a habit among biologists, latterly, to decry chemical methods altogether, while chemists, in their turn, have been rabid in their defence, hence the stand taken by Mr. Rafter is particularly refreshing, and the more so that he is eminently a biologist, and has arrived at his conclusions through calm reasoning. C. P.

AMONG THE PUBLISHERS.

WE have received from D. Appleton & Co., their "Guide Book to Alaska and the Northwest Coast," prepared by Eliza R. Scidmore. It gives a quite minute description of the whole coast of North America, from the Strait of San Juan de Fuca to the Aleutian Islands, with brief notice of the more northern portions of the great Alaskan peninsula. Though written for tourists, it is by no means confined to the superficial aspects of the country, but gives attention to the geographical features, the industries and commerce, the mountains and glaciers and the native tribes, presenting a larger amount of information than might be expected in a book of one hundred and fifty pages. Such a book is necessarily a compilation from various sources; but the author has evidently studied the best authorities, and has supplemented them by her own observations. The country described has certain special attractions for tourists, especially those of scientific proclivities, since the geological formations, the flora and the native inhabitants present some remarkable peculiarities. Many scientific observers, as well as other tourists, have already visited the region, especially its southern part; yet even now it is so little known that visitors who wish to see as much as possible there will find a guide-book indispensable. To such persons, therefore, this handy volume, which is well illustrated with maps and pictures, cannot fail to be useful.

Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

I wish to exchange a collection of 7,000 shells, 1011 species and varieties, American and foreign, land, fluviatile and marine, for a good microscope and accessories. Address, with particulars, Dr. Lorenzo G. Yates, Santa Barbara, California.

For exchange.—I wish to exchange Lepidoptera of South Dakota and other sections, for Lepidoptera of the world. Will purchase species of North America. Correspondence solicited, particularly with collectors in the Rocky Mountains, Pacific coast and Hudson's Bay regions. P. C. Truman, Volga, Brooking county, South Dakota.

For sale.—Wheatstone Bridge wire, made to order, new and unused. Price, $10. W. A. Kobbe, Fortress Monroe, Va.

For sale or exchange.—One latest complete edition of Watt's Dictionary of Chemistry, in fair condition; one thirty volume edition (9th) of Allen's Encyclopædia Britannica, almost new. Will sell cheap for cash or will exchange for physical or chemical apparatus. Address Prof. W. S. Leavenworth, Ripon College, Ripon, Wis.

Exchange.—One celestial, one terrestrial globe, one lunatettis and charts, celestial maps, diagrams and ephemeris from 1830 to 1893, astronomical works, all in good condition. Will sell cheap or exchange. Make offer. C. H. Van Dorn, 79 Nassau St., New York.

The Rev. A. C. Waghorne, New Harbor, Newfoundland, wishes to sell collections of Newfoundland and Labrador plants, all named by competent botanists. He is going on a missionary journey along the Labrador coast, from the middle of July till October, and in return for much needed aid towards (Episcopal) Church purposes in that region, will be glad to be of service to any botanists who may write to him. Letters posted in the U. S. up to July 1 will reach him at the above address, and if posted later will be forwarded.

For exchange—I wish to exchange cabinet skins of Californian birds or mammals for any book on the following list, books if second-hand to be in good order. Manual of Vertebrates, fifth edition, D. S. Jordan; Nests and Eggs of North American Birds, Oliver Davie; Marine Mammals of the West Coast of North America, C. M. Scammon; The United States and Mexican Boundary Survey, Vol. II., Zoology, S. F. Baird. F. Stephens, Witch Creek, San Diego Co., Cal.

Minerals for exchange—John Holl. Rollo, Wilmington, Delaware.

Wants.

AN experienced teacher in general biology wishes a position in a first-class college or university. Three years in post-graduate study. Extensive experience. Strong indorsements. Address E. W. Doran, Ph.D., 1327 G St., N. W., Washington, D. C.

THREE teachers wanted for a male and female seminary in central New York. Typewriting, etc., languages, mathematics, sciences, *et. al.* Send stamp with and for particulars. Box 701, Hempstead, L. I.

A ZOOLOGICAL collector and taxidermist of ten years' experience in the field is now open to engagement, for either field or laboratory work. References furnished. Address Taxidermist, Box 75, White Sulphur Springs, West Va.

WANTED.—A set of Allen's Commercial Organic Analysis, 4 vols. Vols. I. and II. particularly desired. Condition not important, all leaves being present and in place. Address Charles Platt, 34 Lewis Block, Buffalo.

WANTED.—Second-hand copy of Ehrenberg's Radiolaria, Berlin, 1875. Selected diatom slides, cash or both in exchange. D. C. Lewis, M.D. Skaneateles, N. Y.

WANTED, as principal of a flourishing technical school, a gentleman of education and experience who will be capable of supervising both mechanical and common school instruction. Special familiarity with some technical branch desirable. Address, giving age, qualifications, etc., J. B. Bloomingdale, Fifty-ninth street and Third avenue, N. Y.

WANTED.—A young man as assistant in our microscopical department. Queen & Co., Philadelphia.

THE undersigned desires specimens of North American Gallinae in the flesh for the study of their pterylosis. These species are especially desired: *Colinus ridgwayi, cyrtonyx montezumae, deudragapus franklini, lagopus welchi, tympanuchus cupido* and *pedioecetes phasianellus.* Any persons having alcoholic specimens which they are willing to loan or who can obtain specimens of any of the above are requested to communicate with Hubert Lyman Clark, 3922 Fifth Avenue, Pittsburgh, Pa.

A COMPETENT TEACHER of botany in college or university is open to engagement. Address L., Box 86, Rochester, Mich.

SCIENCE

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SCIENCE

NEW YORK, JUNE 30, 1893.

THE EVOLUTION OF CONSCIOUSNESS AND OF THE CORTEX.

BY C. L. HERRICK, PROFESSOR OF BIOLOGY IN DENISON UNIVERSITY, GRANVILLE, OHIO.

It would be difficult to find an illustration of the mutual interdependence of the biological sciences more striking than that afforded by the recent contributions from morphology, embryology, physiology, and pathology to our knowledge of the significance of the cerebrum.

It has been customary to scout at any substantial contribution to psychology from the experimental sciences, and even now, when so much attention is given to psycho-physics or physiological psychology, far too little use is made of the data of modern embryology and histology. It should be apparent to all who are not *a priori* convinced that no relation exists between consciousness and the nervous system, that no satisfactory super-structure can be reared upon any other foundation than that afforded by a minute study of the structure and function of the brain.

It is the great triumph of modern embryological histology, with Professor His as its leader, to have discovered the essential similarity of all nervous elements. What Schwann did for biology at large by means of the cell theory, Professor His has done for neurology through his theory of the neuroblast and its supplement, the "neuron theory."

It was inevitable that we should soon recognize the essential similarity in origin and structure of all nervous cells. The present writer has insisted for some years that the entire fabric of the nervous system, with the exception of a few connective and nutritive elements of secondary nature, is woven by the interblending of neurons of similar character. All such neurons are formed from the epiblast or its derivatives (the perplexing relations of the sympathic system aside). Each neuron arises from neuroblasts or formative cells springing from the ectal surface (ventricular surface by invagination in the case of the axial nervous system), and, after migration to its definitive site, takes on its distinctive character. It has been attempted to show that these are all transitions between the neuroblast and the wonderful variety of nervous elements. The nerves, whether springing from a special ganglion or from the neuraxis itself, are formed, in our view, from the moniliform union of neuroblasts, whose nuclei, when they have served their purpose in forming the fibre, become separated to form the nuclei of the sheath. The recent researches in nerve degeneration and histogenesis all favor this view.

Besides those elements which at once become transformed into the definitive nerve cells, we believe there are intermediate conditions or "reserves," which may subsequently be called into function. Upon this view there is a continuous intercallation of nervous elements going on — a process much more rapid during youth. From the same standpoint it seems probable that there are numerous proliferating stations, where such neurons are continually forming. In the cerebellum and medulla, and even in the cerebrum itself, there are such loci of rapid development. No exception has so far been encountered to the law that the neurons of the central system all spring directly or indirectly from the ventricular surface.

An attentive comparative study of the various groups of vertebrates shows that the development of the various parts of the brain obeys simple and readily discoverable laws, which, when recognized, are as self-evident as the gastræa formation of the embryo. The modifications of the brain-tube, from its primitive uniformity to the wonderful complexity to which it attains in man, form a consecutive series without any complexing hiatus.

Interest attaches particularly to the cerebrum, by reason of its preëminent position, as the latest structural modification, and its close relation to the phenomena of consciousness. Since Rückhard showed that in the fish the roof or cortex of the cerebrum is wanting, or rather represented by a non-nervous membranous pallium, considerable modifications in our conceptions of the sphere of consciousness have been rendered necessary. Remarkable experiments show that the whole cerebrum may be removed without making any noticeable difference in the habits and activities of the fish (save in the case of those functions associated with smell). The writer has studied the axial lobe of the cerebrum of fishes and described numerous distinct cell-clusters and tracts which had hitherto been overlooked. He suggested that the undifferentiated prototypes of the cell-masses, which in higher vertebrates occupy the cortex, are, in this case, retained in the axial lobes. It was shown that the centres for the sense of smell are highly and specially developed, and are connected by strong and distinct tracts with the olfactory organs. It was even ventured to locate a homologue of the hippocampus upon the basis of the tracts. This procedure was evidently regarded by some as rash, but has been amply justified by subsequent developments.

In reptiles we located the olfactory centre or hippocampal lobe in a large part of the cortex, which is closely associated with a curiously modified part of the axial lobe. We suggested that the cortical elements arise as proliferations from the axial lobe, which push out into the thin cortical walls or pallium. In the *Leuckart Festschrift* it was shown that the preplexus in amphibia is analogous in early position and structure to the pallium of fishes.

In a recent number of the *Anatomischer Anzeiger* Dr. Edinger, who is perhaps the ablest living comparative neurologist, works out this form of the solution of this problem in detail with respect to the olfactory and hippocampus. Accepting the suggestion of proliferation from the axial lobe, he shows that the earliest cortex to be formed is that which, in higher vertebrates, is termed ammonshorn or hippocampus. In other words, consciousness first intervenes in the construction of data from the olfactory sense. This suggestion is enforced by the data of comparative morphology. The olfactory is the most primitive of the special sense-organs, and is most closely associated with the cerebrum.

Several years ago we proposed the theory that consciousness must have appeared very late in the evolution of psychical functions; the higher expressions of this faculty, such as reflection, being among the latest endowments of the race. It was shown that such a view would give us less concern in the bloodthirsty procession of ferocious animals which have reddened every page of geologic history. When the greatest diameter of the nerve-tube was in the pelvic region, it was unnecessary to predicate consciousness as a pre-requisite to the simple avocations of the animal.

We believe that, under the law of natural selection, consciousness could only appear when the arena was opened for its serviceable exercise.

Remarkable confirmation of the comparatively accessory status of consciousness has been obtained from two such different sources as the study of hypnotism and experimental psychology. In a most interesting paper printed in the June number of the *Journal of Comparative Neurology* Dr. Edinger describes the results of an examination of the brain of a dog, from which Professor Goltz had removed the *entire cerebrum on both sides.*

The dog lived eighteen months, but, contrary to the predictions of the sceptical, the cerebrum proved to be all but entirely removed. The special senses were not destroyed except smell. Locomotion was not impaired, and general sensation was intact.

