which were themselves in error about one-tenth of a second and required subsequent correction in other ways. Here then was a lesson to astronomers who are all more or less specialists, but it merely enforced the perfectly well known principle that the constant errors of any one method are accidental errors with respect to all other methods, and therefore the readiest way of eliminating them is by combining the results from as many different methods as possible. However, the abler the specialist the more certain he is to be blind to all methods but his own, and astronomers have profited so little by the Encke-Hansen-Le Verrier incident of thirty-five years ago that to-day they are mostly divided into two great parties, one of whom holds that the parallax can be best determined from a combination of the constant of aberration with the velocity of light, and the other believes only in the results of heliometer measurements upon asteroids. By all means continue the heliometer measurements, and do everything possible to clear up the mystery which now surrounds the constant of aberration, but why ignore the work of predecessors who were quite as able as ourselves? If it were desired to determine some one angle of a triangulation net with special exactness, what would be thought of a man who attempted to do so by repeated measurements of the angle in question while he persistently neglected to adjust the net? And yet, until recently astronomers have been doing precisely that kind of thing with the solar parallax. I do not think there is any exaggeration in saying that the trustworthy observations now on record for the determination of the numerous quantities which are functions of the parallax could not be duplicated by the most industrious astronomer working continuously for a thousand years. How then can we suppose that the result properly deducible from them can be materially affected by anything that any of us can do in a lifetime, unless we are fortunate enough to invent methods of measurement vastly superior to any hitherto imagined? Probably the existing observations for the determination of most of these quantities are as exact as any that can ever be made with our present instruments, and if they were freed from constant errors they would certainly give results very near the truth. To that end we have only to form a system of simultaneous equations between all the observed quantities, and then deduce the most probable values of these quantities by the method of least squares. Perhaps some of you may think that the value so obtained for the solar parallax would depend largely upon the relative weights assigned to the various quantities, but such is not the case. With almost any possible system of weights the solar parallax will come out very nearly $8{\cdot}809'' \pm 0{\cdot}0057''$, whence we have for the mean distance between the earth and sun 92,797,000 miles with a probable error of only 59,700 miles; and for the diameter of the solar system, measured to its outermost member, the planet Neptune, 5,578,400,000 miles.

WILLIAM HARKNESS.

WASHINGTON.

THE BALTIMORE MEETING OF THE AMERICAN SOCIETY OF NATURALISTS.

THE thirteenth annual meeting of *The American Society of Naturalists* was held at Baltimore during the Christmas vacation. Considering that Baltimore is the southern limit where meetings may be held by the Society, the attendance was large, forty to fifty members being present.

The first session was called to order by the President, Professor Charles S. Minot of the Harvard Medical School, at 2 P. M. on Thursday, December 27th.

A quorum being present, the Society at once proceeded to the transaction of business. The committee appointed in 1893 to obtain, if possible, the removal of the duty on scientific instruments reported that although they had succeeded in obtaining the coöperation of most of the leading scientific men, yet the inception of the movement had been so delayed that the Gorman Bill was already being considered by the Senate before the petitions could be presented to the House.

The following resolution recommended by the committee was then adopted: "Inasmuch as the repeal of the present iniquitous duty on scientific instruments is imperatively needed by the interests of the country, we recommend that a committee be appointed to present our just demands to the President, to the Chairman of the Committee on Finance of the Senate and the Chairman of the Committee of Ways and Means of the House of Representatives, and to take such other steps as may be practicable to secure the immediate repeal of the duty."

The report of the committee on the revision of the Constitution and By-Laws was unanimously adopted. By the new constitution *The American Society of Naturalists* encourages the formation of other societies of similar name and object in other parts of the country and invites other societies whose chief object is the encouragement of the study of Natural History to become affiliated with it. The affiliated societies shall have a common place and time of meeting with the American Society of Naturalists, the expenses of which are to be paid from a common treasury supplied from a common fee. The records of the secretaries of the different societies are also to be published at common expense.

The discussion upon *Environment in its Influence upon the Successive Stages of Development and as a Cause of Variation*, took place in the Physical Lecture Room, Thursday afternoon. It was opened by four papers and followed by remarks by Professors Cope and Hyatt, Dr. Dall, Dr. C. V. Riley and others.

