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IN DEFENCE OF BEHAVIOURISM:

A Skinnerian reinterpretation of Stenhouse's ethological theory of intelligence, supported by a Galilean philosophy of science.

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ABSTRACT

This thesis attempts to justify a Skinnerian interpretation of intelligence. The justification has three major themes. Firstly it is argued that Skinnerian behaviourism has the status of scientific knowledge comparable to Newtonian mechanics. Secondly it is argued that Stenhouse's ethological theory of intelligence has a number of defects, so that a behaviourist theory which retains the strengths of the ethological theory while avoiding those defects is to be preferred. Thirdly it is argued that certain widely received accounts of scientific knowledge are mistaken; an alternative account is presented. This venture into philosophy of science underlies the other two themes and is presented first.

The supposition that science may be represented in terms of general laws of the form 'All swans are white' is critically examined, following Toulmin's analysis which is illustrated with three exemplars of scientific knowledge.

A Galilean ideal of science is then elaborated. The ideal is formulated in terms of scientific knowledge following Toulmin, and illustrated with three exemplars of scientific knowledge. The processes of revolutionary science, normal science, technology, and justification of theories, are interpreted in terms of the ideal alluded to above with further illustrations. Convergences with de Bono's 'lateral thinking' are suggested. Criticisms of statistical 'social science' are noted. The conventional contrast between physical and social science is critically examined.

A formulation of Skinnerian behaviourism is presented, to demonstrate that behaviourism conforms to the Galilean ideal of science. Various criticisms

of behaviourism are responded to. The proposed criteria for justification of theories are applied to behaviourism.

Stenhouse's ethological theory of the nature and evolution of intelligence is critically examined. The divergent development of ethology and behaviourism from reflexology is outlined. Skinner's critique of Pavlov's concept 'inhibition' is applied to Stenhouse's 'P-factor'. The use of metaphors in science is discussed. De Bono's 'special memory surface' is noted as an alternative to the usual mechanical or electronic storage systems as a metaphor for memory.

Skinner's analysis of the nature and evolution of intelligence is elaborated. Stenhouse's factors and especially the P-factor are reinterpreted in behaviourist terms. It is argued that a behaviourist theory of intelligence is preferable to Stenhouse's ethological theory in terms of the Galilean ideal of science.

Educational and political implications of various philosophical and theoretical positions are also noted.

## PREFACE

While this thesis is organised around and concludes with a behaviourist reinterpretation of Stenhouse's theory of intelligence, that reinterpretation is confined to one final chapter. The preceding four chapters discuss various issues as listed in the Abstract (principally in the philosophy of science both generally and as applied to behaviourism and ethology), which may be of wider interest apart from the support they give to the final chapter.

The existing treatments of these issues vary: some are merely informal and fragmentary, some while substantial have been neglected by more recent writers, and some express what I will argue are seriously misleading views as to the nature of science. In view of this situation it seemed necessary to discuss some of those issues at length in order to clarify and defend the presuppositions of the final chapter; other related issues are discussed briefly by way of digressions. At best those first four chapters may contain some substantial contribution on one or two issues in the philosophy of science; however failing that the bibliography and page-specific references may still prove useful.

Perhaps the most prominent of those issues, both within this thesis and at large, is the scientific status of behaviourism. Thus the short title 'In defence of behaviourism', while not reflecting the structure or conclusion, does indicate a major theme of some wider interest.

Certain key terms may conveniently be elaborated at this point.

'Behaviourism', and hence 'behaviourist', refer here to the science of behaviour developed by

B.F. Skinner and outlined in Chapter 3. This usage differs from that of Skinner (1974), who used the term 'behaviourism' to refer to the philosophy of that science. In this thesis the philosophy of behaviourism is considered to be part of the philosophy of science in general.

'Science' refers to the tradition identified with Galileo, Newton, Mendel, Darwin and many others. It is a complex behavioural phenomenon, in some aspects comparable with and merging into myths (Feyerabend 1961; 1975) and common idiom (cf Toulmin 1953:39 on certain metaphors as the ghosts of dead theories). While it would be difficult to gain assent to any strict definition of science, particular works identified with particular persons (eg. Newtonian mechanics) may generally be accepted as exemplars of science from which an ideal form of science may be abstracted. A theory may be said to be 'scientific' to the extent that it conforms to such an ideal. Science may be analysed into products and processes. In Chapter 2 there are identified the products 'primary knowledge' and 'secondary knowledge', and the processes 'revolutionary science', 'normal science', 'technology' and 'justification of theories'.

Likewise the term 'technology' is not restricted to the applications of physical sciences to the design of hardware; the term is used to refer to the application of any scientific theory to any practical problem. For instance, an application of behaviourism to the solution of instructional problems in schools, regardless of whether or not any gadgetry or even numerical measurement are involved, counts as technology in this sense.

A 'theory' is a coherent body of (justified or hypothesised) primary knowledge, together with its associated secondary knowledge (if any exists). One exemplar of a theory is Newtonian mechanics.

The conventional dichotomy between 'science' and 'social (or behavioural) science' is not observed, in keeping with the view that behaviourism is not in principle different from physical sciences such as Newtonian mechanics. That the dichotomy is usually founded on a lack of familiarity with physical sciences was indicated by Popper (1966:292n44(2)):

"And it turns out that those who believe that intuitive understanding is a method peculiar to sciences of 'human behaviour' hold such views mainly because they cannot imagine that a mathematician or a physicist could become so well acquainted with his object that he could 'get the feel of it', in the way in which a sociologist 'gets the feel' of human behaviour".

The positions labelled 'Aristotelian' and 'Galilean' may not conform in all respects to the views of Aristotle and Galileo respectively. The term 'Aristotelian' is used following Revusky (1974:693); the term 'Galilean' is used (in place of Revusky's 'Platonic') following Cardwell (1972:36).

In view of the diverse topics treated and the volume of the relevant literature, a comprehensive literature review would be unwieldy. As each topic is raised some of the literature relevant to that topic is reviewed and other relevant items are noted without comment.

I wish to thank my supervisor Mr Eric Archer for his guidance and encouragement, Mrs O. Healey for doing most of the typing, and my wife Jan for her support.

## CHAPTER 1 : The Aristotelian View of Science

### 1.1 Varieties of the Aristotelian view

Inspection of a typical collection of 'readings' in philosophy of science, such as that edited by Brody (1970), reveals that much of the discussion concerns statements such as 'All ravens are black' and 'All swans are white'. These are referred to as 'universal generalisations' or 'general laws'. The use of such statements to illustrate issues in the philosophy of science presupposes that some major aspect of science can be represented as general laws. Lakatos stated this explicitly: "For Popper (and for me) without genuinely universal propositions there can be no scientific theories" (1968:332n1, original emphasis).

The importance of such general laws is readily traced to their role as major premises in Aristotelian syllogisms, of the form:

All swans are white	(major premiss)
x is a swan	(minor premiss)
Therefore x is white.	(conclusion)

However, as Toulmin (1953:33) noted, "none of the substantial inferences that one comes across in the physical sciences is of a syllogistic type".

The association of syllogistic inference with the physical sciences does have some historical foundation: the first science to develop was astronomy, and it was in astronomy that the first successful and precise predictions of events (such as eclipses - cf Popper 1961:385f) were made using mathematical formulae. The use of such a formula bears some resemblance to syllogistic inference, to the extent that the formula may be seen as the major premiss, the particular case (eg. 'the next lunar eclipse') as the minor premiss, and the inferred date of that eclipse as the conclusion. However such predictions, along with the modern tide-tables

and tables of future planetary positions, stand to one side of the mainstream of modern scientific theory: they make no use of our Newtonian understanding of the solar system, relying instead on purely numerical methods of fitting general equations to the recorded data (cf Toulmin & Goodfield 1961:41,247). The equations used are capable of fitting any periodic data whatsoever regardless of its conformity to any planetary mechanics. Such ad hoc procedures can hardly stand as exemplars of science upon which to build a philosophy of science.

In the above example of syllogistic inference, the general law was a given premiss from which a conclusion was deduced. We cannot obtain general laws directly from observation, which yields only individual statements such as 'This is a white swan'. The problem of how to infer or induce general laws from a finite number of such individual statements is known as 'The Problem of Induction'. A solution of sorts to this problem is found in various statistical methods, principally those originated by R.A. Fisher who went so far as to claim (1966:7): "Inductive inference is the only process known to us by which essentially new knowledge comes into the world". This is close to the view of science articulated by Francis Bacon. However Brush (1976) and Smolicz and Nunan (1975) argued that while this view may be found in many science textbooks, it is seriously misleading as a general account of how scientific knowledge was actually generated.

Karl Popper's methodology of conjectures and refutations arose as an alternative to Baconian inductivism (cf Lakatos 1971:96-97). However the two views still share the Aristotelian syllogistic representation of science. As a first approximation Popper's view may be formulated thus (cf Popper 1959:40-42):

Scientific 'knowledge' consists of conjectures such as 'All swans are white'. We can never prove such a conjecture beyond reasonable doubt, for it is always possible that a black swan may be found. We can however test the conjecture (the old meaning of 'prove', retained in 'auto proving-ground'), by searching for black swans. If we find one, that refutes the conjecture. The more open to such unambiguous refutation our conjecture is, and the more thoroughly we have tried to refute it (without success), the more highly corroborated it becomes.<sup>1</sup>

Lakatos (1971:113) argued that Popper failed to demonstrate his theory of science as a description of actual scientific research. In general Popper did not even attempt to provide historical exemplars of conjectures and refutations; when he did try "he either plunges into some logical blunder or distorts history to fit his... theory" (Lakatos 1971:113). Popper also neglected to apply his methodology to his own theory of science (as a meta-scientific theory - cf Lakatos 1971:111), that is, to corroborate it by seeking and not finding refutations in the history of science. Lakatos (1970; 1971) searched and found many such refutations. Indeed counter-examples to Popper's conjecture were published much earlier : Henderson (1935:117-118) and Skinner (1953:204-205) both noted that Boyle's Law of gases does not admit of Popperian refutation by the many gas phenomena which do not conform to the Law.

Feyerabend (1965:224) went further, claiming that "no theory ever agrees (outside the domain of computed

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1. This format (text indented and close-spaced) is used here for formulations as well as for extended quotations, the latter being identified by quotation marks. Thus while the above is a formulation of Popper's view it is not a direct quotation or even a close paraphrase. The reference indicates where a comparable exposition may be found. The same format will also be used for formulations of positions proposed in this thesis.

error) with the available evidence" (original emphasis), and noting by way of example (p253n7) that "Not a single planet moves in the orbit calculated in accordance with Newton's celestial mechanics (this has nothing to do with relativistic effects). There exist other as yet unexplained discrepancies exceeding the error of measurement by a factor of 10".

Thus, to follow Popper's view one would have to consider both Newtonian and Einsteinian mechanics as refuted.

There are other versions of Popper's view, in particular that termed 'sopisticated falsificationism' by Lakatos (1970) in contrast to the 'naive falsificationism' presented above. In sophisticated falsificationism what occurs as refutation is not the outright rejection of a theory, but rather its modification into a more correct form. However this is another matter, apart from the general issue of Aristotelian views of science. A case against Lakatos' methodology of sophisticated falsificationism was argued by Feyerabend (1975:181-214).

De Bono (1971: 172) accepted in passing a Popperian view of science, but in his discussion of practical thinking proposed a view of knowledge close to the Galilean view proposed below. He gave a useful critique of the Aristotelian view of knowledge in the course of his advocacy of a less dogmatic alternative (1971; 1973).

Having reviewed the Aristotelian view of science, we turn to Toulmin's analysis of its underlying fallacy and three illustrations of the analysis.

## 1.2 The Aristotelian fallacy

Toulmin argued that it is impossible to treat the statements of theoretical physics as general laws for a

basic logical reason:

"The reason why the form "All As are Bs" does not fit the statements of physics is this: only where one can ask separately, first, "What are these?" (Answer : As), and then, "What common properties have they?" (Answer: being Bs), is "All As are Bs" the natural form in which to couch one's conclusions. One can make this separation in natural history; but in the physical sciences the two questions are interdependent, and in consequence the simple generalisation is out of place"(1953:52, original emphasis).

In the case of 'All swans are white' there exist criteria for identifying a given bird as a swan, independently of its colour. Various anatomical features may serve as criteria, or it may be feasible to test the ability to yield fertile offspring when mated with a known swan. Thus there is no logical problem in identifying a given black bird as a swan, and thus refuting the conjecture in accordance with Popper's scientific method. However, it is easy to find items of established scientific knowledge in several sciences which do not conform to this pattern.

Three such examples will be presented to illustrate the fallacy. All were discussed by A.J. Ayer (1956), one philosopher who did at least attempt to give examples (albeit unsourced) in his discussion of 'laws of nature'.

The first is from Mendelian genetics. Whether it merits the designation 'law' is debateable; one much-used genetics text (Stansfield 1969:8) does not mention it among 'Mendel's laws'. However we will consider it here at face value and return to this issue in Chapter 2. Ayer's (1956:46) example was:

"...when there are two genes determining a hereditary property, say the colour of a certain type of flower, the proportion of individuals in the second generation that display the dominant attribute, say the colour white as opposed to the colour red, is three quarters".

Ayer's concern was with the statistical form of the law, and such concern would be quite proper if the law did conform to the logic of general laws. However Toulmin's objection stands: there is within Mendelian genetics no criterion, independent of the observed proportions of off-spring, either for identifying a hereditary property determined by two genes or for identifying a dominant attribute.

The second example is Charles' law:

The volume of a fixed mass of an ideal gas is directly proportional to its absolute temperature (cf Barrow 1961:6).

Ayer (1956:48) quoted the law omitting the terms 'ideal' and 'absolute', both of which are vital to its interpretation. Toulmin's objection is realised on both these counts. There was for some time after Charles' work (until the later Boltzmann Distribution theory) no independent criterion for absolute temperature, for that was (and still may be) defined in terms of Charles' law. There is no independent criterion for identifying an ideal gas, for a gas is considered ideal to the extent that it conforms to the ideal gas laws, of which Charles' law is one (cf Barrow 1961:6).

Indeed there is no strictly 'ideal gas': every real gas shows discrepancies from the ideal gas laws and, as Harré (1961:82f) (cf Hanson 1965b:19) showed, the very existence of an ideal gas is inconceivable given the accepted kinetic-molecular theory of gases.

The third example is from that outstanding scientific achievement which dominated science for centuries and which is still the basis of most engineering: Newtonian mechanics.

Lest it be thought that Einsteinian (relativistic) mechanics has generally superseded Newton's work, we may note that despite its more adequate representations of a few special phenomena, in general the calculations

required for Einsteinian accounts of any but the most idealised phenomena are prohibitively difficult. For instance, the interactions between planets in the solar system are not treated in Einsteinian terms. In the famous debate over the precession of the orbit of Mercury, which was a major early success for Einsteinian mechanics, it was applied only to the highly idealised model of a single planet in orbit about a point-like sun, and the result of that representation was used to account for the discrepancy between the observed orbit of Mercury and the Newtonian representation of the solar system as an interacting whole (cf Bunge 1961: 431; Hanson 1962:377n24).

Ayer (1956:47) quoted Newton's First Law thus:

"...a body on which no forces are acting continues at rest or in uniform motion along a straight line".

Ayer's concern was that no such bodies exist, which follows from Newton's law of universal gravitation. However this problem vanishes when the term 'force' is replaced by 'nett force' or 'unbalanced force', as in the textbook formulation (Schaum 1961:35):

"A body will maintain its state of rest or of uniform motion...along a straight line unless compelled by some unbalanced force to change that state".

Toulmin's objection holds: there are no general criteria for identifying an unbalanced force, independently of the state of motion of the body acted upon. There are, of course, formulae for calculating particular forces, such as Newton's law of gravitation; in practice one does use these to calculate the nett force acting on a given body. However either of two complications may arise: there may be other unsuspected forces acting on the body, or one may have observed the body's motion relative to a frame of reference which is itself accelerating (i.e. a non-inertial frame). The

only general indication that these complications have been avoided is that the body in question does conform to Newton's laws of motion.

We may conclude that, whatever the nature of scientific knowledge, it does not in general (if at all) conform to the Aristotelian syllogistic ideal of Bacon, Popper, Ayer and many others.

## CHAPTER 2 : A Galilean Ideal of Science

### 2.1 Claims and disclaimers

It is not claimed that all scientific work conforms to the Galilean ideal in the sense of proposing a general law in the philosophy of science, for such a claim would be out of keeping with the above argument that general laws are not characteristic of science. (Here philosophy of science is seen as a meta-science whose subject-matter is the phenomena of the various sciences including itself. As in other meta-activities, such as teaching about teaching, it is generally preferable to maintain consistency between the two levels.)

If there were a general law to be enunciated, a promising candidate would be Mitroff's (1974:193):

"For every abstract philosophical position that can be formulated with respect to the nature of science, there exists a corresponding 'real' scientist whose behaviour and attitudes embodies (sic) that abstract position".

(It may be noted that this is not quite a 'general law' as formulated in Chapter 1, but rather an 'all-and-some' hypothesis of the form discussed by Watkins (1958).)

Rather, it is claimed firstly that there does exist at least some science (both process and product) which conforms to the ideal presented, secondly that this science is amongst that commonly recognised as great or classic science which has stood the test of time, and thirdly that it conforms to this ideal more than to any other ideal of science. The first two claims are to the effect that the Galilean ideal is a viable philosophy of science; this chapter attempts to establish just that. The third claim could be established only by an examination of all other philosophies of

science, including the conventionalist and instrumentalist positions (for critiques see Feyerabend 1964; Lakatos 1971:95) in addition to the Aristotelian position examined in Chapter 1. Such an exercise is beyond the scope of this thesis. However a digression (s2.2) may be in order to indicate terms of reference for a debate between rival philosophies of science.

A substantial amount of historical material will be presented in this chapter, for two reasons: firstly to give ample illustrations of what is meant by the Galilean ideal; secondly to justify this ideal on which depends the subsequent justification of behaviourism and criticisms of ethology.

## 2.2 Methods in philosophy of science

A criterion for progress in the philosophy of science was proposed by Lakatos by way of resolving an apparently vicious circularity in the relationship between philosophy of science and history of science. This circularity was also discussed by Hanson (1963).

On the one hand, if it is to be shown that a given philosophy of science bears any relationship to the historical phenomena of science (eg. the work of Kepler), this requires reference to some report of the phenomena. Thus to justify a philosophy of science one requires a history of science. (However, as Hanson (1969:83) noted, some philosophers of science have been content to force the history into the Procrustean bed of their philosophy, rather than testing their philosophy against the history.) On the other hand a historian of science is confronted with a miscellany of reports of the various activities of various people; to write a history of science is to select out those activities which constitute science and those people who are really scientists, as distinct from the passing

fashions and individual eccentricities of scientists, the inevitable charlatans, cranks (cf Feyerabend 1964: 305) and genuine but misguided workers, and any other 'noise' (in the information-theory sense of the term). Such a process of selection requires a criterion for (or an ideal of) what is 'really scientific', thus a history of science presupposes a philosophy of science. This circularity complicates attempts to criticise or justify any given philosophy of science or history of science.

Lakatos' proposal (1971:117-118) may be formulated:

Progress in philosophy of science is identified by an increase in the extent and detail of historical phenomena which conform to the philosophy.

This may be interpreted in two ways. Firstly, at the level of descriptive philosophy ('what is'), we may argue that a particular philosophy of science is better than its rivals (i.e. constitutes progress as compared to them) in that it represents a greater body of actual scientific work and does so in more detail. Secondly, at the level of prescriptive philosophy ('what ought to be'), we may argue that a particular philosophy of science is better than others in that, had scientific activity conformed more closely to its ideals, it would have been in some sense better science. Argument at this level obviously requires consideration of historical possibilities in conjunction with the actual phenomena. Lakatos appeared to work largely at the descriptive level, although this may be deceptive as the prescriptive level of improving on history is implicit in his (1971) concept of 'rational reconstruction of history'. Both levels occurred more explicitly in Feyerabend's (1975) reply to those philosophers who prescribe methodological rules for science: he argued at the descriptive level that Galileo did not conform

to any such rules, and at the prescriptive level (negatively) that had Galileo conformed to the rules in question he would not have been able to advance the Copernican revolution as effectively as he did. Both levels will be used later in this chapter.

### 2.3 Introduction to Galilean science

Galileo's scientific method was described and illustrated by Cardwell (1972:36-46) and by Rapoport (1958:261-264). Its characteristic feature is the simple principles which explain and provide interpretations of observed phenomena. These principles are seldom easy to discover, for in most phenomena they are obscured by complex interactions between numbers of them. Ideally each principle may be demonstrated in experimental phenomena which are devised to minimise such interactions between principles and thus display one principle for direct scrutiny. We shall see that the three alleged 'general laws' of Chapter 1 may be better represented as such principles.

