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THE LIMITATIONS OF TAXONOMY[1]

IT is a matter of common observation that taxonomic work in entomology is more highly regarded than formerly. I remember less than thirty years ago, when a student of mine went to an eastern entomological center for advanced work, he wrote back to me that no courses in classification of insects were provided; the students, he wrote, were supposed to work this up on the outside. Now in the same institution are offered a series of taxonomic courses in entomology, covering most of the orders. Even at the beginning of the present century, very few entomologists occupied positions where classification was a recognized part of their duties; generally those who did the necessary identification work were paid ostensibly to do something else. But now we find a considerable number of taxonomic positions in entomology in the United States—not all as well paid as they should be, of course, but hopefully established. The great expansion of economic work in entomology has made exact identification a fundamental requisite, as is now universally recognized.

From this fairly secure position I wish to survey the field and point out some limitations of taxonomy which make me doubt whether even within our own fraternity the difficulty of our task is fully appreciated, and whether without a more general appreciation of the requirements of our science we can hope in the next few decades to approach in any acceptable degree toward the ideals we hold.

The first and most obvious difficulty is the inconceivable number of species to be classified. I can well remember when a venturesome but far-seeing entomologist predicted that the living insect species of the world would ultimately be found to number at least a million. At the time many thought the estimate extravagant; but now it is seen to be far too low. It is claimed that the number now described and named is about 640,000, and the annual additions run far into the thousands. Walther Horn has recently put the difficulty this way:

> Whoever as an entomologist looks into the future knows full well that we are steering into a shoreless sea, no matter whether he estimates the total number of insect species at three, ten, or fifteen millions. In the near future any beginner will be grayheaded before he has caught up with what is already known.

[1] Address of the retiring president of the Entomological Society of Washington.

I am reminded of a proposal made years ago by Professor Burt G. Wilder, who was an enthusiast on the structure of the brain. In a published article he lamented the fact that a student beginning in college to specialize on the brain could in the course of an average lifetime just about catch up with what was already known, leaving him no time to make further discoveries. The professor proposed in all seriousness that the study of the brain must be commenced at an earlier age, suggesting as I recall that about eight or ten years would be a suitable one for the beginning student. We might consider the same course in entomology, but even this would relieve the situation for only a few decades, when the increased number of species would fill the time as full as it now is. Obviously nothing but more narrow specialization can overcome the handicap of expanding numbers; later I shall recur to the disadvantage of this.

Another physical difficulty lies in the fact that the larger part of this great host of insect species is not yet captured. Before they can be classified they must be collected and preserved for study.

Allied to this is the almost insoluble problem of seeing the types, when as usual those desired are scattered in museums over the world.

Yet another physical difficulty is the task of assembling the library and indexes which will enable the student to work under proper conditions. And this difficulty increases even more rapidly than the number of insects, as the books required become not only more numerous, but more costly. When I began to study Diptera in 1890 there were but a few expensive works on the subject to be bought; in the succeeding years I was able to add what followed so gradually that it was not a severe tax to build up a library in the order. But the beginner must now be prepared to spend liberally, or else must establish himself in an institution where a large library exists; if he work by himself with only a few books, he will have to confine himself to a very narrow specialty indeed. The indexes present a problem even greater, for they take a vast amount of labor in preparation and are found in only a few places, and even there are more or less imperfect, often including only North American species. If the worker make his own, this is another factor holding him to a narrow field.

If we assume the impossible case that the taxonomist has overcome the difficulties mentioned and has assembled all the material, including types, and all necessary library and indexes, he is ready to proceed with a natural classification of the group. That is, according to the ideal generally accepted, he must make his classification conform to the evolutionary history and relations of the group. How is he to ascertain these? To show how much is involved, I will mention the lines of approach he is supposed to use, taking them from Heikertinger's convenient review of Karny's new book, "Methods of Phylogenetic Investigation," with some explanations of my own.

(1) Paleontology. In the higher insects this is a field but little explored. The fossils as far as known are useful principally for the higher categories, and for the lower orders.

(2) Geographical distribution. This may throw light on the evolution of existing forms and is generally far more useful for relationships of genera and species than the preceding.