Professor Osborn, of Columbia College, in opening the discussion, observed that naturalists were reacting from the discussion of *theories* towards the renewed inductive and experimental study of the factors of Evolution. This was due to the feeling that the prolonged discussion led by Spencer and Weismann had assumed a largely deductive character and would not lead to any permanent results. The inductive reaction had taken two directions: first towards the exact study of Variation, and second towards experimental Evolution. As regards Variation we should not expect to form any laws so long as variations were considered *en masse* without regard to the past and present history of the organisms studied. That organisms vary with their environment is a truism. What we need is a clearer conception and interpretation of this relation as a basis for experimental study in the laboratory and in the field. The first misconception to be removed is that which has sprung up from the misuse of the terms Heredity and Variability. Nägeli pointed out many years ago as Weismann and Hurst have insisted more recently that Heredity includes one phenomenon seen from two sides which may conveniently be termed Repetition and Variation. A large number of the variations recorded by Bateson, for example, are simple repetitions of ancestral structure, and every new variation is to be regarded as the expression of hereditary forces working under new conditions. The first object of investigation is to decide the *time of origin* of a variation, first in race history, second in individual history. Variations which arise as practical repetitions of past experience may conveniently be termed '*palingenic*,' while those which are new to the organism may be termed '*cenogenic*.' As regards individual history the most important question is to determine whether a variation is merely '*ontogenic*,' that is springing up in the course of individual development from some disturbance of the hereditary mechanism, or '*phylogenic*' and constant as distinguished by Nägeli. From recent study of palingenic variation we must recast our conception of Heredity especially in view of the remarkable researches of Cunningham upon the color, and of Agassiz, Giard and Filhol upon the symmetry of the flat fishes (Pleuronectidæ). These characters of enormous antiquity, summoned as it were from the vasty deep, reveal the law that repetition or variation in ontogeny depend largely upon repetition or variation in environment, that for many of the most fundamental characters, development and environment are inseparable and all theories which tend to separate the two are untenable. As regards cenogenic variations or those which are new in the experience of the organism, the distinction between ontogenic variations, or what are commonly called acquired characters, and phylogenic variations is also of pressing importance. The organism may be compared to a clock, keeping regular time upon a base; if the base is tilted slightly the clock may continue to tick but it may not keep the same time; if after the lapse of a long period the base is restored to its original position the clock will tick in correct time as before. This thought shows that the conditions which have been demanded as crucial tests of the permanent phylogenic influence of environment upon organisms will be very difficult to fulfill in experiment—when the repetition of a mesozoic environment is found to produce a repetition of a mesozoic structure. Experiment should now be directed separately upon each of the four stages of development (germ cell, fertilization, embryonic, larval and adult) and then withdrawn, and putting together the results of all the work which has been recently done of this kind we find three classes of variation phenomena coming to the surface; first 'palingenic variations,' second 'saltations,' third 'ontogenic adaptions' (Haeckel); fourth a class of 'phylogenic variations' which have been termed 'mutations' by some paleontologists. We are so far from a solution of the working causes of these four classes of variation that it seems best to consider that we are on the threshold of the Evolution Problem, to take an entirely agnostic or doubtful position as to all the prevalent theories, and press forward in strictly inductive search for laws which may not be forthcoming until the next century.

Professor Edmund B. Wilson, of Columbia College, followed with a discussion of the influence of the environment on the early stages of embryonic development. That a change of external conditions, such as temperature, chemical nature of the medium and the like, causes changes in the rate or form of development has long been a familiar fact, but we have only recently come to perceive clearly how significant are the changes thus brought about and how vital is the part played by the environment in all development, whether pathological or normal. For if a changed mode of development is the 'result' of a change of environment, the normal development must in exactly the same sense be the 'result' of the normal environment, *i. e.*, in both cases we are dealing with a definite physiological response of the idioplasm to external conditions. The facts both of normal and of experimental embryology demonstrate the justness of this point of view. The experiments of Pflüger, Driesch, Roux and others show, for instance, that the forms of cleavage may be profoundly altered by mechanical means, and indicate that some of the normal fundamental cleavage-forms are the direct result of mechanical conditions, such as the shape of the egg, pressure of the membranes, surface tensions between the blastomeres, and the like. Temperature has a profound effect not only on the rate of development, but also on its form. Thus Driesch showed that the eggs of sea-urchins when incubated at a temperature slightly above the normal undergo remarkable changes. The form of cleavage may be considerably altered (without affecting the end result of development), and the gastrulation may be profoundly affected. In some cases 'exogastrulæ' are formed, the archenteron being turned out instead of in, and these undergo all the normal differentiations of the *Pluteus*, though they ultimately perish since the alimentary canal is turned inside out and the larvæ are incapable of taking food. Other physical agents such as gravity have been shown to have a profound effect on development, determining the position of roots and branches in hydroids (Loeb), or even the polar differentiation of the egg as in the frog (Pflüger, Born, etc.).