Complex and subtle interactions between principles, confounding the effects of any principle under scrutiny, are very much a 'fact of life' in science, contrary to the impression of deterministic simplicity that seems to be often gained from a superficial acquaintance with science (eg. Steiner 1974). One discussion of such interactions is Henderson's (1935) comparative account of the analysis of mutually dependent variations in Pareto's theory of social systems and in Gibbs' theory of physico-chemical systems. Another is Skinner's (1953:204-224) account of multiple effects and multiple causes in the analysis of complex cases in behaviourism. Given that such complexities are common it is not surprising that few of the principles comprising scientific knowledge were discovered by anything

resembling Baconian induction. (Some apparent exceptions were Balmer's spectral formula, Kepler's planetary laws and Boyle's law of gases. For comments on these apparent exceptions see Lakatos (1970:147, 152n4) and Toulmin & Goodfield (1962:176-178); for reports of Baconian misrepresentations of the process of scientific discovery see Brush (1976) and Medawar (1964).)

#### 2.4 Derivation of the formulation

If it be accepted that general laws ('All swans are white') have failed as a formalised ideal of scientific knowledge, then it would be appropriate to set them firmly aside in the manner of lateral thinking (de Bono 1973) and to explore a logically contrasting form, of which the simplest is probably existential statements such as 'There exists at least one white swan'. (Bunge (1961:426) conceded that similar statements, of the form "there is at least one  $x$  such that  $x$  is an  $F$ " may be among the postulates of a scientific theory, but noted that they are irrefutable and insisted on a Popperian refutability of the theory as a whole.) The present proposal is that existential statements (of more complex form), irrefutable as they may be, constitute the essence of scientific knowledge. We will develop this proposal as a derivation from Toulmin's views on scientific discovery and knowledge.

Toulmin (1953) presented scientific discovery, eg. that 'light travels in straight lines', as the invention of a novel technique for representing phenomena. The corresponding technique is the use of the ray diagrams of geometrical optics; comparable techniques in other fields are chemical formulae, speed/time graphs in kinematics, free-body diagrams in mechanics, electric circuit diagrams, and Skinner's

cumulative record of responses. Such techniques clearly play a major role in scientific discourse. However, being techniques for representing phenomena, they are hardly what we would call 'scientific knowledge' in the classical sense of 'justified, true belief'. It is one thing to invent a technique for representing familiar phenomena; it is quite another to demonstrate some new fact, in the manner of Faraday with his wires and magnets (cf Skinner 1956:101; Cardwell 1972:147-8).

Toulmin (1972:173) gave a formulation of scientific knowledge, from which that presented in this chapter is derived:

"The empirical knowledge that a scientific theory gives us is always<sup>1</sup> the knowledge that some general<sup>2</sup> procedure of explanation, description or representation<sup>3</sup> (specified in abstract, theoretical terms<sup>4</sup>) can be successfully<sup>5</sup> applied (in a specific manner, with a particular degree of precision, discrimination, or exactitude<sup>6</sup>) to some particular class of cases<sup>7</sup> (as specified in concrete, empirical terms)".

The quotation is annotated for the comments below, which form a prelude to a revised formulation. Lest it be thought that these revisions are undertaken in an excessively cavalier manner, it should be noted that Toulmin gave no elaboration or justification of the details in question.

1. It seems unfortunate that Toulmin, after taking some trouble to repudiate the interpretation of scientific laws as 'general laws', should have phrased his philosophy of science in this form, 'A is always B', which itself invites interpretation as a general law in which A and B may be independently identified. This point will be rephrased in existential form.

2. In this qualification Toulmin was implicitly supplying a standard for scientific knowledge: that the procedure in question have some

(unspecified) degree of generality, inclusiveness or transfer to other situations. Such a standard may be useful in the critical comparison of rival theories, but for the sake of simplicity of formulation this issue will be left open, to be dealt with should it arise in a particular case.

3. The juxtaposition of these three terms 'explanation, description or representation' suggests that as categories they are mutually exclusive or at best only partially overlapping. However Toulmin (eg. 1972:192-199) did not insist on this, and it is convenient to follow what appears to be his usage of allowing 'representation' to include both 'description' and 'explanation'.

4. Toulmin did not clarify this requirement. It may be better set aside as a standard for the specification of procedures of representation, but even so one wonders what is gained by insisting that the specification be in 'abstract, theoretical' terms.

5. Standards of success vary with the 'state of the art' in any field of science; one would need to know what would count as an unsuccessful application of a procedure of representation before this qualification had any force.

6. These points are conveniently considered subsequent to the primary formulation. For example, one can know that there exist phenomena (in particular the motions of the planets) which have been represented by the techniques of Newtonian mechanics, without knowing how those particular phenomena were represented, or the degree of precision attained. Likewise, one can know that there exist phenomena which have been represented in terms of geometrical optics and which conform to Snell's Law of Refraction, without knowing the refractive indices of any materials or the extent

of any deviations from Snell's Law.

7. The specification of a class of cases is appropriate as a qualification to a general law, but may be dispensed with in the context of an existential statement which stands independently of such qualification. For example, the general law 'All swans are white' requires the qualification that it applies to swans native to the Northern Hemisphere, whereas the existential statement 'There exist white swans' requires no such qualification. Here it seems that Toulmin was suggesting that the specification of particular classes of cases to which the law applies, is an intrinsic part of a rigorous formulation of a scientific law. The problems created by this suggestion may be illustrated in terms of Toulmin's (1953:63) own example of such a specification:

"Snell's Law has been found to hold under normal conditions for most non-crystalline materials of uniform density".

With ill-defined qualifications such as 'normal conditions' and 'most', this fails to specify definitely any class of cases to which the law applies. Even the qualification 'of uniform density' is problematic, for no substance is of uniform density when examined on an atomic scale. Further, Toulmin's above suggestion appears to be contrary to his (1953:78) statement that the routine research which discovers the various substances and circumstances to which a scientific law can be applied "can in no way be said to call in question the truth, or the acceptability, of the law itself". This relative independence of the truth of the law from any specification of classes of cases to which it applies will be represented below in the distinction between 'primary knowledge' and 'secondary knowledge'.

In the light of the above comments, the following revision of Toulmin's formulation is proposed as a

Galilean ideal form of scientific knowledge:

There exist phenomena which can be represented by a specified technique so as to conform to specified principles.

Such knowledge will be called 'primary'.

A secondary type of scientific knowledge will also be mentioned; this derives from the primary knowledge by way of specifying the phenomena which have been shown to conform to the specified principles, the particular representations of those phenomena, and the extent of their deviations from the principles.

It might be objected that the above formulation is in some way circular or tautologous. We may note two indications that theories conforming to the proposed formulation are in general empirical rather than tautologous. Firstly, the logical form proposed is similar to that of 'There exist white swans', which is empirical regardless of whether whiteness is a defining attribute of swans - in contrast to that of 'All swans are white', which is indeed tautologous if whiteness is a defining attribute of swans. Secondly, it is argued below (s2.5) that certain classical scientific theories are readily stated so as to conform to the proposed formulation. If it be accepted that those theories are appropriately thus represented, a criticism of the proposed formulation as tautologous or circular would be no criticism at all, for the aim of the formulation is to properly represent the indubitably scientific theories. One is at liberty to advance a usage of the terms 'circularity' or 'tautology' which applies to Newtonian mechanics, but the terms would thereby lose their critical sting.

Techniques of representation are essential to the above formulation of Galilean science. As one learns science, each technique becomes a mode of perception (cf Hanson 1970). Thus the above formulation

converges with Bohm's (1965:219) account of the role of perception in science:

"...scientific investigation is basically a mode of extending our perception of the world, and not mainly a mode of obtaining knowledge about it. That is to say, while science does involve a search for knowledge, the essential role of this knowledge is that it is an adjunct to the extended perceptual process" (original emphasis).

In similar vein Skinner (1969:103) noted that, having identified the principles of behaviour under relatively simple conditions, one extrapolates to the relatively complex "to begin for the first time to see it in a new light".

Toulmin (1953:21) noted that new techniques of representation, new ways of looking at the phenomena, raise new questions. Thus the learning of science involves learning to ask the questions (eg. 'What forces are acting on this object?') raised by various theories. This view of education, emphasising perception and questions rather than information, converges with that of Postman and Weingartner (1969).

Some techniques use words only, but most make use of diagrams or graphs of some form. The role of diagrams in science was discussed by Toulmin (1953: 17-39) and by Hanson (1970). One unusually clearly-interpreted diagram of an unusually complex set of interactions was in Meadows and Meadows (eds 1973:16,37). The scientific use of notation systems, in conjunction with a notation system for behaviourism, was discussed by Millenson (1967:117-137). Further considerations in the proper scientific use of diagrams will be raised below (s4.3).

## 2.5 Exemplars of Galilean scientific knowledge

The three examples of scientific 'laws' discussed

in Chapter 1 are readily represented as Galilean principles, each within a theory conforming to the Galilean ideal of primary knowledge.

Ayer's version of Mendelian genetics may be formulated thus:

There exist cases of inheritance of traits which can be represented as the segregation and recombination of dominant and recessive genes so as to conform to the principle:

When there are two genes determining a hereditary property, the proportion of individuals in the second generation that display the dominant attribute is three quarters.

This existential statement is correct: there are such cases of inheritance. However it scarcely does justice to the scope of Mendel's laws, for Ayer has put up what is but a special case. More generally, Mendelian genetics may be formulated (cf Stansfield 1969:8):

There exist cases of inheritance of traits which can be represented as the segregation and recombination of dominant and recessive genes, so as to conform to the principles:

1. From any one parent, only one gene is transmitted through a gamete to the offspring (Principle of segregation).
2. The segregation of one gene pair occurs independently of any other gene pair (Principle of independent assortment).

The secondary knowledge of Mendelian genetics consists of a catalogue listing traits which conform to the principles, specifying the dominant gene for each and any deviations from the principles such as those arising from differential viabilities of the genotypes.

Ayer's special case arises only when the first mating is between heterozygous individuals which carry one each of two genes (alleles) for that trait, and then only when the viabilities of the three resulting

genotypes are equal, which is not always the case. None of these considerations were mentioned by Ayer.

Charles' law may be formulated:

There exist gas phenomena which can be represented in terms of volume and absolute temperature, so as to conform to the principle:

The volume of a fixed mass of gas is directly proportional to its absolute temperature.

The secondary knowledge may be presented as a catalogue of phenomena, their interpretations in terms of Charles' law, and graphs of volume versus temperature for various gases at various temperatures so as to display deviations from the principle. However in modern practice Charles' law is usually integrated with the related principles known as Boyle's law and Avagadro's hypothesis, to form the principle

$$P.V = n.R.T$$

known as the ideal gas law (cf Barrow 1961:8). The secondary knowledge is usually presented in terms of this more general principle.

Newton's laws of motion are among the principles of Newtonian mechanics, which may be formulated (cf Schaum 1961:35f):

There exist interactions between bodies which may be represented by the technique of free-body diagrams so as to conform to the principles:

1. A body which continues at rest or in uniform motion along a straight line has no nett force acting on it (Newton's first law.)

2. A body of mass  $\underline{m}$  with an acceleration  $\underline{a}$  has a nett force  $\underline{F}$  acting on it, such that

$$F = m.a$$

and the force is in the same direction as the acceleration. (Newton's second law.)

3. For every force on one body, there is an

equal and opposite force on another body.  
(Newton's third law.)

In a free-body diagram, one body is drawn as if isolated from the others, and the actions of other bodies upon it are represented as forces (cf Jammer 1957:244). This technique will be elaborated below (s3.1) by way of pointing out similarities with Skinner's representation of behavioural interaction.

We may note that, according to Ellis (1965:36-39), Newton's formulation of his laws of motion was not in terms of 'force' and 'acceleration' but rather in terms of 'impulse' and 'change of momentum'. The formulations are equivalent, as Ellis showed.

The other principles of Newtonian mechanics are those which specify particular forces. Newton's applications of his mechanics to celestial and projectile motions were done using the principle known as the law of universal gravitation:

4. A pair of bodies of masses  $\underline{m}$ ,  $\underline{m}'$  separated a distance  $\underline{d}$  attract each other with a force  $\underline{F}$  such that

$$F = G.m.m'/d^2$$

where  $\underline{G}$  is the universal constant of gravitation.

Other such principles have been added to Newtonian mechanics, many by later workers, relating to forces such as those of friction, bouyancy, elasticity, fluid resistance, aerodynamics, electrostatics and magnetism.

The secondary knowledge includes an encyclopedaic diversity of phenomena which have been represented as conforming to these principles, together with the magnitudes of constant quantities occurring in the formulae and any discrepancies between the representations and the phenomena. One such phenomenon is the orbit of the moon about the earth. For a relatively simple

analysis (representing the earth and moon only, and as point particles) the constants are  $G$  and the masses of the earth and the moon; the discrepancies are the deviations of the moon's orbit from the elliptical orbit inferred from the principles and that representation. (A fuller analysis would include representations of the sun and of the shapes and rotations of the earth and moon.)

Newtonian mechanics and in particular the laws of motion have defied attempts to assimilate them to any of the current theories of the nature of scientific knowledge (cf Hanson 1965b; 1965c; Ellis 1965; Hanson and Feyerabend in Michalos ed 1974:68-75). For example the laws of motion and their key terms 'mass' and 'force' will not lie down on either side of the dichotomy between observation and theory; as Ellis (1965) showed, they partake of both categories without fully conforming to either.

This unsatisfactory state of affairs is often overlooked, with curious results. For example Jeffrey Gray (1975:1) labelled psychoanalysis as unscientific on the grounds that it fails to preserve the distinction between observation and theory. Yet by the same criterion Newtonian mechanics is also unscientific - which is absurd. The purported criticism merely succeeded in showing a similarity between psychoanalysis and Newtonian mechanics, which hardly casts doubt on the scientific status of the former. To succeed in criticising some theory as unscientific, while recognising the indubitably scientific status of Newtonian mechanics (s1.2 above), one must at least show some point of difference between the two theories. This will be attempted below (s2.9) for psychoanalysis.

To the extent that the above formulation of Newtonian mechanics can accommodate the various

functions of the laws of motion, without being systematically misleading (cf Ryle 1932), it may be a substantial advance in the philosophy of science.

## 2.6 Revolutionary science

For the present discussion this may be characterised as the generation of new primary knowledge. Its apparent irrationality was summed up by Schopenhauer (cit Smolicz 1970:110):

"A scientific truth goes through three stages of development. In the first phase it is rejected as absurd. In the second phase it is admitted as a possible hypothesis which has been suggested many times before. In the third phase it is accepted as self-evident".

Schopenhauer's irreverent view of scientific progress may not be definitive, but it may serve as a first approximation to the more rigorous accounts of major scientific advances, the Copernican Revolution in particular, presented by Feyerabend (1975), Koestler (1959) and Kuhn (1957; 1970). All three agreed that scientific revolutions do not conform to any current theory of rationality or scientific method: Feyerabend's theme was methodological anarchism, Koestler adopted a metaphor of sleep-walkers, and Kuhn emphasised the sociological process of spreading a new belief.

One method of intellectual progress which does not appear to have been explored as a theory of revolutionary science, even by its principal proponent, is de Bono's 'lateral thinking'. De Bono himself (1971:172) subscribed to an apparently Popperian philosophy of science, which he mentioned only by way of contrast to the field of practical thinking in which he advocated the technique of lateral thinking.

De Bono proposed a variety of strategies for lateral thinking; following his 'intermediate impossible'

(1973:90-98) we may propose the following for revolutionary science:

To create a new theory, identify the principles of the old theory and devise principles contrary to them. These will appear absurd or impossible to anyone schooled in the old theory. Then seek exemplars of the principles, elaborating and revising the principles in the process.

It is difficult to find exemplars of this ideal of revolutionary science, possibly because the Aristotelian concern with logic and proof (cf Toulmin 1958) has inhibited the process (cf Watkins 1964). However, moving to the level of prescriptive philosophy, we may construct an exemplar by way of a 'rational reconstruction' (Lakatos 1971) of the Copernican Revolution, with Aristotelian mechanics as the old theory. Among the principles of Aristotelian mechanics were the following (cf Clagett 1963:87f, 208; Feyerabend 1961:23-25):

1. The earth is the centre of the universe.
2. Celestial and terrestrial motion conform to different principles.
3. The natural motion for celestial objects is uniform circular motion.
4. Forces arise only between bodies which are in contact.
5. A body with a constant force acting on it moves at constant speed.

Proceeding accordingly to the strategy above, we might propose the following principles as the basis for a new theory:

1. The earth is but one of the planets in orbit about the sun.
2. Celestial and terrestrial motion conform to the same principles.

3. The natural motion of all objects is uniform rectilinear motion.
4. There is a force between every pair of objects in the universe.
5. A body with a constant force acting on it moves with a constant acceleration.

The strategy may be extended to the principles implicit in the technique of representation of the old theory. For example, before Galileo and in his earlier analyses of the velocities of a falling body, velocity was represented as a function of distance (Hanson 1958: 37-49). Following the strategy above, we might propose to represent velocity as a function of time.

These proposed principles are indeed the principles of Newtonian mechanics. It is of course rather easy to perform the exercise when one knows the optimal product; anyone following the strategy before Newton would expect to make many false starts, and to engage in much random variation and selective retention of the principles (cf D.T. Campbell 1974) as allowed for in the formulation of the strategy.

At the prescriptive level of this ideal of revolutionary science it is suggested that, had the ideal been followed, Newtonian mechanics would have been developed more rapidly and directly than actually happened. The bulk of the work done in the course of the Copernican Revolution was characterised more by a blind loyalty to the old principles, especially uniform circular motion (cf Koestler 1959), rather than by an active generation of principles contrary to the old ones as prescribed in the strategy above.

More generally, lateral thinking involves having a variety of ideas, without necessarily being able to justify them, and trying out any that look even vaguely promising. This corresponds to Feyerabend's (1975: 1-54) 'principle of proliferation', except that

Feyerabend gave relatively little account of how to try an idea out. Some account of this may be found in his (1964:305) distinction between 'respectable thinkers' and 'cranks'. W.T. Williams (1964) gave an example from botany of the importance of 'having an idea' and the pressures from journal editors to pretend one got it by logical deduction.

A second ideal of revolutionary science takes the form of experimental research which is not committed to a particular theory. While it may start off using the technique of representation of some existing theory, it generates or discovers new principles and often develops new techniques of representation as well. It may be formulated:

Given a particular phenomenon hitherto regarded as unpredictable, the investigator seeks out the variables of which the given phenomenon is a function, demonstrates the functional dependences (which are formulated as principles), and displays them as graphical curves.

The formulation is derived from that which Skinner (1931:440) gave as an account of the usual course of reflex investigation. His exemplar was Pavlov's well-known work on salivation in dogs; for a Pavlovian functional dependence displayed as a smooth experimental curve see Keller and Schoenfield (1950:26). Skinner's (1956) account of his own early research also conforms to this ideal, but for the deviation that many of the variables, rather than being actively sought, originated through experimental convenience or as apparatus faults. A more classic exemplar of the ideal is Newton's research on the spectrum produced by a prism, as described by Cardwell (1972:50-51).

This revolutionary experimental research generates and demonstrates its principles in the one process. A

demonstrated principle, such as Pavlov's extinction of a conditioned reflex, may be called a 'fact' in accord with Skinner's (1969:84) usage:

"Unlike hypotheses, theories [i.e. general laws] and models, together with the statistical manipulations of data which support them, a smooth curve showing a change in the probability of a response as a function of a controlled variable is a fact in the bag, and there is no need to worry about it as one goes in search of others. The shortcomings and exceptions [i.e. deviations] will be accounted for in time".

The concept of 'demonstration' will be further developed below (s2.9).

It may be objected that there are philosophical problems in the established usages of the term 'fact', which prevent its being legitimately equated with the term 'demonstrated principle' as used here. However it is not implied that the proposed usage of 'fact' does conform exactly to, or solve all the problems of, the various established usages. It is, rather, proposed that the term be given a revised and single usage within the Galilean paradigm. The only implication concerning the established usages of 'fact' is that they are in general similar to the proposed usage to the extent that there is more positive than negative linguistic transfer of learning from them to the proposed usage. (If negative transfer dominated, it would be preferable to coin a new term.) Such revision and simplification of word usages is common practice in the development of theories in physical and other sciences. Examples from Newtonian mechanics are the terms 'force' (Jammer 1957), 'mass' and 'work' which have various common usages predating Newton; part of the task of learning Newtonian mechanics is to learn the scientific usages of those terms as distinct from the established common usages. Likewise in zoology the term 'fish' is no longer used to

refer to whales and dolphins. Thus the proposal for a revised usage of the term 'fact' is well within the scientific tradition.

To clarify the term 'controlled variable' (in the above quotation from Skinner) in relation to the conventional dichotomy between dependent and independent variables, we may note that probability of a response is the usual dependent variable in Skinnerian experiments, while the controlled variable is the independent variable which is varied by the experimenter during the course of the experiment, unless of course the independent variable is simply time. (An elaboration of this is the methodology of systemic replication, cf s2.9 below)

This usage of 'control' should not be confused with the term 'statistical control' in the usage of R.A. Fisher, whose methods were tried and rejected by Skinner (1956, 1969) for reasons which deserve wider attention than they seem to have received. (While Skinner avoided statistical analyses after those early trials, Revusky (1967) has argued for a limited role for a proposed statistical treatment in Skinnerian research.)

