from the period 1859–1957 as if they were new in 1976, while not even mentioning what does not fit his notions.

REFERENCES

BALL, I. R. 1975. Nature and formulation of biogeographical hypotheses. Syst. Zool. 24:407–430.

CROIZAT, L. 1964. Space, time, form: the biological synthesis. Published by the author, Caracas.

VAGVOLGYI, J. 1975. Body size, aerial dispersal, and origin of the Pacific land snail fauna. Syst. Zool. 24:465–488.

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Why are so many Minute Land Snails on the Pacific Islands: A Response to Leon Croizat

I did not intend to crusade against the theory of panbiogeography, nor did I aim to refurbish the dispersalist model. I simply studied the origin of the Pacific land snail fauna, and concluded that land snails immigrated to the Pacific islands primarily through aerial dispersal. My reason for reaching this conclusion was the observed fact that minute genera, whose shells measure less than 10.0 mm (and most often less than 3.0) occur in significantly greater numbers on the Pacific islands than they do on the continents (60.0 vs. 27.1%). The demonstration of this phenomenon—in however simple statistical terms—was new; it did not originate from the period 1859–1957. Several alternative hypotheses were considered and discarded; two such hypotheses: one, of a mid-Pacific continent, and the other, of mid-Pacific islands, were rejected not because they were similar to vicariant models but because they were unsupported, inconsistent and self-conflicting. At heart of the issue lies the overabundance of minute genera in the Pacific area. If Croizat does not believe that aerial dispersal is responsible for this phenomenon, he should propose a more convincing explanation.

Instead, Croizat engages in what appear to me irrelevant or erroneous arguments. Thus, whether my method was inductive, or deductive, seductive or productive—what other methods can you use? Whether I believe in Wallace's Line—which I do not (see fig. 3 of my article)—I doubt *very* much that dispersal is irrelevant to speciation, however crystal clear that may appear to competent biogeographers like Croizat. And I am positively puzzled on what grounds did he proclaim my propensity to the principles of Ptolemy? Perhaps from a pan-paleo-psychiatric projection of my purported personality? *Parbleu*!

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Science, Philosophy, and Systematics

Popper (1959:62) distinguishes between *strictly universal statements*, which are not spatiotemporally restricted and are applicable to any number of singular cases, and *numerically universal statements*, which are spatiotemporally restricted and refer to a finite number of cases. Popper believes scientific theories and natural laws are examples of the first kind of statement. Strictly universal statements are, in principle, falsifiable but not verifiable, whereas numerically universal statements are both falsifiable and verifiable, at least in principle. A growing number of systematists have begun to adopt some of Popper's viewpoint of science and have treated systematic hypotheses as only falsifiable, and thus ostensibly as strictly universal statements. Recently, Kitts (1977) has criticised these systematists on the grounds that since systematic hypotheses are numerically universal statements—taxa being spatiotemporally restricted and capable, at least theoretically, of enumeration—they can be both falsified and verified.

Popper admits that the distinction between the two kinds of statements is sometimes ambiguous (p. 63): "the question whether the laws of science are strictly or numerically universal cannot be settled by argument. It is one of those questions which can be settled only by an agreement or a convention." Kitts (1977:186), too, notes this problem: "not only logic, but also English grammar fails to make a distinction between the two kinds of universal statements." The crux of the issue, as Kitts apparently sees it, is that systematic hypotheses are verifiable, *in principle*, and in arguing for this position he discusses many issues of importance for systematics. While willing to accept Kitts' basic conclusion, an interested systematist still might wonder, What does it all mean? Where do we go from here? Kitts, it seems, provides no answers to these questions. Yet, it might be claimed that philosophical considerations are pertinent to the science of systematics, if for no other reason than systematists believe that they are.

Despite a multitude of viewpoints within the philosophy of science, most characterize scientific theories or hypotheses as providing a basis for predictions which, in turn, serve as tests of the original theory or hypothesis. There is an expectation—a prediction—that certain observation relationships will hold given the initial premises. If these observations hold more often than not, the hypothesis is said to have some explanatory power. While there is a considerable diversity of opinion in philosophy about the nature of theories, predictions, tests, and the verification and falsification of hypotheses, the above characterization seems basic to most philosopher's beliefs.

