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THE TAXONOMIC OUTLOOK IN ZOOLOGY[1]

By W. T. CALMAN, D.Sc., F.R.S.

THE selection of a systematic zoologist for the honor of addressing you from this chair implies a belief that systematic zoology may have something to say that will not be without interest to those whose studies lie in other fields. I am not sure how far this belief is generally shared. The anatomist, the physiologist, the field naturalist, the student of one or other of the innumerable specializations of biological science, has always been inclined to regard with distaste, if not with contempt, the work of those whose business it is to denominate, classify and catalogue the infinite variety of living things. The systematist is generally supposed to be a narrow specialist, concerned with the trivial and superficial distinctions between the members of some narrow group of organisms which he studies in the spirit of a stamp collector; happy when he can describe a new species, triumphant if he can find an excuse for giving a fresh name to an old one.

[1] Address of the president of Section D—Zoology, British Association for the Advancement of Science, Bristol, September, 1930.

It would be idle to deny the truth that there is in these criticisms, just as it would be easy, although unprofitable, to point out that the substance of them might be directed against the practice of most other branches of research. The specialist, of whatever kind, has a tendency to mistake the means for the end, to become fascinated by technique and to suffer from a myopia that blurs his vision of other fields than his own.

I think, however, that there are some signs of an increasing appreciation of the usefulness and even of the scientific value of taxonomy among the younger generation of zoologists. More particularly, those who are concerned with the applications of zoology to practical affairs are, for the most part, although not invariably, aware of the need for exact identification of the animals they deal with. They do not always realize the difficulties that may stand in the way of this identification. It is a common experience with us at the Natural History Museum to have some mangled fragments of an animal brought in by a

practical man who expects to be supplied with the name of it while he waits. I am afraid that he often goes away with a low opinion of our competence.

It may not be without interest, therefore, if I attempt, in the first place, to give some idea of how matters stand with this part of the systematists' task, the identification and description of the species of living animals.

When Linnaeus published in 1758 the first volume of the tenth edition of his "Systema Naturæ" he named and described about 4,370 species of animals. If we ask how many are known to-day the diversity of answers we get is some indication of the confusion that exists. Some years ago, at the request of the late Sir Arthur Shipley, I endeavored to get from my colleagues at the museum estimates of the numbers of species in the various groups with which they were specially conversant. Some of the answers obtained were very interesting. With regard to mammals I was told "anything from 3,000 to 20,000 according to the view you take as to what constitutes a species." For the most part, however, the authorities consulted were unwilling to suggest even an approximate figure for a very different reason. They told me that great sections of the groups with which they were concerned were so imperfectly surveyed that it was quite impossible even to guess how many of the supposed species that had been described would survive reconsideration.

It may be worth while to consider for a little the second of the two obstacles thus indicated as standing in the way of obtaining a census of the known species of animals. In the days of Linnaeus it is likely that a very experienced zoologist might have been able to recognize at sight any one of the four thousand species of animals that were then known, and when the expansion of knowledge had made such a feat no longer possible, the specialist who confined his studies to one section of the animal kingdom could still aspire to a like familiarity with the species of his chosen group. With this kind of knowledge it is literally true that, as has been said, a systematist recognizes a new species by instinct and then proceeds to search for the characters that distinguish it. Some of the great zoologists who were still working in the British Museum when I entered it more than a quarter of a century ago, men like Albert Günther, Bowdler Sharpe, C. O. Waterhouse and Edgar Smith, had actually an amazing personal familiarity with vast sections of the animal kingdom. They had studied and digested all that had been written on their subject, and if they did not carry the whole of this knowledge in their memory, they could without searching put their hand at once on the volume that would help them. They had no need of "Keys" to help them to run down their species, indeed they rather distrusted such aids for they knew how easily they betray the heedless. Specialists of this type there must always be and we may be thankful for it. Nothing can altogether replace that instinctive perception of affinity that comes from lifelong study. It has often happened that men such as those I have named were able, when confronted with new and aberrant types of animals, to allot them at once to a place in classification which subsequent research served only to confirm. As time goes on, however, the extent of ground that can be covered in this fashion by the most industrious worker is rapidly diminishing. The torrent of publications catalogued in the *Zoological Record* increases year by year, and the specialist, if he is not to be overwhelmed by it, must not allow his curiosity to stray beyond the limits of a narrow corner of the field.

