science, while Chicago, Syracuse and Wisconsin give the master of philosophy, and Chicago the master of letters. The largest number of A.M.'s was given last year by Columbia—193; Harvard with 152, Yale with 69, and Princeton with 52, being the only other institutions that awarded more than fifty. Chicago, with 54, leads in the number of Ph.D. degrees conferred, and is followed by Columbia, 42; Johns Hopkins, 35; Harvard, 33; Pennsylvania, 26; Yale, 23, and Cornell, 19.

Illinois leads in the number of degrees conferred upon students of agriculture—43, and is followed by Cornell with 34. New York University granted 32 degrees in commerce. Pennsylvania, with 95, leads in the number of dental degrees, being followed by Northwestern, 88; Michigan, 46; Iowa, 38, and Illinois, 34. Chicago granted 23 degrees in divinity, Yale 11, and Harvard 7, while Yale conferred 25 degrees in forestry and Syracuse 14 in music. Columbia leads in the number of bachelor's degrees in education—103, Missouri awarding 42; New York University, 37, and Chicago, 21. Northwestern granted 81 degrees in pharmacy; Illinois, 30; Michigan, 28, and California, 27; Columbia only gave 12, but to this number should be added the 110 given by the New York college of pharmacy. The largest number of diplomas (not degrees), namely 219, was conferred by Columbia University to students of the Teachers College.

Rudolf Tombo, Jr.,
Registrar

Columbia University

EVOLUTION AS IT APPEARS TO THE PALEONTOLOGIST[1]

I desire to introduce this paper by the statement of a law which seems to be axiomatic, although it is largely ignored by biologists. I may term it the *law of the four inseparable factors.*

These four factors in the life of organisms are known to us under the terms heredity, ontogeny, environment and selection. The following statement regarding these factors expresses the whole truth:

1. The life and the evolution of organisms invariably center around processes which, in our observations, are grouped under heredity, ontogeny, environment and selection.

2. These have been inseparable and interacting from the beginning.

3. A change introduced through any one of these factors causes a change in all.

This I believe to be the most fundamental law of biology; far more fundamental than the well-known biogenetic law. Yet a survey of recent discussion among biologists as to the theory of evolution shows broad lines of division into several schools of opinion strictly according to the factor from which the subject has been approached. It is true that, conceiving any one of these principal factors as separable, we become involved in endless difficulties; conceiving them as inseparable and continuously interacting under natural conditions, we reach the only true conception of the evolution process. Of these four factors *selection* is the only one which can be experimentally removed through the agency of man; heredity, ontogeny and environment may be modified but they can not be removed.

I shall not stop here to demonstrate, as I shall do elsewhere, that changes may be initiated or find a gateway through any one of these four factors; I shall state simply that under certain circumstances heredity, under other circumstances ontogeny, under still others environment, or finally under selection, a new order of adjustments begins in animals and plants and a new series of characters appears. When such a new order sets in through any one of these

[1] Address before the Seventh International Zoological Congress, Section of Paleozoology.

factors a readjustment of all the others sooner or later ensues.

There are two other laws which I personally regard as demonstrated, although they have not yet reached the stage of axiomatic truth. These are: (4) that all changes in ontogeny, environment and selection sooner or later are reflected in heredity; (5) that all changes in heredity originate (*a*) either from within (keeping in mind always the law of the four inseparable factors) or (*b*) they originate from without through ontogeny, environment or selection.

The *origin of new characters,* not the origin of species, is the central problem in evolution. In using the word *originate* I do not imply *cause;* I use "originate" simply to imply appearance in order of time.

I. HEREDITY AS IT APPEARS TO THE PALEONTOLOGIST

I may now restrict the present discussion to heredity as it appears to the paleontologist. As compared with his brother and sister zoologists the paleontologist enjoys certain peculiar advantages and labors under certain disadvantages.

First, it is a decided advantage that the paleontologist as an observer is practically immortal; that is, if the two or three million years in which he is able to follow certain characters constitute a reasonable longevity. Concentrating his attention on the *history of individual characters,* entirely irrespective of the species question, which is wholly a by-question, the paleontologist may trace such individual characters from their origin through their various changes, through their entire history, in fact. In this sense he is immortal. The zoologist and experimentalist (*e. g.,* de Vries, Bateson, Morgan), on the other hand, is mortal. First, he is not always in a fair position to judge which characters are important and which are unimportant. This, for instance, is the chief difficulty with Bateson's first great work, "Materials for the Study of Variation." Second, the zoologist and experimentalist is too short-lived to observe and measure those changes, if such exist, which are so excessively slow as to be invisible and immeasurable by his mortal eye, and he is most naturally led to the conclusion that visible, observable and measurable changes, viz., *saltations, discontinuities* or *mutations* (of de Vries) are the most important if not the only changes.

