The Approach to Taxonomic Problems[1]

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Of all the large family of zoologists the entomologist, who calls himself a taxonomist, has won the firmest place in the public mind as a simple minded eccentric. He is the character who is continually being caricatured as dashing madly across the fields, butterfly net in hand and goatee rampant. It is to him also that we owe the definition of entomology as that science which consists of moving insect pins from one place in a box to another. This popular stereotype of the taxonomist has come about through a misunderstanding of what taxonomists do and the purposes behind their work. It is also the outgrowth of the personality of individuals, whom we associate with taxonomy, and about whom various wild and unprovable tales have grown up. It is useless to deny that a certain percentage of taxonomists are queer ducks, but it is unfortunate that we seldom hear about those who seem to be normal.

In the early days of entomology in this country, the objectives of the few taxonomists, who had the inclination and courage to attempt the classification of our vast insect fauna, were quite different from those of workers in the same field today. Many of the early workers were firm believers in special creation of species and to them their task was simply to produce catalogues of existing and immutable entities. Later evolutionary theories influenced various workers, and many of the abuses which we lay at their feet were the result of attempts to extend the methods of classical descriptive taxonomy into areas where they could not be applied. Right down to the present, the principal tool of taxonomic entomology regardless of its purposes has been pure intuition. The primary difference between the older taxonomists and modern workers is neither in purpose nor method, but in the belief that intuition is the beginning, not the end, of taxonomic study.

Taxonomy can be defined as the systematic study of the nature and relationship of naturally existing species populations. By such a definition, it falls somewhere between descriptive and experimental morphology and population ecology. In other words, we are dealing with organisms above the level of the individual, but not quite at the level of the biotic community. The principal common ground among taxonomists in all fields lies in their interest in distribution, variation, adaptation, relationships, and inheritance as they affect species populations rather than individuals.

Such an approach is not always regarded with favor. The results of such studies do not always seem worthwhile to everyone. It is argued that the same results could be obtained by the systematic exhaustion of other fields such as morphology, genetics, ecology, and biogeography. Against this argument the tremendous abundance of existing forms of insects, not counting the extinct forms, is a primary objection. Assuming that one person in his lifetime could exhaust one of the special fields such as genetics for each species, it would require about 25,000 individuals to

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complete the study of Coleoptera alone in North America, north of the Mexican boundary. One of the great contributions which taxonomy can make is the discovery of points at which studies in other fields can be started with a maximum promise of results applicable to groups rather than individual forms. It is the old argument of strategy versus tactics.

There seem to be three main steps in approaching any taxonomic problem. The first is still the "intuition" of the older workers, involving the selection of a problem which offers possibilities. There may come a day when intuition is no longer needed, and the routine application of statistical methods carried out by batteries of workers feeding data into machines will make any problem worth attacking. Such a time seems, however, very distant when we consider the number of workers and the financial arrangements under which most of them proceed. The second step is the hard part. It involves subjecting our initial intuitions to the most detailed sort of analysis, attempting to make our data quantitative, and by painstaking and tedious work to remove as much of the subjective element as possible. Here the battery of workers and machines would be most useful, but in itself this sort of detailed analysis can produce little beyond an accumulation of facts. It requires a third step in which by induction we attempt to bring back into focus our original problem and by correlating all the available data arrive at a more complete solution. Here we must call upon every available source of information be it from genetics, ecology, palaeontology, geomorphology, or history to complete our picture.

There are many fine examples of entomological problems which have been conceived and carried out in just such a fashion. I would like to present as an illustration, however, an incomplete problem from my own field, primarily because it points up many of the difficulties to be encountered.

The Background: Thomas Say in 1823 first recognized as distinct from other known forms a group of predaceous water beetles which he described as *Colymbetes bicarinatus*. His description is very meager, but he pointed out various salient features which allowed the ready recognition of the group to which Aubé in 1838 applied the generic name, *Matus*. Thus matters stood for 102 years, until Hugh B. Leech, now of the California Academy of Sciences, took the trouble to look at some of his specimens from various parts of the United States and discovered the surprising fact that *Matus bicarinatus* (Say) was really a complex of related forms rather than a single widespread species. The same year that Leech's paper redefining Say's *bicarinatus*, and describing a new species with a distinct subspecies from northern Florida and Georgia, was published, I discovered a remarkable pygmy species of *Matus* in western Florida. Later another distinct species was discovered in eastern Texas and Arkansas.

In a relatively short period, following the initial recognition of the composite nature of the genus, four distinct populations which we can call species, and a fifth population which we consider a subspecies, have been recognized. It is not hard to understand why these populations remained so long unrecognized when forms of far less distinction in other

groups were well known. *Matus*, as a genus, is beautifully characterized by the sulcate prosternum, tarsal claws, and body shape, and the several species bear a remarkable superficial resemblance to one another. Further, the forms are highly seasonal in occurrence as adults, which makes them rare in collections, and no one apparently had ever looked at more than a few specimens at one time from different places. Even now the male genitalia are the best indicators of the distinctions, and must often be relied on for recognizing the overlapping populations.

The Problem: Now that we have a number of recognizable populations within the genus *Matus*, we can attempt the formulation of a problem which will take advantage of the peculiarities of the group. *Matus* is presumably a relict of the ancient woodland fauna of the Appalachian region. It is confined to the Eastern United States and the various forms are distributed peripherally around the mass of the Appalachian Mountains. Its only relatives are found in Australia, so that we have a striking example of a group of organisms isolated in time and space, and thus presumably of very ancient origin. What can we hope to learn from a study of the existing populations of *Matus?* Half a dozen answers immediately occur to me, but most of them are not directly approachable by taxonomic methods. The immediate taxonomic problem seems to be a study of the population structure of the various forms making up the genus and a comparison of this structure with that of genera which are undoubtedly more recent in origin, less specialized, and show different patterns of distribution.

The Analysis: So far, the analysis of the various populations of *Matus* has not progressed very far. There is still a dearth of specimens available for study, and the accumulation of series of specimens from different localities is a painstaking process. Some interesting facts have already appeared, however, the most promising being that even the described "species" are not uniform. Characters which we consider diagnostic slide up and down scales of variation, biometrical characteristics shift over large areas, and isolated populations present new and undescribed differences. It will be many years before necessary facts for a complete taxonomic analysis can be accumulated.

The Inductions: With the analysis of the populations still incomplete, only tentative conclusions are possible, but several general hypotheses may be of interest. It seems very probable that the genus *Matus* existed long before the Pleistocene in North America, and that by the beginning of the ice age it was already differentiated into distinct "species" of which at least two survived in the Appalachian region. With the spread of the glaciers the populations of these forms spread south and in places were isolated on the Pleistocene islands of Florida and probably elsewhere along the Gulf Coast. The isolation seems to have been partly due to geographic and partly to ecological factors. Many of these isolated populations still exist, and the study of the relationships of these relict forms to one another promises basic data for evolutionary and ecological theory.

In conclusion, I would like to say that I hope that I have made the point that modern taxonomy offers a worthwhile way of looking at biologi-

cal problems. It is not entirely the "cut and dried" process of describing new species, preparing keys, and making determinations, but involves the consideration of data from many other fields and promises starting points for many other kinds of investigation. "Intuition" still plays a large part in taxonomy, but the results of intuition should be the beginning not the end.

Literature Cited

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