

Copyright is owned by the Author of the thesis. Permission is given for a copy to be downloaded by an individual for the purpose of research and private study only. The thesis may not be reproduced elsewhere without the permission of the Author.

REINTEGRATION AND SYNTHESIS IN THE  
BEHAVIOURAL SCIENCES : JUSTIFICATION, A POSSIBLE BASIS  
AND SOME DEMONSTRATIONS OF FEASIBILITY

A thesis presented in partial fulfillment  
of the requirements for the degree of  
Master of Philosophy  
in Psychology at  
Massey University.

Keith Alfred Hawkins

1980

## ABSTRACT

The behavioural sciences are considered to be excessively fragmented both within and across disciplines. The case for this claim is presented, followed by analysis of special problems that confront the development of more unified, coherent behavioural science endeavour. It is proposed that man's status as an evolved biological entity be more fully explored both as a fundamental base from which diverse approaches can grow, and as an ultimate conceptual framework, or set of parameters, within which the various perspectives and formulations should cohere. The argument is developed that the most fruitful starting point in these terms is the exploration of man's basic processes and organization at the bio-psychological level of analysis. Brief investigation of (Skinnerian) behaviourism as a paradigm provides the opportunity for specific demonstration of value, as broader bio-psychological formulations are considered to enable : (a) more satisfactory treatment of anomalies that have developed relative to this paradigm, without loss of the real gains accrued from its development as such (b) productive links to be established with formulations hitherto perceived as 'opposing' (c) the development of more directional, comprehensive theoretical status in what has been an essentially pragmatic development.

---

## ACKNOWLEDGEMENTS

Special thanks to Dr David Stenhouse (who is in no way responsible for shortcomings hereof), and Rachel Weatherall for her patience and skill in typing this thesis and Lorraine Gullery for her additional assistance.

## TABLE OF CONTENTS

<u>Introduction</u>	1
<u>SECTION ONE</u> : The Current Fragmented State of Behavioural Science Enterprise : Some Effects of it, and Discussion of Factors Possibly Implicated in its Development and Perpetuation	14
Reactions to the current multi-discipline structure : the literature of discontent	19
Category one	20
some candidate areas for interdisciplinary co-operation	25
Category two	29
Category three	36
Fragmentation in psychological analysis	47
Basic divisions in psychological analysis	49
Psychology : separate sciences or embattled paradigms	52
Separate sciences	52
Embattled paradigms	53
A reconciliation	55
Paradigm processes in behavioural science	58
Further Factors responsible for the perpetuation of fragmentation, isolationism and hostilities in behavioural science enterprise.	62
Factors implicated in the evolution of the current fragmented state	63
Fundamental discord	63
Complexity	67
Imitation of the pure sciences, splits over methodology	71
Youth	79
Difficulty of proof: Accountability and fashion	80
Outcomes of defensiveness: Perpetuating factors in the current state.	81
General indications of defensiveness	82
Jargon	84
The Need for good theory	84

<u>SECTION TWO: Consideration Of Man's Biological Status</u> as the Basis For a Coherent and Parsimonious Approach to the Study of Behaviour	85
The nature of man and sociological commitments	87
The biological contribution : basics	93
Impetus to incorporation of functional biology into the behavioural/social sciences : some possible advantages	95
Possible reasons for the neglect of the biological contribution	98
Bibliography	102

---

## INTRODUCTION

Within the global behavioural science<sup>1</sup> field, study, practice, and research is excessively fragmented. This thesis is an attempt to:

- (1) identify and analyze the nature of this fragmentation;
- (2) consider possible negative effects, for both the general progress of behavioural science and the efficacy of individuals working within, with particular focus on the general field that traditionally falls under the rubric of psychology;
- (3) suggest how some of the negative effects contingent upon current structure and practice may be (in part) countered; and
- (4) demonstrate the value of the corrective advocated.

Students entering tertiary institutions with the intention of studying human behaviour face, over a number of years, a series of significant decisions. They are initially constrained to choose a select few subjects to study from an impressive list of disciplines that concern themselves with man and his behaviour : zoology (ethology), behavioural genetics, psychology, sociology, anthropology and some less obvious, such as education, philosophy, cultural geography, and economics. Tertiary education is usually so structured that within a year or two selection is substantially narrowed further; the student typically selects one discipline at the expense of the others. Selection doesn't end there, however. Within an established discipline the student (for a variety of reasons) may become an adherent of one particular 'school' of thought over others. (Within sociology, for example, he may embrace a general Marxist orientation, or Parsonian structural functionalism; within psychology Skinnerian behaviourism may be adopted at the expense of a 'third force' orientation).

<sup>1</sup> 'Behavioural science' is used sufficiently generally to encompass all approaches to the study of behaviour. In this regard, note that it is assumed, for example, that animal behaviour studies frequently have as an ultimate goal increased understanding of human behaviour. The validity of these is always in question, and this will find expression in objection to the use of 'behavioural science' as a generic term, especially by those who would stress 'action' over 'behaviour', and 'meaning' over 'mechanism'. Hold your horses : some sort of reconciliation of the basic assumptions and approaches characteristically reflected in term preference is a major goal of this dissertation.

The important outcome is that the emerging behavioural scientist acquires an *increasingly restrictive set of commitments* along several dimensions. He gains a general identity and way of viewing the universe of human behaviour through disciplinary specialization, and a tighter set of commitments through tending to favour one theoretical orientation over others within the discipline. It is the nature and effects of this commitment that differentiates this process from specialization as it occurs within the physical sciences<sup>1</sup>. for interwoven with this process is 'resolution' of dilemmas real to introductory students, but frequently non-existent to someone who has acquired an ever-narrowing constellation of commitments with educational passage.

These dilemmas rest on perception of the disjointed nature of behavioural science theory, knowledge, concepts, and underlying assumptions across and within disciplines. Introductory students frequently feel lost,<sup>2</sup> both with regard to the respective roles of disciplines, and to the validity of different positions identifiable within a given disciplines on specific issues ("who, or what, are we expected to believe?"). A basic premise of this thesis is that guides or criteria for differentiating between behavioural science problems on the basis of the appropriateness of respective disciplines, and theories within, are frequently elusive (and in some areas are near to non-existent). Ready recourse to simple explanations of student confusion as failure of the uninitiated to comprehend the subtleties of differential and selective focus at both inter- and intra-disciplinary levels is denied.

---

1. 'Natural', 'pure' and 'physical' are used as interchangeable with reference to traditional science (e.g. chemistry, physics). Note that specialization, a parallel to that evident in the natural sciences, occurs in behavioural sciences. For example there are physical anthropologists, social anthropologists; sociologists primarily concerned with the relationships between social class and crime, others with migration; and there are psychometricians, psychophysicists, industrial and clinical psychologists, and so on. This is regarded as essentially different in kind to the *narrowing* of primary concern to this thesis, although there are some relationships evident, and some parallel negative consequences. These will be discussed where relevant.

2. A strong impression gained from tutoring, and supported by conversations with other tutors.

Section One represents an attempt to establish that behavioural science is excessively fragmented, this being both the cause of confusion in introductory students and, ultimately, the reason why those well established in a particular niche are often no longer aware of this state, or if aware, deny that it could be any other way. The extent, nature, and consequences of this fragmentation is analysed, with particular reference to aspersions cast on the overall scheme of things by several prominent behavioural scientists, and, correspondingly, their hopes for future improvement.

Initial focus rests upon the current multi-discipline structure of behavioural science, with an argument developed that, rigidly maintained, it represents misconceived emphasis on the significance of emergence at the expense of recognition of unity and continuity in behavioural phenomena.<sup>1</sup> The major preoccupation of this thesis, however, is with fragmentation apparent at the level of analysis conventionally labelled the psychological. There are two major reasons for this:

(1) The author's personal identity is interwoven with the major concerns associated with this level of analysis, i.e. with the study of individual behaviour, as opposed to such concerns as the general comparison of cultures, analysis of institutional outcomes of social group clashes, or reflexology of fruit-fly limbs. Note this does not imply such phenomena are not of interest, or they can be ignored with impunity; rather this is simply a statement of primary personal interest and ultimate preoccupation. In line with general argument condemning rigid disciplinary segregation, some interest and basic understanding of other such matters and products of different levels of analysis is considered not only necessary, but personally inevitable.

(2) Important, complex links are held to exist between fragmentation at the 'macro' and 'micro' levels. Causes and outcomes of the splintered, embattled schools state of psychological analysis parallel those identified in the larger, rigid multi-discipline situation. For example, similar issues such as the proper role of reduction in behavioural analysis are involved, and negative outcomes - such as those that emerge from 'us versus them' conceptions - are common to both states.

---

<sup>1</sup>. Primary concern is that members often act as if disciplinary boundaries are clearly dependent upon and simply related to scale changes in level of analysis.

But the embattled-schools of psychology situation is much more than simply a micro to the multi-discipline state macro; the link is more fundamental and rests on two basic (but related) observations:

(A) Science, and knowledge, is ultimately unitary; behavioural science problems do not fall into neat disciplinary boxes. The very idea of permanently-fixed categories - branches of study, research, learning - may be misconceived.

(B) The common thread that finally binds all behavioural science theory (insofar as it purports to shed light on human behaviour) exists in the assumptions made about the nature of man, of the operating principles of the species,<sup>1</sup> *whether these are explicit or otherwise*. The clarification and analysis of these assumptions centres on the psychological level of analysis (not to be confused with *the discipline of psychology*<sup>2</sup>), although of course other realms/levels of analysis are inevitably involved owing to the ultimate interrelationship of all levels of analysis.<sup>3</sup>

What is of central concern is the incorporation of theory of, and ongoing debate over, ideas and assumptions about the basic machinery of the behaving human being : how it learns, how its functioning is organised and channelled, how its behavioural capacities develop, how it becomes socialized - as reflected in positions adopted along basic dimensions, e.g. essential passivity/activity, malleability/structural constraints, nature and influence of maturational processes in behavioural/skills development, nature and relative importance of 'lower' and 'higher' processes in behaviour determination, relative significance of internal or external sources of control. Positions adopted (consciously or otherwise; covertly or overtly) know no disciplinary boundaries. It is through psychological problems (and hence paradigms) that the necessary links between biological and social analyses are wrought.

<sup>1</sup>. A phrase first encountered when listening to a taped discussion between N. Azrin and D. Premack. Premack used the phrase to point to a conceptual realm largely neglected by mainstream behavioural psychology. (See Azrin, N. or Premack, D. Tape).

<sup>2</sup>. Psychological analysis is not the sole prerogative of psychologists, nor is it the only type of analysis that psychologists engage in in the course of their work. Although these points seem self evident, this is to some degree illusory, and the clarification made is basic to this thesis.

<sup>3</sup>. Even if this is insufficiently realized in the current multi-disciplinary structure and practice of behavioural science.

Evidence for this is provided by the fact that, despite structural impediments, collaborations do occur across disciplinary boundaries, and, even when not actively sought or developed, convergences have emerged. Of particular interest is the overt employment of psychological assumptions, concepts, and theories by certain sociologists. In this regard it should be noted that, while some sociological theorists declare incorporated psychological elements, others are more covert, or perhaps don't even recognise borrowings and dependencies. Most importantly, those that do openly employ psychology theory *draw from conflicting sources*. As basic issues transcend disciplinary boundaries in this manner, an important link between the fragmented state of behavioural science at the macro level (rigid multi-disciplinary structure) and micro (warring schools of psychology) is thus established - for, while the rigidity of disciplinary boundaries is a problem in its own right, an arguably more significant set of differences exists between paradigms at odds over the ultimate concern that links all behavioural science - the fundamental nature of man.

While, as argued, the attack on the basic issues delineated requires centring on the psychological level of analysis, this by no means belittles the importance of efficient inter-disciplinary interaction and co-operation (to believe otherwise indicates too direct a conceptual assimilation of type of analysis with disciplinary type). Failure to develop *multi-disciplinary* approaches has hindered the development, interchange, and flow of ideas and data behavioural science needs to resolve basic conflicts.

To clarify this argument from another direction: focus in the analysis and investigation of assumptions made about basic human organization and process (in psycho/behavioural terms) - the basic material - centres upon the psychological level of analysis rather than:

(a) the socio-cultural, as relationships drawn here are too macro; they shed light on man's basic state but their basic units are the wholes of lower levels. In other words they deal with the outcomes and products, and the very complex relationships that develop between, of the basic processes rather than the processes themselves *in themselves*. (Theory about the basic material develops out of such investigation, but the danger exists that jumps come to be made too readily from socio-cultural

descriptivism to the establishment of principles of man in basic psychological processes and organization - or lack of - terms). It should always be kept in mind the social phenomena and social facts dealt with are dependent upon basic processes even though not fully explainable by them; while culture obviously determines human behaviour and psyche to a tremendous degree, assumptions of near-absolute determinism do not logically follow - unwarranted reduction can occur in the direction of 'culture' just as it can in the direction of 'biology'.

(b) The biological level of analysis is correspondingly too micro; a scale change is required to lift us to an appropriate level of analysis, synthesis, and formulation. At the biological level relationships analysed and articulated are divorced from phenomena emergent from human social existence.

Lest this position be misunderstood it needs to be stressed that the psychological level of analysis is necessarily interdependent with neighbouring realms, and indeed this thesis sets out to establish the importance of sophisticated biological/psychological dialogue, this having been neglected in favour of psychological/sociological commerce. This tendency has been accentuated by the very divorce of sociology from biology. Conventional social analysis is sufficiently distant from biological analysis for social analysts to be lured into rejecting any possibility of biology making a contribution to the understanding of human behaviour. (What they usually have failed to realize is that, through employment and absorption of psychological assumptions and theories, they have inherited certain underlying biological assumptions). In turn, via sociological/psychological commerce, this logic has pervaded psychology to some degree. Consequently, insofar as a common general pattern exists within behavioural science, it lies in an implicit acceptance of the *tabula-rasa* doctrine over detailed consideration of nativism, with resultant emphasis upon environmental processes at the expense of the basic human material - the essence of humanity - and of the necessarily complex relations between. Of major significance is that this has occurred essentially *by default*; initial basic analysis having not been engaged in to any general or comprehensive degree. Note also that the trend perpetuates the very conditions of its genesis.

Upon focussing on the analysis of man at the level of psychological functioning, it becomes evident that certain major problems beset the establishment of a key model; a basic outline of man's psychological processes and organization. *There is deep and fundamental discord* over 'proper' subject matter and methodology; even over whether or not analysis in this realm can properly be considered a science, or if so, be adequate to the task in hand. Given the importance of such a model across disciplines (for the formulations involved link biological and social structure), the development of more unified and coherent behavioural science rests to some degree upon the identification of such problems, and upon the generation of solutions and remedies.

Such an undertaking is massive in implication; perhaps the best that can be hoped for is clarification of the basic problem(s). What does seem apparent is that behavioural scientists have for too long ignored their own humanity; the conducting of behavioural science is in itself a topic for itself. (The assumption that science as methodology will take care of itself is particularly inadequate when man studies man). The development of coherence and unity may largely rest upon such recognition.

The critique must ultimately be lifted to a set of purposes : the general problem in hand is to devise an attack on fragmentation and its negative consequences that does not violate essential diversity, or recognition that a variety of realms and levels of analysis are necessitated by phenomena emergent from lower relations. Drawing attention to unnecessary divisiveness and possible underlying causal factors is in itself a start; the active seeking of convergence and complementarities along with identification of *genuine* and significant differences can be promoted; so too can we actively encourage attempts to develop new logical constructions, viewpoints, methodologies, and formulations that both transcend and unite traditional realms of analysis, categories, and perspectives. The inevitability of specialization need not be denied by recognition that it occurs relative to a base; the issue centres on the nature of that base. Some way of developing overall shape, form and direction in behavioural science is required - some way of enabling a

a coherent overall picture to emerge, within which the products of diversity could form a gestalt.

The general corrective advocated (proposed as a possible part-remedy for current ills rather than a panacea) rests upon the argument that ultimately all perspectives are connected by basic assumptions about the fundamental nature of the material - man (i.e. these provide the common ground for discourse). Man's status as an evolved species begs to be explored : the intrusion of functional biology and evolutionary theory into the analysis of human behaviour creates the opportunity for development of an ultimate conceptual base; a relatively clearly defined common ground.

Accordingly, a basic premise is that the understanding of human behaviour should firmly rest upon *an understanding of behaviour*. Human behaviour *per se* as a starting point is held to magnify vulnerability to arbitrary (and often magical) assumptions, and extreme ideas and pathways. Specifically, sophisticated consideration of the implications of human phylogeny in behavioural terms should provide the following benefits:

(1) Provide a backdrop against which essential complementarities and differences may stand out, and an ultimate set of parameters within which the products of all realms of analysis should cohere. Radical inconsistency between basic assumptions made within a particular scheme and those emergent from evolutionary theory will obligate either rejection of evolutionary theory (which would put the behavioural sciences out of step with the natural sciences) or modification of that specific scheme; at the very least closer inspection of both would be necessitated. Increased sensitivity to the complexity of relations implicit in sophisticated evolutionary awareness may highlight the positive consequences of assuming greater flexibility in behavioural analysis : more interdisciplinary commerce, inter-realm of analysis dialogue, a basis for constant reconsideration of disciplinary boundaries (since 'discipline' simply means 'branch of learning'). The general proposal does not automatically necessitate the assumption that the study of biology equals the study of culture or anything equally as absurd; rather, acceptance of it may increase sensitivity to multi-directional causality flow (instead of disciplinary chauvinism in the form of reduction in one direction e.g. cultural reductionism, or biological reductionism), and

to the possibilities of developing new and constantly changing fields of study and focus.

(2) Provide the necessary basis for a general scheme which will serve to articulate the interaction of diverse paths of thought, research, and data; such a general system of articulation will enable transcendence of conventional typologies, associated encapsulated of logic and the inevitable polemics that follow. The possible unification of theory that may result (as opposed to creation of *the* theory) may shed light on otherwise anomalous findings of various separate paradigms (dead ends, recurrent issues, themes, irresolvable problems and disputes). These anomalies may come to be seen in meaningful relationship, and useful new approaches to research and applied work may be revealed.

(3) Provide some ultimate conceptual pattern or 'master-theory' for student/practitioner to revert to when confronted with diverse and conflicting data and conceptualizations. Extreme confusion can result from contact with the multitude of theories, viewpoints, and data generated by the complexity of human existence. Resultant dissonance and insecurity may lead to either extreme cynicism, with development of negative attitudes toward the very possibility of organized fruitful behavioural analysis, or, alternatively, the acceptance of one particular set of rigid ideas that answer all but explain nothing. An advantage of this general proposition resides in its keeping in central perspective man's very *existence* - an evolved living phenomenon with very real contacts with the rest of nature. This assists the keeping of feet on the ground.

(4) Provides a means of countering and keeping in check a prevalent environmental determinism tendency which (contrary to much social science folklore which degrades biological consideration as indicative of simplistic reductionism) tends to foster excessively simplistic views on behaviour dynamics and causation (since only one set of factors is considered to be significant) rather than the necessary awareness of real complexity.

Section Two elaborates upon this general proposition, and in the course of justification, attempts are made to demonstrate its value. Introduction of biological concepts and considerations into behaviour causation discourse brings with it the bogey of biological determination.

It is an interesting comment on the ultimate inter-relatedness of all fields of inquiry that most vehement opposition to the intrusion of ethology and functional biology has come from sociologists and anthropologists, even when the phenomena in issue centres on individual behaviour.<sup>1</sup> Section Two to that extent is an extension of Section One; although primary concern is with fragmentation at the psychological level of analysis and the possibility of partial remedy through increased sensitivity to biological contributions, opposition is likely to be armed with sociological and social anthropological concepts, data, and theory. In human issues of any importance or centrality, we are locked into multi-discipline discourse. It therefore behoves behavioural scientists (so long as they maintain any pretense of understanding human behaviour to any degree of generality) to develop familiarity with the basic products and materials of all realms of analysis.

The central arguments of two recent books written with sole intent to discredit the intrusion of biological concepts into the analysis of human behaviour are considered in the course of elaboration upon, and justification of, the evolutionary-theory base proposed. (The Biology of Human Action by V. Reynolds, and The Use and Abuse of Biology by M. Sahlins). This is regarded as especially important because, as argued, psychologists (traditionally caught in the middle as it were) have tended to be more influenced by socio-cultural viewpoints than ones readily identifiable as biological. This tendency has been accentuated and perpetuated by a built-in lack of zoological acumen; the ins-and-outs of social determination are much more readily perceived.

Ideas and principles of man have been drawn from socio-cultural analysis; much has been made of cultural variability, the shaping of the individual's psyche through inculcation into specific cultural modes, and so on. The possibility and investigation of fundamental bias (in accord with phylogenetic principles) in psychological processes, organization,

---

1. It is particularly interesting that in most recent times the most violent opposition to the intrusion of ethology into the study and explanation of human behaviour has come from anthropologists rather than psychologists. The anthropology/biology debate has been excessively general, sweeping and black/white; if one assumes any sort of disciplinary hierarchy in behavioural science, we have members of the most 'macro' or 'highest' level of analysis in conflict with some from the 'lowest'. Of major significance is that the debate boils down to dispute over the fundamental nature of man - his operating principles (or lack of!) herein lies the key to resolution.

and channelling has been underplayed by comparison. Of particular concern is the possibility of : selectivity of perception and response; differential ease of behavioural acquisition and, correspondingly, behavioural extinction; relative stability/lability in behaviours; common patterns in repertoire organization; underlying patterns behind individual differences; basis, organization and functions of emotionality, and so forth. Should such emerge (and even the loosest assumption of continuity between man and the rest of the animal kingdom makes that seem highly likely) then the overall picture of man would be complexified geometrically (since we could no longer assume simple environmental programming-of-man models; not only is more sophisticated consideration of two-way interrelationship obligated, but also recognition of complexity emergent from the interaction and inter-relationships of ontological developments with both sources, and so on and so on, is necessitated).

In this light, Section Three briefly examines the most clearly defined (and arguably most influential) paradigm of psychological process : Skinnerian Behaviourism. Analyzed as a paradigm (in Kuhn's sense, 1962), a testing ground for arguments developed in Sections One and Two is provided.

Many psychologists assert radical forms of environmental determination from within its parameters. However, the determinism put forward by radical behaviourists is importantly different to that proposed by many socio-cultural analysts. A clash of basic assumptions about operating principles is evident : put crudely, cultural determinists lay greater emphasis upon programming of the *human 'mind'* (note the use of such terms as symbolism, meaning, beliefs, internalization); the Skinnerian behaviourist, the programming of the *organism's behaviour*. (That this as it stands is an inadequate dichotomy is recognised, and the nature of this inadequacy is teased out during the dissertation. It is worthwhile in terms of the sort of issue it points to, however). Resolution of this dimorphism highlights the inadequacies of both forms.

The consequence of the dominance of behaviourism (for the bulk of this century, and particularly in the USA - but then, as Hebb 1964a argued, American psychology *is* psychology) has been the development of a very restricted view of man within the minds of many psychologists. Within

this view continuity (with the animal kingdom) has been overstressed at the expense of appreciation of 'higher' faculties and abilities. Reaction to this particular aspect of the restricted view has of course occurred. Considerations of a genuinely 'psychological' nature (i.e. concern with the 'psyche', predominantly subsumed under 'cognition') have increased dramatically since the 1950's, with some consequent growth in the understanding of human behaviour. Perhaps unfortunately, much of this reaction has centred around Chomsky's writings; consequently for many the behaviourist emphasis upon continuity has been diametrically replaced by assumptions of non-continuity. In this contingency, not only is all the hard-won ground gained by behaviourism threatened, but also real danger exists that the *tabula rasa* assumption (anti-nativism being rampant among radical behaviourists) will in conjunction be further entrenched, for while those who rally around Chomsky assert nativism in the mental functioning sphere, there is paradoxically little evidence that this formulation is extended to 'lower' processes. Indeed, given the proclivity of behavioural scientists to generate dichotomies and then staunchly defend one side at the expense of other considerations, it is highly likely that some form of a simplistic cognitive determination paradigm will be formed on the basis of near total rejection of behaviourism.

In polemically throwing out the vocabulary of 'instinct' psychology has thrown out with it the possibility of sophisticated consideration of biological impingement upon the behaviour and psyche of man. Therefore, in the interest of securely establishing the appropriateness of functional biology as an ultimate basis for the integration of behavioural science, special consideration is given to these two central issues (continuity/discontinuity; *tabula rasa*/nativism). The balance, especially upset by sweeping reaction originating in psycholinguistics, may hopefully be redressed without loss of the benefits of either behaviourism or of the Chomsky-inspired revolution. Concern is not so much with the 'cognitive' response to radical behaviourism (although considerations of similar nature to those advocated by cognitive theorists would follow a sophisticated biological approach, see for example Stenhouse 1974) as with the pragmatic and arbitrary approach to learning, and environmentalist assumptions associated, that the behaviourist paradigm represents.

The time has come to fully explore the implications of viewing learning and cognition as biologically based, directed and constrained - to adopt a broader frame of reference.

Consideration of behaviourism as a paradigm serves many purposes. Anomalies that have arisen relative to that paradigm are approached from the broader base advocated. In the process reconciliation with formulations hitherto perceived as opposing (e.g. personality theory) serves to demonstrate the value of openness, inter-disciplinary study, and the seeking of complementarities previously obscured by polemics.

This thesis argues that the time has come for behavioural scientists of all backgrounds to transcend fears of naive biological determinism and to reconsider right from basics the implications of man's status as an evolved living entity. The importance of recognizing the full significance of man's social existence (his capacity to symbolize, the role of 'meaning' in human action, self-awareness and human plasticity) has been well established; hopefully social scientists are now sufficiently secure in this to be able to reconsider implications of man's biological nature for his behaviour, and for the sort of social phenomena that emerge from his social existence. Too often such basic (but potentially integrating) considerations are completely rejected because of the assumed futility of nature/nurture debate; the fundamental question "what is man?" has been too simplistically assimilated to the long standing polemics associated. Some basic reconsideration of the properties of man (which rises over and above straight nature/nurture dialectics) holds the promise of clarifying the salient nature of issues (thereby providing a sound base for the establishment of branches of learning with complementary concerns, ranges of convenience, etc) and of providing ultimate checks against the wild flights of fantasy that have too often characterized behavioural science theory. Such a base may also assist us keep in mind that there is such a thing as *man*, and that the various theories and formulations put forward in response to the intellectual problems created by his existence are just sets of ideas and conceptualizations, and need to be constantly viewed with the intent of capitalizing upon convergences, areas of similarity, complementarities - in general, opportunities for progressive recombinations and reformulations - in order that we might move closer to solution of the ultimate puzzle.

SECTION ONETHE CURRENT FRAGMENTED STATE OF BEHAVIOURAL SCIENCE ENTERPRISE :  
SOME EFFECTS OF IT, AND DISCUSSION OF FACTORS POSSIBLY IMPLICATED  
IN ITS DEVELOPMENT AND PERPETUATION

In analysing interdisciplinary trends in research, Piaget contends "there is no hierarchy in the science of man" similar to that found partially in the natural sciences (Piaget, 1973a, p.10). A commitment underpinning this thesis is that this is false in the minds of many behavioural science practitioners, and for this claim Piaget himself provides evidence.

Rather, conventional wisdom (reflected in the practice of most universities) holds the study of behaviour as most appropriately structured in a tiered hierarchy, with each tier represented by a discipline or disciplinary group. Sometimes this structure is diagrammatically represented (e.g. to introductory classes) in the form of concentric rings, each level (moving out from centre) constituting a new perspective or scale. Each new perspective deals with phenomena emergent from lower ones, with treatment in lower level terms presented as possible only at the expense of considerable loss of understanding and explanatory power. Consequently each perspective, or rather its associated disciplinary matrix, is considered to have unique starting points, conceptual schemata, and types of problem to confront. Entry into a specific discipline therefore entails specialised education.

Moving from the centre outward (or base upward) the respective levels are represented by the biological sciences, psychology, sociology, and finally anthropology. While certain disciplines or 'subdisciplines' are sometimes considered to straddle scale boundaries (e.g. ethology overlapping biology/psychology; social psychology overlapping psychology and sociology) the logic of transcendence has the study of behaviour as rightfully fragmented in this basic manner. Within this scheme, interdisciplinary conflicts, or apparent hiatus, are explainable in terms of misunderstandings rather than in terms of fundamental shortcomings in the initial carving up of the subject matter and associated underlying assumptions.