Having pointed out the peculiar opportunity of the paleontologist, let us omit discussion of all other modes or sources of change and concentrate our attention on what is certainly the most vital point, namely, the *origin of new characters in heredity.* We shall, therefore, begin by considering this origin as a question not of ontogeny, nor environment, nor selection, but of heredity pure and simple. By this statement we do not commit ourselves either way upon the question of the inheritance of new ontogenetic or environmental characters, we simply confine the subject to changes which first appear through heredity, that is congenitally or at birth.

II. THE RATE OF ORIGIN OF NEW CHARACTERS

Thus we come to the *paleontologist as a student of heredity.* What are his peculiar advantages and disadvantages in this limited field of observation? Our answer is a bit embarrassing because we find that as to the velocity or suddenness of origin of characters paleontologists have not thus far agreed; they have reached two opposite opinions, as follows:

First opinion: *Hypothesis of sudden appearance.* This was first set forth by Geoffroy St. Hilaire, and has been advocated by Cope, Dollo, Smith Woodward and other paleontologists.

Two unanswerable criticisms of this opinion may be made. First, in attempt-

ing to support this hypothesis of sudden origin by inductive evidence the paleontologist is certainly at a great disadvantage through deficiency of material. In order to demonstrate a sudden change, a discontinuity or a saltation in single characters, one must have all or a very large number of contemporary individuals for comparison. I maintain, therefore, that the paleontologist can never demonstrate a discontinuity, because he must always entertain the suspicion that it may arise from lack of evidence. The zoologist and experimentalist, on the other hand, may demonstrate a discontinuity for the reason that they may have at hand large series of contemporary individuals for comparison. The paleontologist is never favored in this way.

Second, the paleontologist can never prove that the appearance of a new type combining a *number* of adaptive characters is a sudden appearance, because he must always admit the possibility that such a type may have slowly evolved elsewhere and come into the field of his observation suddenly through migration. Thus I maintain that the hypothesis of Cope, Dollo and Smith Woodward as to the sudden appearance either of new adaptive characters or of new types does not rest on a demonstrable foundation so far as paleontology is concerned.

These criticisms, however, in no way invalidate the numerous observations of these and other paleontologists, among both vertebrates and invertebrates, that new adaptive types do suddenly come into the field of observation in geological horizons and mark the beginnings of rapid evolution.

Finally, I am neither opposing nor advocating the so-called "mutation theory" of de Vries; I simply assert that paleontology is not a branch of biology in which this theory can be either proved or disproved.

Second opinion: *Hypothesis of gradual appearance.* So far as I know, this hypothesis is solely paleontological in origin, and is to-day chiefly maintained by certain, although not by all, paleontologists. I regard it as the greatest contribution which paleontology has made to evolution. So far as I know, the first to express it was Waagen (1869). He distinguished *mutations* from local or geographical variations. The mutations of Waagen can only be observed in successive geological levels, *i. e.*, at intervals of many years. They are very constant, although seen in minute features, and can always be recognized again. This was Waagen's original definition of mutations as distinguished from the more conspicuous contemporary fluctuations.

This law of Waagen received the powerful support of Neumayr (1889) and of many other invertebrate paleontologists, and it is receiving fuller support daily. As regards the vertebrates, Osborn in 1886, at the time ignorant of Waagen's law, made the same observation in the study of the teeth of mammals, and termed it the law of "definite variation." It has since been confirmed and extended on a very large scale. Thus vertebrate and invertebrate paleontologists working entirely independently of each other on wholly different materials have reached similar opinions. This *law of gradual change in the origin of single characters, measurable only at long intervals of time, rests on a vast number of observations.*

III. The Adaptive Quality of New Characters

So much for the older history of the subject.

I may now, as a paleontologist, add *three* supplementary statements as to the origin of new characters by heredity which, it is true, rest upon a large number of my own observations, but still requires collat-

eral evidence and further examination by others.