The two usages of 'control' are so contrary as to seriously confound discussions of scientific method. When a variable is controlled in Fisher's sense, it is ideally held constant, while if that is not possible the measurements are corrected for the effects of that variable so that it may be treated as constant. When a variable is controlled in Skinner's sense, it is deliberately varied by the experimenter so that its effects may be seen as changes in the dependent variable. The contrast is reflected in two approaches to the study of animal behaviour. In one approach the aim is to observe without disturbing or altering the behaviour observed, by (in Fisher's sense) controlling effects of the observer's presence. The other approach has been

roughly epitomised 'to understand your animal, poke it". The aim is to identify the variables which alter the animal's behaviour, by (in Skinner's sense) controlling any variables thought to be capable of altering the behaviour.

Fisher's methodology seems to be the basis for the overwhelming bulk of 'social science' research, so a critique of his methodology is relevant to the growing number of strikingly outspoken protests, even in academic journals, against the modern institution commonly known as 'social science'. Two critiques of the general unscholarly nature of the institution were by Sowell (1976) and Weigert (1970). The stifling effects of the modern insistence upon detailed prior designs of research even in physical sciences, and its divergence from the way most of the great classical research was done, were noted by Richter (1953). The alarming possibility that an unknown and possibly very high proportion of published results in experimental psychology are 'false positives' was argued by Sterling (1959; cf Becker 1970:20f). Derek Phillips (1973:121f), after reviewing both empirical and philosophical objections to the usual research methods in sociology, asserted:

"It is time we [sociologists] turned away from our vulgar imitations of what we mistakenly think physical scientists do and looked in other directions..." (emphasis added).

Other objections to the modern obsession with statistical tests were raised by Lykken (1968) and by Meehl (1967); Lakatos (1970:176n1) after reading Lykken and Meehl "wonders whether the function of statistical techniques in the social sciences is not primarily to provide a machinery for producing phoney corroborations and thereby a semblance of 'scientific

progress' where, in fact, there is nothing but an increase in pseudo-intellectual garbage", and hints at "laws for stemming this intellectual pollution which may destroy our cultural environment even earlier than industrial and traffic pollution destroys our physical environment". Reading Chomsky's (1969) account of the role of the 'New-speak' that passed for social science in the attempted Westernisation of Vietnam and similar American contributions to human culture, one feels that Lakatos' fears are not without foundation.

It may be objected that the Galilean account of science is incomplete in that it gives no account of scientific explanation or of causation. Such an objection would presuppose that explanation (as distinct from description) and the identification of causes are characteristic of science. However there is reason to believe that both these presuppositions are false. Toulmin (1953:10) noted that the word 'cause' rarely appears in the writings of professional scientists, and for good reasons. Skinner (1931: 448f) treated the matter thus:

"...a scientific discipline...must describe the event not only for itself but in its relation to other events; and, in point of satisfaction, it must explain. These are essentially identical activities...we may...take that more humble view of explanation and causation which seems to have been first suggested by Mach and is now a common characteristic of scientific thought, wherein, in a word, explanation is reduced to description and the notion of function substituted for that of causation. The full description of an event is taken to include a description of its functional relationship with antecedent events".

This seems to describe Newtonian mechanics rather well, with the proviso that as some mechanical interactions (eg. gravitational attraction) are simultaneous, the term 'antecedent events' might be better replaced by 'antecedent or simultaneous events'.

However the notion that explanation by 'reduction' is essential to science is persistent and needs further clarification. It is true that some phenomena, such as the behaviour of gases, can be 'explained' in terms of their fine structure as particles in motion, which is to say that the gas laws can be 'reduced' to particle mechanics. Such reductionist explanation is presented as the essence of science in what Keat and Urry (1975:27-45) called the 'realist philosophy of science', noting (p40) that such a philosophy fails to represent Newtonian mechanics and theories of similar form such as the ideal gas laws. Considering the status of Newtonian mechanics amongst scientific theories, one may suggest that any philosophy of science which cannot give an account of it has little claim to be taken seriously as a general view of science (as distinct from an account of a particular variety of scientific theory).

Reductionist explanation may be represented in terms of Galilean science thus: the facts (demonstrated principles - cf above) of gas behaviour are among the phenomena interpreted by the theory of particle mechanics. However the demand for such reductionist explanation as a criterion for science is ill-founded; the classic counter-example is Newton's unproductive search for a reduction of gravitational attraction to contact-forces between particles (Kuhn 1957:259). It is ironic that modern physics has gone the other way, representing contact-forces as forces acting at a distance between subatomic particles, in conformity to the principles of electromagnetic forces.

Skinner's arguments against reductionist theories in psychology are in his (1950) and (1956b).

Two types of revolutionary science are described above: theoretical and experimental respectively. In

later chapters much will be made of the role of experiments in science. However it should not therefore be thought that only experimental science conforms to the Galilean ideal. (Galileo himself, by some accounts, was not so much an experimenter as a theorist who presented his theories in the form of 'thought-experiments'.) It will be argued below (s2.9) that experimental demonstration of the principles of a theory provides a strong form of support for the theory. However it is not necessary that a theory should have this support; one might prefer to settle for a notably successful contribution to the relevant technology (which includes clinical behavioural practice - cf s2.8 below) as sufficient justification for a theory. One could even argue that at least some of the useful revolutionary science which has in fact been experimental could have been done, at greatly reduced expense to the public purse, by revolutionary theorists in interaction with practising technologists (cf s2.7 below). However the major function of the philosophy of science in this thesis is to provide a basis for defending behaviourism against the criticisms levelled by ethologists and others. Skinnerian behaviourism happens to have developed as a largely experimental science, so a focus upon the logic of experimental science is appropriate for a defence of behaviourism.

### 2.7 Normal science

Normal science may be distinguished from revolutionary science as research which generates the secondary knowledge of a theory (s2.4 above) while leaving the primary knowledge unchanged.

Kuhn (1970) used the term to refer to the puzzle-solving within a paradigm that occurs between 'scientific revolutions'. This appears to be

equivalent to the present usage, if we allow that to solve a puzzle within a paradigm is to show how a given phenomenon conforms to the principles of a given theory, by applying the technique of representation and identifying any deviations from the principles, allowing also for interactions between principles. The product of such puzzle-solving is of course secondary knowledge as formulated above (s2.4).

For a sample of secondary knowledge we may turn to any science data book and find tables such as 'Properties of Inorganic Compounds' (Tennent ed 1971:83). Each row of this particular table is labelled for one compound, such as CaO (quicklime). Each column is labelled for one property, such as Refractive Index (n). Thus we may read off the datum that the refractive index of CaO is 1.837. This conforms to the ideal of secondary knowledge in that it indicates that the phenomenon of refraction of light by crystals of pure quicklime has been represented by the technique of ray diagrams so as to conform to Snell's Law, with the particular representation  $n=1.837$ . In this case no deviations from the principle have been noted.

While this data has its uses as noted below, those used are so restricted as to cast some doubt on the modern preoccupation with gathering it. One is tempted to see the diligent research which produces such data as one of the excesses of "the passionless pursuit of passionless intelligence" (cf Jack London, cit. Rossen 1969:70), and as an instance of the Law of the Instrument (Kaplan 1964:28): "Give a small boy a hammer and he will find that everything he encounters needs pounding". Give a 'normal' scientist a theory and he will consider it self-evident that every amenable phenomenon needs to be represented in its terms and have the resultant secondary knowledge recorded for posterity. This

compulsive accumulation of data may have gained respectability from its conformity to the Baconian ideal, the dangers of which were pointed out by Brush (1976).

Except as noted below, such data can easily be misleading. To pursue the quicklime example: suppose one did wish to design an optical system involving quicklime, it would be risky to use the data-book value for the refractive index instead of measuring a sample of the quicklime that will be used in the system. Refractive index and most other properties vary with the purity of a substance, and outside of research laboratories one seldom uses the highly purified substances whose properties are recorded in data-books. The one unproblematic use of such data is not as a substitute for observation in a particular situation, but as an aid to it by way of providing criteria for identifying unknown substances or checking the purity of known ones. Perhaps this is the justification for generating data such as the refractive index of quicklime.

In view of these considerations it seems that the view of science as a source of certain knowledge that serves as a substitute for perception is ill-founded. This view seems to be held by Steiner (1974) and by the scientists who rejected a technique for using pigeons to guide missiles (Skinner 1960:584). Among the more readily digestible antidotes is Thomson's (1965), in which in addition to various pragmatic examples he wrote (p87):

"I believe the desire for certainty is a vice. People are certain, but usually when one knows that people are certain one is pretty certain that what they're certain about is wrong. And certainty in any philosophical sense seems to me to be almost obviously absurd. It's contrary to all one's common experience that you can ever hope to foresee everything that can affect the situation".

As with many doctrines of dubious validity, the persistence of the doctrine that science is a source of certain knowledge, may be understood in terms of its political usefulness in maintaining the authority of the ruling establishment. The function of Aristotelian knowledge as a dogmatic substitute for perception, as opposed to Galilean knowledge as a source of diverse representational techniques to aid perception, was indicated by John Fletcher (1977:33; cf Krishnamurti 1972):

Every authoritarian society divides itself as it divides the individual, into alienated halves. Those at the bottom suffer from what I shall call nescience. The natural sensory activity of the 'nescient' - what the person sees, hears, smells, tastes, feels, and, above all, wants - is always irrelevant and immaterial. The authoritarian logogram [Aristotelian knowledge], not the field of sensed experience, determines what is relevant and material. This is as true of the highly paid advertising copywriter as it is of an engine lathe operator. The person acts, not on personal experience and the evaluations of the nervous system [perception], but on orders from above..." (original emphasis).

This pattern is realised in the attitudes of the all-too-common 'scientific expert' who 'knows best what should be done', regardless of what the people with direct practical experience of the problem might have to say. Such is the 'expertise' consistent with knowledge represented as a set of general laws of the Aristotelian type: the expert has only to recognise a swan, and he knows that it must be white. Any apparently black swan may be dismissed as irrelevant and as the product of malevolent trickery (cf the initial reactions from British scientists to reports and preserved specimens of the newly-discovered platypus in Australia). The ideal Galilean expert, by contrast, shows his scientific knowledge upon meeting a practical problem by asking questions and representing

the phenomena in terms which had not occurred to those with the practical experience of the particular problem in question - while making full use of their reports of the phenomena. Indeed the phenomena of greatest interest to a Galilean expert include the results of the various attempts already made to solve the problem; his interaction with the practical people involved is ideally one of learning from them and proposing tentative suggestions, rather than a cursory or independent study and issuing directions. (In practice the attainment of this ideal requires that the practical people are nearly as open-minded and free from Aristotelian dogmatism as is the expert, which is not to be expected in view of the conservative nature of craft traditions - cf Toulmin & Goodfield (1962:36f).) But we encroach on the topic of the next section.

More generally, the distortions of science in modern authoritarian societies (cf Melkin 1976 on biology textbooks in America), and the dependence of progress in Galilean science upon libertarian attitudes and practices, were major themes of Feyerabend (1975).

This is not to decry all normal science. However, the institution of pure science (as distinct from technology) in general, and pure normal science in particular, has attained in Western society a level of social prestige, intellectual dogmatism and consumption of material resources which other cultures allow only to their official religion. Comparing this with the secondary and modest role of normal science within the Galilean ideal, we may suggest that modern science has departed from the Galilean ideal by overemphasising and distorting the process of normal science.

It will be argued in the next section that technology also has the function characteristic of normal science, that of generating secondary knowledge,

while normal science sometimes leads to technologically useful innovations. Thus the terms 'technology' and 'normal science' do not designate mutually exclusive categories of social phenomena. Rather, they identify two representations of or ways of looking at a variety of social phenomena. One emphasises the generation of secondary knowledge; the other emphasises practical action. Either, both or neither may be applicable to any given case. The 'pure normal science' of the previous paragraph may be identified as scientific activity in which the goal of generating secondary knowledge dominates to the extent that any practical problems to which the work might be relevant have little effect upon the work actually done (as distinct from the wording of research proposals submitted to funding agencies). This view allows that pure normal science may still lead to and assist practical action; the characteristic feature is that this happens, if at all, more by good luck than good management. In technology, by contrast, effective practical action is the dominant aim of the exercise.

## 2.8 Technology

Technology seems to be neglected or but cursorily mentioned in most of the literature of philosophy of science; when it is mentioned it is usually in the context of ethics or the abuses of technology rather than any theory of the nature of technology. In the collection edited by Michalos (1974) only one paper is of the latter type and that ended with a lament that so many problems in the nature of technology were still "waiting to be discovered and worked out by philosophers attentive to their own times" (Bunge 1974:46). The two major sources for this section, Homans (1951) and Cardwell (1972), neither are primarily philosophical nor

seem to have received much attention in the philosophical literature.

Homans (1951:15) distinguished between 'clinical science' and 'analytical science', corresponding to the terms 'technology' and 'science' respectively as used here. He mentioned medical practice in cases of anaemia as an exemplar of technology, with a theory of blood chemistry as an exemplar of science. (Medical practice as a case of technology conforms to the wider usage of the term 'technology' proposed in the preface. This may be contrasted with the narrower usage where 'medical technology' refers only to the application of physics, chemistry and other sciences to the design and servicing of medical hardware.) A theory "picks out a few of the factors at work in particular situations [this selection is intrinsic to the technique of representation] and describes systematically the relations between these factors [in the principles of the theory]". Technology by contrast cannot afford such selection. Its primary aim (with apologies to Karl Marx 1888:30) is to change the phenomena, not to write a report about them (cf Ziman 1968:23f). Homans emphasised that in medical practice any factor, no matter how obscure or irrelevant it seems at first, may prove to be important in effecting a change; that this can be equally true in mechanical engineering is apparent from Welbourn's (1964) case-study. Technology escapes the selective perception induced by any one theory, by using a variety of theories concurrently. Each theory picks out different factors or variables, thus making its own suggestion as to where the technologist 'should look more closely' as Homans put it.

The techniques of representation and the principles which constitute primary knowledge are to be found in the diagrams, graphs, equations and technical terms which constitute as much a part of technological communication,

formal and informal alike, as is the Queen's English. As a technologist learns to use these, they become integrated into his very perception and imagination (cf Hanson 1970; Petrie 1976; Ferguson 1977; s2.4 above).

When technology was harmful effects such as air pollution, it is all too easy to put the blame on 'too much science'. On the contrary, we might follow Foecke (1970; cf Bohm 1970:162) in blaming the over-specialized education of engineers, in which they did not learn the theories which would have led them to look more closely at just those effects of their projects which have subsequently proved so damaging. Such theories are to be found in the biological sciences and particularly in ecology, for example the theory of food-chains in relation to high levels of DDT in mothers' milk.

Ideally, as in Homans' brief account, technologists and scientists help one another. The technologists tell the scientists what the latter have left out. The scientists "need the most brutal reminders because they are always so charmed with their pictures they mistake them for the real thing" (1951:15). However it is difficult to find cases of scientists actually acting on such guidance from the lower orders, which fact (if it be so) invites interpretation in terms of the social class structure of scientists (Hudson 1972:53-56). One instance may be Darwin in that he "talked with expert breeders of animals and plants, and sent lists of questions to everyone who might have useful information" (Downs 1956:165; see Ferguson 1977 for more examples). It appears to be common enough for scientists to adopt technological phenomena for study, as Sadie Carnot the pioneer of thermodynamics did with steam engines (Cardwell 1972:129-138); sometimes they show embarrassingly clear signs of not availing themselves of the intuitions of practical people, as in the case of

Parent's simplistic analysis of waterwheels and his ill-founded recommendation that undershot wheels should be installed in preference to the overshot type.

Cardwell's (1972:62-65, 78-82) account of the dogmatism of Parent and other Newtonians of the time is an object-lesson in how not to interpret scientific knowledge (cf s2.7 above). While it seems unlikely that Newton himself would have condoned such dogmatism, the conventions of his time led him to formulate his Principia Mathematica as axioms and formal deductions in the manner of Euclid's geometry which was the current exemplar of certain knowledge. However appropriate it might be for geometry, that format seems to be systematically misleading (in the sense of Ryle 1932) if the dogmatism of many of Newton's followers is any indication. Thus the frequent mention of Newtonian mechanics in later chapters should not be interpreted as a sort of worship of Newton and his Holy Book. Rather it is recognised that, notwithstanding the misleading format of Principia and the misguided dogmatism of some Newtonians, there are many outstandingly successful applications of Newtonian mechanics. Implicit in those applications is an interpretation of Newtonian mechanics; it is that interpretation which is adopted here as an exemplar or standard of scientific knowledge against which we may compare abstract formulations of science, criteria for the scientific status of theories, and specific theories whose status is in question. It is not the easiest of standards to apply, for the interdependence of history and philosophy of science is involved and some criterion for progress such as that proposed by Lakatos (s2.2 above) would be required for a rigorous discussion. (In this thesis the discussion has been simplified by taking certain history of science and formulations of theories such as Newtonian mechanics as

given; if asked to justify them one would do so using Lakatos' criterion.) However what this standard lacks in convenience it gains in validity, for to challenge it would be to argue that under no interpretation is Newtonian mechanics good science.

The roles of Galilean science in technology may be illustrated by an outline of the early development of the steam engine. The development falls into three phases: (1) the invention of the Newcomen engine, (2) Smeaton's fine tuning of it, and (3) Watt's design which succeeded it.

Cardwell (1972:50-59) showed that the invention of the steam engine occurred as a result of the Copernican revolution, and in particular of the acceptance that terrestrial and celestial mechanics follow the same principles (cf s2.6 above). Air could no longer be regarded as a substance filling all space (as in the saying 'nature abhors a vacuum'), for the motions of the earth and the planets were evidently not retarded by air resistance, neither was there an irresistible east wind corresponding to the earth's rotation. Thus it followed that the body of air must be finite and local to the earth; in other words 'the atmosphere was discovered'. Thus it became reasonable to represent the air in terms of Archimedian fluid mechanics, as a finite ocean of fluid with a pressure at the earth's surface analogous to that at the bottom of an ocean of water.

Early successful applications of this theory of the atmosphere, in the manner of normal science, included an interpretation of the operation and the well-known 10-metre working limit of 'suction' pumps. A consequent technological development was the invention of the mercury barometer. Various devices of no particular usefulness were invented, in the manner of normal science, which demonstrated the pressure of the atmosphere; in

particular Von Guericke's apparatus (Cardwell 1972:55) which may be seen as a vacuum version of a modern hydraulic hoist. A large cylinder fitted with a piston was evacuated by a small hand-operated air pump; the pressure of the atmosphere moved the piston with an impressive force which was demonstrated by lifting heavy weights.

Now at this time (c.1670) the terms 'force' and 'power' had not acquired their modern and clearly distinguished usages (Cardwell 1972:55n; Jammer 1957), so the presumption was that the atmosphere was a new source of both force and power. The problem in harnessing this source was to find a more convenient way of evacuating the large cylinder. Some twenty years later a solution was found: the cylinder was filled with steam, which was then condensed by a jet of cold water leaving a fairly good vacuum. This working principle was put to use first in Savery's engine of 1699, and then in Newcomen's more successful engine of 1712. The principle may be formulated:

Steam is admitted to a cylinder, allowing the piston to rise; the steam is condensed by admitting cold water, leaving a vacuum; atmospheric pressure drives the piston down giving the power stroke.

The first phase illustrates the effectiveness of a new representational technique, in conjunction with a few principles, applied to familiar phenomena even in the midst of conceptual confusion. Newcomen's engine obtained power not from the atmosphere but from the burning of coal; the atmosphere functioned in the manner of a weight lifted up by the steam and then releasing that stored energy as mechanical work as it returned to its starting-point. Lateral thinking and in particular the value of an 'intermediate impossible' (de Bono 1973:90-98) is also illustrated. While earlier attempts to obtain

practical power from fire and steam had failed (Cardwell 1972:56), the attempt to gain power from atmospheric pressure, while theoretically impossible, led to practical attainment of the objective that had defied more obvious methods.

The second phase was John Smeaton's research (Cardwell 1972:82f) to reduce the fuel consumption of the Newcomen engine, which was so high that it was hardly worth using the engine except where coal was freely available from nearby mines (cf Cardwell 1972:88). Smeaton built a model engine and represented its performance in terms of power output, and efficiency (as work done per unit of coal burned). He systematically varied the procedures of operation to find those which gave maximum power and maximum efficiency, which happened to occur together. The optimum procedures were that some of the steam be left uncondensed in the cylinder, and that some accumulation of air in the cylinder be allowed. Both of these procedures worked against the engine's working principle of producing a vacuum, and were no doubt anathema to purists such as Savery (cf Cardwell 1972:68f). The anomalous fact remained: Smeaton's procedures nearly doubled the efficiency of the engine, but his research gave no clue as to why those particular procedures were the best. This phase, in contrast to the third, illustrates the limitations in technology of a mathematical input-output analysis which leaves the system studied unchanged in principle.

