Smith: It is true that the 20 minutes separating these two campuses should not be insufferable, but at Michigan or Harvard students can drop in when they have a free hour or so, which is an advantage. From Lamont to Columbia takes an hour; it also takes about an hour from University of Maryland to the Smithsonian.

Traub: In studying biology students should be working more and more with living materials, plants and animals. Do you think then that the museum should take on the functions of zoos, aquaria and botanical gardens, so as to get more of this in one institution instead of several as in most cities?

Smith: Much university instruction is based on living plants or animals and this is entirely appropriate But such observation could never give the student a really full view as to what exists in nature simply because of lack of time. If he is studying a genus of 500 species, he is fortunate if he sees 10 of them alive. It is the same for space—he may not even be in a center of distribution for a family. In the museum we have a concentration of the results of the field work of many individuals—the results of thousands of years work if it had been undertaken by one person. The student sees the results of all this time period brought together in species from all over the world. Students need aquaria and zoos along with museums. It is not necessary that collections of living materials and dried material be kept together, but if they can be so much the better. That is easier at a university museum. However, no zoo, aquarium or botanical garden could have as many samples of different organisms as a museum has.

Admiral Thomas: Would it be possible to have a procurement office to procure specimens?

Smith: It does not seem a very feasible way to assemble museum collections, which are built by individual collectors and by exchange relations. Museums bring together material in as many groups as possible and it is available when the student comes along. After the student comes on the scene it is too late to start assembling the material.

Gressitt: Thank you Dr Smith and Dr Allen. Our next speaker is Dr John Hendrickson, Vice-chancellor of the East-West Center, and former Professor of Zoology at the University of Malaya. Discussant will be Mr Vernon Brock, Director of the Hawaii Marine Laboratory, University of Hawaii.

THE RELATIONSHIP OF ECOLOGY TO SYSTEMATICS

By John Hendrickson

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As an ecologist, my interest lies in the study of the ecology of a particular group of animals in Southeast Asia. As systematics plays a vital role in the study of ecology, I must also be a systematist before I can do a good job in ecology and I have never finished the systematic part of my job. I therefore chose, for obvious reasons, the profferred subject "the relationship of ecology to systematics." I am a man of somewhat strong opinion. I know there may be many among you who will disagree with some of the conclusions I have to present here. I welcome this. I am sure that out of any discussion which might arise, both I and possibly you may benefit.

Ecology is the study of the relationship between organisms and their environment. I like to think of it as nature's housekeeping. This involves everything that goes on in the house including the manner in which the inhabitants affect each other and extending down to the household budget of available energy—how it is distributed and, at times, adjusted. The house is so vast, its inhabitants so varied and their inter-relationships so ineffably complex that only small portions of the problem can be successfully attacked by an individual worker at any one time.

Systematics is the ordering of knowledge implicit in the very meaning of science and is an integrating part of any science. In biology—the science of living things—it is the framework on which the whole structure rests. It is difficult to be a competent biologist of any sort, certainly impossible to be a good ecologist, without being something of a systematist. One must know where the subjects of a study fit into the general inventory of nature's house if the ecological observations and conclusions are to have any meaning to the rest of science.

As I stated, in my own particular case, I wanted to study the ecology of the amphibians and the reptiles of the Malay Peninsula. The systematics of these animals in that area were so inadequately worked out that I had to become a systematist before I could meaningfully work in ecology. I realized the hard facts in about 1952 and the remainder of my time since has been spent in attempting to adequately identify and classify the animals I wish to study, in order to sensibly report on their habits. I continued to observe their habits with great interest and great profit, but I have been held back from reporting these observations because I could not readily identify the animals. I could not relate them satisfactorily to each other, and I could not fit them into the general pattern of the household affairs of nature without a systematic background. In the process of becoming a systematist I have been deeply impressed, not only with the irrevocable dependence of ecology upon systematics but also with the inescapable dependence of systematics in a modern day context upon ecology.

I believe that it is pertinent at this time when we are dedicating this imposing new building at Bishop Museum—a building which will in the minds of most of us, be devoted to work in systematics—to stress the fact that the men and women who will work with the collections to be stored and studied here will not be merely giving names, fixing labels and storing the specimens away in orderly systems. They will be not only providing a biological specimen library, complex and essential as that may be; they will be in fulfilling the requirements of modern systematics, working in and contributing to all fields of biological effort. This includes biochemistry, embryology, animal behavior, ecology and many others, but most of all ecology. This will have major effects on the composition of the staff working here and on their field activities in far-off places. It will affect the distribution of the never sufficient space, and the sharing of the available budget funds. It will even, if I may say so, affect the sounds and smells which will emit from these rooms. Let us consider for a minute the activities of a modern systematist, attempting to break them down somewhat artificially into various levels of function.