Although the animal was completely imbecile, it retained the nervous mechanism for nearly all bodily functions. While these results seem, at first, contradictory to those derived from extirpation and electrical stimulation, yet, as Edinger shows, they merely indicate that the organs and processes of consciousness are merely superposed upon the substructure of the instinctive processes and axial centres.

In man, who has acquired greater dependence upon reflection and other higher functions, the primitive independence of the lower centres is retained for a relatively short time during childhood. The above illustration may at least serve to show how mutually dependent all these sciences are and that we seem to be gradually approximating toward a connected theory of nervous action and evolution.

SOME CURRENT NOTES UPON METEORITES.

BY S. C. H. BAILEY, OSCAWANA-ON-HUDSON, N.Y.

IT may well be hoped that the revived attention which has recently been shown in the study of that interesting class of bodies known as meteorites, will result in giving us a more practical, if not a more certain, basis for their consideration. If in the onset we meet with conflicting theories and much uncertain data, we are only upon the same ground where most scientific inquiry begins. If we cannot tell whence an aerolite comes, we usually do know the fact and date of its fall, its chemical and lithological composition, specific weight and peculiarities of structure, the phenomenon attending its flight, and often the precise radiant point from whence it came. We hold the object in our hands, and can study its physical properties, and its cosmic as well as its telluric history. All these particulars have been observed, compared, studied, and in part determined by thoroughly competent scientific men, and yet, to-day, there is no accepted scientific name to indicate their special line of research, none for this department of science itself. These primary needs are yet to be filled. Heretofore two distinguished writers and students in this field of inquiry have each proposed a specific name for the science, and, while neither of the terms seems to be objectionable, neither of them seems to have been generally adopted or used. In 1847 Shepard proposed the term "Astropetrology," and in 1863 Story-Maskelym suggested that of "Aerolitics" to distinguish it as a department of science. Both from the priority of suggestion, and as a fitting tribute to the zeal and valuable labors of Professor Shepard in that behalf, will it not be proper and convenient to adopt his proposed name, astropetrology, which, in accordance with common usage, by a simple change of its final syllable "gy" into "gist," will also designate a person devoted to its study? How comes it that a subject presenting most interesting and possibly serviceable problems in astronomy and physics should thus far be deficient in the very rudiments of a distinctive science — even a name? Certainly not from lack of patient labor and intelligent investigation by thoroughly competent men. Smith and Genth upon its chemical side, and Newton, Eastman, Langley, Kirkwood, and others upon its astronomical, have, in our country, done much to determine the data upon which present theories rest; while abroad, among a host of others, Haidenger, Meunier, Tschermak, and Brazina have worked at the very bases of efficient progress in scientific research, investigation, and the classification of the objects themselves. In this last-mentioned feature, however, lies a discouraging fact. These several systems do not agree, or rather, while serviceable and consistent in themselves, they, to some extent, seem to antagonize each other in the hands of the collector or possessor of meteoric examples. In a given example not properly labelled, or when labels have been confused, and perhaps changed places, its possessor will probably find it quite accurately described upon reference to one of these systems, but from caution, upon reference to another system, he will find described peculiarities not seen in, and possibly antagonistic to, the same fall as that which he has in hand. How is he to identify it? Specific weight may help the determination, but, standing alone, it cannot be conclusive. Chemical analysis is impracticable and not wholly conclusive. Now, if the absolute necessity of accuracy in the identification of the fall is considered for a moment, there will also result a partial appreciation of its vast importance in all its collateral as well as direct relations. For instance, the supposed example almost exactly resembles another described fall, but one occurred in India, A.D. 1822, while the other fell in Iowa in 1847, both were well observed as to radiant point, time, and course of flight, but each was the reverse of the other in all these important particulars; in short, they only resemble each other in physical characters, and a confusion of their identity may destroy all their value as data in their theoretical and astronomical relations. Identity of radiant point, time, and course of flight and a possible periodicity in observed falls will interest the astronomer even more than identity of chemical composition or physical characters, though each is a factor in his theory, and each must be, if possible, an observed fact. If a single fact may uphold or upset a theory, it should certainly be an observed fact. The purpose of these observations is to inquire what may be done to base investigations of these wonderful phenomena, the most suggestive and impressive of nature's visible displays, and the objects which they bring to us from the regions of space, upon ground more worthy of consideration and research, than as merely objects of a collecting fad, or a money-making zeal in collecting and selling examples. May we not begin by some practical methods for determining and perpetuating the identity of each example by describing and authenticating with the greatest exactness every fall and every fragment? For accomplishing this purpose the number of examples is already large, but it will be constantly augmented by new accessions which may present new physical features and new, perhaps more definite, data, the value of which will be carefully determined by the astronomer and chemist, and probably with greater fidelity and accuracy than by the observer who witnessed its fall, or the author who has the example in his hand from which to write its description. In a subsequent paper I shall venture to suggest some simple expedients for avoiding some defects and errors which have become a great and increasing obstacle to progress in this most interesting department of science.

BIOLOGY IN OUR COLLEGES: A PLEA FOR A BROADER AND MORE LIBERAL BIOLOGY.

BY C. HART MERRIAM, WASHINGTON, D.C.

WHEN it became fashionable to study physiology, histology, and embryology, the study of systematic natural history was not only neglected, but disappeared from the college curriculum, and the race of naturalists became nearly extinct. Natural history, as formerly understood, comprised geology, zoölogy, and botany, and persons versed in these sciences were known as naturalists. Geology gradually came to occupy an independent field, and is now everywhere taught separately; hence, for present purposes, it may be dismissed, with the reminder that the naturalist who knows nothing of geology is poorly equipped for his work. A knowledge of the two remaining branches — the biological branches — was looked upon as sufficient to constitute a naturalist. But the kind of knowledge taught underwent a change; the term "naturalist" fell into disuse to be replaced by "biologist," and some would have us believe that even the meaning of the word biology is no longer what it was. Systematic zoölogy has gone, or, if still tolerated in a few colleges, is restricted to a very subordinate position. Systematic botany is more fortunate, still holding an honored place in many universities, though evidently on the wane.

Is it not time to stop and inquire into the nature of the differences between the naturalist and the modern school of instructors who call themselves "biologists;" into the causes that have brought about so radical a change, and into the relative merits, as branches of university training, of systematic biology compared with the things now commonly taught as biology?

Is it not as desirable to know something of the life-zones and areas of our own country with their principal animals and plants and controlling climatic conditions, as to be trained in the minute structure of the cellular tissue of a frog? And is not a knowledge of the primary life regions of the earth, with their distinctive types, as important as a knowledge of the embryology of the crayfish?

Naturalists delight in contemplating the aspects of nature, and derive enjoyment from studying the forms, habits, and relationships of animals and plants; while most of the self-styled "biologists" of the present day direct their studies to the minute structure (histology) and development (embryology) of a few types — generally lowly forms that live in the sea — and are blind to the principal facts and harmonies of nature. Imbued with the spirit of evolution, they picture in their mind's eye the steps by which the different groups attained their present state, and do not hesitate to publish their speculations — for "they know not what they say." Their lives are passed in peering through the tube of a compound microscope and in preparing chemical mixtures for coloring and hardening tissues; while those possessing mechanical ingenuity derive much satisfaction in devising machines for slicing these tissues to infinitesimal thinness. An ordinary zoölogist or botanist is not constituted in such a way as to appreciate the eagerness and joy with which one of these section-cutters seizes a fraction of a millimetre of the ductless gland of a chick or the mesoblast of an embryonic siphonophore; nor is it vouchsafed him to really understand, though he may admire, the earnestness, devotion, unparalleled patience, and intense satisfaction with which the said investigator spends years of his life in hardening, staining, slicing, drawing, and monographing this same bit of tissue.

Such "biologists" have been well characterized by Wallace as "the modern school of laboratory naturalists" — a class "to whom the peculiarities and distinctions of species, as such, their distribution and their affinities, have little interest as compared with the problems of histology and embryology, of physiology and morphology. Their work in these departments is of the greatest interest and of the highest importance, but it is not the kind of work which, by itself, enables one to form a sound judgment on the questions involved in the action of the law of natural selection. These rest mainly on the external and vital relations of species to species in a state of nature — on what has been well termed by Semper the 'physiology of organisms' rather than on the anatomy or physiology of organs" ("Darwinism," 1890, Preface, p. vi.).

It is hardly an exaggeration to say that in our schools and colleges the generally accepted meaning of the word biology has come to be restricted to physiology, histology, and embryology, and that the courses of instruction now given in biology cover little additional ground, save that they are usually supplemented by lectures on the morphology and supposed relationships of the higher groups. It is against this modern custom of magnifying and glorifying these branches or departments of biologic knowledge until they are made to appear not only the most important part of biology, but even the whole of biology, that I beg to enter a most earnest protest. Far be it from me to deprecate any investigation that tends, in howsoever slight a degree, to increase our knowledge of any animal or plant. Such investigations fulfil an important and necessary part in our understanding of the phenomena of life, but they should not be allowed to obscure the objects they were intended to explain.

Without a knowledge of anatomy and embryology it would be impossible to properly arrange or classify the various groups, or to understand the inter-relations of the many and diverse elements that go to make up the beautiful and harmonious whole that naturalists and other lovers of nature so much admire. Similarly, the architect would be powerless to construct the magnificent edifices that everywhere mark the progress of civilization unless he understood the nature and properties of the various parts that go to make up the finished structure; yet what would be thought of a school of architecture that limited its teachings to the strength of materials or the composition of bricks, mortar, nails, and other minor factors necessary in construction? But would not such a school be strictly comparable with the modern school of histologists and physiologists who, under the head of biology, teach little besides the minute structure and functions of tissues, ignoring the characters that constitute and distinguish species, that show the adaptation of species to environment, that show the processes and steps by which species are formed, and the causes that govern their differentiation and distribution; in brief, ignoring most that is beautiful and interesting in nature, including the great truths that enable us to understand the operations and laws of nature, for the sake of dwelling eternally on details that ought to form merely a part of the foundation for a study of nature.

The evolution of these one-sided biologists is not hard to trace. Early naturalists, such as Linnæus and Buffon, knew little of the internal structure of animals and plants; their classifications, therefore, were based chiefly on external characters, and were correspondingly crude. Cuvier was first to demonstrate the importance of anatomical knowledge in arranging animals according to their natural affinities, but his studies were confined to what is now called "gross anatomy," or the structure of such parts and organs as are visible to the naked eye.

The great improvement made in the microscope in the years 1830–1832 — at which time the spherical errors that had previously rendered its use unsatisfactory were overcome by the proper adjustment of achromatic lenses — paved the way for the discoveries in embryology and the minute structure of the tissues that made illustrious the names of von Baer, Schleiden, Schwann, and a host of others. The revelations that followed created a profound sensation among the naturalists of the time, and, as the microscope became more and more perfect, new paths were opened to the investigator, and the fascination attending its use grew. The increased demand for good instruments stimulated the invention and perfection of high-power lenses and of a multitude of accessories, the use of which, in turn, led to improved methods of treating tissues and to the discovery of bacteria and the various pathogenic micrococci of fermentation and disease. A knowledge of microscopic technic became, and justly, too, a necessary qualification in the way of preliminary training for those seeking to become biologists.