Just how valid and useful is this multi-level approach to the totality of human behaviour? It seems to manifest general academic acceptance of holism over atomism. The subject matter of each level is

viewed as greater than, and very different from, the sum of its parts, these being the subject matter of a lower perspective. At each level of organisation, or of scale, phenomena open up which are new and basically unpredictable on the basis of the more detailed analysis of the entities which make up these higher level studies. At each new level there are fundamental problems requiring intensive research which cannot be solved by further microscopic analysis but need "some combination of inspiration, analysis, and synthesis" (Anderson, quoted by Thorpe, 1974, p.352). A basic motivating conviction behind this thesis is that the anti-reductionist case has been generally simplistically understood within the world of behavioural science, and has found too rigid and substantive an interpretation in the establishment of a clearly defined multi-discipline structure. Emergence has been thus reified at the expense of (a needed parallel) emphasis on essential unity, continuity, analysis and synthesis. An important consequence of this has been the development of rigid ideas about the validity of disciplinary autonomy.

For example, while Comte (acknowledged as the father of sociology) foresaw the necessity of a science that would combine both the biological and the social approach to man, this basic recognition isn't reflected in modern sociological practice. Rather, claims are frequently made that in the analysis of sociocultural phenomena biology may be ignored since culture evolves via its own set of rules (e.g. White, 1949; Sahlins, 1977). The argument in total contains many partial truths, and these cause us to lose sight of basic continuities, or even of 'basics'. As expressed by Freedman (1967, p.153):

"any societal organisation must, in fact, have come about through an interplay of genetic, ecological, sociocultural and psychological variables in what is best considered an acausal system of complete interrelatedness - acausal in the sense that no single aspect of the system takes exclusive primacy .... 'Primary cause' has at some time or other been erroneously inferred at all the levels of explanation mentioned above, depending on the (disciplinary) bias of the investigator."

Piaget is well aware of this sort of process. He writes:

"In some of the social sciences there is certainly a tendency to reduce or, more precisely, annex, for the 'reduction' desired is generally in the direction of the science represented by the authors. Sociologists have been known to reduce everything to sociology, for example" (Piaget, 1973a, p.10)

Piaget stresses that in the natural sciences specialists need a good grounding "in the disciplines preceding their own in the hierarchial order", and notes that in these fields "interdisciplinary research is becoming increasingly imperative" (p.10). He clearly holds the same as valuable for the behavioural sciences; but laments that we have witnessed "a series of partly sterile arguments between the two sciences best fitted to co-ordinate their findings - psychology and sociology", and, while a number of "false problems" have been dismissed, as yet only a "very small measure of collaboration" has obtained (p.12). Why is this so? The problem to overcome, in Piaget's view, is the fear "that any connexion going beyond the frontiers of our own discipline is likely to lead to exaggerated reductions and to a weakening of the specific character of the phenomena under study" (p.11). A major consequence is that (to extend Koch's epigram)<sup>1</sup> behavioural science is a congeries of weird insularities.

Why is this fear more prevalent in the behavioural sciences than the natural sciences? No doubt it is partly based on misconceptions about reductionism, and the role of reduction in the analysis of behaviour. The methodological individual/holism debate has tended to be prematurely and simplistically resolved in favour of holism. There has been a general failure to recognise and acknowledge the ultimate complexities involved. For example, failure to appreciate the respective differences and values of 'broad' and 'indepth' explanations, of 'how' and 'why' questions (see Beloff, 1973, pp.5-6, 13-14), and to differentiate between 'strict' and 'empirical' reduction (see Boden, pp.55-59) is evident in the literature where fundamental debate has arisen. One regrettable consequence has been the promotion of a certain degree of unnecessary fragmentation.

Reduction is seldom an uncontentious activity, as the history of science amply shows. However, behavioural scientists (especially 'social' scientists) may be especially guilty of not heeding the wisdom emergent from decades of philosophical discourse. Popper, for example, teaches that, fundamentally, science is reduction. In one sense, scientists have to be reductionists, have to attempt to identify the known with the unknown. Nothing has been as successful within science as reduction. Furthermore,

---

<sup>1</sup>. "Behavioural science" has been substituted the "psychology" of Koch's (1973) original.

hardly any major reduction has been completely successful - there is always an unresolved residue left by even the most successful attempt at reduction (truly emergent phenomena thereby identified). Finally, an immense amount can be learnt even from unsuccessful attempts at reduction.<sup>1</sup>

Basic misconceptions (viewed as fluid and individually variable constellations) constitute what may be termed behavioural science folk-notions of reductionism. The development of these folk-conceptions has been fostered in two principle ways, both of which reflect the difficulty of gaining a balanced view on the issues involved. On the one hand, strict 'anti-reductionists' in the study of human affairs don't seem to appreciate that "transcendence does not necessarily imply intrinsic irreducibility of human or biological phenomena". On the other, compulsive reductionists don't recognise that transcendence "does mean that new patterns of phenomena have emerged which it is not only possible, but indeed necessary, to study as new patterns" (Thorpe, 1974, p.360). Near total preoccupation with emergence result in practitioners of 'higher' perspectives insisting on analysing practically all in transcendental terms.

Consequently, concern for developing some overall scheme of unity, of coherence, is sacrificed in favour of maintenance of rigid conceptions of essential disciplinary autonomy. Conversely, failure to appreciate on the part of compulsive reductionists (usually members by training of more 'basic' disciplines) some degree of perspective, or level of analysis, autonomy (concomitant with with genuine emergence) has lead to misguided attempts at wholesale reduction. (In the first case, that of 'reduction up' in the sense demonstrated by Piagets sociology example, what occurs may be conceived of as inter-disciplinary piracy, or imperialism, in which members of 'higher' disciplines deny the explanation of 'lower' processes in any but their terms). Attempts at wholesale reduction have unfortunately all too often supplied committed anti-reductionists with plentiful ammunition, thereby furthering the development of anti-reductionist sentiment. (They do, after all, usually have the added card of appeal to human vanity up their sleeve, and are assisted by a general ignorance of the true complexity of basic, e.g. biological, processes). Such anti-reductionist folk-notions as may develop enable loyal adherents to cast

---

<sup>1</sup>. These 'lessons' of course carry no weight if one assumes the notion of science is fundamentally incompatible with, or inadequate for, the study and understanding of human behaviour. Such views are discussed later.

doubts on attempts to forge links across disciplinary boundaries, inferring these to be naive if not dangerous. These folk-conceptions of the automatic evils of reductionism are perceived as being in part responsible for an over-emphasis on *holism* at the expense of *analysis*, on *descriptivism* at the expense of reductive *explanation* within behavioural science endeavour.

To that extent, this thesis is in disagreement with Piaget when he argues that "as for the hierarchies which might be established between the human sciences, this of course remains an open question so long as the central problem of sociology, that of society considered as a whole and the relationships between the subsystems and the whole, is still not resolved" (1973a, p.12). For it seems (as will be made evident elsewhere) that in fact membership of a specific discipline very often carries with it 'resolution' of this problem. The exact nature of resolution varies between disciplines, thereby facilitating fragmentation. Furthermore, resolutions vary within disciplines (i.e. between competing schools), the nature of these differences transcending disciplinary boundaries, thereby complicating (and fragmenting) the overall picture further. Invariably, these resolutions (which need not be explicit, or even consciously worked out) *encapsulate fundamental assumptions about the nature of man; on the basic material, thereby giving lie to any scheme which assumes the ultimate validity and discreteness of disciplinary autonomy, for all are condemned to be locked in continual discourse at this fundamental level.*

It is sometimes declared that disputes over these basic methodological issues - atomism versus holism, psychologism versus collectivism - are as futile as disputes between engineers as to whether what is important in a building is its structure or the materials used. Clearly both are important in different ways. However, within the totality of the behavioural science project (if the current structure and practice is to be taken as evidence) greater emphasis has been placed upon the "in different ways" than the "both are important". To extend the example, imagine an architect designing a house in ignorance of the properties of building materials, or analysing the structure of an established building in a situation in which he is free to make whatever assumptions are convenient about the properties of the basic materials.

To give an example of the types of problems and confusion that can arise : Beloff, after discussing (in a manner personally found, for the most part, to be very enlightening) methodological psychologism and

collectivism (in an attempt to 'tie down' and clarify some recurrent problems in that murky meeting ground of psychological and sociological thought, social-psychology) eventually produced the following by way of an overview:

"Whether we choose to explain social phenomena in terms of human nature or human nature in terms of social phenomena is arbitrary and depends on our interests"  
(Beloff, 1975, p.215).

Now this is precisely where problems emerge. Beloff has unwittingly captured the essence of the problems created by the current multi-discipline state: students of 'higher' perspectives develop basic assumptions about the nature of man that are often radically different from, and mutually perceived as incompatible with, those developed by workers closer to the biological end of the spectrum. This is hardly conducive to the efficient interdisciplinary co-operation increasingly considered vital for progress.

#### Reactions to the Current Multi-Discipline Structure : The Literature of Discontent

Reactions to perceived short-comings of the current logic and structure of the behavioural science framework fall into three major categories. The first two represent variations on the theme that the current structure is not actually wrong, but does give rise to certain problems, some of which could be overcome without radically changing the basic framework. As such, criticisms therein are not intended by their authors to detract from recognition that, for many problems, importations of extraneous concepts may actually impede understanding of problems that fall squarely within the range of a specific disciplinary perspective. They do, however, imply that such matters should not be rigidly pre-judged, and serve to indicate the problems and decisions involved require constant analysis and 'balance'. The third category encompasses more radical questioning of the status quo.

The essence of concern of each category is indicated respectively by the following questions:

1. Are major problems - those confronting the development of further understanding - most effectively dealt with by parceling them off into separate discipline containers? What are the negative outcomes of this approach?
2. Is within-one-discipline training sufficiently broadly based to enable competent confrontation with even those problems consensually allocated

to that discipline? Are the properties of phenomena identified with a specific disciplinary nature sufficiently emergent to be tackled in essential ignorance of the concepts and knowledge of lower perspectives?

3. Do most problems/issues in fact transcend disciplinary boundaries? If so, are attempts to differentiate between levels, perspectives and scale changes in the form of distinct disciplinary boundaries dysfunctional to the growth of understanding and knowledge?

1. There is widespread feeling that many important issues, significant behavioural problems, do not neatly fall into disciplinary categories. Major consequences include clashes of interest (resulting from defensiveness and mutual misunderstandings), and disciplinary imperialism, where discipline members feel a problem topic to be specifically "our area". This obviously diminishes the benefits of interdisciplinary co-operation.

Thorpe (1974, p.353) observes "clearly fundamental questions seem to cluster around just those areas where the scale changes. Thus in biology exciting things seem to occur at the interfaces with chemistry and with psychology". Inclination to exploit these areas would be strengthened by awareness that the history of science shows "most great steps have been taken not by systematically checking every factual box to be sure that each contains what it is supposed to contain, but rather by conceiving new kinds of boxes", and the call for the "removal of arbitrary boundaries between conceptual areas" which will produce "more of what Kirtley Mather called outrageous hypotheses" (Murphy, 1968, p.38). However, reification of hierarchical assumptions into a tight and rigidly maintained disciplinary structure may prevent this occurring, and may in part be responsible for the (arguably) slow progress of the behavioural sciences. <sup>1</sup>.

Recognition that many problems "lie neglected between the established academic frontiers" (Halsey, 1967) led to the development of the Human Sciences degree at Oxford. This multi-discipline degree was developed in the hope of providing a better foundation for research. For the most part, however, interdisciplinary interaction is haphazard, even though many advocate its importance. Eysenck (1967) argues that the divisions between

---

<sup>1</sup>. Hudson (1967) and Andreski (1974) among others argue that we are no closer to gaining significant insight into the truly seminal issues in behaviour causation than we were fifty years ago.

academic subjects are of administrative convenience, and may obscure real and important links, and Reynolds, from another background, declares that he "does not believe in epistemological boundaries in the quest for understanding" (Reynolds, 1976, p.viii). Fletcher (1968) argues the need for a "thorough going and painstaking" effort to establish interaction between the related fields of comparative biology, comparative psychology, and comparative sociology, pointing out that "no one position is ever going to be definitive in these fields".

Words and wishes come cheaply. The stark reality is that it is harder to practice than preach, as was evident in the on-goings of a major multi-discipline symposium on personality, held at Rice University, and recorded in a volume edited by Norbeck, Price-Williams, and McCord (1968). Numerous authors of diverse backgrounds stressed the value of interdisciplinary co-operation, yet most were prone to see fault and error in the research and theories of contributors from other disciplines. The analysis of these criticisms, in the eyes of Norbeck *et al*, indicated a fundamental lack of awareness of the differing starting points and viewpoints used by other disciplines in their attack on problems, rather than insight into crucial shortcomings.<sup>1</sup> This in essence is the problem confronting those who simply assume that calling for interdisciplinary co-operation will solve any problems emanating from the current fragmented structure. In Mead's words, "in multi-disciplinary research if you take a group of people who represent different disciplines .... they can argue from now until Kingdom come and never get any work done at all" (Mead, 1968, p.378)<sup>2</sup>.

---

1. In his inimitable style, Andreski (1974, p.121) makes the point: "academics surpass trade unionists in their closed shop mentality and proclivity for demarcation disputes (which goes so far that knowing something about A is commonly taken as sufficient evidence that one knows nothing about B), while the contacts between subjects often amounts to inter-disciplinary cross-sterilisation through symposia by mutually uncomprehending specialists, which resemble choirs of the deaf with each singer emitting piercing sounds in the face of a total obliviousness from the rest".

2. To give a personal example : when pressed by an enthusiastic sociology masters student the author confessed to an interest in inter-personal perception. The conversation soon broke down due (in my view) to the sociology student's insistence that the phenomena in question could only be profitably approached by sociocultural analysis, i.e. by explaining a given individual's judgements (of other people's behaviour, feelings, intentions, intelligence or whatever in a specific interaction situation) in terms of class or subcultural phenomenology - beliefs, expectations and so on. The validity of my contention that, while I recognised (and therefore was interested in) the significance of socio/cultural membership as an important source of influence on the types and nature of judgements made (principally, perhaps, via the individual's 'implicit personality theory') I was also interested in a wide range of other factors possibly implicated as sources of variation in, e.g. accuracy of judgements made. (Factors such as intelligence, personal social sensitivity, situational factors). To my

mind, it appeared a complex problem, with the phenomena in question likely to prove very individual and situation specific. Simply assuming that situations made decisions for people along rigid lines of cultural/social prescription appeared a *reductio ad absurdum*. Perhaps the other student was an especially poor ambassador for sociology (although he was apparently well regarded within his discipline).

---

These basic problems are not going to be solved easily. On the one hand, we must appreciate the value of diversity. There is merit in the argument that science is above all else a collective exercise to which methodologically and ideologically independent approaches to one and the same phenomena are crucial. Problems may transcend disciplinary boundaries, but this does not automatically necessitate the dismantling of the current structure. A variety of attacks, starting from different points, employing diverse concepts, and perhaps concentrating on slightly different facets of a given behavioural problem may be the best way to reveal the subtleties of any given phenomenon. However, for benefits to outweigh costs, all involved must tolerate this situation. That is, they must develop flexible attitudes, and be prepared to consider the potential of other perspectives to shed light on problems in hand. There are immediate problems : in essential ignorance of the methods, concepts and content of other disciplines, how is one to judge at what point the balance of appropriate range or focus shifts? At the moment, the cost of diversity appears to take the form of poor interdisciplinary communication, inefficient co-operation, neglect of problems that are either diffuse or not readily consigned to particular disciplines, and polemics where perceived competition occurs. Diversity of study often means diversity of output, and we still have to fit the pieces together again.<sup>1</sup>

A more subtle semantic analysis may generate hints of possible resolution. Firstly, it must be stressed that a discipline is simply a branch of learning. Rigid conceptions of disciplinary autonomy, predicated upon simplistic notions of emergence at the expense of sensitivity to essential unity, may be the major factor responsible for the damage wrought by the present structure.

---

<sup>1</sup>. A specific example may be useful. European ethologists, American comparative psychologists, and physical anthropologists have all studied non-human primates, and have discussed (often with conflicting conclusions) the implications for understanding man. The problem, as it were, is clearly not the property of one discipline. In the realm of behavioural science, probably most problems are the domain of several disciplines. The challenge is to maximise benefits of diversity while minimising costs (e.g. pointless time wasting polemics).

Secondly, we must distinguish between explanations of certain types (e.g. a psychological explanation), and disciplines (e.g. psychology). There is not a one-to-one relationship.

Thirdly, for most behavioural problems, questions of different types can be asked. These questions may be identifiable with particular levels of analysis - there will be 'biological' questions, 'psychological' questions, 'sociological' questions and so on. The answer to a question of a particular type constitutes an explanation of that type - e.g. a 'psychological' explanation. It is important that we recognise (a), all explanations should ultimately cohere (the logic of this being provided by answers to more general, 'meta' questions), and (b), types of explanation are not exclusive property, and are not exclusively employed by members of a given discipline.<sup>1</sup>

Fourthly, treatment of problems of even the most basic breadth (even those consensually designated appropriate to a specific discipline) will incorporate components - assumptions, methods, concepts, explanations - originating from other disciplines. Sociologists utilise psychological notions and research findings in their large schemes; psychologists make use of biological and sociological knowledge and concepts. Problems emerge because use of extraneous materials is often implicit (often not even consciously realised), this being accentuated by the obscuring of the very universality and 'fluidness' of behavioural science problems by the administratively rigid nature of the multi-discipline set up.

Fifthly, while we may well wish to establish new levels of analysis appropriate to the study of emergent phenomena *as these become evident*, it does not follow that the disciplines established should, or even could, function purely and simply within that realm in splendid isolation. As behavioural science disciplines necessarily incorporate concepts, theories, and knowledge of other disciplines, efficient interdisciplinary commerce is a necessary rather than simply desirable goal, for the specific content of any given discipline is under constant review and is subject to modification.

Sixthly, the identity of the individual practitioners, (e.g. psychologist) or of particular clusters of concern (e.g. sociological) is not unduly threatened by these realisations. The basis of identity is

---

<sup>1</sup>. Put another way : The asking of a question that may be identifiable as a particular type does not preclude the possibility of asking other types of question of the same phenomena. Conversely being able to give answers identifiable as specific types (e.g. a psychological as opposed to sociological explanation) does not identify the phenomenon as 'belonging' to a given realm of discourse. 'Biology', 'psychology', 'sociology' exist only in the minds of men; our classifications reflect our own behavioural histories; those of men far in the future may bear little resemblance to current ones.

simply shifted from some absolute classification of problems, or from membership of a distinct, clearly defined and delimited perspective within a fixed structure, to a more straightforward and less troublesome acknowledgement of particular interests or preoccupations. Such a basis promotes recognition of the inter-dependence of special interests with the rest of behavioural science, and of one's first and foremost status as a behavioural scientist, albeit one who chooses to concern himself with particular aspects of certain phenomena.

This little diversion into idealism shouldn't be allowed to detract from the realities of the current state. The imperialism and insistence upon isolationism often evident has led one theorist to suggest the workings of an academic version of the territorial imperative! (Bressler, 1967).<sup>1</sup> Certainly, interdisciplinary polemics run high, a further indication that no clear-cut criteria for identifying disciplinary sovereignty exists, and in fact may never exist.<sup>2</sup> (That it may indicate something more fundamental, given that inter-school polemics occur within the bounds of a given discipline, will be discussed later).

All this raises issues of an ethical nature. Julian Huxley argued that scientists must take responsibility for the effects on society of their discoveries. If this is accepted to any degree, then we must also consider a converse : namely, that scientists are morally culpable for non-progress if (a), progress to a greater degree was reasonably possible and (b), that progress would have benefited humanity. We have considered arguments relevant to (a) namely that progress has been held up through a lack of positive interdisciplinary co-operation, and because many important problems have been left neglected as they lie between

1. This little quip is doubly interesting, as it is indicative of the pervasiveness of the sort of issue fundamental to this thesis. Here is a 'biological' notion proposed to explain 'sociological' phenomena - but not by someone identifiable as a naive zoologist or animal behaviourist - Bressler, at time of writing, taught within the Department of Sociology, Princeton University. And no, the statement was not made with tongue in cheek, although Bressler is well aware that at best it could count as a part-truth, and is certainly inadequate as it stands.

2. On-going controversy over Skinner's work provides an excellent example. Skinner's critics come from all walks, and certainly don't hold back if they themselves aren't psychologists (in the manner, say, of one thinking twice about arguing with a physicist over some matter within his field of research). In fact, they often attack precisely because he is a psychologist, and one of a particular type, and they feel he is 'explaining' phenomena that is properly "their field". Skinner (in fine academic polemicist style) suggests that his critics often exhibit "neurotic if not psychotic" responses and show signs of "emotional instability" - all explainable in terms of his basic theoretical principles, of course (Skinner, 1973, p.162-163).

boundaries, or blatantly across them in a 'messy' way. As for (b), there are recurrent assertions in the behavioural science literature that the greatest threat to human survival is a lack of self-understanding (e.g. White, 1966; Mead, 1968; Murphy, 1968; Tinbergen, 1976). Those who hold disciplinary boundaries as sacrosanct may have a lot to answer for.

Some candidate areas for interdisciplinary co-operation and synthesis

Some brief consideration of areas which are often held up as prime candidates for greater interdisciplinary co-operation will serve to round-off this first category of discontent.

First of all, there is the general field/conceptual matrix of personality which still plays a significant role in both scientific and folk conceptions of behaviour causation (despite the best attempts of some sociologically inclined theorists and behavioural psychologists). The notions of personality has been intimately associated with the long standing internal/external locus of control debate, which has frequently resulted in exposure of seemingly irreconcilable differences that run along disciplinary boundary lines.<sup>1</sup> Arndt asks for more sense and fewer polemics in this arena:

---

<sup>1</sup>. To indulge in another personal example : Several students tutored in a general human development course were especially confused by literature suggesting endogenous bases of more-or-less stable individual differences (i.e. inherited constitutional influences on personality development, or, if you prefer, reasonably constant basic tendencies to develop a behavioural repertoire consequent to environmental interaction that is different in significant ways from that of others exposed to similar contingencies). As it turned out, these students were all attending a particular sociology course, within which they had been informed by Dr so-and-so that "there are no such things as inborn influences on personality development" - or at least, this was their understanding of the general message. This made clarification of some of the semantic and logical issues even more pressing (quite apart from simple review of 'evidence') as the students clearly felt that the norm was to go one way or the other, to 'buy' one position at the expense of its perceived opposite, this magically translating somehow into 'knowledge'. (Being committed, knowing where you stand in such misty fields often seems to take precedence over alternatives such as tentative acceptance of partial solutions and associated openness to further argument and evidence). While this and my earlier example may simply reflect poor teaching, 'dumb' students, or both, I suspect such basic conflicts and confusions to be more the norm than exception. Certainly it seems prudent to carefully consider how such blatant misunderstandings can occur at all, even if they do prove to be uncommon. On this specific issue, problems seem to arise from practitioners moving from a position of not being interested in individual differences (owing to the adoption of a specific level of analysis) to the assumption that the phenomena ruled out on methodological grounds *does not exist*. (See Mead, 1968, for further discussion of this specific problem).

"The individualistic fallacy views personality as a structure isolated and independent of the cultural context. At the other pole we find the culturalistic fallacy, which holds that there is no personality structure within man other than a mirror image of the culture in which he exists. Allport resolved the opposition between these two positions by treating them as two different realms of discourse; that is man's behaviour can be understood in terms of motives and traits and it can be explained by the cultural concepts of roles and norms. Neither realm contains the whole truth and neither is false as long as it does not deny that there is another way of looking at man. It is the pre-emptive and dogmatic 'nothing but' position that is nothing but wrong." (Arndt, 1974, pp 229-230).

The problem of increasing recognition of the validity of diverse starting points, and of differential focus (intolerance in itself being reflective of an insensitivity to the complexity and multi-faceted nature of behavioural phenomena) is not readily resolved. Fitting the productions of differing perspectives together in order to create some gestalt, some overall picture, has not been mastered. Fundamentally, this is because we have difficulty deciphering the form of each piece, which makes the task similar to doing a jigsaw puzzle in which the individual pieces constantly change form along all three dimensions. In Mead's words, "we have not as yet a device which - whenever anybody is working on one little piece of a picture - defines those aspects of the situation that he is not dealing with" (Mead, 1968, p.377). One possible grass-roots measure would be to revamp tertiary education, so that instead of single majors, double or multiple become the norm. This could be taken through to post-graduate level. Arguments that highly specific research along conventional disciplinary lines (of Kuhn's 'normal science' nature) would suffer overlook the possibility that major leaps forward may result from the flexibility (hopefully) brought about by such training. Loss of specific preparation and knowledge within a field is not inevitable either - (apart from the interrelatedness of much interdisciplinary material) - basic degree structures could be both lengthened and intensified. If this is likely to lead to increased competence in practitioners (researchers or applied), then why not? As behavioural science graduates increasingly find vocations that require their particular training (e.g. clinical psychology, social workers), societal constraints and real-life accountability may determine that training is both extended and intensified.<sup>1</sup>

---

<sup>1</sup>. N.B. The author is not so naive that he doesn't appreciate particular social forces may strive to keep members of newly emerging professions incompetent, e.g. by developing selection pressures opposite to those that would operate in the normal positive (accountability) direction. In most societies, some relationship develops between competence and power. Power is interwoven with politics, and politics encompasses much more than social welfare, or concern with the social good.

After-all, who questions the length of a basic medical degree?

Several theorists from the realm of personality research would probably concur; Gilbert considers that "the study of personality is necessarily interdisciplinary, cross cultural, and historical in its dimensions", and adds we should do no less than "analyse human personality as an integrated bio-social complex of functions" (1970, p.25). He exhibits awareness of the dangers of overworking a specific focus so that one eventually becomes locked in, indicated by his statement that "it must always be borne in mind that discussions of part processes at one or other level of analysis is purely a convenience for scientific determination of relevant relationships within a specific context" (p.25). Eysenck sounds an almost identical warning : "the distinctions between psychology, physiology, genetics and other biological specialities are man-made ... (and) ... have no counterpart in nature. It may be convenient to cut the clothe this way but to make the suit it has to be sewn together again" (quoted by Cohen, 1977, p.105). Another major personality theorist, Robert White, although counselling that "the scientist is inevitably disposed to deal selectively with human nature", also stresses the need to bring diverse contributions to bear all at once on the study of the individual, a task he clearly feels has been neglected (White, 1966). He contends that better understanding cannot be expected unless "we cover the whole biosocial range, extending our observations from organic foundations such as drive and temperament to social shaping forces such as class, status, and cultural pattern" (White, 1966, p.2). White, too, draws attention to the distortion-through-partial-knowledge problem (see Arndt quote above) when he laments that "only dynamic psychology has regularly kept the individual at the centre of interest and tried to bridge the chasm between biological and social ways of thinking" (White, 1966, p.22). (This is a central issue returned to later). Perhaps these calls and warnings are being heeded, as a steady growth of interdisciplinary research in the personality field, in particular between neurologists, geneticists, multi-variate statisticians, social anthropologists, and psychologists, has been noted (Vernon, 1972).

