

features as nearly as the modification will permit.

It is to my mind impossible to find any support for a theory of evolution by minute changes from the study of anatomical variations. I should not venture to say, on the other hand, that they give any direct support to the theory of mutation; but at least they are not in disaccord with it.

Systematic Work and Evolution: L. H.

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Every object of which we take cognizance must be named if we are to record and convey the ideas associated with it. As the names accumulate, it is necessary that we group them, or provide some scheme of arrangement. We classify all categories, even though we do no more than to arrange them alphabetically. Nomenclature and classification are primary intellectual processes.

The number of organisms that we know has come to be legion. These organisms are described in books. The first descriptions accepted the organisms as they are, without serious inquiry of their origins. Definite names have come to be attached to each kind of organism and definite customs have arisen to control the bestowal of the names. Biological nomenclature has become a rigid bibliographical method.

The first object of classification was to afford a perspicuous arrangement of facts. The facts must be pigeon-holed, else they may be lost. Gradually, however, the idea of relationship between the objects has developed, and these ideas have expressed themselves in crystallized schemes of classification. That is to say, classification of organisms is a combination and compromise of bibliographical method and expression of relationships.

Presently, the organisms themselves began to be studied from the physiological

side. It was discovered that at least some of the named groups of organisms are not entities. There are all grades of differences, from those peculiar to one individual to those peculiar to many individuals, and to groups of individuals. The organisms are multifarious and elastic, but nomenclatorial and taxonomic systems are editorial and arbitrary.

We are all now committed to the evolution philosophy as a working hypothesis. The greatest problems in the study of organic nature are the determining of the lines of ascent and the means by which they have come about. We study plastic material; at the same time we are making a desperate effort, at least on the botanical side, towards rigidity of nomenclature. Our ideas of what constitutes species and varieties are free and extensible enough, but our methods of designating these ideas still follow the formalism of a century ago—are in fact more inflexible than they were in the time of Linnæus. If nomenclature is inelastic, schemes of classification within the genus or species must likewise be inelastic, for the classification is but an expression of our ideas of the relationships of the objects that we name. Our nomenclature does not express either the knowledge or the point of view of our time.

The Present Status of Systematic Work.—There are three elements in the discussion of systematic work as related to transmutation theories: (1) The idea of a species, (2) the methods of naming and recording, (3) the classificatory schemes themselves.

It would be profitless at this time to enter into a disquisition as to what a species is. The many discussions of this subject are so many admissions that no one knows. The only point I care now to make is that we all recognize the fact that the single word 'species' covers groups of widely different grades of value, of differentiation,

and of evolutionary development. This fact has been brought forcibly to our attention again by the stimulating work of de Vries. There are collective species, elementary species and other grades. Our formal nomenclature in practise recognizes only two grades—'species' and 'variety,' with no two persons agreeing which is one or the other. If there are such differing grades in nature, then we must accept the fact and adopt new technical words for the various grades. This has not been done, at least not in practise, because we have not yet sufficiently clear ideas to enable us to do so. These varying grades of species and varieties are the results of processes of evolution, and some, if not all, of these processes are still in operation. Therefore, the new definitions of species-concepts must rest on physiological or functional grounds, not merely on morphological and anatomical grounds.

Many of us feel that the present methods of nomenclature and description will be outgrown, for these methods are made for the herbarium and the museum, rather than for the field. It is a most suggestive commentary that the botanist may know the 'species' when it is glued on an herbarium sheet, but may not know it when growing. The nurseryman or gardener may know it when growing, but not when it is in a herbarium. This is not merely because the botanist is unfamiliar with the field, or the gardener unfamiliar with the herbarium; these men have different fundamental conceptions of what a species is; they use different 'marks,' one morphological, the other largely physiological. I believe that the gardener is nearer the truth. I recall a characteristic remark made by my master, Sereno Watson, when, in the confidence of youth, I asked whether a certain binomial would be accepted a hundred years from now. He shrugged his shoul-

ders and said quietly, 'I don't know; they may call plants by numbers then.'

I have no intention of proposing any new plan of nomenclature—that would only amuse you. I merely feel, as you do, that a change is imminent. Perhaps we shall hold to our main species-groups for history's sake, and then designate minor groups in terms of their qualities. If we find it to be true that there are fluctuating varieties and mutations of differing geneses, then we must assuredly represent these facts in nomenclature and taxonomy. Very likely we shall adopt a scheme wholly different from the current binomial plan for designating one or the other, or perhaps both. We may adopt quantitative names—having determined the main lines of differentiation, may express each variation in names of more or less. I look for some such method to result from the statistical quantitative study of variation. Let me draw an illustration also from plant-breeding practise. The horticulturist and the agriculturist have been holding to the formal or conventional idea of 'variety.' We will suppose that the farmers of a region have grown Jones's Giant White Corn. They have bought and sold and planted this name. They have fed it to the pigs; and the pigs may have thrived or may not, according as the corn contained much or little food value. The name is of no value to the pigs; and, in fact, it is of no real value to the farmer unless it is a guarantee of some particular excellence. Now, the name Jones's Giant White designates corn of certain color and shape of ear and of kernel—features which really mean nothing to the farmer, whereas the starch-content or the protein-content may mean everything. The new plant-breeding does not try to produce a new 'variety' so much as a series of generations that shall have greater efficiency. We shall have, perhaps, fifteen per cent. protein corn, or seventy-

five per cent. starch corn. The name will be of no particular consequence.

