

is presented, however, as a general theory of socio-technical systems, on the basis of a small amount of illustrative data from one working group. The second study is of the performance of a class of school children. Here the analysis is "cross-sectional" rather than "longitudinal": correlations are obtained for individuals across different school subjects, between variables such as work effort, boredom and anxiety. It was found that these correlations were quite different for different individuals in the form. In neither of these investigations are details given of the number of observations on which the statistics were based, the levels of statistical significance, or the way the measurements were made. In each case all the variables were response variables, and no attempt was made to link them with casual variables—such as individual profiles of ability in different subjects, the ability and style of teaching of different teachers and so forth.

The basic problem faced by this book is an important one, and the two methods suggested are of some interest though one is not new, and the data presented are quite inadequate. No reference is made to the work of others who are thinking about the same problem.

Now there has been much debate over whether consistent personality traits can act as parameters; IQ acts in this way, for example, where speed of learning under different conditions is being studied; Eysenck thinks that extraversion, neuroticism and psychoticism can be used in the same way. On the other hand, apart from intelligence there is considerable inconsistency of individuals across different situations, and this is particularly true of social behaviour.

Behaviour is found to vary, furthermore, with the situation as well as with the person, and a number of investigators have been trying to apportion the variance observed between person, situation, and person-situation interaction. The author has found that persons are consistent for groupings of situations, which are unique for them.

Lastly, the idea of studying individuals experimentally or statistically is not new, though not much research has been done in this direction. The longitudinal covariation with time method was used in 1946 by Baldwin and Cattell (who are not mentioned). Shapiro has carried out series of experiments on single individuals. The aim of these studies, however, has been to discover the mechanisms operative in particular individuals, not to discover empirical laws.

The book contains several further miscellaneous chapters, which appear to be unrelated to the principal theme. There is a great deal of mathematics, none of which seems particularly helpful. And it is written in a highly obscure, private terminology, based on the concepts of Kurt Lewin. MICHAEL ARGYLE

Questions and Answers

Explanation in the Behavioural Sciences. Edited by Robert Borger and Frank Cioffi. Pp. xii+520. (Cambridge University: London, November 1970.) £5.00; \$15.

THIS book is a collection of essays about problems of explanation and interpretation in psychology and sociology; each essay is followed by a comment and an answer to the comment. This alternation of authors forms the "confrontations" referred to in the book's sub-title. Many of the authors and commentators are people of distinction within their field.

As is pointed out in the editor's preface, the French Academy of Sciences decided in 1775 that it would no longer examine contributions from authors purporting to square the circle; after reading this book I am inclined to say that I will no longer consider contributions containing chapters called "Is the brain a physical system?" It is an unfortunate characteristic of the "soft" sciences, which include much of psychology and sociology, that long perorations about their histories and frames of reference are published. In the "harder" sciences, such as physics, there is greater concern with the actual gathering and interpretation of real data from well-defined systems. It is my prejudice that many of the doubts expressed by some of the authors in this book are provoked by one of two causes: a lingering vitalism and a premature desire to describe complex systems whose attributes are not precisely defined and about which too little is known. The workings either of the human brain or of human society are of awe-inspiring complexities. A natural result is a feeling, not necessarily explicitly acknowledged, that particular aspects of behaviour are not accessible to experiment or ultimate understanding. All our experience in other fields of science militates against this hypothesis, so it is a little surprising to see its resurgence under several guises in this book. For this reason I find little of pedagogic or heuristic value in the book and am forced to consider it solely on its merits as an entertainment. It fails.

Many of the confrontations reduce to familiar arguments between "Little-endians" and "Big-endians": in view of the polemics involved I can only be thankful that, in this book, neither side is armed. One of the more pervasive Big-endian theses is that some phenomenon is more than the sum of its parts; the triumphant Little-endian response is that parts which have joined to form a system no longer exist as entities. Neither group is apparently aware of the possibility of non-linear interactions, which are particularly likely in biological systems, although

both groups use, rather uneasily, some of the terminology of modern mathematics. A typical situation to which these arguments are applied is that of "the depressed group": are its members necessarily individually depressed or not? The involved and opaque writing surrounding the ill-defined problems considered continually reminds me of *Private Eye's* caveat: "Or not, as the case may be." Here are two examples: "Marginal utility theory endows economic man with a refined discrimination apropos border-line cases" (p. 208); "My explanation (of a tendency to become an habitual criminal) is that these children have inherited a reticular formation which has high thresholds to incoming stimulation; hence it provides the cortex with too little arousal, and causes it to be rather slow and weak in mediating conditional responses which I hold to be fundamental in elaborating a 'conscience'" (p. 423).

I find the lack of new ideas or of intellectual precision disturbing and am forced to return to the preface where the editors helpfully mention Wittgenstein's comment that in psychology there are experimental methods and conceptual confusion and that problems and methods pass one another by. Both contentions are remarkably well illustrated by this book. There are, of course, exceptions: a few of the articles are reasonably clear and sensible, for example, those by Sutherland, Chomsky and Cioffi, but they do not make a book worth £5.00.

ANTHONY ROBERTSON

Machine Intelligence

Progress of Cybernetics. Vol. 1: Main Papers, The Meaning of Cybernetics, Neuro- and Biocybernetics. Edited by J. Rose. (Proceedings of the First International Congress of Cybernetics, London, 1969.) Pp. xiv+521. (Gordon and Breach: London and New York, September 1970.) \$24.50; £10.00.

THESE are the proceedings of a meeting held in London in 1969, which the editor calls the First International Congress of Cybernetics. The editor admits that some "somewhat fatuous contributions were included in order to bring to the surface certain undesirable accretions". He does not elaborate. "A mature science has to be able to live and cope with those who are trying to jump on the bandwagon and use it as a vehicle for their exuberant claims," states the editor. Now this may well be, but is it the best way to cope with this problem by adding to the ever-growing load of papers which the serious worker must scan?

Boulanger asks how can we discuss intelligent machines without defining