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.

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

so with this text. Granted that *The Principal Diseases of Lower Vertebrates* attempts to be encyclopedic, ranging from immobilization and euthanasia through anatomy, etiology, descriptive pathology, and toxicology to surgical and medical therapy, healing processes, teratology and zoonoses of fishes, amphibians, and reptiles, and succumbs to the temptation to tabulate the signs of disease a little too provincially, it is an authoritative text nonetheless.

Parasitologists may wince somewhat at the paucity of morphological measurements that differentiate, perhaps, but the more demanding scholars will find these details by utilizing the excellent bibliographies that follow each chapter as well as clear line drawings that illustrate the main anatomical features.

Pathologists will be impressed by the photographic efforts put forth to depict gross lesions, although some illustrations suffer from whole-beast pictures when localized renditions would have been more illuminating, and what appear to be older close-up photographs suffer from depth-of-field limitations. The photomicrographs are very good.

Veterinarians will find the volume indispensable as clinical reference material, and epidemiologists will consider it a handy, if brief, reference manual, especially for fish zoonoses. Hobbyists will discover that it is easy to read and has two useful glossaries that convert taxonomic inscrutability into understandable and readable prose. But we won't eat fish for some time.

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Anatid Displays

Handbook of Waterfowl Behavior. Paul A. Johnsgard. Cornell University Press, Ithaca, N.Y., 1965. xvi + 378 pp. Illus. $10.

The rationale for this book proceeds from the premise that behavioral characters can be a reliable guide for taxonomy. This view is not new—for example, Heinroth (1911), Petrunkevitch (1926), and Lorenz (1941). An explicit restatement of the methods of behavioral systematics has recently been provided by Tinbergen (1962), who states that, "These arguments [that is, the justification for the criteria by which relationships and origins are established] are all full of pitfalls . . . [but] when the various criteria are applied together and all point in the same direction, they carry conviction" (in *Evolutionary Aspects of Animal Communications*, p. 3). This conclusion is certainly open to debate. The nonoperational nature of many of the taxonomists' concepts (for example, "primitive" or "specialized" characters), the lack of demonstrated independence between biochemical, embryological, or anatomical characters, and the difficulty of measuring the susceptibility of some characters to environmental agents have not been sufficiently recognized by taxonomists. The use of behavioral characters does not avoid these pitfalls, nor does it cause the sum of a string of "uncertainties" to lead to "conviction." One merit of Johnsgard's book is that it does acknowledge (in the introduction) certain of these difficulties. Johnsgard then, regretably, ignores his own caveats, as evidenced by his facile pronouncements about which of two species is more closely related to a third, or which display is more "generalized." However, pointless as his phylogenetic conclusions may be, they do not detract from what is essentially a series of competent descriptions of anatid *displays*. (Note, however, that his title refers to *behavior*, of which displays are but one feature: there is scarcely any material on duck behavior in its more general sense.)

The index, always of particular importance in a descriptive study, allows one to find either references to a particular species or a particular display. The value of the index would be enhanced if the latter entries directly indicated to which species they apply. Even a table from which one could read, for example, the names of all species that engage in "head-pumping" would be useful. The illustrations, principally pen-and-ink drawings, are adequate; the few photographs are generally poor, both in composition and technical quality.

This volume will probably appeal only to the more fanatic devotees of waterfowl, or possibly to teachers of ethology who require examples of displays. (Since both conditions apply to me, I am pleased to have a copy.)

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Physics

Some Aspects of Non-Equilibrium Thermodynamics in the Presence of a Radiation Field. Based on a set of lectures given at the University of Paris, February and March 1961. Richard N. Thomas. University of Colorado Press, Boulder, 1965. xiv + 210 pp. Illus. $5.

This book is based on lectures given by the author at the University of Paris in February and March 1961. The lectures, in turn, were based largely on work by the author and his collaborators. There are also some bibliographical comments and an annotated bibliography which contain references to sources as late as 1965; thus, we may assume that the material in the book represents the author's current thinking on the subject of the spectroscopic analysis of hot gaseous ensembles—stellar or laboratory, with primary emphasis on the stellar part.

The nexus of the author's thesis is that stellar atmospheres *cannot* generally be described by assuming that the gas in each region in the atmosphere is approximately in local thermodynamic equilibrium (LTE) at some temperature $T(z)$. Here z is a coordinate perpendicular to the surface of the system. (Curvature effects and time dependent problems are not considered here.)

If the assumption of LTE were valid, then knowledge of the temperature and chemical composition at a given position in the system would be sufficient to determine, via the usual theory of equilibrium statistical mechanics, the complete local state of the gas. The complete description of the state of the gas consists of specifying the occupation numbers n_k, of the various energy levels of the atoms and ions as well as the velocity distribution of the particles in the various energy states. The electrons and atoms in the ground state are assumed to have a Maxwellian distribution of velocities with local temperatures $T_e(z)$ which coincides with $T(z)$ for LTE.

The inadequacy of the LTE model is due to the low concentration of particles in stellar atmospheres so that collisional processes do not dominate the transition rates. Also, the other simplifying assumption that the local state of the gas is completely determined by the radiation incident on the system from the outside is not valid