PSYCHOLOGICAL BEHAVIORISM, EXPLANATION AND ACTION

by

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ABSTRACT

This study examines the nature of explanation in psychological behaviorism and the relation of behaviorism to commonsensical and philosophical discussion of action. The central thesis in the study is that the type of explanation in behavioral psychology provides a correct but limited approach to understanding important aspects of action. This thesis is supported by an examination of representative criticisms, an analysis of explanation per se, an analysis of the concern within behavioral psychology for action as a class of behavior, and finally, by an argument for compatibility of behavioristic and intentional explanation in which the different explanation types are shown to be appropriate to particular questions.

The study begins with a brief sketch of the behaviorist position and then moves to an examination of representative analytic criticisms; all of which suggest that behaviorism, in spite of whatever facts it offers up, fails to explain action. An analysis of explanation is then given in which a classification of different types of explanation is presented, criteria of adequacy and relevancy are developed and the significance of context dependency for explanation is

demonstrated. The analytic distinctions developed in the consideration of explanation are then applied in an examination of the development of a behavioristic account of action. This leads to a more detailed consideration of some of the work of B. F. Skinner and, in particular, of his claim that a behavioral analysis on the model of operant conditioning can deal adequately with intentional action.

The questions about adequacy of explanation raised in the criticisms of behaviorism are then addressed and it is argued that some of the criticisms rest on a misunderstanding of the explanatory project in behaviorism. Finally, on the basis of the foregoing analysis, the question of the compatibility of the behaviorist position with intentional explanation is examined. Here it is argued that, while a demonstration of compatibility based on an analysis of wants and beliefs as causes of action is inadequate, compatibility can be demonstrated in terms of context dependency.

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CHAPTER 1

INTRODUCTION: BEHAVIORAL PSYCHOLOGY AND THE QUESTION OF EXPLANATION

"Behaviorism" Defined

The word "behaviorism" has a number of related meanings. In order to avoid confusion it is important to make some distinctions between them. Of interest to philosophy, the most important distinction is between logical (or philosophical) behaviorism and psychological behaviorism. Philosophical behaviorism is a *position* within the philosophy of mind, and is perhaps best exemplified by Gilbert Ryle's *The Concept of Mind*.[1] It is essentially a position opposed to Cartesian dualism and is based on analysis of:

(a) what it makes sense to say about mental states,

(b) the criteria for use of third person psychological sentences, and

(c) the translatability of mental statements into physical behavioral statements.

Psychological behaviorism is a school of psychology concerned with giving a scientific account of the behavior of organisms. It is psychological behaviorism and not philosophical behaviorism which I am primarily concerned with

here. However, this is not to deny that some psychologists have also maintained philosophical behaviorism.

Psychological behaviorism is often misunderstood in a number of ways: (a) with regard to its specific claims; (b) its range of positions; (c) its use of theory; (d) its insistence on an objective methodology; and (e) its logic of explanation.

The behaviorist position does though have some general characteristics and specific claims which are familiar and reasonably well understood. Let me begin then by sketching a general position before focusing on less familiar details. Behavioral psychologists have a diversity of views about aspects of methodology and about basic assumptions. Thus the general position I have outlined below only approximates the views of any particular psychologist. Each would refine or change it to some degree. The outline, however, helps to provide a focus for the criticisms that follow. Roughly, then, psychological behaviorism makes the following claims:[2]

1(a) The basic subject matter of a science of behavior should, to a large degree, be restricted to observable data, i.e. first, the observation of physical movements of organisms including measurement of intensity, duration and rate, and second, the observation of changes in the environment of the organism. In giving functional descriptions of organism-

environment interaction, particular behaviors are called 'responses' and particular changes in the environment are called 'stimuli'.

(b) A science of behavior should avoid reference to non-observable psychological entities (e.g. mental states, cognitive constructs, intentions, beliefs) in explaining behavior. Any non-observable variables which are used should be operationally defined, i.e. functions claimed for a variable must be clearly observable in the data.

2(a) The systematic nature of a science of behavior is to be found simply in the development of law-like generalizations which describe the functional relationships between environmental change and changes in behavior.

(b) The law-like generalizations are to be evaluated on the basis of the prediction and control of behavior which they allow.

(c) Behavior as response, is either genetically determined or is learned through a process of interaction between an organism and its environment. The most familiar aspect of this process is 'conditioning'. It is of two general types; the first is classical, S-R conditioning in which an established response to a stimulus is paired with an initially neutral environmental factor until that factor becomes a stimulus for the response; and the second is operant conditioning in which, for example, a particular

behavior is made more probable by certain results being made contingent upon the occurrence of the behavior (in simple but problematic terms, the probability of behavior is increased by 'rewarding' it).

3. Findings in the study of animal behavior, with certain qualifications, may be generalized to human behavior.

4. The most fruitful study of behavior will be an experimental one in which the common guidelines for research in natural sciences are followed. The criteria of fruitfulness here involve the discovery of functional relationships between variables, and the prediction and control of behavior.

5. Some form of determinism is presupposed or assumed in both the development of behavior and the world in general.

6. It follows from the above, according to some behaviorists, that psychotherapy, education and the organizing of society should not be *primarily* concerned with dialogue, understanding and co-operation but with the appropriate manipulation of environments. This presupposes that people working in these disciplines want to effect changes in *behavior*.

7(a) Consciousness and states of consciousness are not of central interest to behaviorists, though not altogether unimportant.

(b) Neither self-awareness nor the 'self' as 'free agent' need play a part in the analysis of behavior.

The general thesis of methodological behaviorism has been summed up as follows:

> There exists causal links between behavior and the environment which are amenable to systematic and scientific study. It is confusing and unhelpful to appeal to states and events of a mental kind going on inside people. This is so because, firstly, these mental events and states themselves have external, observable causes and so we might as well look directly at them, and secondly, these mental states and events are unobservable except by those who have them, and unquantifiable, which makes them unsuitable ingredients to incorporate into a science of behavior.[3]

Criticism and the Problem of Explanation

In reading through criticisms of behavioral psychology one does not find much, if any, criticism of the truth and accuracy of data. Nor does one find much criticism of methodology as a way of getting those data. Criticism, when it is not from a moral point of view, is generally aimed at:

(1) the significance of behavioral findings for important questions in psychology;

(2) the applicability in psychology of the behavioral approach to understanding;

(3) the degree to which a behavioral approach ignores aspects of consciousness either as object for explanation or as factors in the explanation of behavior;

(4) the extension of the behavioral model to the analysis of life outside the controlled environment of the laboratory.[4]

It is this non-factual or non-particular nature of criticism which warrants a philosophical analysis of behaviorism. This does not mean that in evaluation one can ignore experimental studies or detailed findings. It does mean that reference to such will only be useful here in the context of an explication of the general assumptions, organization of subject matter and logic of explanations in behaviorism.

Debates about behaviorism exist in a number of areas, for example, the philosophy of mind, ethics, social science, and learning technology in education. There are thorny issues in all these areas, most of which I hope to avoid by focusing my analysis on an explication which is relevant to commonsensical and philosophical understanding of action. It is hoped such analysis will help bring about a better understanding for all areas in which the subject is debated.

Some non-moralistic criticisms of behaviorism are interesting from the point of view of philosophical analysis because their essential claim is that, with regard to important questions of action, behaviorism, quite aside from any facts it may hold, simply does not explain. It fails to deal with 'action' properly understood. This is because, according to some, it has been limited by a strict adherence to logical positivism. A number of its basic analytic distinctions are confused, and, it fails to take account of the

purposive nature of action and the corresponding need for teleological explanation.

At this point I wish to examine some representative criticisms in order to get clear as to some of the problems which exist concerning behaviorism and explanation. In chapter six a number of points contained in these criticisms are examined in detail, but here, I raise them in a general way only to exemplify the need for analysis of *explanation* in behaviorism. Thus my exegesis is far short of a full interpretation.

Behaviorism and Logical Positivism

Sigmund Koch has put forward a view which I take as representative of those critiques of behaviorism[5] which have essentially the following form: Behaviorism began as a psychology which attempted to emulate physics by using a logical positivist methodology and 'decision procedure' or criterion for meaningfulness. Logical positivism has been shown to be inadequate and behaviorism has failed because it has continued to be based on that mistaken epistemology and logic of enquiry.

I will argue later that this criticism is limited and in fact importantly wrong. It is most adequate as a criticism of Hull's hypothetico-deductive system but wrong in its presumption that behaviorism in general is tied to a *strict* logical positivism. It is also wrong in its assumption that

the demise of the verificationist criterion of meaning, and certain other restrictive claims of logical positivism, has meant that positivism in general no longer is prescriptive and descriptive of science. However, we will return to these points later.

The history of behaviorism according to Koch can be seen as the growth and decline of a positivist orientation in psychology. Classical behaviorism (approximately 1912-1930), centered around the work of Watson, was "an attempt to escape the stagnation of the subjectivist psychologies then prevailing by providing psychology with a decision procedure."[6]

"Neo-behaviorism," Koch's terminology, refers to developments during the 1930's and 1940's which took their direction from the prescriptions of *logical* positivism.[7]

"Neo-neo behaviorism" refers to the period since 1950 in which behaviorists have become less tied to positivism and more interested in matters less amenable to objective study.

The 'decision procedure' of classical behaviorism refers essentially to a set of criteria, which propositions in behavioral science must meet if they are to be meaningful. This bears a strong resemblance to the criteria of logical behaviorism as developed in the philosophy of mind. The criteria were essentially as follows:[7a]

(1) Propositions must be objective, testable, by observation and replicable studies, producing measurable data.

(2) "All lawful statements [were] to be expressed in terms of stimulus and response."

(3) All psychological phenomena were to be characterized as 'peripheral', as functions of interaction between the environment and the most obvious, observable physiological processes "receptors, effectors, and their most direct nerve connections."

(4) Statements about learning were to be phrased in terms of conditioning.

These regulations however were inadequate as guidelines because they gave little direction for the development of theory. And,

> By the late twenties, there was much 'objective' experimentation but few bodies of clearly stated predictive principles comparable to the crowning achievement of physics: its theories . . . the search for a 'decision procedure', thus became a search for a formulary of the techniques for 'constructing' rigorous theory.[8]

While during the classical period this search aimed at insuring objectivity of empirical data, "neo-behaviorism sought to realize and implement objectivism at the level of theory."

> The idea was to ensure that all elements of a system language be 'securely anchored' by explicit linkages to antecedent independent and consequent dependent variables, and in general to effect a point-for-point correspondence of the logical properties of systematic formulations of psychology with those of . . . physics.[9]

The behaviorists, however, turned not to physics but to the logical positivists and neo-pragmatists for direction.

> Psychology in America at least, soon stabilized itself within the ambience of the positivist-neopragmatist-operationist view of science. . . . The hypothetico-deductive reconstruction of science that it put forward was open to the interpretation that properly rule-regulated scientific work was self-corrective, and thus it seemed to offer a more or less surefire instrumentality for scientific advance.[10]

And here the new regulative 'decision procedure' was concerned with, first, criteria for defining and relating intervening variables and, second, a tightening up of the requirements of a 'legitimate observation base' of psychology. Justification for the objectivist position on acceptability of data came from the 'operational criterion' and "certain other formulations of the empirical criterion of meaningfulness of the sort developed by logical positivism."[11]

Koch's criticism relative to logical positivism is often on a very high level of abstraction. This raises the question as to just how significant logical positivism is for behaviorism at the practical level of research. We will deal with this question in our examination of behaviorism. However, some of the terminology can be made more concrete in the following way:

Dependent variables were to be 'defined' by reference to observation just as in the physical sciences (e.g. a measurable index of response). *Independent variables* were to "face the test of simultaneous observability and [be]

definable in either the observation language of physical science or the concepts of physics." He points out that ambiguities concerning 'stimulus' and 'response' were never resolved. Sometimes the terms had a physicalist meaning and other times a molar-behavioral meaning, e.g. a *response* in a Skinner box is a *bar press*, not a particular movement. The *particular movements* that the animal performs in pressing the bar are most often not taken into account.

The Intervening Variable Paradigm

The new decision procedure allowed for the establishment of terms designating functional relations between dependent and independent variables provided they were linked to observables by functional, hopefully quantifiable, relations. In a hypothetico-deductive theory these relationships or functions could be asserted as postulates and tested by experiment without recourse of reference to the 'mental'. For example, 'hunger' or 'drive' to eat may be defined in terms of food deprivation time or by change in blood sugar level at the independent end and to response to food at the dependent end. Thereby we get a term which we can use much as we would 'hunger' in describing behavior; but the new term is (at least in theory) more properly scientific. It has 'objective' meaning.

Koch's positivist characterization of early behaviorism is meant as criticism but he does not say explicitly what is wrong with positivism per se. He does, though, express his concern about its effects in bringing about a lag in psychology "relative to the rest of scholarly culture." He claims that while other disciplines are progressing as science, psychology has been retarded because of its adherence to positivism.

> the emerging redefinition of knowledge is already at a phase, in its understanding of the particularities of inquiry, which renders markedly obsolete that view of science still regulative of inquiring practice in psychology.
>
> . . . the new interests in the particularities of inquiry and the interrelations of the resulting knowledge invite into existence precisely those curiosities, neglect of which has most thwarted the historically constituted aims of psychology.[12]

There are then traditional problems of psychology with which behaviorism cannot deal when tied to positivism, and there are legitimate forms of inquiry which do not accord with the forms of explanation which can be offered up in behaviorism. Koch describes very few examples of problems arising from a too strict adherence to positivism but these we shall deal with at the end of this thesis.

Charles Taylor: Behaviorism and Teleological Explanation

Charles Taylor's criticism of behaviorism and defense of teleological explanation, like the article by Koch, claims that the behavioral position misses, or ignores, important

aspects of our experienced reality. Taylor's focus is, however, much narrower and much more to the point for our purpose here. He claims that behaviorism simply fails to deal with the real defining qualities of action, its intentional and teleological characteristics. This he argues, does not accord with our ordinary language discussion of action. Nor does behaviorism's insistence on mechanistic explanation accord with a sophisticated and justifiable conception of teleology. Taylor makes the following claims:

We see intuitively that amongst all those things going on in the physical world some of the behavior of men and higher animals is unique. This behavior "exhibits an order which can not be accounted for by 'blind accident' of processes of nature." Humans and higher animals are sometimes "conscious of and direct their behavior in a way which finds no analogue in inanimate nature."

