

## BOOK REVIEWS

*Psychology and Theory*, by C. J. Adcock. Wellington: Victoria University Press, 1976. 129 pp.

(Dr C. J. Adcock, an Honorary Fellow of the New Zealand Psychological Society, published his recent book at the end of a long academic career, not long after he entered his seventies. To mark the occasion, two reviewers have given us their impressions of the book, the scholar, and the issues he raises.)

### ADCOCK'S PSYCHOLOGICAL REFERENCE FRAME

Peter McKellar

*University of Otago*

Our task is to build an adequate system of constructs. It arises out of needs as living organisms, and the developed schema must in some way serve these needs.

Adcock (p. 30.)

A thoughtful overview by a New Zealander who has devoted his life to psychology as a science, and a profession, is an event of some moment. Adcock offers his perspective—to use his own term a psychological reference frame—and it is one for advanced students and colleagues. This is not an introductory book. Some chapters deal with core subject-matter: motivation, learning, perception, and personality; others concern logico-philosophical issues like the nature of theory, freewill and determinism. Much attention is paid to learning and motivation, and Adcock argues with justification “Pavlov’s account of conditioning is central to any theory of the learning process” (p. 60). After a consideration of Clark Hull and Tolman the author examines the issue of whether classical and operant conditioning are basically similar or different. The evidence of the text suggests considerable sympathy with the Hullian tradition in learning theory rather than with the Skinnerian standpoint. This is refreshing in the mid 1970s when—precisely because of the enormous contribution of Skinner—we may tend to overlook others of Stature, for example Mowrer. Of Spence, viewed by Adcock as Hull’s “leading successor” the author contends that “most of his own contributions make the synthesis with Tolman easier” (p. 80). Psychology has recently experienced a great loss through the untimely death of Professor D. E. Berlyne. Berlyne is given appreciative treatment by the author for his work on exploratory drives as “the motivation behind our cognitive activities” (p. 47). As in his other publications Adcock gives weight to the pioneer work of William McDougall on human motivation.

The sections on motivation and learning seem stronger than the chapter on perception. I may have my own schema or reference frame in this matter, but it is shared by Adcock himself who writes "the many principles studied by the Gestalt psychologists have been generally accepted into current thinking" (p. 92). Yet elsewhere Gestalt theory and experiment—the Gestaltists represented a tough-minded experimental movement, and today in particular this needs to be restated—receives minimal mention. Ivo Koehler is discussed, but not Wolfgang; and Koffka, Wertheimer and the contributions of Lewin to early experimental social psychology, are similarly absent. Another omission is the functional perception movement of Jerome Bruner and others with its emphasis on the importance of personality variables and individual differences in perceiving, an emphasis which Adcock elsewhere endorses. Developmental studies of perception are another omission, and indeed developmental psychology is accorded a limited place in this overview of the subject. On the other hand the book is strong in its treatment of personality, including the ego reference system, and is written by a psychologist who clearly accords a central place to personality. Adcock asks "What sort of psychology can we hope to supersede the schools?" His answer: "I would suggest a neo-structuralism" (p. 29). He is certainly sympathetic to the types of personality theory which emphasizes structure, notably R. B. Cattell's. Somewhat surprising is the discussion of introversion-extraversion without reference to Eysenck (or indeed Jung). The author pays tribute to the contributions of Sir Cyril Burt. This seems timely when—and in the British popular press—somewhat lesser men have started scrapping over the work of this giant of factorial psychology.

This thoughtful book invites a question. What in the mid 1970s is more important, and less important, in modern psychology? Adcock has provided his perspective, and an appropriate historical context. As regards philosophical roots he gives weight to men like Francis Bacon, Locke and Kant who helped to make the science possible. In his review article (this issue) Gregson takes issue with certain formal and philosophical aspects of the author's thought. For my own part I would have welcomed, in places, a heavy concentration of "therapeutic positivism"—which dissolves the problem just as the therapist dissolves the neurotic symptom—for example in the treatment of the freewill issue. As Francis Bacon pointed out at an earlier date "the human intellect makes its own difficulties". Skinner's somewhat naive treatment of this issue is perhaps treated too kindly; John Locke for his part coined the word "giggerish" to refer to the abuses of language which give rise to pseudo problems. In my view psychology is *not* "an overview of the nature of man"; that is only one part of psychology. As psychologists we are concerned with motivation, learning, perception and individual differences of all living organisms. The evolutionary system as a whole is our subject matter. This perspective is not wholly apparent in the overview of psychology which Adcock

himself so ably presents. Much of the work to which the author refers involves animal experiments, towards which he is perhaps more kindly than the merits of such work justifies. Far too often have animals been merely treated as "things", or "preparations" taken from their cages as a means of testing some hypothesis. In terms of research strategy and biological sophistication much of this work merits highly critical evaluation. A tradition, as Emerson might have said, is often the shadow of a great man. Students of Pavlov were succeeded by students of Hull, Neil Miller and others in often trivial experimental activities. Moreover underlying such categories as "motivation", "perception", and "learning" we encounter some motivationally strange behaviour on the part of psychologists themselves. Much of this work is summarised in Cofer and Appley's text, *Motivation: theory and research* to which Adcock makes frequent reference. Of interest in this context is Harlow's judgment in his 1953 *Psychological Review* article in which he writes of drive reduction theory that "it tends to focus more and more attention on problems of less and less importance" (Harlow, 1953, p. 27). To this Harlow adds "the double-compartment grill box is without doubt the most efficient torture chamber which is still legal" (p. 27). For my part I have always lacked enthusiasm for Harlow's own much publicised and seemingly endless research programme on social and maternal deprivation. A rather different tradition is exemplified in Wolfgang Kohler's classic work on primates. In the *Mentality of Apes* Kohler comments "the first and only desire of the separated creature is to get back to his group". He adds "very small animals are naturally extremely frightened, and show their fear to such a degree that *one simply had not the heart to keep them apart any longer*". (Italics mine; the ethical principle involved, Kohler's). Of great interest is this early experimenter's concern with individual differences between his subjects, and his apparent liking for them. Sultan, Chica, Grande, and Rana emerge as very different personalities. Adcock is much concerned with personality as a central topic of psychology, and rightly emphasizes the need to study individual differences. There would seem to be lessons to be learned from Kohler and others in the same tradition, for example Jane Goodall in the modern period. Moreover the contemporary vigorous interaction between Psychology and Ethology, with its focus on species differences, species-specific behaviour, and exploratory activity is perhaps heralding a newer, healthier and biologically more literate era of psychology.

