directly point out, this procedure yields an overall dissimilarity measure whose contribution from any one character is the same for any two non-overlapping marginal distributions. In consequence, information on the relative dissimilarities of OTU's contained in an ordinal or interval scale of a variable could be lost by the K-dissimilarity measure. In this respect, the K-dissimilarity statistic is quite different from many of the dissimilarity measures currently in use in numerical taxonomy. It is a deficiency of *Mathematical Taxonomy* that the authors provide no discussion or justification of this property of the K-dissimilarity measure.

The discussion of cluster analysis is quite general in that it makes no assumption regarding what measure of dissimilarity is used in the clustering. A dissimilarity coefficient (DC) is taken to be defined by any symmetric OTU $\times$ OTU matrix of non-negative, real dissimilarity values. The notion of "cluster" is formalized as a maximal collection of OTU's between any two of which an arbitrary but specified reflexive symmetric relation holds. In the special case where clusters are required to be non-overlapping, the corresponding relation is an equivalence relation. This is an extremely fruitful approach, for it is then possible to consider a phenogram (termed "dendrogram" or "numerically stratified clustering") as a monotone, continuous mapping of non-negative real numbers (the levels of the phenogram) into the collection of relations on OTU's. The level of the smallest cluster that contains both of two OTU's, A and B, can be regarded as a dissimilarity value between A and B, so that a DC is uniquely determined by a phenogram. Then any procedure which uniquely assigns a phenogram to a dissimilarity matrix can be regarded as a mapping of the set of DC's into a subset of itself.

Within this model cluster methods can be selected by imposing conditions upon the mapping of DC's into DC's. Several conditions are imposed; only two require mention. These are (i) that the result DC have elements less than or equal to the corresponding elements of the data DC and (ii) that the result DC be maximal among the set of DC's satisfying condition i. It is shown that under these restrictions the mapping from data DC's into result DC's is continuous. Jardine and Sibson point out that among hierarchic clustering methods only single linkage analysis fulfills these conditions. They provide examples of discontinuous mappings from data DC's to result DC's for complete linkage analysis and for weighted and unweighted average linkage analysis (the use of the latter two designations is the reverse of the usual meanings of these terms in the numerical taxonomic literature). Methods admitting overlapping clusters are readily considered within the model; several that satisfy conditions i and ii are described, and their properties are analyzed in some detail. The treatment of nonhierarchic clustering methods given in *Mathematical Taxonomy* is the most sophisticated available.

Jardine and Sibson succeed in selecting among clustering methods in a quite rigorous way on the basis of specified formal criteria. The details of their derivations are quite pleasing and would provide worthwhile reading for almost anyone interested in numerical taxonomy. In one respect, however, their choice of cluster analytic methods seems incompletely justified. In selecting cluster methods most appropriate for biological taxonomy the formal criteria used cannot be taken simply as axioms but must rest in turn upon some foundation of biological principle. The justification of a particular cluster method for biological taxonomy is complete only when the connection between biological principles and the formal criteria is established. Most of the formal criteria used by Jardine and Sibson are uncontroversial; their biological justification can reasonably be skipped. Criterion i, above, however, is quite controversial—yet no defense for it is given. Jardine and Sibson have in effect not presented a complete determination of biologically optimal clustering methods, but only a detailed picture of the last several steps in one possible such determination. The value of *Mathematical Taxonomy* might have been considerably enhanced had the authors derived a variety of optimal taxonomic methods corresponding to a set of alternatives to their more debatable axioms.

Mathematical Taxonomy does include, in a separate section, a considerable amount of discussion of the principles of biological classification in a numerical context. This is essentially an appendage to the more technical sections of the book. It is not a defense of the formal criteria used in selecting methods, but is rather a general discourse on the most preferable goals for biological taxonomy. The point of view is quite similar to that espoused by Sokal and Sneath in *Principles of Numerical Taxonomy*. This part of the book could be quite useful as introductory reading to the phenetic taxonomic philosophy.

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Ecological Study of Form

The Adaptive Geometry of Trees. HENRY S. HORN. Princeton University Press, Princeton, N.J., 1971. xii, 144 pp., illus. Cloth, $7.95; paper, $3.95. Monographs in Population Biology, vol. 3.

Botanists seldom give much thought to the shapes of whole plants, perhaps because the growth habit of most higher plants is a repeatedly branched system of units of variable number. It is the form of the units (leaves, flowers, roots) that provides most of the material used by descriptive botanists. The parts, however, are linked to make a more or less integrated whole, a light-trapping, gas-exchanging, water-conducting wick extended between the water and nutrients of the soil and the sunlit, desiccating environment of the air. The form of this whole may be expected to matter very much, and if adaptively critical elements in form can be isolated and measured we may expect to have a tool of great value for comparing species and understanding the working of plant communities. One of the major elements in any functional analysis of the form of plants must lie in the manner in which the canopy is displayed. This, however, is the only part of tree geometry with which this book is concerned—it is almost wholly an analysis of the effects of leaf arrangement on the trapping of light. Horn concludes that monolayers are more efficient in shade and multiple-spaced layers are more efficient in bright light. He develops this view theoretically and from field examples and then extends it to account for changes in plant succession in a text that is vividly and exuberantly written.

Much of this type of canopy analysis has been done before and in a much more sophisticated manner; reading this book is therefore rather like discovering a tribe lost to civilization that has quite independently discovered a primitive form of the internal combustion engine. Does one praise the originality or sympathize with the ignorance?

The analysis of canopy structure was developed mainly by agronomists in Japan, Australia, and Britain in the late '50's and early '60's, through the work of Saeki, Monsi, Takeda, Donald, Black, Brougham, Watson, Blackman, and others. Horn quotes only one paper from any of these authors, and that at second hand.