Behaviorists, according to Taylor, disagree. They take the view that "there is no difference in principle between the behavior of animate organisms and any other process in nature; that the former can be accounted for in the same way as the latter; by laws relating physical events. The introduction of such notions as 'purpose' and mind can only serve to obscure and confuse."[13] This disagreement has a bearing on certain ethical and philosophical reflections and this helps to explain its 'perennial importance'. Its lack of

resolution, however, is in part due to a lack of clarity as to "the meaning of the claim that human behavior is purposive, or, what is the same thing, on what the relevant evidence is which would decide it."[14]

With respect to behavior, teleological explanation, though empirical and causal, has some unique characteristics. Taylor tries to make this point very clear. He says:

> The events productive of order in animate beings are to be explained not in terms of other unconnected antecedent conditions, but in terms of the very order which they produce. These events are held to occur because of what results from them, or, to put it in a more traditional way, they occur 'for the sake of' the state of affairs which follows.[15]

Thus

> An explanation is teleological if the events to be explained are accounted for in this way: if G is the goal 'for the sake of which' events are said to occur, B the event to be explained, and S the state of affairs obtaining prior to B, then B is explained by the fact that S was such that it required B for G to come about.

In other words,

> A teleological explanation is defined as such by the form of the antecedent, a form in which the occurrence of the event to be explained is made contingent on the situations being such that this event would bring about the end in question.[16]

He notes a distinction between stronger and weaker forms of teleological expression. The weak form is the partial explanation given by reference to the goal of a particular action when a number of alternatives are available to attain that goal. The strong form applies where the goal is narrowed by defining it in terms of the particular route by which it was attained. It is the stronger form which he

says is most applicable to animate beings. Consider this, my example (Taylor offers none). If I reach into a basket containing an apple, an orange and a banana, then my grabbing the apple is only partly explained by my purpose of 'getting something to eat' (weaker form). However, if I have agreed not to eat more apples and oranges, then my grabbing the banana may be explained by my purpose in reaching into the basket in order to get a banana to eat (stronger form).

This mode of explanation, according to Taylor, shares important characteristics with mechanistic and causal explanation in science. First, it meets two important criteria that Taylor sees for science in general:

> It gives the antecedent conditions of the explicandum in terms of a set of factors which make evident its connection with others, which makes clear with some exactitude what would need to be changed for other outcomes to eventuate; and it also is capable of building in tiers, that is correlations which explain at one level can be taken as explicanda and explained at another.[17]

Mechanistic, causal and *empirical* claims are distinguished as follows: causal claims are those which state that antecedent events bring about effects; mechanistic claims are those which state causal connections and make no mention of goals (or purpose, defined in terms of goals) as determining the effect of a causal event. Empirical claims are those which can be tested in some standard (scientific) fashion. Teleological claims, particularly about behavior, are not mechanistic, but they are causal and they are empirical.[18] Therefore they may not be ignored on the grounds that they

are not scientific.

Taylor argues that scientific explanations exist at different levels. More basic explanations account for simpler explanations. Science should seek deeper, more basic explanation. Therefore,

> The question whether we should use a framework based on explanation by purpose involves the question whether we should allow a teleological mode of explanation. But this means surely, whether we should allow that the most basic explanation is teleological.[19]

Thus teleological explanation, at least as an explanatory form, cannot be rejected a priori in a science of behavior.

The Intentional Thesis

An important aspect of teleological explanation as applied to animate beings "is that we have to take account of the way the agent sees the situation." Explanations of human behavior, for instance, can only be teleological if we interpret 'requiring B for G' as 'requiring B for G in the view of the agent'.[20] This entails showing "how the agent saw the situation and what meaning it had for him." And this in turn involves "taking into account the conceptual forms through which agents understand and come to grips with their world."[21]

The above analysis by Taylor forms a basis on which the following criticism of behaviorism is made. Behaviorism is wrong because it rejects teleological explanation and any reference to the meaning of the situation for the agent.

The behaviorist position came about with the rejection of teleology and mentalism on positivist and empiricist grounds. Behaviorism , it is argued, began with the mind/body dichotomy and then suppressed the mind term, thus avoiding the problems of interactionism and dealing with unobservables. Taylor summarizes the *behaviorist* position on purposive explanation which arises from this as follows:

> One cannot accept intentional properties because this would involve some strange interactionism. But if one cannot accept intentional qualities, then obviously one cannot accept teleological explanation, for this can only be applied to the goals of and situation as envisaged by the agent. . . . Thus all causality is efficient. But then reciprocally, if all causality is efficient, any attempt to explain behavior by purpose can only be interpreted as the introduction of another efficient cause, viz. 'purpose'. But this is not among the efficient causes that we observe; therefore it must be unobservable; moreover, it must be operating in addition to the causes we observe so it must be in interaction with them. But this is necessarily an unverifiable hypothesis, and of no interest to science. It follows that scientific psychology cannot take account of purposes, and hence of the goals that agents seek and the ways they envisage them.

All this is "self-induced illusion of necessity," according to Taylor, for "there is no self-evidence to the proposition the mental is the unobservable."[22]

In fact one can observe another man's 'mental states' by simply observing him in these states. No reliance on uncheckable introspection is involved here. (I think Taylor's point can be sensibly taken to refer to states of the organism rather than to a non-physical entity 'mind'.) And as previously argued, according to Taylor, teleological explanation is not absurd, unverifiable or non-empirical.[23]

Taylor develops his criticisms in some detail and provides examples of where he believes a behavioral analysis is inadequate. We will deal with these points thoroughly after an examination of behaviorism. However, it is worth noting at this point that he claims; first, that the existence of the phenomena of 'insight' and 'improvisation' argue against a behavioral analysis because the *form of explanation* in that analysis makes it inapplicable to intentional, goal directed behavior; and second, that 'stimulus' and 'response' cannot be defined solely in terms of environment or physiology but must, according to Taylor, be defined intentionally.[24] These points will be considered in chapter six of this thesis.

D.W. Hamlyn: the Vacuousness of Behavioristic Explanation

D.W. Hamlyn, in his paper "Conditioning and Behavior,"[25] puts forward an example of the view that for two reasons behavioral analysis lacks explanatory value. First, behavioral analysis is only topographical and does not provide necessary and sufficient conditions for explanation; and second, the behaviorists have taken concepts with a minimum applicability, such as 'conditioning' and 'stimulus generalization' and stretched their meaning and use to cover phenomena to which they are not applicable.

What studies of conditioning,

> seem to have in common is that there takes place some process whereby a connection *appears* to be set up between two factors in the situation—between stimulus and image. I say 'appears to be set up' because strictly speaking it is merely a matter of hypothesis that there is such a connection. All that can be observed is that the response, or whatever it is, takes place and becomes more probable in the given situation. Whether this is due to an underlying connection and of what kind is another matter. That is where theory comes in.[26]

Theory then is about "underlying connection." If it is claimed that 'conditioning' just means an increase in probability of response following a series of organism/environment interaction, then, according to Hamlyn, the term has "no explanatory value." Though classical 'conditioning' of physiological responses, as first studied by Pavlov, may have a minimal legitimacy (here certain physiological mechanisms were pointed to in explaining the phenomena), it is the extension of the concept in behaviorism which he questions. That is, for Hamlyn 'conditioning' in physiology points to an underlying mechanism, aspects of the nervous system, which make the connection between changes in response and changes in the environment. In behaviorism, on the other hand, 'conditioning' refers only to correlation between these changes. The behaviorists according to Hamlyn do not point to anything paralleling the physiologists' neural mechanisms and thus to call this correlation a case of conditioning is unjustified.

The extension, he claims, ignores the following distinctions: first, between response to a *stimulus* as a clearly defined physical event in the environment and response to a *situation* "as it is seen by the subject; and second, between a movement *caused by* a *stimulus* and a movement *made by* the *subject* in *response* to, and in light of a situation.[27]

'Response" he takes to be a legitimate term. Subjects respond to a situation. They act in accordance with their perception and understanding. This action depends, then, on many factors other than the subject's history of association with that or similar situations. This will be true even in fairly simple conditioning *generalization* studies and,

> to speak of stimulus generalization is at the least misleading, since it is not the stimulus or its effect that is generalized, but rather how the subject perceives the situation in which he finds himself. This reference to perception is vital and cannot be excluded.[28]

To describe situational response as conditioning "affects drastically our conception of what mechanisms can conceivably be involved," i.e. perception and understanding drop out and we are left with a description which makes nothing intelligible.[29]

For Hamlyn, a similar problem arises with studies of operant conditioning.

> In the case of classical conditioning, if it ever occurs, the connection must be a fundamentally mechanical one, and to the extent that it seems a phenomenon cannot be interpreted in that way so also it becomes doubtful whether it can properly be

> classified as a case of classical conditioning. One's understanding of operant conditioning on the other hand, must surely be different, since the connection, if there is one, will be set up only in certain circumstances, and under conditions of a special sort. One would expect the behavior in question to become established only if the effect of it means something to the animal in question. [For example, an effect might be *desirable* to an animal.][30]

In summarizing his comments on operant conditioning, Hamlyn notes,

> To say simply that the animal's action in pressing the lever is to be explained by saying that its action is reinforced by its getting food by that means is not in itself to offer any sufficient explanation. The story needs to be filled in by further details and what is important is the direction in which we must look to provide such details. As I have indicated, to look to the possible mechanism underlying the responses is to concern oneself with necessary conditions only, not sufficient conditions. The details that one needs when concerned with sufficient conditions are such facts as that the animal sees that pressing the lever produces food, this is what it wants, and so on. If it does not see that pressing the lever produces what it wants why should it go on pressing the lever?[31]

In the last sentence it appears that Hamlyn uses the word 'see' to mean 'UNDERSTAND' in some sense. It is in part the absence of reference to an agent's understanding in bringing about his action, that he feels makes a behavioristic account less than explanatory.

The criticisms I have presented have a number of points in common to which I wish to draw attention. First, they offer criticism of a relatively homogeneous view of behaviorism. Examples from the work of Watson, Pavlov, Tolman, Hull, Hebb, Spence, Miller, Guthrie and Skinner are used to exemplify what is held to be wrong with behaviorism in general.

Second, none of the criticisms claim that behavioral psychology generates false, objectively meaningless or unverifiable data. Third, each claims that behaviorism fails to deal with problems of behavior, particularly 'actions' as we ordinarily understand them. Fourth, with respect to 'action' each tries to show in a different way that whatever 'facts' it puts forward behaviorism fails to *explain*, and not only does it fail here, but given the behaviorist's understanding or misunderstanding of what is involved in adequate explanation, it will always fail.

CHAPTER 2

THE NATURE OF EXPLANATION

The criticisms of behaviorism just surveyed all deny that behaviorism does or can answer the important questions we ordinarily have about action. What then is an explanation in behavioral analysis? What kinds of questions does it answer? What are the limits of such analysis and to what extent are the criticisms justified?

In order to get at these questions I wish to examine some problems concerning the nature of explanation in general. Here we will draw some important analytic distinctions and also outline a set of minimal criteria for adequacy of explanation.

This thesis may well be described as psychological meta-theory—it is about psychological theory, makes general claims and comments and is analytic and judgemental. However, it is not analysis from a well-developed metaphysical point of view. It is rather analysis from a commonsensical position. This approach stems from, first, my view that critical thinking in science (and psychology in particular) is not a radically different activity from that of the philosopher or scientific psychologist as laymen; and second, my

view that our judgement, acceptance and understanding of explanations arise out of a form of life in which making sense of experience is a basic skill with a very high survival value. In spite of the bad or lazy thinking, the holding of false or unfounded beliefs and the amount of arguing that goes on in the world, there is tremendous agreement in judgement which is so obvious it may be overlooked as insignificant. Thus we begin with an attempt to get clear as to conditions of adequacy which are generally met in *intuitively acceptable explanation*.

In this section I try to do the following:

(a) analyze the meaning of 'explanation' pointing out some ambiguities and showing the particular uses of the term I am interested in;

(b) define 'event-explanation' and analyze the relation of such explanations to the events they are said to explain;

(c) note necessary conditions for adequate event-explanation and give a rationale for each;

(d) discuss the above analysis in terms of the covering-law model of scientific explanation; and

(e) indicate directions for an analysis of explanation in behaviorism.

Meaning and Reference of the Term "Explanation"

The word 'explanation' has a number of reasonably distinct references all connected with the concepts of understanding and knowing. This point may seem rather obscure. This is not because the analysis is subtle or difficult, but simply because it is not often made. 'Explanation' can be problematically ambiguous and it is important to clarify its use at the beginning of our analysis.[1] The distinctions in which I am interested are drawn on the basis of ontological factors, i.e. *what the explanation is* (performance, proposition set, facts about the world) and on the basis of referential factors, i.e. according to the object of explanation or *what the explanation is about*. These categories may be outlined as follows:

First, there are at least the following categories of things which are explained (referential categories):

1. Meanings—logical relations in propositions, implications, entailments:

   (a) the significance of propositions;

   (b) the meaning of conventional symbols or of particular objects as symbols.

2. Functional Relations—*as co-relation of variables or factors:*

   (a) e.g. mathematical functions connecting one set of

values with another;

(b) mechanical functions where the interrelatedness of parts of a system is such that changes take place in one part of the system in correspondence with changes in another part.

3. *Teleological* Functional Relations—

(a) e.g. biological functions of parts of an organism as they contribute to the economy of the whole;

(b) the adaptive, or life supporting, functions of certain animal behavior patterns.

4. *Intentional* Relations—how the world appears or is understood by individuals and their reasons for action in light of that understanding.

5. *Events*—happenings, particular changes, patterns of change, things coming into being. (One way in which this last category differs from the others is with respect to the importance of time or duration in the characterization of individual members.)

Second, in discussing explanation one may point to several different *ontological* categories of the objects referred to as explanations. Here the following divisions are possible with reference to explanation of events:

1. *The set of facts* in the world which we point to and claim explain a situation. For example, quite aside from anything that is said, a car having hit a power pole

'explains' the pole being broken.

2. *A set of statements* about what is the case from which a statement about the object of explanation *follows:* for example, the statements,

(a) All power poles of this type break when hit with a force greater than 'X'.