The transition from the old school to the new was but a step, and had been led up to by the course of events. The older systematic naturalists rapidly died off while still appalled by the wonderful discoveries of the microscopists; the professorships in the colleges and universities (which, at the same time, were rapidly increasing in number) were filled by young men ardent in the use of the microscope, and each anxious to excel his colleague in skill and dexterity of manipulation and in the discovery of some new form of cell or new property of protoplasm.

But one result could follow the continuance of this state of affairs, namely, the obliteration of the naturalist from the face of the earth — a result that at the present moment is well-nigh attained, for, if there is an "all-round naturalist" alive to-day, his existence is due to accident or poverty. Poverty has kept a few lovers of nature away from college, and by this seeming misfortune they have escaped the fate that would have overtaken them had they possessed the means of placing themselves under our modern teachers of biology. These teachers have deflected into other channels many a born naturalist and are responsible for the perversion of the science of biology. While deluding themselves with an exaggerated notion of the supreme importance of their methods, they have advanced no further than the architect who rests content with his analysis of brick, mortar, and nails without aspiring to erect the edifice for which these materials are necessary.

In trying to reconstruct a general naturalist at the present day, I would rather have the farmer's boy who knows the plants and animals of his own home than the highest graduate in biology of our leading university. The enthusiastic boy, whose love for nature prompts him to collect the birds, insects, or plants within reach, can be easily induced to take up the study of other groups, and thus become a local "faunal naturalist." After acquainting himself with the home fauna and flora, he may develop into a general naturalist if removed to another locality. The chief disadvantage in manufacturing naturalists in this way is that they lack the education possessed by college-bred men — a want sorely felt in after years.

To be well equipped for his work, a naturalist or biologist needs a college education; he needs laboratory instruction in modern methods of biologic research; he needs practical training in systematic and faunal zoölogy and botany with special reference to the extent of individual variation in species, the modification of species by food and environment, and the nature and constancy of specific characters in different groups; he should have the benefit of lectures on the principles of biology and on the geographic distribution of life; and be should be taught to work out for himself the relationships and probable genetic affinities of the members of a few well-selected genera in different groups.

The teacher and professional student who aspire to tread the higher paths of biology are unworthy of their chosen field unless they possess a broad and comprehensive grasp of the phenomena of living things — a grasp that comes only after years of patient study and personal familiarity with animals or plants. Perhaps the true explanation of much of the prevalent kind of biology may be found in the circumstance that a considerable proportion of our teachers are the output of a few institutions in which their studies have been guided by section-cutters and physiologists. They are well trained in methods of research in limited fields, which training may be acquired in the brief space of three or four years, but are ill fitted to impart a knowledge of the leading facts and principles of biology, or of the kind of biology likely to prove most useful to the average student.

Some of our universities encourage and support the most abstruse and recondite investigations in the field of pure science, without regard to an economic outcome — for which they deserve the greatest credit — but such studies are rarely suited to the requirements of the ordinary college curriculum. On the contrary, the tendency of the times in matters of instruction is to render undergraduate courses more practical, so that the knowledge acquired may be useful in after life. With this end in view, it may not be amiss to inquire how the kind of biology now commonly taught compares with systematic and faunal zoölogy and botany? Will anyone attempt to maintain that 10 per cent of the present teaching is of any value in after life, except to the specialist, or that more than one per cent of the students taught biology become specialists? It seems clear, from the standpoint of availability in the ordinary walks of life, that the prevalent kind of biology teaching is a failure. Systematic and faunal zoölogy and botany, on the other hand, while fully equal to the branches now taught as a means of mental discipline, have in addition an economic value, and are sources of permanent interest and happiness to the majority of mankind. Huxley, in one of his early public lectures, said: "To a person uninstructed in natural history, his country or sea-side stroll is a walk through a gallery filled with wonderful works of art, nine-tenths of which have their faces turned to the wall. Teach him something of natural history, and you place in his hands a catalogue of those which are worth turning round. Surely our innocent pleasures are not so abundant in this life that we can afford to despise this or any other source of them" ("Lay Sermons, Addresses, and Reviews," London, 1870, pp. 91–92). Not only are excursions into the country or to the sea thus made more enjoyable, and the tedious delays at the railway station converted into sources of entertainment and profit, but even much of the drudgery and routine of everyday life may be turned to good account. Instead of the mental stagnation that naturally follows the automatic performance of a monotonous daily task, there is an incentive to observation that stimulates the intellect and results in the agreeable acquisition of knowledge. In short, acquaintance with our common animals and plants appeals to an inherent desire to know more of nature in the aspects commonly presented to our senses; it increases the joys and lightens the burdens of life; it promotes the healthy expansion of the intellect and the development of the nobler impulses and sentiments, making better men and better women.

Another argument in favor of a knowledge of systematic and faunal zoölogy and botany is that it largely increases the amateur element in science and brings the great mass of the intelligent public nearer the technical specialist, thus creating that interest in and appreciation of scientific research that leads to liberal endowment. The kind of biology now taught in most of our educational institutions has the opposite effect, tending to deepen the chasm between the people and the specialist. So long as an unfathomable abyss separates science from the intelligent citizen, just so long may the specialist expect to lack the earnest support on which his success so much depends.

The study of systematic and faunal zoölogy and botany may seem superfluous to the physiologist, histologist and technical specialist who are content to contribute their mite to the general fund — a not unworthy ambition — but to those who aspire to solve the problems and master the principals of biology a broader view is necessary — a view that can come only to those who possess an intimate personal acquaintance with the interrelations of living species and the nature and extent of their modifications — for how is it possible to form a clear conception of the operations of natural selection, of the effects of environment on species, of the transmission of acquired characters, of special adaptations, fortuitous variations and so on, without first knowing something of the species themselves? It is true that a few section-cutting physiologists, possessed of speculative minds, have ventured to enter the domain of philosophic biology, but it would be ungracious to contrast their productions with those of such naturalists as Humboldt, Darwin, Huxley, Wallace, Haeckel, Agassiz, Hyatt, Cope, Dall, Allen or Ward.

In order to avoid the possibility of being misunderstood, I wish to reiterate what has been already said in substance, namely, that while the present paper is intended as a plea for systematic biology, no complaint is made against the proportionate teaching of physiology, histology, and embryology, but only against the exclusive or disproportionate teaching of these branches, as if they comprised the whole of biology. And it may be added for the benefit of those who insist that the term biology should be restricted to the phenomena of life rather than the phenomena of living things, that, while unqualifiedly opposed to this narrow view, my present purpose is not to discuss the meaning of words, but to show the necessity of remodelling the current one-sided courses of instruction by the addition of systematic and faunal zoölogy and botany, with a view to the development of a broad and comprehensive school of biology, worthy of the age in which we live.

In my judgment, university training in biology should comprise:

1. *Elementary instruction in general biology*, including cell structure and the structure of the less complex tissues of animals and plants. This involves laboratory work with the microscope and insures the necessary knowledge of microscopic technic.

2. *Lectures on morphology, taxonomy, and the relationships* of the major groups of animals and plants, both living and fossil, supplemented by laboratory work which should be restricted to the study of types and should keep pace with the lectures, if possible.

3. *Systematic work in widely separated groups.* This work must be done in the museum or laboratory, and may be supplemented by lectures. It should include the higher vertebrates as well as invertebrates and plants. In the case of advanced students, original work should be encouraged, particularly revisions of genera.

4. *Faunal work*, consisting of the study of the life of limited areas. Care should be taken to avoid too comprehensive an undertaking; and the groups chosen for study should be selected, as a rule, with reference to the literature or specimens available for comparison. The necessary field-work, if impracticable during the college year, may be done in vacation. Whenever possible, field excursions should be made at frequent intervals during the college year, under competent supervision.

5. *Lectures on the distribution of life.* In time, paleontologic distribution; in space, geographic distribution. These lectures should be illustrated by maps, diagrams, and specimens. Access to zoölogical and botanical gardens and museums is of the utmost importance.

6. *Lectures on the principles and philosophy of biology*, comprising evolution, heredity, migrations, special adaptations, and so on.

Botany and zoölogy should be taught separately under the second and third headings, and together under the first, fifth and sixth. Under the fourth heading they might be taught either separately or together, as most convenient.

Paleontology should form an inseparable part of biology and should not be taught under geology except in its stratigraphic relations. Fossil types should be studied in connection with their ancestors and their nearest living relatives.

The pendulum has swung too far in the direction of exclusive microscopic and physiologic work. When it swings back (and I believe the time is not far distant) the equilibrium will be restored — the perverted meaning of the term "biology" will be forgotten, and the present one-sided study of animals and plants will give place to a rational biology and to the development of a school of naturalists far in advance of those who have passed away.

NOTES ON PENNSYLVANIA GERMAN FOLK-MEDICINE.

BY W. J. HOFFMAN, M.D., WASHINGTON, D.C.

WHILE collecting material relating to the folk-lore of the Pennsylvania Germans I obtained some curious beliefs pertaining to the rattlesnake, and the alleged remedies employed for curing those bitten by this reptile. Many newspaper reports are annually circulated in various portions of the Atlantic Coast States to the effect that the reporter had discovered a veritable "mountain doctor," well versed in the secret properties of plants, and that this personage was widely celebrated for his wonderful skill in curing rattlesnake bites, but that the remedy was preserved with the utmost care as a great and valued secret; or, perhaps, that the reporter of the article had received a sample, but through some unavoidable misfortune he had lost it, etc.

Having consulted with some of these so-called "mountain doctors" to obtain and exchange matters of interest — during the past twenty years — it has been found that nearly all of them employ numerous species of plants for the ills that come under their observation, but that only a few are really acknowledged as possessing a semblance of skill, and still less who are familiar with so-called snake-bite remedies.

The plant employed by one of these "mountain pow-wows," and the only one claimed to possess any virtue, is *Sanicula marylandica*, or sanicle, termed by the natives "master-root," because it "masters the rattlesnake venom." The fresh plant and roots are pounded and soaked in boiling milk, when the mixture is applied to the wound as a poultice. A decoction of the same plant is also taken internally to induce diaphoresis. The decoction is said to be more efficacious if made with milk instead of water. I believe this to be the first instance of bringing this plant to public attention, at least as employed by these superstitious herbalists, and for the purpose stated; but as so much stress is placed upon the good results, even by people of recognized intelligence and education, it might not be amiss to have made a series of chemical and therapeutic experiments to test the efficacy of the remedy.

Another remedy employed by the superstitious of the mountain regions of middle and eastern Pennsylvania is to cut a live chicken in two, and to place the warm, raw surface of one part upon the part bitten by the snake.

Rattlesnakes are of value to the mountain doctors for several reasons. The oil, obtained by draining the reptile after skinning is used to cure deafness. The rattle, suspended from a string, and worn by a baby, will have the power of preventing the wearer from having convulsions during dentition. The tongue of the snake, when worn in the glove, will have the power of compelling any girl, who grasps the gloved hand, to love the one so greeted, even should she ordinarily be indifferent to his attentions.

