ticular muscle groups used or of the exact timing of contractions. Of practical significance is Bernstein's observation that learning of new skills entails first reducing the degrees of freedom of relevant movements (tightening up) so as to plan each movement with deliberation, and later must allow execution of the act in a variety of ways so as to firmly embed in the nervous system a more abstract schema of movement.

Bernstein views perceptual psychology as intimately associated with his own task of categorizing and analyzing movement structures. Both the sensory and the motor fields are organized more in terms of topological properties than of metric ones; this is no coincidence, because they map onto one another as an organism anticipates and evaluates the results of its movements. This insistence that a plan of movement is "sensorimotor" will warm the hearts of a growing band of American psychologists who believe that "learning to see" entails the correlation of body and hand movements with their particular sensory consequences. These "idealistic" discussions of ideas, anticipations, and their plasticity puts Bernstein much closer to the cognitive psychology of Piaget than to the reflexology of Pavlov, even during the 1930's and 1940's when dedication to Pavlovian doctrine seemed to be mandatory for any Soviet psychologist or physiologist.

One of Bernstein's deepest arguments concerns the impossibility of inferring the controlling pattern of neural discharge to muscles from the actual movements of body or limbs. The field of forces to be overcome by muscle action is simply too complex and too variable to allow accurate prediction over any but brief intervals. Systematic use of sensory feedback is required to fulfill the plan, and in fact the anticipation of such feedback must be an integral part of the schema. A systematic study of human (or animal) abilities to correct complex movements should reveal the size of the motor groupings that undergo modification.

In the present volume, Bernstein is content with turning our attention toward these issues and does not adequately present the data upon which his generalizations rest. Time and time again the appetite of the reader is whetted to the point where he is ravenous for some detailed account of the alleged experiments. Yet each time the discussion ends with an abrupt reference to Bernstein's untranslated book *The Construction of Movements*. Bernstein does show a clever method for recording movements on a moving film, but he does not illustrate how an analysis of his "cyclograms" could extract the topological properties that distinguish the particular plan of action. Referring the "motor field" to perception, though a heuristic step, leaves us standing somewhat limply before the same problems of perceptual organization that have baffled us for centuries.

Yet we should not be too critical of this intelligent book, which states the problems of motor organization perhaps more explicitly than any single Western source to date. Now that ethologists have begun an orgy of descriptive accounts of animal movements and electrophysiologists have begun to listen in on conversations among neurons of the monkey motor cortex, the time is very ripe for fresh attempts to infer principles of brain organization from the constructions of overt movements. Any experimentalist with such an attack in mind had better assimilate this valuable briefing by Bernstein, who has scouted the field for the past 30 years.

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The Confrontation with Ecological Complexity

Systems Analysis in Ecology. KENNETH E. F. WATT, Ed. Academic Press, New York, 1966. xiv + 276 pp., illus. $11.50.

The field of ecology, which for many years was a quiet and undramatic one, is being forced into strenuous activity. The main reason for this is that the problems of human society's effect on landscape are becoming grossly visible. Private and governmental agencies are therefore demanding answers to practical questions, and they will not be silenced by ecologists' protestations of academic modesty. In fact, in the classical fashion of small groups stereotyped by more powerful groups, ecologists have begun to accept the idea that they are responsible for solving major problems. The net effect has been very healthy.

It has become clear that classification and description alone will not provide the required insights. It has also become clear that no particular measurement technique will automatically provide ready insight. Quite obviously theoretical sophistication of a high order is going to be absolutely vital before practical answers become available, and the problems are such that there is very little time to lose.

Out of the urgency and out of the tangle of possible approaches three main paths are emerging. They differ in how they approach the stupefying complexity that seems to characterize the natural world. It is possible, faced with extremely complex problems, to make enough simplifying assumptions or to consider distributions of events at a sufficiently abstract level that the complexity is no longer troublesome. One is then left with a problem amenable to reasonably elegant mathematical solution. If well executed, this approach to complexity has the advantages of high intellectual appeal and the generation of abundant testable hypotheses. It has the possible weakness that the simplifying assumptions permit insight into some other world than the one that produced the original problem.

As an alternative, it is possible to search for extremely simple empirical or theoretical generalizations which are valid in nature, in the hope that a sufficient number of these will constrain the class of admissible theoretical models so as to permit prediction. Such generalizations may be based on laboratory experiment or field data. This approach works reasonably well but has the weakness that, although predictions are possible, often the predictions are of low precision or of a non-urgent content.

A third approach has been called "systems ecology." The word "systems" is clearly an all-right word and has therefore had to work very hard. It generally refers to the approach in which an intractably complex problem is divided into a series of more tractable subproblems for the purpose of constructing a model. It has also been used in the sense of "general systems," in which it refers to the search for formal similarities between the attempted solutions of problems in different fields. By a curious but understandable extension it has come to mean approaching an empirical problem with an extensive use of electronic data processing and collecting. Judging from the papers presented in the volume under review, Watt's definition lies between the first and third of the above definitions. This approach has the same strengths and weaknesses in principle as the purely analytic approach, with the important distinction that limitations in ability to handle data are much less for a computer than for a mathematician.