(b) This pole was hit by a car with force greater than 'X'.

(c) There is no other source apparent.

These constitute an explanation of the situation described by the statement "The power pole broke."

3. *A fact-hypothesis set,* where the generalization or covering law is less explicit, or is probabilistic, the propositions making up an explanation might be better characterized as facts and hypothesis. For example: "Though out of control automobiles sometimes simply bounce off power poles; in this particular case, even though the car was travelling slowly, it did hit the pole. And given that the pole is broken and that this sort of thing happens often when power poles are struck, it is fair to say that this is why it is broken."

For our purposes we may collapse the ontological distinctions into just two categories, sets of facts and sets of propositions.

What this analysis indicates is that in using the term 'explanation' our focus of attention may range considerably. It should also be kept in mind that in discussing explanation we are talking about the ways in which propositions and patterns of explanation affect the way we make sense of the world. We are not talking about how aspects of the world other than factors of explanation must be related.

Consider then the following examples:

First, *explanation of meaning.*

1. My saying, "'I have car insurance' means a company has agreed to pay for damages in case of an accident."

2. A dictionary definition of 'car insurance'.

3. A translation of a word from one language to another.

4. Stating a relationship in logic or mathematics, e.g. 12 divided by 3 means 12 divided into 3 groups of equal size.

Second, *explanations of mechanical function.*

1. The statement "It connects the crankshaft to whatever the engine drives, transferring rotating force" answers the question "What does the drive shaft do?"

2. The function of gills in fish is to facilitate acquisition of oxygen.

Explanations of these sorts may be descriptive, co-variance explanations or teleological explanations, e.g.

(a) "The drive shaft turns when the motor is engaged and so does whatever is connected to it."

(b) The drive shaft turns *in order* to transfer rotating force to the wheels.

3. Often statements of physical function relations are bound up with statements of the mathematical functions which describe then. For example, variance in length of a pendulum arm is functionally related to the period of the swing. On this basis a clock out of adjustment may be corrected because the length of the pendulum 'functions' to control its period. Mathematical functions are not of course physical functions, and to describe a relationship between phenomena by a mathematical function is not to specify causally dependent and independent relationships. Adjusting your barometer will not improve the weather. The important point here is that a 'functional explanation' need not say anything about either the causality within the function described or *its* purpose. Third, *explanation of events (or event explanations).*

1. A light comes on (the event). The following set of propositions constitutes an event explanation.

(a) The light and switch were part of a properly wired circuit.

(b) The circuit was connected to a source of electricity.

(c) A switch was thrown completing a circuit through the bulb.

2. A less complete event explanation of the same event is the statement "The switch was thrown" in answer to the

question "Why did the light come on?"

The discussion of these examples is so reasonably clear that it may at this point seem trivial. It is, however, my contention that problematic equivocation arises with regard to 'explanation' particularly in philosophical discussion of the concept. For what is true of one reference or use may be importantly untrue of another. For example, temporal requirements in event explanation may not exist for some instances of function explanation and not at all for meaning explanation, the logical conditions of adequacy we look for in event explanation may not even arise with regard to some questions of meaning. For example, causal explanations require reference to antecedent events whereas explanation of division in mathematics (unless one is talking about appropriate circuiting in a computer) does not seem to require reference to events at all.

Peripheral to the meanings of 'explanation' discussed above are two other meanings which I want to point out in order to delineate the sort of explanation I am interested in at this point. They are, first, justifactory 'explanations' in which reasons are given which justify action; for example, "We hanged him because he was a bad man and deserved to die" and, second, practical 'explanations' in answer to questions about how to do something, e.g. "Could you explain how to adjust the voltage regulator?"

What all uses of the word explanation have in common is their reference to some aspect of the answer to a puzzlement. To the degree one understands a situation correctly, one's puzzlement is reduced. Of course one's puzzlement may be reduced by a misunderstanding, in which case you are under an illusion and simply wrong. However, generally, explanation helps to provide us with understanding. Though this may well be something like a necessary condition for all explanation, it is important that this not mask the differences I have indicated. An explanation of a piece of music may help you to understand it and an explanation of an oil rig explosion may help you to understand that. This does not mean that an explanation of the explosion *must* be very similar in form to the explanation of the piece of music.

Event Explanation

Description, Explication and Explanation

The sort of explanation I am most interested in is event explanation, and here, to give an event explanation of some event, aspect of an event, or series of events is to say *what brought it about.*

An explanation in this sense is to be distinguished from both an explication and a simple description as follows:

1. *A simple description* of an event notes changes and movements without mention of a relation between phenomena.

2. *Explication* (of what is happening) is the detailed description of an event and the context in which it happens or of the individual events making up a larger event.

3. *Event explanation* refers not only to the event in question (which it need not describe) but also to other events and conditions, and points out how the event to be explained results from, is caused by, is made more probable by, or more generally is determined by other events and conditions. There does not appear to be a clear-cut division between either simple description and explication or between explication and explanation. The divisions are, pragmatically speaking, rough ones. There is no doubt considerable overlap but I am not sure that this is a problem for analysis here. What I am seeking after all, is the characteristics of paradigmatically good explanation as a model or position to be used in an analytic tool for description of explanation in psychology. From this perspective a reasonably exact depiction of a model is more important than an exact description of what must take place or what does take place in everyday and scientific explanation. Some examples:

Consider the following situation. We look out a window and observe that it is raining. A *simple description* such as "It is raining" or "Drops of water are falling from the sky" might be offered in response to questions such as: "What do you see?" "What is happening?" "What are you looking at?"

The rainfall event may further be *explicated* by noting that water vapor in the air is condensing and forming clouds. The condensation of water, particularly on dust particles, continues until the drops of water thus formed drop to the earth in the process of which they reach a certain velocity. Here a great deal may be said without giving anything like a satisfactory explanation as an answer to the question "Why is this happening?" Instead one would be answering a question such as "What exactly do we *see* going on out there?" with a more detailed description.

It is in answer to a question about the rainfall such as "Why is that enormous amount of water falling in drops from the sky?" that we get an *explanation*. That explanation will look something like this:

Clouds, large masses of air saturated with water (water vapor), *are being lifted* and *cooled* by the larger air currents. The air is saturated,which is to say that at this particular temperature a given volume of air will not hold more water vapor. The saturation level for air *is proportional* to its temperature. Here saturated air has cooled slightly *causing tiny* drops of water to form on dust particles, drops still light enough to float. As the saturated air cools further, more water vapor is forced out of suspension *resulting in* a growth in the size of the drops to the point where they are too heavy to float in the air and *as a*

result of gravitational force fall to the earth.

Characteristics of Event Explanation

We may note here a number of factors in our explanation example which are not in the first description or the explication:

First, the explanation refers to the event in question as an instance of a general situation. A simple description need not do this.

Second, the explanation focuses on those aspects of the event *critical* to change. (It does not, for instance, talk about the color of the rain which a simple description might very well do.) And it categorizes those aspects of the change as instances of a general pattern of change, e.g. saturation point/temperature.

Third, the explanation notes the event in question as an instance of a general pattern of events in which rainfall is functionally related to and therefore predictable from other events, e.g. formation of water vapor, cooling of air masses.

Fourth, the explanation need only refer to the event in question and not describe it all. (Compare this to the detailed description of an *event* mentioning details and measurements immediately perceived.) All this is not to say that event explanation is not descriptive for it is a kind

of description but is not *simply* description or explication.

Some explanations of events may themselves seem to be just simple descriptions *of a critical antecedent* event. But in such cases there is still an implicit reference to a background of factors which, together with the explicit explanatory comment, constitute *the* explanation. For example, "Why did the light come on?"—explanation, "The switch was thrown." This only makes *sense* in a context of other statements that might be made; about past experience, about circuits and electrical energy. Of course, a *sensible* explanation may still be incorrect. This in turn is dependent upon how well the implicit references accord with the facts. My point here is that any explanation we take as sensible entails *some* context of conditions which we who accept the explanation accept as background.

Some Conditions of Adequacy for Paradigmatically Good Explanation

What conditions must a set of statements have if it is to exemplify good explanation? How does an explanation satisfy us or lead us to a claim of understanding? If we examine the rainfall discussion and its reasonably satisfactory explanation we may note at least the following which seem necessary and perhaps jointly sufficient for good explanation.

1. The set of statements meaningfully functions as an answer to a "Why is X happening?" type of question. That is, right, wrong, correct or incorrect, it at least makes sense. This criterion will, as I hope to show later, always be somewhat dependent upon the context in which the question is put.

2. It must make reference to factors outside those in the *event* to be explained. Only by so doing can it meet our presupposition that all change is an aspect of functional relations between objects. Three points should be emphasized here:

(a) *Outside* factors are factors *outside the event*, not necessarily outside a physical object acting in some way.

(b) At this point 'object' need not be restricted to 'physical' object.

(c) This requirement for explanation in no way invalidates the simple description of phenomena which we cannot yet explain.

3. The set of statements must focus not only upon functionally related factors, but must specify the particular *critical* factors in the situation under discussion. How far back in the causal chain we go may turn out to be simply a practical matter. For example, in the rainfall case, the wetness of the ground and darkening of the sky are certainly

related to precipitation, yet neither factor would figure in an explanation answering the question "Why are drops of water falling from the sky?" Saturation and cooling, on the other hand, are part of the determining conditions that bring about precipitation and must be specified. Here, the causal chain will extend to cover all climatic and meteorological conditions. (In turn, another general question arises which I will not attempt to answer at this point, i.e. what are the criteria for specifying 'critical factors'?)

4. The explanation statements must be either clearly true factual claims or highly probable hypotheses based upon overwhelming evidence.

5. The statements of connection between explanation set and statements describing the event to be explained must be such that an entailment relationship is shown to exist between the explanans and explanandum or between the explanans and a claim to the very high probability of the explanandum.

Briefly, then, an event explanation is a good explanation when:

(a) it is relevant to the question under discussion;

(b) it contains true factual claims and/or a highly probably hypothesis;

(c) it has no invalid reasoning;

(d) it points out critical determining factors of

change outside the explanandum event;

(e) it contains a set of statements which entails a statement of the event to be explained or can be readily combined with implicit knowledge claims to yield such entailment.

Context Dependency and Explanation

Questions and Context

I have outlined above important requirements for event explanations. Each requirement, particularly (b)-(e), has been explored in the philosophy of science.[2] However, it is upon the first requirement, that an explanation must be relevant to a particular question, that I wish to expand. That an explanation is relevant, or to what it is relevant, is not always obvious. I will argue later that there are important problems regarding this point in the debate about behaviorism. But let us try to get a better idea of what is involved here by discussion of some trivial examples.

In the following discussion I have tried to make clear first, that both questions and explanations are context dependent in such a manner that explanation must be judged to be relevant or irrelevant in three separate but related categories; and second, the particular interpretation of a question which an explanation is supposed to answer must be made very explicit in order to fully appreciate that

explanation.

Consider then the general question *"Why did THIS happen?"* and its possible interpretations, when a can of Coke is dispensed from a machine.

1. Where I come from, Coke machines always dispense bottles, "Why did a *can* come out of this one?"

2. I am totally unfamiliar with dispensing machines and wonder "Why is a can of Coke coming out of *a machine*?"

3. I am being shown a Coke machine for the first time (a demonstration) and wonder why (how) a particular input, the coins, trigger the output, i.e. "Why did *putting* the coins in cause a can to be dispensed?"

The question may also refer to a general situation of which this is but one instance. Here we will also consider some possible answers. We note an input-output contingency and the general question is put "Why does this input, two quarters, yield this output, a can of Coke?"

4. I though it would need three quarters. Why only two? There are radically different possible explanations. For example: "The machine is malfunctioning." "The management of the building is subsidizing the cost." "Someone secretly opened up the machine and adjusted the coin sensor."

5. The sign says "7-Up." Why do cans of Coke keep coming out? Possible explanation—"The attendant made a mistake and filled the 7-Up machine with Coke cans." "This is

really a Coke machine only someone has switched the signs around." A question then must be set in some particular context and this context is most closely identified with the overall puzzlement of the person asking the question.

My question about the Coke machine will only be relevant as a question about how that event fits into a *particular* puzzlement that I have. Here a mechanical explanation will be useless if my question is about marketing. An explanation of the *particular* event may not be very informative if my puzzlement is about the *general* pattern of events (Do machines dispense Coke *because* people put money in them? That is why this one did.) Notice that there is nothing in the bald question "Why did this machine dispense a Coke can?" which indicates what *particular* interpretation is intended. This must always come from the context of the question. If this is true of my trivial examples here, surely it will at least sometimes be true in more important and more complex situations, and yet the matter seems to have been given no attention by the criticisms of Koch and Hamlyn and almost none by Charles Taylor.

A further distinction can be developed here on the basis of an example closer to problems about behavior. This is the distinction I wish to make with respect to relevancy of explanation, viz. between 'adequacy' and 'target relevancy'. It would seem that an explanation may

lack relevancy because, though it is still *about* the specific event noted in a question, it is only a *partial explanation;* it may not for instance deal with the causal factor at the right point in a causal chain or at the right level of generalization. This I call a problem of adequacy. However, it may be lacking in relevancy in quite a different way; this is when the explanation is *about a different aspect* of the event than that specified in the question. It is this that I call a problem of target relevancy. Some examples may help to make the distinction clearer:

Consider the following situation: A dog runs into a room, goes by a bowl of meat scraps on the floor and eats a half a head of lettuce from a plate on the coffee table. The question is then asked:

1. Why did the dog eat the lettuce from the plate on the table?

Again consider different interpretations possible here with a simple change of emphasis:

2. Why did the *dog* eat the lettuce from the table?

3. Why did the dog eat the *lettuce* from the table?

4. Why did the dog *eat* the lettuce from the table?

5. Why did the dog eat from the plate on *the table*?

Notice that "because he was hungry" might be an acceptable offhand reply to #1 and part of an answer to questions #2 and #4; but "because he was hungry" will be totally off

the mark given a specific interpretation of #3 and #5.

If, for instance, I thought all dogs were carnivores, then the dog's being hungry does not seem to bear upon the question rephrased as "Why did he eat the lettuce and not the meat?" Or if the dog has been trained *not* to eat off a table but from a bowl on the floor, then again his being hungry will not be part of an answer to the question rephrased as "Why did the dog eat the lettuce *from the plate on the table* rather than the meat from his bowl?"