To what extent has this view of scientific hypotheses been incorporated into systematics? If one can judge by the content of the major books on systematics, the answer is not at all. A perusal of Simpson (1961), Hennig (1966), Mayr (1969), or Sneath and Sokal (1973) provides little comfort for the student interested in the scientific method of systematics. Among recent books only that of Ross (1974) exhibits some attempt to make use of these concepts.

The recent interest in Popper and the applicability of his ideas to systematic analysis can be taken as evidence that systematists are increasingly dissatisfied with past traditions. But should critics, such as Kitts, be directing their comments toward advocates of a strong hypothetico-deductive framework in systematic biology or toward the lack of such a framework in the past traditions. The distinction was not made by Kitts.

There are two main endeavors of systematics to which a hypothetico-deductive method may apply—phylogenetic analysis and classification. The first has received the most attention, mainly by the followers of Hennig, and the details of their efforts need not be discussed here (see Gaffney, 1975; Wiley, 1975; Engelmann and Wiley, 1977; Platnick, 1977). The main goal of these and other workers has been the reconstruction of life's history, and they have all adopted the assumption that all such attempts must be considered hypothetical. But what exactly is being reconstructed? In the main it must be the *pattern* of similarities and differences that have resulted from the phylogenetic process and not the process itself. More precisely, the pattern is the nested sets of synapomorphies postulated amongst taxa; relationships, whether of common ancestry or ancestral-descendant, are defined in terms of that pattern. If it is accepted that the geometry of phylogeny is one of branching and divergence, then the patterns of synapomorphous similarities can be pictured in terms of a branching diagram, the cladogram, without any consideration of the two kinds of relationship that might be thought to exist (Tattersall and Eldredge, 1977). Cladograms are hypotheses about pattern; congruent synapomorphies are the predictions and the tests (Wiley, 1975). Are these hypotheses to be treated as strictly or numerically universal statements? In practice it probably does not matter. They deal with taxa, spatiotemporally restricted, and capable of enumeration. But can they, therefore, be verified, even in principle? Each hypothesis (cladogram) is based on other hypotheses (synapomorphies) that are, in turn, based on observations that themselves are theory-laden (what are similarities, how are they to be recognized, etc.); tests of the cladograms are also hypotheses (newly discovered synapomorphies). Could such a complex nested hypothesis-structure ever be verified? It would seem not. The important point is that cladograms can be falsified if an observed similarity fails to define a set of taxa within that cladogram; such failures are termed nonhomologous similarities (convergences) for that cladogram. The cladogram with the fewest nonhomologous similarities (falsifications) is to be preferred (Wiley, 1975).

The realization that "phylogenetic analysis" is primarily the interpretation of synapomorphy patterns was introduced into systematics by Hennig. It can be suggested that the failure of other theoretical-methodological schools to accept this fundamental research strategy has prevented them from incorporating a hypothetico-deductive method.

What of classification? Are classifications hypothetical and thus testable? Kitts (1977:189–190) seems to be ambivalent: "Classifications are not meant to explain. They are meant to be explained .... And if they are interpreted as systems of hypotheses about associations of character states they can be falsified. And if, moreover, they are interpreted as systems of numerically universal hypotheses about associations of character states they can be verified as well." If one denies, as does Kitts, that classifications are theories and hypotheses, what would be the purpose in testing them? If classifications are hypothetical in structure, what is the hypothesis and how is it to be tested?

The avowed goal of most systematists is natural classification. How is "natural" to be interpreted? Most answer that a natural classification reflects the phylogenetic history of the taxa being considered. But to the extent that systematists can have different views of the phylogenetic process and of the ways in which that is to be "reflected," there can be alternative classifications (Cracraft, 1974). Given this situation the hypothetical nature of classification is not readily apparent, and thus neither are the tests. If one advocates that several classifications may be "consistent" with a given phylogenetic hypothesis as do evolutionary systematists (Simpson, 1961; Mayr, 1969; Bock, 1973), what is the hypothesis and what is the test?

