tion on means for suppressing seizure discharge either by occlusive interaction or specific inhibitory mechanisms would seem to be of highest priority for "basic" research in epilepsy. Yet this quest has been overlooked in favor of quite unproductive electrophysiological exercises. Much of the reported work reflects an emphasis upon technique rather than thoughtful analysis of the real problems of epilepsy and of the ways in which epilepsy can contribute to a knowledge of brain mechanisms. This is perhaps the price which we have paid for the increasing precision and power of experimental methods, which demand considerable expenditures of effort and resources for technically satisfactory results. True interdisciplinary research, which has often been the fruit of an interdisciplinary investigator, has become virtually impossible to achieve in modern neuroscience without the intense and active collaboration of several scientists, supported by substantial technical resources. The ideal of a broadly integrated research program in the neurosciences, which is surely the only way to substantially advance our knowledge of normal and pathological brain mechanisms, has yet to be achieved to any significant degree. It is most unlikely that the disease-oriented concept of research support, which now has fragmented basic neuroscience research into categories related to mental illness, to retardation, and to neurological diseases, each with separate funding agencies, can lead to the much-needed focusing on the central problems in brain research. By coincidence, a book intended to stimulate basic research on the epilepsies has arrived at a time when overall support for medical research is lagging. Perhaps an enforced reduction in activity may induce thoughtful consideration of more fruitful and effective modes of organization and support in the neurosciences.

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## On Change in Biological Communities

**Diversity and Stability in Ecological Systems.** A symposium, Upton, N.Y., May 1969. Biology Department, Brookhaven National Laboratory, Upton, 1969 (available from the Clearinghouse for Federal Scientific and Technical Information, Springfield, Va.). viii, 266 pp., illus. Paper, $3; microfiche, 65¢. Brookhaven Symposia in Biology, No. 22; BNL 50175 (C-56).

This symposium reflects a shift in the recent approaches to the problem of "species diversity," one of the in subjects of ecology today. There is now less emphasis on trying to explain why there are more kinds of plants and animals in tropical than in temperate (read north) zones and more on explaining why any community should have more or fewer species than any other—a shift perhaps due partly to the findings of Howard Sanders on marine benthic diversity, summarized in this volume. Sanders and others have linked in an interesting way the diversity of any community with its historic and present stability. And these are the key words here, for this inexpensive volume of 19 papers reviews in a fairly exhaustive manner what people mean by stability in ecological terms and how it relates to diversity.

The organizers obtained a wide spectrum of participants despite their disclaimer that a number of important people were absent. The list of authors is a fair Who's Who in population biology, which in some ways is unfortunate. Almost all the material presented has been published before, and the banter that follows each paper, though amusing in some cases, does not contribute very much. The same people who gave the papers also made the comments. I wonder where the hungry graduate students were?

There is a curious streak of naiveté in evolutionary theory which runs through several papers. The reasons given for low diversity in caves and hot springs and the tendency to equate a high rate of species accumulation with a high speciation rate are among the few examples. And the intriguing terms "predictable" and "unpredictable" are often used interchangeably with "stable" and "unstable," with interesting but erroneous theoretical results.

The papers of Margalef and Lewontin attempt a rigorous definition of stability in mathematical terms, with perhaps limited success. Quite often some theoreticians in this field give the impression that they find their own world much more interesting than the real one (it may be) and are loath to cross the boundary, even to obtain new things to think about. Fair enough I suppose, but here Lewontin offers a bridge to reality: "If you ask me how probable it is that communities will have 7 of one species, 14 of another, 209 of a third and so on, I can answer that if you tell me two things: (a) what is the configuration of the dynamical space as far as its deterministic elements are concerned, and (b) how much random perturbation goes on." I found that depressing.

How about the solid-data people? Goulden's studies of chydorid Cladocera are all too briefly summarized. In unstable situations early dominant species are generalists with wide niche requirements, hence reduced diversity. Fine. But then he also suggests that species adapt to existing conditions (unstable but predictable?) and that when all have done so the association develops to maximum diversity. Well, that I would think covers about all the possibilities. There are several other papers like this whose data and conclusions are impeccable and logically splendid but which leave the reader grasping fog. What was the question again?

Sanders tells us that for his group of animals, mostly polychaetes and bivalves found in marine sediments, diversity is greatest in areas that are and historically have been benign (stable) and predictable. Although he is convincing, I don't know how widespread taxonomically this pattern is, even among bottom-dwelling marine organisms. My impression is that some groups show it and others do not. Perhaps this is not too surprising for, obviously, what is stable and predictable for some organisms may be quite unstable and unpredictable for others. And Cantlon's paper asserts that perturbations are often necessary to maintain diversity in forest ecosystems. With time and stability diversity goes down, not up. Perhaps so; why not? The fossil record people—Deevey, Simpson, and Goulden—don't or can't tell us.