More clearly yet, if we see the dog grab the lettuce we may ask "Why didn't he eat the meat?" and here to say "Because he was hungry" as even *part* of an answer will be wrong.

The insufficiency of an explanation in reference to the dog's being hungry in these cases is not like a lack of depth or generality in which an explanation is partial and sensible but incompletely answers the question; for example, if "His knee was sore because he ran too much" is given as an explanation we may note that "too much" means running until his knee was sore. This won't do if we want to know why his knee became sore *when he ran*. But it is not off the target in the same way that "The dog was hungry" is off the target if we want to know why he didn't eat the meat and ate the lettuce instead. The claim about his running too much is partially correct. It just doesn't go far enough.

The claim about the dog's being hungry, however, does not seem to address the question at all. These examples indicate the two sorts of relevancy I have in mind. The first (the dog example) I refer to as 'target relevancy' and the second (the sore knee example) has 'adequacy'.

In summary then, we may rewrite our first criterion as follows: A good explanation must be adequate to the specific question under discussion and it must be on target relative to that question. These criteria arise because questions calling for explanation are generally context dependent. They are set in a context of information and puzzlement. Here, increasing the specificity of a question involves increasing the specificity of description of the event or aspect of the event that the question is about. Relevant explanation must be addressed to that specificity of description.

Explanation and Context of Information

Many explanations may bear on an event loosely described; which explanation is appropriate will sometimes depend on context in a way which is better characterized as a 'context of information' rather than of 'question'. Consider, for example, the problem of explaining a falling barometer: by a science teacher to a young child; by a layman to another intelligent adult who is completely unfamiliar with the

instrument; by an instrument technician to a mechanic who is familiar with compression.

Here, no specific question may have been asked. One simply is trying to get another person to 'understand' what is happening. What one says here in the way of explanation will be dependent on the background of information in which you believe your explanation will make sense, and it would seem this sort of dependency will exist at any level of sophistication.

Just as there are general questions (about an event given a general description), and specific questions about specific aspects of an event, so there are general explanations whose parts will be specific explanations. Consider the detective case in which three different people have different but incomplete pictures of how or why some crime took place. It is only when the detective puts all three together that the solution is found. *The* explanation for each observer will be different; it will be that aspect of the whole situation that fits into the information he already has.

The Purpose of Explanation

Which part of a larger explanation is *the* explanation may also be dependent on what the person asking for an explanation is up to; for example, a person baking bread may

want to know why the bread he makes is consistently too moist even though he has tried to follow the recipe closely. If we assume that moistness of bread is a function of four variables—proportion of liquid in mix, size of bread pan, baking time, and oven temperature—then the following 'causal' explanations arise:

(1) Given temperature, baking time and pan size, there must have been *too much liquid added* to the mix.

(2) Given initial liquid proportions, baking time, and pan size, the *oven was set too low.*

(3) Given temperature, baking time, and initial liquid proportion, the bread was put in *pans* which *were too large.*

Now if we experiment we may find that control on the basis of #3 gives us the best results. However, what is most important here is that the original question "Why is the bread too moist?" is for practical reasons couched in the form of asking what is the sufficient condition for an event, given a particular set of necessary conditions; here the sufficient condition will be determined by just what we take as a background of necessary conditions.

J.L. Mackie has made a similar point in his analysis of the condition relationships in causal explanation. According to Mackie *the* cause in a situation is sometimes an "INUS' condition. A factor or event selected as *the* cause will be

an INUS condition when by itself it is insufficient to bring about the effect but is a necessary part of a *set* of conditions which is *un*necessary but sufficient to bring about that effect. This set in turn is part of a disjunctive set of sufficient sets. The disjunctive set describes the necessary condition for the effect.[3]

This analysis arises in part because events and their causes are always found in a 'causal field' (a notion he takes from John Anderson).[4] 'Causal field' refers to the event in question and the total background of connected features. The description of an event and particular connected features, for the purpose of explanation, has flexible and somewhat arbitrary boundaries. Here an event described in one way will have a quite different causal explanation than if described or set off from its background in another way. Consider for example my discussion of the dispensing machine.

Mackie's analysis of cause as INUS condition is one interesting way of looking at the problem. However, if he is correct regarding the role of causal fields, then to analyze a cause as an INUS condition is compatible with claiming, as I do, that such a cause is a sufficient condition against a background of necessary conditions. This is made clear by Mackie's statement in reference to his example of a house which has caught fire due to a short circuit:

> If we take all its features, or even all its relatively permanent ones, as constituting the field, then some of the things that we have treated as conditions—for example the presence of inflammable material near the place where the short circuit occurred—would have to be regarded as part of the field, and we could not then take them also as conditions which in relation to this field, as additions to it or intrusions into it, are necessary or sufficient for something else. We must therefore take the house in so far as it constitutes the causal field, as determined only in a fairly general way, by only some of its relatively permanent features, and we shall then be free to treat its other features as conditions which do not constitute the field, and are not parts of it, but which may occur within it or be added to it. It is in general an arbitrary matter whether a particular feature is regarded as a condition (that is a possible causal factor) or as part of the field, but it cannot be treated in both ways at once.[5]

Thus if we begin with his analysis of the short circuit (in the case of the house fire), as an INUS condition in the *set* composed of short circuit, inflammable material and no sprinkler system, we see that it is an arbitrary matter that the inflammable material and no sprinkler system conditions should be taken out of the causal field. If left there, they are part of a background of necessary (and other) conditions in which the electrical short was sufficient to start the fire.

Clearly there are more detailed ways of looking at causal thinking, but Mackie's analysis is compatible with that suggested here, while it also indicates the complexity, for causal explanation in particular, of context dependency.

To return then to our example, the question "Why is the bread too moist?" is for practical reasons couched in the form of asking what is the sufficient condition for this effect, given a background which includes the set of neces-

sary conditions. The sufficient condition will be determined by just what we take as a background. It is against the background that the cause *is* sufficient.

This leads us back to problems of psychology and action. For though we may entertain many interesting questions about action, psychology is in many ways a practical endeavor; which is to say explanations in behavioral psychology, just as with our baking, will be difficult to appreciate, except in the context of the particular questions asked and the particular background of information held to be relevant to those questions. And just as the kind of results he wants and the kind of experiments he can perform will determine what in the end the baker focuses attention on in solving his problem, so also will these things be important to psychology.

Context Analysis of Explanation and Limitations of the Covering-Law Model

My fifth criterion for event explanation states, "an explanation must contain true statements which *entail* a statement of the event or aspect of the event to be explained." This covers two types of entailment: first, where a singular statement or combination of singular statements is said to explain an event (e.g. "The wiring and bulb were intact" and "I threw the switch" explain why the light came on). Second: where singular statements plus a general law

or law-like generalization together are said to explain an event (e.g. "Whenever metal is heated it expands" and "This metal was heated" explain why the metal expanded in a particular case).

It is in the same area of concern that we find the 'covering-law' model of explanation. Here attempts have been made to establish formal, logical requirements for explanation in science. Strict criteria have been suggested which *limit* the entailment relationship to one of subsumption under a general law, either deductive or probabilistic. This restriction plus the truth of the constituent propositions together are sometimes said to constitute necessary and sufficient conditions for scientific explanation.[6]

It has been shown that this model does not describe all that is called scientific explanation and that taken as prescription leads to absurdity, it cannot be taken as specifying a sufficient condition for explanation. Analysis of this problem points in the direction of commonsensical criteria which I have put forward which deal with empirical concerns and allow a much broader notion of entailment.

Peter Achinstein, in considering a number of deductive models, has noted that:

> To avoid the kind of problem in question we can say that it is an empirical, not an a priori, question whether an explanans describing events and containing laws relating these types of events to the explanandum-event correctly explains the latter.[7]

This leads him to consider a number of attempts at constructing empirical models the best of which come close to describing what goes on in scientific explanation but by so doing fail to analyze fully the relationship between explanans and explicandum. For instance, Brody's Causal Model, according to Achinstein, is made up of the basic D-N model (deductive-nomological type of covering-law model) together with the causal condition that the explanans "contains essentially a description of the event which is the cause of the event described in the explanandum."[8] But the nature of the 'causal' relationship remains unanalyzed.

However, let us consider an example of the kind of problem that arises with the strict covering-law model and some of the ways in which the analysis I have suggested may help to elucidate the problem.

It has been pointed out that the fact that certain information may be deduced from a given law is no guarantee that the law explains that information.[9] Here it is claimed that while appeal to a law ($T = 2\pi\sqrt{\ell/g}$) is legitimate in explaining the difference in period between two pendulums, such appeal will be inappropriate in explaining the difference in *length* of the pendulums. *This* appears to require reference to what brought the two different length pendulums into existence. A calculation of the lengths of the pendulums from their periods and the above law will not be a part

of the explanation. "To explain why the lengths are different, we must appeal to the purposes etc. of their construction, and perhaps the intent of the builder(s)."[10]

Appeal to the general law $T = 2\pi\sqrt{\ell/g}$ will not be part of an explanation of the second question. However, a number of points can be raised here relative to my analysis.

First, with regard to specificity of questions, the question about the *difference in period* is set in a particular context where one knows what chain of events brought about the situation but one is not familiar with the general law relating length to period. It is still a question about a particular situation. Supposing the situation was different, e.g. a physicist familiar with this law may note his two sons playing pianos each to the beat of a different metronome set at different 'periods'. He may note the situation and want an explanation "Why are these metronomes beating at different rates?" And here the event which under certain descriptions, is very similar to the above, will require a very different explanation. For example, "The music teacher set them that way when he saw that I was tired."

Here to appeal to the general law relating length to period, and noting the different settings on the metronomes would be off-target as an explanation.

Second, given a certain description of the events leading up to construction of the pendulums we may have a set of statements which though they do not appeal to a general law *do entail* a statement that the pendulums would be different lengths. For example: "The directions for this demonstration specified two different lengths of pendulum." "We followed the directions exactly."

Third, not every explanation applicable here will be an event-explanation. Consider the following questions:

1. What was the chain of events leading up to these pendulums swinging at two different rates? (An event explanation is required.)

2. What is it about these two pendulums that determines that the periods are always different? (A functional explanation is required.)

3. For what reasons are these pendulums different lengths? Here a practical or justifactory explanation is in order, e.g. the apparatus will best exemplify the law we are studying.

4. How did they get to be two different lengths? (Event-explanation required.)

Fourth, another empirical and practical matter here is relevant to my criterion that an event-explanation must point out factors critical to the change of which the event to be explained is the outcome. While the law $T = f(\ell)$ may

be restated as $\ell = F(t)$, this is simply to restate the mathematical relationship. Empirically the period is not an independent variable relative to length of pendulum. It is not a factor whose change *results* in a change in pendulum length. Thus it is not a critical factor in *bringing about* pendulum length.

This accords with Achinstein's account of the inadequacy of the covering-law models. Here he states:

> In the explanans in each of these models some factors are cited, together with a law-like sentence relating these factors to the type of event to be explained. But given that the factors were present and that the law-like sentence is true, there is no *a priori* guarantee that the event in question occurred because of those factors. Whether it did is an empirical question whose answer, even the truth of the law-like sentence does not completely determine.[11]

From this point of view then we may say the following about the covering-law model:

It does not provide sufficient conditions for adequate explanation. For an explanation of an event to be judged as adequate it must be shown to be addressed to a specific question about that event and here if a functional covering law is not at issue then reference to such law cannot be the major premise of the explanation. And, also, meeting the covering-law criteria does not insure that a factor critical to bringing about change has been specified, e.g. an independent variable.

For practical reasons then, event explanations generally select certain antecedent or simultaneous conditions as

explanatory. The list of necessary conditions for the occurrence of any event appears endless. For practical reasons explanations come to an end and then we simply describe what the world is like. Therefore, as we examine the concern of various behaviorists and their precursors we must try to discern just what sort of question was being asked, just what sort of explanation was being given, and, also, just how the questions and explanations have changed. These things must be dealt with if a legitimate judgement is to be made about the explanatory adequacy in behaviorism.

CHAPTER 3

THE BACKGROUND TO BEHAVIORISM: DESCARTES TO FUNCTIONALISM

"Action" in its most central sense means approximately "behavior of an agent which is done *in order* to achieve some *end*, some effect on the environment or some state of the organism." In discussing the language of action description, Anthony Kenny has noted the paradigm importance of end relatedness.

> Just as performances are describable in the form "bringing it about that p ..." so many activities are describable as "attempting to bring it about that p ...". So, listening is attempting to hear, searching is attempting to find, treating is attempting to cure. All these activity verbs have imperatives, and all such activities can have purposes. An activity-verb such as "enjoy," which cannot be rendered as "bringing it about that ..." has no imperative either. It is thus clear that the form of description "A is bringing it about that p" is the fundamental one for the description of voluntary human action.[1]

This is not unproblematic. Other meanings are often peripheral to the above, synonymous with 'movement', 'behavior' and 'effect'; for example: the action of the planets; the action of a chemical agent; the action of an epileptic during a seizure.

In discussing the background to contemporary behaviorism I have tried to show that action, meaning behavior said to be engaged in in order to achieve certain ends, has been a

central area of concern for behaviorists and their empiricist predecessors. (This, as I hope to show later, is also true to some extent of those who have restricted their study to animal behavior.) The two major points which I have dealt with here are first, the significance of the concept of action for the empirical study of behavior and, second, the changing nature of questions and explanations in that study.

The development of behaviorism from associationism and the physiological study of movement, through comparative psychology and early stimulus—response psychology to contemporary behaviorism with its emphasis on operant conditioning can fruitfully be regarded as the development of appropriate criteria for the explanation of action. Here of course action is to be understood as distinct from 'reflex movement', or behavior in general.

While development has been shaped by the empiricist demand for objectivity in description and parsimony of assumption in explanation, it is important to see that parsimony and objectivity do not rule out a concern for action as we ordinarily understand it. Careful consideration of this point makes less likely the outright rejection of behavior on the grounds that the behaviorist simply doesn't understand 'action' in the ordinary sense of the term. This is not to say that behaviorists agree with the correctness of our ordinary understanding of action. However, I hope to

show that the class of objects bracketed by, and characterized by, our commonsense intentional explanation, and some of the peculiar characteristics of that class have in fact been of central concern to the behaviorists.