First, identification. This is the primary level of activity and the one which is most widely understood by the general public. It is a time-honored function of museums and is important as a basic service. It consists of the recognition and naming of representatives of the vast array of different forms of living things. What is required is the enumeration of the most obvious and constant characters peculiar to a form, enabling its distinction from other forms. It may well be handled by computers some day. Ecology is of little importance in this function, but plays a major part in the use to which the identification is frequently put. Once a specimen is identified, we use ecological knowledge to plan the ways of getting along with it—whether we fight it or encourage it or put it to some use without either fighting or encouraging it—if it impinges on human affairs in any way. For instance, where and how to spray insecticides, how to seek out natural enemies, how to provide food or shelter, etc. One can think of many examples in everyday life of human manipulation of other forms of life for human benefit, be it esthetic or economic. This is the first level of systematic work as I see it.

The second level may be rather artificial to many of you, but I make it a distinct level. I call it delimitation. This consists of setting the limits on any one natural group. It requires the detection and description of how truly the group exists as a common interchanging pool of genetic material and how much detectable variation occurs naturally within this genetic pool. The lumper/versus/splitter argument with which taxonomists are so familiar occurs at this level. What are the limits of variability within a species? Should any particular group showing variously graded differences between individual specimens be given one name or should they be separated and given several names? This function requires judgement; it is frequently arbitrary in the absence of information from other fields of biology. Ecology is of prime importance here because it deals with the most common and the most effective phenomena of natural existence which tend to split up gene pools or to blend them back together again. The importance of this level of systematic activity in strictly human affairs may be illustrated by the classical case of the mosquito *Anopheles maculipennis* which was described in 1818 and was well known as a species distributed over much of Europe. When malaria was identified as a mosquito-borne disease, *maculipennis* was identified as a major vector of the disease over much of the continent. Much money was spent and great effort expended to control this mosquito. Oddly enough, in some areas this campaign to control *maculipennis* mosquitoes drastically reduced malaria as expected, but in other areas it had almost no effect. Eventually it was discovered that the species then known as *maculipennis* was composed of several closely related but ecologically (and actually, morphologically) distinct forms which look very much alike but have different breeding habits, host preferences, etc. It was found that the mosquito being controlled in one place was not transmitting malaria in other areas, so the malaria cases there continued despite the control programs. After the realization that not one but several species or gene pool groups were concerned, it was possible to direct control measures toward the one particular vector species which was important in each area and the control work became effective in all areas.

The third level of systematic work goes into the stage of synthesis which is an important part of all sciences. First, we have analysis followed by synthesis. At this first synthetic level of function, systematics takes groups of facts and builds them into something which is more than the simple sum of all the parts. It involves the combination of unit groups which have overall affinities and the establishment of an hierarchial order among the groups. (The order imposed here is subject to revision at the next-following stage where data bearing on phylogeny are considered.)

At the previous level of delimiting taxa, ecology contributed much in the form of positive data which could be immediately utilized in arriving at decisions and forming groups. At the present synthetic level of assemblage into larger groups, oddly enough, ecological situations seem to operate in a somewhat reverse manner, more often preventing unreal assemblages than producing assemblages. Many of the older groupings were made largely or solely on topological grounds—using things which could be counted and measured. Many of these older groupings are now being shown through ecological evidence to be false, thereby causing a regrouping compromise between taxonomic convenience and evolutionary fact. Here ecology serves more as a guard than a contributor. For example, on purely topological grounds, porpoises might be grouped with fishes; or caecilians, snakes and limbless lizards grouped falsely together. This would result were it not clear that the similar topologies in these types were the end products of adaptation by dissimilar animals to similar ecological situations. It is even possible to illustrate adaptation to different ecological situations resulting in similar topologies which are false bases for groupings.

I wonder if you have ever thought about one interesting aspect of the differences between a gibbon and a man. If the skeleton of a gibbon is compared with a human skeleton, many remarkable topological similarities are immediately apparent. They are the result of the upright posture. Only through ecological grounds is it possible to demonstrate that the gibbon developed his upright posture by hanging down and the human developed his by standing up. The bases are totally different; the grouping, if made as close as topology alone would indicate, is false. We depend upon ecology to guide us away from this. The formation of multivariant entities into groups is a very intricate process and one which should in time receive great stimulus from the application of mathematical set theory and symbolic logic, allowing incorporation of ecological knowledge on a compromise basis and possibly stimulating an objective mathematical approach to the problem, which has not yet been possible as a standard working method. To date we often say that a good group is one which a competent man says should be a good group, but that is not enough if we strive for continual refinement. Without compromise between ecological fact and topological mechanics we risk losing touch with the reality of the natural world and ending up with a meaningless, useless system.

In southeast Asia, the ecology of the frogs and toads I have chosen to work with has not been extensively studied. Although both adult and larval forms are often known, they frequently have not been correctly matched as to species. As more larvae are reared to metamorphosis and more eggs of known parental origin are hatched, we are increasingly gaining evidence that these animals are better named than we realized "Amphibia." Amphibious means leading two lives. We realize that the name is more appropriately suited than we thought, and that adaptation, natural selection and evolution may follow drastically different courses at the two life levels of larva and adult, producing such enigmas as larvae which are almost carbon copies of each other from radically different adults, and vice versa. Some of the species groupings suggested earlier because of very similar larval forms must be discarded completely, throwing the whole systematics into temporary confusion. Other groupings which were not admitted at all because of drastically different larvae must clearly now be considered on entirely new grounds. Ecology and ecological knowledge has caused a major shift and reshuffle in the classification system of the class Amphibia in SE Asia.

