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The business being finished, the Society listened to the annual discussion, which is printed below.

At the close of the discussion, on motion of Prof. Heilprin, it was voted that a committee of three be appointed by the President to inquire into the practicability and feasibility of the exploration of the Antarctic Continent and to report at the next meeting of the Society. The President appointed Professors Heilprin, Osborn and Goodale. The Society then adjourned.

H. C. BUMPUS,
Secretary.

THE FORMULATION OF THE NATURAL SCIENCES.*

FORMULATION is the method of presentation of the forms of our thoughts. Our observations of the facts of material nature are embodied in such classifications as we think best express their relations, and by means of these classifications expressed in language, we convey to others our conclusions in the premises. As the vehicle of presentation, formulation is one of the aspects of language, which as the medium of communication between men, enables them to accumulate knowledge. It is highly important then that the system of formulation should be uniform, so as to convey definite meaning and preserve the truth. The vast number of facts to be marshaled in orderly

array, which constitute the natural sciences, require a correspondingly complex and exact formulation. The advent of the doctrine of evolution into the organic sciences involves the necessity of making such readjustments of our method of formulation as may be called for. It is with reference to this condition and the present action of naturalists regarding it, that I address you to-day. The subject may be considered under the three heads of Taxonomy, Phylogeny, and Nomenclature.

I. TAXONOMY.

Taxonomy or classification is an orderly record of the structural characters of organic beings. The order observed is an order of values of these characters. Thus we have what we call specific or species value, generic value, family value, and so on. These values are not imaginary or artificial, as some would have us believe, but they are found in nature. Their recognition by the naturalist is a matter of experience, and the expression of them is a question of tact. Their recognition rests on a knowledge of morphology, or the knowledge of true identities and differences of the parts of which organic beings are composed. The formulation of these values in classification foreshadows the evolutionary explanation of their origin, and is always the first step necessary to the discovery of a phylogeny.

Taxonomy, then, is, and always has been, an arranging of organic beings in the order of their evolution. This accounts for the independence of the values of taxonomic characters, of any other test. Thus, no character can be alleged to be of high value because it has a physiological value, or because it has no physiological value. A physiological character may or may not have a taxonomic value. The practical taxonomist finds a different test of values, which is this. He first endeavors to dis-

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cover the series of organic forms which he studies. He learns the difference between its beginning and its ending. His natural divisions are the steps or stages which separate the one extremity from the other. The series may be greater or they may be lesser, *i. e.*, more or less comprehensive, and it is to the series of different grades that we give the different names of the genus, family, order, etc.

We know that the characters of specific value in given cases are usually more numerous than those of higher groups. We know that they are matters of proportions, dimensions, textures, patterns, colors, etc., which are many. The characters of the higher groups, on the contrary, are what we call structural, *i. e.*, the presence, absence, separation or fusion of elemental parts, as estimated by a common morphologic standard; and it is the business of the morphologist to determine each case on this basis. In these characters lies the key to the larger evolution, that of the higher aggregations of living things. On the contrary, the study of the origin of species characters gives us the evolution of species within the genus, but nothing more, except by inference.

Classification, then, is a record of characters, arranged according to their values. There still lingers, in some quarters, a different opinion. This holds that there is such a thing as a 'natural system,' as contrasted with 'an anatomical system.' Examination shows that the supporters of this view suppose that there is some bond of affinity between certain living beings which is not expressed in anatomical characters. A general resemblance apparent to the eye is valued by them more highly than a structural character. If this 'general appearance' is analyzed, however, it is found to be simply an aggregate of characters usually of the species type, which by no means precludes the presence of anatomical differences. And these anatomical differences

may indicate little relationship, in spite of the general resemblance of the species concerned, or they may have only the smallest value attached to such characters, *i. e.*, the generic. It is with regard to the generic characters that the chief difference of practice exists. But it is clear that the record of this grade of characters cannot be modified by questions of specific characters. The two questions are distinct. Both represent nature, and must be formulated. In fact, I have long since pointed out that the same species, so far as species characters go, may have different generic characters in different regions. Also that allied species of different genera may have more specific characters in common than remote species of the same genus.