By far the greater part of this literature is written by specialists for specialists, and much of it is unintelligible to any one else. From the time of Linnaeus, however, there have not been wanting publications that have a different aim. We have monographs, synopses, revisions, of all sorts and sizes, attempting to render possible the identification of species without demanding a lifetime of study for each special group. The ideal for such monographs would be, I assume, that they should be intelligible to, and render possible the determination of species by, any properly trained zoologist, even without previous experience in dealing with the particular groups of which they treat.

The zoological department of the British Museum may fairly claim to have done more towards this rediting of the "Systema Naturæ" than any other institution in the world. The long series of monographs, of which the true character is somewhat concealed under the official title of "catalogues," is a monument to the learning and industry of the great zoologists who planned and executed them. Though they remain indispensable to all serious students of the different groups, however, they are now for the most part long out of date, and vast as is their scope, they cover only a fraction of the animal kingdom.

In 1896 the German Zoological Society began the publication of "Das Tierreich," afterwards continued by the Prussian Academy, which was planned to give nothing less than a revision of all the species of living animals. Here again, however, after thirty-four years, only a small part of the ground has been covered and already the progress of research has rendered many of the earlier parts obsolete. Colonel Stephenson tells me that Michaelsen's revision of the Oligochaeta, published in this series in 1900, deals with exactly half the number of species enumerated by the same authority in 1928.

Apart from these attempts at comprehensive revision we have, of course, numerous surveys of local

faunas on a larger or smaller scale, besides monographs of restricted groups, but hardly ever do these fit together without leaving gaps, geographical or systematic.

Take, as an example, the brachyurous Crustacea or true crabs. No revision of the Brachyura as a whole has been attempted since Henri Milne-Edwards' "Histoire Naturelle des Crustacés" published nearly a century ago. The student who wishes to identify a collection of crabs has to begin with local faunas, such as Alcock's invaluable "Materials for a Carcinological Fauna of India" and Miss Rathbun's monographs of the American species; but for regions that have not been thus studied there is no way but to search out and compare the descriptions of species in innumerable obscure publications by writers who had often an imperfect knowledge of what had been done elsewhere. The genus *Pilumnus* is one that is abundantly represented in all the warmer seas of the globe. No revision of its numerous species has been attempted in recent times. I do not even know how the genus is to be defined from neighboring genera; and yet hardly any report on a collection of tropical crabs does not profess to describe at least one new species of the genus.

Another example from a very different group of animals is given by the aberrant lamellibranch Mollusca forming the family Teredinidae, commonly known as "shipworms." During the past ten years a great deal of attention has been given to these animals in the effort to discover means of combating or avoiding their attacks on the timber of harbor works and the like. Nevertheless, the taxonomy of the group remains in a state of the utmost confusion. There is no agreement as to the limits even of the genera, and the inconstancy of the characters that have been used for the definition of species is plain to any one who studies a large collection. Only in one species, the long-known and often-studied *Teredo navalis* of Linnaeus, have we any detailed information as to variability and the changes that take place during growth. In these circumstances the publication of new specific names, except after prolonged study of ample material, can not be regarded as a serious contribution to knowledge. Dr. Bartsch, of Washington, in his "Monograph of the American Shipworms" (1922) simplified his task by the assumption that any species found on the coasts of the American continent must, of necessity, be different from any found elsewhere, and he was thus able to write "n.sp." after twenty-two out of the twenty-nine specific names. It was soon shown, however, by other American zoologists, that this assumption was without foundation, and that the most destructive species on both the Atlantic and Pacific coasts of North America was the European *Teredo navalis*.

A thorough revision of the taxonomy of the shipworms would be a task of much difficulty but it would be of great scientific interest and it might even be of great practical importance. Those who are carrying out experiments on the protection of timber, in this country at least, seldom trouble to inquire what species they are dealing with or even whether they are always dealing with the same one. Professor Barger, for instance, who speaks of *Teredo* as a "species" does not seem to think that it matters. Perhaps it does not, but it is just possible that it does. We do know that different species differ greatly in susceptibility to changes in the salinity of the water, and it seems worth while to ask whether they all react in exactly the same way to the poisons that the chemists try to administer to them. The fact that our knowledge of their specific differences is still very incomplete is no reason why the chemists should not avail themselves of such knowledge as we have.