Finally, to secure rattlesnakes, the "doctor" grasps a silk handkerchief at one corner, and allowing the other end to hang toward the serpent, teases her until she strikes it with her fangs, when he immediately raises the handkerchief from the ground, thus depriving the snake of any opportunity of disengaging herself therefrom, as the slightly recurved fangs are hooked in the material. The "doctor" then either kills the serpent by first grasping her neck with the disengaged hand, so as to prevent her biting him, when he cuts off her head. Should he desire, however, to keep the snake as a curiosity or for sale, he will extract the fangs with a small pair of forceps.

NOTES AND NEWS.

PROFESSOR RICHARD A. PROCTOR, the well-known astronomer and writer, died in 1889, of yellow fever, in New York City. His children were in Florida at the time, and could not be present at the funeral. No suggestion of a resting-place being forthcoming, the astronomer's remains were buried in the undertaker's private lot in Greenwood. The body, it was understood, was to remain there until other arrangements could be made. The lot was in an out-of-the-way part of the cemetery, and the grave was neglected, there being not even a stone to mark the place. The children of the astronomer are all making their own living, and while their wish was to bury their father better, the means were not at hand. Recently, through the efforts of Mr. Edward W. Bok, attention has been called to the matter, and Mr. George W. Childs of Philadelphia, has, with his usual generosity, purchased a lot in Greenwood, near the Flatbush entrance, to which the astronomer's remains will be removed, and in October it is hoped that a suitable sarcophagus of granite will be dedicated with due ceremony.

— The U. S. National Museum has recently come into possession of a very remarkable collection of petrified trunks of an extinct species of tree belonging to a family of plants that is now very rare, but which once formed a prominent feature of the landscape of nearly all countries. These plants are intermediate in appearance between tree-ferns and palms, and have as their best known living representative the common sago-palm, *Cycas revoluta*, of our greenhouses. The fossil trunks above mentioned are from one to three feet in height and from six inches to two feet in diameter. They are in a very perfect state of preservation, turned to solid stone of a brown color. The largest one weighs 900 pounds, and is the largest object of the kind ever reported from any part of the world. They were found lying on the surface of the ground in the vicinity of Hot Springs, South Dakota, and were all sent to Washington by mail under the frank of the Interior Department. The geological formation in which they occurred is not known with certainty, but this class of plants reached its greatest perfection in what is known as Secondary, or Mesozoic time. It is therefore altogether probable that these trunks grew at that remote age and have lain strewn over the plains for millions of years waiting for science to gather them in and make them help tell the story of the earth. They have been placed in the Department of Fossil Plants, in charge of Prof. Lester F. Ward, who recently superintended the taking of fifteen views of them by the accomplished photographer of the National Museum, Mr. T. W. Smillie. This is one of the most important accessions the museum has received of late, and when the collection is elaborated and the results published it will make a valuable contribution to science.

— At Denison University, Granville, Ohio, a new scientific building, known as Barney Hall, is approaching completion. The building, which is one of the most substantial scientific buildings in the West, will cost when finished about $65,000, and will include chemical and physical laboratories, as well as a museum and laboratories of biology. Special attention is to be devoted to neurology and comparative neurology. An extended graduate course in biology, and a number of fellowships have been provided with corresponding increase in faculty.

— "The Story of My Life," by Dr. Georg Ebers, is the title of a delightful autobiography, full of fascinating reminiscences, which will be published immediately by D. Appleton & Co. This autobiography tells of Dr. Ebers's student life in Germany, his association with movements like that for the establishment of kindergarten training, his acquaintance with distinguished men like Froebel and the brothers Grimm, his glimpses of revolutionary movements, his interest in Egyptology and the history of ancient Greece and Rome, and the beginnings of his literary career.

— Without making invidious comparisons, it is safe to say that the exhibit which Messrs. Houghton, Mifflin & Co. have arranged in the gallery in the northwestern corner of the Department of Liberal Arts in the Manufacturers' Building at Chicago is in all respects worthy of somewhat careful examination. The idea evidently is to represent such a library as might be found in the house of a man of cultivation in any part of the United States.

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Attention is called to the "Wants" column. It is invaluable to those who use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The "Exchange" column is likewise open.

CONGRESS OF CHEMISTS AT CHICAGO.

THE committees in charge of the congress have selected Monday, Aug. 21, as the date of the opening of the Congress of Chemists to be held in connection with the Columbian Exposition, in Chicago. The chairman of the committee appointed for coöperation in this congress by the American Association for the Advancement of Science, Chemical Section, is Professor Ira Remsen, Johns Hopkins University, Baltimore, Md. The chairman of the committee appointed by the American Chemical Society is Dr. Wm. McMurtrie, 106 Wall Street, New York, N.Y. The chairman of the committee of the World's Congress Auxiliary, on Congress of Chemists, is Professor John H. Long, 2421 Dearborn Street, Chicago, Ill. The various committees have organized by selecting Dr. H. W. Wiley, chief chemist of the Department of Agriculture, Washington, D.C., as chairman, and Professor R. B. Warder, Howard University, Washington, D.C., as secretary.

The work of the congress has been divided into ten sections, and a temporary chairman has been selected for each section, as follows: Agricultural Chemistry, H. W. Wiley, Department of Agriculture, Washington, D.C.; Analytical Chemistry, A. B. Prescott, Michigan University, Ann Arbor, Mich.; Didactic Chemistry, W. E. Stone, Lafayette, Ind.; Historical Chemistry and Bibliography, H. C. Bolton, University Club, New York.; Inorganic Chemistry, F. W. Clarke, Geological Survey, Washington, D.C.; Organic Chemistry, I. Remsen, Johns Hopkins University, Baltimore, Md.; Physical Chemistry, R. B. Warder, Washington, D.C.; Physiological Chemistry, V. C. Vaughan, Michigan University, Ann Arbor, Mich.; Sanitary Chemistry, H. Leffmann, 715 Walnut Street, Philadelphia, Pa.; Technical Chemistry, Wm. McMurtrie, 106 Wall Street, New York, N.Y.

General and special invitations have already been issued to foreign chemists, and many replies have been received, indicating a large attendance of chemists from abroad at the congress. The following distinguished foreign chemists have already promised to present papers to the congress, and the list will, without doubt, be increased many fold before the date of the opening: Professor L. G. M. Ernest Milliau, Marseilles, On Standard Methods of Oil Analysis; Mr. Farnham Maxwell Lyte, London, On the Production of Chlorine; Mr. H. Droop Richmond, London, On the Accuracy of the Methods of Analyses of Dairy Products; Mr. Pierre Manhes, Lyon, subject to be announced later; Professor B. Tollens, Goettingen, Researches on the Synthesis of Polyatomic Alcohols; Professor Ferd. Tiemann, Berlin, subject to be announced later; Mr. H. Pellet, Brussels, On the Methods of Determining the Percentage of Sugar in Beets; Mr. H. R. Proctor, Leeds, On the Examination of Tanning Materials; Mr. O. Kemna, Antwerp, On the Purification of Water; Mr. Otto Hehner, London subject to be announced; Professor C. A. Bischoff, Riga, subject to be announced; Professor G. Lunge, Zürich, On the Method of Teaching Technological Chemistry at Universities and Polytechnic Schools; Professor Ludwig Mond, Rome, subject to be announced; and Professor W. N. Hartley, Dublin, subject to be announced.

American chemists are invited to take an active interest in the congress and to be present, or, if that is not possible, to send papers on some of the subjects indicated in the classification above mentioned.

Chemists specially interested in each of the subjects for discussion are invited to correspond with the chairmen of those sections in regard to the character of the work and of the papers expected. All chemists who expect to read papers at the congress are earnestly requested to send the titles thereof to the chairman of the General Committee, Dr. H. W. Wiley, Department of Agriculture, Washington, D.C., on or before the first day of August. It will be difficult to arrange for a position on the programme for the titles of any papers which may be received after that date. The time required for each paper should also be noted, so that daily programmes can be provided for in advance. In all cases the place of honor on the programme will be given to foreign contributors. Papers or addresses can be presented in English, French, or German, as the author may select, but where convenient the English language will be preferred.

The committee desires to ask those chemists who propose to attend the World's Congress to make an excursion during the week previous to the meeting to Madison, Wisconsin, for the purpose of attending the meetings of the Chemical Section of the American Association for the Advancement of Science. This will not only be a delightful excursion, as Madison is distant only about four hours from Chicago, but will also enable the participants in the congress to make the acquaintance of the scientific men of the United States and other countries engaged not only in chemical, but also in other branches of science.

Other attractions in Chicago will be meetings of different chemical societies. Among these may be mentioned the American Chemical Society, the annual meeting of which will begin Aug. 21, and the Association of Official Agricultural Chemists, which will hold its annual meeting in Chicago, beginning Thursday, Aug. 24. The sessions of these societies will be so ordered as not to conflict with the business of the congress. The American Pharmaceutical Association, which has a strong chemical section, will also meet in Chicago at or near this time. It is hoped that the Institute of Mining Engineers may also hold its meeting about this time, although no definite announcement can be made in regard to this matter. It is thus seen that this occasion will bring together the active workers in all branches of chemical science in the United States, and enable American chemists to make the acquaintance of distinguished co-laborers from abroad, and the visiting chemists to meet the largest possible number of their fellow-laborers here.

Every possible arrangement will be made for the convenience and comfort of visitors. Intending participants in the congress should address Professor John H. Long, 2421 Dearborn Street, Chicago, Ill., for information in regard to quarters and other accommodations. On arrival in Chicago visitors should report at once to the congress headquarters, Art Institute Building, Lake Front and Adams Streets, where full information will be given them in regard to matters connected with their personal comfort. Wherever possible, intending visitors should write a few days before their arrival to the committees above mentioned, in order that special provision may be made for their comfort when they reach Chicago.

In regard to the climate of Chicago in August, much can be said in praise. While warm days may sometimes be expected, the situation of the city on the edge of a vast, open prairie, extending for nearly a thousand miles north and west without a break, secures even in the hottest day refreshing breezes which cool the atmosphere and mitigate the heat of summer. The lake breezes also do much to render the climate moderate. No one need be deterred from attending the congress on account of fear of severe heat.

It is especially urged that all chemists who intend visiting the World's Fair take this occasion to do so, by which they can combine the pleasure of visiting the Exposition with the benefit derived from attendance at the congress. To American chemists an especial appeal is made to be present for the purpose of welcoming our foreign visitors and showing them the progress of chemical science in the United States. HARVEY W. WILEY.

THE EFFECT OF FOOD UPON THE COMPOSITION OF BUTTER.

BY FRED W. MORSE, DURHAM, NEW HAMPSHIRE.

Practical dairymen, who produce a high grade of butter, lay great stress upon the quality of food with which their cows are fed. Chemists, who have had much to do in the examination of butter for adulterants, have observed that the samples from one region have steadily varied in their composition from those of another region, where different practice prevailed in feeding. These facts have led to many experiments, both in Europe and America, to find out the specific action of different foods upon the composition of butter.

This product of the dairy is composed of fat, water, salt and curd; and of these, the cow is responsible for only the fat and the curd. The former constitutes about eighty-five per cent and the latter barely one per cent of the butter, therefore chemical examinations for variations due to food have been confined wholly to the fat. Butter-fat differs from the fat deposited in the body of the animal by having from five to seven per cent of volatile fatty acids, and only eighty-seven to ninety per cent of insoluble, fatty acids, while tallow has ninety-five per cent of the latter and less than one-half of one per cent of the former. The volatile acids give butter fat its characteristic flavor, and also cause it to be softer than tallow. Butter fat also differs from tallow in having less oleic acid among its insoluble acids. These two characteristics of butter-fat have been studied more than any of its other properties, because of their relations to adulterations, and the studies of food effects have so far been confined to the same lines.