First: *That such origins are adaptive in direction from the beginning.* The cusps of the teeth of mammals offer a peculiarly advantageous field of observation because they are born complete, and, unlike most other organs of the body, they do not depend upon ontogeny for their perfection, in fact, ontogeny and environment destroy rather than perfect them. In seventeen orders of mammals, in thousands of species, and in millions of individuals, a very limited number of similar cusps rise in the teeth; the number is eleven in all. So far as observed: (1) they rise independently, (2) they rise gradually, (3) they rise adaptively; hence I have termed them "rectigradations," *i. e.,* rising continuously, orthogenetically, in definite or straight lines, and finally reaching a condition in which they may be considered adaptive. This phenomenon I first observed in the teeth and later in the origin of horns.

Second: *That such origins are predetermined by hereditary kinship.* This statement, or rather hypothesis, is supported by observations of two kinds. Without interbreeding, animals of similar kinship, near or remote, in different parts of the world originate independently similar characters. For example, the Eocene Equidæ evolved the same cusps in the grinding teeth simultaneously in Switzerland and in the American Rocky Mountain region.

This example has to meet the criticisms, (1) by the paleontologist Depéret, that this is not an independent evolution, but that these rectigradations *are* due to actual community of descent brought about by migration and interbreeding; (2) there is the older criticism of the selectionists, that these similarities are due to the similar action of natural selection working upon fortuitous variations in different regions.

Neither of these explanations is tenable, in my opinion.

Third: *This predetermination is due to a similarity of hereditary potential.* That is to say that animals of similar kinship do not continuously evolve in certain directions, but merely transmit a similar potentiality in the origin of new characters. This both renders possible the occurrence of certain characters and conditions or limits these characters when they do occur. For example, in a certain series of extinct mammals we can predict where a new cusp will arise before its actual occurrence.

As to these three propositions, which are enormously important, if true, we make six notes.

We note (1) that only through some restraining or limiting law of this kind can we explain the marvelous uniformity in the fundamental structure of the teeth of mammals which has now been observed in all orders of mammals except four.

We note (2) that this is not identical with the internal perfecting tendency of Nägeli, because under the law of the four inseparable factors, it operates, in a manner adaptive to new conditions which is entirely incomprehensive to us. Thus, for example, if a primate (a monkey or lemur) begins to imitate the habits of an ungulate by becoming herbivorous, it also begins to acquire the dental cusps of an ungulate in about the same order as these cusps would arise in an ungulate; thus some of the Eocene monkeys so closely paralleled the Eocene ungulates in dental structure that they were at first placed in the order Ungulata.

We note (3) that the kinetogenesis or Neo-Lamarckian theory of Cope and Ryder apparently fails (as pointed out by Poulton), *especially* when applied to the teeth, because the teeth appear through the gums fully formed and are not modified or im-

proved by use, but, on the contrary, are destroyed by use.

We note (4) that there appears to be an analogy between heredity and ontogeny. This hurrying up or acceleration of characters in heredity parallels the acceleration of useful characters in ontogeny. In other words, from unknown causes (even if the Lamarckian inheritance is admitted) characters are accelerated (hastened) or retarded (slowed up) in development, according to the needs of the animal. Thus there arises this most interesting analogy between the hereditary origin of new characters and the subsequent ontogenetic history of characters after they have reached a presumably adaptive condition. In other words, just as the lateral digits of the horse are retarded and the median digits are accelerated, so the origin from unknown causes of new characters is accelerated or retarded, according to the needs of the animal. For example, a postero-internal cusp of the upper grinding teeth, known as the hypocone, and the intermediate cusps, known as the conules, are retarded in hereditary origin in insectivores and in frugivorous animals; they are accelerated in origin in herbivorous animals.

We note (5) that our failure to see any reasons or causes of these timely hereditary origins of new characters has no bearing whatever on the fact of the existence of such origins, *that fact is a matter of direct observation independent of hypothesis.* For my own part I have for many years (ever since I observed this fact and recognized all the difficulties in the Lamarckian explanation) stood as a complete agnostic as to the cause of such origins. I now repeat that we have no conceivable explanation at present.

We note (6) that an important distinction must now be made, namely, that such origins of new characters are chiefly numerical; something is added to the organism which did not exist before, the rudiment of a cusp, or the rudiment of a horn. The changes of form of proportion and of modeling, follow after.

A very interesting thought has just come to me during the preparation of this paper, a paper which summarizes the conclusions I have been gradually forming in the last twenty-one years. The thought is this: That *theoretically there is no conflict between the hypotheses of continuity and discontinuity.* If there does exist hereditary predisposition to evolve in a definite direction, it may manifest itself suddenly, as a saltation, or a "mutation of de Vries," or very gradually as a rectigradation, or "mutation of Waagen."