One cause that has encumbered systematic literature with uncounted pages of useless writing is the prevalent delusion that it is possible to give what is called a "complete description" of a species. This phrase is apparently intended to denote an enumeration of the visible features of the organism so exhaustive as to include not only the characters differentiating it from the other species already known but also those that will serve to distinguish it from species yet to be discovered. Now a moment's reflection will show that a lifetime would not suffice for the complete description of any animal whatsoever, and on the other hand, a very little experience will convince one that it is impossible to predict the kind of characters that will distinguish the next new species. Some years ago I found that all the specimens of the genus *Squilla* in the museum collection from West Africa differed in half a dozen constant, and, once they were pointed out, conspicuous characters from their nearest congeners. It happened that shortly before a German zoologist had given what was intended to be a complete description of a *Squilla* from the same region. His account extended to two large quarto pages, and yet it succeeded in avoiding mention of every one of the features that proved to be distinctive of the species.

If every one who describes a new species were to restrict himself to a bare enumeration of the characters in which it differs from all the known species of its genus, systematic papers might be vastly diminished in bulk, although one suspects that the labor necessary to write them might be correspondingly increased. It may be a counsel of perfection to suggest that no one should introduce a new specific name without undertaking at least a partial revision of the genus including it, but there are very many instances where the multiplication of species might with advan-

tage be postponed until we learn something about those that are supposed to be known.

The number of described species of animals has been estimated at something in the neighborhood of three quarters of a million. It is not at all improbable that between a quarter and a third of that number would be suppressed as synonyms or put aside as *species inquirendae* by careful monographers and that in many groups the proportion would be far higher.

The prospect is not one that can be contemplated with any satisfaction. The successively expanding volumes of the *Zoological Record* give us a picture of systematic zoology being smothered under the products of its own activity. The confusion will grow steadily worse unless systematists come to realize that the mere description of new species is a far less important thing than the putting in order of those that are supposed to be already known, and until, on the other hand, zoologists in general cease to regard taxonomy as a kind of menial drudgery to be done for them by museum curators.

I have alluded to another obstacle to obtaining an enumeration of the animal kingdom in the divergences of opinion as to what constitutes a species. I am not sure that these divergences are not sometimes overestimated. I think that it will be found that in most orders of animals there exists a considerable body of species regarding whose limits there is no serious difference of opinion among competent systematists; but alongside of these we find in almost every order, in most families and even in many genera a difficult residue in which the delimitation of specific groups sometimes seems to be little more than a matter of personal taste. My colleague Mr. Robson has recently brought together a great deal of information on this subject in his book "The Species Problem" to which I would refer any one who needs to be convinced how complex the problem really is. For our present purpose it is enough to take the empirical fact that the majority of animals can, with more or less trouble, be sorted into assemblages or kinds that we call species. We have seen how imperfect and confused is the present state of knowledge even as regards the mere description and identification of these kinds.

The business of the systematist, however, does not end with identification. Even identification requires some kind of classification, if it is only the classification of the dictionary. Since the time of Linnaeus, or rather since the time of John Ray, zoological systematists have believed in the existence of a natural system of classification which it was their business to discover; since Darwin it has seemed plain that this natural system must be, in some way, based upon phylogeny. It has now realized that the relation between the two is not always so simple and straightforward as it once appeared to be. Dr. Bather, in his presidential address to the Geological Society in 1927, discussed the historical and philosophical bases of biological classification. He concluded that "the whole of our system, from the great phyla to the very unit cells, is riddled through and through with polyphyly and convergence" and that "important though phylogeny is as a subject of study, it is not necessarily the most suitable basis of classification." I am not sure that I quite understand what is implied by the second of these statements, but I do not suppose that even Dr. Bather would be prepared to suggest a system of classification entirely divorced from phylogenetic considerations.

Forty years ago the reconstruction of the evolutionary history of the major divisions of the animal kingdom was almost universally regarded as the chief end of zoological research. To-day, except among paleontologists, one might almost say that the phylogenetic period in the history of zoology has come to an end. When one recalls the extravagances of its later developments, the derivation of vertebrates from arachnids and of echinoderms from cirripedes, one can not be surprised that zoologists of the modern school take little interest in it. If we accept this attitude, it follows that problems of affinity and relationship are not worth worrying about. We are told, in so many words, that our business as systematists is identification, not classification; that what we have to do is merely to devise some kind of key or card-index that will enable animals to be quickly and easily sorted into species. As far as the really scientific branches of zoology are concerned an artificial system of classification is as good as, and may even be better than, any other. An illustration of this attitude of mind is seen in a paper recently issued from Cambridge in which *Lithodes* is replaced, without explanation or discussion, among the Brachyura—which, on the card-index system, is doubtless its appropriate place.