The third phase was James Watt's primarily theoretical approach, which was so successful that it "constituted a refutation of the old adage that an ounce of practice is worth a ton of theory" (Cardwell 1972:85). Watt measured the amount of steam supplied to the cylinder in one cycle in a Newcomen engine, and found it to be much greater than the volume of the cylinder. He

interpreted this phenomenon in terms of the principle that steam condenses when heat flows out of it: he inferred that most of the steam was condensing on contact with the walls of the cylinder, which were cold from the spray of cold water at the end of the previous cycle. One conceivable solution was to spray in just enough cold water to condense the steam and produce a good vacuum, while leaving the cylinder and the water still hot. However Watt realised that this was impossible, according to the principle that the boiling-point of water is lower at reduced pressures - so much so that tepid water boils in a vacuum, which phenomenon he was aware of (Cardwell 1972:86).

Watt realised that there were two conditions for ideal operation of the Newcomen principle: no cooling of the cylinder, and a good vacuum. He also saw that realisation of both conditions was theoretically impossible (in the sense of Kastler 1970) in the one cylinder. The solution was another classic of lateral thinking: he proposed two cylinders, one for each principle. The first, where the piston works, would remain hot. The second, where the cold water condenses the steam, would remain cold and produce a good vacuum. He built his engine accordingly. Despite its greater complexity, it was widely adopted on account of its greatly improved efficiency.

Smeaton's research conforms to the ideal of revolutionary experimental science presented above (s2.6). While using an existing technique of representation, it generated and demonstrated a new principle: that maximum power and efficiency occur with partial condensation and retention of air in the cylinder. However this principle applied only to the Newcomen engine. Watt by contrast worked with principles of more general applicability, which had been demonstrated with laboratory

apparatus very much simpler than the steam engine of either form. A further contrast was that where Smeaton worked at the level of an input-output analysis, taking the design of the engine as given, Watt studied the demonstrable processes occurring within the engine as applications of those general principles and their interactions. Watt's primarily theoretical approach was sufficient to prompt the lateral thinking which led to the obsolescence of the engine so carefully fine-tuned by Smeaton.

One cannot even argue by way of consolation that Smeaton's research was a necessary prompt to Watt's: as it happened, Watt's research arose not from Smeaton's anomaly but from the typically theoretical exercise of explaining the dissimilarity in performance between scale models of the Newcomen engine and full-size engines (Cardwell 1972:85). (A comparable attempt to give empirical research a historical priority over theoretical research, at the expense of falsifying quite recent history, was documented by Polanyi (1958:10) in the case of the Michelson-Morley experiment and the rise of Einsteinian mechanics; cf also Lakatos (1970:159-165).)

It was suggested above (s2.7) that technology may also be seen as normal science. This suggestion may be illustrated by each of the three stages in the development of steam engines.

In the first stage the primary knowledge was that of the representational techniques and the principles of Archimedian fluid mechanics, in conjunction with the principle of operation of the Newcomen engine formulated above. The phenomena were those of the operating engine. The secondary knowledge (cf s2.4 above) was that those phenomena conformed to the working principle, at least to the extent that such a machine did operate and produce useful power. The discrepancies were those apparent from Smeaton's and Watt's respective

measurements performed on the engine, described above.

In the second stage, Smeaton's input-output analysis, the primary knowledge involved the technique of representing the machine's performance in terms of 'power' and 'efficiency', and the principles otherwise known as the definitions of those terms. The secondary knowledge was that the performance characteristics of the Newcomen engine conformed to those principles, and that both power and efficiency varied according to operating conditions (as described above) with a maximum efficiency of about 8 million ft-lb. per bushel of coal (Cardwell 1972:83n). No discrepancies were mentioned in Cardwell's account.

In the third stage, Watt's alteration of the design of the Newcomen engine, the primary knowledge was that of the first stage together with the representational technique of heat flows and the principles of steam condensation and of boiling-point depression stated above. The secondary knowledge was that both the discrepancies concerning the operation of the Newcomen engine, and the superior efficiency of Watt's engine, conformed to those principles. Again no discrepancies that would be part of that secondary knowledge were mentioned by Cardwell.

While each of those three cases of technological work generated secondary knowledge, it appears from the above reconstruction that none of them depended upon any secondary knowledge produced by pure normal science, such as may be found in a modern science data book. It might be interesting to examine a wider variety of cases of technological innovation to see whether this is a general pattern. (Of course if relevant secondary knowledge is available then technologists will tend to use it. Several lines of inquiry were open: one could interview the technologists or read their records to see whether the knowledge was in fact used; one could see whether

the knowledge was generated by earlier technology independently of pure normal science; and one could test the dependence of the innovation on the knowledge by attempting an imaginative reconstruction of the innovation as it might have been done, in accord with an ideal of science and a general science of behaviour, but not involving the secondary knowledge in question.) The degree of dependence of technology upon pure normal science would of course be relevant to any questioning of the current status of the latter (cf s2.7 above).

Insofar as the representation of technology in this section is fair comment, allowing for some degree of idealisation as is inevitable in any concise rational reconstruction of developments that took many decades, it seems that the Galilean ideal of scientific knowledge may be readily applied to the process of technology.

## 2.9 Justification of theories

In Chapter 3 we consider the justification of behaviourism in general; in Chapter 4 we call into question the justification of Stenhouse's ethological theory of intelligence. In this section we consider two criteria for justification: application and scrutability; and a related desideratum: demonstration. The problem of justifying these criteria is also considered.

The criterion of application requires that the truth of the existential statement of the Galilean ideal be substantiated by positive instances, that is, that it be shown that there do exist phenomena which may be represented according to the technique of the theory so as to conform to the principles of the theory. As Kuhn (1970:46-47) pointed out, applications of the theory to concrete phenomena serve a double function: firstly to establish the theory as at least a candidate for acceptance when it is first announced, and secondly to

convey the meaning of the theory and its distinctive concepts to successive generations of scientists. An application may involve the theory as a whole, including various interactions between the several principles - as distinct from a demonstration as formulated below.

A classic case is Newtonian mechanics, which was presented in Principia Mathematica together with its application to the planetary motions. Lakatos (1970: 135f) outlined the complex interactions between principles that were involved, and the gradual elaboration of an initial representation (so simplified as to be contrary to at least one principle of the theory) into a fully developed representation of the planetary orbits which had been inferred from previous observations. Lakatos (1970:130n5) also showed that the phenomena or 'data' were themselves by no means 'given' and fixed, but were dependent upon the observational theories of Flamsteed the contemporary astronomer; Newton caused great annoyance by continually revising Flamsteed's observational theories in the light of his (Newton's) theory, which Flamsteed was trying to refute. The circularity is reminiscent of that described above (s2.2) involving history and philosophy of science.

Having shown that one's theory does have applications, one can make a stronger case for it if one can show that it applies even to the phenomena which rival theorists claim as their exemplars. This occurred for Newtonian mechanics with the interpretation of the phenomena of "the circling heavens, a falling stone, smoke rising from a fire, the steady progress of a horse and cart" which Toulmin and Goodfield (1961:248) listed as the exemplars of Aristotle's theory of motion. To use a military metaphor, having gained control of some territory one sets out to conquer the home bases of one's rivals. (This was attempted for the Galilean

ideal as a theory of science, in that the exemplars (s2.5 above) were drawn from Ayer who is a proponent of a rival theory.)

Scrutability (cf Bunge 1961:425) is the requirement, applied to a principle of a theory, that it be demonstrable free from interactions with the other principles of the theory. It is not necessary that such a demonstration be accomplished, for it may be impracticable due to limitations in experimental techniques at a particular time. It is sufficient that such a demonstration be seen as conceivable, not absurd.

For example, Newton's principle of gravitational attraction, published in the Principia 1687, was scrutable: it was conceivable that if two masses were placed close together, it might be possible to measure the gravitational force between them without involving the laws of motion. No absurdity was involved, as would have been the case had the gravitational force been peculiar to planets and other celestial objects. Indeed it was possible to calculate an approximate value for the force between a given pair of masses, by inference from astronomical and geological data. The principle was not actually demonstrated until 1798, in the Cavendish experiment expanded on below.

Freud's theories of psychodynamics show a shift from early scrutability to later inscrutability, consistent with a theory on the defensive (cf Ziman 1968: 28f). Skinner (1969:83f) noted that Freud claimed at first to have interpreted neurosis in terms of neurology, biology and physics. Thus it appears that Freud would have accepted suitable anatomical and physiological studies of the brain, as demonstrations of his principles. However Freud soon retreated from this position, seeing it in retrospect as "a kind of aberration". At that later stage he would presumably have seen the expectation

of such demonstrations as absurd. In Chapter 4 we note similar inscrutability in Stenhouse's theory of intelligence.

Demonstration of all the principles of a theory is its strongest justification, for an accomplished demonstration establishes both application and scrutability as well. Once demonstrated, a theory may remain in good currency despite the advent of other theories able to re-interpret the same phenomena more fully. The case of Newtonian and Einsteinian mechanics was described above (s1.2); the similar case of the continued use of ray optics despite the advent of wave optics was reported by Bunge (1974:32f). There are of course theories such as Aristotelian mechanics which appear to be thoroughly extinct. However on closer examination it may be that they are alive and well in some applications, or (as in the case of phlogiston theory, below) that they never were demonstrated in the present sense.

Demonstration of a principle often requires a contrived experimental system in order to create phenomena sufficiently free from interactions with other principles. Such controlled experimentation was pioneered by Galileo with the use of almost weightless strings, almost friction-free planes and pulleys (Cardwell 1972:36) and similar demonstration apparatus familiar to every physics student. Sometimes an interaction cannot be eliminated but can be identified and compensated for; this commonly happens with friction in demonstration machines and corresponds to (in Fisher's sense - s2.6 above) 'controlling' for friction. Sometimes the ideal system cannot be contrived in the laboratory but must be imagined as the extrapolated limit of a series of real systems; Galileo had to treat free fall in a vacuum in this way, as the limit of a series of systems involving free fall in fluids, ranging from

treacle through water to air. In such a 'thought experiment' one might prefer to say that the principle was merely shown to be scrutable, without having been demonstrated.

There seems to be a common belief that one must hold constant all the extraneous variables in an experiment. On the contrary, Lykken (1968:156n3) argued for 'systemic replication' in which "one allows all supposedly irrelevant factors to vary from one subject to the next in the hope of demonstrating that one has correctly identified the variables which are really in control of the behaviour being studied". (For an example of systemic replication see Bitterman (1960; 1965); for a more extended discussion of systematic analysis, which appears to be the same thing, see Sidman (1960:110-139).) Likewise Skinner (1935:472) argued that the demand for exact reproducibility should be tempered and more weight be given to simplicity or consistency of data: "to insist upon the constancy of properties which can be shown not to affect the measurements in hand is to make a fetish of exactitude". Both of these recommendations point in the direction of a more convincing demonstration of a given principle, remembering that a principle "picks out a few of the factors at work in particular situations and describes systematically the relations between these factors" (Homans 1951:15, cit s2.8 above).

A classic demonstration was the Cavendish experiment mentioned above which demonstrated Newton's principle of gravitation as expressed in the formula (s2.5 above):

$$F = G.m.m'/d^2$$

The apparatus was a torsion balance, in which a suspended rod with a mass on each end moved when two

other masses were brought close to the two suspended masses. The experiment was reported by Sears and Zemansky (1970:60) as a means of determining the constant  $G$ ; however with such an apparatus the principle may be fully demonstrated by varying the sizes of the suspended masses  $m$ , the other masses  $m'$ , and the distance between masses  $d$ , and showing that the resulting variations in the force  $F$  conform to the principle. The method of systemic replication may also be applied by varying (or allowing to vary with weather etc.) the composition and shape of the masses, air pressure, temperature and other variables, to show that they do not interact with the principle being demonstrated and may therefore be neglected.

It was suggested above that a theory, once demonstrated, may be relatively safe from being overturned by a rival theory. However it is not easy to decide whether a theory has been demonstrated, for an apparently accomplished demonstration may be overturned when some apparently insignificant detail is represented as essential by a rival theory. This fate befell Stahl's phlogiston theory as reported by Toulmin and Goodfield (1962:212-228; cf Toulmin 1957). The theory represented the processes of burning and calcination (roasting a metal in air to form a calx) as the release of phlogiston contained in the fuel or metal. It was readily applied to the burning of solid fuels, accounting for the evident loss in weight. It was scrutable under Priestly's interpretation, though not for some other investigators who preferred to avoid the question of whether phlogiston had positive or negative weight. Priestly demonstrated that when minium (lead calx) was heated in 'inflammable air' (hydrogen) contained over water, the 'inflammable air' was consumed and the calx became lead metal. He saw this as the calx absorbing

phlogiston, and thus identified 'inflammable air' as phlogiston itself. In the modern theory, following Lavoisier, we see the hydrogen removing combined oxygen from the calx (metal oxide) to form the metal and water. The remarkable fact is that Priestly himself noticed the formation of water, but dismissed it as a by-product of the reaction. Such complications were and still are common enough in chemical experimentation. However such a complication constitutes an interaction between principles, which indicates that Priestly's experiment did not strictly demonstrate his phlogiston principle.

The phlogiston theory is now thoroughly defunct. Priestly may have thought he had proved it, but in the words of de Bono's '2nd law' (1971:113):

"Proof is often no more than lack of imagination in providing an alternative explanation."

At the risk of starting on an infinite regress, we next consider the justification of the above criteria for justification. Space does not permit a conclusive argument; all that is attempted here is an outline of how the argument might be conducted.

Feyerabend (1975) argued, on the basis of a detailed analysis of Galileo's role in the Copernican revolution (and other historical material), for a position of epistemological anarchism which involves the rejection of any formal criteria for justification of theories (and hence, it would appear, the rejection of the ideal of rational criticism). However, even if we agree with Feyerabend's rational criticism (the paradox is deliberate) of those criteria which he specified, we need not therefore reject all criteria but instead may seek others which are more in accord with the history of science. Thus the criteria proposed may be initially justified to the extent that they escape Feyerabend's criticisms (1975:10-15).

Bunge (1961) discussed some 23 desiderata for scientific theories. However the present discussion does not attempt to specify all that one might hope for in a theory, but rather to specify what one may reasonably insist upon and use as criteria for rejecting theories as unscientific. Two such criteria are proposed: application and scrutability. (The desideratum of demonstration is included in the above discussion because scrutability is formulated in terms of it and because the scrutability of a theory is implied by a demonstration of its principles.) The importance of those two criteria for this thesis lies in their use in the defence of behaviourism (Chapter 3) and in the critique of Stenhouse's ethological theory of intelligence (Chapter 4). Thus it may be objected (in defence of Stenhouse's theory) that one or both of those criteria should properly be considered merely desiderata. The appropriate support for such an objection would be examples of indisputably scientific theories which do not fulfil the criteria in question. Alternatively it may be objected (by way of disputing the scientific status of behaviourism) that there are other criteria for a scientific theory which are not met by behaviourism. (Such other criteria might, for example, be drawn from Bunge's list of desiderata.) Such an objection would of course need to reckon with Feyerabend's criticisms of the usual criteria, and with other criticisms such as that given below (s3.2) of the criterion that the phenomena to which a theory is applied must be predictable.

This concludes the main exposition of a Galilean ideal of science, although certain points are elaborated as asides in the following chapters.

## CHAPTER 3 : Behaviourism

### 3.1 Behaviourism as Galilean science

The idea that Skinner's work conforms to a Galilean or Platonic ideal of science is not new. Revusky (1974) presented it with a few examples. While he used the term 'Platonic', his examples of experimental physics are obviously Galilean. He also showed that major traditions in biology, particularly physiology and immunology, conform to a Platonic ideal rather than to the Aristotelian view of science held by R.A. Hinde (1966: 5). (It is ironical that Hinde (1966:6f) cited Toulmin (1953) with approval, yet was apparently unaware that his view of science corresponds to that natural history which Toulmin (1953) took pains to distinguish from science.) The misrepresentation of the physiology of respiration, of which Revusky accused Hinde, is reminiscent of the misrepresentations of other sciences by that other Aristotelian Ayer, documented in Chapter 1 above.

An implicitly Galilean textbook of behaviourism is Principles of Psychology by Keller and Schoenfeld (1950). Fred Keller was well regarded by Skinner (1953 dedication; 1956:103); Keller and Schoenfeld have dedicated to them jointly both a text in the methodology of behaviourist experiments (Sidman 1960) and a collection of readings to accompany their text (Verhave ed 1966). It will be cited as (K & S) for brevity<sup>1</sup>.

Behaviourism is readily formulated as Galilean science. The primary knowledge is:-

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1. It seems rather odd that, despite the obvious status of the authors in the behaviourist school and a positively glowing introduction by the Editor of the reputable Century Psychology Series, their text is still available in the first printing, of which I have (1977) bought a copy.

There exist behavioural phenomena which can be represented as responses and stimuli (whose temporal patterns may in the operant case be further represented by operant strengths and contingencies of reinforcement respectively), so as to conform to the principles listed below...

Before proceeding to the list of principles, some clarification of the technique of representing behavioural phenomena as responses and stimuli may be in order. Unfortunately Skinner's (1935) paper on the subject was turgid and predated his (1937:491) departure from the representational technique of S-R reflexology, although it is still worth the effort of reading it.

The term 'stimulus' as used here includes the cues and reinforcers of the operant principles, in addition to the conditioned and unconditioned stimuli of respondent principles. 'Stimulus' is the Latin for 'goad' (Skinner 1969:3), and unless one is careful such connotations creep into its usage in operant contexts even though they are appropriate only in respondent contexts.

The terms 'response' and 'stimulus' may be understood as analagous to the Newtonian term 'force'. In mechanics the phenomena we observe are interactions between bodies, otherwise known as actions and reactions. To take a very simple example, we observe the earth and the moon in orbit around their common centre of centre of mass. We represent the phenomena by taking each body in turn and drawing what is called a 'free-body diagram' for it. That diagram shows the body apart from any others in the system. Each interaction between that body and others is represented as a force. In this simple case of two bodies, with the gravitational attraction as the only interaction, each free-body diagram shows one body with a single force acting on it in the direction of the other body. In more complex systems (eg. the sun, the earth

and the moon) each body has several forces acting on it; there may be any number of bodies.

Now it happens that in Newtonian mechanics, for each force (action) on one body there is an equal and opposite force (reaction) on another body. This gives mechanical interactions a convenient symmetry and simultaneity. However such is not the case in behaviour, which is why the two terms 'response' and 'stimulus', unlike the terms 'action' and 'reaction', cannot be replaced by a single term such as 'force'.

A familiar example of interaction between two organisms might be a teacher and a pupil in a tutoring or coaching situation. We represent the teacher as emitting various responses and being acted on by various stimuli; likewise the pupil. Some of the teacher's responses are stimuli to the pupil, some of the pupil's responses are stimuli to the teacher, and some responses of each are not perceived by the other and thus are not stimuli to the other. Functional relationships between the stimuli and responses for each organism are expressed in the principles to be listed below. More complex social systems may be represented in the same manner.

This representational technique is not restricted to interactions between organisms; as Skinner (1953) made clear it is equally applicable to interactions between an organism and inanimate objects. An example is a potter working clay on the wheel. Using the technique of behaviourism we represent the potter as emitting various responses and being acted upon by various stimuli. Using the several techniques of physics we represent the clay as acting upon the potter's body and being acted upon by it. Some of the potter's responses are actions upon the clay; some of the actions of clay are stimuli to the potter.

While formulated in terms of discrete responses,

this representational technique has been modified by Schoenfeld and Farmer (1970) to feature a continuous 'behaviour stream'. This development of the theory is comparable to that in Newtonian mechanics, from the representation in terms of discrete forces as above, to the continuous 'force fields' now used in some applications (cf Bohm 1970). One physical metaphor which may prove helpful with regard to the 'behaviour stream' was suggested in another context by Erich Jantsch (1975:xvii-xviii):

"Quite generally, an evolutionary perspective emphasises process over structure, the exchange of energy over its containment, flexibility and change over stability. It goes even further - it is interested in the order of process. Structure then is an incidental product of interacting processes, no more solid than the grin of the Cheshire cat [a view that finds considerable support in modern theories of atomic structure]. Its physical image is that of a standing-wave pattern as it may be observed, for example, in the confluence of two rivers; when the flow changes, the standing-wave pattern changes, too. It is the processes which determine structure, not the other way around. And it is the ordered sequence of four-dimensional space/time events which determines the course of a process" (original emphasis).

The noun operant is used to designate, not an arbitrary class of responses, but a class whose joint strength varies according to the principles of operant conditioning (Skinner 1969:88-92, 127-132; Catania 1973b). (Likewise the Newtonian term 'mass' is used for, not an arbitrary measure of the amount of matter in a body, but a measure which conforms to the principles of Newtonian mechanics.) The term operant strength as applied to a response is used here in the sense of 'strength of the operant of which the response is a member'. In operant principles 'strength' is used in the sense of probability or frequency of the emitted

response (Skinner 1969:75; K & S:50); in respondent principles 'strength' refers to the physical magnitude or intensity of the elicited response (K & S:12).

Now to the foreshadowed list of principles. It is not exhaustive, but rather selected for importance in interpreting common human behaviours to a first approximation. Skinner (1969:22-25) gave a more comprehensive list, formulated as nutshell experimental exemplars rather than abstractly as below.

Fuller reports of demonstrations, together with comparable formulations in some cases, may be found in Keller and Schoenfeld (1950) as cited for each principle.