In the course of investigations, it has been found that in general, the widest variations in volatile acids and oleic acid are due to the progress of lactation, the latter increases and the former decrease as the period advances. Individual cows also vary widely from one another in the composition of their butter fat, but with regard to the breeds, no definite conclusions can be made.

The effect of food is greater upon the oleic acid than upon the volatile acids and, in nearly all cases, variations in this constituent of the fat have been closely related to variations in the firmness of the butter. This is to be expected, as oleic acid is an oily liquid at summer temperature, and the butter is softer or harder as this acid is present in greater or less amount.

Many of the foods have been tried in such limited amounts that it is unsafe to draw conclusions, therefore only such foods will be mentioned here as have been used in repeated trials. The most notable effect has been produced by cotton-seed and cotton-seed cake. Whenever it has been fed, the volatile acids and oleic acid have been depressed below the average; the butter is deficient in flavor and often too hard to be easily cut with a knife.

In strong contrast to this action of cotton-seed, is the effect of gluten-meal, a by-product from the manufacture of corn-starch. This food is especially effective in raising the oleic acid above the average, and also produces a butter-fat high in volatile acids. The butter from this food is frequently too soft for an ideal product. Corn-meal, however, has always produced a butter-fat low in oleic acid, but has shown no action on the volatile acids. Clover, dry or green, has produced fat high in volatile acids, and with oleic acid slightly above the average. The same is also true of spring pasturage. Early cut straw generally produces a fat with volatile acids and oleic acid below the average.

The action of clover and pasturage in increasing the volatile acids, and that of corn in lowering the oleic acid, explains the practice of the makers of first-class butter, who rely upon these foods to produce a good flavor and firm grain.

ELECTRICAL NOTES.

THE present Electrical Exhibit at the World's Fair contains much more that is of interest from an engineering standpoint than from a purely scientific one. Magnificent as the engineering display is, there is little that is new. Everything is now thoroughly mechanical, one no longer sees the monuments of tortured ingenuity which used to pervade the former exhibitions; in its place are the results of sound and competent engineering skill.

The multipolar machine has evidently come to stay. Three years ago there was not, I believe, a single large multipolar machine made. Almost the only makers of machines above 100 horse-power were the Edison, Brush, and Westinghouse Companies (we are speaking of America, of course; on the Continent of Europe multipolar machines have been the rule), and their machines were all bipolar. Now there is on exhibit hardly a single machine above 50 horse-power which is not multipolar. Splendid examples of these are the Westinghouse, Thomson-Houston, and Edison direct-connected generators.

The general use of the toothed armature is also a new feature. A short time ago the hardy individual who should have proposed designing a large dynamo with toothed armature would have been told that it was impossible to do it, that the consequent increase of self-induction would make it spark so badly that it could not be run, that the only way to make a dynamo whose brushes would not need shifting between full and no load was to have a big air-gap, and all this would have been backed up by alarming mathematical quotations from Ayrton and other writers.

Now we see that the impossible way is the only way, and the designer who neglects the aid of the toothed armature is handicapping himself very much. In passing, one may notice that, if one may judge from several recently-read papers in the English Institute of Electrical Engineers, European designers are not able as yet to design a toothed armature which shall not spark, shall require no shifting of brushes, and shall be highly efficient. Even the machines, which probably furnished the encouragement to American designers to try the toothed armature, i.e., the Brown machines for electro-metallurgy, we learn, recently, had to be sent back to the factory, the armature turned down, and rewound with an exterior winding.

Among the new things in engineering, the large two-phase 1,000 horse-power generators of the Westinghouse Company deserve especial attention. The large amount of work now being done in this line by the various companies is a good augury for the rapid development of the system. If this proves a success, the days of the continuous current will be ended, so far as engineering is concerned. There are three things so far which have hampered the alternating current: (1) Poor all-day efficiency of transformers, (2) noisy arc-lights, and (3) absence of motors. The recent developments in transformer design have resulted in transformers with an all-day efficiency of 94 per cent; the new low-potential arc-lamps give a better light than the continuous-current lamps, and as noiseless; and there only remains the development of the motor system, which now seems to be within sight.

Electric welding is evidently no longer a thing of the future. There are a number of firms making displays, who are using the Thomson process in their business. Several of the wagon-making firms use the welders to make tires and weld axles; wire-making companies use them to join lengths of wire; they are used in making shells for modern machine and quick-firing guns; for joining up lengths of pipe in ammonia ice-machines; and for welding rails together to form a continuous track. This last is a most interesting exhibit, as, if successful in practice, it will lead to a new method of railway construction, for street railways at least. A track has been in operation for some time near the Thomson-Houston factory in Lynn, and the results seem to have been very good. No trouble was experience from expansion or contraction, the friction of the rails in the ground preventing displacement and creeping, and the expansion merely manifesting itself as a stress in the rails, well within the elastic limit.

Among recent practical applications of electricity are the electric chimes and tower-clock system, now on exhibition in the tower in the centre of the Manufactures and Liberal Arts Building. These are the invention of Mr. Attwood, and the chimes have been used for some time in Grace Church, New York. In this system the hammers of the bells are worked from a key-board, like that of a piano, and the largest sized bells can be played as easily and quickly as a piano itself. The mechanism is very simple, the keys making contacts which actuate relays, and these in turn excite solenoids with iron plungers, to which are attached the bell ropes. Instead of the key-board, a small cylinder, like that of a music-box, can be used, which automatically plays the chimes every hour or quarter.

The most valuable part, however, is the electric tower-clock arrangement. In this, instead of the ordinary cumbersome clock-movement requiring frequent rewindings, an ordinary clock is used, which may be placed anywhere, in an office, for instance. Every minute this clock makes a contact, which actuates a little battery motor, and this turns the hands of the tower-clock one minute ahead. As the impulse is given at the middle of the minute, the tower hands are never more than half a minute out of time. The actuating clock may be synchronized from Washington if desired. This system seems to give a very good tower clock for a fraction of the present price. One advantage is the fact that no winding is required, six or seven Leclanche cells furnishing enough current to run the clock for several years.

Another exhibit which will be gladly hailed by those who have had to do much telephoning will be the automatic telephone exchange in the gallery of the electrical building. In this system, the telephones are the same as usual, but in front of the wooden box which supports the transmitter are placed a number of keys. If a subscriber wishes to call up number 1324, for instance, he presses key number 1, once; key number 2, three times; key 3, twice; and key 4, four times. He then presses another key, and if the subscriber he wishes to communicate with is talking to some one else, it signals him that fact; if the line is open, it puts him in communication. When he is through, he presses the key again, and it disconnects him.

Several central offices have been put in, and are working satisfactorily, and a number of other cities have decided to replace their present central office by this automatic system.

At present there is one disadvantage which the system has, i.e., the need of four wires instead of two; but, from an examination of the machines, there seem to be several ways by which two wires could do all the work, and doubtless this improvement will soon be made. Even with the increased expense, the better service will more than compensate for the increased cost in wiring, and, of course, the central station expenses will be much reduced.

A very complete exhibit is that of the Bell Telephone Company. This includes an interesting historical exhibit of the various forms of telephone receivers and transmitters invented by Mr. Bell. A central station is shown at work, the methods of connecting up the lines, etc.

One of the new things is the use of paper insulation for telephone cables. Seemingly impracticable as this seemed to be a few years ago, it is now a complete success. Of course, it is evident that its low specific inductive capacity gives it marked advantages over any other kind of insulation, and that by its use speech could be made clearer, and transmitted further, but it would at first sight appear that it would be difficult to keep up its insulating qualities. This, however, has been done, and nearly every switchboard observed was wired with this insulation. The operation of putting it on the wire is shown in the Electrical Building. R. A. F.

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.

Peculiar Nesting of a King-Bird.

A CURIOUS incident, showing a peculiarity of bird-life, came under my notice within the last month (June, 1893). We have been boring an artesian well about five miles south of Beaumont, mound rising out of the great coastal prairie lying between Beaumont and Sabine Pass, in Jefferson County, Texas, and in the course of the operations have built a derrick about seventy-five feet high. After the derrick had been built a few weeks, it was visited by a great number of birds of various kinds, whether with a view of locating or not, I do not know, but one would think a well outfit, with all its noise and wet, a very unfavorable location for bird-life. Among the visitors came a pair of king-birds (*Tyrannus tyrannus*), which, after an apparently careful inspection, became convinced that they had found a satisfactory location for their home. A sheltered point, where two of the cross-beams came together in a corner of the derrick about twelve feet from the ground, was selected and the pair began building a nest. Notwithstanding the noise of the machinery and the continual passing up and down of the man in the derrick (the nest was built in the same corner as the ladder is located on the outside of) the nest was completed and three eggs deposited. Then something occurred that killed the female, and the male, after moping around for a day or two, also disappeared. That, I thought, was the end of that pair's nesting; but apparently not, as in a day or two the same male bird returned, bringing with him another mate. The outlook was again considered, and the pair began building another nest in the same location, resting the new nest on the top of the old one, building, as it were, a second story to it. After the new nest was completed, but before any eggs had been deposited, wondering what could have become of the eggs already laid, I went up the derrick, and, carefully raising the new structure, brought out the old eggs. Replacing the new nest as best I could, the birds continued to occupy it, and the female is now setting upon a full nest of eggs of her own laying, and I am now looking forward with considerable interest to the advent of a young brood to see how they will thrive under the circumstances.

I have asked several of my ornithological friends if such an occurrence has anywhere come under their observations, but have in all cases received a negative answer. WM. KENNEDY.

Austin, Texas, June, 1893.

The Tucumcari Fossils.

IN *Science*, May 26, pp. 282–283, there is an article by Mr. W. F. Cummins of the Texas Geological Survey, entitled "Geology of Tucumcari, New Mexico," in which he says: "Mr. Marcou . . . endeavors to avoid the conclusion (that the beds are Cretaceous) by saying that either the determinations of the fossils found by me were incorrect or that they did not come from that locality, and suggests that the labels on my packages were loosely put on and became mixed with collections made elsewhere; and on this flimsy subterfuge (to give it no harder name) still insists on the correctness of his reference to the Jurassic."

Mr. Cummins tells at length of the good care he took not to have any confusion of labels. So my suggestion cannot stand. I accept fully the explanation.

Now there remain two points, which are the most important: First, the correctness of the determination of the fossils; second, the stratigraphic position of the Jurassic strata of the Tucumcari between the Trias and the lower beds of the Neocomian, at Comet Creek, an affluent of Washita River, and at the great band of the Canadian River.