(3) Ontogeny, or the development of the individual. This is an application of the biological law that the individual in its development recapitulates the evolution of the species to which it belongs. There is scarcely a group of animals in which the application of the law is attended by greater difficulty than in insects, especially the higher forms. In Diptera, for instance, the larva is not ancestral to the adult in structure, but rather a side development, as Comstock has pointed out. This cuts out the later ontogeny, leaving only the early embryonic stages to be considered, which at best are significant only for the higher categories.

(4) Morphology. This is what is generally used, almost to the exclusion of other lines of study.

(5) Ecology. This may indicate causes of variation in climate or immediate surroundings.

(6) Teratology, the study of monstrosities, which sometimes are much more primitive than the normal form of the species. For instance, Professor Wheeler once figured a specimen of Dilophus having a supernumerary antenna growing out of a fore coxa; this antenna was of a much more primitive kind than that of the normal Dilophus, and seemed to indicate that Dilophus is a derivative of the Nematocera, which in this case is abundantly confirmed in other ways.

(7) Experiment.

(8) Chemical and serological methods.

Merely to enumerate this list is to show how far we are from the ideal classification as yet. We rarely see any important use of factors other than morphology and distribution.

One of the fascinating but difficult problems of classification is what to do with complex species. We all know that the geneticists have produced a host of Drosophilas which if occurring in nature would be called distinct species or genera, but which are explained as being merely assortings of the hereditary matter in the chromosomes. When we know the secret of their origin we can not call them species; they are more like the breeds of domestic animals. But amid other species in nature we may be misled by the occasional occurrence of a striking form arising in the same way, which looks to us like a true species.

There are also in nature the well-known cases of insect strains, where habit differs and is inherited in the apparent absence of structural characters. For instance, *Exorista larvarum* of Europe attacks both the gypsy moth and the brown-tail moth, while in this country the same fly, as far as structure indicates, attacks neither. The apple maggot fly has apparently produced two offshoots, one of which attacks the blueberry in Maine, the other the snowberry in British Columbia, while neither troubles the apple.

A few miscellaneous cases may be added. There are many species of plant lice which have two host plants migrating from one to the other at certain seasons, and changing on the second plant to a form which taken by itself would be supposed to be quite a different genus from that on the first. Dr. A. C. Baker, you will recall, recently stated in our society that when he transferred a certain aphid to a different food-plant from the normal one, in a few generations he detected constant differences in the anatomy; these differences might have been the basis of a new varietal name if he had not known the history of the case. Mr. S. A. Rohwer tells me of a parallel case in sawflies, where he had drawn up the description of a new species, but happened before publication to discover that it was a known species transferred to a different food-plant. Mr. H. S. Barber finds that fireflies at Plummer's Island have a certain mode of flashing at one time in summer, and two weeks later a different one. The seventeen-year locust has a little brother with a shrill voice, which has been given a different specific name. I have recorded that in the middle west the pale *Cerodonta dorsalis* changes in its late fall brood into the dark form *femoralis,* characteristic of the Rocky Mountain region. A series of classic experiments have shown how light and dark forms of Lepidoptera may be produced by modifying the temperature and humidity during the pupal period. The beetles inhabiting the narrow, cold coastal strip of Oregon and Northern California are darker in color than the same species living a few miles farther inland. Dr. W. R. Thompson has lately published figures of three recognizable forms of first-stage larva in *Masicera senilis,* the later stages of which seem to be all alike. Similarly Dyar and Knab found a number of cases in mosquitoes where two distinct larval forms produced apparently a single species of adult.

These are only a few illustrations of what I mean. The problem I wish to call to your attention to in this connection is the difficulty of expressing such complicated and varied relations in the scientific name. We are limited to two or at most three words, and it seems to me that the mass of information we are asked so to convey is beyond the capacity of language. If we trust to varietal names, we shall have so many different kinds of varieties that the traditional use of this category for minor structural differences will be completely obscured.

Somewhat akin to the preceding are the difficulties arising from the introduction of new groups of characters, especially those of the larvae. It is natural to assume that the larval characters can not contradict those of the adult when brought into the taxonomy, and yet in the higher orders, as the Diptera, where the present larval form is not ancestral, the statement is not axiomatic. Our existing classification of the adults may be a misinterpretation of evolution which can be corrected by reference to larval characters; but it is just as likely that we are misinterpreting the larval characters, which have been as yet so little studied. I would not mention genitalic characters here as presenting a difficulty in separating species, for they are helpful in the extreme; they do, however, lead to new combinations in genera and are as much subject to misinterpretation here as the larval characters.