The most stimulating paper is that by Slobodkin and Sanders, replete with diagrams in the best Levins style. One needs to read only this clever overview to get the gist of the problem and perhaps what's wrong. They tell us that high productivity is not related to high species diversity; that areas of high predictability are rich in species because in such areas the probability of speciation is increased (but they demonstrate no real relationship between predictability and isolation, and I'm oldfashioned about this), the probability of extinction decreased, the probability of immigration increased, emigration decreased, and competition diminished. Now that is real meat. Let me reshift the approach for a moment back to the tropics-versus-other-areas comparison. Given that for most groups tropical areas, and I include the cool tropics, are richer in species than other areas, are tropical areas those that fit the picture presented by Slobodkin and Sanders as being the most stable and the most predictable? The answer is, of course, it depends—in some cases absolutely not. Arctic areas that I have worked in are much more predictable in all sorts of biologically relevant points than are the lowlands of Panama. Physical stability of the tropics in terms of temperature is probably true, but I doubt that it is true for rainfall. In six years of working in the Central American tropics, I have seen population explosions and crashes in mammals, birds, and insects which were fully the magnitude of any reported from anywhere. Whole groups of plant and bird species did not attempt reproduction for one, two, and in some cases even three years, presumedly because of some environmental "perturbation." To be sure, the relationships are complex, but they are not stable in any sense of the word. I fail to detect the classic "buffered" tropical terrestrial community. Watt in this volume comes to somewhat the same conclusion. In short, the real terrestrial world would seem not to agree with the real marine world.

Perhaps the wrong questions are being asked. Maybe even the question "Why are there so many kinds of animals?" may not be quite appropriate. I don't know, but a lot of bright people don't seem to be making much progress. This symposium did offer several ideas on which new approaches might be based. The points call for a disregard of universality in a number of things. Miller, in a pithy summary of competition theory, shows that competition is not necessarily the same for all species. Little beasts react toward one another in a way basically different from that in which big ones do. Whittaker points out that plants and insects may show an indeterminate evolutionary increase in diversity, a fact that if true makes these groups fundamentally different from all others. One theory won't do. As others have suggested, the factors controlling diversity in one particular group from 75°N to 25°N may indeed be different from those working on this group from 25°N to 25°S. Through several of the papers runs the feeling that historical accident may be a much more important component of species diversity. I hope not.

To summarize: a broad and perhaps classic review of a field that may be going stagnant; nothing much new, but some things said very well and provocatively.

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## An Evolutionary Group

**The Biology of Higher Cryptogams.** William T. Doyle. Macmillan, New York, and Collier-Macmillan, London, 1970. x, 166 pp., illus. Paper, $4.95. Current Concepts in Biology Series.

This book should have a broad appeal. The author has elaborated his own evolutionary interests in the presentation of material on the higher cryptogams. He has not only brought together an array of general information but has combined it with challenging subject matter pertinent to his concern with the morphogenetic potentialities of the groups to which he is clearly dedicated.

Doyle has set off the higher cryptogams, some 34,500 species of spore-bearing land plants, from the angiospermous seed plants, which include more than 300,000 species. His objective is to point out the collective advantages of the spore-producing plants due to their heritage of that long geological past during which plants have evolved and been selected for living on land. The seed plants, presently aggressive and dominant, are probably not much more than half as old.

First and foremost, however, Doyle has published an excellently conceived program for basic morphogenetic studies resting on two well-developed premises. First, the spore-bearing land plants, the archegoniates, have been and still are successful evolutionary experiments. Second, these plant groups, however varied, are all organized at a simpler level than are the later-evolved seed plants, the gymnosperms and the angiosperms. They ought, therefore, to be very appropriate groups for planned morphogenetic investigations, if the study of morphogenesis is defined as a systematic effort to understand why groups of organisms do continue to exist as successful evolutionary ventures. As Doyle and others have recognized, the common traits that set off these plants from those geologically later, more highly organized seed-bearing types are those fundamental characteristics that lend themselves to the explorations of the morphogeneticist.

This "little book" is bigger, however, than a program designed only for morphogenetic studies. Today scientist and nonscientist alike are constantly reminded of the needs of conservation, of efforts to safeguard the genetic pool as represented by extant plants and animals. It is important to be aware, not only scientifically but practically and even esthetically, of those groups of organisms, as Doyle notes, which play little part in the economics of everyday living.

Doyle also points out over and over that the higher cryptogams are a "natural evolutionary group" of land plants. Whether they are of common origin or are polyphyletic "still elicits lively discussion." Consensus of taxonomists, paleobotanists, and comparative morphologists treats the higher cryptogams as sufficiently diversified in geological time and sufficiently constant in their diversification to be divided into five classes: the Bryopsida, consisting of some 23,000 hornworts, liverworts, and mosses; the Psilopsida, of perhaps 2 surviving species, possibly representing the earliest land plants; the Lycopsida, of some 180 species of lycopods (*Lycopodium* and *Phylloglossum*), some 600 species of selaginellas (*Selaginella*), and some 60 species of quillworts (*Isoetes* and *Stylites*); the Sphenopsida, of some 25 to 30 species of horsetails (*Equisetum*); and the Pteropsida, of some 10,000 species of ferns of many genera.

The living bryophytes and vascular cryptogams compete successfully today with other plants in many of the biologically difficult, even marginal, places on the earth's surface. Mosses, lycopods, and ferns constitute a considerable part of a ground cover of the world's forested natural areas, even in poorly illuminated jungles; representatives grow in the exposed, often desiccated, sun-drenched regions above timberline on mountains, and on rocky ledges; species of selaginellas persist in the deserts and mosses in the northern tundra; species of quillworts and certain genera of ferns thrive in water; and other species of quillworts grow well and reproduce in mountain vernal pools that dry out completely during the summer months; many other spe-