In his concluding pages Professor Adcock writes in a thoughtful way about ethical issues. He discusses the power to coerce human individuals, adequate feedback from the governed, and adequate cognitive systems (pp. 122-123). Other ethical issues arise from the activities of psychologists themselves, and relate to what they themselves do to their human and animal subjects. Contemporary social psychology is justifiably concerned about the widespread, and often silly, use of deception techniques. In animal work, in the study of maze learning—to take a

classical example—J. B. Watson proceeded with enthusiasm to reduce sensory cues: blinding by removal of eyeballs, surgically producing anosmia, deafness, etc., etc. Other investigators followed up this tradition. One fatuous experimenter studied whether chopping off the leg of a rat would impede its previously established maze running ability. This together with enthusiasm for blinding chimpanzees, and rearing rats blinded from birth seems to me—as it would have to Kohler—a motivationally strange activity for an adult psychologist. Within the reference frame of modern psychology, and its antecedents, we have much to be proud of, and not a little that should stimulate the healthy emotion of shame. Leaving ethical issue aside, it takes a very long time to get scientifically discredited—and sometimes morally objectionable—research *out of* textbooks. Thus Brady's ulcerated "executive monkeys" and Neil Miller's shock-induced so called "displacement" experiments, will be with us for many years to come. They are certainly to be found in numerous recent textbooks. To succeed in the task of establishing an adequate system of constructs there is much in psychology that we need to reject.

Adcock has written a valuable, humane and interesting orientation to our discipline. He has, I think, been somewhat too kindly to his predecessors, in certain areas. To take another instance, in his own area of personality psychology, as Rae Carlson has pointed out, too little attention has been paid to detailed study of the individual case. Carlson's 1971 review article revealed that no less than 71 percent of the personality studies she surveyed had been conducted on students, usually first year psychology classes. Moreover they mostly involved observations made on one occasion only. The field of psychology as we know it today contains much that is trivial, and much we have reason to forget. Adcock's scholarly book will perhaps help to point to a brighter and better future for the subject.

#### REFERENCES

- Carlson, R. Where is the person in personality research? *Psychological Bulletin*, 1971, 75, 203-219.
- Cofer, C. N., and Appley, M. H. *Motivation: theory and research*. New York: Wiley, 1964.
- Harlow, H. F. Mice, monkeys, men and motives. *Psychological Review*, 1953, 60, 23-32.
- Kohler, W. *The mentality of apes*. London: Kegan Paul. (2nd edition), 1927.
- Watson, J. B. *Psychological Monograph*. 1907. 8: 33.

## ON READING ADCOCK ON THEORY

R. A. M. Gregson

*University of Canterbury*

To attempt an overview of the nature of man, and to attempt to do it briefly and at the same time with a systematic exposition, is perhaps a task that is only wisely undertaken when one has acquired the perspective of a lifetime that has been devoted to teaching and expounding a committed view. Not many of us would care or dare to set in cold print an essay in philosophical psychology on the grand scale, it is a mark of Adcock's sustained interest in advancing his view of psychology, through all the vicissitudes and false starts which we have seen in the last fifty years, when the focus of psychological attention has ranged from the physiological to the existential, and from the psychophysical to the humanistic, that he has outlined a view that has its roots clearly in the 1920's, a little before that great excursion into Behaviourism began—at least for some. The easy way is to let the confusion and contention roll on, and retire into leisurely reflection, but those of us who know Adcock would not expect that of him. He has comments to make, analogies to draw, heresies to pursue or even to advocate, and he does: moreover, he writes in a way that shows the questions of psychology still matter, and matter a lot, for him. For him the world doesn't make sense unless he has a coherent psychology, and it is not comfortable to live with doubts and incompleteness.

Adcock writes impatiently; he shoots through the problems of other minds, and the epistemological issues that bedevil perception, in a way that leaves the neophyte bemused and the professional infuriated, and he uses powerful words to leap horrendous gaps. He writes of "uniformities of behaviour" (p. 1), "essential difference" (p. 2) and "potentiality of infinite phenomenal appearances" giving us "maximum guidance in the control of behaviour". Help, stop! Some problems of philosophy (and, incidentally, systems theory) centre on the question of deciding if it is meaningful or necessary to assume infinite sets, or to assume that guidance is ever maximum—whatever that may mean. Many questions in psychology can be solved only if one stops using language quite that way, and either quantifies sets and functions or leaves them out of the picture. Adcock brings in the key words such as "suitable coding" (pp. 3 and 4) and the "maximising" of relations between input and output in order to set the stage for his neo-structuralism later on. It looks like the language of cognitive psychologists, but if we use this sort of language part of the game is to build it so that measurement procedures and formal models, in algebra or whatever, have some constrained and testable relations to raw data on the one hand and abstract theory on the other. This is not an advocacy of operational definitions, heaven forbid that that metaphysics should be exhumed, but simply a plea that we use powerful words

like "coding" and "relating" and "maximising" in such a way that we can say, at least some of the time, when the things to which they refer are happening and when they are not. Adcock doesn't; I think because he is working backwards from the factor model and the personality models later in the book to find a formal underpinning for his commitment. He needs, as a theoretician, such an underpinning; he sensibly rejects both dualism and common sense, and then finds himself creating a new metaphysics. For example (p. 7) we are told that a hypothetical construct is "based on the total existing cognitive reference frame", and that advocates of a rigorous approach to theorizing who oppose the use of the construct oppose its use for that reason amongst others. It would be true to say that advocates of rigorous theorizing oppose utterly the use of expressions like "total existing cognitive reference frame" because we can never know if we are dealing with such a thing nor ever know if it is necessary to postulate the wretched thing; if it is there it is there in some sense all the time, and hence says nothing to the scientist; remember the aether?

