

man to live with. His singular action when after the death of his first wife he sought another is one of the most humorous of matrimonial adventures. At his request his friends sought for a suitable companion. Eleven ladies with strangely diverse qualifications were passed in review, and of them, after much vacillation, Kepler chose Susannah Reutlinger, the daughter of a cabinetmaker. Of her he wrote that she had an education worth the largest dowry. "Her person and manners are suitable to mine—no pride, no extravagance. She can bear to work; she has a tolerable knowledge how to manage a family; middle aged, and of a disposition and capability to acquire what she still wants." For fifteen years Susannah shared Kepler's joys and sorrows and difficulties, and she bore him seven children.

In his work Kepler found the fullest satisfaction, and a discovery elated him as much as it did Davy. In Kepler there was none of the cold, passionless calm we associate with Cavendish. Like a Luther, he regarded himself as an instrument of the Almighty, and his studies were interspersed with prayer. He had once written a small treatise on the Divine Wisdom as shown in the Creation, and

his subsequent works contain many passages of exaltation. When after seventeen years of searching he discovered the third of his laws his delight knew no bounds. "Nothing holds me", he said, "I will indulge in my sacred fury; I will triumph over mankind by the honest confession, that I have stolen the golden vases of the Egyptians, to build up a tabernacle for my God, far away from the confines of Egypt. If you forgive me, I rejoice; if you are angry, I can bear it. The die is cast; the book is written, to be read either now or by posterity—I care not which. It may well wait a century for a reader as God has waited six thousand years for an observer." Kepler's work, however, was appreciated immediately by his contemporaries, while of his discoveries the famous French physicist Arago once wrote: "Les lois de Kepler sont le fondement solide et inébranlable de l'astronomie moderne, la règle immuable et éternelle du déplacement des astres dans l'espace. La gloire de Kepler est écrite dans le ciel; les progrès de la science ne peuvent ni la diminuer ni l'obscurcir, et les planètes, par la succession toujours constante de leurs mouvements réguliers, la raconteront de siècle en siècle."

### The Taxonomic Outlook in Zoology.\*

By Dr. W. T. CALMAN, F.R.S.

THE anatomist, the physiologist, the field naturalist, the student of one or other of the innumerable specialisations 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.

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 realise the difficulties that may stand in the way of this identifica-

tion. 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 systematist's 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 4370 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 endeavoured 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 3000 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

\* From the presidential address to Section D (Zoology) of the British Association, delivered at Bristol on Sept. 4.

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 Linnæus, it is likely that a very experienced zoologist might have been able to recognise 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 recognises 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 anyone else. From the time of Linnæus, 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 re-editing 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. Col. Stephenson tells me that Michaelson'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 scarcely ever do these fit together without leaving gaps, geographical or systematic.

The number of described species of animals has been estimated at something in the neighbourhood 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 inquirendæ* 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 realise 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 over-estimated. I think that it will be found that in most orders of animals there exists a considerable body of species regarding the limits of which there is no serious difference of opinion among competent systematists; but alongside 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. Mr. G. C. Robson has recently brought together a great deal of information on

this subject in his book "The Species Problem", to which I would refer anyone 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 Linnæus, 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 is now realised that the relation between the two is not always so simple and straightforward as it once appeared to be. Dr. F. A. 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 palæontologists, 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 cannot 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. So 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.

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 under-rate 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 specialisation is needed for intense research; but the Poet of the Breakfast Table, laughing gently at the narrow specialism of the Scarabee, can scarcely 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 endeavour 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. In a lecture delivered in

London in the early part of last year by that very distinguished experimental biologist Dr. Hans Przibram, he suggested that we might have to consider the possibility that every species of metazoan has 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 (*Rev. Gen. Sci.*, 11, No. 10, May 31, 1929, p. 293). 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", he says, "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 organised 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 example, 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. Palaeontologists, 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 Prof. Przibram's suggestion.

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.

Prof. 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 cannot 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 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.

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 point of view 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. Someone 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 æsthetic 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."

### Obituary.

PROF. H. W. WILEY.

