Book Reviews

Directions in Brain Research

Basic Mechanisms of the Epilepsies. Based on a symposium, Colorado Springs, Colo., Nov. 1968. HERBERT H. JASPER, ARTHUR A. WARD, JR., and ALFRED POPE, Eds. Little, Brown, Boston, 1969. xxvi, 838 pp., illus. $30.

Epilepsy, among the morbid conditions which affect human behavior, occupies a singularly prominent position in the history of brain research. Focal seizures, and their experimental counterpart, electrical brain stimulation, have been a major source of knowledge concerning the relation of brain function to human experience and behavior. It is timely that this volume should appear to mark a century of brain research during which the study of epilepsy has made signal contributions. Assertions concerning localization of brain functions are found in the Hippocratic writings, but these were ignored for centuries until J. Hughlings Jackson resumed the clinical analysis of focal epilepsy in the 1860's. After 1930, spurred by the introduction of electroencephalography, the pace of research on epilepsy and on cerebral function in general was notably accelerated. By 1950, general correlations between the clinical and electrical manifestations of many seizure types had been established. Most of the surface of the human brain had been functionally mapped by electrical stimulation during surgery for treatment of focal seizures. The role of the "reticular activating system" in the disturbance of consciousness typical of certain types of seizure had also been demonstrated. With the availability of microelectrode techniques for recording activity from single neurons, of the electron microscope for defining neural ultrastructure, and of increasingly precise neurochemical techniques for analyzing the molecular constituents of the brain, the stage was set for a massive attempt to comprehend the fundamental mechanisms of brain function.

This book provides a catalog of some of the results of that campaign, with special reference to the problem of epilepsy. This is a semiofficial treatise, conceived by a special task force on basic research on the epilepsies which was formed under the auspices of the National Institute of Neurological Diseases and Stroke. It contains contributions by over 60 authors, among them some of the leading neuroscientists of the Western world. It aspires to the role of "a comprehensive monograph . . . presenting a collation and synthesis of modern knowledge in this area." The 30 individual chapters, most of which are followed by related but not integrated discussions, represent a broad survey of topics deemed relevant to the physiology of epilepsy. The pressure for a complete treatment of fundamental neural mechanisms has led to the replication of a certain amount of material which has appeared elsewhere. This redundancy would have been more understandable if the editors had adequately implemented their stated intention to provide an integrated presentation of basic neuroscience research relevant to epilepsy. Unfortunately, the high quality of several chapters is overshadowed by the general lack of cohesion and organization among individual papers. The student and interested clinician will find little guidance in organizing the large body of experimental material within an overall conceptual framework which relates these specialized studies of neural function to the broader problems of the epilepsies. The introductory chapter by the editors is particularly deficient, providing an inadequate review of clinical epilepsy together with some banal generalizations concerning the importance of detailed interdisciplinary knowledge of brain mechanisms for an understanding of the epilepsies.

Despite the deficiencies of organization and orientation, the volume contains a substantial amount of well-presented information on excitatory and inhibitory mechanisms at the neuronal level, reviews of the chemistry and pharmacology of synaptic transmission, and some useful material on general cerebral metabolism. All of this might be viewed as "basic neuroscience" having no necessary relationship to epilepsy. A few other papers are worthy of mention, including a review of sleep and seizures by Pompeiano, a scholarly treatment of denervation sensitivity by Sharpless, and some whimsical mathematical remarks on self-organizing systems by Minsky.

The remainder of the volume is devoted to papers more specifically related to various aspects of experimental epilepsy. A lot of work, which is documented here in considerable detail, has been expended in studying electrophysiologic and metabolic concomitants of seizures. In view of the considerable diversity of clinical epilepsy, both as to etiology and electroclinical manifestations, it is not entirely clear what information of general value for the understanding of seizures or of normal brain mechanisms may be derived from the analysis of experimental epileptic foci and pharmacologically or electrically induced convulsions. One thing the book makes abundantly clear is that most of these studies are "needle in the haystack" searches, whose empirical strategy and ultimate goals lack conceptual clarity. After an extensive listing of findings, none of which serve to elucidate mechanisms substantially beyond what could have been guessed from a knowledge of synaptology and certain hoary facts about seizures, the pious belief is repeatedly expressed that what is needed is more research. There is a quixotic flavor to these quests, with "epileptic neurons" for windmills and microelectrodes for lances.

The enormous efforts going into some of these pseudorelevant investigations might well be devoted to a more systematic analysis of normal structure-function relations as exemplified by the work of Eccles's group, which is again reviewed here by the master. This fruitful direction is also exemplified by the revealing developmental analysis of cortical excitatory and inhibitory mechanisms by Purpura and associates. There is a great need for a systematic and concerted analysis of the regional morphologic and physiologic features of normal brain, which can be studied concurrently by means of the powerful anatomical, chemical, and electrophysiological techniques now available. We already know that the major differences in structure among brain areas are paralleled by differences in susceptibility to epileptic activity. The reasons for this should more readily be revealed by study of normal brain than of the capricious and uncontrolled manifestations of induced seizure foci. Informa-

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.

HERBERT G. VAUGHAN, JR.
Department of Neurology,
Albert Einstein College of Medicine,
New York City

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 old-fashioned about this), the probability