Within the general analysis of the individual's behaviour field (i.e. 'psychology') Eysenck (1967, 1972) has urged recognition that most problems in psychology transcend the discipline. More general acceptance of this,

Cohen (1977, p.106) insists, would mean that "in the far future, accounts of human behaviour will be framed at all levels" (he lists several disciplines). More detailed arguments about the validity, or even possibility, of an autonomous psychology are discussed later. For the time being, it should be noted that the issue involved encompasses longstanding dilemmas. A recent assessment of Freud's works in the light of these dilemmas resulted in the conclusion that a major component of Freud's legacy is "a demonstration of how to be interdisciplinary" (Jahoda, 1977, p.158). From this basis, Jahoda delves into both benefits and problems of interdisciplinary work from the standpoint of traditional psychology. Noting that many of the problems psychologists are called upon to solve are also tackled by non-psychological disciplines, she remarks (in similar fashion to Mead) "the logic of interdisciplinary research has not yet been spelt out". Consequently, sometimes a variety of studies are incorporated under one cover in the hope that the reader will integrate what research, because of separate languages and a variety of preferred forms of data collection, did not. Other efforts "that do not rely on such pious hopes" usually pay the price of "the abandonment of hard-won theoretical formulations in each discipline" through combination of the various languages (social psychology is discussed in this light). In short, while Jahoda is uncertain whether truly interdisciplinary theories are possible, she does hold as both possible and desirable enlargement of "the testing ground for psychology", which can concurrently "illuminate the tacit assumptions about psychological processes which other disciplines dealing with man in society have to make" (Jahoda, 1977, p.159-160). She claims that this process of broadening psychology by consideration of material from other disciplines, mastered by Freud, is possible without loss of identity as a psychologist.<sup>1.</sup>

Note that calls for interdisciplinary co-operation often come from investigators of what might be termed central behavioural science concerns. As early as 1957 Lindsley bucked against mainstream behaviourism by arguing the inadequacy of reliance on behavioural assessment techniques in the study of motivation. He insisted worthwhile understanding would arise from nothing less than composite consideration of contributions from a range of biological and social disciplines (Lindsley, 1964). Investigating emotional

---

<sup>1.</sup> These points made by Jahoda both reiterate some already discussed and anticipate important later ones.

behaviour, in particular anxiety, Levitt (1971, p.221) remonstrates "the investigation ... has been perennially handicapped by a traditional isolation of disciplines ... progress in research requires a union of investigators from biological and behavioural sciences".

Child development would seem an obvious candidate for interdisciplinary co-operation. However, while the importance of a multi-level approach (requiring the amalgamation of data from biochemical through to sociological levels to explain "even simple aspects") has been adequately established in the literature, this is far from manifest in general research, theory and teaching (see Blurton-Jones, 1976, for discussion).

Preoccupation with abnormal behaviour should make apparent that "man is of a piece" (London and Rosenhan, 1968). This is evident in recurrent requests for co-operation between social and biological sciences in this field (e.g. London and Rosenhan, 1968; McCord, 1968; Eysenck, 1967). But perhaps the domain within which most dramatic demonstration that significant problems lie between and above orthodox perceptions of disciplinary boundaries is language. Primate studies in this area have been distinctly multi-disciplinarian, and have given rise to new understandings and conceptions of language (see Premack, 1971; Linden, 1976; Rumbaugh, 1977). Traditionally, communication has been studied by a "bewildering variety of perspectives" few of which "share fundamental agreement on what language is" (Linden, 1976). Chomsky, of course, clearly believes the time has come to transcend traditional boundaries (he writes "in this area of convergence of linguistics, psychology, and philosophy, we can look forward to much exciting work in coming years" (1971, p.135). His work has done much to bring this about. The polemics associated serve to remind that issues/problems/phenomena usually do not neatly fall under any one disciplinary rubric. This obliges consideration of the effects of specialisation - of inculcation into the viewpoints and practice of a specific discipline.

2. Specialisation at and within a specific disciplinary perspective may reflect an interpretation of the role of transcendence in the phenomena of behaviour that is excessively substantive and naive. Consequently practitioners may be inadequately equipped to satisfactorily confront even those problems consensually designated to their discipline.

Even if we were to accept the basic assumptions underlying the present behavioural science structure, it does not automatically follow that a high degree of disciplinary specialization is thereby validated. For, so long as we perceive a need for interdisciplinary co-operation, and recognise that all the products of diverse disciplinary activity should ultimately fit together again, basic problems are generated by specialization. For example, who is to fit all viewpoints and contributions together again in response to a problem of any significant generality? In essential ignorance of the contents, concepts and methods of other disciplines, how is the individual able to best judge the range of his perspective, to judge where it is most fruitfully employed, respective to others? This problem is compounded by a common (perhaps 'human') tendency to overwork, over-extend, one's understanding - to attempt to explain too much with too little. Williams (1969, p.632) refers to this desire to generalise as 'universalitis', and argues it has crippled much behavioural science enterprise.<sup>1</sup> As Heine (1974, p.vii) argues, "it is characteristic of members of a scientific discipline to pursue their interests in splendid isolation from any other body of knowledge. Universities have fostered this conceit by departmentalising the faculty and, thereby, adding additional barriers to such weak ecumenical impulses as might exist".

The interdisciplinary co-operation considered so vital (by those quoted in category one) is hardly likely to be fostered in an atmosphere of interdisciplinary chauvinism, conflict, rivalry, and misunderstanding. Reification of emergence in a rigidly maintained disciplinary hierarchy reflects a general lack of appreciation of what Thorpe calls "the fascinating things that can be discovered by looking at both the whole and the part, but most of all by looking at their relationship to one another" (Thorpe, 1974, p.359). Greater awareness of this (requiring, as it does, liberation from the shackles of specialist socialization and the unnecessary constraints imposed by folk-notions of reductionism) would promote the forging of links across established boundaries of knowledge.

---

<sup>1</sup>. Piaget (1973a, p.67) comments that social-psychology displays "the kind of imperialism that is the mark of a science in its youth". It should be kept in mind that all behavioural science is in its youth.

Misplaced and over-riding fears of the evils of reductionism (reaching its zenith in the spectre of biological determination) often seems to preoccupy social theorists (and those heavily influenced by social theory, e.g. many psychologists) in a most negative, counter-productive way. (Some complexities are analysed in Section Two). These fears seem to erode recognition that at any level, topics can be illuminated even if not fully resolved by reductionist techniques. It would be blatantly idiotic to prepare chemists for research without considerable training in physics, or for a physiologist to function in ignorance of chemistry. The fact that chemistry essentially deals with phenomena emergent from physics, and physiology with phenomena emergent from physics and chemistry, does not lessen the need for considerable understanding of these 'lower' fields.<sup>1</sup> But the current structure of the behavioural sciences and its associated assumptions doesn't just inhibit interaction on the basis of fear of the consequences of 'downward' reduction; assumed sovereignty of disciplinary boundaries (based on implicit acceptance of the over-riding significance of emergence) often leads to reluctance on the part of 'lower' perspective practitioners to involve themselves in the analysis of phenomena more generally associated with 'higher' perspectives. For example, excessive caution may have held back many good biologists and zoologists from getting involved in the study of human behaviour (Blurton-Jones, 1976).

Recognition that those 'higher' up the scale necessarily incorporate (not always explicitly) material from 'lower' levels may facilitate interdisciplinary discourse, critical comparisons, and fruitful debate. Lack of this type of discourse generates the danger that practitioners will, in essential ignorance, adopt lower level concepts or research

---

1. Loyal disciples of the present structure and practice will not gain relief by arguing (along with Piaget) that whereas a strict hierarchy (based upon clear-cut emergence) exists in the physical sciences, it does not in the behavioural sciences. If this is so, then behavioural science disciplines should be better able to engage in commerce, and should be able to readily combine their respective products to create a global, all encompassing picture. For in such a structure, disciplinary boundaries are presumably based on an essential plurality of focus, with each discipline dealing with different facets of phenomena attacked universally by behavioural sciences. Greater, not less, incentive to counter the negative effects of isolationism is incumbent on this type of analysis; this type of argument constitutes the essence of category three of discontent.

findings on the basis of whatever is convenient for their larger scheme ends. To take a concrete example : the anthropologist Anthony Wallace contends that anthropologists have been avid consumers of personality theories originating in other disciplines for years (Wallace, 1968). This being the case, then perhaps they should have more extensive grounding in the field, i.e. study within those disciplines where the theories originate. A review of sociocultural literature reveals that sociologists and anthropologists tend to draw on Freud when they require a psychological analysis; most academic psychologists would consider this woefully inadequate if not completely misleading. (That this may also say something negative about academic psychology is conceded; however, the main point is that the incorporation of psychological components occurs a long way from where most of the appropriate action - discourse, research, analysis - is). Note also in this regard that projective tests - in particular the Rorschach - were used extensively by social anthropologists well after academic psychology had developed sophisticated awareness of its inherent short-comings. Anthropological theory construction on the basis of such results - i.e. interpretations of test performances - must therefore be rather suspect. Conversely, much psychological personality and behaviour theory must make social anthropologists shudder on the basis on inadequate, or misleading, sociocultural conceptualisations and assumptions therein. Borrowing of concepts and findings from other disciplines may often occur on an *ad hoc* or *post hoc* basis, i.e. material may be selected according to its suitability to bolster a largely pre-formulated construction. Thus, the failure of problems to fall into neat disciplinary containers may create problems that are accentuated by disciplinary specialisation in two basic ways : (a) conflict generated by 'identity indignity', a 'hands-off' phenomenon aided and abetted by ample evidence of incompetent cross-discipline borrowings, and exaggerated by ruling assumptions of disciplinary autonomy, and (b), reluctance on the part of some perfectly competent but modest practitioners to involve themselves in analyses that traditionally fall outside the bounds of their discipline, but which never-the-less involve basic assumptions or concepts originating in their realm, whose abuse could be prevented by their intervention. This second unfortunate consequence is no doubt also in part dependent upon assumptions of essential disciplinary autonomy, i.e. a "I know my place" sort of syndrome.

Isolationist tendencies, and such shortcomings contingent upon narrow specialisation as may exist, are probably accentuated by the terminology of causation. Particular influences on behaviour are discovered within a discipline's conventional sphere of operations, and there after become 'determinants' of that behaviour. Now it should be noted that the 'determinants' with which most behavioural scientists are concerned, especially those tackling human social phenomena, determine only in a loose statistical sense. One possible effect of the use of this terminology is closure to the possibility of other factors being implicated in the morphology of the same behavioural phenomenon from other perspectives. George Kelly (1955) understood this well, and always took care to delineate his focus.<sup>1</sup> On such a basis interdisciplinary stock-taking offers obvious opportunity for cross fertilisation.

Overall there is, however, little evidence of acknowledgement of Julian Huxley's warning that intensive specialisation reduces any branch of science to a condition approaching meaninglessness in current behavioural science structure and practice. (Unfortunately, as he also remarked, specialised meaninglessness is lauded as a sign of true science by some - perhaps the status hungry). Rather, we may be approaching a situation in which specialisation creates vested interests, trained incapacity, and intellectual narrowness (Halsey, 1967; Bressler, 1967). The case for a substantial, initially broad training base may have to be continually argued. Certainly there is 'real life' evidence of its value. Bruner, for example, delved widely in the early days of his academic life (see his interview with Hall, 1970), and this trend is remarkably consistent across a wide-range of eminent behavioural scientists (see Cohen's interviews, 1977). Further comment on the rigidity of the established structure arise from Cohen's interviews. Chomsky recalls that his postgraduate work didn't fall neatly into a disciplinary category and so he had no job offers after completing graduate school at Harvard; Neal Miller believes that as research funds tighten up, interdisciplinary work is the first to fall by the wayside. Furthermore, if it came to a crunch, each department would invest in a 'pure' researcher/teacher of their type rather than one with expertise in several fields. (Miller notes that John Dollard "Had a good deal of

---

<sup>1</sup>. However, this didn't stop him firing off facetious put-downs and broadsides at those who chose to adopt alternative perspectives and starting points. Still, at least they knew exactly what they had to respond to; what the debate was really about.

trouble that way between psychology, anthropology and sociology ... let the other take him, each department said". (Cohen, 1977, p.260).

Just as the ultimate everything-is-related-to-everything-else nature of behaviour denies ready parcelling off of subject matter into neat disciplinary boxes, so too, it places subtle constraints upon the efficacy of specialists. Andreski, focussing on human social phenomena, eloquently elucidates:

"Owing to interwoven quality of the various strands of social life, no narrow specialist can offer advice on policy which merits attention. Thus somebody who spends all his time on studying race relations may not be the best person to make forecasts about them, because future situations will, in all likelihood, be equally influenced by factors outside his field of interests, such as transformations of family patterns, political re-alignments, or the position of the trade unions. True, some degree of specialization is inevitable, but the impossibility of finding truly isolated social or cultural systems exposes the investigator to the danger that, in consequence of specializing too narrowly, he may be unable to understand what he is specializing in. By concentrating exclusively on one time and place an anthropologist may not be able to distinguish between what is peculiar to it and what is universal or at least widely shared. The massive trivialisation of sociology and politology goes hand in hand with the increasingly common ignorance of history and ethnography among the practitioners. Moreover, owing to the slippery nature of the concepts he cannot avoid using, a social scientist ought to possess a high level of skill in logic and philosophy, as well as some grounding in the natural sciences." (Andreski, 1974, p.212)

Recognition of the shortcomings of specialization creates a tall order for behavioural science training - perhaps rightfully so. Some degree of specialization *is* inevitable - given the knowledge explosion (perhaps this would be better put as 'literature explosion') - and is to some degree desirable (given the positive nature of much 'normal science' activity). It really becomes a question of the nature of specialization, and of the base that underlies it. This leads into the concerns discussed in category three; an overview of the drawbacks of specialization will highlight the relationships between all three categories of discontent. The major dangers inherent in disciplinary specialization as it exist are:

- (1) Specialization may restrict awareness of genuine contributions that could possibly be made to the individual's understanding of the behavioural phenomena of special interest by practitioners and materials (data, theory) operating outside of the discipline. This may be accentuated by defensive responses to perceived encroachment, the true

multi-disciplinary nature of a given problem not being recognised.

(2) When the need for incorporation of extraneous materials is recognised, selection problems may occur. The danger is that convenience, rather than a sophisticated examination of validity, will determine choice; overt incorporation of these components being deemed necessary, the larger scheme will be considerably strengthened or undermined according to the ultimate validity of the materials used. An adequate basis for discrimination is therefore of considerable importance.

(3) Specialization tends to limit awareness of inadequacies of the current structure and practice of behavioural science, for inculcation into the ways of thinking of a particular discipline carries with it a particular set of answers to major questions that deny the meaningfulness of the questions; that tend to explain them away rather than confront them.<sup>1</sup> (This helps explain problems in interdisciplinary communication, and interaction - the 'sets of answers' sometimes don't match very well).

(4) Ruling assumptions of the essential validity of current structure and practice (implicit in general acceptance) tends to result in the penalising of multi-discipline practitioners. This of course becomes a sociological issue in its own right.<sup>2</sup>

1. The sociologist Dennis Wrong (1961) expresses a facet of this problem clearly: "if the initiating questions are forgotten, we readily misconstrue the task of theory and the answers previous thinkers have given become narrowly confining conceptual prisons, degenerating into little more than a special, professional vocabulary applied to situations and events that can be described with equal or greater precision in ordinary language".

2. To reiterate a central point : it can be considered to be in essence a sociological issue; this doesn't imply that sociologists analysing it shouldn't (or won't) employ psychological or perhaps even biological concepts. Nor does it mean that members of other disciplines can't illuminate the problem. It does suggest, however, that adequate understanding of the phenomenon will likely rest upon the formulation of a logical construction appropriate to that level. In other words, there will be more things that can be said about the phenomena in hand than simple reliance upon psychological and biological analyses would allow. (A possible - simplified - biological contribution to understanding of such phenomena may reside in the theory that human phylogeny has resulted in a predisposition to defend perceived territory, and to unite on a common-ground, perceived - similarities basis against intruders and outsiders. Experiential factors being necessarily interwoven with the development of behaviour patterns along these lines, this may be considered a psychobiological concept, the nature and adequacy of which is the subject of intense debate across and within disciplines - hence this thesis).

3. This third category of discontent links complaints that have in common an underlying sentiment that the logic of emergence has been too dictating a force, and an inappropriate one, in the determination of behavioural science structure. The ultimate implication is that no attempt should be made at this stage to rigidly establish permanent, clear-cut fields of study.

Recognition of problems generated by dividing the investigation of behavioural phenomena into separate disciplines has motivated some to suggest we should actively and constantly explore alternatives. Piaget judges resistance to this as indicative of failure to appreciate the true significance of disciplinary relations:

"the object of any innovatory trends is to push back the frontiers horizontally and to challenge them transversally. The true object of interdisciplinary research, therefore, is to reshape or reorganise the fields of knowledge by means of exchanges which are in fact constructive recombinations... their significance far exceeds that of a mere tool for facilitating work, which is all they would amount to if used solely in a common exploration of the boundaries of knowledge. This way of viewing collaboration between specialists in different branches of knowledge would be the only possible one if we admitted a thesis to which far too many research workers still unwittingly cling - that the frontiers of each branch of science are fixed once and for all, and that they will inevitably remain so in the future." (Piaget, 1973a, p.67)

There are of course implications for the training of vocationally-oriented behavioural scientists inherent in this view. A reconstruction of tertiary education may be profitably based on a working back from carefully established ideal characteristics of practitioners. As occupational requirements change (whether research or applied) so should education, with new mixes transcending traditional borders (the basis of them being irrelevant to new concerns) so as to produce workers maximally prepared for role requirements. This should prove preferable to simple reliance on collaboration between people of diverse backgrounds (given the discussed difficulties for co-operation inherent in specialization).

The study and treatment of abnormal behaviour is a prime candidate for reconstruction in this sense. Transgressions of built-in limits by the many specialists involved is no doubt responsible in part for the multitude of concepts and theories that are championed polemically within this area. Over-extension of basically sound (within their range of convenience) ideas in itself reflects mutual lack of awareness of ultimate complexity, and of possible contributions that can be made by others from different backgrounds in the attack on a given problem. The unfortunate

consequence of all parties pushing their respective barrows willy-nilly has been the development within the global psychiatric realm of "a welter of confusing opinion, within which dogmatism and nihilism co-exist" (Bliss, 1968, p.ix). This outcome - either acceptance of a few rigid ideas as 'the truth' or rejection of the entire field - is endemic where behavioural scientists are obliged to leave the sanctuary of ivory tower existence to confront real-life phenomena in all its complexity.

We must strive to be constantly alert to the possibility of reconstructions, of creating new syntheses, and of establishing new kinds of theoretical integrations that have little in common with traditional conceptions. Growing recognition that current practice is not particularly conducive to this is reflected in Mead's (1968, p.377) assertion that "we need a science of behaviour, not a bunch of behavioural sciences", Halsey's (1967) elaboration on the "exciting possibility of a unified science of behaviour" emerging from convergence of genetics, psychology, ethology and sociology, and Chapple's argument that attempts to parcel out aspects of human society between academic disciplines are simply unworkable (Chapple, 1970). As Halsey (1967) remarks, many of the great figures of the past such as Darwin, Freud, and Comte took it for granted that science is a unity. In short, our approaches to general problems should be multi-disciplinarian rather than interdisciplinary.<sup>1.</sup>

The on-going emergence of convergences stumbled upon in spite of orthodox intentions and plans (they often lie undetected) provides evidence of the validity of this proposition. In reviewing the previously mentioned Rice symposium on personality, it was noted certain concepts derived from one field appeared as central elements in the formulations of writers of other disciplines. For example, the concept of self-identity, originating in psychology, was employed as a central idea in sociological writings. Other concepts, independently formulated in different fields, were revealed as closely similar or analogous when carefully analysed. Few papers failed to show strong influences from other disciplines (Norbeck *et al*, 1968).<sup>2.</sup>

---

1. This argument is implicit in Piaget's statement that "each discipline employs parameters which are strategic variables for other disciplines, and this opens up a vast field of research for interdisciplinary collaboration ... (but)... *collaboration is only too often reduced to juxtaposition*" (Piaget, 1973a, p.12). (My emphasis). Clearly on-going active reconstruction is required.

2. It must be kept in mind that authors attending such interdisciplinary events may be on their best behaviour in this regard; the broad interdisciplinary tone of their presentations may be token to the occasion.

Another example of the basic inter-dependence that exists irrespective of mass awareness is provided by Gardner Murphy, who identifies five principal methods in psychology (cross-cultural, developmental, clinical, experimental, quantitative) none of which were devised by professional psychologists!

Some convergences have occurred because analysis of common ground has stirred indignation in 'opposing camps' (as they become, once competition is perceived). This forces interaction - usually quite heated. Tinbergen (in interview with Hall, 1974) notes the validity of Koehler's four-phase process, in which the other 'sides' point of view is firstly ignored (its possibly too uncomfortable to contemplate - a comment on the psychological nature of science). Secondly, the other point of view is criticised (when its apparent it won't go away, and may start people thinking that it is the right view). Thirdly, the issues not being simply resolved in some win/lose, black/white manner, collaboration occurs. Paradoxically, the final stage is reached when all concerned can claim "its all so self-evident we don't need to talk about it anymore". Such a process has been evident in the interaction between ethology and comparative psychology. As late as 1955 a major comparative psychologist (F. Beach) purported to have conducted an extensive review of animal behaviour studies without coverage of the work of European ethologists (Burghardt, 1973). Intensive debate took place in the literature (and at conferences); collaboration occurred (Eibl-Eibesfeldt, 1970, lauds learning theorists such as J.S. Lehrman who compelled ethologists to clarify concepts; the influence of ethology the other way has been much noted, and will be discussed in detail later); and now the situation is such that Tinbergen (interview with Cohen, 1977) can claim that "you can't really talk about 'ethologists' anymore" as ethology and comparative psychology have merged substantially (zoology and psychology share the same building at Oxford, Tinbergen points out).<sup>1</sup>

Convergences cannot be relied upon to occur automatically. As Pribram (1968) argues, the same event is usually structured differently in different realms of discourse, thereby hindering communication and recognition of similarities, complementarities, and differences. He advocates more active

---

1. Ever the interdisciplinary diplomat, Tinbergen may be overplaying the resolution of conflicts in this area. Polemics similar to those characteristic of early learning theory/ethology debate are really just getting underway in the larger sphere, with the extension of ethology into the analysis of human behaviour strongly opposed by sociocultural analysts in particular. Detailed consideration of this debate occurs in Section Two.

attempts to cut across conventional borders, in which he gains support from Mead (1968) who contends that, if data accumulated within various disciplines were submitted to an analysis capable of spelling out convergences, then behavioural science progress would significantly improve. The problem is to develop that form of analysis.

Events have of course been made in limited spheres. These serve to demonstrate not only that the need for cross-disciplinary merger has been felt, but also that the problems involved are substantial.

Social psychology, for example, represents an attempt to bridge the gap between psychological and sociological modes of thought and analysis. It exists because psychology cannot be identified simply with the singular, the subjective, and the 'inward' view, and sociology with the social, the objective, and 'external' view. Yet, claims Heine (1974, p.vii), "despite Lewin's repeated assertions decades ago that  $B = f(PE)$  - behaviour is a function of person and environment - psychologists concerned with personality have managed to avoid coming to grips with E and sociologists have given little credence to P's role in social process". It might be fairer to suggest that they have emphasised (or over-emphasised) in accord with their major preoccupations; the point remains that social-psychology as a merger is at best only partially successful, as it tends to fall into two basic orientations. Sociology inclined and psychologically inclined theorists each endorse their own brand of social psychology. The real problems remain - personality theory often has very limited predictive power because individuals are remarkably responsive to their social milieu, and sociological prediction and analysis is often woefully inadequate because "individuals do not do what they are theoretically supposed to do" (Heine, 1974, p.vii). While the development of the field is in itself of value - for it symbolises the need to transcend borders, conventional categories, and modes of analysis, and to recognise the complexity of human behaviour - it also serves to demonstrate an appropriate *modus operandi* has not been developed. As Heine (1974, p.xvii) comments:

"so called interaction doctrines<sup>1</sup>. derive interpretations of personality and social system from a common theory of social action; the concept of person is emphatically social and stands in contrast to the idea of a person as a psycho-biological organism who happens to inhabit a thoroughly social environment.

---

<sup>1</sup>. Note this refers to 'interaction' theories of social psychology; they are not interactionist theories of the developmental kind.

In these altogether unsettled formulations of the relation between individual and society, the unsettling source will, from the standpoint of sociology, appear again and again as the 'problem of personality'; and, from the psychological standpoint, 'the problem of the group'.

Perhaps the problem is that *it is* an attempt to bridge branches of learning hitherto regarded as essentially separate - and hence are represented in behavioural science structure by separate disciplines. Recognition of inherent problems is implicit in Tajfel's (1977, p.582) comments that social psychology needs to develop a more adequate analysis

"of man as both a creature and creator of his society.... (social psychology's) subject matter lies in the area between the biological and social sciences .... (biological) perspectives contribute to the understanding of how and why man became the kind of social animal he is; they also define his limitations, particularly in relation to laws governing his development, both as a species and individual .... but.... man has created much of his environment, social and physical .... the understanding of these modes of adaptation requires a level of analysis that transcends the biological."

A very fundamental change in behavioural science orientation may be required, the nature of which is hinted at in Tajfel's synopsis of the problems confronting the development of an adequate social psychology (present value being very much unproven in his eyes). In the analysis of man's social life, one has to be able to transcend conventional boxes, disciplinary boundaries and conceptualisations, and, perhaps more importantly, transcend awareness of transcendence! Tajfel implicitly appears aware that recognition of phenomena emergent from man's social existence requires new levels of analysis, but this does not mean lower levels of analysis - rather, the products of these - cease to have value in the comprehension of social phenomena in total. We have to develop new and more sophisticated ways of relating, of reconciling, knowledge and concepts derived from varying theoretical constructions and languages associated with differing levels of analysis. We must be as aware of the dangers of false abstraction - of the sort of magical thinking that often goes hand in hand with emphasis upon recognition that wholes may possess characteristics not fully explainable in terms of the properties and relations to one another of their constituents, these characteristics in turn determining in some ways the properties of constituents and relations between - as we are of the dangers of wholesale reduction. Recognition

that certain wholes (the delimitation of which is essentially arbitrary) possess *some* characteristics that are in some sense more than the sum of parts does not mean that the characteristics of the parts and their relations *no longer have significance in the understanding of that whole*. Furthermore, the existence of emergent properties must be demonstrated rather than assumed.

It may well be legitimate to limit study and analysis to the 'new' relations, emergent phenomena, that develop from social interaction; this carries with it all the dangers of specialization,<sup>1</sup> and is arguably harmful when globally labelled 'social psychology' or 'social psychological theory'. The danger of delimiting concern in this manner is that often, ultimately more is 'explained' within the bounds of the construction developed than the initial focus warrants (i.e. Piaget's reduction to sociology is indulged in). It would be a logical disaster to analyse properties emergent from social interaction, the properties of the group, group structure and so on, and then explain all 'lower' processes and constituent characteristics in these terms. Yet reduction of this sort appears common in (especially socially oriented) behavioural science literature, while careful analyses of the properties of parts, the sum of parts, the relations of parts, (i.e. the necessary, if ultimately insufficient, components of the 'whole'), and the relationship between all these constituents with the whole, appears to be not so actively engaged in. The sheer complexity of such a task may be in part responsible; but fumbling with such complexity may be the price we have to pay for progress. (Certainly avoiding confrontation with complexity is aided and abetted by rationalizations of the evil of reductionism - any self-respecting social scientist knows no light can be shed upon the secrets of human existence in social/behavioural/action terms by indulging in biological theorising, for example).

Hint of possible resolution as far as social-psychological phenomena is concerned resides in Tajfel's statement that biological perspectives contribute through detailing the kind of social animal man is, the limits and laws governing his development both as a species and individual.

---

1. viz. leads to tendency to attempt to explain too much in terms of parameters of personal knowledge, accentuated by essential ignorance of other factors involved, especially on the perimeters; loss of energy and time expended in 'territorial' defence against alternative approaches, bordering analyses, etc., (possible complementarity being beyond recognition); other dysfunctions of defensiveness.

While social-psychological theory often exhibits awareness that man's cultural modes of adaptation require a level of analysis that transcends the biological (i.e. the standard sociological version of social-psychology theory), they do not correspondingly manifest awareness of the other side of the coin. Attempts to transcend both - to view both sides of the coin, and the necessary connection between - appear noticeable by their absence. Interesting in this regard is Heine's representation of psychology's side of social psychology as 'personality' and personality theory - yet how often does this concern with the individual-organismic, individual processes facet reflect sophisticated awareness of evolutionary and functional biology? Often assumptions basic to personality theorising are drawn from a fertile and essentially arbitrary imagination, or they reflect mirror-image like the social determinist assumptions emanating from social analysis. A solution to the problem of developing an adequate attack on problems conventionally labelled social-psychological may await more subtle and adequate formulations of 'social', 'psychological', and 'biological', such that tentative steps may be taken in the direction of a psycho-biology of social interaction and social phenomena.