(1) Operant emission: Responses are emitted without stimulus or reinforcement. The strength of any given operant prior to any reinforcement is called its operant level, which is routinely determined in controlled animal experiments (K & S:76). However most human responses, except in infants, are easily seen as parts of conditioned operants and no strictly 'operant level' can be determined, only a 'baseline' showing the effects of prior and current reinforcement. The same may also be true for higher animals raised in diverse and uncontrolled environments - such as Kohler's 'insightful' apes (Millenson 1967:333-335).

(2) Operant reinforcement: A response is followed by a stimulus, and the operant strength rises to a higher stable level. The stimulus is called a reinforcer (K & S:42-51). This is also known as the Law of (Operant) Conditioning or the Law of Effect. Two forms can often be distinguished: in positive reinforcement the reinforcer is the presentation or onset of some stimulus, in negative reinforcement the reinforcer is the removal or cessation of some stimulus, and the distinction becomes arbitrary when the reinforcing event is a change from one stimulus to another (K & S:61f).

(3) Operant extinction: An operant occurs without reinforcement and its strength decreases (K & S:70-83).

(4) Operant spontaneous recovery: An extinguished operant rises in strength without reinforcement (K & S: 76-78).

(5) Operant cue discrimination: An operant is reinforced in the presence of one range of values of a stimulus (called positive cues) and is extinguished in the presence of other values of the stimulus (called negative cues). After this process (called discrimination training) the operant strength rises following the onset of a positive cue and falls following the onset of a negative cue. The operant is then said to be discriminated to the cue (K & S:115-163). The process is usually known as 'operant stimulus discrimination'; the term cue is adopted here to avoid confusion with the stimulus in respondent processes, and to avoid both the cumbersome term 'discriminative stimulus' and the typographically inconvenient symbols ( $S^D$ ) for positive cue and ( $S^\Delta$ ) for negative cue. However the term stimulus discrimination will be used to refer generically to the operant and respondent processes.

(6) Operant cue generalisation: After cue discrimination has occurred, a second cue is presented which is similar but not identical to the positive (or negative) cues previously used. The operant strength increases (or, for a negative cue, decreases) to a degree which is a function of the closeness of the cues along some physical dimension (K & S:115-163). The term stimulus generalisation will be used to refer generically to the operant and respondent processes.

(7) Operant variability: The successive responses of an operant vary along various physical dimensions (K & S:164-196). Some instances of this are

known as 'positive transfer of training' (cf K & S:169).

(8) Operant differentiation: Some variations of an operant are reinforced while others are extinguished. The operant is said to be shaped as the physical dimensions of the model response change (K & S:164-196). In practice this may be combined with operant cue discrimination to shape two distinct operants out of one, as in childhood language learning (cf K & S: 380-387).

(9) Respondent elicitation: A stimulus elicits a response. A stimulus and its response are known as a reflex or respondent (K & S:4).

(10) Respondent reinforcement: An unconditioned stimulus (UCS), which elicits some response R, occurs together with a second stimulus. Subsequently that second stimulus alone elicits R. The second stimulus is then called a conditioned stimulus (CS) (K & S:15-20).

(11) Respondent extinction: A conditioned stimulus (CS) occurs alone, and the strength of the response decreases (K & S:68-70).

(12) Respondent spontaneous recovery: After extinction, and without further reinforcement, the strength of the response elicited by the CS rises (K & S:70).

(13) Respondent stimulus generalisation: A stimulus which is similar but not identical to a given CS or UCS elicits the corresponding response at a strength which is a function of the closeness of the stimuli along some physical dimension (K & S:115f).

(14) Respondent stimulus discrimination: A CS which is variable is reinforced for some values and extinguished for others (K & S:115-118).

The secondary knowledge of behaviourism may be found in reviews of the applications of behaviourism, such as Skinner (1957) and the various sources listed below (s3.3) for applications of behaviourism.

Certain other processes, while arguably principles in their own right, may be represented as special cases of the above processes. Examples are:

(15) Escape learning: After operant negative reinforcement, a subsequent presentation of the negative reinforcer (aversive stimulus) increases the strength of the response (cf Sidman 1966:485). This may be represented as the one stimulus being both a negative reinforcer and a positive cue for the response.

(16) Frustration: A schedule of continuous operant reinforcement is abruptly terminated. The following responses increase in magnitude, variability and damaging effects, and are often called 'aggressive' or 'angry' (K & S:342f; Millenson 1967:445-447). This may be represented as respondent elicitation of responses such as adrenalin secretion (where the stimulus is the abrupt cessation of the contingency), in conjunction with operant cue discrimination (where the abrupt cessation of the contingency is a positive cue for increased magnitude and variability of responses). (With repeated terminations of a continuous schedule, in the procedure called periodic reconditioning, both the respondents and operants of frustration undergo extinction (K & S 89-91); this suggests that both may be products of prior learning.)

This principle (16) is similar to the frustration-aggression hypothesis of Dollard et al, stated as "frustration leads to aggressive action" (Hilgard & Bower 1975:356). It may be objected that this is only a hypothesis, not a principle. The term 'hypothesis' did not arise in the exposition of Galilean science, but it may be taken as referring to a principle which is not yet demonstrated, as was the case for Newton's law of gravitation until Cavendish. In accord with the view argued above (s2.9) that it is not necessary that a scientific principle be actually demonstrated, as distinct

from being scrutable, the term 'principle' refers also to such hypotheses. While this may dissolve the objection by semantic clarification, there is more to be learned from it. Demonstrations of the principle in question have been reported (K & S; Millenson 1967 - as above). Unless it be argued that the alleged demonstrations did not succeed in displaying the principle free from confounding interactions with other principles, there is no reason to call the principle a hypothesis in any Galilean sense. What does remain unproven is the truth of the principle interpreted as an Aristotelian general law, that aggression is an inevitable consequence of frustration (H & B:356) or that all instances of frustration are also instances of aggression. For an Aristotelian the principle may indeed remain merely a hypothesis.

(17) Secondary positive reinforcement: After operant cue discrimination, the positive cue functions as a positive reinforcer in operant reinforcement (K & S 231-261). It is also known as 'conditioned reinforcement' by an 'acquired reinforcer'. It might be thought that the phenomena would be more simply represented in terms of the principle of respondent reinforcement, or an even simpler principle of stimulus-pairing; most of the phenomena do seem amenable to such representations. However Millenson (1967:249f) argued that merely pairing a stimulus with a primary reinforcer is not sufficient to make the stimulus into a secondary reinforcer: the pairing is effective only when it conforms to the principle of operant cue discrimination.

(18) Secondary negative reinforcement: After respondent reinforcement, the conditioned stimulus functions as a negative reinforcer in operant reinforcement (K & S:249-261). Here there seems to be no objection to the simpler representation in terms of

respondent reinforcement or stimulus-pairing; indeed the first example given by Keller and Schoenfeld (p249f) does not obviously conform to the principle of operant cue discrimination (despite their use of operant notation), for the unconditioned stimulus is "inescapable" and therefore not contingent upon any response. The reported phenomena could perhaps be represented in terms of operant cue discrimination in conjunction with superstitious negative reinforcement (cf K & S:102-104), but in the absence of any objection to the simpler respondent representation this seems pointlessly complex.

For most phenomena represented in terms of operant reinforcement differentiation or cue discrimination, one or both of the above processes of secondary reinforcement must be included to produce a representation consistent with the principles, even where a primary reinforcer such as delivery of food occurs. According to the principle of operant reinforcement it is the response followed by the reinforcer that is strengthened. If the reinforcer is delayed more than a second or so after a given response, as is usually the case with primary reinforcers, there is time for some other response to occur and be strengthened instead even though it was not effective in producing the primary reinforcer (cf K & S 208-212; Skinner 1951:560; Millenson 1967:238). However, provided that the given response is followed immediately by some stimulus which has a history with that organism as a positive cue or a conditioned stimulus and which is a reinforcer when presented on its own, we may represent the observed strengthening of the given response as secondary positive or negative reinforcement respectively. For example, in the 'Skinner Box' the click of the food dispenser is such a stimulus, having the history of a positive cue for the response of approaching the food dispenser. (In the

early stages of training a rat, the response of approaching the food dispenser is the one strengthened by the primary reinforcer of food presentation, and has to be discriminated to the click of the dispenser before the click becomes a positive secondary reinforcer.)

If the above formulation of behaviourism is adequate in relation to its limited length, and if it is not systematically misleading, then behaviourism conforms to the Galilean formulation of science proposed in Chapter 2. This is but a first step towards showing that behaviourism is good Galilean science.

### 3.2 Replies to criticisms

Skinner (1974; 1969:98-104) identified various common objections to behaviourism and dispelled them as being based, in general, on over-simplified and naive views of the science. In this section we treat one general objection which reveals a very common misunderstanding of the physical sciences, followed by replies to some critics who are academics in the field of behavioural studies.

There is a common belief that scientific analysis cannot be applied to such complex and indeterminate phenomena as ordinary human behaviour. However the problems in so doing are not unlike those met in applying scientific theories to the complex phenomena that are quite common in inanimate systems, of which the weather is but one example. Skinner (1957:156) noted the comparability of, on the one hand the irregular behavioural record curve that would be obtained for the response of sipping one's breakfast coffee, and on the other hand the irregular cooling curve that would be obtained for the temperature of the coffee in the cup. However it could be objected that the irregularity of the cooling curve is due to the interaction between the

cooling process and the behavioural process. Hence it may be worthwhile to elaborate the case of the weather as one where irregularities obviously occur in the absence of disturbances from the behaviour of humans or other organisms.

Weather predictions are notoriously both imprecise and unreliable; in that sense the weather is indeterminate. Yet meteorological phenomena may be interpreted in terms of the physical principles of thermodynamics and the like. These principles have been demonstrated to high degrees of precision, which is to say that the laboratory phenomena which demonstrate them are highly determinate. The indeterminacy of the weather arises from the complexity of interactions between parts of the weather system, and in particular from the instabilities and positive feedbacks that arise in particular configurations of the system as in the formation of a tornado. When human behaviour shows similar indeterminacy at times (cf Forrester 1971), one wonders what is gained by setting it beyond the reach of scientific analysis.

The weather is not the only case where physics has problems in giving precise analyses of complex systems. For example, a small number of bodies interacting under gravitational attraction alone would be a very modestly complex physical system. Yet this is the subject of the 'multi-body problem' which has troubled astronomical physicists since Newton (as any proof of the stability of the solar system depends on a general solution to this problem) and for which "a general solution... is not yet indubitably secured" (Hanson 1965:118). Further, Siegel (1965:122) argued that such inability to give the rigorous deductive proofs "such as are claimed to be possible in the 'exact science' of physics" is endemic throughout physics, and (p126) that wider recognition of this "would furnish a basis for ridding science of the

the invidious exclusion of the social sciences by the physical sciences, and of the excessively sharp distinction between the quantitative and non-quantitative sciences".

Further, one might argue that the application of behaviourism can be easier than is the case in physical sciences, at least at a qualitative and semi-quantitative level sufficient to guide practical intervention. The behaviourist can perceive the responses and stimuli of human interaction directly with the unaided senses, whereas the physicist has to use indirect means such as instruments or inference to 'perceive' the forces, electric currents and other components of his representational techniques. The practising physicist or engineer seldom has the luxury of a current meter in every wire and a strain gauge on every girder, nor can the meteorologist perceive the air pressures, temperatures and velocities of the many parts of his weather system except by very scanty 'sampling with instruments. Yet the behaviourist interacting with a clinical subject, or to a lesser extent moving about a classroom, can have this sort of direct perception of responses and stimuli. Particularly in practical work (i.e. technology, cf s2.8 above), despite the impression given by E.L. Glynn (1975), it is not always necessary to count responses and plot a curve in order to see what is going on (Skinner 1951; Ferster 1967). When control such as that shown in the graphs of Siepkes (1973) and Williams (1959) is the order of the day, a skilled behavioural observer can follow the changes in sufficient detail to monitor the success of the intervention. We may however concede that the plotting of such graphs plays a major role in the perceptual training which produces a skilled behavioural observer. This concludes the digression and also the discussion of the objection on principle to any science

of behaviour.

When considering some objection to behaviourism in particular, an appropriate first step is to check that the objection does apply to one's own school of behaviourism, in this case the Skinnerian or operant school which according to Kantz (1972) is so clearly defined within psychology as to be somewhat isolated from non-operant psychology. One could perform this check by examining the arguments in each case. However this takes time, and a rapid method of screening objections is available: one checks the author index or bibliography of the work for citations of Skinner or any of the more prominent Skinnerians such as Keller, Millenson, Sidman and Schoenfeld. It is almost axiomatic that a substantial criticism of a position should cite at least one of its major exponents. When a writer such as Konrad Lorenz (1965) addresses his criticisms to behaviourism and cites Pavlov, Hull and Thorndike but not Skinner or any of the better-known Skinnerians, it seems reasonable to assume that the criticisms are addressed to non-Skinnerian varieties of behaviourism.

Thus it is difficult to know what to make of the criticisms of "behaviour modification theory" and of "operant conditioning" presented by Stenhouse (1974) and many others. Both terms in their current usage refer specifically or primarily to Skinnerian behaviourism. Yet on closer examination some of these criticisms betray an apparent ignorance of the literature of behaviourism. One example is from Stenhouse (1976:55):

"...while effective techniques of behaviour modification have been developed, we have little real understanding of why or how they work and, most significantly, behaviour modification theory offers no guidance whatsoever as to how intelligent and responsible (let alone morally acceptable) decisions can be made as to the long-term goals towards which behaviour should be modified".

To take the last point first, the design of cultures was a recurring theme in Skinner's writings (eg. 1953:3-10; 1969:1-74; 1972:3-65) in which behaviour modification theory (i.e. behaviourism) was an integral part of the consideration of long-term social change. He even wrote the utopian novel Walden Two (1948) illustrating his proposals; it is difficult to believe that Stenhouse was unaware of this book and its close relationship to behaviour modification theory. (By way of counter-attack, we may note that ethology has its own problems in the context of morality and social change: as Skinner (1969:196) suggested,

"Konrad Lorenz's recent book On Aggression [1966] could be seriously misleading if it diverts our attention from relevant manipulable variables in the current environment to phylogenetic contingencies which, in their sheer remoteness, encourage a nothing-can-be-done-about-it attitude".

The political dangers of ethology thus applied were elaborated by Montagu (1976:283-299); the relevance of behaviourism to social revolution was explored by Holland (1974).)

Then there is Stenhouse's claim that we have "little real understanding of why or how [the techniques of behaviour modification] work". In view of the fact that most of the techniques in common use are transparent applications of the principles of behaviourism (s3.1 above), often only one or two principles in each case, it is difficult to see how this falls short of a "real understanding of why or how they work". It may of course be that Stenhouse is referring to some "real understanding" which no Galilean science (such as Newtonian mechanics) can give of the relevant phenomena. No clarification of this expression was given or cited by Stenhouse, which makes it difficult to discuss it without being accused of misinterpretation. Short of

allowing it to pass without comment, one is forced to propose conceivable clarifications and criticise them - an exercise which is uncomfortably close to a 'straw man' argument. (Perhaps the distinction is that a 'straw man' is a version of some position which is contrary to some available clarification of that position.) One interpretation of Stenhouse's expression would be that a "real understanding of why or how [certain techniques] work" is one that, in the words of Skinner (1950:69) "appeals to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions" - different, that is, from those of the observed facts. This may well be Stenhouse's meaning, for it accords with his somewhat free use of metaphorical expressions (and analogies) discussed below (s 4.3). If so, it would appear that he is discounting behaviourist theory for its lack of explanation by metaphor. However there are other theories which conspicuously lack explanation by metaphor, among them Newtonian mechanics. Is it suggested that the latter theory gives "little real understanding of why or how [the corresponding techniques] work"? Behaviourism is hardly devalued by the assertion that it lacks some feature which Newtonian mechanics also lacks.

More of a misrepresentation than a criticism is the view that Skinnerian behaviourism is an S-R theory akin to Pavlovian reflexology. This interpretation is difficult to credit, considering that Skinner's programme began with the recognition (1937:491) that "there are responses uncorrelated with observable stimuli". A clearer denial of the basic presupposition of S-R theories would be hard to imagine. Yet the S-R interpretation was put upon his (1938), as noted in his (1966:xii) contrast between S-R theories and his own

in which "the stimulus occupied no special place among the independent variables". Regardless of Skinner's protest the view is still current, as in the educational psychology text by De Cecco (1974:38), in the theoretical paper by Kendon Smith (1974) cited below, and in another critic cited by Stenhouse (1974:292; 1976:52) with approval: Kuo (1967:6, 140-142). To each of these a Skinnerian may reply in the words of N.R. Hanson (in Michalos ed 1974:75), "Never mind, someone could have said the things he says I say. And then his comments would have been devastating" (original emphasis).

One criticism by Kuo (1967:140f), quoted by Stenhouse (1974:292), does at least bear on Skinner's work:

"In the case of operant conditioning, many advocates prefer to carry out their experiments without reference to pre-experimental history [of the individual subject], extra-experimental conditions (environmental context or setting), and, of course, what happens under the skin of the subject" (original emphasis; insert added by Stenhouse).

One might as well criticise Cavendish (s 2.9 above) for carrying out his gravitational experiment without reference to the history of the bodies used as masses, to the cloud patterns overhead, and to what was happening inside the bodies - any of which could conceivably affect the results. To some extent operant advocates do what Kuo accuses them of, but no more so than would any Galilean experimenter. Actually Skinner (1938:55-57; 1969:111f) did recognise and attend to the environmental context and history of the experimental material by way of 'control of extraneous factors' - not that they are extraneous to the theory, but rather that they are extraneous to the particular demonstration in hand (cf the text on methods of operant research by Sidman 1960). Such control is a major

aspect of Galilean experimental design, two elementary examples of which are the use of highly purified (and hence atypical) substances in chemistry, and the restriction of delicate electricity experiments in London to Sundays, due to the electrical disturbances of the underground trains (Thomson 1965:85).

As for what happens inside the skin of the subject, Skinner (1956b; 1969:280-284) gave compelling reasons for his leaving physiology to the physiologists and concentrating on collecting the behavioural facts (demonstrated principles, s 2.6 above) which physiology may, some day, account for. (This relationship between theories, similar to that termed 'reduction', may be represented as the principles of behaviourism becoming part of the phenomena of physiology.)

In similar vein Stenhouse reacted to Bitterman's (1965:100) closing sentence, which was:

"Clearly bringing the study [of behaviour] into the laboratory was the first real step toward replacing guesses with facts about the evolution of intelligence and its relation to the evolution of the brain" (insert added by Stenhouse).

Stenhouse (1974:289) retorted:

"It would appear that results emanating from laboratory investigations are factual while those of field studies are not! It would clearly be unreasonable to expect evolutionary and ecological issues to receive adequate consideration within a context in which that sort of statement could be made".

It does indeed follow from Skinner's Galilean concept of a 'fact' as a demonstrated principle, that in general facts arise from laboratory investigations but not from field studies. Field observations generate reports of phenomena, which are certainly factual as opposed to fictional. However the complex interactions and correlations between variables in the field, combined with the practical difficulties of manipulating variables

in the field, generally preclude any Galilean demonstrations of principles. (One exception might be the observed rise and fall of industrial melanism in moths in England, as a demonstration of natural selection.) The researcher can generate interpretations of the phenomena observed in the field, but often there are plausible alternative interpretations (cf de Bono's Second Law s2.9 above; also Skinner 1969:184-186 on the African honey guide). In the absence of relevant experimental demonstrations one is indeed left with "guesses" as to which interpretation best represents the processes occurring in the field. Far from showing "philosophical naivety with regard to 'fact'" (Stenhouse 1974:290), Bitterman's conclusion is consistent with an elaborated Galilean philosophy of science.

The objection to laboratory phenomena as atypical was taken further by Stenhouse (1974:290) with the insistence that an animal's behaviour in the laboratory can be understood only in relation to "its natural behaviour in the field". This had already been answered by Skinner (1969:202f) with the rejoinder that "everything is the product of natural processes", laboratory and field phenomena alike, noting also that different 'natural' environments often generate different behaviours. Further, Skinner (1969:191) pointed out that, due to the marked ecological changes over the last few thousand years,

"no land mammal is now living in the environment which selected its principal genetic features, behavioural or otherwise. Current environments are almost as 'unnatural' as a laboratory. In any case, behaviour in a natural habitat would have no special claim to genuineness. What an organism does is a fact about that organism regardless of the conditions under which it does it. A behavioural process is none the less real for being exhibited in an arbitrary setting".

The above points illustrate the fundamental split

between field and experimental students of behaviour, corresponding to the split between Aristotelians and Galileans elaborated above (Chapters 1 & 2). That this fundamental difference in scholarly outlooks is localised in neither time nor academic discipline, is indicated by John Tyndall's (1881:467) reply to the advocates of the theory of spontaneous generation of life as an explanation of fermentation:

"I have not the slightest objection to this explanation, provided proper evidence can be adduced in support of it. But the evidence adduced in its favour, so far as I am acquainted with it, snaps asunder under the strain of scientific criticism. It is, as far as I can see, the evidence of men who, however keen and clever as observers, are not rigidly trained experimenters. These alone are aware of the precautions necessary in investigations of this delicate kind" (original emphasis).