So far I have been dealing with physical limitations, tending as I think to delay or prevent the consummation of a complete classification of insects. I now pass to certain difficulties of a much more complicated order, the psychological or subjective ones, which have to do with the mentality of the taxonomist. It was for some time my intention to call this talk "The Psychology of the Taxonomist," but I finally decided to take in the physical factors.

The taxonomist has on the average no special psychological peculiarities, but I think in him the working of ordinary processes will account for some common deflections of judgment.

One of these processes is the magnification of one's own work or property, by virtue of which in a few years of study an entomologist becomes convinced that the suborder with which he works ought to be considered an order, the family ought to be two or three families, and so on. I had been working only a couple of years on Diptera when I got hold of the Hyatt and Arms treatise on insects, in which Dr. Hyatt expressed the opinion that the Diptera are the highest of all the large orders. I was delighted with the idea, so much so that I published an article in SCIENCE (XIX, 1892, p. 244) endeavoring to support it. For a number of years I was thrilled when I thought that my favorite order was the highest. But that was a good many years ago; now I feel no personal interest in the matter. I might add that my article drew a note from a dipterist who was specializing in the housefly group, in which he claimed that the housefly, and not as I had thought the sheep-tick, is the highest of insects.

It is of course an equally common fallacy to attach too much importance to a thing simply because it is close at hand. This error is akin to the preceding, but can be separated in part at least. Probably the reason so little attention is paid to fossil insects, or to exotic forms, may be looked for here. The indifference of most entomologists to insects from outside the country or region to which they limit themselves is almost appalling. One encouraging sign of the times is the change in this regard in the United States within ten or fifteen years, in which I am happy to say the National Museum is taking the lead.

The subject can not be analyzed without mention of the case where the individual is so ambitious to attach his initials to as much as possible of the framework of classification that he seems bent on introducing the greatest possible number of new names. It is hard to diagnose some cases clearly, as an entomologist may merely be infected with the idea that all change is improvement; or he may honestly and perhaps with justice believe that previous work in his specialty has been notably poor.

Of all fallacies in taxonomy, the all-pervading one is that the relationships are better expressed by the continual dividing of the categories. We "raise" a suborder to an order, a subgenus to a genus, etc., world without end. The result achieved is generally a reduction of the value of the term—in other words, the inflation of our taxonomic currency. I think it no exaggeration to say that the family has about half the value now that it had when I began, and the genus probably one fourth. These changes have been made without materially altering the indications of relationship, which were just as clearly evident before inflation as after.

In this connection it might be worth while to consider this fact; suppose a class of animals—any class—to be divided into twenty-eight orders; each of these to be divided into twenty-eight families; each family into twenty-eight genera; and each genus into twenty-eight species. We should then have provided classification for 614,656 species, or by chance almost exactly the number now known in Hexapoda. Of course no one would think of suggesting that every genus should have twenty-eight species, but should not the law of averages have some scope in a number so immense as 600,000?

There is one phase of the matter which does not always receive consideration. Classification has two functions—to express the relations between organisms, and to give us names for the different kinds which shall be stable and universal. Linnaeus worked out the idea of a scientific name consisting of the genus and species; the latter was the name proper, while the former connected the species with a group of similar forms. One name defined, and the other located the species. The genera of Linnaeus were comprehensive, about like the family of the present day, which was absent from his scheme. Immediately after the publication of the tenth edition of Linnaeus, Geoffroy proposed to divide the genus, where large, into families; the idea was not accepted, however, and some years later the family came in again, this time to be accepted, as the next category above the genus. I have often thought that our scientific names would better fulfil their function if Geoffroy's scheme had been followed. Under it the generic name would still be as comprehensive as our present family name, a great advantage over our existing system, under which the genus often fails to connect the species with any other whatever, and in many other cases with only one or two.

In the second part of Brauer and Bergenstamm's large work on the Muscoids of the Vienna Museum, this sentence occurs:

> Whoever works with a narrow group that is rich in species and begins to separate these, ascending from the smallest groups to higher ones, easily falls into error regarding the value of categories; for it is easy to distinguish so many within the limits of a single genus that is rich in species that, *without keeping in view the entire field of zoology*, it seems that all the categories repeat themselves within the single genus.