The most remarkable and significant examples of environmental influence are, however, found in the effect of change in the chemical environment. In the case of sea-urchins Ponchet and Chabry found that in sea-water deprived of calcareous matter the *Pluteus* larva is unable to develop its spicular skeleton, and Herbst showed that the same result was produced by a very slight excess of potassium chloride in the water even though the normal amount of calcareous matter were present. In both cases the larvæ not only fail to develop spicules, but are unable to produce the characteristic ciliated arms. Thus arises a larva having a simple ciliated belt and very similar to a young *Tornaria*. This is a very instructive case; for it shows in the first place that a definite character (formation of the skeleton) has a fundamental though very subtle relation to the external environment, and in the second place, that this relation indirectly extends to other characters (ciliated arms) that follow upon the development of the first character. Such cases pave the way to a rational conception of epigenesis, by showing the multiplication of effects in ontogeny and the complicated results that may follow from a single and apparently insignificant condition of the environment.

Even more striking results are those obtained (by Herbst) by the addition of a minute percentage of lithium chloride to the sea-water. The primary result is to cause exogastrulation (like the effect of raised temperature). Beyond this, however, the entoderm area (*i. e.*, archenteric region) often becomes abnormally large. The entoderm may then be reduced to a mere knob consisting of only a few cells, or may even disappear altogether so that a blastula is formed that *consists entirely of entoderm!* This extraordinary result, if it can be accepted, shows that even so fundamental a process as the differentiation of the germ-layers stands in a vital relation to the chemical environment. It is a revelation of the importance of environmental influences in development and it shows that we must readjust our conceptions not only of development but also of inheritance, of which development is an expression. Our attention has been focussed too closely upon the formal morphological aspect of development which we have regarded too largely as the result of a pre-organized germ-plasm operating like a machine. Embryological development must be thoroughly re-examined from a physiological point of view, full weight being given to the essential part played by the environment. This point of view in no way sets aside the necessity of assuming a specifically organized germ-plasm for every species as the basis of inheritance. It simplifies the problem, however, and opens the way for further investigation, which is practically barred by the artificial and formal theories of development advocated by Roux and Weismann.

The third paper read was by Professor

W. K. Brooks, of the Johns Hopkins University. The subject was: *An Intrinsic Error in the Theories of Galton and Weismann.* It will be published in full later. The principal point taken was against the theory of variation springing from a mixture of ancestral characters. It was shown that many lines of descent may arise from a very small number of parents and represent a slender thread, consisting of very few strands, many individuals of the same species having an identical remote ancestry. In other words, sexual environment instead of being unlimited is very narrow, and as we pass backwards the number of ancestors increases rapidly for a number of generations, and then decreases instead of increasing indefinitely. The causes of variation are therefore to be sought rather in modern conditions of organisms than in the remote past.

Dr. C. Hart Merriam, of the United States Department of Agriculture, contributed an exhibition and discussion of a beautiful series of mammal and bird types exhibiting protective coloring and a number of dynamic variations. The origin of protective colors is to be sought in fortuitous variation preserved by selection. The theory of the direct action of environment in modifying color as in the bleached types of the desert regions is not borne out by observations and is disproved in the case of nocturnal types. A second and distinct class of facts comes under the head of Dynamic Variation, and to this class we refer to modifications of the beak, of the feet and limbs as due primarily to the habits and activities of the animals themselves.

At the close of the afternoon session, Professor E. B. Wilson, of Columbia College, exhibited by means of the stereopticon, lantern slides, prepared from photographs taken from sections, illustrating the cytological changes during maturation, fecundation, and segmentation. The different effects of the various killing, fixing and staining agents upon the ultimate details of cell-structure, were admirably brought out.