Tyndall proceeded to describe what we may recognise as standard and typically Galilean procedures for preventing contamination of experimental cultures by microbes from the air or from unsterilised objects.

Some critics try to give a Popperian refutation of some general law that supposedly lies at the heart of behaviourism. Seligman's (1970:406) version is "That all events are equally associable and obey common laws". This general law is readily disowned from the Skinnerian school even if Skinner did make an unguarded remark which could be interpreted that way; according to Herrnstein (1977:595) this doctrine of equipotentiality "is not to be found in Skinner's theoretical writings". Even though the logic of Galilean science does not admit of disproof by counter-example for it has no Aristotelian general laws, the alleged refutations provide some instructive examples of the pitfalls of scientific inference. For example, Breland and Breland (1961:183), describing a "misbehaviour" of some chickens relative to

an environmental contingency of reinforcement, reported:

"After they have pecked a few capsules off the slide, they begin to grab at the capsules and drag them backwards into the cage. Here they pound them up and down on the floor of the cage. Of course, this results in no reinforcement for the chicken and yet some chickens will pull in over half of all the capsules presented to them" (emphasis added).

However it is no easier to establish that a given response whose strength is increasing or is stable at a high level, is not followed by a reinforcer, than it is for a physicist to establish that a given accelerating body has no net force acting on it. In this behavioural case there are plenty of events which are candidates for reinforcers, in the movements of the capsule as it is pounded by the chicken. Such movements, as stimuli, may well have become secondary reinforcers, as cues for feeding, on previous occasions when the chicken pounded lumps of food on the floor breaking them into edible pieces (cf Skinner 1969:191f); or the movements may have been primary reinforcers as the responses which produce them would clearly have aided the survival of chickens throughout evolutionary history. Here, as with Kohler and his apes (Millenson 1967:333-335), it is the critics of behaviourism who have ignored the pre-experimental history, under the very conditions when a Galilean scientist would attend to it: when the experimental phenomena cannot be represented in conformity to the principles of the theory (cf Thomson 1965).

Sheldon (1974) showed a better grasp of the logic of behaviourism, in recognising that Skinner's definition of an 'operant' is not independent of the 'law of operant conditioning' (cf Skinner 1969:88f, 127-132; Catania 1973b). However Sheldon counted this as evidence against behaviourism's claim to be scientific, invoking what is obviously Popper's demarcation criterion (of science from

pseudoscience) without identifying it as such or giving any hint that it might be in dispute (cf s1.1 above). Sheldon's (1974:176) formulation of the criterion is at least unusually rigorous and may be quoted as a statement of the Aristotelian fallacy refuted by Toulmin (1953:52, quoted in s1.2 above). Sheldon wrote:

"If a statement is to have the status of a law, it must be possible to conceive of circumstances that would show it to be false\*, and for this to be possible it is necessary that at least some of the terms contained in it should be defined independently of the relationships that, according to the statement itself, hold true between these terms and others in it".

The part before the added asterisk is Popper's demarcation criterion; the remainder is the general Aristotelian fallacy. In passing we may note that it is odd that Popper has propagated this criterion which depends upon the very Aristotelian mode of definition which, in an informal digression (1966:9-21), he repudiated and contrasted with scientific practice.

Another version of Sheldon's criticism is the hardy chestnut known as the circularity of the law of effect. This was well roasted by Keller and Schoenfeld (1950:66) but, phoenix-like, is with us still. The fact that the Newtonian definition of 'force' has the same allegedly debilitating relationship with the corresponding laws (of motion) seems not to trouble the scholars who raise this objection. Perhaps they prefer Aristotle's physics? Keller and Schoenfeld (1950:66) clarified the logic of the principle known as the law of effect with a formulation that is notable for this thesis in its adoption of the existential form for primary knowledge:

"There are stimuli which have the power to strengthen the operant responses that produce

them. This strengthening may be termed 'operant conditioning', and the stimuli may be referred to a class called 'reinforcing'".

As the principles of operant and respondent extinction will feature in the behaviourist theory of intelligence (Chapter 5), two criticisms which bear specifically on those principles are now examined. One is that of R.A. Rescorla as reviewed by Hilgard and Bower (1975:74-76). Pavlov had "proposed that, during experimental extinction, an active inhibitory process was building up, becoming associated to the nonreinforced CS, so as to overcome and impede the positive response to the CS...Skinner (1938) looked at such data and questioned whether one really needs such an inhibitory notion: why would not simple loss of excitation account for all of Pavlov's data?" H & B:74, original emphasis). Rescorla's experiments involved a tone as a 'safety signal' in what was obviously an operant avoidance contingency (cf K & S: 311-314), despite Hilgard and Bower's representation of it in purely respondent terms. More seriously, they made no attempt to answer the question (corresponding to that attributed to Skinner): 'Why would not simple loss of excitation account for all this data?' Indeed their phrasing of the question is unfortunate in that the terms 'why...not' and 'simple' prejudice the answer; a more open question, more in keeping with Skinner's usually careful choice of words, would be 'Can the principles of extinction and related principles account for all this data?' Now one of the related principles is operant cue discrimination, related in that it involves selective extinction of a response in the presence of a negative cue. Representing Rescorla's tone as a negative cue, it appears that his "evidence...showing the existence of conditioned inhibitory factors" H & B:74) conforms to the principle of operant cue discrimination and may thus be

interpreted in terms of extinction. Yet for Hilgard and Bower (p76)

"...it is no longer possible to doubt the reality of such opposing or 'antiresponse' factors that arise from negative correlations between the presence of a 'neutral stimulus' (the CS) [the negative cue] and the appearance of a reinforcing stimulus".

They seem to have missed the nature of Skinner's (1938:97) "principal argument against the notion of a suppressing force in extinction", which is one not of evidence but of elegance (cf Harré 1961:16). Skinner argued that the interpretation of an observed state of inactivity is needlessly complicated by this notion of inhibition (for the same evidence can be accounted for without it); it also creates an inscrutable distinction between reconditioning and original conditioning which can be eliminated only at the expense of absurdity. Despite their detailed chapter on Skinner, in this discussion (which is in the chapter on Pavlov) Hilgard and Bower showed an apparent lack of familiarity with Skinner's work.

The last in this review of criticisms is Kendon Smith's (1974) argument against the principle of operant extinction. Much as one is tempted to rule him (p124,144) out of court for associating Skinner with the notion of "S-R bonds" (!), his argument that "extinction is simply one form of attenuation by punishment" (p135) does raise some interesting issues. He argued that every unreinforced response is punished by the effort of making the response, on the premiss that "effort is a recognised form of punishment". Now that premiss may well be true for sufficiently intense effort, however his inference presupposes more than the stated premiss; it presupposes that all effort is punishing, which is at best debatable. He gave no source for his premiss, such as might enable us to check

whether the evidence would support the required interpretation, perhaps because it is difficult to conceive of evidence that would adequately support such a general law.

Smith's argument against Skinner's theory of behaviour followed the familiar pattern : the theory under attack was represented as simplistically as possible, while its interpretations of those phenomena which form the exemplars of the rival theory were ignored. On extinction Smith (1974:135) wrote: "Traditional learning theory...has tended to regard extinction as the only conceivable true weakener of behaviour". Granted, Skinner (1953:184) did state that the effect of punishment, in an experiment to demonstrate its effects as compared to extinction, was "a temporary suppression of the behaviour, not a reduction in the total number of responses". However Skinner went on to identify three distinct effects of punishment, the third of which (1953:188) was that "any stimulation which accompanies the [punished] response, whether it arises from the behaviour itself or from concurrent circumstances, will be conditioned... Any behaviour which reduces this conditioned aversive stimulation will be reinforced" (original emphasis). Thus any of a range of behaviours, excluding the punished behaviour, will be negatively reinforced. If some such behaviour is reinforced to the extent that it occupies most of the organism's time, and it is incompatible with the punished behaviour, then the punished behaviour 'attenuates' because the organism is too busy doing something else. Here we have an interpretation of Smith's 'attenuation by punishment', in terms of the principles of respondent elicitation, respondent reinforcement and operant negative reinforcement. Thus it appears that Smith's proposal has no advantage over

Skinner's theory with respect to either evidence or elegance - and a distinct disadvantage in the lack of evidence for the presupposed general law about effort.

### 3.3 Justification

Applications of behaviourism abound. A few general sources are Skinner (1953), Honig (ed 1966), Verhave (ed 1966), the Journal of the Experimental Analysis of Behavior, and the Journal of Applied Behavioral Analysis. A source on classroom applications is Harris (ed 1972). Interpretations of the phenomena which are the exemplars of rival theories have been published : for psychiatry by Chandra (1974), for psycholinguistics by Catania (1973), for mentalism by Homme (1965), and for ethology by Bateson (1976) and Peterson (1960). (For the record, the experiment outlined but not attributed by Skinner (1974:46) is that of Peterson (1960), cf Skinner (1969: 128)).

Demonstrations of each principle have been performed (Keller & Schoenfeld 1950 as cited for each principle above); scrutability is thereby entailed. Indeed, as is the way with revolutionary experimental science (s2.6 above), new principles are in general announced only as they are demonstrated. Keller (1969) reported demonstrations of many of the principles with humans. Some clinical applications conform closely enough to a single principle to count as demonstrations, for example operant extinction of bed-time tantrums by Williams (1959).

In terms of the criteria for justification presented for Galilean science (s2.9 above), behaviourism appears to be amply justified. This concludes the formulation and defence of behaviourism as a Galilean science.

## CHAPTER 4 : An Ethological Theory of Intelligence

### 4.1 : Stenhouse on intelligence

The nature of intelligence is one field to which behaviourism does not appear to have been applied in any systematic and elaborated discussion (but see s5.1 below). The recent symposium The Nature of Intelligence (ed Resnick 1976) did not list Skinner or any of his better-known followers in the author index. An ethological theory of intelligence is chosen for examination here, as a prelude to a behaviourist theory of intelligence (Chapter 5), because ethology is one theory in the field which has an initially plausible claim to give a scientific interpretation of the behaviour of biological organisms. The more popular cognitive and information theories depend on metaphors drawn from studies, not of organisms, but of machines and particularly logic-machines such as computers (cf Skinner 1974:122). We will follow the application of ethology to the phenomena of intelligence by David Stenhouse (1965; 1974; 1976) who, if the acknowledgements (1974:9) to Niko Tinbergen, Julian Huxley and Konrad Lorenz are any indication, is well within the ethological school.

Among the reviewers of Stenhouse's (1974) major work was W.R. Thompson (1975:615). He criticised Stenhouse's key concept, the P-factor, for being "ubiquitous and protean", and the book for trying "to explain too much by too little and [placing] in the same context, overly disparate types of data". However one might as well criticise Newtonian mechanics for its ubiquitous and protean concept 'force', for trying to explain too much with so few laws, and for placing together the overly disparate data of celestial planetary and terrestrial projectile motions.

Stenhouse's work seems to have been overlooked in

some circles: Resnick's above-mentioned symposium, while containing a discussion on the possibility of an ethological theory of intelligence, did not cite Stenhouse. However, more important for this essay is that Stenhouse provided an elaborated theory against which to pit behaviourism and with it the philosophy of Galilean science.

Lest the following criticisms of Stenhouse's theory be misinterpreted, it should be made clear that the argument is not with the wealth of biological phenomena used to support the theory, nor with its applications to the role of creative intelligence in education and in the future of human society. Indeed one would like to translate those chapters of his (1974) into Skinnerian terms to make them available to a wider readership and to show how they would complement other applications of behaviourism. However that exercise is beyond the scope of this thesis, one task of which may be seen as to show that such a translation would be feasible and worthwhile.

Stenhouse's theory may be viewed from four perspectives. (The sequence is analytical, not chronological.) Firstly he defined intelligence. Secondly he analysed intelligence into four factors. Thirdly he interpreted one of those factors, the P-factor, as the inhibition of instinctual or habitual responses. Fourthly he addressed the problem of outlining a phylogenetic continuity of development as required for an evolutionary theory of intelligence consistent with man's origin from other species.

#### 4.2 : Intelligence defined

Stenhouse (1974:31) defined intelligent behaviour thus:

"Intelligent behaviour is behaviour that is

adaptively variable within the lifetime of the individual".

This definition seems unproblematic; however problems arise when we turn to the definition of 'intelligence' as the capacity for such behaviour. Stenhouse put his definition of intelligence on a level with D.O. Hebb's 'intelligence A', defined (Hebb 1949: 294, quoted by Stenhouse 1974:32):

"...an innate potential, the capacity for development, a fully innate property that amounts to the possession of a good brain and a good neural metabolism" (original emphasis).

Stenhouse noted that "Hebb is mistaken in suggesting that 'a good brain and good neural metabolism' are 'fully innate'"; further support for Stenhouse's view comes from the recent demonstrations by Colin Blakemore (1971) that responsiveness of neurones in the visual cortex is a function of early visual experience. More troublesome is the term 'capacity' with its implication of a scrutable (if not demonstrated) upper limit, as in 'the lifting capacity of a crane'. It is difficult to imagine how such an upper limit might be demonstrated except in the course of experimental manipulation of the sort described by Skinner (eg. 1969:201f on the responses of dolphins) and dismissed out of court by Stenhouse (s3.2 above). The alternative term used by Hebb, 'potential', has a long history of vague and inscrutable usages (Skinner 1956b:261f) and thus needs rather more explicit clarification than was offered by either Hebb or Stenhouse.

#### 4.3 : The four factors

It is difficult to determine just what sort of existence or reality Stenhouse's four factors are accredited with. He (1974:73-89) identified them severally with W.C. Halstead's four factors, which were produced by factor analysis of a battery of tests in the usual

psychometric way. However Stenhouse appeared to claim something more than factor-analytic validity for his factors.

The factors appeared in a diagram (1965:815; 1974:86) captioned "The functioning of four-factor biological intelligence in evolution", which might be expected to elucidate the status and interrelationships of the factors. Unfortunately this diagram is not accompanied by the systematic interpretation expected when comparable diagrams are used in chemistry and physics. Despite the proliferation of symbols no key is supplied: it is left to the reader to puzzle out what distinction, if any, is drawn between the different forms of arrow; what is represented by an arrow terminating in another arrow instead of in a box; and what is represented by an arrow crossing the circle which presumably represents some boundary of the organism.

In addition to the failure to provide a key or other guide to detailed interpretation, Stenhouse's diagram shares the weaknesses which Skinner (1956b) identified as inherent to diagrams of inscrutable 'inner processes' inferred from observations of behaviour. Stenhouse (1974:115) made it clear that his factors are inscrutable: we are not to expect any test which will measure one factor apart from the others; they cannot be disentangled.

Perhaps the meaning of 'factor', or at least an appreciation of its aura of precision, may be found in the mathematical usage: several factors multiplied together give a product. Is intelligence the product of the four factors? Skinner (1969:83f) cautioned against such borrowed usages:

"The advantage in representing processes without the use of metaphor, map, or hypothetical structure is that one is not misled by a spurious sense of order or rigor".

Whether or not the term 'factor' is an example of this, we will see somewhat clearer examples in Stenhouse's accounts of the individual factors.

In similar vein Skinner (1969:276f) warned against taking too seriously the qualities, traits, abilities (and, we may add, factors) invoked to account for observed functional dependences:

"We may say that a man is tall and strong and that he possesses height and strength, but we do not then say that he is tall because of his height or strong because of his strength".

To propose such an explanation would be to repeat the error of the doctor in Moliere's play who explained the effects of opium as caused by its dormative powers, or of teachers and parents who explain a child's slow progress in reading as due to dyslexia. We will have occasion to suspect that Stenhouse has repeated this error. Skinner continued,

"A trait may be useful in directing our attention to a variable responsible for a class of behaviours, but the variable is the thing to study".

(1) D-factor : Sensorimotor efficiency. Stenhouse (1974:83) adopted this from Halstead, and argued that

"Halstead's D-factor relates to both motor and sensory efficiency and might be regarded as a resultant or product of their several efficiencies".

Here Stenhouse claimed that the D-factor has the form of an efficiency which is a resultant or product of several efficiencies. These emphasised terms have rigorous usages in mathematical physics, and in the absence of guidance to the contrary we can but seek to apply those usages here. The 'efficiency' of a system is defined as the ratio of (useful) output to input, where both are measured in the same units, typically those of energy. In a process analysed into a series of stages one can indeed calculate the overall efficiency as the arithmetic product of the several efficiencies of the stages.

However there is no indication of how Stenhouse's concept of 'efficiency' might be quantified as a dimensionless quantity on a ratio scale, as required for such a calculation. (One cannot meaningfully multiply two temperatures on the celcius scale, for it has an arbitrary zero and thus is not a ratio scale. One cannot multiply two masses to get a third mass, for mass is not a dimensionless quantity.) As for the term 'resultant', its rigorous usage concerns vector quantities, not scalars such as efficiency. It may of course be that a different interpretation was intended. However unless some rigorous interpretation of Stenhouse's terms is available, it appears that any sense of order and rigour introduced by those terms is spurious.

(2) C-factor : The central memory store - Here Stenhouse's metaphor is that of an office "card index system" or card file. This is a familiar system with its own characteristic principles of operation. As with any metaphor, the shared familiarity saves both writer and reader the trouble of working through an explicit formulation of those principles. The effectiveness of a metaphor or analogy depends upon just this shared familiarity of somewhat stereotyped phenomena. However for a critical analysis of the metaphor we do need an explicit formulation of what everyone knows about card files, such as the following:

Each item of information placed in a card file remains unchanged and in the same place until altered or removed by some operator from outside the system. Bits of information are grouped together in a hierarchical system of categories, corresponding to cards, marked-off sections of a drawer, whole drawers, and so on. The card file is totally passive: ideally nothing happens except through the actions of an external operator. In practice precautions are taken to maximise conformity

to this ideal, such as the use of permanent inks and the exclusion of agents of change such as mice and children.

Such are the principles of operation of the familiar card file. However Stenhouse's (1974:72f) is no ordinary card file:

"...the cards must not be confined rigidly to the drawers in which originally they happened to be placed; they must be capable of translocation from one drawer to another, so that the system becomes not only flexible but also dynamic... This requirement is sharply accentuated when it is considered that the requirements for action, for behaviour, are often of a more or less emergency nature".

Here we have lost contact with the common reality of card files. Short of tumbling a card file in a concrete mixer, it is difficult to make it "not only flexible but also dynamic". One could of course postulate a human operator as part of the system, but as the system is part of an explanation of human intelligence this would amount to a little-man-inside theory. Stenhouse made no such postulate, but left the reader to imagine the mechanisms which might give a card index system the principles of operation required.

The requirement that the memory system be 'flexible and dynamic' was elaborated (Stenhouse 1974:70-73) to the following principles of operation:

1. Selective abstraction and discarding to prevent overloading.
2. Short-term and long-term memory, with selective transfer from short-term to long-term memory.
3. Recall of related patterns rather than of isolated items.
4. Formation of new patterns within memory.

Such is the process supposedly represented by Stenhouse's 'card index file' metaphor. Expressed independently of that metaphor, the above principles of memory would be a useful description of its functional characteristics.

The particular metaphor chosen is so incongruous as to be more misleading or confusing than helpful; any sense of order and rigour introduced by its use appears to be spurious.

Here we digress from the specifics of Stenhouse's theory to some general comments on the use of metaphors (including models and analogies) to represent the mind or the dynamics of behaviour, and a note on de Bono's 'special memory surface' which provides an alternative metaphor to Stenhouse's card file.

A metaphor may be valuable as a technique for representing a process which would otherwise be difficult to imagine or even perceive. (For the dependence of perception upon metaphor and myth see Feyerabend 1975.) However, to avoid misleading or confusing one's readers one must select as the 'parent situation' of the metaphor some system which does feature operating principles very similar, and preferably isomorphic, to those of the phenomena being represented. Selecting an appropriate metaphor is not likely to be easy when the phenomena to be represented are as complex as the behaviour of the intact organism. The usual approach has been to take the metaphor from the most complexly-functioning contrivance of the day. For Descartes (Skinner 1931:432-434; Millenson 1967:4-6) it was the hydraulic figures in the French royal fountains; early in this century it was the telephone exchange; today it is the computer (cf de Bono 1969:21f for an argument similar to the following). We may commend Stenhouse for spurning fashion and employing the humble card file. However card files and computers alike are common contrivances precisely because they perform well certain functions which most humans do poorly, such as storing information unchanged. This alone indicates that they are unlikely to conform to similar working principles, and the indication is strengthened

when we note that conversely humans do easily what no ordinary card file can do: recall as an integral pattern what was received as separate items, and likewise all of the four principles of memory derived above from Stenhouse's account. Likewise the most advanced computers may be developing some ability to read ordinary printed text, whereas a human after learning to read one alphabet can read a variety of typefaces, poorly-formed handwriting, poorly-reproduced typescript and even a substantially different alphabet (such as the children trained on the Initial Teaching Alphabet met in ordinary books) with no special training and often with ease; on the other hand even the earlier digital computers performed arithmetic at a speed and precision far greater than most humans could match. (A similar contrast can be seen between the analogue and digital types of computer, whose working principles are quite different: each type can, if of sufficient complexity, mimic the other but with a severe handicap in terms of speed of operation and range of problems that can be solved at all.)