Adcock hints repeatedly at possible solutions to some of the problems he raises, he so often reaches out to grasp the nettle and at the last moment swerves and comes up with a bunch of daffodils. For example (p. 8) we note that "many people . . . find such concepts do not fit into their reference frames (so) they remain sceptical about results whose probability level they would otherwise find quite acceptable. Such behaviour is not at all illogical in principle". He is writing about psi phenomena, one of his own longstanding interests to which he has made valued research contributions, but the issue here is not psi, or scepticism, it is whether or not people, given the strength of their own beliefs, are willing to revise them consistently with new data when the data appear. We have formal definitions of consistency, we know that many people are hyperconservative when measured against a Bayesian norm, and that such conservatism is to some extent removable with training, and that it also can serve as a personality measure well anchored in experimental procedures. But Adcock simply says that such behaviour (i.e. refusing to change one's mind) is not illogical in principle. On the contrary, refusing to change one's mind to an appropriate degree, not more and not less, can consistently be defined as very illogical, as soon as we admit the subjective probabilities to which Adcock refers. Why does this matter, why am I quibbling at page 8 when we have to get to page 129? It matters precisely because Adcock makes it matter; in his fundamental postulates he writes that "the ultimate test must always be the . . ., effects of incorporating the observation into the cognitive system". But for a psychologist, at least of the contemporary cognitive sort, the fundamental problem is to tease out what precisely is supposed to go on when observations are "incorporated" into a "cognitive system". The questions we ask about such incorporation and such systems have become the subject of experiment and question, instead of assertion,

precisely because we have found some ways of looking at their fine-grain structure in the laboratory. The interest in much of contemporary psychology is not in the privateness or publicity of pain and pleasure, but in the ways in which experience and behaviour are reorganised, or not reorganised, as the individual moves through space and time. Again (p. 11) he very commendably reminds us that "references to pain and pleasure are most certainly not references to exclusively physical constellations of events" and then says "With some degree of inevitable error we interpret in terms of our own subjective experience". The use of "inevitable" we accept, it shuts up the sort of person who comes to W.E.A. classes having read some Moore or Russell, but what of "interpret"? Perhaps we get at Adcock's style, and objective, by allowing that if one wants to talk to the layman about psychology, then one may have to use the language of common sense to espouse a position that is not quite common sense, because common sense is an inadequate sort of psychology. But he obviously isn't writing for the layman, he assumes too much of the reader: instead I feel he is being autobiographical and telling us how he has worked through the problems for himself. What then do we make of the extended metaphors which delight if not inform? He has a most unusual car whose compression ratio varies with its speed (p. 5), and (p. 12) which transmits power when other cars do work, but we perhaps get the point, mechanistically.

On the computer analogy, which no modern writer can ignore, he comes to grief. Let us see how he does it, because we can then jump to the heart of his drive typology (chapter III) around which, to me, the whole book is built.

Adcock attempts (p. 13) to draw a parallel between a computer processing environmental data and a human being. He asserts that "for the computer the actual patterned input will be its primary data, but, if it attempts to get beyond a solipsistic position, it will need to assume that it is itself part of a wider system . . ." The idea he advances is that at this level of analysis there is a great deal in common between man and computer. Now there might be, but any demonstration that there is requires us to get right what computers do. The weakness in Adcock's account is again a magic word, this time "patterned", jumping a gap in the argument. The input to a computer is patterned first in the strict sense that it is ordered in time, so that each event  $e$  is recordable as a pair,  $e, t$  where  $t$  is an auxiliary variable counting in time. The patterns that it constructs can be on  $n$ -tuples in time, that is local sequences in some longer sequence, and given the  $t$  variables the computer can then build on patterns over different values of  $e$ ; it can build two sorts of patterns, ones that destroy the  $e, t$  pairings and ones that preserve them. Now the capacity to order inputs, like a Turing machine, is a priori to being able to accept input from the environment, and given this ordering the computer needs little else but the capacity to store, match and erase its own records in order to

mimic accurately most of what man can do. There is no need, as Adcock says, for the computer to assign its own input to one universe and some assumed world to another, but it may actually pay it to do so, because then it can readily construct world pictures out of the invariances, at some level of abstraction, which are properties of its ordered input sequences. If it does this it can in fact become a more accurate device for drawing maps of its environment; an example is the computer that takes in signals from moon probes which radio their signals back to earth, and statistically abstracts features to enhance the contrast in photographs it prints, for us, of the far side of the moon. A possible physiological analogue may arise in the case of contour enhancement in vision through the phenomena associated with Mach bands. So the computer may function better, in exploring its universe, by assuming that there is a fundamental distinction between its records and the world it receives those signals from which it uses, via temporally ordered sequences, to build its records. Data are not the events they represent, as Coombs pointed out at length. All this does not establish a parallel between man and the computer, it sharpens differences. But Adcock gets into a deeper problem when he continues "its own electronic functioning will be at one and the same time a possible object for its own study and the means by which it does study". This is disputable; Gödel showed, about the same time that McDougall was cataloguing names of drives, real or hoped-for, that there are necessary logical limits to the extent that a system can prove theorems about itself in its own language. There is a limit to how a machine can give a coherent account of how it functions. We do not know if the same argument imposes a necessary limit on man describing himself sufficiently to build a scientific psychology.

For Adcock, the distinguishing feature of man is valuing, and valuing rests on drives; we are given a list of these drives, from conation to disgust, and reasons why they are helpful as explanatory constructs. Much of this discussion, and the material on learning theory, is obsolescent except to long-suffering undergraduates. The treatment of Estes is so marred by printing errors (p. 81) and misquotation that the reader should be baffled about why mathematical learning theory developed and how it differs profoundly from the deterministic models of Hull, or of Adcock. There are general questions that nag; if theory comprises setting up a list of alternative competing and enhancing drives that exert some control on behaviour, is there such a thing as random behaviour, even in the short term? What are the rules for deciding, at any point in time, which drive is dominant, which drives will take over next at what rate, and what does the scheme of drives enable one to predict, rather than postdict, about the single individual? It is an odd feature of drive theory as he uses it that it is never clear what is to be predicted or explained, except in a most chatty and superficial way. Typology theories are out of favour, except as taxonomies based on static properties which an individual carries for