At eight o'clock the Society had the pleasure of listening to Professor William Libbey, of Princeton, who told of his experiences during *Two Months in Greenland.* The lecture was illustrated by a large number of magnificent views of Polar Scenery.

After the lecture the members were entertained by the authorities of the Johns Hopkins University and the citizens of Baltimore at a most pleasant assembly in McCoy Hall.

The Society reassembled at nine o'clock on Friday morning, Dec. 28th.

Officers for the year 1895 were chosen as follows:

President—Professor E. D. Cope, University of Pennsylvania.

Vice Presidents—Professors Wm. Libbey, Jr., Princeton University; W. G. Farlow, Harvard University; C. O. Whitman, Chicago University.

Secretary—Professor H. C. Bumpus, Brown University.

Treasurer—Doctor E. G. Gardiner, Boston, Mass.

Committee-at-Large—Professors E. B. Wilson, Columbia College; W. H. Howell, Johns Hopkins University.

The following persons were elected to membership in the Society:

William Ashmead, U. S. Dept. Agriculture, Washington; Severance Burrage, Mass. Inst. Tech., Boston; W. E. Castle, Harvard University; H. E. Chapin, University of Ohio, Athens, Ohio; J. E. Humphrey, Johns Hopkins University; M. M. Metcalf, Woman's College, Baltimore; H. C. Porter, University of Pennsylvania; W. H. C. Pynchon, Trinity College, Hartford; Charles Schuchert, U. S. National Museum; Norman Wyld, late of Bristol, England.

The report of the Treasurer showing a balance of somewhat over $200 was accepted by the Society.

The Society, on motion of Professor Bumpus, appropriated a sum not to exceed $130 to equip the American table at the Naples Station with proper microtomes, and a committee of three was appointed to attend to this matter.

Professor J. S. Kingsley detailed a 'bibliographical project' originating with Professor G. W. Field of Brown University. This proposes to put into the hands of workers in zoölogy a bibliography of current literature, in such a form as to be readily accessible, the latter to be readily combined with the earlier, and to present the matter both as to subjects and as to authors. By a vote of the Society, a committee of five was appointed to consider this 'project' and to report in print both in SCIENCE and in *The American Naturalist.*

President Gilman, in a very pleasant and cordial way, then welcomed the members of the visiting societies to Baltimore, speaking on behalf of the authorities of the Johns Hopkins University and of the citizens of Baltimore.

President Minot chose for the subject of his address '*The Work of the Naturalist in the World.*' The object of the naturalist is to discover the truth about nature and to publish the results of his work to the world. The conditions of success are readily to be observed. First and foremost is truth. The naturalist's first business is to get at the truth, and the obstacles which stand most prominently in his way are: (1) the limitations of his own abilities, and (2) the limitations of accessories for carrying on his work. The naturalist must observe, experiment and reason, and his training must necessarily be along these lines. Experimentation is necessarily more difficult than observation, for in the former case the naturalist asks why, not how. The great work of the future, as is already being shown, is to be done by the experimenters. Our notion of causation is still in a very rudimentary condition.

Again, the reasoning faculty is one of our weakest points. The naturalist must learn to carefully distinguish between discussion and controversy, and while being led and taught to indulge freely in the former with all the intelligence at his command, he must also be taught to avoid the latter.

The naturalist is naturally exposed to many evils, such as this matter of controversy, which tend to cause him to depart from his proper mission, viz., of getting at the truth. He is especially likely to be led astray by impatience to get results. Preliminary communications are a very great as well as a very prevalent evil. The opinion of the speaker was very pronouncedly adverse to this form of publication. The greed for priority leads many even fine workers far astray.

The tendency to speculate is a third evil, and this has perhaps reached its culmination in the doctrines of Weismann. Another evil is the one which leads us to accept too readily simple and well finished conceptions. Herbert Spencer furnishes us with an illustrious example of the effects of this.

In the matter of publication, four classes may be distinguished: (1.) *Original Memoirs;* (2.) *Handbooks;* (3.) *Text Books;* (4.) *Bibliographies.* The last three are important both in form and in the matter. The first are like digestive organs. It is their function to assimilate crude facts and render them digestible. Advice to prune and digest such matter for publication is much needed. Details not bearing directly upon the subject should be carefully excluded. Most original papers could be 'boiled down' to one half, and some even to one tenth of the amount that is really published. The English write best and this may be owing to the example of Huxley. The Germans and Americans who copy after them come next, and the French are the greatest sinners in the matter of verbiage.