Noting that computers and the like have misled inquiries into the principles of human thought, de Bono (1969) devised as a metaphor an electro-optical system which is a two-dimensional array of cells each of fairly simple design; the whole is perfectly constructible. He called it the 'special memory surface' or the 'thousand bulb model'. Functionally it appears to be an apt metaphor, to the extent that the four principles of human memory listed above may be discerned in its workings<sup>1</sup>.

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1. I do not share the reviewer Hunter's (1977:125) unsubstantiated view that "these arresting comparisons between mechanism and mind are often more ingenious than just". On the other hand, neither can I rigorously substantiate my own view on this, for until the system has been constructed one is restricted to imaginative inference of its functioning based on familiarity with vaguely comparable systems.

Structurally it is also realistic, to the extent that its topography and its pattern of internal connections are recognisably related to those of the brain-retina system. It provides an alternative to the favoured atomistic (cf Allport 1955:9f; Bohm 1970) information theory, for it has no discrete and stable 'bits' of information. Being an intrinsically self-active perceptual system, it is self-contained with neither operator nor any system to transform visual images into input data coded in some category system (contrast Bruner's (1973:7) axiom that perception is an act of categorisation, critically discussed by Southon 1976). Pattern recognition is also intrinsic: the special memory surface needs no associated process of information retrieval from a distinct memory as would pattern recognition in a digital computer.

Here we may note a pattern of progress in physiological psychology consistent with Skinner's practice of leaving physiology to the physiologists:

Behavioural research generates behavioural facts, then physiologists such as de Bono may generate physiologically scrutable representations of such facts.

(As it happened the behavioural facts used by de Bono were not Skinner's but those he gathered himself - cf de Bono 1971.) However the generation of new physiological representations (such as de Bono's metaphor) is obviously hindered if, as is usually the case, the behavioural facts are expressed in terms of some existing physiological metaphor. According to Skinner (1969:282) "the unhappy result is that physiologists usually look into the black box for the wrong things", a trap which de Bono, despite his qualifications in both psychology and physiology, seems to have avoided.

De Bono's model of brain processes is clearly at an early stage of development; however if the above

discussion is sound then the model deserves recognition as a substantial step in the right direction which could prove as productive for psychology as the hydraulic model was (and still is in teaching) for the theory of electrical circuits. Some alternative to the metaphor of storage in memory is urgently needed, as this metaphor has caused a great deal of trouble (Skinner 1969:272-280; 1974:119-122; Atkinson 1976). We may close this aside on metaphors with a comment from Skinner (1974:122):

"It is not the behaviourist, incidentally, but the cognitive psychologist, with his computer-model of the mind, who represents man as a machine".

Such is the model of mind implicit in Stenhouse's C-factor.

(3) A-factor : Abstracting and generalising. Stenhouse (1974:78) described the A-factor thus:

"There must be an ability for seeing similarities and differences, if some memory items rather than others are to be selected to act as modifiers of present behaviour. Such perception of similarities and differences depends, logically, upon a power of abstraction and generalisation...".

The first sentence is unproblematic, if we allow that "seeing similarities and differences" may be interpreted as 'seeing a stimulus A as similar or different to a (present or past) stimulus B'. However the second sentence appears to claim something more than this: there is in addition a power, upon which the phenomenon logically depends. It is difficult to see how this 'power' could be interpreted as anything more substantial than the 'dormative powers' of opium mentioned above.

(4) P-factor : Power of Postponement - This factor formed the theme of Stenhouse's work on intelligence; it was his distinctive contribution to the theory of intelligence in general and creative intelligence in particular. He described it thus (1974:80):

"...a power to delay or withhold the instinctive responses as an essential precondition for the emergence of adaptive variability from within the rigidity of instinct-systems".

As Stenhouse (1974:87) noted, this is similar to Pavlov's 'internal inhibition' and it is clearly a form of behavioural inhibition. At this point we move on from the factorial aspect of Stenhouse's theory and focus upon the relationship between the concepts 'instinct' and 'P-factor'.

#### 4.4 : Instinct and inhibition

In the following passage Stenhouse (1974:55) showed his usage of the term 'instinctive':

"The behavioural repertoire of an instinctive individual consists of a number of inbuilt responses to the stock situations likely to be encountered in the course of the normal way of life in the typical habitat of the species... we can generalise that instinctive behaviour is adapted to the normal, average, or standard conditions under which the species lives : instinctive behaviour patterns therefore tend to be stereotyped and automatic. The essence of instinctive as contrasted with intelligent behaviour is its conservatism,...".

To this concept Stenhouse assimilated learned behaviour:

"...learned components of instinctive patterns, once learned, are as fixed and unvarying as extreme 'instinctive' actions" (original emphasis).

The key characteristic of instinctive behaviour, implicit in the above and more explicit elsewhere is perseveration : an instinctual or habitual response may continue indefinitely, regardless of its consequences except that there is usually some level of intensity of punishment which is sufficient to terminate the behaviour (Stenhouse 1974:87). (He used the term 'negative reinforcement' in place of 'punishment' - a common and bothersome confusion.) Thus the function of the P-factor is to inhibit the otherwise automatic and perseverative responses of habit

and instinct.

In developing a critique of the P-factor, we follow J.W.N. Watkins' (1964) suggestion that an account of the historical origins of a concept often aids the understanding of it rather more effectively than does the more authoritative and conventional style which he called 'didactic deadpan'. So we will sketch the development of both ethology and behaviourism from the primitive common stock of pre-Pavlovian reflexology. (General sources are Stenhouse 1974:34-41 and Skinner 1931; 1937.)

At some risk of oversimplifying the history, it may be said that reflexology before Pavlov was the study of respondent elicitation without regard to the history of the stimulus for the organism concerned. This conforms to the philosophical stereotype of cause-effect relationships, which stands in contrast to the interactive principles of Galilean science (cf Henderson 1935:12f; Millenson 1967:359-362). Reflexes also lent themselves readily to reductionist neurological explanation in terms of the 'reflex arc'; the resultant concept of innate, structural connections between stimulus and response appears to be the ancestor of the concept 'instinct'. To this concept of fixed and unchanging reflexes Stenhouse (1974:37) erroneously assimilated Pavlov's results on conditioned reflexes:

"...it must be recognised that conditioned reflexes once established are just as invariable as structurally or physiologically fixed reflexes. The dog can be conditioned to salivate in response to the ringing of a bell... but once it has been conditioned it cannot not respond to the conditioned stimulus, neither can it respond to a different stimulus to which it has not yet been conditioned".

Digressing, we may note that here Stenhouse appears to be following Pavlov's (1927:7) view of the nature of a science of behaviour:

"Our starting point has been Descartes' idea of the nervous reflex. This is a genuine scientific conception, since it implies necessity... a stimulus appears to be connected of necessity with a definite response, as cause and effect. It seems obvious that the whole activity of the organism should conform to definite laws".

Pavlov (p8) further compared reflexes to "the driving-belts of machines of human design" - a metaphor also to be found in the writings of Karl Marx, another writer whose philosophy owed more to cause-effect determinism than to Galilean science and who nevertheless claimed the prestige and authority of the scientific tradition. The metaphor of driving-belts itself requires closer scrutiny : as anyone with practical experience with machines knows, there is nothing strictly inevitable about their interconnections. Breakdowns can occur at any moment, as when a weld breaks or a belt flies off without warning. Both the normal functions of mechanical linkages such as driving-belts, and the various processes by which they fail, are accepted as proper subjects for scientific investigation - as in the physics of mechanical failures. As the necessity of connections invoked by Pavlov as a criterion for science is not evident in the practice of modern physical science, which is apparently none the worse for neglecting this criterion, it would be difficult to defend Pavlov's insistence upon it for behavioural science.

Here we have evidence that the feedback from technologists to scientists mentioned above (s2.8) has not occurred where it might have. Even the discussion by Millenson (1967:360f) of the supposed law that pure water at atmospheric pressure boils at 212°F, shows an ignorance of the vagaries of boiling liquids that any practising chemist could have put right: far from it being inevitable that a pure liquid boils on reaching its 'boiling point', it is liable to superheat to some higher temperature and then erupt violently. (It is to

prevent this that boiling-chips are used.) Such are the hazards of lack of interaction between diverse fields of study (cf Bohm 1970).

However, despite Pavlov's philosophy of science, his (1927) experimental results on extinction clearly showed that the dog once conditioned may indeed not respond to the conditioned stimulus. Likewise (cf Skinner 1938:174) it may indeed respond to a different stimulus to which it has not yet been conditioned. These effects constitute the demonstrations of the principles of respondent extinction and respondent stimulus generalisation respectively (s3.1 above). Thus Pavlov's results did not conform to the paradigm of rigid determinism. The appearance of consistency was preserved, but only by the ad hoc postulation of 'inhibition'.

Worse trouble was in store : at times the stimulus was not to be found. Pavlov (1927:11f) dealt with this by the ad hoc and ad lib postulation of reflexes with unidentified stimuli, such as the 'freedom reflex' and the 'investigatory reflex'. Other workers adopted different strategies when studying responses which occurred in the apparent absence of corresponding stimuli.

Ethological theory was founded with Lorenz's observation of 'vacuum activity' in a captive starling which went through the motions of hunting and eating non-existent insects despite having an abundance of other food. Clearly nothing like the required stimulus was present. Lorenz remained faithful to the quest for causal explanation in terms of some identifiable stimulus. Unable to find any outside the organism he postulated one inside in the form of an accumulation of 'action-specific energy' (A.S.E.). An external stimulus, such as a flying insect for the starling, was assimilated to this theory as the 'releaser stimulus' which triggers

the release of the A.S.E. and thus the response. However in the absence of any external releaser stimulus, an over-accumulation of A.S.E. can trigger its own release. This is the internal stimulus which causes the observed 'vacuum activity'. The triggering process is explained (Stenhouse 1974:41) in terms of

"A hierarchical model of the neural mechanisms (understood in a functional rather than a structural sense) of the central nervous system, which mediate the causality of behaviour and thus achieve control and directiveness.

Included here are the Innate Releasing Mechanisms".

The qualification in brackets presumably indicates that these neural mechanisms are inscrutable : no neurologist should expect to find them in the brain, and the ethologist is excused from the task of producing diagrams representing constructable mechanisms which would demonstrate the working principles in question.

Such inscrutable mechanisms are not new in the history of science; there is a disreputable precedent in the Ptolemaic astronomy which preceded the Copernican revolution (Koestler 1959:69-83). It may of course be argued that the similarity between ethological and Ptolemaic concepts is purely superficial and obscures a more crucial contrast. However that remains to be shown. In view of the deadening effect which the Ptolemaic approach had upon progress in astronomical theory, any apparent similarity between a modern science and Ptolemaic astronomy is cause for some concern and a closer investigation than will be attempted here.

A more specific criticism of those ethological concepts, especially interesting in view of the unrestrained criticisms of behaviourism by ethologists (and, ironically by Koestler), was made by Skinner (1969:183):

"We no longer use dynamic analogies or metaphors, as in explaining sudden action as the overflow or bursting out of dammed-up needs or drives [or A.S.E.].

If these are common practices in ethology, it is evidently because the functional relations they attempt to formulate are not clearly understood".

Skinner's argument here takes the form of a comment on the 'state of the art' in ethology, by analogy with the earlier stages of psychology and its subsequent behaviourist development. Skinner's statement may be expanded along the following lines : The use of dynamic metaphors was once common in psychology but has been abandoned within behaviourism as more direct and literal representations of functional relations have proved to be more adequate and less misleading. The implication is that the continued use of such metaphors in ethology reflects poorly on its state of development and might likewise be eliminated by the adoption of more direct and literal representations.

Skinner (1937:491) reached the same turning-point as Pavlov and Lorenz when, after doing Galilean experiments within the reflexology paradigm, he came to see that "there are responses uncorrelated with observable stimuli". Unlike Pavlov and Lorenz he took the more revolutionary path : he abandoned the quest for a causative stimulus (much as Kepler had abandoned the quest for uniform circular motion in the planetary orbits) and developed a radically different concept of the causation of behaviour. This was in contrast to the push-pull mechanisms of commonly-understood machines and defied reconciliation with the prevailing atomist, reductionist concept of 'cause' (cf Day 1969). Skinner (1974:40f) later represented his concept of causation as selection of responses according to their immediate consequences, analogous to Darwinian selection of genetic traits according to their longer-term consequences for the survival of the genes carried by the organism (cf Campbell 1975). Skinner (1974:41; cf 1972:353) noted

that "There was prior to Darwin little or nothing in physical or biological science that foreshadowed selection as a causal principle", and (1974:63) that there are marked similarities between the debate over evolutionary theory and the debate over behaviourism. Expanding the latter point, both the vulgarisation of Darwin's theory into the politically-loaded slogan 'Survival of the Fittest' (as recounted by Kropotkin (1904:1-5), and subsequently continuing despite his scholarly clarification of who are the fittest) and the misrepresentations of Skinner's behaviourism as an S-R theory (s3.2 above) may be represented as attempts to assimilate the new theory into an old concept of causality, rather than accommodate the concept of causality in general to the new theory. It appears that in retaining old concepts of causation, it is the ethologists rather than the behaviourists who have failed to accommodate the implications of the Darwinian revolution.

Given that the above is a fair representation of the history, it appears that ethology remained faithful to the principle of mechanical causation, implicit in reflexology, that the organism cannot not respond to the stimulus. Where Pavlov invoked inhibition, Stenhouse invoked the P-factor. Skinner (1938:96f, cf 3.2 above) criticised Pavlov's concept of inhibition; let us see how Skinner's line of argument might be modified as a reply to Stenhouse.

Ethological theory represents behaviour in terms of fixed, automatic responses which (in the form of A.S.E.) build up inside the organism and must be released in the form of the corresponding responses. Let us consider an organism which is inactive in the presence of a stimulus to which it has previously shown a strong instinctual or habitual response. (To relate this to the issue of intelligence, we may allow that such

temporary inactivity will sometimes be adaptive if only because it allows time for more thorough perception of the whole situation, which in turn allows behaviour to come more accurately under the control of the organism's history of reinforcement in similar situations.) In Stenhouse's theory this inactivity is represented as the action of P-factor inhibition upon the built-up A.S.E. which would otherwise be released by the stimulus (as 'releaser stimulus') acting through the innate releaser mechanisms. This is an inelegantly complex representation for an observed state of inactivity. More seriously, the postulated complex processes within the organism are so inscrutable as to be, in the words which Skinner (1938:97) used of Pavlovian inhibitory processes, "little more than mythology". Further, a subsequent reinstatement of the response must be represented as a cessation of P-factor action, although it has all the external properties of conditioning the response originally. This can hardly be justified by representing original conditioning also as a cessation of P-factor action, since we would be led to the absurd conclusion that all possible responses pre-exist in the organism, each with a store of accumulated A.S.E., the release of which is being inhibited by P-factor action (cf Skinner 1938:97 for this paragraph).

It may be objected that behaviourism itself assumes that all possible responses pre-exist within the organism prior to their emission. Skinner's reply was that the emission of a response does not imply its pre-existence within the organism any more than the emission of light by an incandescent light-bulb filament implies the pre-existence of light within the filament.

The problem of P-factor inhibition is not restricted to cases of inactivity: it arises in modified form when what is observed is not inactivity

but some behaviour which is incompatible with the instinctual or habitual response for that situation. Common to both forms of the problem is that a previously-strong response does not occur, and the complex ethological representation of the non-occurrence of that response. What is observed may in the latter case be termed 'variability' rather than 'inactivity', in that the response has varied.

This is not to criticise Stenhouse's argument for the importance of such inactivity and variability in the phenomena of intelligent behaviour. The point at issue here is the value of Stenhouse's representation of that inactivity and variability in ethological terms. In Chapter 5 we will see how behaviourism gives a more elegant representation of such inactivity and variability.

#### 4.5 Evolutionary continuity

Stenhouse's theory does follow the implication of the Darwinian revolution for the theory of human intelligence, even if not the implications for the nature of causality. The implication followed is that human intelligence is not categorically unique and discontinuous from animal intelligence, but has been gradually shaped out of the behavioural patterns of lower animals by the process of natural selection. It is in giving an account of this process that Stenhouse's theory is of the evolution of intelligence.

Evolutionary continuity poses a special problem for an ethological theory of intelligence, for intelligence is represented as a non-occurrence or failure of instinctual behaviour patterns: as inhibition of responses which are normally and adaptively automatic, and as variability in responses which are normally and adaptively stereotyped. Such deviations from highly organised behavioural patterns would usually be

maladaptive and thus would be culled out rather than favoured by natural selection (Stenhouse 1974:56). Stenhouse proposed a resolution to this paradox; however it is sufficient for this discussion to note that the paradox is a consequence of the ethological concept of 'instinctual behaviour' (s4.4 above) and does not arise in a behaviourist theory of intelligence.

This concludes the critical discussion of Stenhouse's theory. A variety of objections are raised. Some may be merely at the stylistic level or concern lack of clarification of certain points; some may be equally applicable to several other theories of behaviour; some may bear on ethological theory in general rather than Stenhouse's particular application of it to intelligence. The strengths of Stenhouse's theory have largely been left unmentioned. This chapter is not claimed to be a balanced review of Stenhouse's theory, but rather a catalogue of its weaknesses (assuming the Galilean ideal of science) that might be improved upon in a behaviourist theory of the same phenomena.

## CHAPTER 5 : A Behaviourist Theory of Intelligence

### 5.1 Skinner on intelligence

Skinner (1969:183) indicated a behaviourist representation of intelligence and its evolution, in the following excerpt from a discussion on the phylogeny and ontogeny of behaviour (i.e. the 'nature-nurture' issue):

"Another kind of innate endowment, particularly likely to appear in explanations of human behaviour, takes the form of 'traits' or 'abilities'. Though often measured quantitatively, their dimensions are meaningful only in placing the individual with respect to a population. The behaviour measured is almost always obviously learned. To say that intelligence is inherited is not to say that specific forms of behaviour are inherited. Phylogenetic contingencies conceivably responsible for 'the selection of intelligence' do not specify responses. What has been selected appears to be a susceptibility to ontogenetic contingencies, leading particularly to a greater speed of conditioning and the capacity to maintain a larger repertoire without confusion".

Now the 'repertoire' is the set of differentiated responses available to an organism at a given time; 'maintenance of a repertoire without confusion' may be interpreted as the continuance of control of distinct responses by distinct stimuli (whether by operant or respondent discrimination), as opposed to a blending of responses and stimuli according to the principles of generalisation and variability. The capacity to maintain a large repertoire is clearly important in intelligence; however to simplify the discussion we will focus upon the processes of acquiring a large repertoire as distinct from maintaining it. The processes of acquiring a repertoire are simply the processes of operant and respondent conditioning. Thus we may arrive at the following proposal:

Intelligence may be represented as susceptibility to ontogenetic contingencies, that is, to the

processes of operant and respondent conditioning.

The term 'susceptibility' may require some clarification. It is used in a sense consistent with the C.O.D. (4th ed) definition of 'susceptible' as "admitting of ..., open or liable or accessible to ..., impressionable, sensitive, ...". 'Susceptibility' may be used in the wider sense of 'susceptibility to contingencies', but here it is used mostly in the more specific sense of 'susceptibility to a particular process whereby the contingencies alter or maintain the organism's behaviour'. Susceptibility to a process such as operant reinforcement, as distinct from the process itself, is a variable descriptive of the individual organism with respect to that process. It is roughly indicated by the rate or speed at which the process occurs - as measured, for example, by the time or the number of reinforcers taken for the frequency of response to rise to ten times the operant rate (in the case of operant reinforcement). To put the matter more rigorously, again for operant reinforcement, the rate at which that process occurs in a given organism is a function of several variables such as the physical properties of the reinforcer, the time delay between the response and the reinforcer, the schedule of reinforcement, and the organism's state of deprivation with respect to that reinforcer. Another of those variables is the organism's susceptibility to the process of operant reinforcement. Different variables will of course be relevant to different processes.

Organisms may be compared with respect to susceptibility to a particular process, on an ordinal scale, by comparing the rates at which that process occurs for the respective organisms - with the other controlling variables for that process held as constant as is required to give a stable rank order of rates. Further to a strictly ordinal measurement, qualitative

comparisons of differences between susceptibilities may be made. Where the difference in susceptibilities between two organisms is small, precise constancy of the other controlling variables may be required. For larger differences in susceptibilities (an extreme case is Bitterman's (1960; 1965) comparison of different species), the other variables may be allowed to vary to a greater or lesser degree without altering the rank order of rates.

