# End of an Era in Biology

George Gaylord Simpson

What, in objective terms, is a gene? What is its action at the molecular level? What is the fine structure of a ribosome? How do intracellular energy transactions occur? What are the relationships among natural selection, canalization, and deme differentiation? What is the anatomical basis for concentration of urine hyperosmotic to blood? How is information coded in transmission within a neuron? How is species-specific bird song determined?

Those are some of the most important biological questions that have been answered in recent years. In no instance can the answer be considered absolutely complete and final, but in all the given instances the nature of the answer, at least, is now clear. Further research is needed, but from now on that will be more laborious than adventurous.

The preceding questions cover a wide range, but they have one significant thing in common: each is essentially confined to one- or at most two-levels in the hierarchy of organization, in the gamut from atom or ion to multispecific community. It is easy to frame related questions that span several or many levels. To what extent and by precisely what means does action of specific genes determine particular adult structures and behaviors? What chemical or physical events in just what parts of the nervous system accompany the perception and the later recall of happenings in an organism's environment? All of the one- or two-level questions given as examples in the first paragraph can be expanded to more levels in this way. It then becomes evident that present answers to the expanded questions are far less satisfactory. In some instances not even the probable nature of the answer is known. The multilevel questions may not invariably be more complex or more difficult, but they tend to be so, and certainly we have made less headway toward answering them.

A generous sampling of recently current answers to important biological questions is given in **Ideas in Modern Biology** (Natural History Press, Garden City, N.Y., 1965. 563 pp., $8), edited by John A. Moore. This volume is composed of the texts of the papers presented at the plenary symposia of the XVI International Congress of Zoology, which was held in Washington, D.C. in 1963. Although published commercially and independently, it is also volume 6 (and last) of the proceedings of that Congress.

The questions here discussed include all of those in the opening paragraph of this review and many more. The quality is guaranteed by the list of authors, all eminent zoologists, and interest is guaranteed by their subjects. Under genetic continuity, Meselson writes on duplication and recombination of genes, Spiegelman on gene action, both strictly confined not only to intracellular reactions but also to DNA and, in the second chapter, RNA. Cell biology is represented by eight pages on structure and function, still mainly DNA–RNA, by De Robertis; an atlas of electron micrographs by Porter, with legends so complete as to constitute a short monograph (this was not part of the symposium at the Congress); discussion of synthesis of DNA and RNA (slightly overlapping the first chapter) and, in minimum, polypeptides by Ingram; and a chapter on energy transfer, mainly the ATP–ADP system, by Lehninger. Under the rubric of development are a historical review, unique for this volume, by Jane Oppenheimer, and chapters on cellular differentiation by Markert and on cellular interactions by Abercrombie.

Under "evolution" Rendel briefly discusses the special topic of quantitative changes in DNA and, presumptively, genes; Lewontin exemplifies his work on overall fitness and multiple selective peaks; Mayr condenses into ten pages some themes of his large book on evolution at the species level; and Kurtén provides a healthily skeptical summary of some paleontological data on evolutionary processes. A section titled "Phylogeny," distinguished from evolution, suggests some editorial problems of categorization. Prosser deals with the problem of emergence, including philosophical aspects, and has an equivocal concern for communication theory, which seems after all to have little to contribute. Schmidt-Nielsen treats anatomy and physiology of excretion in a comparative but not, in fact, a phylogenetic way. Only Carter is really concerned with phylogeny, here at high categorical levels and largely as a plea for older and not the most novel points of view. Finally, the topic of behavior is represented by Bullock on neurophysiology, by Thorpe on the ontogeny of behavior (partly conditioning, partly interplay of genetics and learning in bird song), and by Tinbergen on behavior and natural selection. Tinbergen's point is that actual study rather than speculation has generally shown that individual behavior in birds has selective value even when this is not evident at first sight.

It is inevitable and therefore is not open to criticism that a single volume on ideas in biology should present only a small sample of such ideas. The fact that five out of nineteen contributions are centered on the DNA-RNA system is probably fair sampling from a strongly biased population. It represents an imbalance of a few years ago, now passing at the primary level of research, although probably with some time to run in textbooks and other second- or third-hand works. Readers will surely bear in mind that fields represented here—physiology, for example—do have many more modern ideas than here appear, and that whole fields not even mentioned here—systematics or ecology, for example—also are replete with modern ideas, constitute major parts of zoology, and are pursued by great numbers of zoologists, probably in sum a majority of zoologists today.

John A. Moore, who had the extremely onerous task of arranging the Congress's program and who has edited this volume, is understandably enthusiastic about the result. Indeed he and the contributors deserve the highest praise and thanks from all biologists. Yet a question may be raised on the basis of Moore's preface and the emblem of the Congress, a phoenix, which "symbolizes the reunion of zoology

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from its separate specialties." Moore speaks not of a reunion but of the end of a union: "It is probable that ours is the last generation of biologists that can attempt to take all of biology as its domain." One may agree that these symposia marked the end of an era and yet have different reasons.