Here is the explanation of the error; we do these things without keeping in mind the whole field of zoology. In our little field we readily distinguish several orders of groups, one above the other; and without the survey just mentioned, we attach to these the names which in the broad view belong to larger categories. It is the commonest fault of taxonomy, and one to which the beginner is especially likely to fall a victim. He soon comes to feel that the conservative course is likely to be interpreted as inability to see characters.

There may be less difference than is commonly supposed between splitters and lumpers in what they see—the difference may be far more in the terms they use. This is especially true in the matter of genera. In fact, a taxonomist may be a lumper in genera and a splitter in species, and I think a good deal may be said in favor of this position.

It will, I think, be apparent that there are great and varied difficulties in our task, and as we contemplate the future they seem even more serious. As specialization becomes ever more narrow, it will continually be harder to attain the perspective which alone can guide to the correct use of the chief categories. Intensive study is already modifying our idea of the species to a great extent, so much that some are beginning to doubt if such an entity exists; and yet without it we are completely undone and will have to begin over again. The physical and personal limitations of those who do the work must always keep the quality far below the ideal which I have discussed. Criticism is usually a generation behind publication, so that the poorest work may stand, as we often see, for twenty or forty years before it is competently revised. Moreover, the ablest taxonomists are likely to be the very ones to succumb to the higher rewards of administrative and economic work, and so fail to make the contribution of which they are capable.

I wish I could close with a strain of optimism, but the best I can say is this: Taxonomy demands the highest talent, and those who prove their fitness should have every facility and inducement; there is increasing recognition of this fact, and here lies the only hope that this basic science can perform for humanity the service demanded of it.

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THE ELECTRICAL ENGINEERING RESEARCH SITUATION IN THE AMERICAN UNIVERSITIES

PRODUCTIVE research is both the criterion and vehicle of scientific progress. This fact makes university research of especial importance, not only because of the additional knowledge that is obtained, but also because of the close relationship existing between collegiate research and scientific education. In view of the great amount of discussion going on at the present time regarding engineering education it is interesting, and very much worthwhile, to investigate the research situation in the technical schools of this country. The following paragraphs give the results of such an investigation into the field of electrical engineering.

It is a relatively simple matter to determine with satisfactory accuracy the amount of productive research being done in our electrical engineering schools. Practically all researches of permanent value which American electrical engineers perform are reported in the *Transactions* of the American Institute of Electrical Engineers. To obtain a very good idea of the electrical research being done in this country it is merely necessary to count titles in the *Transactions* for a period of years, omitting articles from foreign sources, presidential addresses, and papers of a general nature which do not represent contributions to science. Fortunately nearly all the articles published in the *Transactions* are of a technical nature, and represent research work of some kind, or at least the accumulated experience gained by years of observation of electrical equipment.

During the six-year period from 1920 to 1925, inclusive, 442 technical articles appeared in the *Transactions*, of which 54, or 12.2 per cent., had a college source. This gives an average of nine papers a year as the annual production of our many teachers of electrical engineering and of their students doing thesis work. Considering the results separately by years gives the following table:

Articles in *Transactions* of the A. I. E. E. 1920–25

Year	Total	College	Per Cent. College
1920	60	5	8.3
1921	36	3	8.3
1922	66	5½	8.1
1923	80	13	16.25
1924	113	13½	11.95
1925	87	14	16.1
Total	442	54	12.2

The fractional titles are the result of joint authorships. It is to be noted that while the amount of research being done in America in electrical engineering jumped very greatly as soon as research programs got under way after the war interruption, yet the proportion coming from colleges is substantially constant, and is in all cases surprisingly small.

It might be expected that the remarkable development of the radio field in recent years would absorb much of the creative effort of electrical engineers. Analysis shows that such is not the case. A survey of articles appearing in the *Proceedings* of the Institute of Radio Engineers for the six-year period from 1920 to 1925 shows that 174 articles were published, of which twenty-seven and one half, or less than five per year, had a college source. Furthermore, the radio field goes so far into the realms of the physicist that over half of these five articles per year must be credited to physics rather than to electrical engineering.

The results given above do not include all the research results published by electrical engineers, for the electrical engineer does not always report his researches in the publications of the national radio and electrical societies. From time to time important articles which should be credited to electrical engineering appear in other places, particularly in the magazines of the physicists and mathematicians. However, a glance at the bibliographies of electrical engineering papers shows clearly that the number of such stray articles that should be credited to the American electrical engineer, and particularly to the engineering schools, is relatively small, and can be