The above simplified interpretation or 'definition' of intelligence shares with Hebb's (1972:163) 'intelligence B' the feature that it refers to the state of an organism at a given time, without distinguishing between the effects of genotype and of developmental history. It also avoids confusion between intelligence and successful performance in general, consistent with the educational use of intelligence tests as a predictor of future learning as distinct from a measure of past learning. (The distinction is important in that successful performance of particular tasks may be attained by slow learning under prolonged exposure to optimal instructional contingencies, whereas the intelligent individual is characterised more by speed of learning particularly under suboptimal contingencies. Where instructional contingencies are suboptimal, as alleged by Skinner in regard to schools (1968:119, "The method does not teach; it simply selects those who learn without being taught"), it follows that intelligence (in the proposed sense) is highly correlated with successful performance in general - in conformity with the current usage of the term 'intelligence'. However the correlation may vanish under optimal contingencies, as suggested by Skinner's (1951:561) observation that under systematic operant conditioning "all normal dogs learn with about equal facility".) Another point in its favour is that the proposal avoids the view that intelligence is whatever

intelligence tests measure - a view which, through its implication that the tests are valid by definition, confounds any critical examination of the tests and their uses.

The above behaviourist interpretation of intelligence may be compared with Stenhouse's ethological view (cf s4.2 above):

Intelligence is the capacity for behaviour that is adaptively variable within the lifetime of the individual.

The two interpretations are equivalent to the extent that most if not all of the processes of operant and respondent conditioning are processes by which behaviour varies within the lifetime of the individual. Further they are processes by which the behaviour of the organism adapts to the prevailing contingencies of reinforcement, although there is a complication in the concept of 'adaptation'.

The complication is common to both evolutionary and behaviourist paradigms, on their different time-scales of generations and seconds respectively: the optimisation of short-term adaptation sometimes leads the species or organism into a 'blind alley' which proves maladaptive in the long term. The dinosaurs and many other species achieved a high degree of adaptation to the prevailing contingencies of survival, but in that process of specialisation they lost the variability of form and function which characterised those species which did survive the subsequent changes in the contingencies. The behavioural analogues occur in several forms. One was illustrated by Skinner (1969:192):

"A hungry pigeon which was being trained to guide missiles [Skinner 1960] was reinforced with food on a schedule which generated a high rate of pecking at a target projected on a plastic disc. It began to peck at the food as rapidly as at the target. The rate was too high to permit it to take grains into its mouth, and it began to starve".

(This may be interpreted as operant response differentiation (with regard to rate of responding) combined with a lack of operant cue discrimination (with the food as positive cue for slow pecking) and provides a ready example of failure to maintain a repertoire of two response-rates without confusion.) Other forms of conditioning which can prove maladaptive include continuing to respond to a 'stretched' schedule such that more energy is consumed in the responses than is available from the reinforcers (Skinner 1969:119), 'superstitious' conditioning (Skinner 1969:177), and the mutually painful interactions, so often seen between parents and children, analysed by Skinner (1951:565f). Subject to such qualifications of the interpretation of 'adaptation', it appears that the above ethological and behaviourist definitions of intelligence are equivalent.

### 5.2 Stenhouse's factors reinterpreted

In view of the above (s4.3) comments on the problems of factorial theories in general and Stenhouse's factors in particular, there seems little need to propose a behaviourist counterpart to Stenhouse's factorial theory. What behaviourism may reasonably be called upon to do is to account for the phenomena which are represented by Stenhouse's theory, and in particular to account for the prominent place of the inhibitory P-factor in Stenhouse's theory.

It was argued above (s3.2, 4.4) that the behavioural phenomena represented in terms of inhibition may more elegantly be represented in terms of operant and respondent extinction. An examination of the above (s3.1) list of principles of behaviourism shows that extinction occurs in three other principles, in addition to operant and respondent extinction as such:

Operant cue discrimination  
 Operant differentiation  
 Respondent stimulus discrimination.

These three principles in turn play key roles in most behavioural phenomena of practical importance. The learning of language and of most non-verbal skills has been represented in terms of operant cue discrimination in conjunction with operant differentiation (K & S:192-195). The role of operant cue discrimination in secondary positive reinforcement, and the pervasiveness of secondary reinforcement, were noted above (s3.1). Respondent stimulus discrimination, while not intrinsic to secondary negative reinforcement, plays an important role in practice: after respondent reinforcement with a noxious unconditioned stimulus, the response is elicited not only by the conditioned stimulus but also (in accord with the principle of respondent stimulus generalisation) by some range of similar stimuli. These then are also secondary negative reinforcers and will reinforce any operants which terminate them. Extreme cases of such generalisation are found in clinical phobias (cf K & S:125) but even mild generalisation may lead to avoidance of stimuli whether or not they are effective (i.e. correlated with a noxious stimulus). In general, avoidance of ineffective stimuli is maladaptive. Such avoidance may be eliminated by respondent stimulus discrimination: as the respondents to the ineffective stimuli undergo extinction, so these stimuli cease to be secondary reinforcers and in turn the avoidance operants reinforced by them selectively extinguish, becoming discriminated to those stimuli which are correlated with the noxious stimulus. Thus we may, at the level of abstract theory, account for the prominence of the P-factor in terms of the comparable prominence of extinction in the processes of operant and respondent conditioning.

Two examples may serve to illustrate these roles of extinction in evolutionary contexts. One is Bitterman's (1965) experiment in habit reversal learning in a variety of vertebrates: fish, turtle, pigeon and rat. Stenhouse (1974:289-292), while criticising Bitterman's interpretations, conceded (p322) the relevance of the experiments to the extent that the P-factor would greatly facilitate habit reversal learning. Such learning may be represented as operant discrimination training with 'reversals' : at each reversal the positive and negative cues are interchanged. Performance was recorded as the number of errors (responses to the negative cue) made between a reversal and the attainment of mastery level, which was set at six or fewer errors in a 40-trial day. Results were plotted as mean errors (for a group of subjects) against number of reversals. Thus the number of errors inversely represented susceptibility to operant extinction, and a progressive decline in the number of errors represented an increasing susceptibility to extinction - which may be seen as an increase in intelligence during exposure to a particular contingency of reinforcement. Comparing the various graphs given, it appears that susceptibility to extinction varied between species, in the ascending order: fish, turtles, pigeons, rats; conforming to their evolutionary sequence. The occurrence of a progressive increase in susceptibility also conformed to that sequence.

A second example involves a behavioural pattern central to Stenhouse's (1974:136-150) account of the evolutionary selection of the P-factor: the 'impassivity display' in both social dominance and parent-offspring relationships. The roles of parent and of dominant individual in a social group have this much in common: best results (in terms of evolutionary survival) are produced neither by indiscriminate activity nor by total

inactivity, but by considerable "forebearance" (Stenhouse) and ability to "keep your head when all about you, are losing theirs and blaming it on you" (R. Kipling) in conjunction with what may be loosely described as decisive and well-timed actions. More succinctly, what is required is a relaxed demeanour in conjunction with selective and skilled responses. Now a relaxed demeanour may be represented as the consequence of operant and respondent extinction, while skilled and selective responses are the outcome of operant differentiation and operant cue discrimination, both of which in turn depend on extinction. Thus it would follow that impassivity displays are favoured by susceptibility to operant and respondent extinction. (Since both terms are independently measurable, we may put this as a researchable hypothesis: that impassivity displays are highly correlated with susceptibility to operant and respondent extinction.)

If the evolution of intelligence consisted solely of increasing susceptibility to extinction, one would expect that the higher animals would show generally reduced levels of activity. However there is also susceptibility to operant and respondent reinforcement to be considered. A concurrent increase in susceptibilities to extinction and reinforcement would account for such inter-specific constancy in general levels of activity as may be observed. One of many casual observations indicating that humans have exceptionally high susceptibility to reinforcement is in Skinner's (1951:564) account of how to train animals, where he noted that "the human infant can be reinforced by very trivial environmental events" such as the flashing on and off of a table lamp, whereas with a dog one gets comparable results only by conditioning a secondary reinforcer through making it a positive cue for feeding.

There appears to be scope for systematic interspecific studies of susceptibilities to reinforcement (cf Bitterman 1960 on interspecific behavioural methods.)

Stenhouse (1976:56) introduced a fifth factor in addition to his previous four:

"...a factor I to stand for a general level of instinctive drive or motivation,...[not as] strictly a factor of intelligence as commonly conceived... [but as one that] should be taken more into account than it is in current IQ tests".

It would appear that the phenomena represented by the I-factor are much the same as those represented by susceptibility to reinforcement.

However, as Stenhouse (1976:56) conceded, his discussion of the I-factor does gloss over a host of complexities in the field usually known as 'motivation'. In exploring a few of these complexities, we may shed a little light on the age-old problem of distinguishing between genius and madness and hint at some classroom implications. Roughly we may characterise the operant behaviours of at least the more solitary forms of both genius and madness as deviant in that they are not controlled by the usual reinforcers, which in general are the primary reinforcers (food, sex, pain, etc.) and the reinforcers emitted by other persons in the course of social approval and disapproval. The persistence of behaviours under such conditions, whether or not it is exceptional enough to be called 'genius' or 'madness', may be represented in three ways in accord with the principles of behaviourism. (It is not necessary to follow Herrnstein (1977) in confusing the principle of operant reinforcement by invoking 'self-reinforcing' responses; for a critique of a position similar to Herrnstein's see Catania (1975).)

(1) The behaviour may be composed of perseverative operants, continuing without reinforcement apart from the

effect of adventitious reinforcers which coincidentally follow the responses in question (as in superstition - K & S:102-104). Such perseveration may be purely a product of adventitious reinforcement, or it may be a product of past schedules of intermittent reinforcement (K & S:100-102). Even an individual of apparently normal intelligence can develop an impressive resistance to extinction under certain schedules (Bijou & Baer 1966: 752). For example, a habitual gambler may continue for long periods without winning.

(2) The behaviour may be under the control of secondary negative reinforcers, whether correlated or uncorrelated with any noxious stimulus in the manner of a phobia (cf above). For example, a student may produce unusually clear technical drawings after a history of being caned for every imperfection, and may continue to do so long after the threat of caning has ended.

(3) The behaviour may be under the control of non-social secondary positive reinforcers, which are the products of the individual's interaction with the physical environment. A student may produce unusually clear technical drawings after a history of constructing objects following others' drawings of varying quality, during which clarity of drawing became a positive cue for successful construction. We may hypothesise that the first two representations describe two varieties of 'madness' and milder forms thereof, while the third is a major component of genius. Such hypotheses could be tested against case-study material.

With regard to genius, a "highly consistent picture of the productive scientist" emerging from a variety of studies is quoted from Taylor and Barron (1963:385f) by Stenhouse (1974:207f). Susceptibility to non-social secondary positive reinforcement is implicit in the following items from the 'picture':

"1. A high degree of autonomy, self-sufficiency, self-direction."

"2. A preference for mental manipulations involving things rather than people..."

"4. A liking for method, precision, exactness."

"7. ... relatively little talkativeness, gregariousness, impulsiveness."

"9. Marked independence of judgment, rejection of group pressures toward conformity in thinking".

However data is inevitably selected by the representational techniques employed by the observers, and the above items show little indication that the various researchers were looking for indications of susceptibilities to reinforcement as such (except perhaps in item 4). An autobiographical passage which more clearly indicates the hypothesised function of non-social secondary positive reinforcers is however available from Skinner (1967:17; cf Forster 1970):

"Much more important [than the Protestant Ethic] in explaining my scientific behaviour are certain positive reinforcements which support Feuer's answer to Weber in which he shows that almost all noted scientists follow a 'hedonistic ethic'. I have been powerfully reinforced by many things: food, sex, music, art, and literature - and my scientific results. I have built apparatuses as I have painted pictures or modelled figures in clay. I have conducted experiments as I have played the piano. I have written scientific papers and books as I have written stories and poems. I have never designed and conducted an experiment because I felt I ought to do so, or to meet a deadline, or to pass a course, or to 'publish or perish'. I dislike experimental designs which call for the compulsive collection of data, and, particularly, data which will not be reinforcing until they have been exhaustively analysed. I freely change my plans when richer reinforcements beckon. My thesis was written before I knew it was a thesis. Walden Two was not planned at all. I may practice self-management for Protestant reasons, but I do so in such a way as to maximise non-Protestant reinforcements... in general my effects on other people have been less important than my effects on rats and pigeons - or on people as experimental subjects. That is why I was able to work for almost twenty

years with practically no professional recognition" (original emphasis).

The distinction between social and non-social reinforcers corresponds roughly to that commonly made between 'extrinsic' and 'intrinsic' reinforcement. In the classroom context it is a little too easy to designate only the teacher-controlled reinforcers as extrinsic, and to assume that academic behaviours occurring in their absence are being maintained by intrinsic reinforcement (cf Catania 1975). However we may distinguish at least four modes of persistence of behaviour other than teacher-controlled reinforcement. The most obvious is social reinforcement from persons other than the teacher, mainly peers. Three others follow from the preceding analysis: perseveration (including superstition), secondary negative reinforcement and non-social secondary positive reinforcement. Only the last is, in general, both sufficiently sensitive to the detailed topography of the behaviour (i.e. its effective skill) and sufficiently independent of conformity to social norms to support creative intelligence. There may be scope for systematic observation of various teaching styles to find those which best promote susceptibility to non-social secondary positive reinforcement.

The distinction between social and non-social reinforcers requires some clarification in situations where the skilled behaviour in question is itself an interaction with people - as in experimental research with human subjects (cf Skinner above) and in teaching. Non-social reinforcers in such contexts are the direct effects of exercising the skill, which for a science teacher might include the stimulus of pupils asking questions which contain the key terms of the topic being taught (cf Southon 1975). Social reinforcers are in general approval or disapproval, whether from the same

persons (eg. pupils) or from others such as the school principal. The distinction may be illustrated by a contrast between two hypothetical teachers. The classroom behaviours of one teacher are reinforced by pupils' increasing mastery of the subject-matter, regardless of complaints from both pupils and teaching colleagues that the teaching methods are old-fashioned (or too radical, as the case may be). The behaviours of the other teacher are reinforced by the approval and attentions of colleagues and pupils alike, regardless of the lack of evidence that the pupils are learning what they are supposedly being taught. The two teachers are under the control of non-social and social reinforcers respectively.

Of Stenhouse's four factors the one most in dispute above (s4.3) was the C-factor or Central Memory Store. Memory as the storage and retrieval of information or images is not invoked in any of the behaviourist principles (s3.1 above). The metaphor of memory as storage was criticised by Skinner (1969:273-280; 1974:119-122) and is one of the mentalist terms that a behaviourist theory does well without (cf Skinner 1969: 182f). The phenomena usually represented as remembering and forgetting are represented differently in behaviourism.

Skinner (1950:79f; cf 1960) demonstrated that "the mere passage of time between conditioning and extinction is a variable which has surprisingly little effect". Keller and Schoenfeld (1950:77-81) suggested that, in general, retention of conditioning is intrinsic; 'forgetting' is to be interpreted as the effect of extinction, reinforcement of incompatible responses to the same stimuli (or cues), and more generally confusion of the forgotten response with subsequent conditioning involving similar stimuli or similar responses. Skinner (1969:274) clarified the matter with the statement "What is 'stored' is a modified organism, not a record

of the modifying variables".

It might be objected that memory belongs to the category of cognitive processes, and that the treatment of memory in terms of behaviours therefore involves a category mistake. However what we have is a behaviourist representation of the phenomena usually represented as remembering and forgetting, rather than a behaviourist definition of the term 'memory' in its established usage. The term 'memory' with those implications involving a dichotomy between cognitive and behavioural processes, presupposes a particular theory: the dualist "official doctrine" described by Ryle (1949) which is apparently much the same as the mentalism which behaviourism is to supercede (Skinner 1974). If the above arguments (s3.1) that a science of behaviour is not in principle different from a physical science are sound, then the situation with regard to the term 'memory' is analogous to that with regard to terms such as 'phlogiston' (s2.9 above) and 'force' (s2.6 above). In developing a new theory one may reject a given term of an existing theory altogether, as modern chemists have done for 'phlogiston', or one may give it a revised usage (and hence implications), as Newtonians have done for 'force' and as Einstein has done in turn for many Newtonian terms (Feyerabend 1970b:221). In neither case do the scientists concern themselves greatly with the previous usage and implications of the term; they are certainly not expected to preserve or respect those usages. Thus a behaviourist can hardly be criticised for violating the implications of the mentalist usage of the term 'memory'. What may be expected, and what are offered by behaviourists, chemists and physicists alike, are representations of the respective phenomena previously represented by the term in question. Sources of such behaviourist representations are cited above (s3.3).

The nature of the exercise of reinterpreting the phenomena of a rival theory was expressed, in the case of behaviourist and psycholinguistic theories, by Catania (1973:15):

"Hopefully, we have at least shown that what can be said in the language of psycholinguistics can also be said in the language of behavior: there is nothing in psycholinguistics which the behaviorist must pass over in silence".

It might also be noted that the term 'category mistake' was introduced by Gilbert Ryle (1949:16-24) by way of showing the absurdity of the mentalist "official doctrine". Indeed Ryle's argument was that mentalist concepts should be reinterpreted in behavioural terms, when a rigorous and non-misleading interpretation is required.

The above objection appears to presuppose the widely-held doctrine that common usages of terms are in some sense inviolate, or at least privileged over or more fundamental than scientific usages of the same terms. This doctrine received some critical attention from Feyerabend (1961:67; cf 1955):

"...all the subtle and boring analyses of the various linguistic schools of today, Wittgensteinians included, proceed from the assumption that the common idiom and the common belief behind it is a good basis for philosophy",

and from Sellars (1958:288):

The motto of the age of science might well be: Natural philosophers have hitherto sought to understand 'meanings'; the task is to change them" (original emphasis).

Feyerabend (1963) has elaborated this view in terms of the mind-body problem. More generally, Feyerabend (1975) argued that the demand for meaning invariance would, if enforced, prevent major scientific progress such as the Copernican Revolution (cf Feyerabend 1965b).

The remaining two factors are readily reduced to

behaviourist principles. The D-factor of Sensorimotor Efficiency may be represented as susceptibility to operant discrimination and differentiation in the context of non-verbal skill learning. The A-factor of Abstracting and Generalising may be represented as susceptibility to operant and respondent stimulus generalisation.

### 5.3 Evolutionary continuity

To simplify matters a little, we may say that the behaviourist theory represents intelligence as susceptibility to reinforcement and extinction. The limiting case of zero susceptibility corresponds to Stenhouse's ideal (s4.4 above) of instinctive behaviour as proceeding regardless of its consequences and uncontrolled by any stimuli other than the innate releaser stimulus. Thus, contrary to common belief, behaviourism does not exclude instinctive behaviour from its frame of reference. Skinner (1969:200) noted that nest-building in pigeons is not learned, and his reasoning is instructive with regard to the issue of field versus experimental studies raised above (s3.2):

"Not so long ago it might have been possible to debate whether a pigeon somehow or other learns to build its nest, but now that we have examined the behavior of pigeons under a fairly wide range of contingencies, we can be sure that it does not. A program which would shape the behavior of building a nest, with no contribution whatsoever from genetic endowment, can almost certainly not be arranged. If the pigeon had an inherited capacity to be reinforced by various stages in the construction of a nest, the assignment would be less difficult, but still staggering".

Thus the opinion as to whether a given behaviour observed in the field is controlled by phylogenetic (innate) or ontogenetic (environmental) variables is not based solely on field studies, where neither class of

variables can be systematically manipulated, but depends on laboratory studies where at least the ontogenetic variables can be manipulated. If in the laboratory we can demonstrate sufficient susceptibility to reinforcement, extinction and the other processes of conditioning to account for the given behaviour being learned through exposure to the environmental contingencies prevailing in the field, then we may assume that the behaviour is learned in the individual. However when systematic attempts to demonstrate the required susceptibilities have failed, as in the above case of pigeons, it becomes reasonable to assume provisionally that the given behaviour is innate or instinctual.

In terms of evolutionary continuity, the evolution of intelligence may be represented as a gradual increase, from a low or zero level, of susceptibilities to reinforcement, extinction and all the various processes of conditioning. As these susceptibilities increased, the ontogenetic contingencies had a gradually increasing effect on the organisms' behaviour, which thereby became less controlled by phylogenetic contingencies (cf Skinner 1969:205). Both types of contingency remain important and show several interactions (Skinner 1969:203-208). We may note that this account is free of the paradoxical form of the ethological account (s4.5 above).

This concludes the behaviourist interpretation of the nature and evolution of intelligence, with special reference to Stenhouse's factors. Insofar as the case argued here is sound, the way is now open for some future behaviourist reinterpretation of Stenhouse's (1974) account of the role of creative intelligence in society.

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Where an article is known to be reprinted in a collection, a reference to the collection is also given following a stroke (/). The cited source is indicated by (cit) after the pagination. A collection containing several cited articles is listed separately and cross-referenced in the entry for each article.

An author's first name is generally given where known, as this sometimes speeds the use of a library author catalogue. Book subtitles are generally included as they may assist a decision on whether to obtain the book. Issue numbers for journal articles are given where known (in brackets after the volume number) to assist in cases of unbound holdings. Some redundant information is omitted, such as the locations of a few well-known publishers located in lesser-known places (eg. Penguin), or where the location is part of the name (eg. University of Chicago Press). 'University Press' is abbreviated to 'U.P.' (eg. Oxford U.P.).

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rather than to preserve strict uniformity of format at the expense of including redundant information and excluding some that may be useful.

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