It is quite true that the categories of the physiologist, the ecologist, the geneticist, and so on, often cut across the dividing lines of the most natural classification we can devise, but both the divergences and the coincidences are worthy of closer consideration than they sometimes receive. If there is any truth in the theory of evolution it is obvious that functions and habits have an evolutionary history behind them, but it is no less obvious that this history has not been independent of the history of the organisms that display them. The details of this history we shall never fully know and even its broad outlines may perhaps always remain misty. A natural system of classification expressing even these broad outlines may prove to be an unattainable ideal, but each step towards it

holds out the promise of usefulness in other and possibly remote fields of research.

A great deal of current work and still more of current speculation in zoology seems to me to suffer from this neglect of the taxonomic outlook. In the zoology of the later nineteenth century the comparative method was still the chief tool of morphology. The relative importance of structural characters was measured by the extent of their persistence through larger or smaller divisions of the animal kingdom. This point of view tends to be lost sight of with the increasing emphasis on the experimental method. The systematic zoologist, in listening to the exponents of the modern lines of research, is apt to be impressed by the little account that is taken of the vast variety of animal life. To say this is not to underrate in any way the advances that have been made in these lines within the present century or the revolutionary changes they have made in our views on many fundamental questions. Physiology, for example, is to-day a vastly different science from what it was thirty years ago, partly because the physiological laboratory has a more varied fauna than it had then. Nevertheless, the zoologist, conscious of the unending diversity of structure and of habits among animals, sees the physiologist's results against a background of which the physiologist himself seems to be sometimes forgetful.

One hesitates to suppose that the students of heredity are really so forgetful of this background as they sometimes seem to be. No doubt intense specialization is needed for intense research; but the poet of the breakfast table, laughing gently at the narrow specialism of the Scarabee, can hardly have foreseen the day when a university in his own country would have upon its teaching staff an officer named in the university calendar as a "Drosophilist."

It is possible, however, that the prevailing lack of interest in questions of phylogeny may have a deeper significance. Those departments of biology that are being most actively studied at the present day are preoccupied with the interplay of forces acting here and now. They ignore the impressions that time may have left on the material of their study. It is as though a crystallographer, studying a pseudomorph, should endeavor to explain its form in terms of its chemical composition and the forces governing the arrangement of its molecules, without taking account of its past history.

From ignoring anything, it is but a short step to denying its existence, and here, it seems, we have already arrived. Some of you may possibly have listened to a lecture delivered in London in the early part of last year by that very distinguished experimental biologist Dr. Hans Przibram, in which he suggested that we might have to consider the possibility that every species of metazoan had developed independently of all the others from a distinct species of protozoan. The same view was set forth by him in a lecture delivered in Paris on the "Theory of Apogenesis."[2] As the English lecture has not been published I will translate as closely as I can from the French one:

> I do not think it likely that a single substance can have given rise to a general phylogenetic tree according to the classical diagram representing the affinities of species and their distribution in space and time. All the facts would be explained more easily by supposing that there existed, at the beginning, many organized substances developing side by side into species, each of the latter passing through stages more and more advanced without actual relationship of descent between the different species.

Many authors have believed in a multiplicity of the primordial forms of life, but few have suggested an independent origin for grades lower than the main phyla. Przibram, with strict logic, has carried the same reasoning down to the individual species. Most biologists with whom I have discussed the matter refuse to take his suggestion seriously. This, I venture to think, is a mistake. Przibram has simply carried to their inevitable conclusion certain lines of thought that we meet with everywhere in current biological literature; that conclusion is either one of the most significant results of recent biology or it is the *reductio ad absurdum* of much contemporary work.

Geneticists have made us familiar with the doctrine of the inalterability of the gene, with its corollary of evolution by loss of factors, which, by the way, seems to differ little from Przibram's apogenesis. The experimentalists have proved (if it wanted proving) the plasticity of the phenotype, as, for instance, when Przibram himself shows that the length of a rat's tail is a function of the temperature to which the individual and its immediate progenitors have been exposed. As for the inheritance of impressed modifications, the more unequivocal the experiments devised to demonstrate its reality the more clearly do they show it to be of so fugitive a kind as to have no significance in evolution. Paleontologists, as Dr. Bather has told us, have proved beyond the possibility of doubt the occurrence of parallel and even of convergent evolution, without telling us where we are to stop in applying the principle. Many supposed examples of adaptation fail to stand closer scrutiny, and therefore the whole idea of adaptation is declared to be a subjective illusion. All these results at any rate place no obstacles in the way of Professor Przibram's suggestion.