The fourth level of systematics is the higher level of synthesis—the construction of phylogenies. After identification, delimitation and an attempt at synthesis, it is now the duty of the systematist to design a system of classification to conform as closely as possible to the realities of actual existence. As George Gaylord Simpson puts it "A systematist must construct a phylogeny which would approximate or in some estimable way reflect the order of nature." The system of classification then should be more than a pigeon-hole system or a convenient and accurate index; it should show as a kind of model, the evolutionary history of the living forms we are dealing with. Now this goal is impossible of complete attainment. Nevertheless, we are irrevocably committed toward approaching that goal as closely as we can. Ecological phenomena were at the bottom of most of the major divergences which occurred in the course of organic evolution. Anatomical phenomena operate and have operated to mask some important divergences. A broad base in ecology is the only real insurance the systematist has against many errors. The first fishes to leave the water and take to land did not do it because of any inner compulsion to become amphibians, or because they were filled with the spirit of adventure; they were making a simple ecological adjustment and finding a new and better way to live in nature's vast household. It was a minor ecological adventure which led to a major evolutionary diversion.

The last level of what I think of as systematic endeavor has been recognized only for a few years. It is very much a development of our times and I myself have personally known and talked with many of the men concerned with the new development. Since about 1940 when Julian Huxley identified what he called the "New Systematics," systematists have progressively realized that they were not in fact confined in their discipline to speculation and long-distance detective work concerning the ancient and untestable major phenomena of evolution in the form of phylogenies. By observation and experiment on populations of living organisms it is possible to study the why and the how of evolutionary processes going on right now all around us. With the growth of genetics, biochemistry, ecology and similar disciplines, the systematist can synthesize knowledge from many disciplines and, in repeatable form, conduct experiments which give a fuller understanding of the evolutionary background of particular groups and of the process of evolution itself.

This is in the nature of a breakthrough phenomenon. It is the basis for a tremendous spurt of activity in systematics. And now—if never before—as he tries to create and manipulate small evolutions in his plant and animal populations, the systematist will recognize the inextricable linking of systematics and ecology.

Brock: I would like to first comment, since I have the microphone, that I have had some slight involvement with systematics of fishes and have observed some of the trends of recent years. This is from the point of view of fish research which is going in the direction where conventional categories used by systematists, such as the terms species or subspecies, lose their value. These don't identify groups the fisheries people need to work with. They need to work with a breeding, homogeneous entity, which in some terminology is called a subpopulation. This cannot often be recognized by external characteristics. Some of the means of identification relate to blood groups, which presumably are not affected by external, environmental factors or selective, evolutionary factors. This has lead the fishery student in some areas to deal with populations that have no decent terminology, so to speak. This is the present situation with the tunas, for example.

Mr F. A. Bianchi (Hawaiian Sugar Planters Association Experiment Station): I think Mr Brock said something controversial when he said that blood groups should not be subject to forces of evolution. Please explain.

Brock: Perhaps to the obvious forces. These seem to be groups for which it is difficult to envision any selective factor which would choose one over the other. This is perhaps a statement which reflects our ignorance rather than a statement of fact. They seem less susceptible to selection since they seem to have no obvious application, so to speak, in the animal as other characteristics might have—as the external morphology might have.

Hendrickson: I think that Brock's specific subject is far more interesting than my attempt to cover the general field. However, if it were not for strictly ecological work, you would not have realized that this particular gene pool existed and these systematics would not exist at all without the ecological knowledge. Scientists who are "ecosystematists" must be developed. There is no reason to have ecology and systematics as separate disciplines.

Brock: I agree with you in this regard.

Traub: I agree with most of what Dr Hendrickson said, and think he did it very well. But I question the statement that ecology does not contribute to systematics. Time and again we have found mosquitoes, mites, or rodents which do not act as they should. Tree-inhabiting and ground inhabiting rats are given the same name. This makes you reconsider your taxonomy and you find often that you have been dealing with different animals.

Hendrickson: I think I listed five levels of systematics. One of these is called the computer level, and that is the only one that ecology is not involved with.

Allen: In your use of the terms "good groups" and "bad groups", are you referring to taxonomy or biological control?

Hendrickson: A group of limbless animals running all through the Animal Kingdom is a bad group, based purely on topology with no relationship to the actualities of animal existence. A grouping which can be referred to a gene pool reality would be a good group. I object to the ancient and still current tendency to use topology—the bristles you can count, or not count—rather than the realities of the dynamic living animal. I tried to avoid using the word taxonomy and spoke only of systematics. But some people have adhered to the idea that taxonomy and systematics are synonymous.

Gressitt: Thank you Dr Hendrickson and Mr Brock. Our next speaker is Mr Curtis Sabrosky of the U. S. Department of Agriculture at the National Museum. He is one of our Bishop Museum collaborators who participated in the field work on our "Insects of Micronesia" project. Discussant will be Dr L. W. Quate of the Museum.