Book Reviews

The First Decade of Numerical Taxonomy

Numerical Taxonomy. The Principles and Practice of Numerical Classification. PETER H. A. SNEATH and ROBERT R. SOKAL. Freeman, San Francisco, 1973. xvi, 574 pp., illus. $19.50. A Series of Books in Biology.

A well-worn path leads from revolution to respectability. Ten years ago, R. R. Sokal and P. H. A. Sneath published their *Principles of Numerical Taxonomy* and became the *enfants terribles* of a discipline not noted for innovation. They have since achieved at least a stalemate, perhaps even a victory. In this context, they have rewritten their manifesto. What task can be more difficult than the second edition of a rebellious work that succeeded?

The dust jacket of the first edition proclaimed, in publishers' best prose:

> Numerical taxonomy is a revolutionary approach to biological classification. . . . Instead of qualitatively appraising the resemblance of organisms on the basis of certain favored characters, a taxonomist using this new methodology will attempt to amass as many distinguishing characters as possible, giving equal weight to each. . . . The aim of the new system is to rid taxonomy of its traditionally subjective nature, so that any two scientists, given the same set of characters but working entirely independently, will always arrive at identical estimates of the resemblance between two organisms.

The opposition of traditional systematists centered on two issues:

1) The equal weighting of characters. Traditional classifications are supposed to reflect phylogeny. They do this by emphasizing those features that organisms share by common descent. We give greater weight to the presence of epipubic bones than to bipedal posture: kangaroos and koalas are close phyletic relatives, kangaroos and men are not. Instead, Sokal and Sneath urged the wholesale collection and processing of unweighted characters: moreover, they abandoned the criterion of phyletic consistency, arguing that the most "natural" classification would group together those organisms having most characters in common. (They did not, of course, deny that such classifications might reflect phylogeny as a secondary property.)

2) The cult of experience versus the new vulgarians. Traditional taxonomists could assign their weights because they had spent enough time with their groups to know them as we all know (and unerringly distinguish) human faces. Sokal and Sneath replaced seasoned experience with a heartless computer. Any characters would do as long as you had enough of them. An "intelligent ignoramus" might produce as good a classification as an expert. Anyone could do it; in fact, if measurements could be made automatically, then no one might do it.

In the acrimonious debates that filled the pages of *Systematic Zoology* for more than five years, an important distinction was usually blurred or ignored: the numerical taxonomists were trying to do two very different things. One can easily, as I do, accept one aim and reject the other.

1) The use of numerical methods in systematics. Since the acceptance of evolutionary theory, the field of systematics has undergone only two great changes in procedure. The "new systematics" of the 1930's and 1940's replaced typology with population thinking at the species level and produced an essential correspondence between the taxonomic species and a real unit in nature: the interbreeding population. The spread of numerical methods in the 1960's may have had an even broader impact, for it extended above the species to all levels of the taxonomic hierarchy. It replaced the subjective sorting of units into rigid pigeonholes with an objective assessment of continuous variability. It required new skills, new curricula, and new sources of funds. The numerical school of Sokal and Sneath spearheaded the movement, but they did not create it. The development of high-speed computers guaranteed that multivariate procedures long known in theory would be applied extensively. But evolution would have triumphed without Darwin, and still we praise him.

2) The phenetic philosophy. Sokal and Sneath rigidly tied their numerical skills to a definite philosophy of classification. (Yet the same numerical methods could be used in the service of an uncompromisingly orthodox evolutionary systematics—we may value an objective display of morphological differences among forms for many purposes; yet one need not construct a classification from one's phenogram.) They made a methodological claim—that taxonomy could be rigidly empirical, objective, and repeatable (that, in other words, this most subjective of biological endeavors could become a "hard" science). And they advocated a set of procedures to guarantee that method—large numbers of well-distributed characters, equal weighting, calculation of a similarity matrix for all groups, and definite procedures for the construction of dendrograms from the matrix.

During the past decade, numerical taxonomists have identified themselves as a "school." They have organized symposia, met among themselves, publicized and polemicized. They have provided an object lesson for those who think naively that science is moved by the press of disembodied ideas. Their success in spreading the use of numerical methods has been unbounded. The polemics inspired by suspicion of the computer have all but disappeared; the Luddites are silent. On the other hand, the phenetic philosophy of classification has made little headway.

Enter, at the end of this decade, a new edition of the founding document, this time by Sneath and Sokal. I had expected a work of courtly compromise in the best tradition of successful assault—an account of the utility of numerical methods, a clear introduction to their use for the uninitiated, and a quiet retreat from the precepts of phenetic philosophy that have not worn well over the years. Instead, the authors hold firmly to the phenetic line. But ten years after its greening, this philosophy has lost much of its luster.