A large number of parameters have to be taken into account in interpreting canopy structure, including leaf area, foliage density, leaf angle, daily and annual changes in the sun's path, and also water and gas fluxes. These have been elegantly handled in general computer models predicting productivity (for example, by De Wit), and there is very little that Horn has to say about trees that is not general to vegetation. There is tragedy here, not just in Horn's book but in the failure of most ecologists to make the slightest attempt to follow the literature of agronomy and of forestry. An understanding of complex systems can only come from the study of simple ones; the simplest terrestrial systems are those of cropped grassland and managed forest. Granted, these simple systems are complicated enough, but they have been made susceptible to theoretical analysis and much formal testing. Ecologists must read this literature even if it shatters some of their conceit. For those many readers who will find that Horn's monograph opens a new vision of the nature of vegetation I append a brief bibliography to correct the perspective:

P. Boysen-Jensen, *Biol Med.* **21**, 1–28 (1949); M. Monsi and T. Saeki, *Jap. J. Bot.* **14**, 22–52 (1953); H. Kasanaga and M. Monsi, *Jap. J. Bot.* **14**, 304–24 (1954); D. N. Moss, *Crop Sci.* **4**, 131–35 (1964); C. T. DeWit, *Versl. Landbouwk. Onderz.* **663**, 1–57 (1965); C. M. Donald, *Adv. Agron.* **15**, 1–118 (1963), and many papers in recent issues of *Crop Sci.* and *Agron. J.*

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Reef Research

Regional Variation in Indian Ocean Coral Reefs. Zoological Society of London Symposium No. 28, London, May 1970. D. R. STODDART and MAURICE YONGE, Eds. Published for the Society by Academic Press, New York, 1971. xxxvi, 584 pp., illus., + tables. $28.

This symposium volume provides an up-to-date account of Indian Ocean reefs, from the Red Sea to western Australia, along with some of the more specialized aspects of reef research being carried out in the region. For those with a general interest in reef origins, environments, and biotas it provides most interesting reading. Though specialists concerned with reefs will encounter many things to argue over, they will nonetheless find valuable data and provocative interpretations. The volume constitutes an important source, to be placed within arm's reach alongside Wiens's *Atoll Environment and Ecology* (1962), Maxwell's *Atlas of the Great Barrier Reef* (1968), and the Smithsonian's *Atoll Research Bulletin* (1951 to present).

The volume is most fittingly introduced with a tribute by C. M. Yonge to Thomas F. Goreau, whose untimely death in 1970 grieved all who knew him and his varied and important contributions to our knowledge of reefs.

The first and last papers in the volume are by D. R. Stoddart. The first is a scholarly and instructive overview of the history, environments, and biotas of Indian Ocean reefs and sets the stage for virtually all the papers that follow. The last concerns problems and prospects; it is a critical review of both the accomplishments of the symposium and the direction of future reef work. It would be a good idea for most reef biologists to study these, for, as K. G. McKenzie points out, quite a few papers in this symposium tend to neglect the time component. The papers are appropriately grouped by subject—Geology and Morphology of Reefs, Regional Studies of Reefs, Distribution of Corals, and Other Reef Invertebrate Communities—fish and calcareous algae being somewhat neglected.

Reef research is a classical field, and various aspects of it have developed at different times and have advanced at vastly different rates. Darwin's theory of subsidence, offered to explain certain morphological features of oceanic reefs, generated a series of reef drillings. The first, by the British Royal Society on Funifuti in 1896, proved beyond a doubt that Darwin was for the most part right. However, the most instructive reef drillings were made in the Pacific, at Bikini and Eniwetok in connection with atomic testing shortly after World War II (Emery, Tracey, and Ladd, 1949) and more recently at Mururoa (Lalou, Labeyrie, and Delibrias, 1966). Unfortunately, Indian Ocean reefs have not yet been drilled, so morphological studies of them suffer from a lack of detailed chronologies, especially related to the Pleistocene, for comparison with those of the Pacific. It can be observed from the literature, however, that atoll rim, terrace, and lagoon depths, to approximately 90 meters in the Chagos Archipelago, are comparable to those of many atolls in the Pacific Ocean, and this suggests comparable Pleistocene histories. It would be very instructive to make shallow drillings (to about 50 meters) and subject the materials to radiometric analyses to see if the chronologies of the past 250,000 years or so are also comparable.

The first deep-sea drillings were made in the Indian Ocean by JOIDES (the Joint Oceanographic Institutions for Deep Earth Sampling) early this year. Several, on the Ninetyeast Ridge, and especially on the Laccadive-Chagos Ridge, were at approximately the same depth (1700 to 1800 meters) as the platforms of many present-day atolls and banks in the Indian and Pacific oceans. These drillings encountered Middle Eocene to Middle Paleocene chert. Comparable chert has been taken near the break in slope on Horizon Guyot (Mid-Pacific Mountains). Correlations of age, depth, and morphology between these two widely separated regions, then, suggest comparable histories as regards subsidence rates, at least since the Paleocene, and indicate that both had basins on the order of 3000 meters below sea level by the end of the Cretaceous.

Further deep-sea drillings are to be made by JOIDES this year, in the Indian Ocean, and some of these will be particularly interesting to those concerned with reefs. R. L. Fisher and Elizabeth Bunce (leg 24) plan at least one site on the Mascarene Plateau at a depth of 1600 meters with the hope of penetrating 700 to 900 meters of coralline reef debris. These and related works are currently providing data that will satisfy many of the criticisms leveled by McKenzie of reef work accomplished to date in the Indian Ocean.