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

ignore or misrepresent strong evidence indicating that the length of the period of sensitivity greatly depends on the nature of the young bird's experience.

Curiously enough, the assertions about the uniqueness of imprinting are preceded in the book by sections containing much of the evidence incompatible with those claims and many of the criticisms of the way Hess has drawn strong conclusions from inadequate data. Indeed, the literature survey strikes me as remarkably fair and can be criticized only for mild inaccuracy and for incomplete coverage. The "personal view" which characterizes the rest of this book is almost absent in these review sections; they might almost have been written by somebody else. Hess has further accentuated the isolation of the literature survey from the main theme of the book by dealing with his own findings separately from those of other experimentalists. Disarmingly he justifies this separation on grounds of his own distinctive "theoretical bias." It could be argued, however, that just for this reason he should have presented all the evidence relevant to a controversial issue in the same place.

When Hess does eventually attempt to deal with his critics he accuses *them* of "theoretical bias." Because of their blinkers, he argues, they have failed to appreciate that they have been studying, not imprinting, but other quite different processes such as "socialization" or "ordinary learning." They obtained evidence incompatible with his claims because, for example, their birds were much older than his. Whether all the damaging evidence can be dismissed quite so easily is seriously open to doubt; and it must be stressed that most people currently working on imprinting have accepted the need for fresh thinking in order to cope with the complexities uncovered by experimental analysis. Hess can, of course, still defend the purity of his original conception of imprinting by postulating more and more processes superimposed on what he regards as the genuine article. The net result, however, is an increasingly unwieldy assemblage of unrelated explanations, and the enterprise begins to look like a desperate attempt to prop up a tottering and elderly edifice built on sand.

But if he has not updated his thinking, Hess has made a determined effort to change his methods in recent years. The final chapter of his book describes how, after a lifetime of working in the laboratory, he has started to investigate what happens under natural conditions. I am sure he is right about the insights which such studies can bring and also agree with him when he emphasizes the perceptual constraints on imprinting and the strong predispositions of the young birds to learn certain things at particular stages of development. He attributes the development of such internal control to "genetic programming," but these ideas are vague and, by modern standards, unsophisticated, and Hess never develops them to the point where they could be tested. Analytical thinking is not his strong suit. His gift is as a vivid expositor, and he undoubtedly writes in a way which catches the imagination of the uninitiated. So even though much of his book is misleading and its theoretical arguments are stale, it may, nevertheless, stimulate many people to find out more about the striking phenomenon it describes.

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Sub-Saharan Mathematics

Africa Counts. Number and Pattern in African Culture. CLAUDIA ZASLAVSKY. Prindle, Weber, and Schmidt, Boston, 1973. xii, 328 pp., illus. $12.50.

Africa Counts explores two interrelated topics—the kinds of mathematics used by sub-Saharan peoples in their everyday lives and the way in which African social organization has influenced the development of African mathematical knowledge. Relying almost exclusively on secondary material collected from an impressive range of sources, Zaslavsky has produced a book that should prove extremely useful to professional anthropologists and educators. Anyone interested in mathematics or African cultures will find it attractive and readable.

Zaslavsky demonstrates repeatedly the error of the general notion that mathematics plays so small a role in African tribal life that "Africans could only count 'one, two, many.'" She does this by providing detailed analyses of the number systems of several African tribal groups and describing systems of time reckoning, measurement systems, and architectural principles. In a chapter devoted to the construction of African number systems, we learn of base 5, base 10, base 5-10, and base 5-20 systems that have been in use for centuries. The Yoruba base 20 system (which includes number names to 1 million) operates on a subtractive system that is mind boggling to this naive reader: 45 is expressed as "five from ten from three twenties." Other chapters contain interesting examples of the application of mathematical principles in a variety of practical and recreational contexts (including logic puzzles similar to the missionary and cannibals problem but with other casts of characters).

For reasons of trade, Europeans made it their business to know local African currencies, and Zaslavsky is able to draw upon several scholarly works concerning this aspect of African mathematical knowledge. Here we encounter complex systems for calculating equivalences across monetary systems (in the year 1500, 0.15 ounce of gold was worth 1200 cowrie shells in Mali). We also find consistent sets of units within a system (among the Igbo, 6 cowries equaled 1 unit of money, 60 cowries equaled 10 units). Here too we encounter the most telling evidence in support of the author's thesis that the level of mathematical sophistication is a function of social conditions, not of inherent "mathematical capacity." The point is made quite generally in contrasts between groups like the Yoruba, who have long engaged in trade, and the bushmen of the Kalahari, who trade almost not at all; the former have a complex number system, the latter do not. The same conclusion is urged on us in a unique way by an "experiment of history" perpetrated by avaricious Europeans. Discovering in the 16th century that many African groups preferred cowrie shells to gold as a medium of exchange, the Portuguese and other traders imported boatloads of cowrie shells in their trading ships. The debasement of cowrie currency led to a well-documented corresponding increase in the complexity of tribal number systems!

As Zaslavsky points out in her preface, African mathematical knowledge is a vast field awaiting investigation. Her attractively produced and lively summary should help draw scholarly attention to this interesting and potentially important topic.

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