most of his life, like hair colour. They are out of favour because they only lead to cataloguing, and a catalogue is no more a description of a dynamic process extending through time and space than a dictionary is a description of a language. Yet for Adcock, though a drive is at one moment (p. 53) a construct justified only to the extent that it explains behaviour, later (p. 87) things have made a category jump; "our prime need is to adjust our behaviour so as to maximise our drive satisfaction". It seems unreasonable for man to have a need to maximise an explanatory concept, and we are still maximising in the same mysterious way that we were on page 2. Not being a personality theorist, and not understanding what Adcock means by feedback (p. 104), because he uses the word in an unusual way, so that the diagram he shows isn't feedback, and the example he gives wouldn't work without a lot more detail to prop it up, I conclude with a comment on his open and closed systems (p. 117). Very properly he raises the questions of free-will and determinism, to show that they are in his view pseudo-questions which cannot be used as objections to a scientific account of behavior. He raises these points not to deal with philosophers, who are of little import, but with those much more dangerous critics who want to misuse psychology without moral responsibility on the one hand, or object to psychotherapy being done at all on the other. It is a mark of Adcock's breadth that he ends with a plea for social responsibility and social morality, but no false optimism that these would be guaranteed simply by democratic institutions. His personal values, and his concern for human feelings and for the right of dissent, come through, and it matters not one iota whether this was because he had a drive to communicate or because some other more fashionable and less circular reason prevailed. Let us just say he wrote about how he saw the world, and psychology in it, and be thankful that he put it on record.

This is the second of a proposed series of Monographs put out by the Department of Social Welfare, the first of which was "The Battered Child". It is a large and heavy volume covering an intensive statistical analysis of not only the characteristics of illegitimate children and their parents, but also of the placement of such children throughout New Zealand. The sample comprised 84 percent of all ex-nuptial births occurring during 1970.

The book in fact is much more than a series of statistical tables, since the authors have included chapters dealing with statistical trends in New Zealand, have provided comparable statistics in selected Western countries, reviewed widely the literature concerning research papers on the topic, and examined the problems faced by the unmarried mother who keeps her child. Moreover, the fathers of such offspring are also subject to brief scrutiny.

The common stereotype of the parent of an illegitimate child is one of a young, unmarried woman having her first child. One of the findings of this study is that a considerable proportion of parents is neither young nor unmarried: the greatest illegitimacy rate is from the 25-29 year olds, 25 percent of mothers were or had been married, 33 percent had had illegitimate children before, and 33 percent lived in stable cohabitation.

One fact that is quite outstanding, and that has been presented as evidence for and against dozens of different arguments, is the very high percentage of ex-nuptial births occurring in this country. New Zealand is second only to Sweden in the number of ex-nuptial births as a percentage of all births, and the margin between the two countries is very small. As the authors point out, the illegitimacy level in New Zealand fluctuates from year to year, and they suggest it is no longer reasonable to assume a steadily rising trend. However, the overall illegitimacy rate (number of ex-nuptial births expressed as a rate per 1000 single, widowed and divorced women) for New Zealand is substantially the highest of the six other western countries examined.

It is impossible to find a single satisfactory explanation for this fact. There are many factors which have conflicting influences on illegitimacy level in a given country: e.g., a reduction in the numbers of parents marrying (as is seen in Sweden); an increase in the number of pregnancy terminations (shown in the British figures); increased use of contraceptives; and an increase in the prevalence of extra-marital sexual intercourse. Obviously, more fine-grained research is necessary before any attempt at explanation is possible.

For any such statistical surveys as this, the important factor is how the information is treated once revealed. Because of restrictions necessarily imposed by the study, the literature review, though reasonably wide (covering studies of demographic and associated characteristics

of illegitimacy, factors associated with women who decide to place children for adoption, living situations of children after birth and investigations of how they fare at a later age) has little depth. Conflicting conclusions from research are not examined in detail. Further, there is a paucity of research specifically relating to New Zealand, a not uncommon cry in the area of child, and parent-child behaviour. The general consensus from this review is that illegitimate children are disadvantaged, in that they are more likely to appear in court for misconduct and parental inadequacy, more frequently display signs of social maladjustment, are over-represented in the population of state wards (non-legitimated, non-adopted), and are more often victims of child abuse.

A final point: it would have been valuable to have had a small paper, without the many statistical tables, procedures, and molecular detail of the population studied, summing up the researcher's information and conclusions and relating it to previous studies. For most people, it is a difficult job separating this information from the structure of the research. However, this volume makes a most useful source book for those particularly interested in New Zealand social and parent-child issues.

Deryn Cooper.

*Psychology in New Zealand* by Wayne Innes. Auckland, 1976. Pp. 40.

In this contentious tract a social psychologist fulminates against his clinical and educational colleagues. They are charged with being therapeutically ineffective, having immature personalities, and maintaining elitism and racism in society. But it is not clear whether applied psychologists should put their house in order or pull it down and the author rather spoils his case by admitting (p. 36) "Of course, there are some good psychologists". Anyone expecting a comprehensive and balanced account of the state of psychology in New Zealand will be disappointed. Such an evaluation is needed but the aim of this private publication is different, it is to stir the social conscience of psychologists as well as asking people to question the need for their services. But the response may be a Bronz cheer from psychologists and indifference from the public.

W. A. M. Black.

*Behaviorism*—A review of the first six issues.

It is not customary to review journals. I am led to review *Behaviorism* for two reasons. The more specific is that I am disturbed by the philosophical naivety of many graduate students—disturbed, for example, by students who do not understand the word 'reductionism', or worse, who espouse an unabashed, even crusading mentalism. The fault, I fear, lies more with their teachers than with the students, and

I would like to persuade both teachers and students that it is worthwhile to take a look at contemporary philosophical debate in psychology, and that *Behaviorism* is one place where they could start. A second, and more general reason comes from a concern with the widely held belief that behaviorism, as the philosophy of the science of behavior, is now moribund (Segal and Lachman, 1972). Few will have been aware of the quiet defence of behaviorism, especially radical behaviorism which has appeared from time to time in the *Journal of Experimental Analysis of Behavior* (e.g. Day, 1969a, b; MacCorquodale, 1969, 1970; Salzinger, 1973; Schnaitter, 1975; Schoenfeld, 1969; and Segal, 1975). Reading the issues of *Behaviorism* may persuade (or reassure) some people that reports of behaviorism's death (Koch, in Wann, 1964), like Mark Twain's, are greatly exaggerated.

*Behaviorism* was founded by, and continues to be edited by Willard F. Day, with R. J. Herrnstein and W. V. Quine as Associate Editors, and a distinguished group of psychologists and philosophers as Editorial Consultants. It began in 1972 with the aim of facilitating "critical discussion of issues pertaining to the contemporary practice of behaviorism", including conceptual issues, methodological proposals and the discussion of ethical issues involved in the application of a behaviourally-based technology. It has survived, producing 3 volumes of two issues each, up to the end of 1975.