The intellectual or conceptual beginnings of behaviorism as psychology can be traced at least as far back in time as Descartes who argued that there are two classes of behavior, voluntary and involuntary. Descartes claimed that voluntary behavior was controlled by the mind, while involuntary behavior was essentially mechanistic reaction to changes in the environment. In Descartes' view, as only humans have souls and appropriately functioning minds, only humans engage in voluntary behavior.[2] Now what is important to this view is that, in spite of being a basis for rationalist psychology that followed, it legitimized the giving of a non-mentalistic explanation of *animal* behavior and at least *some* human behavior. According to Howard Rachlin:

> The effect of Descartes' dualism was to divide up the study of behavior; involuntary behavior came to be studied by physiologists specializing in the study of the body; voluntary behavior remained in the realm of philosophers.[3]

Psychology, viewed as that branch of philosophy concerned with mind, was limited to thoughts, feelings, mental states and their relation to actions or voluntary behavior. On the grounds of the inaccessibility of other minds, its methodology was limited to introspection.

The British empiricists—Locke, Berkeley, and Hume—though obviously not followers of Descartes in their views on epistemology did, according to Rachlin, accept the mind/body, voluntary/involuntary distinctions. However dubious this may be, their focus of concern was certainly empiricist. Having arrived at the conclusion that mind, its content and processes, were determined by experience, they looked for an understanding of this determination.

> What the empiricists needed to find was some sort of 'mental glue' to hold together all of the sensations capable of being experienced. . . . This principle was the principle of association by contiguity: If sensations occur together often enough, one alone can cause the memory of the rest.[4]

I find little evidence that early empiricist philosophers were concerned to give an account of action except in so far as an explanation of the development of ideas upon which actions were based would help in understanding those actions. They were concerned with sensation, perception and the acquisition of knowledge. What is important here is their attempt to use a simple theoretical assumption, i.e. associationism, to show how social, environmental interaction could effect psychological change. In other words, they were beginning to ask questions not just about the meaning, purpose and function of psychological factors but also questions about their development as a function of environmental interaction.[5]

Early Studies in Physiology

In the 17th and 18th centuries, physiologists made advances in explaining some behavior (involuntary behavior and the bodily movements of action), through functional and mechanistic description of movements in response to stimuli and through the detailed examination of bodies. Of particular importance here were, (a) the tracing out of the nervous system, (b) experiments showing that organs could show signs of 'life' or continue to function in a dead body (or when removed from the body), and (c) the response of muscle tissue (contraction) to simple mechanical stimulation.[6]

These advances in knowledge of the physiology of movement, which may seem rather uninteresting today, argued for the eventual success of an overall mechanistic account of behavior. This in turn argued for the extension of a mechanistic account which would make superfluous the explanation of some bodily movement as 'action' meaning 'the purposive act of some agent'.

Two methodological aspects of the early work in physiology were important in its success and would continue to be a part of physiological studies and later S-R behaviorism. First, the subject matter was *limited* to physical bodies and their movements in response to physical stimuli. Second, the method of study was objective in the sense involving

repeatable experiments, physical manipulations, clearly observable findings, and physical measurements.

Physiology, as a result of restricting interest to mechanisms of the body, needed only to ask questions about how that mechanism functions and it looked for answers in terms of a background understanding of how purely physical organisms and mechanisms worked. Here the question "Why did he move his hand from the stove?" would become "What set of facts explains a sensing of heat leading to a set of muscular contractions?"

In the 19th century physiological studies continued to increase our understanding of bodily functions, including movements, e.g. detailed information was accumulated about nervous conduction, muscular movements and patterns of *movements as responses* to stimuli. These movements were called reflexes. However, two areas of behavior seemed outside the range of physiology at that time:

> (1) Behavior seeming to arise from within the person himself, such as (a) an apparently voluntary raising of the arm for which no cause or stimulus can be found in the environment, (b) complex behavior . . . for which no correspondingly complex environmental stimulus can be found.
> (2) *Learning.* Most reflexes seem to be permanently fixed in the body. How can they explain learning to sing a song for instance, or learning a whole repertoire of songs?[7]

While a simple reflex could be understood as a response to a clearly observable stimulus (e.g. a knee jerk reflex) involving transmission of energy along neural circuits, it was not clear to early physiologists how a physiological

account of an integrated and complex behavior was to be given. (It is one thing to explain physiologically the withdrawal of a hand from a flame and quite another to explain, physiologically, phoning the fire department in response to the same flame.)

The 19th century physiologist, Sechenov, was one of the first people to attempt to show how complex behavior might be understood as complex reflex behavior.[8,9] Sechenov attempted to show how complex, apparently voluntary acts can, in a broad frame of reference, be understood to be essentially involuntary. He did this by addressing the questions "How might a physiological system function?" or "What features might it have which would facilitate a high energy response following a very low energy stimulus?" He hypothesized that "inhibitory mechanisms" existed in organisms. These held energy in check, like a trigger catch in a cocked gun or a valve in a pressurized water system. Stimuli which elicited response were actually releasing-stimuli analogous to trigger pressure or valve turning. He then conducted research on humans and animals which to some degree confirmed his hypothesis. Essentially he showed that reflex time could be modified. In the case of the frog, reaction time could be reduced by removing areas of the brain (perhaps thus removing the inhibitory mechanism) or it could be increased by putting salt on areas of the brain (stimulating the

mechanism).[10]

> In a series of experiments with humans, Sechenov showed that people's reflexes work more slowly when they are tickled than when they are not tickled.[11]

Thus it is argued, tickling increases the inhibition of reflexes.[11]

Stimuli then, of a very low energy level, could have two general effects: First, to release energy for activation of large motor reactions, and second, to give rise to the brain responses we call thoughts.

In reading Sechenov, a number of points stand out. First, the general question he asks about action is always "What *physiological facts and hypothesis* will explain this action *described as bodily movement*? That is, he argues for a reflex-physiological account of all action. His detailed physical descriptions are organized in terms of the argument he is making for the extension of a reflex-physiological account to all human behavior;

> All the endless diversity of the external manifestations of the activity of the brain can be finally regarded as one phenomenon . . . that of muscular movement. Be it is a child at the sight of toys, or Garabaldi smiling when he is persecuted for his excessive love, or Newton enunciating universal laws and writing them on paper . . . everywhere the final manifestation is muscular movement.[12]

Second, while his detailed experimental work appears to be concerned with anatomy and involuntary responses he offers only argument for the extension of his *theory* that there exists "A special mechanism which inhibits the activ-

ity of the reflex apparatus" to voluntary behavior.

> Let us then a priori accept that in the brain of man there exists mechanisms which inhibit muscular movements. . . . The fact is that inhibition does exist in all cases, but we must learn to inhibit movements just as we learn to perform these movements.[13]

Third, his analysis as I have examined so far is *not* addressed to, nor does it seem to bear upon, the problem of learning except to *say* that in childhood we learn to use groups of muscles and to inhibit different aspects of generalized reflexes.

Fourth, he was concerned to deny the correctness of some mentalistic explanations of action which appealed to thoughts of an agent as causes.

> It is generally accepted that if one act follows another the two acts stand in causal relationship (post hoc-ergo propter hoc): This is why thought is generally believed to be the cause of behavior; and when the external sensory stimulus remains unnoticed,—which happens quite frequently,—thought is even accepted as the initial cause of behavior. Add to this the extremely subjective character of thought, and you will understand how firmly man must believe in the voice of self-consciousness, when it tells him such things. In reality, however, this voice tells him the greatest of falsehoods; the initial cause of all behavior always lies, not in thought, but in external sensory stimulation, without which no thought is possible.[14]

But his physiological account of behavior in general, especially complex learned behavior, was almost entirely speculative. To Sechenov it made sense. It was a logical extension of what he had shown to be the case with simple reflexes but it failed to account for the learning of responses, adaptive changes, or behavior in general which developed as a function of experience.[15]

Pavlov, Physiology, Behavior, and Learning

The most important early physiologist to come up with an empirically researched answer to the question "What physiological factors (aside from maturation) explain the development, acquisition, or learning of responses?" was I. P. Pavlov who demonstrated that reflexes or responses to stimuli could be *acquired* by a process he called conditioning (classical conditioning). He went on to detail that process and aspects of the nervous system central to it.

One must be careful not to construe Pavlov as a behaviorist. He was a physiologist interested in the activity of the brain and nervous system.[16] In order to understand this activity better, he experimented with changing patterns of stimulus and response. This meant observing changes in an organism in relation to controlled changes in its environment. We will discuss Pavlovian conditioning further in chapter four. However, at this point we may note that while Pavlov's work is quite separate from the developments in American psychology following Darwin's theory of evolution (Functionalism and comparative Psychology), they have an important feature in common. Both exemplify a *shift* away from explanation in terms of a structural account of the organism to explanation in terms of a functional account of the organism in its environment. Though Pavlov's position is far less radical here, both were especially interested in

what was seen as *adaptive* processes, physiological processes or patterns of behavior which were functional for the whole organism, in particular, processes of learning.

Structuralist theories in physiology and psychology which had emphasized description and analysis of component features, or 'ideas' in the case of empiricist associationism, were inadequate in explaining what is happening when organisms 'act', i.e. when behavior is goal-directed and achieves something for the organism. Functional explanation in relation to the organism's survival in the environment seemed to go some way toward dealing with this problem.

Now functional explanation, whether in biology or psychology, is not necessarily opposed to a structural account of behavior. But from a functional process oriented point of view, a structural account will always be inadequate because its object is molecular while the organism as a whole responding system exhibits properties and patterns of behavior which are a function, not of the rules for combination of component movements, but rather of the needs of the organism as a whole and in a particular environment.[17]

Consider the dispensing machine example discussed earlier. The question "Why is a can of Coke coming out of this machine?" if taken as a question about methods of marketing, will not be answered by a structural description of the machine, nor is the *particular* S-R relationship at issue.

It is rather the existence of the situation in general that is of concern.

Functionalism and Comparative Psychology

While Pavlov was interested mainly in physiology, what is important here is his concern to describe changing patterns of response as patterns of organism-environment interaction and not as internal processes. In the case of comparative psychology and functionalism, it is even clearer that new questions were being given central concern.

A new basis from which to approach the problem of just why we do the things we do came with Darwinian theory which made a convincing case for the biological continuity of species including humans. From this it followed that the study of animal behavior in the laboratory might lead to knowledge of behavior in general including human behavior. The possibility that there were mental processes in animals similar to those in humans became more viable; thus, inferentially, we might learn something about human mental processes through animal studies. Also, an acceptance developed of the idea of adaptation (both physiological and behavioral) through a process of environmental interaction which today would be classified as a feedback process. Darwin used the term 'natural selection' to refer to ~~a~~ this process ~~involving whole species in which~~, It was argued, that for whole species over long periods of time

differential survival provides the feedback to a form of life with minimal inheritable variations and random mutations. A similar concept was also central to homeostatic models of mechanical, physiological or psychological functioning. Many life processes can be described as homeostatic systems or negative feedback systems aimed at achieving and maintaining some goal or state of the organism, e.g. body temperature, fluid levels, and blood salinity in physiology; eating as controlled by hunger and satiation; and approach-avoidance behavior controlled by desire and anxiety in psychology. From this perspective mental processes were adaptations to the environment and it was the psychologists' task to show the details of these processes, their present function and their etiology.[18]

Though early comparative psychologists were concerned with mental processes in animals, they obviously could not get anywhere by asking their subjects for introspective reports. In other words, they were forced to attend to behavior patterns and changes in those patterns relative to changes in the environment if their study was to be objective.

> The important point for discussion is not whether there really is such a thing as consciousness, but that when attention was no longer focused on consciousness, introspection lost its position as the prime method of psychological investigation.[19]

In discussing the rejection of structuralism, we may ask in just what way, if any, was the functionalist school

at odds with other schools of psychology? The brief examination I have given so far does not show the functionalist school denying either the particular findings of physiological research or even that of rationalist or mentalistic psychology. At some point it may have done so, but what is significant for my purposes here is that the comparative psychologists, e.g. Lloyd Morgan, Thorndike, and Watson, were *not asking* what facts about mental events explain behavior nor even, primarily, what facts about physiology explain behavior. They were asking what facts about an organism's *interaction with the environment* explain behavior. (This question applied to both whole species over geological time periods and individual members of a species over relatively short periods of time.) They were concerned to give an account of change, development and learning of behavior by facts bound up with the notions of adaptation, feedback and organism-environment interaction.

CHAPTER 4

TWENTIETH CENTURY BEHAVIORISM AND THE PROBLEM OF ACTION

The shift in concern which came with the advent of behaviorism was a major step in the development of psychology as science, and though it has been said that behaviorism was an attempt to ape turn of the century physics, it must be remembered that it arose from a developing science of animal life. This included the physiological studies of Sechnov, Pavlov and Sherington and the animal behavior studies of the comparative psychologists following Darwin. I have suggested that these studies address different questions from those found in introspective or speculative psychology[1] and that they were, in some respects, more successful studies because those different questions could be more easily and clearly answered.

These new approaches bore upon the understanding of intentional action, or voluntary behavior, in a way quite different from introspectionist psychology. They allowed for a shift in point of view towards action and a new characterization of action as an object for study. This can be seen as part of a development dating from at least the 17th century. With Descartes came the division of psychology

into mental life and physical mechanism. Here, physical movements were said to be brought about by *volitions*, often connected with thoughts, feelings, and emotions. Following Darwin a further area of study in which action might be found, gained emphasis. This was behavior as adaptation to the environment. This, along with the development of the concept of the reflex, allowed for the study of action relationally characterized as a type of organism-environment interaction. Thus the stage was set for a new objective study of behavior, including action.

The shift in concern here can be put in the following way: It had never been very clear just how actions as physical movements *follow causally* from mental states, through volitions. Now an important possibility, though perhaps not a new idea, was contained in comparative psychology and reflex physiology. This was the possibility that intentional actions, as a class of physical movements, could be shown to follow causally from specifiable physical events. Thus action might be objectively explained while avoiding the whole question of volition.