Less formal attempts have been made to bridge sociological and psychological analyses. The writings of Parsons, Homans, and Blau (all sociologists) overtly manifest Inkeles' (1959) claim that sociological theory incorporates psychological assumptions.

Talcott Parson's overt attempts to bring psychology into sociological analysis have been limited to attempts to reconcile Freudian theory with his 'structural - functionalism'. While this is encouraging insofar as it implies recognition of the interrelatedness of disciplines and levels of analysis, it tends ultimately to parody such recognition. Freud's concepts and theoretical priorities suffer severe permutations and distortions through Parson's endeavours to fit them into his pre-judged construction of relations. The id virtually disappears, as does the ego largely; and while some degree of individuality consequent to organism - environment interaction is conceded, this appears a token gesture, as Parson's over-riding conceptualization of personality is that it is a socially generated societal resource; the link between biological organism and social structure (see Parsons, 1961).

Interestingly enough, Freudian theory is often employed in socio-cultural analyses where theorists wish to discuss relations between psychological and social processes, or where they feel compelled to make explicit their psychological assumptions. Yet psychoanalysis (as a body of theory) is far from a major pre-occupation within modern academic psychology. Use of Freudian concepts by socio-cultural analysts makes a mockery of the very essential continuity and interrelatedness of behavioural science that it implies, for either (a) socio-cultural analysts who employ Freudian conceptions simply are not aware of the criticisms of those most overtly concerned with processes of that nature, namely, academic psychologists, or (b) academic psychology has been misguided in rejection of Freud, and socio-cultural analysts are aware of something which psychologists are not. Either way, the situation speaks volumes for the current multi-discipline state. (One might further ask : upon what basis do anthropologists and sociologists who employ Freudian theory as a reliable guide to psychological processes select concepts from his range of writings? - for a Freudian concept or formulation can usually be found to suit any event or pre-formulation).

Homans and Blau (separately) have also covertly incorporated psychological assumptions and conceptualisations into their analyses of social phenomena. They have utilised behaviourist notions in contrast to Freudian borrowings and props. Assuming that all socio-cultural analysis does incorporate assumptions and theories of psychological process (but usually implicitly), therein lies an important link across disciplines and levels of analysis. The outcomes of psychological functioning and processes form the basic units of socio-cultural analysis; in turn psychological constructions (analysis and synthesis) incorporate as basic units outcomes of biological analyses, syntheses, and constructions. Constructions of the relations and processes of psychological phenomena - psychological analysis - provides the inevitable and necessary link between biology and social structure. (This is evident, for example, in the writings of Skinner. On the one hand he refers to the products of biological analyses, but as 'givens', starting points to be accepted rather than further analysed, as his task is to study behavioural phenomena emergent from interaction of the

biological organism with its environment. On the other hand he engages in informal socio-cultural analyses. That he is attacked from both directions - for mis-representing biological knowledge on the one hand in the form of his basic assumptions about the organism, and for reducing social complexity to mere manifestation of the operation of a few conditioning principles on the other - is a positive comment on the inter-relationship of all levels of analysis).

As behavioural problems do not fall readily into neat disciplinary piles; as convergences occur across disciplinary borders despite structural impediments; and as psychological theorists overtly employ biological data and concepts, social theorists psychological data and concepts; behavioural science structure, practice, and associated assumptions about levels of analysis and so forth should be constantly under review. In Piaget's words:

"the link between a 'higher' (in the sense of 'more complex') and a 'lower' field results neither in reduction of the first to the second nor in greater heterogeneity of the first, but in mutual assimilation such that the second explains the first, but does so by enriching itself with properties not previously perceived, which afford the necessary link. In the case of the human sciences, in which there can be no question of growing complexity or of declining generality, because all aspects are to be found everywhere, and because delimitation of the different fields is a process of abstraction rather than a question of hierarchy, mutual assimilation is still more necessary and there is no danger of vitiating the specificity of phenomena. The difficulties, however, are considerable. But, apart from the difference between various forms of university training, which is undoubtedly the main obstacle to be overcome, the common logico-mathematical techniques that are gradually coming into general use are at once the best indication of the convergence that is called for and the best means of effecting a junction." (Piaget, 1973a, p.67)

Cybernetics as a form of awareness, source of concepts, and method of analysis, is determinedly transdisciplinary, and provides an exemplar of sorts for reconstructions proposed later. However, immediate preoccupation rests upon elaboration of the subtle but significant relationships that exist between the multi-disciplinary state of behavioural science, and fragmentation in the study and analysis of

behaviour at the level of the individual (i.e. traditionally, the field of psychology). This is most effectively conducted on a point-by-point basis.

(1) As sociological theory incorporates psychological theory, fragmentation within psychology forms a basis for fragmentation within sociology. Differences between models of the basic functioning of man transcend disciplinary borders; the issue of fragmentation at the psychological level of analysis is not distinctly separate from the general fragmentation of behavioural science. Just as physiological findings incompatible at base level in a psychological formulation will seriously weaken that scheme (despite the fact that it may not be *strictly reducible* to physiology), so too will socio-cultural theory be undermined by relatively definitive resolution on some dimension of man's basic nature if this is incompatible with assumptions implicit in the larger scheme.

(2) As psychological explanatory relations and formulations incorporate basic assumptions predicated upon biological knowledge and assumptions (or interpretations thereof),<sup>1</sup> differences between psychological paradigms are based in part on differences in the employment of these assumptions; in other words, differences between psychological paradigms rest in part on the status accorded to man as a biological entity, and the perceived significance, and role of, biological phenomena for human behaviour/psychological functioning.

(3) Connecting as it does the biological and social spheres of analysis, psychological analysis serves as a central focus for analysis and synthesis of models of man in behaviour/psyche processes and functioning terms. Basic differences in psychological paradigms rest upon (often metaphysical) basic assumptions about man - the basic material, the essence of humanness rather than its substantive products, and these basic differences divide across disciplinary borders. Resolution of these differences may therefore be highly significant

---

1. 'Paradigm', 'revolutionary' and 'normal' science, are conceptualizations derived from Kuhn (1962). A paradigm is an intellectual context of accepted techniques and aims, and of accepted criteria both of inference and of the relevance of particular phenomena (Stenhouse, 1971). 'Paradigm', therefore, does not = theory, nor discipline. Prime concern here is with the 'sociological' sense of the term; paradigm as the possession and governor of a group of practitioners; a constellation of commitments on conceptual, theoretical, and methodological grounds. Further discussion is incorporated as relevant later in this section.

in terms of unifying behavioural science endeavour, and for providing a solid, common base for more detailed exploration. Models of man's basic status in psychological behavioural processes and functioning terms (operating principles, species-specific characteristics) may be viewed as made up of a constellation of points on various interwoven dimensions, the various dimensions being basic issues such as essential passivity/activity; malleability versus structural (in broadest sense) constraints; importance of experiential factors in behavioural development relative to maturational factors, and the nature of relations between; the relative importance of 'higher' and 'lower' processes, and whether such a differentiation can and should be made; the relationship of human behavioural/psychological processes to those of other species.

Basic psychological models confront the problems of definition and elaboration of learning, behavioural development, behaviour/skills acquisition of socialization in general. It is important to distinguish between the analysis of these processes - which say something fundamental about man - and the material acquired; what man does with these processes, which lifts the level of analysis into the sociocultural realm. The nature of the processes does not determine the 'material', or 'contents', of human production and social existence (although very general predictions, viz. constraints, predispositions, predilections, may be possible); conversely description of content (products, social structures, materials<sup>1</sup>) does not tell us a great deal about psychological/behavioural process qua process, psychological/behavioural organisation qua organization. The link between basic models of man in this sense and sociocultural analysis is not straight forward; nevertheless some basic distinction is not only possible but necessary if longstanding (and currently intensifying) conflicts between "naive biological reductionists" and "sociocultural mystics" are ever going to be even partially resolved. The type of clarification suggested at least offers the promise of identifying what the real issues are, these usually being far from apparent in broad-based, sweeping, across-disciplines debate on 'man'.

---

<sup>1</sup>. Keep in mind the very abstract senses in which 'contents', 'products', and 'materials' are being used here.

At this stage it is sufficient to note the basic complexity involved. Constraints and organizational principles such as may exist in human psyche/behaviour may ultimately determine limits to cultural variation, and may be the basis of some cultural universals (the identification of which may have been hindered by preoccupation with superficial variants in surface manifestation). But a basic model of human functioning will not allow detailed prediction of cultural form, variations, social structure, or of the exact nature and outcomes of 'macro' social processes. (Description and analysis of these may well be assisted and illuminated, however). Conversely, the analysis of social structures, institutions, norms, and so forth may well shed light on the basic nature of man at the psychological functioning level - but we should be wary of the error of formulating absolute principles of man on the basis of socio-cultural descriptivism. The study of products is not the study of processes, despite the obvious (and potentially very confusing) relationships between.<sup>1</sup>

(4) The necessary interaction of psychological analysis with biological analysis on the one hand and socio-cultural on the other is impeded by the multi-disciplinary state of behavioural science. Resolution on the various issues that in combination form the skeleton of a model of essential human functioning is hindered as relevant conceptualizations, data, and explanatory relations, derived from other levels of analysis filter through slowly, and are often distorted in the process.

#### Fragmentation in Psychological Analysis

One will search psychological literature (research and theory) for a coherent set of principles that define man's basic psychological/behavioural organization in vain. The extreme fragmentation of psychological endeavour, apparent along numerous dimensions, makes the

---

<sup>1</sup>. Certain arguments - that psychological structure and process is interwoven with social process, that people only become 'human' through socialization, that 'the individual' is a hypothetical construct due to man's social existence, that talk of 'basic nature' is misguided as environmental influences begin at conception etc., etc. - are of course important, but should not be allowed to sabotage the very basic clarification sought here. In fact, such considerations usually lead to thinly-disguised, premature and unwarranted formulations of man as *tabula rasa*.

multi-disciplinary nature of the general state seem very simple and tidy by comparison.

Hull (quoted by Koch, 1973, p.82) wrote in the 1930's:

"Among psychologists there is a bewilderingly large diversity of opinion, ... we are divided into sects, too many of which show emotional and other signs of religious fervour. This emotionalism and this inability to progress materially toward agreement obviously do not square with the ideal of objectivity and certainty we associate with scientific investigation; they are on the other hand more than a little characteristic of metaphysical and theological controversy."

Koch comments that we are still divided into sects, but perhaps thrice as many. Dismay at the splintering and polemics evident within psychology is frequently expressed. Some examples:

"The debate (between learning and gestalt theorists) was intemperate and engendered loyalties that still exist..." (Hebb, 1964a, p.4).

"Acceptance tends to be anonymous, while rejection is personalised." (Jahoda, 1977, p.149).

Commenting on the dichotomy between personality and cognition evident within psychological theory, research, and testing, Heim (1970, p.19) wrote:

"This is an affront to psychology, to common sense, and to semantics. It is an untenable distinction ....."

"There is far too much controversy that isn't productive at all ... the great thing in psychology is that there is a temptation to get drawn into endless arguments so that you waste time you should be spending doing research." (McClelland to Cohen, 1977, p.38).

"There has been all too much divisive rhetoric among our psychological systems". (Brinckerhoff, 1973, p.36).

"Behaviourism turned on its subjectively oriented predecessors with vindictiveness ... what had been a fresh and exciting point of view became dogma" (Gergen, 1971, p.9).

"Trying to combine personality with experimental psychology means sitting between two stools, which means you get clobbered by both sides" (Eysenck to Cohen, 1977, p.118).

"The field of psychology is fragmented and splintered, with schools and approaches so numerous they can hardly be counted, and then with individual variations within each" (Ellis, 1973, p.177).

It seems as if the tendency to view alternative approaches as fundamentally incompatible, and to engage in hostilities rather than seek common ground, is primary in psychological enterprise.

### Basic divisions in psychology

Various divisions within the field of psychology have been considered sufficiently meaningful in terms of negative consequences, both for the efficacy of individual practitioners and for growth of knowledge, to be explored within the literature.

London and Rosehan (1968) have written of the problems generated by the separation of abnormal psychology from experimental, and Heim (1970) has elaborated at length. She outlines two stereotypes that she believes to exist, one of clinical psychologists (as held by experimental psychologists), the other of experimental psychologists (as held by clinical psychologists). Existing as they do within the minds of the 'opposing' camp, the stereotypes are extremely negative in tone, but perhaps of greater significance is Heim's assertion that a vicious cycle is in progress, with the two stereotypes corresponding more and more closely to two existing types of psychologist, and with the schism between clinical and experimental psychology ever deepening.

The most commonly referred to and broadest division within psychology has been that emergent from its development along two paths, generally labelled as experimental psychology and correlational psychology (Vernon, 1972; Maddi, 1976; Wiggins, 1976).

With experimental psychology, laboratory procedures are developed with the aim of controlling aspects of the subjects' environment in such a way that variations in the behaviour of a subject may be attributed to operations performed by the experimenter. The need for operationalism and experimental control is emphasized, and the dangers of introspection, intuitivism, and 'common sense' psychology are stressed. Correlational psychology, on the other hand, attempts to clarify relationships already existing in 'nature' and to point the way toward clusters of such variables which might be most representatively studied in the more controlled setting of the laboratory. This, however, does not occur as often or as fruitfully as it could simply because of factors associated with the basic 'separate schools' nature of the situation.

In addition to this, whereas experimental psychology attempts to discover relationships which hold for all individuals, correlational psychology is most concerned with differences among individuals within such laws.

The central problem increasingly recognised with this essentially dual development of psychology is that, while it should be obvious that psychology is equally concerned with both approaches (individual differences research, fathered by Galton, on one hand; the general laws, environmental 'determinants' of behaviour and experimental study of functional relationships approach fathered by Pavlov, on the other), such a view is not widely held (Eysenck, 1967). Hirsch (1968) supports this claim, arguing that experimental psychologists "ignore individual differences almost as an item of faith".

Wiggins insists that the two movements have progressed in relative isolation for reasons "more historical than logical", while Eysenck asserts that "the neglect of one line of investigation by those engaged upon the other is one of the great weaknesses of modern psychology, and it seems likely that many of the troubles of our science arise from this cause" (Eysenck, 1967, p.341).

Wiggins, being more closely associated with the general correlational stream, tends to concentrate on criticizing the inadequacies of the experimental school approach. He argues that "it is now recognised that laboratory control, instrument development and mathematical techniques are in themselves not enough to ensure inclusion of relevant variables in a scientific study of human behaviour" (Wiggins, 1976, p.4). In essence, he is proclaiming the need for a more enlightened selection of relevant variables as an initial step before laboratory experimentation, and implies that this can be provided by the knowledge, techniques and methods of the correlational approach. This is sound enough as far as it goes, but leaves the uneasy feeling that Wiggins, while recognizing the inadequacy of the dual stream situation, is not fully aware of the shortcomings and limitations of both approaches, especially the correlational. No doubt thoroughly imbued in the correlational paradigm, he seems to believe that application of the statistical methods core to the correlational approach will in itself efficiently and effectively uncover major significant relationships which the experimental approach can tease out in finer detail. The inadequacies of this sort of assumption are discussed later.

Eysenck's discussion of the undesirability of this fundamental two-stream approach in psychology is broader. Insisting that experimental psychology cannot continue to neglect individual differences, he also points out that, on the other hand, the personality (correlational) side could benefit greatly from the fruits of experimental psychology as dozens of schools of personality exist, yet in his estimation there is very little in the way of factual support, theoretical rigour or experimental demonstration for any of them.<sup>1</sup> Vernon (1972) supports these views. Lamenting the neglect of personality because of its complexity, he calls for increased consideration of whole man and his motivation. As he points out, the relative neglect of personality within academic psychology hasn't prevented the development of a situation in which we find "a welter of different theories, based upon different methods and constructs. There is little agreement on data to be collated, still less on definition."

It would be a mistake to assume that further splintering has occurred only on the personality/correlational side of this major division. Two distinct camps are distinguishable in the experimental realm; operant conditioners on the one hand, the rest on the other (Hilgard and Bower, 1975).

Clear-cut specialization (as opposed to 'schools' type of adherence discussed so far) has also been questioned within psychology. Bruner (1970b, p.54) warns of the implications of educational psychologists being "very, very far from where the knowledge gets made", for example, while Anastasi (1967), herself well established within the field, has drawn attention to the dangers of specialization in testing (psychometrics). She asserts that this trend has increasingly led to naive interpretation of test scores that are not in keeping with the advance of psychological knowledge, and that, on the other side of the coin, other psychologists who use tests often have an inadequate knowledge of them, because their training in the area has been thin ('not their speciality'). Anastasi quite clearly holds the creation of the separate field of psychometrics as largely responsible for the perpetuation of misconceptions about tests (the consequences of this being damaging to the whole of psychology).

---

<sup>1</sup>. Except, of course, his own!

The problems, inherent limitations, and stupidities of isolationism and compartmentalization are, then, beginning to gain recognition within psychology. Exactly *why* psychology is so fragmented, and whether this is inevitable, is not readily agreed upon.

Psychology : separate sciences or embattled paradigms?

Analyses of the reasons for the fragmented state of psychology fall into two major groups. The first group of reasons essentially hold that psychology is not, and cannot be, a coherent science, but must be recognised as a collection of sciences. Failure to recognise this has been the cause of inter-group hostility. The second type of analysis places more emphasis upon the validity of competition between schools of thought and modes of analysis, i.e., recognizes that the various systems often are fundamentally incompatible, the hostilities emerging from attempts to establish the superiority of one formulation over others. A careful analysis demonstrates both to be partially correct; both sorts of explanation can contribute to understanding of the fragmented state of psychological analysis, and of the general fragmentation of behavioural science.

Psychology as separate sciences. Beloff asserts that psychology is not a single unified science but a collection of more or less loosely affiliated disciplines each with its own peculiar concepts and laws, its own methods and techniques. He argues that "each is strictly limited with respect to its appropriate domain and with respect to its explanatory potentialities". Failure to recognize this situation (i.e., 'schools' as partial and provisional contributors to the "infinitely complex" and many sided study of man, an "essential plurality") suggests Beloff, accounts for the notorious controversies that have dominated psychological history, and "for the fact that, in place of a series of complementary sciences, one finds instead an array of embattled schools locked in mutual hostility and mutual incomprehension" (Beloff, 1973, p.12).

A more radical position is forwarded by Koch (1973, p.86):

"psychology cannot be a coherent science, or indeed a coherent field of scholarship, in any specifiabile sense of coherence that can bear upon a field of inquiry."

While not wishing to contest the implicit message that ultimately, comprehension of behaviour will require understanding of the products of diverse branches of inquiry (rather than the acceptance of one 'explains-all' theory), these 'explanations' leave several questions unanswered. Why hasn't this "essential plurality" been better understood and accepted? Why haven't the complementarities of the "separate psychological sciences" been recognised and developed? Beloff himself sows the seeds of doubt of the adequacy of this explanation when (despite acknowledging its practical successes) he refers to behaviourism as "the joker in our pack" (p.x), for this type of comment brings to awareness the fact that behavioural science is in itself a human activity, the very conducting of which becomes a legitimate behavioural science problem.

Embattled paradigms. An opposing explanation of fragmentation and associated polemics holds that no one paradigm has been sufficiently able to demonstrate fundamental validity at the expense of other. While separate approaches may well focus on different facets of behaviour, taking this as evidence of a straight-forward separate-sciences situation doesn't do justice to the forces of academic fashion, the true nature of inter-school polemics, and the fact that differential focus and concern reflects differential commitment on basic dimensions. Different approaches often reflect differences in basic beliefs as to where the key to the secrets of behaviour lie.

Certainly there is much to suggest that an understanding of conflicts, of the development of separate competing paths of thought and commitment, will be gained by a Kuhnian type of analysis rather than assuming progressions, developments, and new modes of thinking and analysis have developed in some strictly orderly and logical fashion (within which the 'separate sciences' all fill neatly arranged niches, the total pattern of which forms a comprehensive attack on behavioural problems, in keeping with the essential multi-faceted nature of those problems). Hebb (1964a), for example, writes that the basic premise underlying cognitive theorist/S-R theorist clashes is "either S-R formula is the whole story, or else it is irrelevant", and points out that "the sedition of one period may be the good sense of another". The desire

to adhere to one set of simple answers for most problems is also noted by Tajfel, who talks of "crude extrapolations, reductionism of sorts in the form of simplistic adherence to belief in the over-riding role of one or two processes or principles" (with Cohen, 1977). While it may finally prove impossible to view psychology as a coherent science (only time will be the judge, and anyway the question hinges upon semantics to a large degree) we should be wary of perpetuating what Heim (1970) has identified as a longstanding convention in psychology - that of dividing psychological concepts into separate compartments, of dichotomizing what are in fact continua, as the separate-sciences argument provides a perfect rationalization for this.

A favourite gambit in psychological historiography is to review developments as a series of orderly progressions, logically predictable given certain key research findings, and carrying the implication not only that we know so much more now, but also that we *know better* now. Kagan (1973, p.42) writes of psychology as a young discipline (the first half of this century) seeking roots, and "like a child, attached itself to an absolute description of nature" and so was "absolutistic, ... and intolerant of ambiguity". It is the advantage of a Kuhnian type of analysis that we recognise processes involved then are still operative.

Counter to Kagan's implications, and regardless of whatever real gains have been made in knowledge, absolutism is alive and well in psychology and underlies many inter-school conflicts. (Reasons for assuming that it may be a perennial problem in behavioural science are discussed shortly). Broadbent (1964; with Cohen, 1977) for example, believes that, with the growth of various brands of humanistic psychology, despite its objective achievements his form of 'scientific psychology' is under threat; he fears that psychology will regress.

The problem confronting Beloff's analysis then, is that the various approaches don't behave toward each other as separate sciences operating within the same general band of analysis. Rather, they tend to act as if they are in the process of building the psychology<sup>1</sup>; the

---

<sup>1</sup>. McClelland (with Cohen, 1977) suggests psychologists do not usually place their new brick of knowledge on someone's well-established one; they want to start a new pile.

validity of which the rest will come to recognise in time. Pressure still exists to accept one system over others (a brief glance through behaviour modification texts illustrates this, as most feel obliged to present caricatures of psychodynamic and trait theories which they then knock down).<sup>1</sup>

Separate sciences/warring paradigms : a reconciliation

Both analyses of the causes of fragmentation and inter-school hostility within psychology do not, in themselves, do justice to the complexity of factors involved. Furthermore, general acceptance of one as a valid analysis could have severe repercussions for the situation. If it is simply accepted that psychological problems, being many sided and complexly related, generate separate approaches that are uniquely appropriate to the study and analysis of one particular facet of behaviour, then the danger is that progress-producing conflict between systems will not occur (for systems that perceive competition tend to keep each other up to the mark). Conversely, if it is assumed that one psychology can be developed, and that identifiably different approaches exist, then tendencies to accept or reject systems wholesale may be promoted. On the one hand, we must be alert to differential focus (and hence attempt to fit together in a larger formulation the respective schemes and articulations of relations generated by the respective 'sciences'); on the other, constant look-out for areas of genuine competition between systems is desirable, for clear advantage of one over another in the ensuing debate/research creates an anomaly for the 'defeated' shema, and thereby promotes an environment for revolutionary reconceptualizations in Kuhn's (1962) terms.

Ironically, the same training prescription - wide, varied education across the psychological sciences/approaches - can be drawn from both analyses. If polemics occur because of failure to recognise essential plurality, then practitioners must become aware of the basis of that plurality, i.e. must have some basic understanding of all the sciences

---

<sup>1</sup>. After interviewing a wide range of prominent behavioural scientists (all referred to as psychologists, but actually of diverse training background, viz. doctors, linguists) Cohen (1977, p.14) concludes "they do not share the same assumptions, the same language, the same idea of what counts as a decent piece of research ... no one school is willing to allow the others a right to exist as psychologists" and "one man's paradigm is another's untruth..." (p.342)

in order to know the appropriate domains and explanatory potentialities of each. Conversely, if fragmentation is largely based upon the existence of genuinely competing paradigms, then it is desirable that students are familiar with the basic exemplars and commitment constellations of each; in this way the limitations of each may ultimately, and more rapidly, be transcended.<sup>1.</sup>

It seems reasonably obvious that the various psychological systems can be viewed as both separate sciences insofar as they manifest some degree of differential focus, and as paradigms, because, not only is there bound to be some competition between concepts, methodological prescripts, and basic assumptions made, but also the very basis of differential focus reflects variation in commitment upon such issues as : what is psychology? What ought it be? What counts as a psychological explanation? What is the most direct and appropriate way to uncover the laws of human psychological process and function?

It is, for example, quite possible to positively assess and draw together into some larger formulation the products of both cognitive and behaviouristic theory and research (to recognise that there is not just one set, one type, of process in human functioning), and still recognise that there are areas of genuine conflict between the two sources viewed as paradigms; and also that firmly entrenched proponents will object to the mixing, confluence, and discriminative use of concepts and principles in this manner.<sup>2.</sup>

1. Somehow a balance must be struck that adequately reflects two principles, captured on either side by the following quotes:

"To know the truth partially is to distort the universe...An unflinching determination to take the whole evidence into account is the only method of preservation against the fluctuating extremes of fashionable opinion" (A.N. Whitehead).

"In order to see anything at all, we must give up the hope of seeing everything" (J.S. Bruner).

2. Piaget (1973b, p.48) makes several points closely related to those attempted here when he writes : "Needless to say, the structures used in these various branches of psychology are not all identical. This diversity also holds out great promise, for sooner or later the problem of their co-ordination will arise, and the differences as well as the possible transitions from certain structures to others will have to be taken into account. It is perhaps from such a system of transformation and interchange that psychology will develop a fundamental unity, though it is still only a remote dream today."

Recognition of the breadth and diversity of subject matter tends to lead to non-acceptance of any one theory or method as having proper monopoly, and to consider that complex problems are profitably approached, even simultaneously, from different viewpoints. However, there remain those who see virtue in rigorous adherence to a particular school or methodology, insisting on attempting to explain all within the parameters of their particular theory or approach. For them there truly is only one paradigm; they often seem to be unaware of genuine competition, let alone the possibility of expanded knowledge through integration or synthesis. Broad, integrational approaches are denounced. 'Eclecticism' often carries distinctly negative tone in psychological discourse. It often seems to imply a lack of rigour, scientific strength, or more generally, suggests the activity of the near-ignorant or uninitiated.

Psychoanalysis and behaviourists (of radical or metaphysical leanings) appears to be the two most closed shops in this regard. The reluctance of the psychoanalytic school to recognize at all the validity of experimental testing of Freudian theory (or any other type of testing involving members from other schools) is legendary in psychological literature. Perhaps a better example, because of its prominence in current psychology, is that of the radical behaviourist movement where the active proponents sometimes strive to reject all other approaches, dismissing them as being 'pre-scientific', superstitious or even totally illogical (e.g., see Skinner, 1953; Gewirtz, 1971). Writing on developmental psychology, the prominent behaviourist Bijou (1973, p.78) makes the claim that the field "no longer needs the grand theoretical designs proposed by Piaget, Freud, Erikson, Gesell, and Werner"! Hilgard and Bower (1975, p.248) claim Skinner and his followers "have felt no responsibility for the task of co-ordinating their work with that of others studying learning (and the indifference is regrettably often mutual)". They point as evidence to the extremely high proportion of papers cited in bibliographies in the *Journal of Experimental Analysis of Behaviour* (the house organ of the operant conditioning movement) that are in fact papers published previously in the same journal (approximately twice the percentage that appear in other psychology journals of a comparable specialize nature).

Psychoanalysis and Skinnerian behaviourism are quite clearly candidates for analysis in Kuhn's terms as paradigms. But what of the rest of psychology? Can a Kuhnian analysis assist in the identification of bases of fragmentation?

Paradigm processes in behavioural science.