If organisms are to be recognized on their merits, then we must cease to class some forms as 'natural' and others as 'artificial.' In the future, the products of the breeder and the plants of the garden are to find their rightful place in systematic plans. They illustrate processes of evolution; and if these processes are hastened by man, the products are all the more worthy of consideration in man-made schemes. The old-time distinction between native forms and domestic forms is arbitrary, unnecessary and pernicious. All animals are animals and all plants are plants.

If we are to designate minor groups or differences in terms of their real qualities, you will still ask how it can be done as a matter of practise, how we shall be able quickly and clearly to determine what particular animal or plant we have in hand. This is really a part of the problem—how to express our ideas without confusion. In the first place, I should say that the change in point of view will come slowly and we shall work out the means as we proceed. I desire only to suggest the direction in which progress seems to lie. In the second place, I should say that in the future we may care less for merely naming a thing than we have in the past—perhaps our formal nomenclature may well stop with characters that are gross and evident. In the third place—and this is the real crux of the matter—I should say that formal nomenclature must never stand in the way of our expressing the full truth about organisms. At best, nomenclature is a makeshift. It is a secondary consideration. If this statement is not accepted, then the only alternative is to say that systems of nomenclature and classification belong to one realm and that biological studies belong to another, and that, therefore, these systems can not be expected to conform to our

expanding knowledge. This position would be untenable from the fact that classification is always re-adapting and re-shaping itself to our changing points of view; and nomenclature can not be wholly divorced from taxonomy. Taxonomy represents a progressive effort; nomenclature a conservative effort. Our current phytographic and zoographic methods do not allow us to express our ideas of species.

Every systematist knows how unsatisfactory the mere 'determining' of species is. It consists mainly in matching certain arbitrary characters or marks with similar marks of specimens in the 'collection.' We may have no knowledge whether these marks have any significance in the physiology or phylogeny of the species, that is, whether they are really of any biological value. In theory, we try to hold the systematist to what we call consistency in the determining of species; but as a matter of fact the systematist is constantly changing his mind as to the values of diagnostic marks—and herein, it seems to me, lies the safety of systematic work. A few years ago a botanist sent me a plant to name. When I had returned the name he upbraided me by saying that he had sent me the identical plant the year before and I had then given it another name. I replied that it was his own fault, for he had no business to send me the specimen twice.

The question really comes to this—Shall we know two kinds of species, one of taxonomy and one of biology? If so, then it is scarcely worth while to try to construct any scheme of taxonomy that shall endeavor to express our latest ideas of the ascent of organisms, for a scheme of classification for formal species is needed only for the purpose of ready reference. Comstock has stated the question well in the following paragraphs:*

* 'Evolution and Taxonomy,' Wilder Quarter-Century Book, pp. 44 and 45.

There will also arise, I believe, in a work of this kind, a necessity for distinguishing between the essential characters of a group and those characters which are used by the systematist merely to enable students to recognize members of the group. For it seems to me that the essential characters of a group of organisms do not lie necessarily in the presence or absence of any structure or structures, or in the form or any part or parts of the body of the living members of the group; but rather in the characteristic structure of the progenitor of the group, and in the direction of specialization of the descendants of this progenitor.

The recognition-characters are those usually first observed by the investigator, and are those commonly given in taxonomic works. In many cases these recognition-characters are also essential characters, especially in the case of groups that have been thoroughly studied. But by the taxonomic methods now commonly used, search is chiefly made for recognition-characters. The more skilled the systematist the more likely is he to discover and use as recognition-characters those that are really essential, although the distinction pointed out here may not be recognized by him.

Very likely we shall not abolish the present systems of nomenclature and description in the larger units, but we shall modify and extend them. We shall break away from the old lines of cleavage. We shall learn what marks that are correlated with function can be used as expedient diagnostic characters. We shall make an increasing effort to use absolute characters, not merely relative and comparative ones. We ought to make the 'type' of the species, the real biological or phylogenetic type, not cling merely to the 'original' specimen that chanced first to be named. What we now call 'types' may be wholly unusual and even non-significant forms. If the book or literary type is in time to be the real type, then we shall re-group our species-units, and this will be the greatest possible gain.

If we decide that literary-species must come, in the future, to correspond to the physiological or elementary species, then we may hope to express the direction of evolution fairly well in our taxonomic schemes. These taxonomic schemes must

proceed centrifugally and dichotomously rather than lineally. They must arrange about foci. I wish to quote again from Comstock:

If the history of a group be worked out in the manner indicated, the student will feel the need of recording his results in such a way as to indicate the phylogeny of the divisions of the group. But as the necessities of book-making require a linear arrangement of descriptions, this is somewhat difficult; for the natural sequence of groups should be represented by constantly branching lines rather than by a single straight line.

It seems to me that the most practicable way of meeting this difficulty is to begin with the description of the most generalized form known, and to follow this with descriptions of forms representing a single line of development, passing successively to more and more specialized forms included in this line. When the treatment of one line of development has been completed, take up another line, beginning with the most generalized member of that line and clearly indicating in the text that a new start has been made.

In making the foregoing suggestions I am well aware that I have not devised any definite nomenclatorial or taxonomic schemes by which they can be carried out. I doubt whether it is worth while to devise any schemes. We need only to establish a few principles and to look upon the present methods as temporary, allowing new methods to grow as our ideas grow. There can be no finality in such schemes or systems. We have lately seen a vigorous revival of the effort towards 'stability' of nomenclature; but nomenclature is only a bit of language, and language can never be stable if it is vital. It was the old idea that systematic work is for the purpose of making record; it is the new idea that it is for the purpose of expressing the meaning of the organic creation.

Ethology and the Mutation Theory: WILLIAM MORTON WHEELER, Curator of Invertebrate Zoology, American Museum of Natural History.

"The mutation theory," as we learn