I have said that action is, in the broadest sense in which we are interested here, goal-directed behavior, behavior *aimed* at achieving some effect. In this chapter I hope to explicate the continuing development of an objective stance towards action, as a central theme in 20th century

behaviorism. This does not mean that I see behaviorism as a psychology of action where 'action' means a unique form of activity requiring for its explanation attention to its intentional nature.

Behaviorism is a limited endeavor and if anything its proponents have argued against the need and validity of such a psychology. They have not only avoided the question of volition but have also tried to ignore questions of intentionality. However, they have still had to deal with the intentional nature of action and this has helped to shape the development of behaviorism, to define its problems and its limits. In so far as behaviorists have denied the intentional nature of action they have been wrong, but they have not always denied it. At these times and at others, when they have struggled to ignore that intentional reality and get on with their objective account of behavior, including action, their research has been very fruitful. It is to explication of these matters that we now turn.

The Founding of Behaviorism

J. B. Watson has set the date for the advent of behaviorism as 1912. Watson, who worked in the functionalist school, put forward the view that claims about observation of overt behavior were consistently verifiable while 'observations' of states of consciousness in animals were generally

unverifiable and generally disagreed upon. This was a problem for the methodology of psychology. Watson's answer was to restrict psychological research to overt behavior while maintaining a functional view of overall behavior as exemplified in the following remarks:

> Any other hypothesis than that which admits the independent value of behavior material, regardless of any bearing such material may have upon consciousness will inevitably force us to the absurd position of attempting to construct the conscious content of the animal whose behavior we have been studying.[2]

> The psychology which I should attempt to build up would take as a starting point, first, the observable fact that organisms, man and animal alike, do adjust themselves to their environment by means of hereditary and habit equipment.[3]

While Watson's contribution was largely prescriptive and methodological, the work of Pavlov on conditioning, and Thorndike on instrumental learning provided the empirical findings critical to the development of a behavioral model.

Pavlov the physiologist, in studying the salivation reflex in dogs, noted that the dogs came to salivate in response to formerly neutral stimuli when the neutral stimuli were presented with (in a number of ways) the stimulus which already triggered salivation. The original stimulus was called an unconditioned stimulus (US) and the new stimulus the conditioned stimulus. The process by which an animal came to respond to a new stimulus was called conditioning. It is basically as follows:

In some situations, if an unconditioned stimulus (US) regularly elicits a certain response (R) and if a neutral

stimulus (one which does not elicit that response) is paired with the US a number of times, then at some point the neutral stimulus alone will elicit the response. It will then be a conditioned stimulus.

Pavlov argued that this process was not peculiar to the salivation response and that the studies in his laboratory would "become classical objects for the new type of research" the study of conditioning.[4] After extensive experimentation he was able to give a detailed description of the process in which measurements of change in magnitude of response and latency of response were accurately carried out and shown to provide reasonably smooth curves when compared with variables such as deprivation, stimulus intensity, contiguity of pairing and numbers of pairings.

Thorndike did experimental work in what today would be called operant conditioning. He showed that in some situations a law-like relationship could be found when one compared the rate at which particular responses occurred and the way in which reward and punishment were made contingent upon the response. In very simple terms a response which brings satisfaction is more likely to recur and those which bring discomfort are less likely to recur. This he called The Law of Effect.

> Thorndike's Law of Effect was a genuine modification in the classical principle of association by contiguity. Finally, Thorndike suggested that an association may not require any ideational process in the animal. This suggestion of his was tentative and

> limited to animals, but it was opposed to the view of classical associationism, and the behaviorists who followed Thorndike extended it boldly.[5]

Although both Pavlov and Thorndike hypothesized about physiological processes involved in the acquisition of new behavior, what was important to psychology was first, their success at finding, through empirical study, significant patterns of interaction between behavior change and environmental change without referral to internal states or processes (neither mental nor physiological), patterns significant for prediction and control of the behavior under study. Second, these findings were achieved by a methodology which was objective in the minimal sense that data and manipulations could be accurately described and measured in such a way that other researchers could examine the studies, repeat the experiments, test related hypotheses and confirm or disprove their claims.

In discussing the background and beginnings of behaviorism the following points arise:

1. By the turn of the century it was becoming increasingly clear that advances in knowledge could be made while simply ignoring certain classical debates in psychology (e.g. mind/body, objectivist/introspectionist, empiricism/rationalism, free will/determinism). Psychologists could get on with their work without explicitly addressing these problems.

2. Behavior itself, organized movements of the whole organism as distinct from mental entities behind those movements or physiological components making them up, became an area of study for psychologists.

3. In particular, there was an increase in the objective study of animal behavior from both a psychological and biological point of view. Both, however, were functionalist views.

4. While this third area, behavior, was added to the original division of psychological study into mind and neural physiology, the distinction between voluntary and involuntary behavior remained.

5. Developments in Gestalt psychology, functionalist comparative psychology and physiology all helped to *delineate questions* about behavior which, by their very characterization, it appeared could not be answered by explanation in terms of a structuralist physiological account of how the components of a mechanism (the organism) functioned while it behaved. These were questions about learning (the acquisition of new responses), goal-directed behavior, and action.

6. Questions about goal-directed behavior and learning could however be addressed by a developing empirical psychology. But the background of knowledge, of understanding and of a growing empirical criterion of meaningfulness had led to an overall context of understanding which both shaped the

particular form of the questions asked and determined what sort of facts could be explanatory (e.g. purpose as homeostasis and adaptation).

B. F. Skinner in 1938 described the effect of this background on early reductive behaviorism.

> When a science of behavior had once rid itself of psychic fictions it faced these alternatives; either it might leave their places empty and proceed to deal with its data directly, or it might make replacements. The whole weight of habit and tradition lay on the side of replacement. The altogether too obvious alternative to a mental science was a neural science and that was the choice made by a non-mentalistic psychology. The possibilities of a directly descriptive science of behavior and its peculiar advantages have received little attention.[6]

Teleology, Purpose and Behaviorism

In examining 20th century behaviorism I cannot review all the important literature about the wide range of topics in behavioral psychology. What I have done is select material relevant to the problem of purpose (purposive action) and point out its importance for behaviorism.

There is a belief about behaviorists held by some critics which stands in the way of attaining a fruitful understanding of various positions within behaviorism. This is the mistaken belief that all behaviorists assume that our understanding of purposive behavior, of acting to achieve some goal, is simple minded mythology and of no interest to a psychologist wanting to give a scientific account of behavior. Combined with this is the misunderstanding that

it is the behaviorist position that correct explanation in psychology entails reducing all descriptions of activity to bodily movements made in response to stimuli. Some behaviorists may well have held this view (e.g. Hull), however not all have, and as I hope to show here, it is those behaviorists with a far less restrictive view of behavior who have been most important in the development of behavioral analysis since J. B. Watson first put forward argument for a strictly behavioral psychology.

Charles Taylor has put forward a version of the belief that our ordinary understanding of purpose, our acting to achieve some goal is of no interest to the behaviorist.

> S-R theorists wish to do away with all notions involving intentionality. Indeed under the influence of empiricist epistemology, they tend to believe that these notions are not genuine empirical concepts, and cannot be given a definite empirical sense unless operationally defined.[7]

And Taylor goes on to quote the behaviorist C. L. Hull as superficially denying this position, Hull states:

> The present approach does not deny the molar reality of purposive acts (as opposed to movements), of intelligence, of insight, of goals, of intents, of strivings, or of value; on the contrary, we insist upon the genuineness of these forms of behavior. We hope ultimately to show the logical right to the use of such concepts by deducing them as secondary principles from more elementary objective primary principles.

Taylor warns us that

> Hull's insistence on the genuineness of these forms of behavior must not mislead us for he is categorically against the view that "what is called goal or purposive behavior . . . cannot be derived from any conceivable set of postulates involving mere stimuli and mere movement" and yet this is what is essential to our ordinary notion of action.[8]

For Taylor, then, the postulates of behaviorism deal with mere movement, while action involves far more than this. Therefore, according to Taylor, behaviorism cannot deal with action. I have outlined Taylor's general position earlier. However, before offering any judgement about that position, let us look at the way in which a number of the behaviorists have dealt with action.

"Action," "Behavior" and "Physical Movement"— The Question of Subject Matter for Behaviorism

In order to understand the developments in behaviorism relative to the problem of action it is necessary to see that what we ordinarily understand as purposive or intentional behavior has always been a main *area* of concern for the behaviorist. This is not to deny that behaviorists have avoided talking about purposive behavior in an ordinary language manner. They have been committed to giving some sort of scientific account of actions, but such an account presupposes at least recognizing purposive or intentional behavior as a particular class and giving some account of the common properties of the members of that class.

R. F. Kitchener, in the conclusion of his study on the meaning of "behavior" in behaviorism, makes the claim that:

> To say, as philosophers are wont to say, "'behavior' means a physical movement of the organism" seems, to put it boldly, simply false . . . although this description of 'behavior' has a germ of truth in it, it is also misleading. For it suggests behaviorists,

> who seem to adopt such a view, are really interested in movements as their subject matter. But as I have pointed out even those behaviorists are not interested in physical movements per se but only as they help them to understand behavior in action.[9]

I do not wish to argue for this point or its implications just yet, but it would seem worthwhile to review a number of Kitchener's examples as this view of behaviorism is critical to my thesis. J. B. Watson, in his book *Behaviorism*, refers to behavior as "anything the organism does—such as turning toward or away from a light, jumping at a sound, and more highly organized activities such as building a skyscraper, drawing plans, having babies, writing books, and the like."[10]

Watson's examples indicate an interest in more than simple reflexes or movements. He was also interested in what the psychologist calls 'molar behavior'. Kitchener makes distinctions between molar behavior, molar movements, and molecular movements as follows: While we ordinarily call *molar* behavior 'action':

> Let us call such things as muscle twitches and glandular secretions cases of *molecular movements*. Such things as an arm-rising, a head turning, a postural change we call *molar movements* (since they can be observed by the naked eye). We could think of molar movements as being described kinematically in terms of some space-time frame of reference. In philosophical literature molar movements would ordinarily be termed simply 'movements' whereas molar behavior might be labelled 'actions'.[11]

Watson however did assume that molar behavioral descriptions are in theory reducible to molar movements and in turn molecular movements in the context of our developing

knowledge of physiology and conditioning. This does not mean that Watson was particularly interested in the *physiology* of movement. It does mean that he maintained a mechanistic view of the *etiology* of action as the organization of movements forming units of molar behavior.

Although Watson recognized actions as particular objects of psychological research he did not accept the need to give a teleological explanation or even description of them. It was a number of other behaviorists, whose work we will examine below, who did so and this was partly in response to Watson's 'mechanism'.

Purposive Behaviorism

The terms 'teleological' and 'purposive' need not be taken as equivalent. We may *stipulate* a rough distinction as follows: 'teleology', the more inclusive term, refers to situations in which phenomena can be described as functioning, or existing, to bring about some effect of the system of which it is a part; here no intention or awareness is required. 'Purposive' refers to situations in which behavior may be described as taking place in order to achieve some goal or effect that an organism is *trying* to attain. With respect to 'action' behaviorists can, perhaps, be fruitfully seen as trying to give an analysis of 'purposive' behavior which accords with this distinction, *but* is also wholly

objective and peripheral. This distinction, though problematic, has been central to controversy about the explanation of behavior. However, the distinction is a useful one to make in beginning our discussion.

If we apply this distinction to behavior we may note examples of teleological behavior, breeding, mating rituals amongst birds and mammals, mass migrations and 'instinctive' behavior in general which need not be classified as purposive but is teleological in the sense that it is functional for the organism as a whole, and brings about effects which satisfy certain needs. We may also note behavior which appears intentional and purposive, e.g. walking to a store to buy food, striking a match to light a fire, filling out an application for a license (and by considerable projection on our part) a dog scratching at a door to get someone to open it.

While purposive description with reference to consciousness remains an important aspect of commonsense discussions of human action in particular, it was replaced in classical ethology with the distinctions between first, the functional, species typical behavioral patterns (e.g. mating rituals) and second, corresponding to purposive behavior, 'appetitive' behavior, for example an animal's trial and error "searching" behavior when it is deprived of food.[12] This approach got around the issue by being purely topographical in the

'analysis' or description of behavior.

Although J. B. Watson avoided teleological accounts of any sort, other behaviorists were very concerned to formulate an objective, scientific account which adequately captured their intuitions as to the teleological nature of some behavior. Kitchener mentions a number of such early behaviorists.

W. S. Hunter suggested three ways in which behavior, the subject of psychology, differed from behavior, the subject of physiology. First, it is externally observable behavior constituting adjustment to the environment; second, it is often adjustment to a social environment; and third (and most important here), it is behavior defined in terms of extrinsic function whose particular movements, or constituent behaviors, have been learned.

The third point here is in contrast with intrinsic functioning such as organ function and reflex muscle contractions.[13]

Another early behaviorist, Edwin Holt "was concerned in a fundamental way with the question, what is the nature of behavior? One way of putting this question is by asking; "What is the correct description of behavior?"[14]

Our answer here need not be taken as an explanation but simply as description and the intuitively acceptable answer will very often be a teleological one, e.g. the bird piling

twigs in a tree is gathering twigs *to build* a nest. The man walking along a street is going to the store to buy bread. Note here that the descriptions in these examples utilize molar concepts, the behaviors are defined in terms of their goal, or function, or effect, or possible effect; there is no referral to biochemistry or physiology. The objects of the descriptions are complex movements organized in terms of attaining a particular end, and finally, the objects of the descriptions are the behaviors of the whole organism and not just of its parts.

According to Holt then, behaviors of psychological interest are responses of the whole organism, the parts of which are organized in terms of what the responses are to achieve. In Holt's terminology, behavior must be understood in terms of its 'objective reference'.[15]

> In behavior (as opposed to movements) there is a genuine objective reference to the environment which is not found so far as I can learn, in the inorganic or in the organic world prior to integrated reflex response. This is the novelty which characterizes behavior.[16]

Again, as with Hunter, the concern is not to give a teleological explanation of the etiology of behavior but to give a delineation of behavior in general for psychology which accords with our commonsense perceptions and yet is not at odds with a scientific explanation. This, for Holt, was still to be a clearly mechanistic account.