The anticipation naturally intrudes itself that the characters which distinguish the steps in a single evolutionary or genealogical line must disappear with discovery, and new ones appear, and that they must be all variable at certain geological periods, and hence must become valueless as taxonomic criteria. And it is therefore concluded that our systematic edifice must lose precision and become a shadow rather than a reality. I think that as a matter of fact this will not be the result, and for the following reasons: In the first place, when, say all the generic forms of a genealogical line, shall have been discovered, we will find that each one of them will differ from its neighbor in one character only. This naturally follows from the fact that two characters rarely, if ever, appear and disappear contemporaneously. Hence, generic characters will not be drawn up so as to include several points. For a while, there will be found to be combinations of two or three characters which will serve as definitions, but discovery will relegate them to a genus each. Each of these characters will be found to have what I have called the 'expression point,' or the moment of completeness, before which it

cannot be said to exist. In illustration I cite the case of the eruption of a tooth. Before it passes the line of the alveolus it is not in use; it is not in place as an adult organism. When it passes that line it has become mature, has reached its expression point, comes into functional use, and may be counted as a character. Such will be found to be the case with all separate parts; there always will be a time when they are not completed and then there will be a time when they are. These lines, then, will always remain as our boundaries, as they are now, for all natural divisions from the generic upwards. This condition cannot exist in characters of proportionate dimensions, which will necessarily exhibit complete transitions in evolution. Hence, proportions alone can only be used ultimately as specific characters.

Some systematists desire to regard phyletic series as the only natural divisions. This may be the ultimate outcome of paleontologic discovery, but at present such a practice seems to me to be premature. In the first place, as all natural divisions rest on characters, we must continue to depend on their indications, no matter whether the result gives us phyletic series or not. In the next place, we must remember that we have in every country interruptions in the sequence of the geological formations, which will give us structural breaks until they are filled. There are also periods when organic remains were not preserved; these also will give us interruptions in our series. So we shall have to adhere to our customary method without regard to theory, and if the phyletic idea is correct, as I believe it to be, it will appear in the final result, and at some future time.

Authors are frequently careless in their definitions. Very often they include, in the definition of the order, characters which belong in that of the family, and in that of the family those that belong in the genus.

Characters of different values are thus mixed. The tendency, especially with naturalists who have only studied limited groups, is to overestimate the importance of characters. Thus the tendency is to propose too many genera and other divisions of the higher grades. In some groups structure has been lost sight of altogether, and color patterns, dimensions, and even geographical range, treated as characters of genera. As the mass of knowledge increases, however, the necessity for precision will become so pressing that this kind of formulation will be discarded, and definitions which mean something will be employed. Search will be made especially for that one character which the nature of the series renders it probable will survive, as discoveries of intermediate forms are successively made, and here the tact and precision of the taxonomist has the opportunity for exercise. In the selection of these characters, one problem will occasionally present itself. The sexes of the same species sometimes display great disparity of developmental status, sometimes the male, but more frequently the female, remaining in a relatively immature stage, or in others presenting an extraordinary degeneracy. In these cases the sex that displays what one might call the genius, or in other words, the tendency, of the entire group, will furnish the definitions. This will generally be that one which displays the most numerous characters. In both the cases mentioned the male will furnish these rather than the female; but in a few cases the female furnishes them. The fact that both sexes do not present them does not invalidate them, any more than the possession of distinct reproductive systems would refer the sexes to different natural divisions.

I have seen characters objected to as of little value because they were absent or inconstant in the young. I only mention

the objection to show how superficially the subject of taxonomy may be treated. So that a character is constant in the adult, the time of its appearance in development is immaterial in a taxonomic sense, though it may have important phylogenetic significance.

II. PHYLOGENY.

The formulation of a phylogeny or genealogy involves, as a preliminary, a clear taxonomy. I refer to hypothetical phylogenies, such as those which we can at present construct are in large part. A perfect phylogeny would be a clear taxonomy in itself, so far as it should go, did we possess one; and such we may hope to have ere long, as a result of paleontological research. But so long as we can only supply parts of our phyletic trees from actual knowledge, we must depend on a clear analysis of structure as set forth in a satisfactory taxonomy, such as I have defined above.

Confusion in taxonomy necessarily introduces confusion into phylogeny. Confusion of ideas is even more apparent in the work of phylogenists than in that of the taxonomists, because a new but allied element enters into the formulation. It is in the highest degree important for the phylogenist, whether he be constructing a genealogic tree himself or endeavoring to read that constructed by some one else, to be clear as to just what it is of which he is tracing the descent. Is he tracing the descent of species from each other, or of genera from each other, or of orders from each other, or what? When I trace the phylogeny of the horse, unless I specify, it cannot be known whether I am tracing that of the species *Equus caballus*, or that of the genus *Equus*, or that of the family Equidæ. When one is tracing the phylogeny of species, he is tracing the descent of the numerous characters which define a species. This is a complex problem, and but little progress has been made in it from

the paleontologic point of view. Something has been done with regard to the descent of some living species from each other. But when we are considering the descent of a genus, we restrict ourselves to a much more simple problem, *i. e.*, the descent of the few simple characters that distinguish the genus from other genera. Hence, we have made much more progress in this kind of phylogeny than with that of species, especially from the paleontologic point of view. The problem is simplified as we rise to still higher divisions, *i. e.*, to the investigation of the origin of the characters which define them. We can positively affirm many things now as to the origin of particular families and orders, especially among the Mammalia, where the field has been better explored than elsewhere.