The effect of the work of the naturalist upon his own character is especially shown in his optimism. Literary men seem much inclined to grow pessimistic. This point is well illustrated by a comparison of the recently published letters of Asa Gray and of James Russell Lowell. Lowell's letters show increasing pessimistic views toward the end of his life, while those of Gray remain uniformly optimistic. Something of this was undoubtedly due to the different temperaments of the two men, but much was also due to the different nature of their work. Gray could always see new things unfolding before him.

One drawback in the naturalist's life is his comparative loneliness and isolation. Seldom has he in his own neighborhood another interested in the same particular line as himself. Reunions of naturalist societies, such as those at the time meeting in Baltimore, counteract this to a considerable extent, but there is need of even greater affiliation.

The influence of the naturalist upon mankind in the way of teaching them competence had not been considered sufficiently. In political questions competency comes in, and the solution of much of our present trouble lies not so much in restricting the right to vote as it does in restricting the right to become a candidate. We, as naturalists and as citizens, should uphold competence. Our schools, even the best of them, judging by their results, do not educate properly. The naturalist should see to it that our schools educate, with science in its proper place. It is the duty of the naturalist to advance the development of the university. The schools use elementary knowledge to advance the mind in acquisitiveness, and the college uses advanced knowledge in the same way, but the university attempts to advance the mind in independent work, to develop and discipline originality.

To carry on its proper work the university needs a large endowment, at least $10,000,000. It is not possible to teach zoölogy unless the proper instruments and books are provided. The university, above all, needs proper professors. The qualifications of a professor in a university should be: (1) the ability to carry on original researches himself, and (2) to train others to carry out original work.

The annual discussion on '*Laboratory Teaching of Large Classes*' followed Professor Minot's address. Professor Alpheus Hyatt, of the Boston Society of Natural History, introduced the subject somewhat as follows:

Teaching has two objects in view: (1) to train the faculties of individuals, and (2) to increase the store of information. The importance in laboratory teaching of bringing the pupil into contact with the organisms themselves is absolutely necessary. The term, 'large classes,' is relative. It may mean twenty, thirty, forty, up to several hundred. In teaching large classes, there must be taken into account the matter of division into sections, rooms, assistants, apparatus, etc. The first point to be insisted upon is the matter of personal contact between the pupils and the instructors. In experience with Boston teachers, the classes numbered five hundred. Tables were provided for the whole number, and on these tables were placed the trays of specimens on which the exercise was to be given. The specimens were thus arranged before the exercises by assistants. The lecturer then proceeded to demonstrate the various points upon his own specimens, and the pupils followed him by working out the same points on the specimens in the tray. The specimens kept the lecturer down to his subject and also kept the pupils at work. Of course the field was necessarily limited.

The initial expense for providing the material was small, being about $10 for geology, $15 for botany, and $25 for zoölogy. Diagrams and crayon sketches, magnifying glasses, and various methods of a simple kind were made use of. These methods were afterwards used with smaller sections with even more satisfactory results. Examinations were given to test the pupils' proficiency, not only in knowledge of the subject but also of methods of study. For this purpose test objects were given the pupils to examine and describe. At the close of his paper, Professor Hyatt exhibited some specimens of these examinations.

Professor H. C. Bumpus, of Brown University, spoke upon the subject from the zoölogical point of view. The value of laboratory work depends largely upon good material, which should be supplied in abundance and in excellent condition. At the present time there is no excuse for supplying poor or scanty material, since abundance of excellent material can be obtained at small cost. The importance of having the best dissections and best drawings obtainable in the laboratory itself cannot be overestimated. It does not induce the laziness and attempts at shirking that seem to be the fear of so many teachers. If the student desires to copy a fine dissection he is to be encouraged to do so, and any teacher can readily detect the sketches copied from a chart or diagram. The speaker said also that he had found it an excellent plan in certain difficult cases to supply blanks on which the outlines of important structures were laid down, the details to be added by the pupil. A printed outline of the order of work, directions for manipulation, and questions to be answered from the specimens are a great help. The need of competent assistants is obvious.