SUMMARY

The following facts are those which are put forth through paleontological observation, for verification by others:

1. That many origins of new characters are through some internal action in heredity.

2. That many important adaptive characters arise determinately, definitely, but by extremely slow stages.

3. That degrees of similarity in such origins correspond with degrees of kinship.

4. That degrees of kinship also affect to a certain extent, but not absolutely the time of appearance, or the time of the origin, or the rate of evolution.

5. That such origins find expression not spontaneously, or irrespective of conditions, or from purely internal mechanical causes, but through some entirely unknown and at present inconceivable relation to ontogeny (habit and use), to environment (external conditions), and to selection.

6. That if such origins do spring from internal hereditary principles, as they appear to do in many cases, slow origins (mutations of de Vries) may be simply

due to the same law operating with a different velocity.

HENRY FAIRFIELD OSBORN

SCIENTIFIC BOOKS

Temperatur und Zustand des Erdinnern—eine Zusammenstellung und kritische Beleuchtung aller Hypothesen. Von HERRMANN THIENE. Jena, Fischer. 1907. Pp. 107. Price two and a half marks.

This useful paper is the result of a prize offered by the Jena philosophical faculty for a critical review of the literature and theories as to the temperature and state (solid, fluid or gaseous) of the earth's interior—a useful undertaking since the literature is much scattered. Astronomers, mathematicians and physicists as well as geologists, have contributed to it. The conclusions of the author, an assistant in the Jena Mineralogical Institute, are that the earth has an outer crust, of the composition of diorite, and an iron core. The surface density is about 2.8; the mean density is between 5.4 and 5.7. The density at the center according to Stieljes must be between 7 and 12.16, having due regard to all the facts, including the moment of inertia (resistance to change in its axis of rotation and the effect of the attraction of the sun and moon on the equatorial bulge) and the difference of gravity at pole and equator. Thiene does not describe the methods, but the results and assumptions merely of the different writers. The reviewer would note that our knowledge of the density must be the more inaccurate the nearer the center since the density of any ellipsoidal layer has less and less importance and effect either on the mass or the moment the nearer the center respectively the axis of rotation it is. Laplace's law of density is shown by the author to agree with the known facts. But any law in which the constants were so taken as to be consistent with the known data would, if expressed and expanded in a series in which the density is a function of the ratio of the distance from the surface of the earth to its radius, according to Maclauvin's theorem, reduce to Laplace's law for the first two terms.

It is obvious, though Thiene does not remark it, that other things being equal the law of density will be different and the densities at the center less if the temperature keeps on increasing clear to the center than if it increases less rapidly or attains a maximum.

Thiene leans to the view that the temperature increases toward the center more and more slowly from a rate of something like 1° C. in thirty-three meters to begin with so that the greatest heat reached is probably from 2,000° C. to 10,000° C. He is not aware of the arguments of See and Chamberlin for an increase in temperature clear to the center and a possible increase in the gradient. He would attribute the heat to the original warmth of condensation. The Kant-Laplace theory is taken as established.

The interior he believes a plastic crystalline (anisotropic) solid mass, which would, however, turn into a fluid or possibly a gas were the pressure removed.

A list of references at the end and an alphabetical list of authors are valuable additions and enable one to grasp the scope of the work which seems fairly full for Germany. An American can hardly think that the hope of the author that nothing essential has been overlooked is fulfilled. He mentions the metallic interior without mentioning Durocher. He could not, of course, have had access to so recent a work as Chamberlin and Salisbury's geology, but many of the thoughts therein collected have appeared in the *Journal of Geology,* to which he seems also not to have had access.[1] He discusses and turns down theories of a gaseous interior without mentioning See. And by the way he does not note that a temperature of 10,000° C., together with the theory of an iron core favored by him, and the critical temperature of iron and platinum which he cites, from 5,000° to 7,000°, would needs imply the possibility of a gaseous center.

The bearing of theories of isostasy is but mentioned. Neither Dutton nor Gilbert's work with Putnam is mentioned nor that with Woodward, only one of the least important of whose papers is cited. To the

[1] Other writers too recent to be mentioned are Hayford, Gregory, and those cited by Love in SCIENCE, N. S., Vol. XXVI., No. 669, Oct. 25, 1907.