1. Mr. Cummins says: "myself and my assistants discussed the fossils in the field as we picked them up, and our note-books show that we then determined them as they are now designated." . . . "I made up small suits and sent them to various parties for determination, . . . and there was unanimous agreement as to all the species I have *published*." It is important to add an explanation as regards the species *published*. Only one species has been published by Mr. Cummins, a leaf of a fossil plant; all the invertebrate fossils are only quoted, without descriptions or figures. Here is the list given by Mr. Cummins:—

"*Gryphoa dilatata*, var. Tucumcarii Marcou; *Ostrea marshii*, as determined by Marcou, but in reality *Ostrea subovata*, Shumard; *Gryphoa pitcheri*, Mortón; *Exogyra texana*, Romer; *Ostrea quadriplicata*, Shumard; *Trigonia emoryi*, Con.; *Cardium hillanium*, Sow.; *Cytheria leonensis*, Con.; and a single leaf of a dycotyledonous plant, which I described and figured under the name

Sterculia drakei. It will be apparent to everyone acquainted with the fossils of the Cretaceous that those enumerated belong only to Cretaceous strata."

It would have been well if Mr. Cummins had given the names of the "parties," as he calls the experts, for in no other part of geology is it so important to know the paleontologists who determined the fossils. When in the field in 1853 I determined the *Gryphœa* as the *Gryphœa dilatata*, or a variety of it, of the Oxfordian of Europe, and the *Ostrea* as a *Ostrea marshii* of the Lower Oolite of the Jura. After my return from the field, I submitted my fossils to Louis Agassiz, Alcide d'Orbigny, de Verneuil, d'Archiac, Pictet, etc. M. de Verneuil, an excellent paleontologist, as well known in America as in Europe, reported on my fossils before the National Academy of Science of France, and called them *Gryphœa dilatata* and *Ostrea marshii;* and he refers the Tucumcari strata to the Jura. Finally, I have given long descriptions and excellent figures of the two fossils in my volume, entitled "Geology of North America," and also in *Bulletin Société Géologique de France*, vol. xii., 1855. So my two fossils had received all the attention possible, and can be regarded with safety as correctly determined.

Let us see now what guarantee we have as to the correctness of the determination by Mr. Cummins and his "various parties for determination" of his fossils, as he calls his anonymous paleontological assistants. The value of determination of fossils depends much on the name of the paleontologist employed. To be sure, anyone, even the greatest paleontologist, makes mistakes; but it is generally admitted that they are less liable to errors than others. Mr. Cummins is unknown as a practical paleontologist. Until three years ago, he was regarded as a collector of fossils in Texas who has supplied two paleontologists, Messrs. Cope and C. A. White. It this case Mr. Cope has nothing to do, for all the fossils are invertebrates. Mr. White has charge of the Mesozoic invertebrate fossils at the U. S. National Museum, and Mr. Cummins, in a letter to me, says that he did send his Tucumcari fossils to Washington for determination. So it may be assumed that Mr. White is one of the experts, who has agreed to the determinations made by Mr. Cummins. Now Mr. White, during twenty years, has constantly confounded, in all his paleontological memoirs, the *Gryphœa dilatata*, var. *tucumcarii*, with the *Gryphœa pitcheri;* and more, he has said, in some of his papers, that the Lower Cretaceous of Europe has no representative in North America.

As regards my other fossil, the *Ostrea marshii*, which, according to Messrs. Cummins and White, "is in reality *Ostrea subovata*, Shumard." I shall quote from a letter of Mr. Cummins to me, dated Feb. 25, 1892: "I have compared the Tucumcari specimens with *O. subovata*, Shumard, and do not believe they are the same." And I shall tell what occurred in my house during the last visit of Mr. White, in 1884. Mr. White took up a fossil on my chimney mantel-piece, looked at it attentively, and exclaimed: "What a beautiful Cretaceous fossil; it is the most perfect I have ever seen from Texas." My answer was: "The fossil is not Cretaceous; it is the typical *Ostrea marshii*, picked up, with my own hand, in the Lower Oolite of the Jura Mountains at Frickberg, in Argovia, Switzerland." Every one can draw his conclusions as to Mr. White's ability to determine specimens of the *Gryphœa dilatata* and *Ostrea marshii* types.

I have said already before in another paper, and repeat it, that it is impossible to find the typical *Gryphœa pitcheri* (I mean the one described and figured in my "Geology of North America," Plate iv., Figs. 5 and 6) in the same bed with the *Gryphœa dilatata*, var. *tucumcarii*, and the *Ostrea marshii* of Pyramid Mount, on the Tucumcari area.

As to the *Exogyra texana* quoted by Mr. Cummins, it is an incorrect determination of a fossil having some distant affinity of forms. The four or five other fossils in Mr. Cummins's list, are, at all events, not sufficiently characteristic, even if properly determined, for "the conclusion that the beds are Cretaceous."

2. As to the stratigraphic position of the Jurassic strata of the Tucumcari, it is so clear and so striking that a few words will dispose once more of the question. At the Tucumcari there is no discordance of stratification or interruption of any sort between the Trias beds below and the Jurassic beds above. It is a continuous series, with most striking differences in the lithology and paleontology between what is Trias and what I call the Jura. How far those deposits extended eastward and southward, it is difficult to say in the present condition of our limited knowledge of the geology of Texas. Very likely they did extend eastward all over the Indian country of the Commanches, Kioways, Kichais, and Delawares, as far as near Topofki Creek and Delaware Mount; southward they went as far as the upper part of the Trinity River basin, and covered all the upper Brazos and upper Colorado Rivers of Texas. After their upheaval above the sea, at the end of the Jura period, erosions on a great scale occurred and swept away all the Upper Trias and a part of the Middle Trias to such an extent as to reduce the plateau of the Jura Trias nearly to the actual Llano Estacado, obliging it to recede from the vicinity of Topofki Creek several hundred miles westward. Then over the eroded part of the Middle Trias, at Fort Washita, at Comet Creek, and at the Great Band of the Canadian River, an arm of the Lower Cretaceous sea, extended in a narrow strip, as a sort of gulf, which extended as far north as southern Kansas, according to Mr. Cragin's discoveries.

In that gulf, strata, mainly of limestone, were deposited; and at Comet Creek, on the Washita River, where I saw it in 1853, those limestone rocks are a perfect mass of *Gryphœa pitcheri*, with some *Caprotina texana* at the base of the formation. The division of the Texas Cretaceous, to which those "*Gryphœa pitcheri* limestones" belong, has been called since "Fredericksburg Division," and are the homotaxis or equivalent of the Lower Neocomian of Europe, as I have always said ever since.

Mr. Cummins says there is no disagreement between him and Mr. Hill as to the age of the strata of the Tucumcari, which are referred by them to the "Denison beds" of the "Washita Division;" that is to say, a group of strata far above, and consequently younger, than the Comet Creek beds with *Caprotina* and *Gryphœa pitcheri.* So, according to Messrs. Cummins and Hill, the Tucumcari strata, which they call "Denison Cretaceous beds," were deposited in perfect concordance on the top of the Upper Trias, and long after the deposit of the Fredericksburg Division at Comet Creek. A material impossibility, against all stratigraphic and paleontologic principles of formation in a flat country over immense plains; for there is no doubt that the Neocomian strata of Comet Creek, deposited in interrupted discordance over the strata of the Middle Trias, are younger than the strata of Tucumcari, deposited in perfect concordance of stratification, without any interruption, over the uppermost part of the Upper Trias.

What a strange story, unique in the annals of geographical geology. A description of the Tucumcari area, made simply during a difficult and even then dangerous exploration, with all the proofs, stratigraphic, paleontologic, and lithologic, has stirred up an opposition without precedent as regards its long duration. Now—June, 1893—it is forty years since I started from Boston for my exploration by the thirty-fifth parallel, for a Pacific railroad from the Mississippi River to the Pacific Ocean; and, although one concession has been made in my favor, by almost all my adversaries—the correctness of my reference of the lower beds of the Tucumcari to the Trias—the opposition continues, with a degree of intensity and, I am sorry to say, of unfairness never equalled.

Mr. R. T. Hill, after his two visits at the Tucumcari, in 1888 and 1891, has not yet published anything reliable, only a few contradictory statements, without proofs and against plain stratigraphic and paleontologic facts.

Mr. A. Hyatt, after a thorough exploration of two months' duration of a part of the Tucumcari area in 1889, asked me to look over with him his quite extensive collection of fossils, and placed before my eyes his detailed sections of Monte Revuelto. I did not see a single fossil in his collection which can be called a Cretaceous fossil; when, on the contrary, the *Gryphoa* and *Ammonites* had all the most indisputable characters of Jurassic fossils.

For some unknown reasons, not only the report of his exploration has not been published, but even his administrative report as head of a special exploration of the U. S. Geological Survey, in which each explorer gives, every year, the summary of the work done to the director of the survey, has not yet been issued, although the volume in which it ought to be inserted was printed three years ago. The stopping of the distribution of the "Eleventh Annual Report" is somewhat mysterious. Two other printed Annual Reports, the twelfth and thirteenth, remain also undistributed, waiting for the distribution of the eleventh.

Mr. A. Hyatt, in a printed letter in *The American Geologist* for April, 1893, p. 281, admits that his verbal opinion, quoted by me at page 213 of the same periodical, "is correct;" but that he had "at present absolutely no opinion about the age of rocks of this region." A rather curious conclusion for an explorer who has passed two months on the same ground where I was only two days, and who has studied the collection of fossils he made during a whole winter.

Evidently there is some secret about it. My old adversaries, almost all alive now, with the exceptions of the two Shumards, Meek and Newbery, are still at work against me. But I have resisted their combined attacks during forty years, and I can continue very well the defence of my observations and opinions.

However, I shall say nothing more for the present, waiting until after the publication, by some paleontologist, of the fossils collected at the Tucumcari by Messrs. Hill, Hyatt, and Cummins, with descriptions and good figures; for it is absolutely useless to discuss any longer, without proper documents in the hands of geologists, in order that everyone interested in the question may be able to judge for himself as to the conclusions arrived at by the different parties. JULES MARCOU.

Cambridge, Mass.

Natural and Artificial Cements in Canada.

YOUR issue of March 31, 1893, contains an article on "Natural and Artificial Cements in Canada," which in part is incorrect, and I wish to set you right with regard to the class of raw material from which the "Star" Portland cement is manufactured.

In the first place, Star cement is manufactured from shell marl, which is thoroughly decomposed, and containing from 95 to 98 per cent pure carbonate of lime, the clay used is an alluvial blue clay.

The analyses of our clays and marl show them to be of superior quality and equal to any deposits of a similar nature; this has also been fully demonstrated in the practical results obtained by users of the cement when manufactured. E. BRAVENDEE.

Napanee Mills, June 12.

Sound and Color.

On reading Professor Underwood's paper on the above subject in *Science* for June 16th, some rather peculiar experiences of my own, which I have never read or heard of in others, were freshly brought to mind.

When intently listening to certain, but by no means all, eminent speakers, and to a few operatic singers of great renown, I have for some years past distinctly detected, or rather have involuntarily become conscious of, an emanation of color from the head of the speaker or singer with each distinct tone of the voice. The more impassioned the words and tones the more intense the color, and the larger the visible aureole or color area. The color has thus far been limited, with a few exceptions, to a transparent and ethereal but decided blue. It emanates suddenly with each explosion of sound, passes upward like a thin cloud of smoke, and fades like a swiftly dissolving view. I noticed it for the first time while listening to Professor Felix Adler, later on when listening to Colonel Ingersoll, faintly over the head of William Winter; again quite distinctly in case of General Sherman and General Horace Porter, faintly in case of some other public speakers, including Anna Dickinson, Helen Potter, the elocutionist, and some eminent divines, but not at all in case of President Cleveland and some other equally prominent public men.