Constraints of time and space prevent full elaboration upon Kuhn's (1962) conceptualizations of scientific activity; some knowledge of these is assumed in the reader. In brief, Kuhn argues that the history of science does not consist in a steady expansion of knowledge but rather in a series of revolutions or paradigm shifts, in the course of which one world view is replaced by another deemed more successful. Kuhn's use of 'paradigm' varies (in twenty-one identifiable ways in his 1962 text, according to Weimer and Palermo, 1973, p.237. note b), but generally usage falls into two classes, the one of predominant concern here being his 'sociological' sense, which refers to a constellation of commitments at four levels : the conceptual, the theoretical, the instrumental, and the methodological. These provide the 'rules of the game' for science; they define legitimate objects and methods of study, and an established mode of conceptualization. 'Normal science' is puzzle solving endeavour heavily guided by the paradigm; philosophical and metaphysical considerations are held in abeyance in order to get work done. Normal science is highly selective, accepting only those problems which tradition claims can be turned into puzzles that can be solved.

The paradigm processes of immediate concern are those that assist understanding of the fragmented state of psychology and associated polemics. These are the processes of (a) selection of 'relevant' data, (b) the communication of easy concepts to new entrants, (c) treatment of inference as fact, (d) distortion of the historical picture, and (e) a tendency for paradigms to become over-simplified, and to drop out of the picture over-time.

(a) selection of relevant data. The study of behaviour involves selection from a complex nexus of events in time. In Stenhouse's words (1971, p.172) "a particular paradigm does not determine facts simply as facts - but it does determine which facts are to be regarded as relevant to any particular issue". (Note that in 'normal science' the paradigm is taken for granted. It is, as it were, firmly implanted into the psyche and behaviour of the practitioner - he doesn't consciously stop and think : is this in keeping with my paradigm prescriptions?).

This process of selection of relevant data takes many forms. Firstly, there is the setting up of experiments in such a manner that only a limited range of results are likely (those predicted by the paradigm). The philosopher Abraham Kaplan once noted "we are forever asking nature if it has stopped beating its wife". Premack (1971a, p.136) provides an example : the Skinner box is a biased test space, as "if all experiments were confined to this apparatus, it would be impossible to disconfirm the absolute theory of reinforcement despite the fact that the theory is false".

(While on the topic of animal studies, it is humorous to recall Bertrand Russell's reflection that animals studied display the national characteristics of the observer - animals studied by Americans rush about with an incredible display of bustle and pep; those studied by Germans sit still and think).

Paradigm determined bias of this nature can be an extremely subtle affair. Work on self-fulfilling prophecy suggests experimental results with human subjects are significantly modifiable via non-verbal communication (unintended, and unnoticed by the practitioners) from experimenter to subject (Rosenthal *et al*, 1974). Paradigm membership may result in research results being regarded as near-definitive if favourable, rather than as partial answers, with other questions attached. As Hudson (1967, p.27) points out, exceptions are often "regarded as contaminations rather than as starting points for further inquiry". He adds that Darwin forced himself to immediately note in written form exceptions (theories, facts, or observations in opposition to his general beliefs) as he was aware of a tendency to readily forget such factors.

Mahoney (1976a) conducted some interesting research into this sort of phenomena. He sent (bogus) experimental reports for evaluation to a sample of academic psychologists, expecting them to be judged in terms of logic and evidence. However, he found reviewers tended to recommend (for publication) the article only when it reported evidence that supported their positions. When the data contradicted their positions, they were much more inclined to find fault in the research methodology and interpretation, and urged non-publication. (Experimental procedures were identical for all the papers, but results and discussions were manipulated).

(b) Stenhouse (1971,p.172) suggests that in paradigm socialization, textbooks tend to embody an effect of "differential communicability of concepts" by which concepts that can be easily communicated are emphasized, while those more difficult to communicate tend to be passed over. This is closely related to (c), the tendency to treat matters of inference as if they were matters of fact, it being much easier and reassuring to describe and deal with blacks and whites than shades of grey. Perhaps the most blatant process is (d), the distortion of the historical picture through making it seem as if only one paradigm, the one supported, was ever seriously in question, and secondly it is made to seem that particular factual discoveries "exerted a determinative force in the formulation and subsequent support of the successful paradigm" (Stenhouse, 1971, p.172).

Behavioural science literature abounds with examples of this process. A striking recent one is provided by Mancuso, who in his eagerness to formulate a scheme that will transcend the traditional split between experimental and personality theory (a "cognitive theory of personality") indulges in some considerable distortion of recent behavioural science history. He insists that ethologists have borrowed from psychoanalytic theory to explain their observations of aggression and sexuality in animals, and appears to view Tinbergen as the major villain : "some of them (particularly Tinbergen) seem to be convinced that psychologists have worked only within the psychoanalytic framework" (Mancuso, 1970, p.15). He is striving to drive home the point that "formal psychology" (by which he means experimental) has not been regarded as a source for comprehensive theories of personality.<sup>1</sup>

This is of fascinating relevance, (1) because Tinbergen has been one of the most moderate of ethologists in his interaction with psychologists, especially regarding issues related to aggression, (2), because Tinbergen

---

<sup>1</sup>. The drawing in of ethologists appears a little odd - they have hardly been preoccupied with generating personality theory, and anyway there is substantial evidence closer to home within the conventional psychological literature that experimental psychology has been neglected as a source of ideas and information for the development of personality theory. (Lorenz would probably suggest Mancuso's action in this regard is designed to generate greater in-group unity by identifying out-group threats, thereby mobilizing predispositions deep-rooted in pre-human phylogeny!)

clearly enjoys considerable commerce with experimental psychologists and has read widely in that realm (as will be made obvious elsewhere in this thesis), (3), because such blatant, pointless and polemic distortion is hardly conducive to the development of the widespread commerce necessary for the establishment of the "integrated, comprehensive theory" that Mancuso desires, and finally (4), it lends urgency to the task this thesis holds as necessary for the development of greater coherence in the overall-study of behaviour: the incorporation of sophisticated evolutionary conceptualizations to counter the extremes of the cognitive revolution.

That paradigm processes have been at play is evident in the rediscovery of the wisdom of old masters, the works of whom tended to be rejected wholesale in the rush to create 'the psychology' from the 1920's onward. Writing on the learning/maturation controversy, White (1973) elaborates upon the "myths, legends, and fairytales" taught to students, exaggeration of differences between historical theorists being one consequence. Nash (1970, p.81) comments:

"as we grow older we discover the 'old man' to be not so perverse as we found him in adolescence - and so it may be time to admit that our professional fathers - Hobhouse, the elder Drever, McDougall, and others - were not without some useful insights after all."

McDougall with his (infamous) list of instincts has been a favourite target for environmental determinists of various sorts, yet his complex contribution is regaining recognition rapidly. Andreski (1974, p.147): "we witness in recent psychology a flight from the middle ground of good sense which used to be cultivated by people like McDougall", Boden (1972, p.66) claims that even as early as the 1920's McDougall's influence prompted many behaviourists to reject strict reducibility (as opposed to empirical reducibility); and Burghardt (1973b, p.328) points out that McDougall anticipated much modern thought with his break-down of behaviour into cognitive, affective, and conative components.

Broadbent (with Cohen, 1977) clearly holds no truck with the idea that time and experience have automatically brought progress to all areas of psychological activity; discussing applied research in air traffic control towers, he comments "to find something on a complex situation like this you have to go back to William James". In similar fashion, *Psychology Today* (1972; no specific editor) suggests that James' views on emotion have been in the past soundly battered, but that recent (more complex)

research suggests that they "contain more than a grain of truth".

Further evidence that paradigm processes operate, and that behavioural science is in itself very much a psychological and sociological phenomenon, is provided by the neglect of Lashley's work, which, being out of tune with the ruling Zeitgeist, lay largely neglected for twenty years (Boden, 1977; Burghardt, 1973b; Weimer and Palermo, 1973).

These processes of paradigm socialization ultimately lead to it becoming over-simplified and replaced by a revolutionary reconceptualization better able to deal with the anomalies that have inevitably arisen over time. Discussion of the processes does foster the view that the historical nature of psychology is a genuinely problematic issue rather than a matter of textbook history, and that progression is essentially a psychological affair (with 'conversion' involving basic and far-reaching changes in one's pattern of basic commitments; a new way of seeing the world) rather than a strictly logical and orderly step-by-step progression.

Further factors responsible for the perpetuation of fragmentation, isolationism and hostilities in behavioural science enterprise.

While Kuhn's conceptualizations assist understanding of the fragmentation in behavioural science, it must be noted that behavioural science presents special problems over and above those presented by the analysis of physical science activity. As Kuhn (1962, p.viii) notes:

"... spending the year in a community composed predominantly of social scientists confronted me with unanticipated problems about the differences between such communities and those of the natural scientists among whom I had been trained. Particularly, I was struck by the number and extent of overt disagreements between social scientists about the nature of legitimate scientific problems and methods. Both history and acquaintance made me doubt that the practitioners of the natural sciences possess permanent answers to such questions more than their colleagues in social science. Yet, somehow, the practice of astronomy, physics, chemistry or biology normally fails to evoke the controversies over fundamentals that today often seem endemic among, say, psychologists and sociologists."

Psychologists often cannot agree on what should count as a psychological explanation. J.A. Deutsch (quoted by Boden, 1977, p.1) expresses the problem this way:

"There is no accord among psychologists about what the facts they have accumulated are evidence for. This does not mean that they are merely in disagreement about the edifice they wish to erect; they have not even decided what constitutes a building. That is, not only do they disagree about the explanation of their findings, but they are not clear about what it would be to explain them."

Clearly there are special, recurrent barriers to the development of unified behavioural science. Certain factors, aided and abetted by the usual paradigm socialization processes, aggravate whatever fragmentary tendencies ordinarily exist in science.

At a crude level of analysis, several factors appear likely major causes instrumental in the evolution of man's current approach to the study of behaviour (complex inter-action between them is expected). The extreme complexity of the subject matter is undoubtedly implicated. There is the very youth of organized, systematic attempts to apply scientific methodology to the study of behaviour. The arguably premature rejection of training in philosophy (as a useful co-discipline to behavioural science study) and the often evident desire to unquestionably imitate the methodology of the 'pure sciences' are also considered to be important factors. Also relevant is the difficulty of deciding 'correct' focus, priorities, etc., reflected in the longstanding lack of consensus over proper realms, goals and methods of study. Lack of ready recourse to clear-cut evidence makes reconceptualization-provoking anomaly collection difficult; there is little in the way of mutually accepted material to base show-downs between conflicting formulations upon. These extreme difficulties confronting behavioural science result in defensiveness on the part of practitioners (or so it will be argued), and manifestations of this defensiveness perpetuate the fragmentation. Consequently discussion of these special problems requires a two-tiered attack : firstly identification of the underlying factors, secondly, discussion of perpetuation of the basic fragmented state by its consequences.

#### Factors implicated in the evolution of the current fragmented state.

##### (1) Fundamental discord

The extent and depth of basic discord in psychology should not be underestimated. There is little general consensus upon what psychology ought to be; consequently there is even less on the extent of progress made.

It is possible to make a very general conceptual three-way split. On one extreme are radical humanists; on the other radical (metaphysical) behaviourists. Somewhere in between fall cognitive theorists. Like any crude continuum-type classification, the above doesn't do justice to the multi-dimensional nature of differences in basic commitment, to the splintering within each category, or the innumerable positions of more-or-less eclectic clusters.

The extent of these basic differences underscores the humans-studying-humans nature of behavioural science. The inherent problems may be the greatest obstacle to the development of behavioural science; the problems of gaining any real degree of objectivity<sup>1</sup> at the base level in the study of human kind may be of a different order from those hindering objectivity in physical science. The central issue is not objectivity in research (although there are special problems here as well) but the sort of objectivity required to get outside fundamental and deeply ingrained assumptions and viewpoints about man, his essence and relationship to the universe, nurtured in the individual since birth. The fundamental base of science has been pre-empted by the behavioural theorist's very existence.<sup>2</sup>

Certainly there are mammoth differences between humanists and radical behaviourists at the most fundamental of levels. A valuable distinction can be made between humanism as a prescription for psychological study (set of starting points, basic assumptions about human kinds uniqueness, methodological prescripts), and humanism as a prescription for life, a

1. The quip "on which side are you neutral?" seems particularly salient in behavioural science practice. (Origin unknown).

2. Skinner is well aware of this basic problem. He writes (1973, pp.156-157): "Behaviour is a discouraging field because we are in such close contact with it. Early physicists, chemists, and biologists enjoyed a kind of natural protection against the complexity of their fields; they were untouched by vast ranged of relevant facts. They could select a few things for study and dismiss the rest of nature either as irrelevant or as obviously out of reach. If Gilbert or Faraday or Maxwell had had even a quick glimpse of what is now known about electricity, they would have had much more trouble in finding starting points and in formulating principles which did not seem 'oversimplified'....The behavioural scientist has had no such luck. He is all too aware of his own behaviour as part of his subject matter. Subtle perceptions, tricks of memory, the vagaries of dreams, the apparently intuitive solutions of problems - these and many other things about human behaviour insistently demand attention. It is much more difficult to find a starting point and to arrive at formulations which do not seem too simple."

religion of sorts. In the current analysis, the latter rules itself out of further consideration (although its disruptive influence on psychology as organized study cannot be ignored<sup>1.</sup>), while the former functions as a valuable corrective to the extremes of behaviourism (against which it should not be held as a stark alternative).

Most importantly for psychological science *qua* psychological science are the differences identifiable between those who clearly view themselves as either 'behaviourists' or 'cognitive theorists', a split that parallels the empiricism-rationalism schism of antiquity. The definitive difference here resides in respective answers to the question: "is observable behaviour to be the subject matter of psychology, or is it to be simply regarded as evidence of the operation of cognitive ('mental') processes which are themselves the proper subject matter? Is psychology the study of the mind, or of behaviour? Skinner clearly believes progress can only be made by rigorous examination of actual behaviour followed by precise formulation of overt relationships revealed. (It should be noted, however, that he has increasingly talked of rule-governed behaviour; see Skinner 1973, or his interview with Cohen, 1977). For Chomsky (1971, 1973) knowledge of a multiple of S-R relations provides no explanation for behaviour in any worthwhile sense; rather the role of psychological science is to develop schemes of internal structure, states, and organization of/<sup>the</sup>device that produces this set of input-output relations (Hilgard and Bower, 1975). Behaviour is interesting only insofar as it uncovers laws of the mind, these being only partially revealed by the behaviour in itself.

There seems to be no *a priori* case against the possibility (and desirability) of reconciliation, and time will probably see progress in that direction (for it really comes down to a question of theory versus 'objective' pragmatics; they are not inevitable opposites, and weaknesses of either extreme are, hopefully, increasingly apparent).

---

<sup>1.</sup> Ellis believes Maslow's 'third-force' was developed as a valuable complement to psychoanalysis and behaviourism. However, many followers subsequently "pushed pell mell into astrology, magic ... (into) all kinds of non-scientific and anti-scientific realms in their frantic need to push back the boundaries of the human mind... in the process they have practically thrown humanistic psychology back into the dark ages and have espoused all kinds of unverified and unverifiable clap-trap" (Ellis, 1973, p.2). Koch, critical though he is of views that unquestionably hold psychology to be a coherent science, echoes in kind: "the 'human potential' movement is a threat to human dignity ... it (has) a conception of human so gross as to make behaviourism seem a form of Victorian sentimentality" (1973, p.90).

However, it seems likely that, as with philosophy, progress in the form of increased consensus at this most basic of levels will be slow. The man-studying-man syndrome, it is suspected, will mean that there will always be fundamental dissent. Hopefully a middle ground of good sense and moderation will exist, but there will always be groups on the fringe who will choose to discount the fruits of organized and public study of human behaviour (this being more possible in human rather than physical sciences for reasons soon to be discussed).

The lack of fundamental consensus problem is many sided and pervasive, and automatically bears on judgements made of the degree and significance of progress achieved.

Those who are most likely to feel dramatic steps have been made in the understanding of human behaviour appear to belong to very closed, 'tight' schools, e.g., psychoanalysis, or behaviourism. The committed psychoanalyst has a fully self-supporting unsinkable theory, while the behaviourist's pragmatic approach in a limited sphere has resulted in the accumulation of considerable knowledge, the value of which is often subject to debate. Members of each school appear to be quite content that they have made great progress and believe they will continue to do so (Skinner: 1973, comment that Aristotle could not have understood a page of modern physics, but Socrates would have little trouble in following most current discussions of human affairs, is an exclamation of frustration at those who refuse to accept the validity of his approach, not an admission of personal failure). These two schools are of course the most clear-cut examples of paradigms in Kuhn's sense; getting on with the job overrides any basic doubts.

Others are less content, and two categories are discernable. The first reflects greatest discontent; a feeling of hopelessness and despair rooted in the belief that virtually no progress has been made (see Levitt, 1971, p.219 ; Hudson in interview with Cohen, 1977). The second holds that some progress has been made, but it is of little consequence - progress, it is claimed, has resulted from looking where the light is rather than in the more murky areas where more meaningful relationships may lie. A variant on this position sees 'revolutions' and paradigm shifts in the history of psychology as indicative of negative rather than positive pressures - Koch (1973, p.84) for example argues

"the various phases of behaviourism succeeded each other not by virtue of differential productivity, but rather because of the dawning recognition that significant problems and segments of subject matter were being evaded."

Still another variant resides in the deploring of pre-occupation with trivia (see for example Cohen's interviews with Festinger, McClelland, and

Eysenck), a common activity that raises one fundamental question : are the various critics denouncing the same type of work? It seems unlikely, as there appears to be little concord on what is significant, or what factors accumulated are evidence for (Boden, 1972). The whole area of judgement of progress made (not simply a coffee-table discussion, as judgements of progress determine prescriptions for future action, e.g. on the basis of "what has worked") is a difficult one. Berelson and Steiner (1964) after conducting a massive review of research findings failed to agree between themselves on the issue - one felt significant steps had been made, the other not (despite their collaboration - a strong comment on the psychological nature of psychology!). The difficulties involved are demonstrated by Beloff - after writing "the history of psychology ... has been more a succession of shifting viewpoints than a series of revelations" (p.20) he later proclaims, after commenting upon Milgram's fascinating experiments on obedience, "so much for those who say that psychological experiments never demonstrate anything we do not know already" (Beliff, 1973, p.229).

The ongoing identity crisis of psychology has been productive insofar as it has promoted debate upon what psychology ought to be, what it can be, and what it has most profitably been. This has been reflected in the large number of 'meta' psychology books produced since the 1960's. Hopefully open debate will increase awareness of what other approaches are attempting to do and have done; after a period of polemics some links may be forged capitalizing on respective strengths of various approaches. As far as this whole question goes, apart from the difficulty of objectively assessing advances made (so much is taken for granted by occupants of any profession), it must be argued that more meaningful questions to ask are : how adequate is our knowledge in this area? what should be done? what sort of base exists for us to work from in this area?

## (2) Complexity

Problems of fundamental discord are both indicative of and intensified by the sheer complexity of behaviour, and the immensity of the task of studying human existence. The overwhelming complexities involved are increasingly recognized, irrespective of basic beliefs held on the correct

way to confront them.<sup>1</sup> Relationships once believed to be straightforward are now recognised as far more subtle; this is occurring across the board as the following quotes indicate:

"Research on nest building in canaries shows the complex interactions between hormones, physiological changes, behavioural changes, and outside stimuli that affect even those apparently simple behaviour - everything in behaviour is related to almost everything else..." (Tinbergen, interview with Hall, 1974).

"In experimental and physiological psychology laboratories, it is increasingly realized that if only one response is measured, in the presence of one stimulus condition alone, one may observe nothing more than a basic decrease in response rate; but as the numbers of stimuli and responses studied in relation to each other increases, the richness of even the simplest behaviour process becomes evident" (Nevin, 1973, p.402).

"Behaviour whose roots are simple and determinate has nevertheless issued, even in the rat, into something much more complex and self-regulating... there is no warrant for treating human beings as bundles of isolated responses, when greater levels of organization and complexity are found in animals less elaborate than ourselves" (Broadbent, 1964, pp.202-203).

"There is considerable evidence amassing that there is a lack of simple causal associations between child-rearing practices or parental attitudes and such variables as aggression, anxiety, or authoritarianism" (Vernon, 1972).

<sup>1</sup>. Wilson (a sociobiologist) and Skinner are very much opposed in their basic and methodological assumptions - yet compare their statements:

"The subjects we are talking about are more difficult than physics or chemistry, by at least two orders of magnitude" (Wilson, 1975, p.574).

"Human behaviour is at least as difficult a subject matter as the chemistry of organic matter or the structure of the atom" (Skinner, 1953, p.42).

These two statements are particularly interesting as few behavioural scientists have been as strenuously attacked for simplifying human behaviour as these two.

"There is increasing suspicion of the value of premature and often disappointing attempts to reduce the complexities of human social behaviour and experience to 'simpler' or 'elementary' laws of functioning." (Tajfel, 1977, p.582).

The extreme complexity of the subject matter especially with reference to human behaviour has prompted some to point out the possible inherent limitations imposed by the situation where, ultimately, man is trying to understand himself, expressed eloquently by Andreski (1974, p.19) "whereas the mind might be able to make a perfect model of things simpler than itself, its ability to work out models of objects which are equally or more complex must be subject to severe limitations." Elsewhere he argues psychology is perhaps the most difficult of all sciences (natural or social), as it involves man's attempts to "lift himself by his bootstraps, using the mind to understand the mind". Consequently significant discoveries are rare, and must remain "exceedingly approximate and tentative" (p.25), as "the mind is scarcely provided with the means of grappling with a reality which is not only staggeringly complex but also fuid, elusive, and opaque - a reality which can be apprehended only with the aid of abstractions, which are themselves so indirectly based on sense perception that they are always slipping into the realm of pure fantasy" (p.60).

Chomsky, discussing his 'principles of the mind' argues in similar fashion:

"These biological structures enable us to construct extremely rich, very penetrating systems, scientific theories if you like. Some of them are common sense. Some of them are articulated, which allows us to understand things rather deeply far beyond any evidence that's available to us. However, these same principles which give such enormous range to our system of understanding also limit its scope. These two facts are very closely linked together. Any sort of principles that enable you to construct a rich theory on the basis of limited data, also is likely to limit the class of possible theories that you can attain. Now it may very well be that among the theories we are able to attain by our biological endowment there is included the theory of mind, or it may be among the theories that we are not able to attain is included the theory of mind ... we are biological organisms with fixed capacities that provide both the range and ultimately, the limit of our understanding." (Chomsky in interview with Cohen, 1977, pp.98-99).

These arguments do not imply attempts to formally understand behaviour are futile. They do however, underline the self-defeating nature of attempts to explain all with a handful of principles, rigidly maintained in the face of competition from competing schemes. Hopefully, increased awareness of complexity means that the systems-building of the 1920's - 1950's has well and truly been left behind.<sup>1</sup>

Such a hope is probably prematurely optimistic, if not eternally so. It may be that the creation of, and dogmatic clinging to, simplistic models is a reaction to that complexity. The behaviour of behavioural scientists, faced with the immensity of analysing the intricacies of behaviour, may well in itself say something about the human creature. (A comparison of statements from proponents of competing paradigms on this issue would be a fascinating exercise!) Perhaps entrenching oneself into a set-way of viewing behavioural science problems is not so much a reaction to behavioural complexity *per se* as a defense against the realization that human wit has generated so many schemes in response to the innumerable and nebulous problems of behaviour that doing justice to consideration of them all would require constant flexibility and hard work.

Pointing to complexity may foster the seeking of complementarities between schemes (on the assumption they may all be partially right, or worthwhile in different ways) in order that new formulations with increased power may be developed. But will this ever be enough? Perhaps the ultimate complexity of behaviour will always make mischief for behavioural science debate. Take for example the forever on-going controversy of the value of the I.Q. Bereiter, discussing absolute rejection of the value of I.Q. as a source of information or as an operationalized construct writes:

"It can be shown that IQ accounts for only about 12% of the variance in income and indeed seldom accounts for more than 25% of the variance in anything, other than performance on closely related tests. The implication is that IQ is all-around a pretty trivial variable.

What we have here, however, is a completely subjective judgment masquerading as a logical implication. Who is to say whether a variable that accounts for 10% of the variance in a criterion is trivial or important? Consider longevity as a criterion. It is doubtful whether any variable account for more than 10% of the variance in how long people live. Does it follow, then, that

---

<sup>1</sup> Various attempts were made during this period to generate the theory of learning, which was to be (in the logic of the time - and of today for some, for that matter) the theory of behaviour.

things like nutrition, sanitation, medical care, and heredity are unimportant? No, it is precisely because they and a number of other variables are each in its own right of great importance that no single one of them can account for a very large share of the variance. And so it may be with criteria like job performance and income. Everyone knows that it takes all kinds of virtues to make an exemplary performance. Everyone knows that economic success is a chancy business and depends on a host of factors. And so it would run against common sense if IQ or any other single variable were found to account for most of the variance in these criteria. In short, variance-accounted-for is not a very decisive measure of the importance of a variable. It is too much influenced by competing variables." (Bereiter, 1976, pp.39-40).

Furthermore it must be recognized that when an individual rejects the value of the I.Q., a void is not created in his explanatory conceptualizations. Something else fills its place, some other criterion or (probably imaginary) factor is deemed important. This eventuality creates an ideal basis for irresolvable polemics. (Bereiter actually found a significant number of post-graduate students where prepared to dismiss the I.Q. as having no value whatsoever. Ideology clearly plays havoc for science in the analysis of human behaviour - hopefully the irony of this will not be lost forever on behavioural scientists).

### (3) Imitation of the pure sciences, and splits over methodology

Kelly (1955) identified three major methods of devising testable hypotheses. Firstly there is the *hypothetico-deductive*. A theory is stated, hypotheses deduced, and experiments conducted to test the hypotheses. Secondly there is the *hypothetico-inductive*. Theory is developed from generalizations based upon observations. Thirdly there is the *statistical approach*. Factor-analysis is relied upon to pin-point meaningful relationships.

All these approaches are of value - but used appropriately. Schools or general research programmes frequently adopt one approach (this becoming the scientific method in psychological science) and ignore the others. Not only does this make the forging of links across schools difficult, but also heavy reliance on any one particular methodology carries with it severe drawbacks. The *hypothetico-deductive* tends to result in rigidity, and restricted findings that may reflect the suitability of material for precise laboratory testing rather than its ultimate significance or relevance. Relied upon exclusively, the *hypothetico-inductive*, while conducive to good broad observation of relationships that may be highly significant, tends to result in the development of sweeping conclusions difficult to test (and when roughly testable, not necessarily rejected or significantly modified on production of unfavourable outcomes). Kelly facetiously labelled the factor-analytic (third) approach the statistical dragnet. It has also been referred to as the statistical shot-gun. While

quick and reasonably objective analysis of ideas and observations are possible, factor-analysis can lead to sterility as far as idea production is concerned, and may lead to the assumption that "the greatest volume defines the greatest truth" (Pervin, 1970, p.33).

The proclivity of users of one approach to see only the weaknesses in the others has two major negative outcomes : (a), polemics result that obscure the possibility of convergence and recognition of complementarities, and (b), the weaknesses inherent in over-reliance upon one approach has undermined progress in any specific field, and thereby, progress in general. (Advances of a genuine and general kind would tend to produce unification momentum).

Reliance upon statistical analysis has frequently come under fire. It has been suggested the approach has contributed more to the development of statistical techniques than it has to the understanding of human functioning (e.g. by Hudson, 1967). Hudson argues the level of statistical sophistication and manipulation striven for within psychology is inappropriate in an exploratory science, where good basic idea production, and simple analysis, would be most conducive to the establishment of a sound general base.<sup>1</sup> (His fear is that petty sophistication, statistically speaking, prevents any general progress being made).

