Book Reviews

The Forming of the Geological Survey

Government in Science. The U.S. Geological Survey, 1867–1894. THOMAS G. MANNING. University of Kentucky Press, Lexington, 1967. xiv + 257 pp., illus. $7.

The thesis of this book is that certain major contemporary issues regarding the operation of scientific agencies by the federal government were generated in the late 19th century, during the formative years of the Geological Survey. These issues are related to the control, scope, cost, quality, and social applications of government science.

As essential background to the founding of the Geological Survey in 1879, Manning sketches scientific exploration of the American West following the Civil War. Here the issue of military vs. civilian control of government science began to take shape after 1867 with the organization of the U.S. Geological Exploration of the Fortieth Parallel and the U.S. Geological and Geographical Survey of the Territories, the first directed by Clarence King as an operation of the Corps of Army Engineers, the second by F. V. Hayden under the sponsorship of the Department of the Interior. How the ensuing contest between soldiers and civilians for the management of scientific exploration in general and of topographic mapping in particular was resolved in favor of the civilians is a story of spirited political infighting well told in this book.

The issues of scope, cost, quality, and social applications came to the fore between 1881 and 1894, when John Wesley Powell was Director of the Survey. Clarence King, Powell's predecessor, had concentrated on economic geology, in particular on studies of mining camps in California, Nevada, and Colorado. Determined to broaden the scope of the Survey's investigations, Powell encouraged studies of paleontology, geomorphology, and other basic aspects of the geological sciences, while pushing ahead to complete the topographic mapping of the United States as quickly as possible. His appropriations, never adequate for the multidisciplinary program envisioned, were periodically cut back on grounds of economy or as reactions against Powell's seeming fondness for projects which promised no economic returns. Inaccuracies discovered in some of the topographic maps hastily produced during the Powell regime raised the question of the quality of government science—unfortunately at a time when the Survey needed more rather than less financial support.

The issue of applying government science to social reform came to a head between 1888 and 1890 in connection with Powell's proposals for an irrigation survey of the arid West. Powell envisioned the development, through topographic and hydrologic studies, of largely independent agrarian communities each controlling the water of its own drainage basin through systems of reservoirs and canals. Reaction against this scientific collectivism led to the abandonment of the irrigation project and to Powell's resignation.

Although this is a work of political history, references to pertinent scientific publications of the Survey and to letters and biographical accounts of the principal characters appear throughout the text. The book ends with a sketch of recent developments within the Survey to 1963, an essay on source materials, and a detailed index. Four maps locate the principal geographic areas discussed.

Few would disagree with the author's appraisal of the Geological Survey as "the government's most productive research agency during the nineteenth century." That this was so is all the more amazing in view of the alleged pettiness, selfishness, hypocrisy, and plain cussedness on the part of many who attended the drawing of the plans, the laying of the keel, and the launching of the ship.

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Doing and Knowing

The Co-ordination and Regulation of Movements. Papers translated from Russian and German. N. BERNSTEIN. Pergamon, New York, 1967. xii + 196 pp., illus. $8.

"The motor activity of organisms is of enormous biological significance—it is practically the only way in which the organism not only interacts with the surrounding environment, but also operates on this environment with respect to particular results. The theoretical lag in this area in comparison with the physiology of receptors or of internal processes is therefore very puzzling."

So wrote Nicholas Bernstein near the end of a long career of exploration into the structural organization of human movements. This translation of selected articles and essays of three decades certainly provides moral support for those of us in the West who keep insisting—each in his lonely corner—that the study of what animals *do* merits at least as much experimental attention as what they *see*. Western psychology remains so stubbornly Greek in character that most of us never imagine any entrée to the mind but through the eyes or ears. It is not surprising that a Soviet scientist should study motor skills, since the Leninist epistemology stresses "knowing" by manipulating and transforming the world. No doubt there is official enthusiasm for any project that could increase muscular efficiency of workers, athletes, or cosmonauts. Bernstein's emphasis on the motor system is far from dreary propagandism, however—his ideas are refreshingly modern, and his pitch is directed toward down-to-earth research in neurology, child development, or brain physiology.

Along with neuropsychologists such as Lashley and Hebb, Bernstein sets aside the simplistic idea that integrated movements can best be described as a chain of interlocking reflexes. Although he does not deny that postural support and certain synergies of joint movement often have a reflexive basis, he emphasizes the more abstract underlying "plan of movement." Sequences of movement can be considered as *Gestalt* entities as well as visual groupings. For example, we can write our name large or small, quickly or slowly, on horizontal or on vertical surfaces. There is a large variety of human skills whose essential organization cannot be described in terms of the particular 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 be-