I do not claim to have read all the papers published, or to have understood all the papers read. Looking back, however, over the six issues, there seem to be three themes which have emerged. One theme not unexpectedly, has been concerned with the exposition and criticism of the writings of B. F. Skinner. His work has been sufficiently prolix, provocative, ambiguous and inconsistent to provide for generations of debate, so it is not surprising that almost every issue has a paper on this topic.

It is difficult, in a brief compass, to do more than give one's own tenuously supported opinion about the worth of these contributions. They range from the valuable and constructive to those which set up a caricature straw man which is then attacked heartily. Keat's paper "A critical examination of B. F. Skinner's objections to mentalism" (Volume 1, No. 1; henceforth 1, 1 etc.) is an example of the first sort. Keat identifies five reasons why Skinner rejects mentalism:

- "(1) It lacks 'explanatory power'.
- (2) It involves the employment of 'theories'.
- (3) It tends to invoke 'Homunculi' or 'inner agents' . . .
- (4) It distracts our attention from the study of *behavior*.
- (5) It involves a dualistic ontology of the 'mental' and the 'physical' ". (1, 1; p. 55).

He argues that reasons 1-4 are at best inconclusive, but that (5) which may be the most powerful, is one which Skinner himself discounts. This criticism may make the radical behaviorist uncomfortable, but does at least provide both instructive exposition and a starting point for further work. The other sort of criticism is seen in Theophanous' paper "In defence of self-determination: a critique of B. F. Skinner" (3, 1). While undoubtedly well-intentioned, the dark picture painted of evil psychologists manipulating their fellow men is based on a substantial misrepresentation of Skinner's position. Skinner is a determinist, but it is hardly fair to represent him as asserting that "the environment *totally* determines all behavior" (3, 1; p. 99, italics added), since he has several times acknowledged genetic and biological factors to be important (Skinner, 1969). His followers, confronted by autoshaping, instinctive drift, and other perplexities are even more aware of these variables (see Schwartz, 1974).

Skinner has several times drawn attention to the parallel between organic evolution and individual behavior change, and has stressed that analogy between natural selection and reinforcement: "The environment is obviously important, but its role has remained obscure. It does not push or pull, it *selects*, and this function is difficult to discover, and analyse". (1971, p. 30). He lays emphasis on the *principles of selection* because he played a major part in their explication, but acknowledges the need to discover complementary *principles of variation*, and this has been done by others (Gilbert, 1972; Staddon and Simmelhag, 1971). Staddon, in a paper "On the notion of cause, with application to behaviorism" (1, 2) provides a further good discussion on this matter.

Especially in his 'popular' writings, Skinner does give the impression of relying exclusively on the concept of reinforcement to explain all the phenomena of behavior. This weakness is the subject of an essay by Malone (3, 2) in which in a critical, but sympathetic way he compared Skinner's heavy reliance on reinforcement with William James' rejection of a similar reliance on 'interest':

Skinner is not particularly interested in phenomenal description, or even in careful objective description of the behavior of others. His goal seems to lie in convincing his readers that whatever the phenomena . . . his analysis can deal with it. His account in turn, rests almost entirely on the concept of reinforcement . . .

I don't feel it unfair to say that Skinner . . . has often used the term 'reinforcement contingencies' as James *could* have used 'interest'. . . . James, unlike Skinner, recognised that 'interest' is only a summary term for all the ways that the world acts in phylogeny and ontogeny to make events more or less interesting.

If reinforcement is used in a similarly vague way, then a translation of psychological phenomena into reinforcement—derived terms is of limited usefulness. (3, 2; p. 146-147.)

Skinner may thus share some of the blame for the sometimes harmful preoccupation with "reinforcement" evident in behavior modification, an error occasionally compounded by the belief that an event is a reinforcer because the therapist/teacher/parent says it is.

Two other authors deserve mention in this context. Vorsteg (2, 1) for an analysis of Skinner's determination, which he argues involves a confusion between determinism and lawfulness; and Knapp, for indices to *Walden II* (3, 2) and *Beyond Freedom and Dignity* (2, 2).

A second theme has dealt with conceptual difficulties, and definitional problems in behaviorism. Attention has focused on the concept of the operant (Catania, 1, 2. Sheldon, 2, 2) and reinforcement (Goldiamond, 3, 1 Smith, 2, 2). The most significant of these papers is Goldiamond's "Alternative sets as a framework for behavior formulations and research" (3, 1). It begins as a comment on proposals by Michael (3, 1) and Mixon (3, 1) that the operant terminology be reformed by removing the term "negative reinforcement". He argues that this is simply tinkering with the "appalling" terminological confusion, because present terminology is bound up with a unilinear view of behavior:

"The two frameworks to be considered are (1) the present *unilinear* framework which describes behavior by contingencies into which it enters, and (2) a proposed framework of *alternative* sets, which requires a description not only of behavior and its relation to environmental events, but also of alternative sets of behavior-environment relations". (p. 50).

He suggests that with its recent emphasis on the analysis of choice, operant research is beginning to do this, and that practice is outstripping the conceptual and terminological means available to handle the data. While he is not the first to suggest that greater efforts should be made to capture the complexity of the stream of behavior (Dunham, 1970; Schoenfeld and Farmer, 1972) Goldiamond's suggestions for terminological and conceptual reform are distinctive, and extensive. I fear, however, his reforms may suffer a common fate—rejection through indifference.

A third theme, and one which may appeal to a wider audience, has been concerned with ethical and legal issues in behavior modification. It began with the reprinting of Wexler's now famous "Token and taboo: behavior modification, token economies and the law" (1, 2) and has continued with papers by Goldiamond in an invited reply (2, 1) and Wexler (3, 2) and Vargas (3, 2).

Except for the area of methodological innovation, the journal appears to be meeting its expressed aims fairly well, and there are, of course, many papers which do not fall within the themes I have outlined. I hope, however, that I have indicated the tone of the journal with

reasonable fidelity. I hope too, that *Behaviorism* will gain a wide readership, because I agree with Scriven that there is "no longer any adequate justification for forcing the beginning psychologist to repeat for himself, and perhaps never transcend the philosophical errors long since exposed as such" (Scriven, in Wann, 1964).