In case of singers, the most noted instances I can recall are the DeRetszke brothers, Jean and Edward, Mdme. Emma Eames, Lilli Lehmann, Mdme. Albani, Vogel, and Gudehus.

In case of Mdme. Lehmann the blue color verged towards a liquid green, and with Albani it was a pale sheen of silver vapor. In case of Vogel, the tenor, the aureole was an evanescent and very pale straw color. In Mdme. Mielke the blue became a velvety purple or violet. Mdme. Nordica emitted an aureole of pale, translucent gold; Emma Juch gives me the impression of a delicate and liquid pink, while Patti seemed to emit no distinguishing color, but rather a kaleidoscopic blending of many colors.

I should be glad to hear from others who have noted similar phenomena, for I have been inclined to question the reliability of my own impressions, vivid as they have been, and many times repeated. Professor Underwood's recital inclines me to accord them a little more respect. SAMUEL S. WALLIAN, M.D.

Washington Heights, City.

Age of Guano Deposits.

THE following particulars, recently given me by a friend who, years ago, was a sailor, and whom I know to be a man of the strictest veracity, may be of interest as possibly throwing some light on the age of guano deposits.

In the year 1840 his vessel loaded with guano on the island of Ichabo, on the east coast of Africa. During the excavations which were necessary, the crew exhumed the body of a Portuguese sailor, who, according to the head-board, on which his name and date of burial had been carved with a knife, had been interred fifty-two years previously. The top of this head-board projected two feet above the original surface, but had been covered by exactly seven feet of subsequent deposit of guano.

ROBERT RIDGWAY.

U. S. National Museum, Washington, D.C., June 23.

Correction.

IN 1887 I published in the *Canadian Record of Science* an account of a Permian glacial moraine in Prince Edward Island. I have recently examined this formation more carefully, and am not at all positive about its age. The bedding and jointage are conformable with the underlying formation, but the cementing material is purely calcareous, and the induration, though complete, may be recent. In the absence of organic evidence, I do not think we can positively say that this conglomerate is not Quaternary. F. BAIN.

North River, P. E. Island.

BOOK-REVIEWS.

Geological Survey of Missouri. Vol. II. A Report on the Iron Ores of Missouri. By FRANK L. MASON. Jefferson City, December, 1892. Plates, Map, etc. 366 p.

Vol. III. A Report on the Mineral Waters of Missouri. By PAUL SCHWEITZER. Jefferson City, December, 1892. Plates, Map, etc. 256 p.

THERE are but few States in the Union that have not had at some time or other geological surveys of a part or the whole of their territory. As a general rule, the surveys have been conducted by different geologists, the same one seldom holding his position for a long period, and, in point of fact, the survey itself frequently ending before a decade has elapsed. There are, of course, notable exceptions to this, Minnesota, for example, where the State geologist has issued twenty annual reports, and New York, which has enjoyed an almost uninterrupted existence since 1836. Yet more remarkable in this latter case is the fact that the present head of the survey has been such for nearly fifty years and was one of the original corps in 1836. This veteran, as everyone knows, is Professor James Hall, still one of the most indefatigable of all American geologists.

The State of Missouri has had numerous surveys, which have been carried on under various heads. The first survey existed from 1853 to 1862, and published five reports; the second lasted from 1870 to 1874, and issued four reports; the third from 1876 to 1879, and published only one report; while the fourth has lasted from 1889 to date, and has published three bulky volumes, of which the present ones are two, five bulletins, an atlas of maps, and a biennial report. We thus see that under the present management more work has been done than in any of the other surveys lasting twice as long.

In Vol. II. of the reports Mr. Mason has given much valuable information relative to the iron ores. In his introductory remarks he discusses the forms in which iron occurs and the relative value of the various ores. He then takes up the kinds found in Missouri, describes their distribution, and examines in detail coarse and fine specular ore, limonite, red hematite, carbonate, and bog ores. In this discussion, various facts are brought out of interest and value to geologists and students of physical geography. For example, it is concluded that the strata lying about the Archæan outcrops of the Ozark Mountains are of Cambrian age instead of Lower Silurian, as they have been almost universally considered. An excellent description is given of Iron Mountain, Pilot Knob, and other large deposits of ore, and this is followed by an account of the probable origin of the ore beds. The veins are regarded as veins of infiltration, fissures occurring in the rocks having been filled by the solvent action of percolating water through iron-bearing porphyries. The changes produced in topography of pre-Cambrian time by this action of water are briefly sketched as follows: —

"In the first place, whatever the origin of the porphyries, it is allowable to imagine the porphyry region to have been, in pre-Cambrian times, mountainous, or at least hilly. These hills and valleys must have had cracks or fissures in the rocks as we find them to-day. Naturally, erosion or weathering and denuding agencies would begin at the highest points. The products of disintegration would wash from the higher to the lower points. Iron dissolved from the decomposed rocks would, by means of percolating waters, find its way to the fissures in the unweathered rocks at a lower point. In these fissures it would be precipitated, either chemically, by coming into contact with alkaline or other reagents, or would, by slow absorption of oxygen, be made insoluble. This in time would fill the crevices and fissures in the lower rocks with a substance much less susceptible to weathering influences. The rock-mass thus cemented would, as a whole, also tend to resist weathering more effectually than the rock not thus protected. Iron deposits filling fissures would not be formed at the highest points, since solution would tend to carry it either deeper into the hills or into the valleys below. The result of this would inevitably be that the erosive agencies would be much more effective on the elevated portions of the country than on the lower. Gradually the hills would tend to reach the valley level; the valley country, being protected by iron dykes and veins, would resist such erosion. The final result would be that in many cases the hills would be changed to valleys and the valleys to hills" (pp. 57–58).

An interesting account of the Ozark uplift occupies one of the chapters. The region generally goes by the name of the "Ozark Mountains," but it is mountainous in name only. It is an elliptical, dome-shaped elevation, about 140 miles wide and about 200 miles long. Its greatest elevation is about 1,100 feet The average slope to the southeast is a little more than one degree, while to the northeast it is less. The region may be divided into (1) plateau, (2) hilly or "mountainous," and (3) river bottoms. In the plateau region the rivers have their origin. The surface is mostly gently rolling, well-drained, and not steep enough to prevent easy tillage. Following the streams down in either direction the bluffs grow higher and higher, the streams more numerous, and there is soon reached a country cut by deep cañons, or gorges, with steep-walled divides. This is the "mountainous" region, but when one climbs to the summit of the divide, instead of a commanding view, there appears to be a plain spread out on all sides. The "mountain" crests are all at the same level. Floating down the streams the bluffs and hills grow higher and higher, but the fact is soon apparent that, instead of mountains and hills having been thrust up, the plateau has been etched into relief by the streams. The river bottoms begin in the mountain region, at first of little value or extent, but gradually widening out to from one-half to two miles. Here the hills lose their sharp crests and steep slopes, and the bottoms rise gradually by an easy slope to the uplands.

Throughout the region the streams are peculiar. In many cases they spring directly from the foot of a tall cliff and begin at once to cut their gorges. These grow deeper, the walls frequently rising, by a succession of precipices of from 50 to 100 feet, to a height of 500 feet. The courses of the streams are very tortuous. At one place the Osage River flows a distance of seventeen miles when it can be intersected by crossing overland only a single mile. A departure from a direct line of from three to seven miles is not at all uncommon. The absence of bowlders is also noticeable; this being due to the fact that the masses of rock falling from the cliffs are soluble limestone or friable sandstone, and both are quickly removed. The streams are also of large size, yet water courses on the surface generally have no water in them. At the same time many rise to impassable floods in a few hours. In May, 1892, the Current River rose 27 feet in about eight hours. The floods subside as rapidly as they rise, the cause in both cases being the character of the country. There is nothing to conserve the water and it runs off as fast as it falls.

Yet another feature of the region is in the large springs. One of these, called Meramac Spring, is said to flow at the rate of 10,000 cubic feet per minute. Current River rises from a spring of about equal size, and these are but two out of a large number. These springs are, of course, only the outlets of underground rivers. Sinking Creek flows for a long distance as a surface stream. "A few miles from where it empties into Jack's Fork it runs into a *cul de sac*, formed by a crescent-shaped mountain 500 or 600 feet in height. Just before reaching this mountain it sinks from sight and reappears a mile away on the other side of the mountain in the form of a large spring." Naturally, with a scant coating of soil on the hills, the vegetation is not there luxuriant. Twenty or thirty years ago the hills were reported to be bare, but now they are covered with a thin growth of jack-oak, hickory, cedar, and yellow pine. But the growth in the river bottoms, where the soil is rich, is luxuriant, the trees being close together and of gigantic height. Here are found sycamore, gum, elm, water-maple, water-birch, ash, hickory, and numerous oaks. All are so connected by a net-work of vines that it is nearly impossible to get between them.

The age of various sandstones and limestones that have been described in different Missouri reports has long been a vexed question and one that has given all who have attacked it great trouble. This question is taken up by Mr. Mason, and his conclusions may be summed up as follows: —

The geological age of nearly all of the rocks of the Ozark uplift is Cambrian, and the name "Ozark series," originally proposed by Broadhead, is adopted for them. Sandstones alternate with limestones, and these have been known as the first or saccharoidal, second and third sandstones, and the first, second, third, and fourth magnesian limestones. The saccharoidal sandstone has been generally correlated with the Calciferous of New York, and the St. Peter's of Minnesota and Wisconsin; and the magnesian limestones with the Lower Magnesian of the upper Mississippi Valley. Mr. Mason does not believe the evidence sufficient to make more than one sandstone and one limestone formation. For the first he proposes the name of *Roubidoux sandstone*, and for the second the name of *Gasconade limestone*. The outcrops of both sandstone and limestone have been correlated by lithological characters, but it is shown that the two rocks vary greatly. Sections taken along Current River for a distance of sixty miles and along the Gasconade for forty-eight miles show so much variation that it is impossible to trace the different sandstones or limestones with any certainty. There is, however, a stratum which bears fossils of the same general character over wide areas, and by its aid the connection between the sandstone and limestone can be traced. From the lists given it would appear that the affinities are more nearly with the Cambrian than the Lower Silurian epochs. The conclusions given will probably render a reconsideration of the age of certain beds at Eikie's quarry, near Baraboo, Wisconsin, necessary. These beds have been generally regarded as Lower Magnesian. They are probably more likely Potsdam. We have not space to go further into details, but we commend the volume to the consideration of geologists.

The second of our titles treats of a vastly different subject. The first part is taken up by a general discussion of mineral waters in respect to their origin, composition, etc.; and the second, by far the larger part, is devoted to a detailed account of the mineral springs of the State, with over 150 analyses of waters. The waters are divided into muriatic, alkaline, sulphatic, chalybeate, and sulphur. The origin of each of these is briefly discussed. The methods of analyses, classification, and therapeutic uses are also considered. For those who are especially interested in analyses of water and for the citizens of Missouri and other States who desire a knowledge of the location and uses of the various springs the volume is invaluable. It is a volume to be consulted rather than one to be read. JOSEPH F. JAMES.

Washington, May 13, 1893.