The use of statistical techniques in personality research has arguably been to some degree self-defeating. An examination of what the personality theorist often does yields a rather different picture to that conjured by his espoused interests : far from being concerned with the unique person or the study of the individual, his tasks have centred on sorting and testing procedures that group and arrange people either in accordance with prevailing typologies or by statistical distribution of particular traits. This is of course frequently valuable and necessary work; problems emerge from delusions inherent. Personality theory, to fully justify that title, should lead to understanding of the integrating forces within each individual

---

1. The use of sophisticated and esoteric statistical techniques to analyse results of questionnaires and tests of rustic (if not absurd) simplicity has been frequently mocked (see e.g. Anastasi, 1967; Cronbach, 1970; Heim, 1970; Hudson, 1967). Eysenck responds to such criticism by arguing most statistical procedures used could be understood by bright sixth-form boys, and Cattell (1972, p.363) asserts that in the effective employment of concepts from factor analysis the chief problem is not any lack of precision or potency in the approach itself, but "the fact that only about one-tenth of doctoral level psychologists are trained in factor analysis", and "the remaining ninety-percent develop imaginary criticisms against the unknown." No doubt the real issue revolves around flexibility, priorities, and balance (in means-ends terms).

life. That which can be said about large number of people is often rather trivial. Integration of a conceptual kind should not be disrupted by over-reliance upon 'truth' falling out of broad statistical approaches.

It is interesting that some of the most influential theory has been based upon work with small samples - Freud, Piaget, Skinner are obvious exemplars of this kind of approach. (Perhaps this is why Skinner often points to the same phenomena as Freud did, even if his assumptions of the processes and mechanisms involved are dramatically different. See Skinner 1953, and his interview with Cohen, 1977 within which he makes it clear he believes Freud contributed a great deal. He believes Eysenck, who employs broad statistical techniques, has been too harsh in his attitudes to Freudian theory). Particularly with Piaget and Skinner, there has been an interesting relationship between on-going feedback from immediate observation and the ultimate development of principles of broad applicability. The thinking, tinkering, modifying researcher has been an essential component in the research project.

There may be a common tendency in human affairs to displace value from ends to means. A behavioural scientist obsessed with jargon and techniques resembles, in Andreski's (1974, p.116) eyes, "a carpenter who becomes so worried about keeping his tools clean he has no time to cut the wood."<sup>1</sup> These tendencies are no doubt accentuated by feelings of helplessness in the face of seemingly unmanageable complexity (and possibly caution, as there are many highly controversial issues in the behavioural science realm). Alternatively, preoccupation with methodology may sometimes reflect failure to recognise the complexity, concomitant with belief that meaningful and significant understanding will magically emerge from machine-like application of methodological technology.<sup>2</sup>

- 
1. Those who fear means have usurped ends all too often in psychological research and practice may find some of Andreski's other analogies entertaining. He writes:  
 "despite the ingenuity of their recipes, the exponents of the over-sophisticated quantitative methods of research remind me of old films with Laurel and Hardy or Charlie Chaplin, where you would see boxers flexing their muscles, making energetic knee-bends, fierce faces and menacing gestures, and then waving their arms in the air without ever coming to blows ... methodological rigorists are like cooks who would show us all their shiny stoves, mixes, liquidizers and what-not, without ever making anything worth eating" (Andreski, 1974, p.121)
  2. Koch (1973, p.81) claims " a meaningful thinking" pervades modern scholarship. " A meaningful thought or inquiry regards knowledge as the result of processing rather than discovery. It presumes that knowledge is an almost automatic result of gimmickry, an assembly line, a methodology."

While obviously the development of appropriate methodological techniques is important, over-emphasis may occur at the expense of the development of good ideas, flexibility, and innovation. Practitioners must also learn how to theorize, see relationships between superficially diverse data, know the right questions to ask and so on. The multitude of what are often seemingly disparate pieces of information that fall out of statistical analyses should not entrench practitioners further into established positions through selective acknowledgement; rather ways must be developed to fit them all together in useful ways. (That psychology hasn't been at all successful in this is argued by McClelland in his interview with Cohen, 1977).

Methodological expertise must be developed *in conjunction* with tolerance of ambiguity and flexibility. Sontag (1973) claims that the Fels Research Institute have a strong policy of recognising the problem as the important thing - anything goes methodologically-wise if there is any possibility of light being shed. Hopefully, such an approach is (or will become) the norm rather than exception.

Differences of opinion are also evident between proponents of 'pure' and 'applied' research. Laboratory experiments are increasingly criticised as artificial (Argyle, 1975a). Beloff (1973) suggests that what emerges from a standard social-psychology experiment is a fact about a particular interpersonal encounter rather than a fact about the subject's behaviour as such, and argues the more subtle the behaviour under investigation, the greater the distortion. It must be recognised however, that the experimental situation is still real, with real outcomes - the problem is one of interpretation.

Obviously laboratory experiments with human subjects have their place. Naive belief that (par with physical science) laboratory research will generate the worthwhile psychology in itself is of concern. As Argyle contends, discovery occurs very often as a result of informal and not particularly well controlled research.

Broadbent (1977, p.31) writes: "In contrast to the physical sciences, applied psychology can only rarely make use of generalizations established by theoretical work ... the practical situation may often reveal new and unsuspected aspects of human nature, the understanding of which is a gain to general knowledge as well as to the solution of the particular problem."

Elsewhere (in interview with Cohen, 1977) he elaborates upon the dangers of reliance upon laboratory experiments with a personal example. In his earlier years he conducted experiments on noise. Laboratory experiments indicated noise had no real effect on task performance. Industrial and naval experience suggested otherwise - but experimental psychologists dismissed this as the results of suggestion. After a number of years of applied work, Broadbent was able to demonstrate, under more natural conditions and with more complex tasks than usual lab experiments employed, these were subtle but significant relationships between noise level and performance.

Applied research, then, may help keep psychology honest (see Cohen's interviews with Broadbent, Hudson, and McClelland on this issue). This appreciation must be paralleled, however, by recognition of ethical issues implicit in research that manipulates people. In the field of behavioural modification, it is frequently argued that applied work is research and practice at one and the same time (e.g. Kazdin, 1973; 1975). Premack (undated tape) warns of the moral issues involved, and argues we need to learn a lot more about the basic human state before we go out and offer to manipulate behaviour on behalf of some institution or social system, the essential nature of which may be unsuited to man. Conducting applied research in situations where the most significant results are likely and avoiding violating ethical standards at the same time is not going to be easy - but then, human psychology never was going to be.

Frequently the ills, lack of progress, and splintered nature of psychology are laid at the door of deliberate attempts to imitate the approaches and methodologies of the pure sciences. Koch argues that perception of psychology as a science of the same order and nature as physical sciences is "to persist in applying a highly charged metaphor" which "shackles (psychological) fields with highly unrealistic expectations" (Koch, 1973, p.87). A common fear expressed is that in attempting to be adequate to science, psychology has become inadequate to man.

Attempts to study behaviour and 'mind' became more deliberate and formalized at a time when the natural sciences were in their heyday. Not surprisingly, psychology was modelled on natural science. Confrontation with the complexity of the order involved in the 'making sense' of behaviour (leading to the grasping of methodologies already in existence), and the

possible desire to rapidly acquire status may well have been major factors involved. Apart from difficulties that have arisen from applying borrowed techniques to a dramatically different subject matter, and through modelling on a misunderstood ideal that appears likely to be very different from that which actually happens in the pure sciences (Heim, 1970; Hudson, 1967) imitation has resulted in a premature and overriding tendency to quantify and classify. Heim holds this responsible for the compartmentalization and the dichotomization of "what are in fact continua", suggesting that psychology has been pushed "into running before it can walk, with the predictable result that it sometimes stumbles and sometimes goes backwards" (Heim, 1970, p.15).

Hudson (1967, and in interview with Cohen, 1977) argues imitation of a hypothetical model has been disastrous, and a case of academic insecurity. In this he is supported by discussion of a specific facet of imitation, operationalism, on the part of Travers (1971, p.352). Travers writes of the:

"intellectual sterility produced by rigidity of beliefs concerning the applicability of Bridgman operationalism to the development of a language of psychology when the fact is that it could never be applied to Bridgman's own area, namely, physics. The core terms were never introduced through this kind of hocus-pocus at all."

Elsewhere Travers elaborates that in 1930's operationalism offered the promise of bringing order into the confused and confusing vocabulary of behavioural science. However, in the form in which it was applied it failed because

(a), "of the implication that seemed to go with the procedure that the mere operational definition of a term gave it scientific standing... behavioural scientists were slow to realize a well defined term is not necessarily a good scientific term" and (b), "behavioural scientists failed to recognise that statements about phenomena of any consequence cannot generally be restricted to operationally defined terms which by their very nature represent gross categories far too crude for most scientific uses."

A comment on the eagerness of behavioural scientists *to be considered scientists* in the traditional sense, and on the naivete of their interpretation of 'science', is provided by consideration of reactions to parapsychology. The most militant opposition comes from psychologists; on the other hand, numerous eminent physical scientists have been patrons. (see Beloff, 1973, for discussion). While one factor may be the interest

of psychologists in the psychological aspect of interest and belief in this area, and their knowledge of human vulnerability to suggestion etc, the disturbing possibility remains that psychologists, less secure in their status as scientists, possess too rigid and simplistic a model of science. They may more readily construe parapsychology as a surreptitious attack on science, and an open invitation to mystification.

These rigid interpretations of science do not augur well for progress (due to inherent inflexibility) and unification (as what other schools do is obviously "not properly scientific"). Attitudes to and concepts of science therefore may be both cause and consequence of the fragmented, non-progress making state of psychology. Rigidity and misconceptions hinder the development of a broad, flexible psychology; this limits progress; this uncomfortable state results in greater attempts to gain the status and respectability of 'science'.

Gilbert (1970, p.2) sums up the problem nicely:

"by mimicking the methods and concepts of physics, we neglect the essential properties of animal life and, beyond that, the more complex properties of human life that have emerged in the course of millions of years of evolution."

Behavioural science has been extremely slow in developing methods appropriate to subject matter and ends. The use of film and associated technology has only recently gained prominence as a form of methodology in its own right. The value of film to settle long-standing controversies was demonstrated wayback-when, when Muybridge's sequence showed once and for all that a horse lifts all four feet from the ground when it gallops. Arguments that most behavioural science problems and subjects are of a different nature and order shouldn't distract from the possibilities implicit. Film provides an objective (within certain specifiable limits), permanent record of events unmodified by interpretations or report. (The vast majority of social science debates are not over interpretations of phenomena - they are over interpretations of reports). Certainly Mead (1968) sees merit in this approach with respect to the inability of members of different disciplines to jointly make sense of phenomena. Film takes things back to basics; it can be replayed, re-analyzed, discussed directly and so on in a manner that research reports cannot. The subtleties of one generation's insights may be less readily lost with this type of record (film, audio discussion etc).

Ethologists, with their traditional emphasis upon getting things right, right from basics in the form of comprehensive observation have employed filming techniques to good advantage (see Eibl-Eibesfeldt, 1970; Hass, 1970). The replaying of film at various speeds enables detection of patterns of behaviour that usually escape attention (a sort of "is the fish aware of water?" phenomenon).

That status has been a primary motive behind imitation of natural sciences, and a means of befuddling would-be critics, has been persuasively argued by Andreski. He demonstrates how certain psychological formulae (a), do not make sense in mathematical terms, and (b), could be stated just as simply (but much less ambiguously) in ordinary language. "The constant recourse to the letter 'in' helps to cash in on the prestige of mathematics", he argues (Andreski, 1974, p.71). But psychologists are not the only behavioural scientists to come under his critical eye; elsewhere he accuses Levi-Strauss of "threatening people with mathematics : muttering darkly about algebraic matrices and transformations without revealing their exact nature" (p.87).<sup>1</sup>

Feelings that behavioural sciences have been misconstrued as such (i.e. should be 'behavioural studies') have lead to the adoption of extreme anti-science views. As human behaviour seems so varied, delicately complex, and obscurely motivated many despair of finding valid generalizations to explain and predict human thoughts, actions, and feeling - despair at the very possibility of constructing a science of human behaviour.

But diametrically opposing 'meaning', 'understanding', and 'relevance' with 'science' is a totally illogical over-reaction to awareness that 'science' and 'scientific methodology' have been so construed within psychology that it has often failed to remain true to its subject matter. Those that assert, for example, that 'meaning' plays such a role in human experience, behaviour and action that a science of behaviour is not possible have forgotten Einstein's admonition that it is not the task of science to give us the taste of soup.

---

1. Andreski elsewhere complains about the "soul-destroying taboo against touching anything that cannot be quantified, and superstitious reverence for every scribbling that looks like mathematics" (p.145), and cites the situation in which a famous mathematician ended a debate over the existence of God by writing a complex formula, ending "therefore, God exists". No-one was prepared to argue further. The case may be over-stated, but the dissatisfaction is valid enough. As Poincare once said "The natural sciences talk about their results. The social sciences talk about their methods."

Furthermore, science need not be narrowly equated with the experimental method. All that is required to set up in business as science is (a) develop the capacity to generate testable hypotheses (b) devise means of establishing facts that go beyond the deliverances of unaided common sense or the traditional insights of humanistic studies, and (c) develop explanations with genuine predictive power. To quote Hebb's (1964a, p.14) quote of Bridgman: "The scientific method, as far as it is a method, is nothing more than doing one's damnest with one's mind, no hold's barred". Perhaps the proviso should be added that we still need methods of verification/falsification, but that these should be adequate to the complexity of phenomena involved. We need ways of testing alternative hypotheses; some way of retaining contact with the harsh stubbornness of data.

'Science' simply refers to certain prescriptions, criteria. As such it cannot be equated with intuition, empathy or whatever - but *it can employ these human characteristics*. We do not need to revert to generating poetry that masquerades as science in order to construct a thorough and systematic body of thought that is relevant - as Beloff (1973) concludes, "objections to psychological science should serve as a reminder of the audacity which the idea implies - note, the idea is audacious, not fallacious".

#### (4) Youth

The relative youth of the behavioural sciences is often cited as the reason for many problems associated with the study of behaviour, and to moderate claims of lack of progress. While consideration must be given to the effects of youth, the point has to be made that 'youth' as a general umbrella explanation for the shortcomings of the behavioural sciences may function as a scapegoat and so prevent the possible identification of specific problems/causes that may be perhaps rectified once recognized.

Piaget clearly believes youth is implicated in the fragmentation of behavioural science:

"it generally takes time to discover its main trends, because these are far from being conscious from the start ... and are discovered only by trial and error and often by exaggeration of initial theories" (Piaget, 1973b, p.13).

At best, awareness of the comparative youth of man's organized endeavours to understand behaviour, heightened by a respect for the complexity and immensity of the undertaking, should serve to make us even more aware of the dangers of solidifying boundaries and assumptions that keep apart the various approaches that have evolved. An academically liberal atmosphere should be striven for within which new, innovative (even creatively eclectic) approaches would be tolerated if not actively encouraged.

(5) Difficulty of proof : accountability and fashion

In psychology, propositions are not usually 'proven' or 'disproven'; evidence is found for and against. This highlights a major and special problem of behavioural science : it is extremely difficult to demonstrate the superiority of one paradigm over another. (This may account for the theological-like tone of much inter-school argument). Revolutionary reconceptualizations resulting in the rejection of one paradigm in favour of another come about through perception of significant anomalies relative to a particular paradigm. In psychology, it is often possible to 'not recognise' anomalies, e.g. question the scientific base of some embarrassing finding. Furthermore, some theories are virtually unsinkable; they have the capacity to predict (or post-dict) any eventuality (see Eysenck and Wilson, 1973, for discussion of Freudian theory in this light). As revolutionary reconceptualizations are essentially psychological affairs in themselves, this is a particularly pressing problem. For as Ellis (1973, p.29) has pointed out, some of the greatest scientists in history have had great difficulty in changing fixed ideas, even in the face of clear-cut evidence.

Another problem besetting behavioural science is that it has been pre-empted by non-science modes of thought, against which it is very difficult to prove the superiority of a systematic formulation. Everyone is a psychologist; however as other people don't literally explode or fall apart upon being treated inappropriately, the inadequacies of private psychological theories are seldom revealed as such to the possessor.

Behavioural science is inherently political, and problems of accountability emerge. History has shown how hard orthodoxies are to overthrow, even where evidence against conventions is overwhelming (recall the resistance to new medical methods proposed by Semmelweis, even when he had clear-cut evidence that his anti-septic methods saved lives). With

the sensitive nature of many psycho-social problems, it is very tempting for behavioural scientists to busy themselves with trivia, thereby keeping in good with the powers that be. Certainly there are few negative consequences for perpetual non-production of anything significant; this is difficult to demonstrate, anyway. Work that forces others to reconsider basic conceptualizations will on the other hand inevitably involve the practitioner in controversy.

Awareness of the outcomes of the current state has driven one critic to assert that, no less than in other realms of human activity, the academic world is vulnerable to swings of fashion and associated polemics (Fletcher, 1957). Mead (1968) has recounted in dismay the adoption of one particular methodology within behavioural science after another (at approximately 10 yearly intervals). This would be fine if it amounted to progression, but the "new approach" all too often isn't built upon the old one's strengths (see Mead, 1968, for further discussion). In similar fashion, Koch (1973) has argued that paradigm changes in psychology are not cumulative or progressive - large, sweeping generalizations are just simply replaced by a different set.

Emphasis upon 'critical thinking' in tertiary education is frequently a misnomer; it provides the ideal opportunity to reject theories, formulations and points of view without getting to understand them, and as such tends to result in greater, not less, support for the ruling *Zeitgeist*.

Outcomes of defensiveness : perpetuating factors in the current state.

Accepting that there are special problems in the conducting of behavioural science, and assuming that these will tend to produce a sort of academic insecurity in practitioners, then it would be expected this would be manifest in various forms of defensiveness.<sup>1</sup> Various noticeable trends in behavioural science activity may support this premise - and present an overall picture in which the basic problems are perpetuated by the nature of defensive reactions.

---

<sup>1</sup>. Lest I be considered to be implicitly favouring a particular paradigm (Freudian) in postulating 'defensiveness' as a causal process, I should like to point out that Skinner is well aware of the phenomenon referred to (see Skinner, 1953). Awareness of certain relationships does not commit one to any particular theory of mechanism.

The trends referred to are captured in Berelson and Steiners (1964, p.12) overview of behavioural science, formed after conducting a massive review of findings:

"(there is) too much precision misplaced on trivial matters... too much respect for insights that are common place, too much indication and too little proof, too little genuine cumulation of generalizations, too little regard for learning of the past, far too much jargon."

General indications of defensiveness, and consequences thereof

Beloff (1973, p.21) argues that all too often "psychologists have made their impact more by exaggerating to the point of paradox some unexceptional claim than disclosing some striking new fact". Cohen concluded after his extensive interviews with leading psychologists that "they seek, in effect, to parody the positions held by their opponents."

Such behaviour does smack of insecurity. Another example is provided by the intelligence-insulting "littleman inside" put-downs radical behaviourists use to discredit theorists prepared to consider factors other than readily indentifiable environmental ones in the behaviour of individuals.

It is almost as if psychologists go out of their way to simplify, and thus make manageable, the complexities and real differences in the behavioural sciences. The paradigm socialization processes previously discussed are magnified to the level of the grotesque. White (1973) has eloquently laid the blame for much inter-school rivalry and hostility on the failure to recognize differential focus of study as far as it validly exists. Reviewing the long standing maturation/learning controversy, he suggests that there is little evidence of major differences between James, Hall, McDougall, Watson, Thorndike, Hull and Skinner in their writings. They simply have been concerned with different aspects (dimensions, facets) of the total phenomenon of behaviour. Subsequent followers have failed to recognize delineation of concern of the key figures (the subtle difference in focus) and to see the essential complementarity of the various approaches. This is not to deny that overlap in focus and therefore areas of theoretical debate will exist, just that these may be less significant than generally believed. Another example is provided by Burghardt (1973b), who claims that at the turn of the century Thorndike made it clear that he thought of his works as fitting into the approach advocated by Whitman (the father of ethology after Darwin), and that his concern with learning (carried out on the individual level) complemented

the process performed by evolution at the species level. Yet for a period of fifty years the intellectual descendants of each man became increasingly isolated from, and in many respects intolerant of, the other.

Commenting on Kelly's cognitive theory of personality, Bruner (1970a, p.61) wrote:

"one gets the impression that the author is, in his personality theory, overreacting against a generation of irrationalism."

It may be argued that swings of the pendulum are essential to progress. However, pendulums don't progress - they go back-and-forth from one extreme to another. Imagine a chemist, upon discovering the strength of reaction resultant from some unusual combination of chemicals actually to be less than predicted by traditional wisdom, writing a paper in which he deliberately underplays the strength of explosion in order to make good his point - hard to, isn't it? Argument that such deliberate falsification is of a different nature to the exaggerations outlined simply serves to highlight the differences between physical and behavioural science.

In their endeavours to establish their perspective as *the science* of behaviour, behaviour theorists risk turning behavioural investigation into a game rather than an orderly scientific enterprise. (At the very least they provide entertainment for cynics, secure in the knowledge that they know all that is worthwhile knowing about behaviour).

#### Jargon and the encapsulation of logic

Apart from the problems created by the use of different terminologies for interdisciplinary commerce (see Hinde, 1975, for discussion of 'ritual' in this regard), jargon is of concern for three different reasons. It can serve to camouflage propoganda, or a simply paucity of ideas (Andreski, 1974). It can serve to distract from awareness that the important issue is the adequacy of the underlying ideas, not the 'scientific' nature of the terminology used (Hebb, 1964a). But most importantly, jargon may function as 'panchrestons' (Wilson 1975) and encapsulate the logic of a group of practitioners. A panchreston is a word or concept, covering a wide range of different phenomenon and loaded with different meaning for each user, a word that attempts to 'explain' everything but explains next to nothing. They function as banner words, to call together and unite the loyal, and to fend off recognition of that which is unexplained.

A subtle example is provided by Premack (1971, p.145), remarking on responses to novel findings emergent from his innovative work on reinforcement/punishment:

"There is a tendency to misunderstand the option which are presented by 'new phenomena' - a tendency reflected in the common remark "thats interesting, but its not so and so". However, interesting is not a category. Replicable outcomes cannot be left in limbo, denied membership in one class without some other class being provided for them."

Torrance (1977, p.548) clearly views the term 'role' as a panchreston:

"The main problem with the concept of role is its lack of constraint; it is a tearaway word which tends to carry all of human behaviour indiscriminately away with it."

#### The need for good theorizing

Wittgenstein once said that in psychology there are experimental methods and conceptual confusions. Leeper as early as 1948 argued that the chief immaturity of behavioural science may be in its theorizing rather than in insufficiency of factual knowledge. We need to develop the ability to see overall coherence, to see how each piece fits. In short, behavioural science needs a working perspective, a basis for priorities. In the babylonian confusion of languages that reigns in the sciences, that concern themselves with behaviour, it is easy to assume fragmentation of the order extant is an inevitable consequence of specialization in the face of complexity. There is however "a neutral system of reference awaiting full development - the development of life" (Hass, 1970).

SECTION TWOCONSIDERATION OF MAN'S BIOLOGICAL STATUS AS THE BASIS FOR A  
COHERENT AND PARSIMONIOUS APPROACH TO THE STUDY OF BEHAVIOUR

In the complex and problem-ridden realm of human behaviour analysis, man's basic status as an evolved, biological creature stands out as solid bedrock in the shifting sands of uncertainty. Behavioural science is obliged to at least consider the possible implications, for there are no *a priori* reasons for assuming there are no connections between phylogeny and current behaviour.

Dissatisfaction on the part of many prominent behaviour scientists with the splintered approach to the study of behaviour is paralleled by frequently expressed discontent with the inadequate awareness of the role of biological factors characteristic of much social science endeavour.

For example, the self-confessed former radical-behaviourist Mahoney (1974) decries a lack of exposure in training to some of the complex bio-social interactions which exist. From a developmental point of view, Nash (1970) claims to have found the environmentalism that he was teaching not actually wrong, but incomplete. He argues that the developmental literature lacks 'gestalt' - works tend to be "either/or" with regard to biological or social determinants. From the personality literature, Kugelmass notes that "man is a biosocial being with heredity and environment inseparably entwined in their influence on his behaviour, yet recent psychological research has failed to take into account the biological factors responsible for individual differences". (in Eysenck, 1967, p. viii). And while Hebb has always decried the use of 'heredity' as an explanation (he refers to it as "a ragbag for disposing of difficulties"), he does not follow the usual pattern of moving from that argument to an environmentalist pragmatic position, for he argues "one must understand both (constitution and experience) in order to understand either" (Hebb et al, 1973).

Again, this area reflects a failure of behavioural science to improve upon insights of long ago. Sinnott (1961, p.13) recalls to attention McDougall's admonition of decades ago:

"(we have) an intolerably absurd state of affairs obtaining; namely, two sciences of the functioning of organisms, on the one hand mechanical biology, on the other psychology; two sciences completely out of touch with one another; the one ignoring the mental life of men and animals, the other trying vainly to relate it intelligibly to the bodily life".

As always, complexity makes for mischief in human intellectual conduct. In the as yet little understood interweaving of cultural learning and biological programming we are prone to overemphasize one or the other. Traditionally the general trend in behavioural science has been to overemphasize the environmental, although its appears recognition that it is living, evolved organisms that do the behaving is gradually increasing. It is reassuring to read, in discussion of hypotheses about innate predispositions,:

"in an intellectual climate emphasizing the uniqueness of man, the endless plasticity of humans to shaping by their cultures, and the diversity and uniqueness of cultures, we have hardly been prepared to face such possibilities squarely. An emerging intellectual climate that stresses similarities between natural systems of all kinds, biological and social; that assumes that biological and psychocultural are ultimately two sides of the same coin; and that seeks universals rather than insisting on uniqueness and relativism, makes it possible and urgent to ask such questions, whatever the answers turn out to be" (Keesing and Keesing, 1971).

This statement comes from an anthropological text, which makes the extreme anti-biology views of Shalins and Reynolds (respectively) particularly interesting, as they argue, to quote Shalins, from the "vantage of what *culture* is" (Shalins, 1977, p.x). But apart from their views, the most religious-faith-like dismissal of functional biological explanations of behaviour come from sociologists:

"the battle line (anthropologists) abandoned is still vigorously manned by various sociologists and sociologically oriented social psychologist who explain *all* such individual differences on the basis of subcultural membership or situational variables, and steadfastly deny the existence of stable, enduring antecedent predispositions to behaviour and development" (Ausubel, 1971, p.31).

The 'nature of man' and sociological commitments

Bressler (1967) points out that sociology relies heavily on the master paradigm of structural functionalism which maintains virtually no commerce with biologically based theories of human behaviour. In his view no contemporary sociologist has published a major work that confronts recent developments in such fields as genetics, ethology, or even comparative psychology. Rather, standard treatment is to either (1) "ignore biogenic explanations and biomeliorative proposals", or (2) introduce them in a polemic context to discount their importance.

This orientation of contemporary sociological theory is surprising given that the founder of the discipline of sociology, Comte, stressed that social theory should be in "entire accordance biological laws", and that "social phenomena must always be founded on the necessary invariableness of the human organism". He advocated that general propositions of sociology be validated in part through systematic observation of animal societies (see Bressler, 1967, for full discussion). Comte anticipated the modern functional-biological revolution!

Wrong<sup>1</sup> (1961) asserts that sociologists frequently possess a one-sided view on human nature - he labels it "over-socialized man". Wrong asserts that it is essential to recognize that in the beginning there is a body (he fears sociologists instantly react to the spectre of biological determinism). (Mead, 1968, has also commented upon "the body being out of fashion" a long, long time - more a reflection of academic whim and fashion than good sense in her view). In Wrong's view, the fact that sexual drives and the quest for power have probably been over-estimated on occasions is no reason to deny their reality as motives. Inkeles (1959) has claimed that very little sociological analysis is ever done without using at least an implicit psychological theory. Wrong does not see how, at the level of theory, sociologists can fail to make assumptions about human nature.

---

<sup>1</sup>. Both Bressler and Wrong are professors of sociology, and so some reasonable familiarity with the sociological literature on their part is assumed.

He fears that if such assumptions are left implicit then sociologists will inevitably presuppose a view of man that is tailor made to their special needs. Some sociologists overtly indicate their psychological borrowings and bases. Blau makes use of Festingers notion of cognitive dissonance to help explain the "underlying process in the legitimation of authority". Elsewhere (in Grusky and Miller, 1970) he writes that "man needs to be socially reinforced and it is the desire for this that motivates most of his actions".