It is in this field that the unaccustomed hand is often seen. Supposing some phyletic tree alleges that such and such has been the line of descent of such and such orders or families, as the case may be; soon a critic appears who says that this or that point is clearly incorrect, and gives his reasons. These reasons are that there is some want of correspondence of generic characters between the genera of the, say, two families alleged to be phyletically related. And this want of correspondence is supposed to invalidate the allegation of phyletic relation between the families. But here is a case of irrelevancy; a generic character cannot be introduced in a comparison of family characters. In the case selected, the condition is to be explained by the fact that although the families are phyletically related, one or both of the two juxtaposed genera through which the transition was accomplished has or have not been discovered. The same objection may be made against an allegation of descent of some genus from another, because the phyletic relation between the known species of the two genera cannot be demonstrated. I

cite as an example the two genera, *Hippotherium* and *Equus*, of which the latter has been asserted with good reason to have descended from the former. It has been shown, however, that the *Equus caballus* could not have descended from the European *Hippotherium mediterraneum*, and hence some writers have jumped to the conclusion that the alleged phyletic relation of the two genera does not exist. The reasons for denying this descent are, however, presented by specific characters only, and the generic characters are in no way affected. Further, we know several species of *Hippotherium* which could have given origin to the *Equus caballus*, probably through intermediate species of *Equus*.

Some naturalists are very uncritical in criticising phylogenies in the manner I have just described. They often neglect to ascertain the definitions given by an author to a group alleged by him to be ancestral; but fitting to it some definition of their own, proceed to state that the ancestral position assigned to it cannot be correct, and to propose some new division to take its place. It is necessary to examine, in such cases, whether the new group so proposed is not really included in the definition of the old one which is discarded.

The fact that existing genera, families, etc., are contemporary need not invalidate their phyletic relation. Group No. 1 must have been contemporary with group No. 2, at the time that it gave origin to the latter, and frequently, though always, a certain number of representatives of group No. 1 have not changed, but have persisted to later periods. Some genera, as *e. g.*, *Crocodylus*, have given origin to other genera (*i. e.*, *Diplocynodon*) and have outlasted it, for the latter genus is now extinct. The lung fishes, *Ceratodus*, are probably ancestral to the *Lepidosirens*, but both exist to-day. Series of genera, clearly phyletic, or *Batrachia Salientia*, are contemporaries. Of

course we expect that the paleontologic record will show that their appearance in time has been successive. But many ancestors are living at the same modern period as their descendants, though not always in the same geographic region.

III. NOMENCLATURE.

Nomenclature is like pens, ink and paper; it is not science, but it is essential to the pursuit of science. It is, of course, for convenience that we use it, but it does not follow from that that every kind of use of it is convenient. It is a rather common form of apology for misuse of it to state that as it is a matter of convenience, it makes no difference how many or how few names we recognize or use. An illustration of this bad method is the practice of subdividing a genus of many species into many genera, simply because it has many species. The author who does this ignores the fact that a genus has a definite value, no matter whether it has one or five hundred species. I do not mean to maintain that the genus or any other value has an absolute fixity in all cases. They undoubtedly grade into each other at particular places in the system, but these cases must be judged on their own merits. In general there is no such gradation.

Nomenclature is then orderly because the things named have definite relations which it is the business of taxonomy, and nomenclature its spokesman, to state. Here we have a fixed basis of procedure. In order to reach entire fixity, a rule which decides between rival names for the same thing is in force. This is the natural and rational law of priority. With the exception of some conservative botanists, all naturalists are, so far as I am aware, in the habit of observing this rule. The result of a failure to do so is self-evident. There is, however, some difference of opinion as to what constitutes priority. Some of the aspects of

the problem are simple, others more difficult. Thus there is little or no difference of opinion as to the rule that the name of a species is the first binomial which it received. This is not a single date for all species, since some early authors who used trinomials and polynomials occasionally used binomials. A second rule which is found in all the codes, is that a name in order to be a candidate for adoption, must be accompanied by a descriptive diagnosis or a plate. As divisions above species cannot be defined by a plate, a description is essential in every such case.