A Handy Book for Brewers. Being a Practical Guide to the Art of Brewing and Malting. By HERBERT EDWARDS WRIGHT, M.A. London, Crosby, Lockwood, & Son. 530 p. 8°

MR. WRIGHT has, in the present volume, expanded and enlarged an earlier work well known to the profession, entitled, "A Handbook for Young Brewers," giving the conclusions of modern research in so far as they bear upon the practice of brewing, as well as much practical detail, manipulative and structural. Few books of the size other than mere statistical records contain the amount of information herein included, and if the author has sacrificed style to space it can not be considered a fault in this instance. The book is not intended for general reading, but for the student of brewing, and is to supplement rather than to supplant practical teaching at the works. There is much, however, that is of value to others, both to chemist and to general scientist, as witness the excellent chapters on water, the laboratory, on ferments and fermentation, yeasts, etc. The latter subjects in particular are cleverly treated, and nowhere do we remember seeing the various theories and hypotheses massed together so conveniently for comparison and ready reference. Complete details of malting and brewing operations are carefully given, differing customs are placed in juxtaposition, and in all cases the scientific discussion of chemical and vital changes accompanies the description of the process. It is unfortunate that the glossary originally planned as an appendix to the text was finally omitted, as there are few industrial operations with more technical and "shop" expressions than brewing, and the free use of these in some of the chapters — the author resting, of course, upon his intended glossary—would be rather confusing to the uninitiated. The subject is one of many ramifications, and as such could more easily be handled in three volumes than in one, but Mr. Wright has succeeded admirably with this difficult condensation, and has omitted nothing essential to a thorough knowledge of the subject. C. P.

An Outline of the Documentary History of the Zuñi Tribe. BY A. F. BANDELIER. *Somatological Observations on Indians of the Southwest.* BY DR. HERMAN F.C. TEN KATE. *In a Journal of American Ethnology and Archæology.* J. Walter Fewkes, editor. Vol. III. Boston and New York, Houghton, Mifflin, & Co. 1892.

THE scientific work accomplished by the Hemenway Expedition is gradually becoming known to the world through the medium of Dr. Fewkes's journal. The documentary history of the Zuñis during the 16th and 17th centuries, by Mr. Bandelier, is of absorbing interest and reflects the vast labor that had been expended in its compilation. In the identification of the Seven Cities of Cibola with the ancient Zuñi pueblos, the evidence formerly adduced is made so conclusive, by the introduction of new data, that it seems impossible for any one to fail to be convinced. The events which led to the Pueblo uprising against the Spaniards in 1680 are minutely recorded. Probably half the paper is devoted to copious notes and citations from original sources — principally manuscripts now in the hands of the Expedition. On page 114, the date of Fray Juan del Bal's arrival in New Mexico is given as 1771, instead of 1671, an obvious misprint, as the missionary was killed in the revolt above alluded to.

The second part of the volume is a summary report by Dr. ten Kate of his anthropologic observations of the Pima, Papago, Maricopa, Yuma and Zuñi Indians, as well as of the human remains found in the ruined puebles of the Salado Valley, Arizona, and in one of the Cibolan cities. Although the investigations of Dr. ten Kate and Mr. Cushing were from totally different points of view they unite in the conclusion that "the pre-Columbian Arizonians were closely related to the Zuñis of to-day." In the opinion of Dr. ten Kate the types of North American Indians are not exclusively American, but present only the characteristics of the Mongolian. The author does not mean to imply that our Indians are strictly Mongolians, or that America has been populated from Asia, for he says "the American race is, somatologically speaking, not a type, but has characteristics which can only be called Mongoloid." F. W. HODGE.

Washington City.

Laboratory Calculation and Specific-Gravity Tables. By JOHN S. ADRIANCE, A.M. Second editon. New York, John Wiley & Sons. Interleaved. 114 p.

IN some respects the author has in this second edition given us a new book, having enlarged the original tables and added others of importance. This increase of substance has, too, a value of its own in works such as these, being of far greater importance here than similar additions in general and descriptive works, for not only is the reputation of a book of tables based upon the accuracy of the figures, but also largely upon its completeness. We have all relied more or less upon like works for aid in laboratory calculations, and yet when certain data are found absent, how soon the book will fall into disuse entirely and make its way to the top shelves! Mr. Adriance, however, himself a consulting chemist and fully alive to the necessities of the case, has chosen not only such tables as are in constant use, but also those of frequent or less frequent need, covering extraordinarily well the field of ordinary chemical analysis. Such a work as this is of true assistance, and despite the claim urged by some chemists as to the possible introduction of error when using "ready-made results," we believe the chance of error to be greater when these same results have to be calculated under the strain of physical fatigue, following a long day or night of analytical or experimental work. Naturally all tables of factors, and all data of this kind, should be proven in moments of leisure, and in important cases, notably those involving legal testimony, they should be thrown aside altogether; but for daily use in the laboratory, they are invaluable in the saving of time and mental labor. Not only is the substance of the book "good," but in appearance it is neatness itself, each word and figure is clear-cut and distinct, an element highly important in tabular statements. The book is interleaved and ample opportunity thus given the chemist for additions and remarks C. P.

Chemical Theory for Beginners. By LEONARD DOBBIN, Ph.D., and JAMES WALKER, Ph.D., D. Sc., Assistants in the Chemistry Department, University of Edinburgh. London and New York, Macmillan and Co., 1892. 240 p.

Of the vast number of text-books bearing upon chemistry, we have but few treating of its foundation or theory. The smaller works merely touch upon theoretical discussion, while the larger treatises presuppose an extensive knowledge of the same. It is then a fact that only those students with the advantages of able instruction and scientific associations arrive at a really clear understanding of the ground-work of chemical notation, reaction and law. "Students enter the laboratory at once," is a familiar phrase in many college announcements, and there are excellent arguments for such a custom, provided a thorough study of theory accompanies the practical demonstration. More often, however, in general science courses the theory is disposed of in one or two brief lectures, all effort being concentrated upon a rapid advance into the field of "the elements, their compounds, their characteristics and reactions."

The above work is, we believe, rather unfortunately named, for while it will be easily understood by a beginner, and is a most excellent book for such a one, still it can and will be read advantageously by many advanced students and practical chemists. The wording is smooth and attractive, always interesting, never fatiguing; the student is carried forward, by an easy and natural progression, from the nature of things to a study of chemical action, of combustion, the laws of Boyle, Charles, and Avogadro, of density and of the atomic weights. Chemical notation is not reached until the 12th chapter, where, with the knowledge already gained, its discussion is of value and intelligible to the beginner. The later chapters contain an entirely non-mathematical exposition of the more important principles of general chemistry reviewed in the light of recent research, treating of the kinetic molecular theory, mass action, solution, electrolysis equivalence, the periodic law, etc. C. P.

Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

For sale at low price.—A fine old-fashioned photographic camera, rosewood box, one foot square, lenses, four inches diameter, made by C. C. Harrison. Plateholders, troughs, baths, etc., all in large wooden case, formerly the property of the late President Moore, of Columbia College. This is a fine example of an instrument of the best make for the old wet-process methods, and valuable to any institution or amateur interested in the history of photography in the U. S. Address M. S. Daniel, 236 W. 4th St., New York.

I wish to exchange a collection of 7,000 shells, 1001 species and varieties, American and foreign, land, fluviatile and marine, for a good microscope and accessories. Address, with particulars, Dr. Lorenzo G. Yates, Santa Barbara, California.

For exchange.—I wish to exchange Lepidoptera of South Dakota and other sections, for Lepidoptera of the world. Will purchase species of North America. Correspondence solicited, particularly with collectors in the Rocky Mountains, Pacific coast and Hudson's Bay regions. P. C. Truman, Volga, Brooking county, South Dakota.

For sale.—Wheatstone Bridge wire, made to order, new and unused. Price, $10. W. A. Kobbe, Fortress Monroe, Va.

For sale or exchange.—One latest complete edition of Watt's Dictionary of Chemistry, in fair condition; one thirty volume edition (9th) of Allen's Encyclopædia Britannica, almost new. Will sell cheap for cash or will exchange for physical or chemical apparatus. Address Prof. W. S. Leavenworth, Ripon College, Ripon, Wis.

Exchange.—One celestial, one terrestrial globe, one lunatettis and charts, celestial maps, diagrams and ephemeris from 1830 to 1893, astronomical works, all in good condition. Will sell cheap or exchange. Make offer. C. H. Van Dorn, 79 Nassau St., New York.

The Rev. A. C. Waghorne, New Harbor, Newfoundland, wishes to sell collections of Newfoundland and Labrador plants, all named by competent botanists. He is going on a missionary journey along the Labrador coast, from the middle of July till October, and in return for much needed aid towards (Episcopal) Church purposes in that region, will be glad to be of service to any botanists who may write to him. Letters posted in the U. S. up to July 1 will reach him at the above address, and if posted later will be forwarded.

Wants.

A GRADUATE of an American Polytechnic institution and of a German university (Göttingen), seeks a position to teach chemistry in a college or similar institution. Five years' experience in teaching chemistry, Address Chemist, 757 Cary St., Brockton, Mass.

AN experienced teacher in general biology wishes a position in a first-class college or university. Three years in post-graduate study. Extensive experience. Strong indorsements. Address E. W. Doran, Ph.D., 1327 G St., N. W., Washington, D. C.

THREE teachers wanted for a male and female seminary in central New York. Typewriting, etc., languages, mathematics, sciences, *et. al.* Send stamp with and for particulars. Box 701, Hempstead, L. I.

A ZOOLOGICAL collector and taxidermist of ten years' experience in the field is now open to engagement, for either field or laboratory work. References furnished. Address Taxidermist, Box 75, White Sulphur Springs, West Va.

WANTED.—A set of Allen's Commercial Organic Analysis, 4 vols. Vols. I. and II. particularly desired. Condition not important, all leaves being present and in place. Address Charles Platt, 34 Lewis Block, Buffalo.

WANTED, as principal of a flourishing technical school, a gentleman of education and experience who will be capable of supervising both mechanical and common school instruction. Special familiarity with some technical branch desirable. Address, giving age, qualifications, etc., J. B. Bloomingdale, Fifty-ninth street and Third avenue, N. Y.

WANTED.—A young man as assistant in our microscopical department. Queen & Co., Philadelphia.

THE undersigned desires specimens of North American Gallinae in the flesh for the study of their pterylosis. These species are especially desired: *Colinus ridgwayi, cyrtonyx montezumae, deudragapus franklini, lagopus welchi, tympanuchus cupido* and *pedioecetes phasianellus.* Any persons having alcoholic specimens which they are willing to loan or who can obtain specimens of any of the above are requested to communicate with Hubert Lyman Clark, 3922 Fifth Avenue, Pittsburgh, Pa.

QUERY.

Can any reader of *Science* cite a case of lightning stroke in which the dissipation of a small conductor (one-sixteenth of an inch in diameter, say,) has failed to protect between two horizontal planes passing through its upper and lower ends respectively? Plenty of cases have been found which show that when the conductor is dissipated the building is not injured to the extent explained (for many of these see volumes of Philosophical Transactions at the time when lightning was attracting the attention of the Royal Society), but not an exception is yet known, although this query has been published far and wide among electricians.

First inserted June 19, 1891. No response to date.

N. D. C. HODGES, 874 BROADWAY, N. Y.