[2] *Rev. Gen. Sci.*, 11 (No. 10): 293, May 31, 1929.

It is to be noted that although the theory of apogenesis is called a theory of evolution it does not deal at all with evolution as that word was used by Darwin. It has nothing to say on the origin of species. On this question it is no more than a doctrine of special creation at one remove. It has no light to throw on classification. If we are to abandon belief in community of descent the whole architecture of the "Systema Naturæ" becomes meaningless.

Professor Przibram claims that "all the facts would be explained more easily" upon his hypothesis, but there is one point on which he speaks with a hesitant voice, and it seems to me a very significant exception. "We can not decide," he says, "whether the differing though related species that inhabit islands or isolated territories are descended from a common source or result from the accidental separation of species which formerly occupied the region together."

Let me recall to you the opening words of the "Origin of Species": "When on board *H. M. S. Beagle* as naturalist, I was much struck with certain facts in the distribution of the organic beings inhabiting South America, and in the geological relations of the present to the past inhabitants of that continent." So Przibram ends where Darwin began. The geographical and geological distribution of organisms, which for the one are merely the negligible residue of unexplained facts, were for the other the very heart and core of the problem he set himself to consider.

It is worth remembering that among Darwin's other qualifications as an interpreter of nature he was an experienced taxonomist, and before he wrote the "Origin of Species" he had produced one of the finest systematic works ever written in his "Monograph of the Cirripedia." Those of us who were present at the memorable Darwin-Wallace celebration of the Linnean Society in 1908 remember how the veteran Alfred Russel Wallace discussed "the curious series of correspondences both in mind and in environment" which led Darwin and himself, alone among their contemporaries, "to reach identically the same theory," and how he gave the first place to the fact that both he and Darwin began by collecting beetles and thus acquired "that intense interest in the mere variety of living things" which led them to speculate upon the why and the how of "this overwhelming and, at first sight, purposeless wealth of specific forms among the very humblest forms of life." It might be worth while to inquire whether a training that proved useful to Darwin and to Wallace would not be of some value to students of zoology even at the present day.

My predecessor in this chair told you that "the present position of zoology is unsatisfactory," and he found the chief hope for the future in the application of the experimental method. He may be right. I am not so sure. The experimental method has answered many questions and it will answer many more, but there are some questions, and these well worth the asking, to which experiment will never find an answer. No one will maintain that taxonomy by itself will answer them, but it will often suggest where the answer is to be sought for, and it will provide a standpoint from which both questions and answers will be seen in a true perspective.

Finally, I would recall a remark once made in my hearing by a wise old naturalist, the late Dr. David Sharp. Some one had been remarking on the decline of systematic zoology and predicting the extinction of systematic zoologists. Dr. Sharp replied, in effect:

> I have seen many passing fashions in zoology, many departments of research becoming popular and then falling into neglect; the one branch that will never fail to attract is the systematic one. The esthetic satisfaction to be derived from contemplating the mere variety of animal forms and from tracing the order that runs through all its diversity appeals to a very deep instinct in human nature. There will always be systematic zoologists.

THE USEFULNESS OF PSYCHOLOGY[1]

By J. McKEEN CATTELL

IT is a pleasure to receive this beautiful gold medal of the Society of Arts and Sciences from Mr. Russell, the president of the society, who in its conduct and in other directons has endeavored to bring together the fine arts and the natural sciences. In some of its aspects science is a fine art and both are children of the creative imagination, born with hard labor. As Keats wrote:

> Beauty is truth, truth beauty;—that is all
> Ye know on earth, and all ye need to know.

It is a satisfaction to be introduced by Professor Thorndike, my friend and colleague for more years than he might like me to tell. To him we owe in large measure the present application of psychology to education, the most useful achievement of our science.

It is an honor without parallel to be associated with the earlier recipients of this medal in science, Mr. Edison, Professor Michelson and Dr. Millikan,

[1] Address at a dinner of the Society of Arts and Sciences, the Hotel Biltmore, New York, on the occasion of the award of the medals of the society, April 17, 1930.