A solution to the above problem might be that classification be constructed so as to mirror the hypothesized nested patterns reflected in the cladogram. If patterns of synapomorphies are accepted as valid estimations of the result of life's history, then they also would seem to be a useful basis on which to formulate "natural" classification. If one adopts the convention that classifications should precisely translate the nested sets of taxa in a cladogram, then in that sense such a classification might be considered hypothetical. But it is the cladogram that serves as the basis for the classification, so extended discussions about whether classifications are scientific hypotheses and capable of testing would seem to be beside the point.

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REFERENCES

BOCK, W. J. 1973. Philosophical foundations of classical evolutionary classification. Syst. Zool. 22:375–392.

CRACRAFT, J. 1974. Phylogenetic models and classification. Syst. Zool. 23:71–90.

ENGELMANN, G. F., AND E. O. WILEY. 1977. The place of ancestor-descendant relationships in phylogeny reconstruction. Syst. Zool. 26:1–11.

GAFFNEY, E. S. 1975. A phylogeny and classification of the higher categories of turtles. Bull. Amer. Mus. Nat. Hist. 155:387–436.

HENNIG, W. 1966. Phylogenetic systematics. University of Illinois Press, Urbana.

KITTS, D. B. 1977. Karl Popper, verifiability, and systematic zoology. Syst. Zool. 26:185–194.

MAYR, E. 1969. Principles of systematic zoology. McGraw-Hill Book Co., New York.

PLATNICK, N. I. 1977. Cladograms, phylogenetic trees, and hypothesis testing. Syst. Zool. 26:438–442.

POPPER, K. R. 1959. The logic of scientific discovery. Basic Books, New York. (1965. Harper Torchbook, New York).

ROSS, H. H. 1974. Biological systematics. Addison-Wesley Publ. Co., Reading, Massachusetts.

SIMPSON, G. G. 1961. Principles of animal taxonomy. Columbia University Press, New York.

SNEATH, P. H. A., AND R. R. SOKAL. 1973. Numerical taxonomy. W. H. Freeman and Co., San Francisco.

TATTERSALL, I., AND N. ELDREDGE. 1977. Fact, theory, and fantasy in human paleontology. Amer. Sci. 65:204–211.

WILEY, E. O. 1975. Karl R. Popper, systematics, and classification: a reply to Walter Bock and other evolutionary systematists. Syst. Zool. 24:233–243.

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Classification and Prediction: A Reply to Kitts

The terms "prediction" and "predictivity" have been bantered about in systematics for some time, most recently by Kitts (1977):

> "The claim of some taxonomic methodologists that classifications are predictive seems to amount to a claim that taxa are equivalent to strictly universal hypotheses about character state associations" (p. 188).

By "strictly universal hypotheses," Kitts refers to Popper and the idea of a statement that "claims to be true for any time and any place and for an unlimited number of individual cases" (p. 185). By "character state associations," he refers to the occurrence, for example, in mammals of milk glands and ear ossicles. He considers that predictions derived from classification take the form of predicting that "some character states would continue to be associated in the future course of evolution" (p. 189).

I believe that Kitts is correct in his belief that such concepts float through recent systematic literature, and I believe that he is correct in his criticism of them. He frames his criticism within the context of the philosophy of Popper, and he concludes that biological classification concerns not *strictly universal statements*, but rather *numerically universal statements*, "which refer to a finite class of elements within a particular spatiotemporal region" (p. 185). For him the conclusion is important, for it allows that classifications can be verified as well as falsified, and it removes classification from the category of "natural laws" as conceived by Popper.

I think that Kitts may be premature in this judgement, not because his conclusions do not follow from his argument, but rather that his argument neglects another kind of prediction—the only useful kind that I can imagine is derivable from classification, and that I would illustrate