We are still told (pp. 5 and 418, for example) that taxonomy should be objective and empirical. Yet different methods of clustering yield different classifications, and the authors now doubt whether a "best" method even exists: "We still do not know a method for an optimal taxonomy (or if one exists) and therefore cannot advocate one" (p. xiii). Moreover, the hypothesis of nonspecificity is in disarray (pp. 97, 100–102, 289, 426). Characters of different status (morphological versus biochemical), from different stages of ontogeny, from different parts of the body, or even different (and large) subsets randomly selected from a total array, can yield rather disparate classifications: "Had the hypothesis of nonspecificity been fully valid, any set of characters would lead to sample estimates of parametric similarity value and the question of number of characters would simply be a statistical one" (p. 106). We shall, after all, need taxonomic experience to select best characters and best classifications. A classification is not an observation; only the discredited inductivist model of science would ever lead us to believe that it might be. *A classification is a human decision, constrained by a bevy of facts, about how best to order nature.*

The equal weighting of characters is still championed (pp. 109–113), but phenetic similarity is redefined (p. 29) to allow for unequal weighting as long as its basis is explicit: "It would seem that phenetic similarity can be based on equally or unequally weighted characters as long as the operation for obtaining the similarity has been defined explicitly by the investigator" (p. 29). Yet once the door is opened to weighting, the myth of objectivity can scarcely be maintained; for who can decree a universal method for assigning weights?

Our rebels have mellowed; and how could it be otherwise, for they are not dogmatic men. I am only a bit sorry that the dead hand of their first edition has led them to reassert claims and reopen issues for which they did not fare well during the decade of debate. The philosophy of evolutionary systematics has survived, and largely prevailed over the original phenetic credo. But the practice of evolutionary systematics will never be the same again.

The first edition has also exerted an unfortunate tyranny in some technical matters. Cluster analysis on coded characters was favored in 1963, but ordination based on continuous characters has deservedly grown in popularity since then. As Sneath and Sokal admit (p. 246), "Three-dimensional plots or models of a group of OTU's [operational taxonomic units] have become an almost standard procedure that may replace the dendrogram as the most common method of representation of taxonomic results." Yet ordination receives only nine pages, while dendrograms and their construction are afforded more than 100.

In all other respects, the new edition leaves its predecessor far behind. It is a tribute to the success of the first edition that it seems so archaic only ten years later. Numerical cladistics did not exist in 1963; it now merits a full chapter. The long section on taxonomic structure (pp. 188–308) presents clearly such a rich array of phenetic techniques that it compares to the first edition as *Don Giovanni* to Mozart's childhood ditties.

Yet the finest proof of the authors' success is their assiduous and exhaustive catalog of numerical publications during the past ten years. (It is, perhaps, a bit overzealous. Our authors are not godfathers to all these works. The umbrella of numerical taxonomy cannot shelter every paper that applies multivariate methods to a biological subject.) The bibliography alone spans 60 pages. An appendix lists multivariate studies according to taxonomic group. Another chapter traces the use of numerical methods in related fields (from carbonate sedimentation to Latin elegiac verse).

The last chapter contains some intriguing hints that the authors recognize that their chief contribution is as advocates of numerical methods, rather than as proponents of the phenetic philosophy. For they defend as interesting in themselves numerical results and procedures that undercut the phenetic credo. Thus, a failure of nonspecificity is welcome on p. 432: "Incongruence between floral and vegetative characters must have biological meaning." And a procedure for weighting characters in ecological studies is defended on p. 437.

By accepting evolution rather than God as the ground of taxonomic resemblance, Linnaeus could have functioned quite well as a systematist (at least for higher taxa) well into the 1950's. Today, he would have to retool. Numerical taxonomy has revitalized an ancient subject. One of the stuffiest areas of biology has become one of the most exciting.

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The Nature of Early Learning

Imprinting. Early Experience and the Developmental Psychobiology of Attachment. ECKHARD H. HESS. Van Nostrand Reinhold, New York, 1973. xvi, 472 pp., illus. $19.50. Behavioral Science Series.

When recently hatched birds such as ducklings are hand-reared for a few days they strongly prefer the company of their human keeper to that of their own species. The remarkable process which can so dramatically influence the development of social relations is called "imprinting" and is justifiably famous. Its fame has been largely due to the popular writings of Konrad Lorenz and, more recently, of Eckhard Hess, the author of this new book. Apart from being one of the first to investigate the process experimentally, Hess has continued to hold distinctive, if not widely shared, views on its nature. It is appropriate, therefore, that he should have presented his book as a "personal view" rather than as a critical review or as a synthesis of available evidence.

The main thesis of the book is that imprinting is quite unlike other forms of learning. Hess believes that the mechanism evolved long before mammals and birds separated from reptiles as distinct taxonomic groups. Even when the phenomenon was not part of the repertoire of a species, he argues, the process remained available as a tool for future times when the pressures of natural selection would require individuals to learn something fast at a particular stage in their lives. I am not convinced that Hess has adequately thought through these ideas about the evolution of imprinting. Nevertheless he uses them as justification both for isolating the work on imprinting from other studies of learning and for generalizing from birds to man.

Indeed, Hess seems to have such a clear view of the unique nature of imprinting that any evidence challenging that view must be irrelevant by definition. How else can one explain his determined attempts to reestablish claims which he first made some 15 years ago and which have been vigorously attacked on empirical grounds ever since? For example, he argues that imprinting must take place within a "genetically programmed" period early in the life cycle and that the program switches off sensitivity at a sharply defined point after hatching. To maintain such a position, he must either