The early behaviorists mentioned here were not attempting to explain *why* actions came about by reference to purpose. They were not, for instance, trying to show that intentions are causes. Having noted that the behavior we call purposive, action, is goal-directed, they were attempting to develop a model for *describing* action which accorded both with this fact and with a functional, causal explanation of action.

R. B. Perry's Analysis of Teleology in Behavior[17]

The analysis of teleology in behavior offered by R. B. Perry dealt with aspects which were critical to the work of E. C. Tolman and B. F. Skinner. In particular, his analysis dealt with:

(a) the difference between S-R classical conditioning and operant conditioning;

(b) the variability, plasticity and response equivalence of behavior; and

(c) the docility of behavior, i.e. the determination of a particular response by the tendency of the system of which it is a part to achieve a particular goal.

Perry was not arguing for a teleological determination incompatible with causal explanation. He claimed that teleological determination in a behavioral system is a class of functional relationships between the behavior of an organism

and its environment. Within this class there is a range of characteristics and complexities. Here simple feedback systems, analogous to a furnace thermostat control, are the least complex, e.g. tropisms in simple animal forms. More complexity is exemplified in 'instinctive' behavior such as bird migration, and the highest level is to be found in human action in carrying out an involved plan 'in order to achieve' some end. For example, having set out to holiday in New York this year one engages in a variety of tasks all aimed at attaining that goal.[17a]

Note that the range of characteristics in the three examples is not simply quantitative but is also qualitative with respect to variability and persistence. The thermostatically controlled furnace does only one thing to achieve its goal: If doors and windows are left open it will continue 'trying' until it runs out of fuel. (The behavior does not extinguish much less adapt.) Geese flying south are responding to a triggering stimulus and continue until they reach their winter range. They do not, however, simply leave the nesting area and fly non-stop to the Gulf of Mexico. They travel at different rates, make stops, get diverted by storms, make mistakes and find alternate routes. It would appear they sometimes follow the lay of the land, sometimes the pattern of the stars and perhaps are sometimes guided by the earth's magnetic field.[18] Here there is a

significant, if limited, variability of response and a degree of learning relative to a goal.

Organizing, preparing and actually going to holiday in New York may be done in a tremendous number of ways (many different combinations of discrete behaviors can add up to going to holiday in New York). If due to an airline shut-down you are stranded in Calgary, you may take a bus. If there is confusion at customs and you are refused entrance you may learn how to deal with the matter and do so. If in a hurry you may change plans and find a faster route. But in all this your behavior can be described in part as *going to New York*. For example, when blocked by customs you *persist* and this is persistence to a goal. When the planes are down you *vary* your behavior in order to get to New York.

The Concept of Learning Relative to a Goal

At the end of this range of teleological complexity, where purposive behavior would normally be referred to as 'action', we most clearly see *learning* and *change in behavior relative to a goal*. Here the most complex subclass of 'learned' behavior, and the one most difficult to give a peripheral account of, will be human deliberative action involving consideration and judgement. 'Learning' is ambiguous in psychology and may mean one of two things or both; first, the development of skill or ability, and second,

more important to behaviorism, the increased probability of a particular response to a situation (approximately the same as 'performance'). The concept of learning relative to a goal is an especially important one in understanding later development in behaviorism. To say that learning (sometimes) takes place relative to a goal is to say the following: First, in reference to *conscious states*, if an organism cannot 'get' what it *wants* by engaging in the behavior that it *expects* will achieve its goal, it will *try* something else. When it finds a successful method it will continue *to use* that method instead of the old one. Second, in a non-intentional, behavioral account, particular behavior will be said to *increase in frequency*, or become more *probable* in the following situation:

(a) A particular response now has a significant probability of bringing about a particular effect (the goal).

(b) The occurrence or rate of occurrence of that response is *contingent* upon the *continued probability of the effect*.

(c) If in this situation the particular response ceases to be followed by the particular effect. Then:

(d) Other responses are emitted and that response which is followed by the effect with some significant probability will increase in rate.

A shorthand version of the above may be given in the following way: (1) If a set of stimuli contingent upon a particular response (a certain effect) ceases to be brought about by that response, (2) other responses will bring about that effect, and (3) if that effect is a strong reinforcer, other responses will be emitted and the response which brings about the effect will increase in rate. E. C. Tolman notes another aspect of learning relative to a goal which was important to Perry. This is the situation in which improvement in goal attainment, i.e. reduction in time or effort in reaching a goal, comes about over succeeding trials as an organism learns more efficient behavior.[19]

Both intentional and behavioristic accounts are teleological *to the degree* that they explain behavior not only in terms of antecedent triggering stimuli but more importantly in terms of what the behavior achieves for the organism. The purposive *behavioristic* account differs from the intentional account in that, while the intentional account makes reference to the various states or orientations of the organism, the behavioral account is an account of changes within the functional relationship between the behavior of an organism and its immediate environment. Prediction in the behavioral account (and explanation) are made in reference to the reinforcement history of the organism and not to its particular states. This is still explanation of a

teleological phenomena, purpose behavior.

E. C. Tolman and Operational Definition of Purpose

E. C. Tolman was clearly concerned with action. He argued that molar behavior is an emergent phenomenon with particular descriptive and defining properties, one of the most important of which was its purposiveness.

In the 1925 article, "Behaviorism and Purpose," he attempted to explicate an objective use of the term 'purpose' as a property of the behavior of an organism and not as a mentalistic category.

> When an animal is learning a maze, or escaping from a puzzle box, or merely going about his daily business of eating, nest building, sleeping and the like, it will be noted that in all such performances a certain *persistence until* character is found. Now it is just this *persistence until* character which we will define as purpose . . . such a description appears whenever in order to merely identify the given behavior a reference to some 'end object' or 'situation' is found necessary.[20]

Tolman's concept of purpose is perhaps broader than Perry's, which was essentially the persistence to a goal and the tendency to make improvement in acquisition of that goal. He does, however, credit Perry with first pointing out the "possibility of a purely objective and behavioristic definition of purpose."[21] Like Perry, Tolman does not propose a teleological account at odds with causal explanation. He was arguing for the appropriateness of a purposive account of behavior where the appropriateness could be determined

experimentally:

> To sum up then, whenever, in merely describing a behavior, it is found necessary to include a statement of something *toward which* or *from which* the behavior is directed, there we have purpose. But we may analyze further. Just when is it we find a statement of a "toward whichness" or of a "from whichness" thus necessary? We find it necessary, whenever by modifying the various attendant circumstances, we discover that the same goal is still there and still identifying the given response. Thus when we make minor changes in the position or nature of the *intervening* objects and the behavior readjusts so as to again come to the same end object, the case is one of purpose.[22]

At this point Tolman insisted that his use of the terms 'purpose' and 'cognition' were defined behavioristically and were not mentalistic. He argued for the 'fruitfulness' of this approach. "We are not denying that [the] physiological concepts will ultimately prove the more comprehensive and accurate. . . . But the date at which this last will be possible is far distant."[23]

E. R. Guthrie and the Distinction Between Act and Movement

E. R. Guthrie was concerned with action and gave particular attention to making a distinction between movements and acts. In a survey of behavioral psychology of the 1930's he discusses a number of difficulties in "Codifying the Laws of Learning":

> Experimenters in the field of learning have failed to make clear to the public or to themselves that two fundamentally different kinds of research have been in progress. Some psychologists and physiologists have been interested in the prediction of movement or glandular secretions without any reference to the utility of the movement or its consequences . . . while the psychologist

> whose interest is in the goal-reaching capacity of animals will make goal attainment the entry in the record, and not the means used, which may be varied.[24]

As an example consider Pavlov's interest in physiological response to stimuli compared to Tolman's interest in goal attainment and maze learning where specific motor or physiological description are not at all central.

Again, in discussing the conditioned response and how conditioned responses are to be characterized, he notes that bar pressing in a Skinner box "is an act, in the sense that it means the outcome of *movements which are not specified . . .*"

> the difference between act and movement is of vital importance in learning theory because our accepted notion of mechanism of response is that nerve impulses actuate muscular contraction, and if association or conditioning is to be related to changes in the nervous system it is specific movement patterns which must be dealt with.[25]

Guthrie was, to some extent, at odds with Tolman and Skinner whom he saw as being concerned to explain actions as parts of whole-organism environment interaction while he emphasized the explanation of action as integrated movements whose integration and very existence, as response, was to be accounted for by processes of conditioning and associative learning of composite movements.

Purposiveness, though a genuine property of behavior, was for Guthrie as much a problem getting in the way of explanation as it was an aspect of behavior to be pointed to in explanation. For example, intentional explanations which

do not deal with the process whereby a particular behavior comes about are often *satisfying* when we see behavior as successfully achieving a goal.

> When the outcome of an activity is the achievement of a goal or purpose, there is no need to explain it. We did it, and take credit. But when we fail, we find objective reasons for that failure, reasons which others will understand. If we succeed in getting the engine of a boat started this success needs no explanation other than that we intended to do just that; we are a person of insight. If we fail to start the engine, we go into mechanical details and put the blame on the physical mechanism of the engine, or we talk in terms of forgetting or the association of ideas.[26]

Guthrie is only partially correct here. Sometimes intentional explanations are a problem because they do not deal with the process whereby behavior comes about; e.g. explanations such as "*He bought beer because he wanted to*" or "He misbehaves *because he is bad*" may be vacuous. But sometimes such explanations are both explanatory and adequate. This depends on the context. More important here, both Tolman's and Skinner's analyses of purposive behavior do deal with the etiology of action though not necessarily with the same factors of development as does Guthrie's analysis. However, it would appear that Guthrie could not avoid at least a teleological delineation of behavior without reducing his study to physiology and thereby missing those aspects of behavior that the psychologist is interested in in the first place. For example, 'the moving of my arm as I reach to pick up a pencil' may be described in great detail (including neurophysiology and history of learning). But the behavior

or act was initially specified as reaching to pick up a pencil and this is not dependent on the particular physiological or movement description. At the same time any set of movements will only constitute my reaching to pick up a pencil if picking up the pencil is the goal of my movement.

C. Hull: A Systematic Behavior Theory

The behaviorism of Clark Hull was Perhaps the most thoroughly theoretical and simultaneously mechanistic of all behavioral systems. It was mechanistic in the sense that it contained no reference to consciousness. It was theoretical in the sense that it was a detailed 'hypothetico-deductive' system with an abundance of intervening variables, symbolic constructs, postulates, derivations, deductions and theorems. It was aimed at two general areas: (1) the reductive explications of particular behaviors, including actions, and (2) explanation of the development of behavior as learning.

A detailed explication of Hull's theories would be very difficult and not to the point here. However, his position within behaviorism has been succinctly expressed by E. R. Hilgard in his essay entitled "Hull's Systematic Behavior Theory":

> Like Watson's, Hull's theory is avowedly mechanistic and studiously avoids reference to consciousness. Its central concept is habit, and it derives most of its information about habit from experiments with conditioned responses. Complex behavior, furthermore, is derived step by step from what is known about

> elementary forms of learning. In these respects the theories of Watson and Hull are alike, but in other respects Hull's system represents a great advance over Watson's. Hull took the detailed findings of conditioning experiments much more seriously than Watson, who was satisfied to make use of the general paradigm provided by conditioned responses. Hull adopted (and adapted) Thorndike's Law of Effect, whereas Watson rejected it. For Watson's policy of denials and negations, Hull substituted a positive program of trying to explain purposes, insights, and other phenomena difficult for behaviorism to encompass.[27]

Hull developed a schemata for the description of purposive behavior which avoids or tries to avoid all teleological terms. His strategy was to show how, given that organisms have certain innate characteristics and that certain laws of learning hold, goal-directed behavior would be learned. This analysis included behavioral parallels to various 'conscious' aspects of goal-directed behavior such as expectancy.

Consider the following situation: At time T^1 you get up from your desk and head for the kitchen to get something to eat (the goal). At T^2 you are walking down the hall. At T^3 you are eating. A behavioral (Hullian) analysis which tried to do justice to the purposive nature of your walking down the hall at T^2 *might* look something like this:

1. You are an organism with the following innate characteristics:

   (a) A need for food.

   (b) A disposition to emit need termination responses under certain 'stimulus' and 'drive' conditions (presented with food when you have not eaten all

day—you are likely to start eating).

(c) A sensitivity to drive-reducing stimuli (when you get full you stop eating, generally) which are reinforcing stimuli (next time you are hungry you are likely to emit similar drive-reducing behavior —a case of operant conditioning).

(d) A tendency to generalize responses to similar (and/or close in time stimuli from specific conditioned S-R situations.

(e) The possession of a 'drive' which is accompanied by a drive-stimulus (when you do without food you get hungry).

2. Your walking to the kitchen for food was learned as a result of the original reinforcement of response (eating in the kitchen) *generalizing* to *progressively more remote stimuli*, in particular the drive stimulus existing prior to your eating. Thus, walking down the hall to the kitchen for food, you are now having an 'anticipatory goal response'. In other words, through a history of reinforcement made possible by the kind of organism you are, the whole sequence of events leading up to the actual goal attainment has now been learned (and this includes our 'anticipation').

I will take up this point later to disagree with the effectiveness of Hull's analysis. I think it does not work, in this case, because of some unresolved problems about the nature of action. However, it must be noted that Hull went

to tremendous lengths to give a detailed non-teleological account of purposive behavior (my example contains barely a sketch of his approach). He did much to suggest how behaviorism could be made more rigorous and this was in part by demonstrating how operational definitions of terms might be based on observations in psychology. However, his construction of an extensive hypothetico-deductive system while it may have led to interesting research by others, has not itself been very productive as measured by its ability to facilitate prediction and control.[28] Here, it is not clear that Hull's postulated intervening factors such as 'drive' and 'anticipatory goal response' are to be understood as intervening variables, purely theoretic, or as measurable events in the organisms. In any case they are perhaps no more capable of operational definition than are ordinary intentional terms such as 'desire' or 'expectancy'. His analysis of action can be seen as in part an attempt to avoid mentalism and teleology by substituting a set of 'scientific' variables for intentional concepts. This did not show that those concepts are meaningless or useless outside of an operational, behavioral definition.

It is interesting to note B. F. Skinner's comments on the role of grand learning theories such as Hull's. His comments directed to a large extent at Hull's work, and written in 1950, parallel his early criticism of Watsonian

reductionism.