What, finally, of value for money? At US\$12 per annum airmail, reduced to approximately NZ\$6.00 by the Vice-Chancellors' benevolence, it must surely be rated 8 out of 10.

#### REFERENCES

- Behaviorism: A forum for critical discussion. Volumes 1, 2 and 3.* W. F. Day (Editor). University of Nevada, Reno; 1972-1975.
- Day, W. F. On certain similarities between the *Philosophical Investigations* of Ludwig Wittgenstein and the operationism of B. F. Skinner. *Journal of the Experimental Analysis of Behavior*, 1969 (a), 12, 489-506.
- Day, W. F. Radical Behaviorism in reconciliation with phenomenology. *Journal of the Experimental Analysis of Behavior*, 1969 (b), 12, 315-328.
- Dunham, P. J. Punishment: Method and theory. *Psychological Review*, 1971, 78, 38-70.
- Gilbert, R. M. Variation and selection of behavior. In R. M. Gilbert and J. R. Millenson (Eds). *Reinforcement behavioral analyses*. New York: Academic Press, 1972.
- MacCorquodale, K. B. F. Skinner's *Verbal Behavior*: a retrospective appreciation. *Journal of the Experimental Analysis of Behavior*, 1969, 831-841.
- MacCorquodale, K. On Chomsky's review of Skinner's *Verbal Behavior*. *Journal of the Experimental Analysis of Behavior*, 1970, 13, 83-99.
- Salzinger, K. Inside the Black Box, with apologies to Pandora. A review of Ulric Neisser's *Cognitive Psychology*. *Journal of the Experimental Analysis of Behavior*, 1973, 19, 369-377.
- Schnaitter, R. Between organism and environment: A review of B. F. Skinner's *About Behaviorism*. *Journal of the Experimental Analysis of Behavior*, 1975 23, 297-307.
- Schoenfeld, W. N. J. R. Kantor's *Objective Psychology of Grammar and Psychology and Logic*: A retrospective appreciation. *Journal of the Experimental Analysis of Behavior*, 1969, 12, 329-347.
- Schoenfeld, W. N., and Farmer, J. Reinforcement schedules and the "Behavior Stream". In W. N. Schoenfeld (Ed. *The theory of reinforcement schedules*. New York: Appleton-Century-Crofts, 1970.
- Schwartz, B. On going back to nature: A review of Seligman and Hager's *Biological Boundaries of Learning*. *Journal of the Experimental Analysis of Behavior*, 1974, 21, 183-198.
- Segal, E. F. Psycholinguistics discovers the operant: a review of Roger Brown's *A first language: the early stages*. *Journal of the Experimental Analysis of Behavior*, 1975, 23, 149-158.
- Segal, E. M., and Lachman, R. Complex behavior or higher mental processes: is there a paradigm shift? *American Psychologist*, 1972, 27, 46-55.
- Skinner, B. F. *Contingencies of reinforcement: A theoretical analysis*. New York: Appleton-Century-Crofts, 1969
- Skinner, B. F. *Beyond freedom and dignity*. Harmondsworth: Penguin, 1971.
- Staddon, J. E. R., and Simmelhag, B. The superstition experiment: A re-examination of its implications for the principles of adaptive behavior. *Psychological Review*, 1971, 78, 3-43.
- Wann, T. W. (Ed.). *Behaviorism and phenomenology: Contrasting bases for modern psychology*. Chicago: University of Chicago Press, 1964.

Nevile M. Blampied  
University of Canterbury.

*Social Exchange Theory* by J. K. Chadwick-Jones. European Monographs in Social Psychology. Academic Press, 1976.

*Social Psychology and Intergroup Relations* by M. Billig. European Monographs in Social Psychology. Academic Press, 1976.

These two recent additions to the European Monograph Series, although sharing in part their subject matter, provide a contrast in style and approach.

Chadwick-Jones' Monograph, true to its title, presents an account of what may fairly be described as the three major social exchange theories, those of Thibaut and Kelley, Homans and of Blau. The work of Thibaut and Kelley gets pride of place as the most comprehensive of the three. Chadwick-Jones reviews experimental work arising from the theory and attempts to extrapolate from it. In this latter, he clearly indicates that the theory has only been partly successful. Chadwick-Jones presents his material factually and unemotionally, not often drawing conclusions himself but leaving the experimental evidence to speak for itself. With Homans and Blau, Chadwick-Jones spends less time, concentrating on the relationships between non-behavioural elements of the theories and their behavioural correlates. The book ends with a short but effective dissertation on exchange theories and reductionism. Chadwick-Jones rarely reveals his own thoughts or leanings, and as a consequence the book has a clinically sterile flavour. His expositions of the three theories are good, but the style does not lead the reader to become involved in the material. Indeed, since Chadwick-Jones does not reveal his own position on the various issues, one does not even know whether one should argue with him or not!

Billig's treatment of his more diverse subject area of intergroup relations is very different. He presents material within the context of his own theoretical position which is psychodynamic in nature, and evaluates it from this standpoint. This is particularly the case in earlier chapters which review Freudian group psychology and its derivatives, frustration-aggression hypotheses and so on. In these earlier chapters, the text is more destructively critical than constructive. Billig evaluates and finds wanting a number of specific theories as diverse as Rokeach's stance on left-right-wing dogmatism and Sherif's analysis of "group" and "super-ordinate" goals. It is later, when he begins to relate some of these diverse ideas to practical issues and to his own theoretical position that the book truly comes alive. This reviewer was particularly impressed by Billig's analysis and appraisal of Gaming and Rational Conflict. He makes a clear distinction between theoretical gaming models and the influence of social processes in real-life human decision-making. The extension of this argument, to show how "ideologies" and "social categories" influence specific groups in their intergroup relations is well made, and linked effectively with a criticism of theoretical reductionism in this area. In this latter, there is an interesting contrast with Chadwick-Jones' position.



The Billig approach warms up as it goes along and takes the reader along with it. All sections and chapters are carefully interrelated and each one leads on naturally from its predecessor. Billig presents a comprehensive treatise which is integrated by reference to a central philosophical position, which is nonetheless not too obtrusive, nor too "off-putting" even to the behaviourally-oriented reviewer. It is the kind of book you have to read right through however; otherwise you miss the overall picture. One wonders if students in our current climate of grade-credit oriented universities will find the time to do this!

George Shouksmith.