Reunion and synthesis in science are not obtained by mere physical juxtaposition of disparate and independent studies. Most of the chapters in this book, each excellent in itself, are one- or at most two-level studies, as noted at the beginning of this review, highly restricted, and with few or no broader implications. Subjects that necessarily involve more levels and true synthesis are either omitted or treated in a different way. (Note Oppenheimer's review of "classical" questions in embryology and conclusion that they "are the questions we are still asking today.")

There are indications that the next era will be one of tackling more complex problems, linking together the one-level contributions of the recent past. If this fine symposial volume marks the end of an era, it excellently exemplifies the firm basis for the next era of biology.

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## Academic Cargo Cult

**The Revolution in Anthropology**. I. C. Jarvie. Humanities Press, New York, 1964. xxii + 248 pp. $6.75.

This book is a critical evaluation of the shortcomings in the theoretical orientation of the functionalist (Malinowski) and structural-functionalist (Radcliffe-Brown) schools of anthropology—that is, of British social anthropology. At the same time it presents a critique of interpretations of cargo cults in Melanesia and offers the author's own interpretation. More important, it is the first step in the evolution of the author's own thinking as a philosopher and critic of the social sciences, for whatever the deficiencies of this work (it is a revision of the author's Ph.D. dissertation) it reveals a young scholar of promise.

British social anthropology has been as much a "closed society" as the Melanesian communities that Jarvie characterizes by this term, for until recently its members communicated almost entirely with one another and some even boasted that they read no psychology and no works written by Americans. Jarvie claims that, when the members of closed societies suffer severe feelings of deprivation, they tend to develop a theory of their problems. A prophet will then claim to have a way of translating the theory or ideology into a specific program for action. People follow him insofar as they feel they have the same problem and subscribe to the same general theory of what is wrong. Jarvie's problem was that as a student of anthropology at the University of London he was frustrated by what he felt to be the shackles of social anthropological dogma. He switched to philosophy and discovered the saving doctrine of his teacher, Karl Popper, whose ideas he applies to specifying what is wrong with social anthropology and what it needs in order to be saved. If other social anthropologists feel similarly frustrated, Jarvie may gain something of a following and may even be cast in the role of prophet.

These remarks should not be taken as condemnation of Jarvie's work. My point is simply that Jarvie himself is a protagonist in a particular enactment of the same general kind of social-psychological process of which cargo cults in Melanesia are also particular enactments. Scientists and philosophers are not supermen, and academic communities are unexceptionally human communities. Change in intellectual circles follows the same general patterns that characterize change in primitive societies. The "uniformitarian" principle applies to human behavior as well as to geological process.

Jarvie's criticism of the failings of social anthropological theory and his analysis of what is basically at fault are essentially sound. Sociology deals with the recurring patterns of event and social arrangement that characterize human communities. These patterns are obviously products or artifacts of what individual human beings do. They are explained by human behavior. Social anthropologists have argued, on the other hand, that they explain human behavior. This tautology forces social anthropologists to conclude that people act as they do in order to maintain the patterns and the equilibrium of the whole society, making of these things a final cause. As Jarvie points out, this theoretical stance makes it impossible to deal satisfactorily with social change. To do this, theory must deal with people, with their aims and their circumstances as they perceive them. Such an approach, which Jarvie labels "situational logic," allows for more satisfactory explanations of change, as he undertakes to show in relation to cargo cults.

It is here that the parochialism of English social theory is evident. American social psychology, since Cooley and G. H. Mead, has used a "situational" approach in its analysis of human behavior. American anthropology has been much concerned with culture as the conventional standards by which people perceive their situations and make their choices relating to them. It is interesting to learn that such an approach to behavioral phenomena is original with Karl Popper.

There are other points of criticism, such as equating psychology with the study of the irrational and using the term "rational" without definition but in a sense that I find strange. Not having done field work, Jarvie fails to understand its complicated and important role in anthropology. The writing is overly polemical, and the book was badly edited—many references cited in the text do not appear in the bibliography, for example.

The important point remains that Jarvie is constructively challenging what has been going on in social anthropology. His thinking has not been influenced by the body of American behavioral and social theory most closely akin to his own. We in America may regret this, and Jarvie's book may seem less revolutionary to us. But its appearance is, in the context of British social anthropology and by Jarvie's own situational logic, a noteworthy and welcome event. It will be a pity if it is dismissed out of hand.

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## Comparative Pathology

**The Principal Diseases of Lower Vertebrates**. H. Reichenbach-Klinke and E. Elkan. Academic Press, New York, 1965. xii + 600 pp. Illus. $20.

After some 19 years of training and study in comparative pathology, I am not inclined to read 600-page texts in my field from cover-to-cover at one sitting. But I challenge anyone who nodded in agreement with my opening sentence to resist the temptation to do