Most sociological theory can be conceived of as a response to the Hobbesian question of social orders - if men act in self-interest, how is social cohesion possible? (It is interesting to note Hobbes died in 1679! So much for the sociological concession to more recent models of 'natural' man). The two standard 'answers' provided by sociological theory constitute a model of human nature that he believes pervades modern sociology. They are, (1), that the solution to the problem of social order can be explained in terms of internalization of social norms, and (2), that man is essentially motivated by the desire to achieve a positive image of self by winning acceptance or status in the eyes of (all-important) others. (Homans calls it the "Social Mold theory" of human nature).

Certainly the dominant sociological paradigm - Parsonian structural - functionalism - reeks of this sort of model. Wrong has criticized Parson's solution to the Hobbesian problem by stating that "it has tended to become precisely the kind of elaboration of a set of answers in abstraction from questions that is so characteristic of contemporary sociological theory". To a modern sociologist imbued with the conception that action follows institutionalized patterns, opposition of individual and common interests has only a very limited relevance or is thoroughly unsound. The presence in man of motivational forces bucking against the hold social discipline has over him is denied.

Man, Wrong asserts, is increasingly seen by sociologists as a role-playing creature responding eagerly to the expectations of others in the group settings in which he finds himself. Cicourel (1973) has similarly argued that current social analyses confound the opportunity to develop insight into the subtle, contextual aspects of everyday social interaction, through the rigidity of set answers and the reification of associated terminology (e.g. 'social structure').

The 'explanation' of social control through 'internalization' constitutes the weakest aspect of the model.

Durkheim (Wrong asserts) in his later writings claimed that social rules enter directly into the constitution of the actors themselves - they do not merely regulate externally. Constraint becomes internal, psychological and self-imposed. Wrong asserts that Freud's theory has become the source and model for the conception of the internalization of social norms that today plays so important a part in sociological thinking. However, he fears that *the meaning of internalization has been subject to change as a result*. At worst, internalization has been equated with learning or habit formation in the simplest sense. The notions central to Freud's view - the stress of inner conflict, the tension between impulses super-ego controls the out comes of which cannot be pre-judged, have disappeared. Wrong claims that in psychoanalytic terms to say that a norm has been internalized or introjected to become part of the super-ego is to say no more than that a person will suffer guilt feelings if he fails to live up to it, *not that he will in fact live up to it in his behaviour*.

Wrong claims that confusion over the two "meanings" of the term socialization, is largely responsible for the "over-socialized" view of man he feels "social mold" theorists adhere to. The meanings are: (1) that associated with the transmission of culture, and (2) that associated with the process of 'becoming human', of acquiring uniquely human attributes from interaction with others. All men are socialized in this sense, but this doesn't mean that they have become completely molded by the particular norms and values of their culture. Wrong can be taken to mean here that the distinction between the meanings of the term socialization is a useful one, but that some theorists have taken the second meaning to a conceptually rigid extreme. (A neat distinction can be made between 'oversocialized man' - the creation entirely of society - and social man, the socialized animal). In this regard it is interesting to note that adult socialization has only recently excited scholarly attention (Van Maanen, 1976). Does this imply sociologists have assumed that after childhood the individual is sufficiently 'programmed' by 'internalization'? (Certainly this is compatible with the predominance of Freudian borrowings to 'explain' psychological processes).

At the very least structural-functionalist sociologists need reminding of Alfred North Whitehead's comment that "each human being is a more complex structure than any social system to which he belongs". The "social mold" nature of the ruling perspective results in a playing down of individual differences and tends to draw a picture of uni-directional processes which have uniform effects on a largely passive and socially created man.

Parsons does of course 'treat' individual difference. He has stressed that the personality is not just a mere epiphenomenon of the structure of society; it becomes "an independent system through the relations of its own organism and through the uniqueness of its life experiences". However, elsewhere (1961 p.152) he writes:

"Problems of social organization are not limited to occupational contexts, but apply to human relationships generally. The activism that has characterised American mastery of production applies equally to personal relationships...In extending the development of resources to a more organized and differentiated set of mechanisms for developing and regulating personality, the constellation of role relationships that constitute it<sup>1</sup> becomes, in the last analysis, the crucial link between character and society".

Again, elsewhere he adds that whereas from the social side, 'role' is the key concept, from the personality side, a corresponding concept of relational needs is used. (The need for love is given as an example). Needs create motivations. Is Parsons introducing a model of human nature based upon a set of needs through the back-door? No. He writes "the basic organization of the motivational system cannot be derived from instinctual sources, but must come from identifications and internalized objects". Freud's notion of internalization is useful; his id doesn't warrant serious attention.

But surely Parsons is guilty of gross inconsistency. When he refers to "the relations of its own organism" as something in some way separate from "the uniqueness of its life experiences" he is implying organismic based individual differences. If there are such differences (and who could seriously deny it?), and they are significant in developmental/behavioural terms, then we are entitled to assume the dimensions along which people vary are *human characteristics*, based upon elements of human nature.

---

1. I hope readers are secure in the knowledge that their personalities are simply constellations of role relationships.

Several relevant points arise from this discussion.

- (a) The generality with which the term 'socialization' is used allows a great deal to go unstated and unanalysed.
- (b) For the most part, sociologists have not even done psychology justice. Convenient borrowings from Freud, particularly in the form of a much modified 'internalization' conception underline this. What of the psychological knowledge about learning processes, the different types of learning, interaction thereof, situational determinants, individual differences in the developmental process etc. What are the *processes* of socialization? What do their processes tell us about human nature?
- (c) If psychology hasn't been done justice, sophisticated biological theory certainly hasn't.
- (d) The real danger exists that in social analysis, a sort of verbal spiralling may occur in which theorists may become entangled in an endless web of abstraction. Constant contact with basic assumptions, and links with that which is known at lower levels, must be maintained.<sup>1.</sup>

1. Sociological structural functionalism has come under increasing criticism in the last decade (although, being a water-tight answer-all, it is still influential). An early critic was Rex (1961, p. 75-76) who asked the following pertinent questions of it:
  1. "Is not the effect of functionalism's attempt to demarcate social and individual determinants of behaviour in a hard and fast way merely to produce an obscure definition of the social?"
  2. "Does not the true distinction between social and individual determinants of behaviour not lie in the fact that the former (social) is the *product* of social interaction? and is this concept closely akin to, although more complex than, the concepts in terms of which individual action is explained?"
  3. "Does not the attempt to exclude all discussion of purpose from sociological analysis result in the exclusion of an illuminating and legitimate source of data?"

Child (1964, p.267) presents a partial answer to the same basic question (i.e. the Hobbesian) but with assumptions diametrically opposed to those usually identifiable in sociological analysis:

"social integration in man could not have been primarily an intelligent, fully self-conscious integration, but was unquestionably, in large measure, instinctive. Indeed it is a pertinent question whether many of the more primitive and evanescent types of social integration among human being at the present day e.g. crowds, boys' gangs, etc are not to a considerable extent instinctive rather than intelligent".

Use of 'instinctive' immediately solicits cries of 'circular reasoning' and so forth in most behavioural science discourse (particularly social). Several points need to be made:

- (a) Reference to 'cultural factors' or 'socialization effects' very often amounts to vacuous circularity in itself.
- (b) The distinction (between intelligence and instinct) is a useful one insofar as it points to human constitutional characteristics of phylogenetic origin. The constitution of the body amounts to the end result of millions of years of tuning; the differences and interplay between the central nervous system and autonomic nervous system in human functioning are pointed to by the instinct/intelligence dichotomy. There are extremely complex interrelationships between the cerebral cortex, reticular activating system, and autonomic nervous system in on-going behaviour. While cortical control of behaviour is extremely important in humans, behaviour should be viewed as an extremely complex constellation of events, the control of which is provided by numerous interacting sources of information - social environmental input at any one time just being one. Another advantage of a crude instinct/intelligence dichotomy is that the self-regulating capacity of the behaving system is recognised along with the more fixed learnt and 'instinctual' components. *Tabula Rasa* sociological formulations appear to view cortical control as near total; the brain is an efficient computer programmed by cultural inculcation. This neither does justice to human 'intelligence' nor the role of lower processes, let alone the complex interplay between.

Conditioning, for example, obviously depends upon structural aspects of the human machine. It is indefensibly arbitrary to award any one source of information (directive; control) primacy *a priori* in behaviour causation, especially when this leads to assumptions of near total control. Each behaviour needs to be viewed independently in this regard.

The biological contribution : basics

Tiger and Fox (1974) argue that there has been a replacement of theoretical blindness concerning physical structure by a theoretical blindness about behaviour. Prior to Darwin, 'obvious' anatomical similarities (especially between apes and man) had simply not been noted; the same may now apply to behaviour. In similar vein, Hass (1970) argues that despite constant utilization of man's known physical continuity with the animal kingdom (e.g. application of knowledge of heredity from experiments with the fruitfly and propoise; discovery of functions of human muscles and nerve cells based upon experiments with frogs, fish, and rodents; injection of hormones from animal glands into man) the continuity of behaviour is not adequately recognized. He argues that the main questions faced in the social sciences are rooted in concept systems developed in an age when man's descent from the animal kingdom was still unknown - in Linden's (1976) words, the behavioural sciences developed in Platonic rather than Darwinian worlds. (This may explain the preoccupation of sociologists with Hobbes views on the basic human condition).

The major contributions to be made by the 'biological approach' to the understanding of behaviour come from the recently established disciplines of ethology and behavioural genetics, with the as-yet neonatal sociobiology a possible major force in the future. As focus is on ethology within the third major section of this dissertation, some brief outline of its basic stance and orientation is in order.

Ethology began as a type of behavioural study based within zoology. Emphasis is placed on the need to observe and describe the natural behaviour of as many species as possible, and to interpret behaviour as the result of evolution moulded by natural selection. Ethologists have emphasized the non-learned aspects of animal, and even human, behaviour, this in part being a reaction to a perceived over-emphasis in psychology on learning, and on the results of highly 'artificial' experiments performed on a limited range of species.

The greatest controversy exists around ethological interpretations of human behaviour. Many major points are recurrent in the literature of ethologists in the debate against extreme environmentalism in the explanation of human behaviour. A basic one as put by Tinbergen - "cultural or psychosocial evolution must not be allowed to obscure the effects of the genetic evolution which preceded it, and which still determines the direction, and the limitations, of human behaviour" (1977, p. 217).

Ethologists stress that man's biological nature has much to do with the kind of environment he creates, and the capacity for learning is a biological characteristic subject to the constraints of his physiology (Hass, 1970; Eibl-Eibesfeldt, 1970).

In developmental terms, the individual in maturity is the product not only of his history, but also of the history of his species, and to understand development fully human evolution must be taken into account. Nash (1970) writes of "feedback loops" and of the development of the child being organized around biological influences; of the environment interacting with the biological nature of the child.

It is sometimes argued that while consideration of basic biological forces may be relevant to the understanding of child development and behaviour, it will be of little value in the analysis of the behaviour of fully mature adults. A postulated social dominance drive, for example, may seem more credible when observing children's behaviour in gangs than when observing adult interaction. Writers of ethological bent are most reluctant to accept this objection simply *a priori*. Fletcher (1968) notes the range of traditional writers from many fields who have been persuaded that the mental experience of men in society, and the very nature of society itself, could not be understood without taking into account the 'instinctual' aspects of human existence - the "springs of human action" in McDougall's words. Adam Smith, Darwin, McDougall, Freud, Mill, Hobhouse, Westermack, Pareto - all fall within this category.

It is interesting to note in passing the paradox that some of the most influential theorists in psychology have been well aware of man's status as an evolved primate, yet the implications of this have not been exploited by the discipline as a whole.

Piaget, Bruner, Chomsky - all have been 'interactionist' viewing the child not as passive but as an active organism influential in its own development in line with species-characteristic patterns of complex, dynamic interaction between the structures of organism and environment. This involves a further paradox insofar as these theorists have been concerned with those aspects of human functioning used frequently, to deny the relevance of man's biological status and continuity with the animal kingdom in understanding his behaviour, i.e. the faculties of language and intellect.

Impetus to incorporation of functional biology into the behavioural/social sciences : some possible advantages

Much impetus to consider the behavioural implications of man's status as a biological entity and place in the natural order of things is provided by the great areas of mystery, the 'big' questions that discerning students of human functioning face after experiencing discomfort with glib and incomplete explanations of behaviour. Conventional social determinist 'explanations' often leave the present author with residual curiosity; it often seems that relevant superficialities have been identified and some simple relationships enounced, but there is *more to it* than they indicate. Behavioural science often seems to be tinkering rather than pushing for fundamental relationships. This has been captured eloquently by Tiger and Fox (1974) "no theory of behaviour amply and believably helps us satisfy our deep curiosity about ourselves".

Such dissatisfaction may lead to interest in what biology may contribute to understanding for several reasons. It will probably ultimately result in a concern with the question of what truly lies at the root of the differentness of man from other species. At a sophisticated level, this first of all involves comprehension of fundamental similarities - the continuity between man, his evolutionary history, and the rest of the animal kingdom. Such an overall view of man will highlight his uniqueness rather than undermine his dignity, with a net outcome of increased sophistication of understanding. Awareness of the vast discrepancy between man's achievements in a technological sense (his mastery over the physical environment) and his failure to make equally dramatic progress in understanding and controlling intraspecific behaviour leads to questioning of the tabula rasa assumption with regard to behaviour.

(If it were correct, then surely human behaviour would be readily controlled and more predictable?)<sup>1</sup>. Loosely linked to this point is awareness of the highly mysterious whims and moods which often prompt irrational conduct. As Beloff notes, "we do not need to swallow the more far-fetched ethological notions in order to recognize that certain objects will be intrinsically more attention-getting than others, some kinds of behaviour more readily acquired than others, and that, beneath the veneer of civilization, there exists within each of us certain ineradicable urges that we share with our simian ancestry" (1973, p. 86). Ellis has come to similar conclusions over a period of years of conducting psychotherapy, and acknowledges a debt to ethologists (Ellis, 1973).

Certain concurrent findings from conventional social science research tend to leave residual curiosity even after 'social' explanation. Groups tend to choose leaders taller than average. Despite what they claim, people perceive more positive personal attributes in physically attractive individuals (Bourne and Ekstrand, 1976). In the area of non-verbal communication, consistent sex differences fall out of the research (Rosenthal, 1974; Argyle, 1975). No doubt significant social determinants exist and have been argued for all of these findings. However, a feeling is often experienced that such explanations are not so much wrong as incomplete. As a follow-up of the references will show, researchers with solid conventional social science backgrounds (and in Rosenthal's case, reputation) are increasingly prepared to consider evolutionary factors in explaining the various phenomena.

Those who advocate greater consideration of biological factors often identify positive advantages overlooked by those more automatically alienated. For example in the analysis of animal studies, they point out the suggestive analogies, rich source of concepts, and rich source of hypotheses that such studies provide (see Bressler, 1967; Bliss, 1968).

---

<sup>1</sup>. Skinner (1973) of course argues that inappropriate behavioural analysis is the root of this problem. But behavioural therapists are increasingly aware of the problems of 'counter-control'; this has promoted interest in self-control strategies. In turn this has developed into something of a testing ground for the Skinnerian paradigm, with much heated debate, anomaly collection, ad hoc theoretical adjustments, and revolutionary reconceptualizations (see Goldiamond, 1976; Kanfer, 1973; Mahoney, 1974 and 1976; Premack and Anglin, 1973; and Thoresen and Wilbur, 1976 for a good cross-section of the issues and associated debate).

Such workers are not unaware of the dangers of generalizing findings to human behaviour, but appreciate that well conducted animal research may help focus attention on particular variables, and at the same time increase the range of investigative tools, rather than give definitive answers to the puzzles of human behaviour. In the area of methodology, Tinbergen has long asserted the contribution made (and being made) by ethology to other behavioural disciplines, and members of other disciplines are beginning to acknowledge this contribution (e.g. see Argyle, 1975a).

There is a growing belief evident within the social science literature that weaknesses recognized within the social sciences will be countered to some extent by increased contact between the social and biological sciences. The expression of such a belief has already been liberally referenced within this dissertation. This belief has in large part been generated by demonstration (on the part of initially just a few who were prepared to go against the established order of thinking and subsequently suffer heavy criticism) of the value of such a merger. Within psychology for example, the British behaviourist Broadbent has argued that excessive concentration on just one species (the rat) and a total neglect of innate behaviour resulted in blocks to the progress of knowledge - "it is hard to see how we could have waited forty years without an analysis of exploration had this not been so" (Broadbent, 1964, p. 181). These weaknesses, he goes on to claim, have been increasingly countered by the influence of zoologists, and he credits ethology with having changed attitudes within psychology to the concepts of 'drive' and 'reward'. In this regard, it is perhaps significant that Koch, for all his nihilistic conclusions about psychology as a science, suggests that in the areas of biological psychology and ethology "there are at least grounds for believing that particular findings are of permanent value" (Koch, 1973, p. 85). Cohen (1977, p.14) found only one common hope among his eminent behavioural scientist interviewees "everyone seems to think ethology should flourish and will come up with the answers to many problems".

Fundamentally, desire to broaden focus to include the biological stems from frustration with the failure of the social sciences to realistically consider the possibility of authentic human nature existing, a stance adopted by default rather than elaborately thought through. Social scientists were initially wary of naive circular explanations, have been educated within a framework of pre-Darwinian ideas about the fundamental status of man, and appear to have been sufficiently convinced of the validity of fragmented, compartmentalized approaches to ignore calls for broadened approaches based upon the establishment of a bio-grammar of human behaviour.

Consequently the tendency has been toward simply descriptive rather than explanatory work (see Osipow, 1973; Wilson, 1975). Argyris (1973, p156) has drawn attention to delusion implicit on this tendency; he writes :

"Crozier, though lacking an explicit model of man, was able to conclude that the inhumanity of organizations toward individuals was not confirmed by his data (how can one define inhumanity without a concept of man) yet in the same work, he stated that nervous tension arises from monotonous and repetitive work, that apathy and social isolation is great, and that work loads produce pressure".

Having made the point, Argyris stresses that a broad, comprehensive approach to the study of behaviour is essential - he writes that "meaningful *a priori* statements about the predispositions of human beings can only be developed by understanding the logic of their psycho-socio-genetic development".

#### Possible reasons for the neglect of the biological contribution

Factors involved in the failure of the social sciences to exploit biological knowledge are numerous. An obvious one relates directly to section one of this dissertation - specialization. Social scientists have become increasingly naive about biology, and biologists are often unaware of the complexities of behaviour, especially human (Tiger and Fox, 1974).

However, some diffusion across disciplinary boundaries does occur. Bressler (1967) for example notes that sociology has assimilated considerable knowledge from anthropology, economics, political science, psychology and social philosophy. Given this, the distinct isolation of the social sciences from biology requires further examination.

A strong aversion to 'reductionism' is no doubt implicated. This often leads to misinterpretation as to when the intellectual sin has actually been committed. Many in their zeal have failed to detect that the spectre of biological reductionism has been at least matched in crudity by the intellectual perversion of extreme social determinism in the analysis of behaviour.

It must however be conceded that much faulty theorizing of a generally biological nature has been exposed, especially well back in the past. For example, often the 'proof' for the existence of 'instincts' has rested on "gratuitous teleological assumptions" and "circular inferences from observed behaviour", and too often writers have failed to be "meticulous in distinguishing postulated innate tendencies from learned responses" (Bressler, 1967, p. 179).

However overreaction has led to the baby being thrown out with the bath water. With the rapid advance of evolutionary biology into the realms of the study of behaviour causation, the time has come for serious reconsideration of the constructs employed by those of biological training (represented most prominently by ethologists) in the development of their own increasingly sophisticated theories).

The legacy of academic repulsion from naive social Darwinism remains influential. Aversion to behavioural biology may be based upon the suspicion that explanations that begin with the organism inevitably lead to predatory ethics.

Even though relevant biological considerations have often been poorly represented, social scientists have failed to realize that moral implications are more complex and ambiguous than usually supposed. Liberal values are not necessarily threatened. Bressler (1967) has argued that contentions of poor chances of (1) altering an awkward behaviour pattern in terms of prevention before emergence, (2) change after emergence, or (3) channelling of it for productive purposes exist or are relevant according to the phenomenon being studied and are not exclusively identifiable with either the social or biological approach (Bressler, 1967). Furthermore, a comprehensive and accurate understanding of behaviour causation should be the goal of the behaviour scientist (if he is to justify that title) irrespective of what he believes it would be right or proper to discover.

It is an incredible vanity to believe one can predict with any great accuracy the long term consequences for human functioning of any particular finding (along any dimension - epistemological, behavioural, or moral).

Another likely major factor, perhaps even primary to all those considered so far, revolves around man's basic vanity and long standing assumptions about his place in (or, perhaps more appropriately, outside) the natural order of things. This was expounded as far back as 1896 by Thomas Huxley, who wrote of man's "strongly rooted prejudices regarding his position in nature" (Huxley, 1967, p.137). Social scientists thoroughly imbued with notions of the uniqueness and grandiosity of man, his wide ranging abilities and magnificent achievements, are not going to readily perceive the relevance of "animalistic" interpretations of human behaviour.

Consideration of biological factors principally via ethology appears to be on the increase within the social sciences. Nash (1970) has noted the increasing number of specialists interested in biological approaches to understanding psychology, while Dilger (1968) has pointed to an ever-increasing tendency for psychologists and ethologists to co-operate in their investigations. Bowlby (1969) and Storr (1968) have attempted to synthesis psychoanalysis and ethology. Goffman (1969, 1971) uses an ethological extension in sociological writings. Callan (1970) considers mutual relationships between anthropology and ethology. Tiger and Fox (1974) have attempted an integration of evolutionary and ethological thinking with all social sciences. While there were earlier works (e.g. Fletcher, 1958; Russell and Russell, 1961) it is interesting to note the recentness of much of the literature with this orientation. Perhaps with the earlier mentioned explosion of 'meta' books ("what should the social sciences especially psychology be? what are proper goals?") there has been a parallel expansion of interest in developing a broader orientation, one that takes cognizance of man's basic biological status. Maybe rather than experiencing the final demise of "the general theory" (Wiggins, 1976) we are witnessing the beginning of a new era.

- Anastasi, A. Psychology, psychologists and psychological testing. American Psychologist, 1967, 22, 297-396
- Andreski, S. Social Science as sorcery. Harmondsworth, Middlesex : Penguin 1974.
- Andrew, R.F.. The information potentially available in mammal displays. In R.A. Hinde (Ed.) Non-verbal communication. London : Cambridge University Press, 1975.
- Argyle, M. (Ed.). Social encounters. Harmondsworth, Middlesex : Penguin, 1973.
- Argyle, M. Bodily communication. London : Methuen, 1975a
- Argyle, M. Non-verbal communication in human social interaction. In R.A. Hinde (Ed.) Non-verbal communication. London : Cambridge University Press, 1975b.
- Argyle, M. and Dean, J. Eye contact, distance, and affiliation. In M. Argyle (Ed.) Social encounters. Harmondsworth, Middlesex : Penguin, 1973.
- Argyris, C. Personality and organisation theory revisited. Administrative Science Quarterly, 1973, 18, 141-167.
- Arndt, W.B. Theories of personality. New York : Macmillan, 1974.
- Ausubel, D. Historical overview of theoretical trends. In J Eliot (Ed.) Human development and cognitive processes. N.Y. : Holt, Linehart and Winston, 1971.
- Azrin, N. Behaviour modification. A taped discussion with D Premack. N. Hollywood, Calif.: Center for Cassetts Studies, undated.
- Baer, D.M., Wolf, M.M. and Risley, T.R. Some current dimensions of applied behaviour. Journal of Applied Behaviour Analysis 1968, 1, 91-97.
- Bandura, A. Vicarious and self-reinforcement processes. In R. Glaser (Ed.), The nature of reinforcement. New York : Academic Press, 1971
- Bankart, B.B. and Bankart, P.C. Operant conditioning. In Psychology Encyclopedia. Guilford, Connecticut : Dushkin, 1973.
- Bateson, P.P.G. Rules and reciprocity in behavioural development. In P.G. Bateson and R.A. Hinde (Eds.) Growing points in ethology. Cambridge : Cambridge Univ. Press, 1976.
- Beach, D.R. and sokoloff, M.J. Spatially dominated non-verbal communication of children : a methodological study. Perceptual and Motor Skills, 1974, 38, 1303-1310.
- Beer, S. Cybernetics. The fontana dictionary of modern thought. London : Fontana, 1977, p.151.

- Beloff, J. Psychological sciences : a review of modern psychology London : Crosby Lockwood Staples, 1973.
- Bereiter, C. IQ and elitism. Interchange, 1976-77, 7(3), 36-45.
- Berelson, B., and Steiner, G.A. Human behaviour : An inventory of scientific findings. New York : Harcourt, Brace and World, 1964.
- Bijou, S.W. Ages, stages, and the naturalisation of human development. In D.C. Charles and W.R. Looft (Eds.). Readings in Psychological development through life. New York : Holt, Rinehart, and Winston, 1973.
- Bijou, S.W. and Ribes-Inesta, E. (Eds.). Behaviour modification : Issues and extensions. New York : Academic Press, 1972.
- Birdwhistell, R.L. Kinesics. In M. Argyle (Ed.) Social encounters. Harmondsworth, Middlesex : Penguin, 1973.
- Blatt, S.J. Patterns of cardiac arousal during complex mental activity. In Harper et al (Eds.) The cognitive processes : Readings Englewood Cliffs, N.J. : Prentice-Hall, 1964.
- Bliss, E.L. (Ed.) Roots of behaviour. New York : Hafner, 1968.
- Blurton Jones, N.G. Non-verbal communication in children. In R.A. Hinde (Ed.) Non-verbal communication. London : Cambridge University Press, 1975.
- Blurton Jones, N.G. Growing points in human ethology : another link between ethology and the social sciences? In Bateson, P.P.G. and Hinde, R.A. (Eds.) Growing points in ethology. Cambridge : Cambridge Univ. Press, 1976.
- Boden, M.A. Purposive explanation in psychology. Massachusetts : Harvard University Press, 1972.
- Breger, L. From instinct to identity. Englewood cliffs, N.J. : Prentice-Hall, 1974.
- Bressler, M. Sociology, biology, and ideology. In Glass, D.C. (Ed.) Genetics. New York : Rockefeller Univ. Press and Russel Sage Foundation, 1968.
- Brinckerhoff, R. Freudianism, behaviourism and humanism. In F.W. Matson (Ed.) Without/within : behaviorism and humanism. Monterey, Calif. : Brooks/Cole, 1973.
- Broadbent, D.E. Applied psychology. The Fontana dictionary of modern thought. London : Fontana, 1977
- Broadbent, D.E. Behaviour. London : Methuen, 1964.
- Broadhurst, P.L. & Wilcock, J. Instinct. In H.J. Eysenck, W. Arnold, and R Meili Encyclopedia of psychology. (Vol.2). London : Search Press, 1972.

- Bruner, J.S. A cognitive theory of personality. In J.C. Mancuso (Ed.) Readings for a cognitive theory of personality. N.Y. : Holt, Rinehart and Winston, 1970 (a)
- Bruner, J.J. Bad education - a conversation with Jerome Bruner and Elizabeth Hall. Psychology Today, 1970, Dec., 50-57.
- Buck, R. Sex, personality, and physiological variables in the communication of affect via facial expression. J of Personality and Social Psychology, 1974, 30, 587-596.
- Budd, W.C. Behaviour modification : The scientific way to self control New York : Libra, 1973.
- Burghardt, G.M. Ethology. In Psychology Encyclopedia. Guilford, Connecticut : Dushkin, 1973(a).
- Burghardt, G.M. Instinct and innate behaviour : toward an ethological psychology. In J A Nevin (Ed.) The Study of Behaviour. Glenview, Ill. : Scott, Foresman, 1973 (b).
- Catania, A.C. Elicitation, reinforcement, and stimulus control : In R. Glaser (Ed.) The nature of reinforcement. N.Y. : Academic Press, 1971.
- Cattell, R.B. Factor analysis. In H J Eysenck, W. Arnold, and R. Meili (Eds.) Encyclopedia of Psychology. London : Search Press, 1972, pp 358-363.
- Chapple, E.D. Culture and biological man. New York : Holt, Rinehart and Winston, 1970.
- Chance, M.R.A. and Jolly, J.C. Social groups of monkeys, apes, and men. London : Cape, 1970.
- Charles, D.C., and Looft, W.R. (Eds.) Readings in psychological development through life. New York : Holt, Rinehart and Winston, 1973.
- Child, C.M. Physiological foundations of behaviour. New York : Hafner, 1964
- Chomsky, N. Language and the mind. In A. Bar-Adon and W.F. Leopold (Eds.) Child language. Prentice-Hall, 1971
- Chomsky, N. The case against B.F. Skinner. In F.W. Matson (Ed.) Without/within : behaviourism and humanism. Monterey, Calif. : Brooks/Cole, 1973.
- Cicourel, A.V. Cognitive sociology. Harmondsworth, Middlesex : Penguin 1973.
- Cohen, D. Psychologists on psychology. London : Routledge and Kegan Paul, 1977.
- Cook, M. Interpersonal perception. Harmondsworth, Middlesex : Penguin, 1971.