Perhaps the central and most difficult problem facing ecology is precise prediction of the behavior of field populations. Papers by Stark and by Paulik and Greenough present examples of this kind of problem. The magnitude and multidimensionality of the problems are fascinating. The data used vary from infrared photographic records to x-rays of tree bark. The difficulties are those of econometrics added to those of meteorology with a few biological problems thrown in. The scope of these investigations, and their apparent feasibility, is more surprising than the fact that firm, precise predictions have not yet been forthcoming.

One serious difficulty in ecological research is that the volume of data that must be collected and processed is stupendous. This is particularly true in situations in which the behavior of organisms is important. Hamilton, Savage, and Kavanau are each concerned with automatic systems for recording and reducing behavioral data.

Kavanau has placed mice in what is essentially a computer-environment-control system in which the computer is informed of the behavior of the mice and the mice can, in turn, control the computer so as to modify their own environment in a variety of ways. The behavior revealed by this arrangement makes the mice seem much more exciting than any animals that ever occupied a Skinner box or desperately leaped from an electrified platform. I am fascinated and delighted by any study which can unequivocally conclude that "mice of the genus *Peromyscus* tend to react to the arbitrary imposition of a regime by opposition to it. . . ." Not only is this a promising foretaste of new problems that will be approachable only through highly automated data processing, but somehow it lends hope to the world.

Both Hamilton and Mott are concerned with the statistical problems of data reduction. Hamilton deals with the problem of behavioral data from individual organisms, whereas Mott develops the variance components in data from growing populations.

Holling's general approach is that of a very close feedback relation between experimental data and computer simulation, with the experimenter being guided in his future experimentation by the discrepancies between the simulated and the actual systems. He has been developing some fascinating results in the theory of predator-prey interaction by this technique. Pimentel reviews a series of ecological situations to emphasize the complexity of the problems involved, without attempting a detailed analysis of any one problem. Watt terminates the book with a vision of highly automated future research in ecology, somewhat reminiscent of a technocrat's Utopia.

The quality of each contribution is high. From the standpoint of an expert on computing techniques or a person looking for new insights into the analysis of highly complex problems I think the book may be a little disappointing. This is not a surprise, nor is it a denigration of the book. Some insight is provided into the very beginnings of what will be a major area of intellectual concern 20 years from now. The courage to face this now, on the part of both Watt and his publisher, should be commended.

It should be pointed out that in one sense this is not a book at all, but rather another instance of a commercial publisher's exploiting the slowness and rigid editorial policies of scientific journals. It is a comment on the scientific establishment that work in so many new and as yet uncrystallized areas is first published commercially.

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A Zoological Puzzle

So Excellent a Fishe. A Natural History of Sea Turtles. ARCHIE CARR. Published for the American Museum of Natural History by Natural History Press, Garden City, N.Y., 1967. xii + 248 pp., illus. $5.95.

Anyone who has read Archie Carr's *The Windward Road,* published in 1956, will compare Carr's new book with it. The two are basically on the same topic. The difference between them is indicated by their subtitles. That of *The Windward Road* was "Adventures of a Naturalist on Remote Caribbean Shores." That of *So Excellent a Fishe* is "A Natural History of Sea Turtles." The first book is, by comparison, lush in language, vivid with the color of places and people, but lean in information because the information was still to be obtained. The new book deals with the same places, the same people (with, however, many additions to the dramatis personae), and the same problems; but the problems now are in closer focus, and the lush scenery is in the scarcely noticed periphery. Carr's writing skill is undiminished, although those who enter the present book with the previous book in mind may suffer some disappointment until they are caught by a narrative skill that is as good at the exposition of a problem in biology as at the recounting of an incident in travel.

The problem which Carr's adventure in *The Windward Road* centered about was the "riddle of the ridley": Where did the ridley (Kemp's turtle, *Lepidochelys kempi*) breed? The solution came with the discovery of a film of the fantastic "arribada"—the nearly incredible mass egg-laying by perhaps 40,000 ridleys on a beach near Tampico, Mexico. This was the climax of a long, plodding search by Carr and the discovery of rare individual nestings.

Carr's horizon has widened now. He is heavily involved in the problems of sensory physiology in sea turtles. The difficulties are severe, since these are animals that are invisible (except for the most random contacts) during much or most of their life cycle. It is true that, if you have judged time and place correctly, the very first moments of the sea turtle's life are accessible—the moments when the hatchlings scramble in communal panic out of the buried nest (the phrase is not Carr's but it describes the way in which they stimulate and so aid each other to get out of the sand that traps them), the moments also when these hatchlings begin by some sense still ill understood to find their way to the sea. (Carr's co-workers have done great service here testing the hatchlings with colored spectacles, with colored lights, and with polarized light.) The mystery begins with the hatchlings at sea. Where they go and how they live is not known to Carr or to anyone. That they may go very far is well established; from Ascension Island to the Brazilian coast is one frequent trip that Carr discusses. The return migration some years later is even more of a mystery. How do they find their isolated mainland beaches or their islands? How does the individual turtle find these remote places, and how did the turtles in an evolutionary sense learn to find them? Carr ranges uneasily