IN Harvey Washington Wiley, who died on June 30, we lose a man who was a great Uesanian warrior in the cause of pure food, a man of imperious character, officially a perfervid Puritan idealist and extremist, yet in the society of friends the perfect Yorick, "a fellow of infinite jest, of most excellent fancy". As first administrator of the American Food and Drugs Law, his own beloved child, he was nothing short of an all's-fair-in-love-and-war man: perforce, in fact, he had to adjust his methods of attack to the times and to those of his foes, as he was severely up against trade interests. He trod heavily upon not a few corns and it is clear that, occasionally, his pendulum swung beyond the limits of scientific reason; still, the end was one to justify almost any means. He had courage and, in large measure, won, as he definitely established a sound public opinion.

I first met Wiley in 1903, at his most active period, at a gathering of Agricultural Experiment Station workers, in Minneapolis, at which I was present as Lawes lecturer. He was the life and soul of a large meeting; ever full of resource. Thus, on one excursion, in a dry town on a very hot day, displaying a surprising geographical instinct, he took some of us poor sufferers to a pharmacy and tendered a prescription on our behalf: the medicine we got passed all the Brer Rabbit tests for good ale and no doubt saved our lives. A few years later he and I forgathered at Washington, in the Cosmos Club, an institution the worth of which will be known to many. We met one afternoon in the main square, to go out to the Country Club. He was carrying a parcel and there was a suggestive bulge at his hip. We were to pass the county border, into an arid region. The parcel and that bulge were of no slight aid to our evening's pleasant intercourse. There is an immortal to this tale. At the time of his retirement, late in life, he passed under petticoat rule, to become thereafter an exemplar of dryness. The injury done to him by his previous depravity—at least so his friends claimed—was made obvious by the arrival, without undue delay, of two healthy boys. Several months ago, hearing that he was very ill, I wrote to cheer him, suggesting that he set an example by repenting of his later sin. This amused him, I was told. The reply, bearing his signature, gave too much advice for the good of my soul to be his. I could only write back that John Barleycorn had not done much obvious harm to either of us. My old friend became in fact a first-class humbug in the matter of drink: probably he was never a man of really balanced, scientific judgment. We have to hold such men

very much in mind, however, in taking stock of the States: too few realise how rigid the American outlook often is.

Only recently, the *Times* told us, an American Senator wrote to our Ambassador in Washington to protest against the exercise of his right to take liquor into the Embassy, suggesting that such action was likely to have the most serious effect upon our international relations. Do Americans recognise how entirely they are cutting us off from rational intercourse with them? We welcome them here in crowds and they do not seem to return habitual drunkards. Few of us go from this side as travellers—we only visit the States when compelled, either on business or when imported to join in colloid worship at Cornell or some similar academic frivolity. Few Uesanians understand how impossible it now is for us to risk travelling in their country—the danger of their soft drinks. Yet it is one that is full of beauty and interest, as I can vouch; the only difficulty is that there is so much of it. Whatever it be, it is a land in which, at times, every pore of you aches for beer; one where the hart ever pants for the cooling Milwaukee stream.

Straining at the gnat ethanol, to-day, Americans swallow the camel caffeine in canfuls: however, a missionary from here is now in Canada who will disabuse them (of course piously, on week-days) of this delight. As a matter of fact, Wiley, in 1912, warned the American public against the danger of too much caffeine. He was often here and always amusing. Describing once the activities of his Department, he told how advice was given which led to the need for water in one of the arid regions of Texas being overcome: this was done by growing onions between rows of potatoes; the eyes of the potatoes watered so much that artificial irrigation became unnecessary.

It matters little where a man like Wiley came from. *Pro forma*, let it be told that he was born in Indiana on October 10, 1844. In 1863 he went to College; in 1868 he began to study medicine, graduating M.D. from the Indiana Medical College in 1871. Probably medical education at that time was no great shakes. He then had a year in the Lawrence school at Harvard; became professor of chemistry in Butler College, in 1873; from 1874 to 1883 he was professor of chemistry at the Agricultural College, Purdue, Indiana, spending a year in Germany during this period. State chemist of Indiana in 1881, he was made chief of the Division of Chemistry in the U.S. Department of Chemistry in 1883. He was president of the American Chemical Society in 1893–94. In 1901 he was pro-