- Cronbach, L.J.. Essentials of psychological testing. (3rd ed.)  
New York : Harper and Row, 1970.
- Darwin, C. The expression of the emotions in man and animals.  
London : Murray, 1873 (date of reprint lost).
- Davis, D.E. An inquiry into the phylogeny of gangs. In Bliss, E.C.  
Roots of behaviour. New York : Hafner, 1968.
- Day, W.F. Humanistic psychology and contemporary behaviourism. In  
F.W. Matson (Ed.) Without/within : behaviourism and humanism.  
Monterey, (Calif.) : Brooks/Cole, 1973.
- Dewsbury and Rethlingshafer : Comparative psychology : a modern survey.  
New York : McGraw-Hill, 1973.
- Dilger, W.C. Behaviour and genetics. In Bliss, E.L. (Ed.) Roots of  
behaviour. New York : Hafner, 1968.
- Dollard, J. and Miller, N.E. Higher mental processes. In M.R. Goldfried  
and M. Merbaum (Eds.) Behaviour change through self-control.  
New York : Holt, Rhinehart and Winston, 1973.
- Duckworth, D.H. Personality, emotional state, and perception of non-  
verbal cues. Perceptual and Motor Skills, 1975, 40, 325-326
- Ehrman, L., and Parsons, P.A. The genetics of behaviour. Massachusetts :  
Sinauer, 1976.
- Elkman, P. Non-verbal leakage and cues to deception. In M. Argyle  
(Ed.) Social encounters. Harmondsworth, Middlesex : Penguin, 1973.
- Eibl-Eibesfeldt, I. Ontogenetic and maturational studies of aggressive  
behaviour. In C.D. Clemente and D.B. Lindsley (Eds.) Aggression  
and defense : neural mechanisms and social patterns (brain  
function, (Vol.V). Berkeley : University of California Press, 1967.
- Eibl-Eibesfeldt, I. Ethology : the biology of behaviour. New York : Holt,  
Rhinehart and Winston, 1970.
- Eibl-Eibesfeldt, I. The expressive behaviour of deaf and blind-born. In  
M. Von Cranach and I. Vine. Social communication and movement.  
New York : Academic Press, 1973.
- Eibl-Eibesfeldt, I. Similarities and differences between cultures in  
expressive movements. In R A Hinde (Ed.) Non-verbal communication.  
London : Cambridge University Press, 1975.
- Eibl-Eibesfeldt, I. Phylogenetic and cultural adaptations in human behaviour.  
In G. Serban and A. Kling (Eds) Animal models in human  
psychobiology. New York : Plenum, 1976.
- Eibl-Eibesfeldt, I. The biological unity of mankind : human ethology,  
concepts and implications. Prospects, 1977a, 7, 2, 163-184.

- Eibl-Eibesfeldt, I. Evolution of destructive aggression. In Aggressive behaviour. Alan R Liss, 1977b.
- Eliot, J. (Ed.) Human development and cognitive processes. New York : Holt, Rhinehart and Winston, 1971.
- Ellis, A. Humanistic psychotherapy:the rational-emotive approach. New York : Julian Press, 1973.
- Estes, W.K. Reward in human learning : theoretical issues and strategic choice points. In R Glaser (Ed.) The nature of reinforcement. N.Y. : Academic Press, 1971.
- Etkin, W. Social behaviour from fish to man. Chicago : Phoenix, 1967.
- Evans, R.I. A serious preacher takes a stand : a sketch of Konrad Lorenz; and Lorenz Warns. Psychology Today, Nov. 1974, 82-93.
- Eysenck, H.J. (Ed.) Experiments in behaviour therapy. Oxford : Pergamon Press, 1964.
- Eysenck, H.J. The biological basis of personality. Springfield, Ill. : Charles C. Thomas, 1967.
- Eysenck, H.J. Psychology is about people. Harmondsworth, Middlesex : Penguin, 1972.
- Eysenck, H.J. How scientific is freudianism? Encounter, 1978, L (j), 36-40.
- Eysenck, H.J. and Wilson, G.D. The experimental studies of Freudian theories. London : Methuen, 1973.
- Ferster, C.B. An experimental analysis of clinal phenomena. In S.W. Bijou and E. Ribes-Inesta Behaviour modification : Issues and extensions. New York : Academic Press, 1972.
- Ferster, C.B., Culbertson, S. and Boren, M.C.P. Behaviour Principles (2nd ed.) Englewood Cliffs, N.J. Prentice-Hall, 1975.
- Feyereisen, P. Theories concerning certain expressive movements. Revue de Psychologie et des Sciences de l'Education, 1974, Vol. 9 (1) 89-113 (Abstract only).
- Fletcher, R. Instinct in man London : Allen and Unwin, 1957; 2nd ed., 1968.
- Foppa, K. Learning theory. In H.J. Eysenck, W. Arnold, and R Meili. Encyclopedia of psychology. (Vol.2). London : Search Press, 1972.
- Freedman, D.G. A biological view of man's social behaviour. In W. Etkin, Social behaviour from fish to man. Chicago : Univ. of Chicago Press, 1967.
- Fuller, J.L. and Waller, M.B. Is early experience different? In Bliss, E.L. (Ed.) Roots of behaviour. New York : Hafner, 1968.

- Gagne, R.M. Some relations of reinforcement theory to education. In R. Glaser (Ed.), The nature of reinforcement. New York : Academic Press, 1971.
- Gergen, K.J. The concept of self. New York : Holt, Rhinehart and Winston, 1971.
- Gewirtz, J.L. The roles of overt responding and extrinsic reinforcement in "self" and "vicarious-reinforcement" phenomena and observational learning and imitation. In R. Glaser (Ed.). The nature of reinforcement. New York : Academic Press, 1971.
- Gilbert, G.M. Personality dynamics : a biosocial approach. New York : Harper and Row, 1970.
- Glaser, R. (Ed.) The nature of reinforcement. New York : Academic Press,
- Glass, D.C. (Ed.) Genetics. New York : Rockefeller and Russell Sage, 1968
- Goldfried, M.R. and Davison, G.C. Clinical behaviour therapy. New York : Holt, Rhinehart and Winston, 1976.
- Goldfried, M.R. and Merbaum, M. (Eds.) Behaviour change through self-control. New York : Holt, Rhinehart and Winston, 1973 (a).
- Goldfried, M.R. and Merbaum, M. How to control yourself. Psychology Today, 1973, November (b)
- Goldiamond, I. Self-control procedures in personal behaviour problems. Psychological Reports, 1965, 17, 851-868
- Goldiamond, I. Fables, Armadyllics, and self-reinforcement. Journal of Applied Behaviour Analysis, 1976, 9, 521-525.
- Goldiamond, I. Self-reinforcement. Journal of Applied Behaviour Analysis, 1976, 9, 509-514.
- Gooch, S. Personality and evolution. London : Wildwood, 1973.
- Grant, E.C. Non-verbal communication in the mentally ill. In R.A. Hinde (Ed.) Non-verbal communication. London : Cambridge University Press, 1975.
- Grinder, R.E. A history of genetic psychology. New York : John Wiley and Sons, 1967.
- Hall, C.S. Psychoanalysis. In Psychology Encyclopedia. Guilford, Connecticut : Dushkin, 1973.
- Hall, E. Ethology's warning. A conversation with Nobel Prize winner Niko Tinbergen. Psychology Today, March 1974, 65-80
- Halsey, A.H. Biology and sociology : a reconciliation. In Glass, D.C. (Ed.) Genetics. New York : Rockefeller Univ. Press and Russell Sage Foundation, 1968.

- Harpers, R.J.C., Anderson, C., Christensen, C., Hunka, S. (Eds.)  
The cognitive processes : Readings Englewood Cliffs, N.J.  
 Prentice-Hall, 1964.
- Hass, H. The human animal. London : Hodder and Stoughton, 1970.
- Hebb, D.O. Drive and the C.N.S. (conceptual nervous system). In Harper et al (Eds.) The cognitive processes : Readings. Englewood Cliffs, N.J. : Prentice-Hall, 1964(a).
- Hebb, D.O. The American revolution. In Harper et al (Eds.). The cognitive processes : Readings. Englewood Cliffs, N.J. Prentice-Hall, 1964(b)
- Hebb, D.O., Lambert, W.E., and Tucker, R.G. A D.M.Z. in the language war. Psychology today, 1973, April, 55-62.
- Heim, A. Intelligence and personality. Harmondsworth, Middlesex : Penguin, 1970.
- Heine, P.J. Personality and social theory. London : Allen Lane, 1974.
- Hilgard, E.R. Behaviourism. In H.J. Eysenck, W. Arnold and R. Meili Encyclopedia of Psychology Vol. 1. London : Search Press, 1972.
- Hilgard, E.R., and Bower, G.H. Theories of learning. (4th ed.) New York : Appleton - Century - Crofts, 1975.
- Hinde, R.A. Animal behaviour : a synthesis of ethology and comparative psychology (2nd ed.). New York : McGraw-Hill, 1970.
- Hinde, R.A. Biological bases of human social behaviour. New York : McGraw-Hill, 1974.
- Hinde, R.A. (Ed.) Non-verbal communication. London : Cambridge University Press, 1975.
- Hinde, R.A. and Stevenson-Hinde, J. (Eds.). Constraints on learning : limitations and predispositions. London : Academic Press, 1973.
- Hinde, R.A., and Stevenson-Hinde, J. Towards understanding relationships : dynamic stability. In P.G. Bateson and R.A. Hinde (Eds.). Growing points in ethology. Cambridge : Cambridge Univ. Press, 1976.
- Hirsch, J. Individual differences in behaviour and their genetic basis. In E.L. Bliss (Ed.) Roots of behaviour. New York : Hafner, 1968
- Hirsch, J., and Hostetter, R.C. Behaviour genetics. In P. London and D. Rosenhan (Eds.) Foundations of abnormal psychology. New York : Holt, Rhinehart and Winston, 1968.
- Homme, L.E. Control of coverants : the operants of the mind. In M.R. Goldfried and Michael Merbaum (Eds.). Behaviour change through self control. New York : Holt, Rhinehart and Winston, 1973.

- Hooff, J.A.R.A.M. van. A comparative approach to the phylogeny of laughter and smiling. In R.A. Hinde (Ed.) Non-verbal communication. London : Cambridge University Press, 1975.
- Hudson, L. Contrary imaginations. Middlesex : Penguin Books, 1967.
- Huxley, Thomas. Man's place in nature. In Grinder, R.E. A history of genetic psychology. New York : John Wiley and Sons, 1967.
- Jahoda, M. Freud and the dilemmas of psychology. London : Hogarth Press, 1977.
- Kagan, J. On the need for relativism. In D.C. Charles and W.R. Looft (Eds.) Readings in psychological development through life. New York : Holt, Rhinehart and Winston, 1973.
- Kagan, J. Personality development. In P. London and D. Rosenhan (Eds.) Foundations of abnormal psychology. New York : Holt, Rhinehart Winston, 1968.
- Kanfer, F.H. Self-regulation. Research, issues, and speculations. In M.R. Golfried and M. Merbaum (Eds.) Behaviour change through self-control. New York : Holt, Rhinehart and Winston, 1973.
- Kazdin, A.E. Methodological and assessment considerations in evaluating reinforcement programs in applied settings. Journal of Applied Behaviour Analysis, 1973, 6, 517-531. (Includes reviewers comments).
- Kazdin, A.E. Behaviour modification in applied settings. Homewood, Ill. Dorsey Press, 1975.
- Keesing, R.M. and Keesing, F.M. New perspectives in cultural anthropology. New York : Holt, Rhinehart and Winston, 1971.
- Kelly, G.A. The psychology of personal constructs. Vol. one. New York : W.W. Norton, 1955.
- Kelly, G.A. The psychology of personal constructs. Vol. two. New York : W.W. Norton, 1955.
- Kelly, G.A. A brief introduction to personal construct theory. In Bannister, D. (Ed.) Perspectives in personal construct theory. London : Academic Press, 1970.
- Kelly, G.A. Behaviour is an experiment. In Bannister, D. (Ed.) Perspectives in personal construct theory. London : Academic Press, 1970
- Kendon, A. Some functions of gaze direction in social interaction. In M. Argyle (Ed.) Social encounters. Middlesex : Penguin, 1973.
- Klein, G.S. Perception, motives, and personality. New York : Alfred A. Knopf, 1970.
- Koch, S. Psychology cannot be a coherent science. In F.W. Matson (Ed.) Without/within : behaviourism and humanism. Monterey, Calif.; Brooks/Cole, 1973.

- Krebs, J. Monkeys prefer kin. Nature. 1978, 274, 311.
- Kuhn, T. The structure of scientific revolutions. Chicago : Univ. of Chicago Press, 1962.
- Lawick Goodall, J. van. In the shadow of man. Glasgow L Fontana/Collins, 1974.
- Leach, E. The influence of cultural context on non-verbal communication in man. In R.A. Hinde (Ed.) Non-verbal communication. London : Cambridge University Press, 1975.
- Lehrman, D.S. Interaction of hormonal and experiential influences on development in behaviour. In Bliss, E.C. (Ed.) Roots of behaviour. New York : Hafner, 1968.
- Levitt, E.E. The psychology of anxiety London : Paladin, 1971.
- Linden, E. Apes, men and language. New York : Penguin, 1976.
- Lindsley, D.B. Psychophysiology and motivation. In Harper et al. The cognitive processes : Readings. Englewood Cliffs, N.J. Prentice-Hall, 1964.
- Lippman, L.G. Learning theory. In Psychology Encyclopedia. Guilford, Connecticut : Dushkin, 1973.
- Lischke, G. Id. In H.J. Eysenck, W. Arnold, and R. Meili. Encyclopedia of psychology (Vol.2). London : Search Press, 1972.
- Logan, F.A. Incentive theory, reinforcement and education. In R. Glaser (Ed.) The nature of reinforcement. N.Y. : The Academic Press, 1971.
- Logan, F.A. Self-control as habit, drive and incentive. Journal of Abnormal Psychology. 1973, 81, 127-136.
- London, P. and Rosenhan, D. (Eds.) Foundations of abnormal psychology. New York : Holt, Rhinehart and Winston, 1968.
- Lorenz, K. Evolution and modification of behaviour. London : Methuen, 1965.
- Lorenz, K. On aggression. London : Methuen, 1967.
- Lorenz, K. The animal kingdom. Tucson, Arizona : Learning Plans, 1969 (Tape)
- Lorenz, K. and Leyhausen, P. Motivation of human and animal behaviour. New York : Van Nostrand Rhineholt, 1973.
- MacCorquodale, K. Behaviourism is a humanism. In F.W. Matson (Ed.) Without/within : behaviourism and humanism. Monterey, Calif.: Brooks/Cole, 1973.
- Maddi, S.R. Personality theories : a comparative analysis. (3rd ed.) Homewood, Illinois : Dorsey Press, 1976.

- Maher, B. Behaviour dynamics. In P. London and D. Rosenhan (Eds.) Foundations of abnormal psychology. New York : Holt, Rhinehart and Winston, 1968.
- Mahoney, M.J. Cognition and behaviour modification. Cambridge, Mass.: Ballinger, 1974.
- Mahoney, M.(a) Seekers after truth. Psychology Today, April, 1976.
- Mahoney, M.J. (b) Terminal terminology : a self-regulated response to Goldiamond. Journal of Applied Behaviour Analysis, 1976, 9, 515-517
- Mancuso, J.C. (Ed.) Readings for a cognitive theory of personality. N.Y. : Holt, Rhinehart and Winston, 1970.
- Marcos, L.R. The emotional correlates of smiling and laughter. American Journal of Psychoanalysis, 1974, 34, 33-41.
- Matson, F.W. (Ed.) Without/within : behaviourism and humanism. Monterey, Calif. : Brooks/Cole, 1973.
- Mattejat, F. Impulsive therapy. In H.J. Eysenck, W. Arnold, and R. Meili, Encyclopedia of psychology (Vol.2). London : Search Press, 1972.
- Mead, M. Problems and progress in the study of personality. In Norbeck et al (Eds.) The study of personality. N.Y. : Holt, Rhinehart, and Winston, 1968.
- Medawar, P.B. Does ethology throw any light on human behaviour? In P.G. Bateson and R.A. Hinde (Eds.) Growing points in ethology. Cambridge Univ. Press, 1976.
- Mehabrian, A. Non-verbal communication. New York : Aldine-Atherton, 1972.
- Mikulas, W.L. Behaviour modification : an overview. New York : Harper and Row, 1972.
- Mischel, W. Continuity and change in personality. In D.C. Charles and W.R. Looft (Eds.) Readings in psychological development through life. New York : Holt, Rhinehart and Winston, 1973.
- Morris, D. (Ed.) Primate ethology. London : Weidenfeld and Nicolson, 1967.
- Murphy, G. Personality : a biosocial approach to origins and structure. New York : Harper and Brothers, 1947.
- Murphy, G. Psychological views of personality and its study. In Norbeck et al The study of personality. New York : Holt, Rhinehart and Winston, 1968.
- Nash, J. Developmental psychology : a psychobiological approach. Englecliff N.J. : Prentice-Hall, 1970.

- Nevin, J.A. (Ed.) The study of behaviour : learning, motivation, emotion and instinct. Glenview, Ill. : Scott, Foresman, 1973
- Norbeck, E., Price-Williams, D., and McCord, W.M. (Eds.). The study of personality : an interdisciplinary appraisal. New York : Holt, Rhinehart and Winston, 1968.
- Nye, R.D. Three views of man. Belmont, Calif. : Wadsworth, 1975.
- Osipaw, S.H. Theories of Career Development (2nd ed.) New York : Appleton Century Crofts, 1973.
- Palermo, D.S. Is a scientific revolution taking place in psychology? Science Studies, 1971, 1, 135-55.
- Parsons, T. Social structure and personality. New York : Free Press, 1970
- Patterson, C.H. A model for counselling and other facilitative human relationships. In W.H. Van Hoose, and J.J. Pietrofesa (Eds.) Counseling and guidance in the twentieth century. Boston : Houghton Mifflin, 1970.
- Pervin, L.A. Personality - theory assessment and research. New York : John Wiley and Sons, 1970.
- Phillips, D.C. and Kelly, M.E. Hierarchical theories of development in education and psychology. Harvard Educational Review 1975, 3, 351-375
- Piaget, J. Main trends in interdisciplinary research. London : Allen and Unwin, 1973a.
- Piaget, J. Main trends in psychology. London : Allen and Unwin, 1973b.
- Premack, D. Behaviour modification. A taped discussion with N. Azrin. N. Hollywood, Calif. : Center for Cassette Studies, undated.
- Premack, D. The education of Sarah : a chimp learns language. Psychology Today, 1970, 4, 55-58.
- Premack, D. Catching up with common sense or two sides of a generalisation : reinforcement and punishment. In R. Glaser (Ed.), The nature of reinforcement. New York : Academic Press, 1971a.
- Premack, D. Language in the chimpanzee? Science, 1971b, 172, 808-822.
- Premack, D. and Anglin, B. On the possibilities of self-control in men and animals. Journal of Abnormal Psychology, 1973, 81, 137-151.
- Pribram, K. Toward a neuropsychological theory of person. In Norbeck et al (Eds.) The study of personality. N.Y. : Holt, Rhinehart and Winston, 1968.
- Reynolds, V. The biology of human action. Reading : W.H. Freeman, 1976.
- Rex, J. Key problems of sociological theory. London : Routledge and Kegan Paul, 1970.

- Rosenthal, R., Archer, D., DiMatteo, M.R., Koivumaki, J.H., and Rogers, P.L. Body talk and tone of voice : the language without words. Psychology Today, 1974, Sept, 64-68
- Rudner, R.D. Philosophy of Social Science, Englewood Cliffs, N.J. Prentice-Hall, 1966
- Russell C., and Russell, W.M.S. Human behaviour London : Deutsch, 1961.
- Sahlins, M. The use and abuse of biology. London : Tavistock, 1977.
- Sampson, E.E. Social psychology and contemporary society. New York : John Wiley and Sons, 1971.
- Schell, R.E. (Ed.) Developmental psychology today. (2nd ed.) New York : Randum House, 1975.
- Scherer, S.E. Influence of proximity and eye contact on impression formation. Perceptual and Motor Skills, 1974, 38, 538.
- Scott, J.P. Hostility and aggression in animals. In Bliss, E.L. (Ed.) Roots of behaviour. New York : Hafner, 1968
- Shapiro, E. (Ed.) Psychosources New York : Bantam, 1973.
- Siiter, R. Language. In Psychology encyclopedia. Guilford, Connecticut : Dushkin, 1973
- Siiter, R. Learning-to-learn. In Psychology Encyclopedia. Guilford, Connecticut : Dushkin, 1973.
- Sinnott, E.W. Cell and psyche. New Haven, Connecticut : Harper Torchbook, 1961.
- Skinner, B.F. Science and human behaviour. New York : Macmillan, 1953.
- Skinner, B.F. A case history in scientific method. American Psychologist, 1956, 11, 221-233.
- Skinner, B.F. Humanism and behaviorism. In F.W. Matson (Ed.). Without/within : behaviorism and humanism. Monterey, Calif. : Brooks/Cole, 1973.
- Skinner, B.F. Beyond freedom and dignity. Middlesex : Penguin, 1973.
- Skinner, B.F. About behaviourism London : Jonathan Cape, 1974.
- Skinner, B.F. and Matson, F.W. Humanistic behaviorism? An exchange. In F.W. Matson (Ed.) Without/within : behaviorism and humanism. Monterey, Calif. : Brooks/Cole, 1973.
- Smart, M.S., and Smart, R.C. Recalled, present, and predicted satisfaction in stages of the family life cycle in New Zealand. Journal of Marriage and the Family, 1975, May, 408-415

- Sontag, L.W. Somatopsychics of personality and body function. In D.C. Charles and W.R. Looft (Eds.) Readings in psychological development through life. New York : Holt Rhinehart and Winston, 1973.
- Spregal, J.P. and Machotka, P. Messages of the body. New York : The Free Press, 1974.
- Stenhouse, D. Scientific creativity : 'normal' or 'revolutionary'. Australian Journal of Education, 1971, 15, 171-84
- Stenhouse, D. The evolution of intelligence : a general theory and some of its implications. London : Allen and Unwin, 1974.
- Stenhouse, D. Evolutionary, adaptive, and ethological considerations in the assessment of intelligence. Interchange 1976-77, 7, 3, 51-61.
- Stenhouse, D. Human ethology, education, and learning. Interchange, 1977-78, 8, 3, 88-101.
- Syndes, M., Grether J. Staring and compliance : a field experiment of hitch-hiking. Journal of Applied Soc. Psych. 1974, 4.
- Tajfel, H. Social psychology. The Fontana dictionary of modern thought. London : Fontana, 1977.
- Takuma, T. Personality II. In H.J. Eysenck, W. Arnold, and R. Meili Encyclopedia of psychology (Vol.2). London : Search Press, 1972.
- Thomas, A., Chess S., and Birch., G. The origin of personality. Scientific American, 1970, August, 2-9.
- Thoresen, C.E. and Mahoney, N.J. Behavioural Self Control. New York : Holt, Rhinehart and Winston, 1974.
- Thoresen, C.E. and Wilbur, C.S. Some encouraging thoughts about self-reinforcement. Journal of Applied Behaviour Analysis. 1976 9, 518-520.
- Thorpe, W.H. Learning and instinct in animals. (new ed.) London : Methuen, 1963.
- Thorpe, W.H. Animal nature and human nature. London : Methuen, 1974.
- Tiger, L., and Fox., R. The imperial animal Frogmore, St Albans : Paladin, 1974.
- Tinbergen, N. The study of instinct. Oxford : Clarendon, 1951.
- Tinbergen, N. Ethology in a changing world. In P.G. Bateson and R.A. Hinde (Eds.) Growing points in ethology Cambridge : Cambridge Univ. Pres, 1976
- Tinbergen, N. Ethology. The Fontana dictionary of modern thought. London : Fontana, 1977.

- Tobach, E., and Schneirla, T.C. Eliminative responses and "emotionality". In Bliss, E.L. (Ed.) Roots of Behaviour. New York : Hafner, 1968
- Toman, W. Unconscious. In H.J. Eysenck, W. Arnold, and R. Meili Encyclopedia of psychology (Vol.3). London : Search Press, 1972
- Torrance, J. Role, role theory. Fontana dictionary of modern thought. London : Fontana, 1977.
- Travers, R.M.W. Some comments on the language of the psychology of reinforcement as used in the conference papers. In R. Glaser (Ed.) The nature of reinforcement. N.Y. : Academic Press, 1971
- Treisman, M. Mind, body, and behaviour : Control systems and their disturbances. In P. London and D. Rosenhan (Eds.) Foundations in abnormal psychology New York : Holt, Rhinehart and Winston, 1968.
- Tschanz, B. Imprinting. In H.J. Eysenck, W. Arnold, and R. Meili Encyclopedia of psychology (Vol.2). London : Search Press, 1972
- Van Maanen, J. Breaking in : socialisation to work. In R. Dubin (Ed.) Handbook of work, organisation and society. Chicago : Rand McNally, 1976.
- Vernon, P.E. Personality. In H.J. Eysenck, W. Arnold, and R. Meili Encyclopedia of psychology (Vol.2). London : Search Press, 1972
- Von Cranach, M. and Vine, I. Social communication and movement. New York : Academic Press, 1973.
- Wahler, R.G. Some ecological problems in child behaviour modification. In S.W. Bijou and E. Ribes-Inesta, Behaviour modification : issues and extensions. New York : Academic Press, 1972.
- Wallace, A.F.C. Anthropological contributions to the theory of personality. In Norbeck et al (Eds.) The study of personality. N.Y. : Holt, Rhinehart and Winston, 1968
- Weimer, W.B. and Palermo, D.S. Paradigms and normal science in psychology. Science Studies 1973, 3, 211-44.
- Wessman, A.E. Cognitive psychology. In Psychology Encyclopedia. Guilford, Connecticut : Dushkin, 1973.
- White, L.A. Energy and the evolution of culture. In The science of culture, 363-93, 1949.
- White, R.W. Lives in progress (2nd ed.) New York : Holt, Rhinehart and Winston, 1966.
- White, S.H. The learning - maturation controversy : Hull to Hull. In D.C. Charles and W.R. Looft (Eds.) Readings in psychological development through life. New York : Holt, Phinehart and Winston, 1973.

- Wiggins, J.S. Personality and prediction : Principles of personality assessment. Reading, Massachusetts : Addison-Wesley, 1973.
- Williams, R.J. The biological approach to the study of personality. In W.R. Baller (Ed.) Readings in the psychology of human growth and development. (2nd ed.) New York : Holt, Rhinehart and Winston, 1969.
- Wilson, E.O. Sociobiology : the new synthesis. Cambridge, Mass : Pelknap Press, Harvard Univ. Press, 1975.
- Wrong, D.M. The oversocialised conception of man. American Sociological Review, 1961, 26, 2, 183-193
- Wylie, R.C. The self-concept (Rev.ed.) Vol.1 Lincoln : Univ. of Nebraska Press, 1974.