*Approaches to the Study of Social Structure* by P. M. Blau (Ed.). London: Open Books, 1976. ix + 294 pp. N.Z. \$6.95.

This book presents a number of theoretical approaches to the sociological study of social structure by eleven American and one British academic. Bottomore presents a Marxist view of social structure and history, Lenski an interpretation of social structure in evolutionary perspective which is critical of cultural explanations of social structure. Both emphasize the link between advancing technology and change in social structure. Homans and Coleman argue that social structures are rooted in the psychological processes of individual behaviour. All the other authors give priority to social factors.

The theories most prominent in the book are functionalism and structuralism. Merton provides a brief, clear introduction to structural analysis that involves a confluence of elements deriving principally from Durkheim and Marx but also from de Saussure and Jacobson in linguistics, Levi-Strauss in anthropology, Piaget in psychology, and Jacob in biology. Lipset discusses the complementary understanding of social life provided by functionalism and Marxism, and argues that close inspection of these theories provides substantial areas of agreement. Lipset and Coser both locate the sources of social conflict and change in social structure.

A recurrent theme in the book is that sociology (in the English-speaking world) has undergone a breakdown in consensus about the nature of the discipline, theoretically and methodologically, in the past decade or that the apparent consensus of the 1950's and 60's was a myth. The statement of Polish sociologist Stefan Nowak, quoted by Merton, conveys the tone of the book: "we will have to live for a long time with many *partial theories*—mutually complementary, and cumulative in different senses of the term, applicable to different aspects of social reality, answering to different theoretical questions, and useful for different practical social purposes".

B. G. Stacey.

*Towards the Validation of Dynamic Psychotherapy: A Replication* by D. H. Malan, New York: Plenum, 1976. pp. 298.

This book is very misleading. It purports to have verified an earlier but inadequately presented study, but in fact it describes an interesting method by which the occurrence of psychodynamic phenomena might be recorded. That much apart, very little can be said in its favour. Malan seems merely to have been intent upon resurrecting data from past records, and he appears to have fashioned his research design as he went along. He gives little information about the total pool from which he drew a sample of 39 and could carry only 22 right through to follow up for variable periods after a maximum of 40 weeks of undefined psychoanalysis from a total of 11 analysts of unstated training and experience from undeclared stables. Two of the analysts then made sundry retrospective assessments of subjective factors such as motivation and transference interpretations from records of initial interviews, first sessions and projective tests.

Much play was made of the fact that three of the five observers were "uncontaminated" by prior knowledge of the outcome, but, since they were themselves novitiates in psychoanalytic training, it is unlikely that they were sufficiently independent in their intellectual and perhaps emotional orientation to be at odds with their mentors. More than that, the authors (p. 186) left the observers to define some of the criteria they were to observe *and* to devise their own ratings *and* to apply them without practice! No matter how sloppy the assessments, they were duly quantified with touching recourse to an arithmetic accuracy of ".125" before being subject to "hundreds of correlations"—tau rather than product-moment. Inevitably some results turned up trumps by chance alone, but no matter because the hypotheses, methods and measurements were so loose that in one instance Malan actually said. . . .

"it makes little difference to the original hypotheses whichever of these alternative (explanations) is chosen" (p. 140)!

He also admitted that in one instance he even changed the method of scoring to get agreement between different observations (p. 241), while elsewhere he pays lip service to scientific objectivity. Incidentally, he claims to have had a statistical adviser, but I can't think that he took advice.

Despite all this slipshod work, Malan claims to have verified the results of his earlier study. But, a careful reading of his conclusions shows that he merely obtained evidence in favour of the "transference/parent link" (p. 273). Hardly an unexpected discovery from studies of the classical psychoanalytic school rather than of the more recent that include social dimensions of behaviour.

Those who cannot brook psychoanalytic psychotherapy will have a field day with this book if they can be bothered to read it. Those who look forward to a proper assessment of psychoanalysis with

controlled studies must go on wanting. Is it too much to hope that the Tavistock Clinic will soon establish a reputable research group? The matter is far too important to be left to well-meaning amateurs.

A. J. W. Taylor.

*A Primer of Multivariate Statistics* by R. J. Harris. New York: Academic Press, 1975. xiv + 332 pp.

*Multivariate Statistical Methods* by D. F. Morrison. Second Edition. New York: McGraw Hill, 1976. xv + 415 pp.

For most people, courses entitled "Psychological Statistics" refer to activities grudgingly undertaken by undergraduates most of whom see them as not only a chore but an obstacle to spending more time on what current fashion dictates to be those areas of Psychology (or Education) most relevant to Real Life. It is hard not to sympathise with this attitude since most such courses, when they have passed the stage of descriptive statistics, usually focus on techniques for testing simple hypotheses using data gathered under strictly controlled sampling and experimental conditions. (Nevertheless, courses covering the variance-partitioning designs appropriate to such conditions are quite easy to teach and whoever grasps the basic principles should be competent to design and analyse fairly complex experiments aided by whichever of the texts of, for example, Keppel, Kirk, Lee, Lindman or Winer is found most comprehensible.) One also, however, encounters Real Life as a place where not all of the operations of all of the variables can be controlled all of the time and is therefore at a distinct disadvantage relative to one who chooses to work in the laboratory when it comes to ease of entry to the techniques appropriate to the analysis of data. A course in statistics may well have enabled one to grasp the nettle of multiple regression, but not as a special case of canonical correlation; it may have introduced one to factor, or more likely, component analysis not as a means for transforming a set of actual data into a set of uncorrelated composites of these data, but rather of determining the alleged basic structure of a correlation matrix, and it almost certainly will not have removed the aura of mystery surrounding it by demonstrating its relationship to analysis of variance (Burt, 1947). Again, one's course will have taught the difference between uncorrelated and correlated t-tests, but not that it can be risky to draw conclusions from a string of t-tests carried out on correlated variables such as those of the sub-tests of the W.A.I.S. and the profile elements of the M.M.P.I. and 16 P.F. as if each such t-test were independent of the others. Further, until recently there was a severe dearth of text and handbooks on multivariate design and analysis (in general, as opposed to factor analysis) which were not daunting to those timorous of strings of bold-face symbols each representing an ordered set of data.