The botanical side of the question was considered in a paper by Professor W. F. Ganong, of Smith College. The experience given was obtained in managing classes of about 200 men at Harvard, and the plan given was worked out under the guidance of Professor G. L. Goodale. The conditions under which the instruction was undertaken were: (1) The classes were too large for individual teaching by the instructor; (2) laboratory hours must be adjusted to other academic work, to insufficient accommodations, and sometimes even to yet other considerations; (3) many students of diverse attainments must be taught how to work and to think scientifically, and must be kept progressing together through the stages of a logically graded course, and (4) large quantities of special material must be provided for at unfavorable seasons.

In conducting such classes competent assistants were necessary, each to have not more than twenty men under him, and these were to remain under his special charge throughout the course. Such assistants may be readily recruited in any large university where there are special students doing advanced work. The assistants met the instructor to talk over plans and details of coming work. Uniformity of plan was insisted upon, but details of method were left to the assistant. The instructor did not devote himself to any one section, but visited each one as often as was possible. Weekly guides were printed for the use of the student, indicating the points to be studied, their relative importance, and any necessary information given. They were intended to supply just enough data to enable the student to progress to correct conclusions.

The materials required were arranged in the course, so that in the winter such things as could be grown easily or procured out of doors, as seeds, seedlings and buds, came first, and then followed the succession of opening buds, leaves, flowers and fruit made accessible by the advance of spring. In other words, the time of giving the course and the grouping of the subjects was so arranged that the material for each subject was in proper condition when it came before the class. Some of the weekly guides accompanied the paper, for examination.

Discussions were presented by Professors H. W. Conn, Marcella O'Grady, E. S. Morse and C. S. Minot, and the additional fact was brought out that a good synoptic collection was a desirable feature of the laboratory equipment, in order that the pupil might not have too narrow a view of each group of organisms, such as he is likely to carry away from the study of a single type.

After passing a vote of thanks to the authorities of the University, the citizens of Baltimore and the University Club for the hospitality extended to it, the Society adjourned.

The annual dinner of the affiliated Societies took place at 'The Stafford' at 7:30 on Friday evening. No set toasts were given, but informal speeches formed a very pleasurable close to this reunion.

W. A. SETCHELL, *Secretary.*
YALE UNIVERSITY.

THE PRINCETON MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION.

THE third annual meeting of *The American Psychological Association* was held at Princeton College on Thursday and Friday, December 27th and 28th, under the presidency of Professor William James, of Harvard University. Psychology is the youngest and likewise one of the most vigorous of the sciences. Although the Association is small, consisting of those only who are actively engaged in psychological investigation, and the members are widely scattered, there were sixteen papers read, exclusive of those presented in the absence of their authors. Indeed, the only drawback to the pleasure of the meeting was the fact that the program was so crowded that there was not sufficient time for discussion and social intercourse. The short intervals between the meetings were, however, pleasantly filled, owing to the hospitality of President Patton and Professor Baldwin, and the excellent accommodations of the Princeton Inn.

The Association was welcomed to Princeton by President Patton in a fitting address in which he alluded to the importance of such meetings, not only for the advancement of science, but also for the cultivation of inter-university friendliness, to the death and life-work of President McCosh, and to the prominent place always given to philosophy and psychology at Princeton.

The papers presented covered a wide range of psychological topics. Experimental psychology proper was not so fully represented as in the Philadelphia and New York meetings, owing to the detention of several members, but all the communications were strictly scientific in method.

The first paper, *Minor Studies and Apparatus*, by Professor Sanford, was, indeed, of purely experimental character, coming from Clark University, where President Hall has given such a prominent place to experimental psychology. Professor Sanford first showed charts demonstrating that the retinal fields for color are relatively smaller in the case of children than in the case of adults. In the second study he reported experiments on the accuracy with which an observer can distinguish by different senses which of two stimuli is first presented. A flash of light is perceived relatively earlier than a sound—contrary to results formerly published by Exner. In a third study primary memory was investigated. In a fourth study questions were asked students concerning the confusion of related ideas, for example:—How do you distinguish your right from your left hand? How do you call up a forgotten name? How do you collect the attention? What were your favorite games when a child? What is the earliest