It is on the question of description that a certain amount of difference of opinion exists. From the codes of the associations for the advancement of science, and of the zoölogical congresses, no difference of opinion can be inferred, but the practice of a number of naturalists both zoölogists and paleontologists in America, and paleontologists in Europe, is not in accord with the rule requiring definition of all groups above species. It has always appeared to me remarkable that a rule of such self-evident necessity should not meet with universal adoption. However, the objections to it, such as they are, I will briefly consider. It is alleged that the definitions when first given are more or less imperfect, and have to be subsequently amended, hence it is argued they have no authority. However, the first definitions, if drawn up with reference to the principles enumerated in the first part of this address, need not be imperfect. Also an old-time diagnosis of a division which we have subsequently found it necessary to divide, is not imperfect on that account alone, but it may be and often is the definition of a higher group. But you are familiar with all this class of objections and the answers to them, so I will refer only to the positive reasons which have induced the majority of naturalists to adhere to the rule.

It is self-evident that so soon as we abandon definitions for words, we have left science and have gone into a kind of literature. In pursuing such a course we load ourselves with rubbish, and place ourselves in a position to have more of it placed upon us. The load of necessary names is quite sufficient, and we must have a reason for every one of them, in order to feel that it is necessary to carry it. Next, it is essential that every line of scientific writing should be intelligible. A man should be required to give a sufficient reason for everything that he does in science. Thus much on behalf of clearness and precision. There is another aspect of the case which is ethical. I am aware that some students do not think that ethical considerations should enter into scientific work. To this I answer that I do not know of any field of human labor into which ethical considerations do not necessarily enter. The reasons for sustaining the law of priority are partly ethical, for we instinctively wish to see every man credited with his own work, and not some other man. The law of priority in nomenclature goes no further in this direction than the nature of each case requires. Nomenclature may be an index of much meritorious work, or it may represent comparatively little work; but it is to the interest of all of us that it be not used to sustain a false pretence of work that has not been done at all. By insisting on this essential test of honest intentions we retain the taxonomic and phylogenetic work within the circle of a class of men who are competent to it, and cease to hold out rewards to picture makers and cataloguers.

Another contention of some of the nomenclators who use systematic names proposed without description, is, that the spelling in which they were first printed must not be corrected if they contain orthographical and typographical errors. That this view should be sustained by men

who have not had the advantage of a classical education, might not be surprising, although one would think they would prefer to avoid publicly displaying the fact, and would be willing to travel some distance in order to find some person who could help them in the matter of spelling. But when well educated men support such a doctrine, one feels that they have created out of the law of priority a fetish which they worship with a devotion quite too narrow. The form of our nomenclature being Latin, the rules of Latin orthography and grammar are as incumbent on us to observe, as are the corresponding rules of English grammar in our ordinary speech. This cult, so far as I know, exists only in the United States and among certain members of the American Ornithologists' Union. The preservation of names which their authors never defined; of names which their proposers misspelled; of names from the Greek in Greek instead of Latin form; of English hyphens in Latin composition; and of hybrid combinations of Greek and Latin, are objects hardly worth contending for. Some few authors are quite independent of rules in the use of gender terminations, but I notice the A. O. U. requires these to be printed correctly. Apart from this I notice in the second edition of their check list of North American Birds, just issued, only eighteen misspellings out of a total number of 768 specific and subspecific names, and the generic and other names accompanying. These are of course not due to ignorance on the part of the members of this body, some of whom are distinguished for scholarship, but because of an extreme view of the law of priority.

In closing I wish to utter a plea for euphony and brevity in the construction of names. In some quarters the making of such names is an unknown art. The simple and appropriate names of Linneus and Cuvier can be still duplicated if students

would look into the matter. A great number of such names can be devised by the use of significant Greek prefixes attached to substantives which may or may not have been often used. Personal names in Greek have much significance, and they are often short and euphonious. The unappropriated wealth is so great that there is really no necessity for poverty in this direction. It should be rarely necessary, for instance, to construct generic names by adding prefixes and suffixes of no meaning to a standard generic name already in use.

E. D. COPE.

*THE ORIGIN AND RELATIONS OF THE
FLORAS AND FAUNAS OF THE ANT-
ARCTIC AND ADJACENT REGIONS.**

The Geology of the Antarctic Regions. ANGELO HEILPRIN, Philadelphia Academy of Natural Sciences.

Reviewing our present knowledge of the Antarctic regions, Prof. Heilprin stated that it rests almost where it was a half-century ago, when Sir James Clark Ross (1841, 1842) made his memorable cruises in the 'Erebus' and 'Terror,' and attained the high southing of 78° 10'. This was at a position almost due south of New Zealand, along a coast line, sharply defined by elevated mountain masses, to which the daring British navigator gave the name of Victoria Land. At that time other patches of ice bound land, or what was presumed to be land, had already been discovered and named by Bellamy, Biscoe, Dumont d'Urville, and Wilkes—such as Clarie Land, Sabrina Land, etc., south of the Australian continent; Enderby Land, Kemp Land, Graham and Alexander Lands, south of Patagonia—and from these had been constituted the Antarctic continent of Wilkes and of many modern geographers. Murray,

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