For the psychologist, student or professional, R. J. Harris's *Primer of Multivariate Statistics* does not represent a new departure from this state of affairs. In Britain, Hope's (1968) *Methods of Multivariate Analysis*, and in the United States, Tatsuoka's (1971) *Multivariate Analysis* seem to have marked the beginnings of a fairly serious effort gently to expound the principles and applications of multivariate statistics to behavioural scientists. Of these two, Hope's is by far the more condensed and serves to illustrate by admirably simple examples that a given set of matrix operations will achieve a certain result. While never chatty, it is plainly addressed to the reader. Tatsuoka's is much more a text, more lucid and comprehensive in its coverage, but but less engaging in its style.

Harris's book also gives the impression of being addressed to the reader, which is not surprising given its origins in the teaching activities of its author. It uses a substantial part of the first chapter to demonstrate the parallels between the statistical procedures to which most psychologists are accustomed, e.g. correlation and sampling, and their multivariate counterparts and extensions. This leads one further into the book than would probably be the case if the first chapter were merely an exposition of matrix algebra. The latter is found in one of the Digressions at the back of the book, to which the reader is referred after encountering the elegance of matrix formulations following the relative clumsiness of the conventional derivation of weights in multiple regression, which topic (with which most readers will have at least some prior acquaintance) takes up the bulk of the second chapter. At the end of this chapter are problems in the solution of which the reader is urged to use the appropriate segment of a BMD or SPSS programme, and to which answers with brief commentary are supplied. This same approach of using known starting points, minimising the amount of page-space devoted to sheer algebraic symbolism and using example-exercises which are not collections of random numbers, leads the reader from Hotelling's  $T^2$  via fairly simple multivariate analysis of variance techniques and canonical correlation to principal components and factor analysis. Discriminant analysis is dealt with rather sketchily for this reviewer's taste and is not related to classification issues, one of the strengths of Tatsuoka. Harris does, however, offer useful comments on issues such as non-linear and relationships between variables, violation of distribution assumptions and the application of the general linear model as a concept unifying a wide array of statistics. His final large section is one on the use of the BMD, SPSS and OMNI-TAB package for conducting the methods of analysis dealt with in his text, and includes some main programmes for calling procedures from the set of IBM scientific subroutines where this leads to easier computer usage. These are valuable, although installation-dependent and hence should be treated with care. More space could have been spent, perhaps, on helping the novice to read outputs from the BMD and SPSS programmes relevant to the techniques Harris deals with.

From all this, it may be gathered that Harris's *Primer* can almost be read rather than ploughed through, by an intelligent non-mathematician. The same cannot be said for Morrison's text, the new edition of which retains the same virtues as the first, namely, a rigorous deduction from mathematical/statistical first principles of every form of multivariate analysis with which it deals. Although it is ostensibly addressed to the needs of students, researchers and consulting statisticians in the life sciences and described as "an elementary source", this should not be interpreted as meaning that it is a book for those who simply want to flirt with the basic concept of something before putting data through a computer in order to apply it. It is first and foremost a text for the fairly dedicated. To the first edition's coverage of tests of hypotheses on means, multivariate analysis of variance, canonical correlation, principal components and factor analysis, the second adds a brief but very useful chapter on discriminant analysis and a section on analysis of trend. Canonical correlation, formerly having a chapter to itself, is now a brief section in a chapter concerned with testing hypotheses concerning covariance matrices. (If the reader has noted a similarity in coverage between Harris and the first edition of Morrison, it is not there by coincidence. Harris's introduction to multivariate analysis, he says, was provided by Morrison.) These additions and alterations are valuable but not sufficient to oblige the owner of a first edition to rush out and buy the second, although I suspect he may take long looks at somebody else's copy.

Of these two books, Harris's is by far the better introductory text. (Its only disappointment is in the lack of an adequate coverage of classification procedures based on discriminant analysis. For this the user must consult Overall and Klett (1972), Tatsuoka (1971) or Van de Geer (1971) for an exposition at the same level as Harris's if the section in Morrison is still misperceived as too difficult.) As noted already, it is eminently readable and with some supplementary notes should provide an excellent text for courses concerned with the demonstration that the bark of multivariate statistics is far worse than their bite.

#### REFERENCES

- Burt, C. A comparison of factor analysis and analysis of variance. *British Journal of Psychology (Statistical Section)*, 1947, I, 3-26.
- Hope, K. *Methods of Multivariate Analysis*. London: University of London Press, 1968.
- Overall, J. E., and Klett, C. J. *Applied Multivariate Analysis*. New York: McGraw Hill, 1972.
- Tatsuoka, M. M. *Multivariate Analysis: Techniques for Educational and Psychological Research* New York: Wiley, 1971.
- Van de Geer, J. P. *Introduction to Multivariate Statistics for the Social Sciences*. San Francisco: Freeman, 1971.

Alan R. Forbes.

*Soviet Psychology: History, Theory, Content* by J. McLeish. London: Methuen, 1975.

If one wants to learn about Soviet psychology, then the first precaution one should take is to avoid reading any critiques offered, all too generously, by *godfearinganticommunists* and their academic byproducts, unless one can also read Russian and go to the source materials directly. McLeish has done a service to English speaking psychologists by reviewing the background, development and meta-theoretical stance of modern Soviet psychology without telling us how good or bad Marxism is. This book can profitably be used to teach students who are honest enough to ask, why and how Soviet psychology rejects both mentalism and behaviourism, why the methodology of Soviet psychology has much in common with our own though the objectives differ, and why the reduction of psychology to physiology is firmly rejected by Soviet psychologists although their historical development over the last two centuries has been repeatedly focussed on psychophysiological questions.

There is a comprehensive review of what has happened since Pavlov, but I would like to have seen more on cybernetics in Eastern Europe; an area which has provided the nearest to a common language between Soviet and Western psychologies in the last decade. The book stops about the mid 1960's, when the pace of change and growth began to accelerate. If at the end of McLeish's review, which is as near to impartial as I have seen, the reader, begins to suspect that no one, marxist, nonmarxist, or antimarxist, has yet got an adequate framework within which to structure psychology, then my sympathies are extended to him (or her).

Robt. A. M. Gregson.

#### NOTICE

Members who wish to have abstracts of the Conference papers published in *The New Zealand Psychologist* in the second issue of 1977 should follow the instructions for preparation which were published in the April 1976 issue, page 60. The dateline for copy will be September 16, 1977. The attention of members is drawn to the format and arrangement of abstracts published from last year's conference, in November 1976, which may serve an convenient precedents.