> When we attribute behavior to a neural or mental event, real or conceptual, we are likely to forget we still have the task of accounting for the neural or mental events. When we assert that an animal acts in a given way because it expects to receive food, then what began as the task of accounting for learned behavior becomes the task of accounting for expectancy. The problem is at least equally complex and probably more difficult. We are likely to close our eyes to it and to use theory to give us answers in place of the answers we might find through further study. It might be argued that the principal function of learning theory to date has been, not to suggest appropriate research, but to create a false sense of security, an unwarranted satisfaction with the status quo.[29]

> It is argued that research would be aimless and disorganized without a theory to guide it. The view is supported by psychological texts which take their cue from logicians rather than empirical science and describe thinking as necessarily involving stages of hypothesis, deduction, experimental test, and confirmation. But this is not the way most scientists actually work.[30]

Let us look then at Skinner's approach to explaining action in which 'theory' is minimized.

CHAPTER 5

B. F. SKINNER AND THE EMPHASIS ON OPERANT CONDITIONING

I have been arguing throughout this paper that behavioral psychologists and their precursors were faced with the problem of giving an objective account of behavior of organisms. This involved getting clear as to exactly what was to be understood by 'behavior'. This in turn meant having to give an objective account of what we casually call purposiveness or, particularly in the case of humans, intentional behavior, action. Actions, it has been noted, tend to result in the achievement of goals and are defined or described in terms of their achievements rather than the particular movements which make them up.

While Watson and Hull aimed at analysis which avoided any hint of teleology, focusing in particular on movements and stimuli, B. F. Skinner developed a system for the analyses and description of behavior which takes this commonsensical understanding of action, albeit in a rarified form, as a basic proposition.

Skinner saw the S-R behaviorism of the 1920's and 1930's as inadequate in dealing with much complex behavior.

His comments written in 1938 sound like criticisms of behaviorism one might read today.

> With the discovery of the stimulus and the collection of a large number of specific relationships of stimulus and response, it came to be assumed by many writers that all behavior would be accounted for in this way as soon as appropriate stimuli could be identified. Many elaborate attempts have been made to establish the plausibility of the assumption, but they have not, I believe, proved convincing. There is a large body of behavior that does not seem to be *elicited*, in the sense in which a cinder in the the eye elicits closure of the lid, although it may eventually stand in a different kind of relation to external stimuli. The original 'spontaneous' activity of the organism is chiefly of this sort, as is the greater part of the conditioned behavior of the adult organism, as I hope to show later. Merely to assert that there *must* be eliciting stimuli is an unsatisfactory appeal to ignorance.[1]

For Skinner, however,

> An event may occur without any observed antecedent event and still be dealt with adequately in a descriptive science. I do not mean that there are no originating forces in spontaneous behavior but simply that they are not located in the environment. . . . This kind of behavior may be said to be *emitted* by the organism, and there are appropriate techniques for dealing with it in this form.[2]

This was to be done by placing an emphasis on *rate* of behavior and by attending to behavior in two categories of response.

> The kind of behavior that is correlated with specific eliciting stimuli may be called *respondent behavior* and a given correlation a *respondent*. The term is intended to carry the sense of a relation to a prior event. Such behavior as is not under this kind of control I shall call *operant* and any specific example *an operant*. The term refers to a POSTERIOR event.[3]

At the very beginning of "The Behavior of Organisms" he argues for the scientific study of behavior per se, distinct

from any inferential, assumed or generally non-observable factors and here he gives a number of definitions of 'behavior' for such a science.

> Behavior is what an organism is *doing*—more accurately what it is observed by another organism to be doing. But to say that a given sample of activity falls within the field of behavior simply because it normally comes under observation misrepresents the significance of this property. It is more to the point to say that behavior is that part of the functioning of an organism which is engaged in acting upon or having commerce with the outside world.[4]

Although there has been considerable development and change in the Skinnerian position since 1938, he has been remarkably consistent on the significance of the operant/ respondent distinction. It was over forty-five years after making the above remarks that he wrote

> Possibly no charge is more often leveled against behaviorism or a science of behavior than that it cannot deal with purpose or intention. A stimulus-response formula has no answer, but operant behavior is the very field of purpose and intention. By its nature it is directed towards the future; a person acts in order that something will happen, and the order is temporal.[5]

B. F. Skinner has argued that his operant psychology rather than a theoretical S-R psychology is capable of a fruitful analysis of operant behavior "the field of purpose and intention." Let us review then some of the main concepts of operant psychology.

The study of operant behavior has ranged from observation of trial and error learning (e.g. Thorndike) to the study of learning in controlled operant conditioning

experiments (practically all work published in the *Journal of the Experimental Analysis of Behavior*), to analysis of animal behavior in field situations, and to a whole variety of human social situations. But a number of basic concepts remain throughout. These can be described in a reasonably straightforward fashion but the details of the concepts are in some places extremely subtle. What follows then is a basic outline of what I take to be a number of concepts critical to the discussion here.

First, operant psychology is primarily concerned with the role of learning in the etiology of action (goal directed behavior), and all patterns of response which may be shaped by the consequences of their *particular* behaviors.

Second, its *focus* of attention is not on the relationship of behavior to events which may stimulate, trigger or elicit it (though this is not ignored) but rather on the relationship between changes in patterns of behavior (in frequency and particular characteristics) and changes in the consequences of particular behaviors. This notion has been put in the following way:

> a) All animals and humans are behaving creatures.
> b) A given act is followed by an experience that is a consequence of that act.
> c) The quality of the consequence influences further action. An important point is that the consequence arises in the outer environment. Therefore the environment holds the key to most of the changes that occur in the way a person behaves.[6]

The position thus stated is very clear, but in this form is unnecessarily strong and open to obvious objections. The following statement more aptly describes the position: It is assumed that for all organisms the probability or frequency of a behavior *may* be influenced by the consequence of that behavior.[7] Behavioral psychologists do in fact look for the limits of conditioning as a factor in the etiology of behavior.[8]

An analysis of the function of behavioral consequences is then critical to the explanation of operant behavior. Here it is just those situations in which consequences do affect behavior which are called instances of operant conditioning. In these situations the probability of behavior is said to be affected by the consequences of the following types:

—*Positive Reinforcement* refers to a consequence which increases the probability of the response which brought it about.

—*Punishment* is a consequence which decreases the probability of the response which brought it about.

—*Negative Reinforcement* is the cessation of an aversive stimulus as a consequence of a behavior. This increases the probability of the behavior.

—*Negative Punishment* or *omission* is simply a change in

contingency such that a positive reinforcer is absent following a behavior which it was formerly reinforcing.

There are many variants of operant conditioning procedures, the most significant of which for our purposes are *free-operant procedures with*:

(a) schedules of reinforcement as the independent variable.

(b) behavior shaping through reinforcement of successive approximation as the overall dependent variable.

(c) extinction by ommission of reinforcement.

(d) *discrimination learning* in which a subject learns to respond to one stimulus rather than another.

Free-operant procedure is distinguished from discrete trial procedure. In the latter case the experimenter sets the subject in a position to make a single response and then measures the time taken to make the response. Change here is noted as a function of change in reinforcement or number of trials. In the free operant procedure, the subject is placed in an apparatus and is 'free to respond at any time'. The experimenter manipulates the consequences of responses and records the frequency of response as a function of number of trials and changes in reinforcement.

Shaping

In some experiments a rat placed in a 'Skinner' box will *press* the lever (the eventual operant) at a low rate. The experiment then proceeds by varying the consequences of pressing the bar and noting changes in rate of response. If the rat does not initially press the bar the experimenter can engage in a 'shaping' procedure by presentation of food upon successive approximations to a bar press. Thus, initially, the rat may be rewarded for moving close to the bar. Then only when it is close to the bar and facing it. Next, only when it is close to the bar; faces it, and raises its paws. And finally only when it presses the bar. Further experimentation then proceeds.[9]

Schedules of Reinforcement

With positive reinforcement the reinforcement may be (a) constant, after each response, or (b) partial, and here different schedules are possible.

1. On a fixed-interval schedule (e.g. every sixty seconds.
2. On a fixed-ratio schedule (e.g. every fourth response.
3. On a compound schedule (some combination of the above).

4. On a variable—interval schedule or variable—ratio schedule.

Different schedules yield different learning curves, different rates of response, and different rates of extinction of the response when reinforcement stops. The largest difference is between response learned under constant reinforcement which extinguishes relatively quickly and that learned under partial reinforcement which generally takes much longer to extinguish.

This outline of some of the basic concepts of operant psychology in the experimental study of behavior is an extremely limited one, however adequate for discussion here. I make this point in part to indicate that psychological behaviorism may be much more complex than some of its critics appear to think it is. A similar problem arises of course when a psychologist like B. F. Skinner 'extends' his claims about operant conditioning, devoid of sophisticated detail, to human society in general, and, without emphasizing the degree to which such analysis is hypothetical outside the laboratory.

The Explanatory Pattern of Operant Psychology

There are a number of general characteristics of operant analysis which arise from this description. They are:

1. The meaning of important terms are *defined*, e.g. positive reinforcers are defined as events, contingent on a behavior, which increase or maintain the frequency of that behavior.

—Punishment is that consequence which decreases the frequency of behavior.

2. However, the determination of whether an event or class of events is a significant consequence (e.g. reinforcer) is always an *empirical* matter.

To search for a reinforcer, or a history of consequent determination is not to assume that a particular act *must* have been determined in just this way. One need only assume that behavior is often determined this way and that the behavior now under study may be such a case. Operant terminology might be criticised for being circular, but the fact that the meaningfulness of a claim about operant conditioning is dependent upon a definition of terms does not make research any less empirical.

3. *The object of explanation* in which operant conditioning is appealed to will be either a pattern of behavior or a particular instance of a pattern. In terms of operant conditioning, an action cannot even be discussed except as one instance of a series of similar responses or as one instance in a series of successive approximations.

4. An explanation in terms of operant conditioning will be *part* of an appropriate answer to the following sorts of questions about action:

(a) What facts explain that an individual now makes this sort of response to these sorts of situations?

(b) What particular facts about the history of this organism's interaction with its environment would have to have been different if it were to behave differently than it does now?

(c) What changes in the environment now will bring about specific changes in behavior in the future?

(d) What facts may explain how an individual learned to act in a particular way?

5. Explanation in terms of operant conditioning is *not* applicable in answer to the following sorts of questions:

(a) What is your purpose in doing 'X'?

(b) What reason do you have for performing this action?

(c) How is this action to be understood?

(d) In what context does it make sense?

Often people can simply tell you their *reason* for acting, and assuming they are being honest, in one sense of 'reason' their answer cannot be wrong. It may often be at odds with what we think are the 'real reasons'. But it will very often *accord with* our commonsensical 'operant'

analysis. For example, if a man runs into the house and his reason for this action is to get out of the rain, this is not at odds with an operant account of his learning to run into the house when it rains.

6. All cases of action characterized as a physical performance aimed at bringing about a measurable effect can be given a description which will *conform* to an operant account. This of course is not sufficient to show that every case of action so described is the outcome of operant conditioning. Consider the following examples:

A man seldom *discusses his work* (the operant), with his wife. On one of these occurrences *she seems exceptionally interested* (reinforcement) in his comments. During the next week she responds with interest almost every time he discusses his work (the schedule). During the following week he discusses his work with his wife each day and on the Thursday of that week he spends almost a half hour describing in detail a problem he has been dealing with at work (increased probability of response). This is the first time he has engaged in such detailed discussion with his wife for more than a few minutes. Now the questions arise: Why did the man give his wife a detailed description of the problem? What reasons would he offer for having done so? What factors brought this event about?

However one might *describe* the situation, the scenario as just depicted *conforms* to the operant conditioning paradigm of positive reinforcement as described here. A series of events leading up to many instances of human action and also particular instances of animal behavior will also conform to the pattern.

That it conforms to the pattern does not though prove that this is an instance of operant conditioning, e.g. perhaps the man's boss instructed him to thoroughly discuss an issue with his wife that day. It may have been an issue he would have discussed in detail with almost anyone he met when he got home. Perhaps he has started taking a stimulant drug on the way home each day and on this day he doubled the dose.

Though we may accept that operant conditioning is a real phenomenon, whether or not it explains a particular behavior will depend both on what aspect or characterization of the behavior we are asking about and on the chain of events leading up to that instance of behavior. Thus, if operant psychology is to be extended to activity in society outside the animal laboratory, it must be taken as clearly hypothetical and then substantiated by something like the same sort of rigorous methodology that is required of it in the laboratory.

Summary

A number of critics of behaviorism (Taylor and Hamlyn from the common sense, purposive position, and Koch and Rodgers from the phenomenological position) have characterized the debate roughly as follows:

The behaviorist position, *they claim*, is that all behavior is *just* more or less complex stimulus-response interaction arising from repeated incidents of 'conditioning'. 'Explanation' thus consists in cataloguing changes in behavior as a function of changes in environment, describing a limited case of conditioning and inferring or assuming that the conditioning took place everywhere else. (Here there is no place for agency.) These opponents of behaviorism claim that what is really going on, at least in the case of some higher organisms, in particular man, is that many behaviors, actions, arise only when those actions appear to the organism as having a high probability of bringing about an effect, an effect which is a goal meaningfully selected and perceived by the organism and to which he is intentionally oriented.

Now what this second, non-behavioral, claim means, and whether it can mean something significantly at odds with what all or some behaviorists have said, are questions central to an understanding here. What I have shown at this point is

that to construe the general behaviorist account of action *simply* as *environmental change, a stimulus, yielding a response* or as 'muscle twitch psychology' is to miss the point that behaviorists have very often been grappling with many of the same problems of understanding action as other psychologists.[10]

I have argued by example here that the *purposive nature* of behavior which we commonly call action has been a major concern of behavioral psychology. This is not to say that behaviorism is really a kind of intentional explanation in disguise, nor is it to say that the behaviorist's analysis is always significant or that it negates non-behavioristic accounts. It does mean that a behavioral analysis may well be applicable to, and informative concerning, questions about action in philosophy, in psychology and in everyday life. This last point will now be developed as we return to an examination of criticisms of the behavioral thesis in the light of the above analysis.

CHAPTER 6

CRITICISMS REVISITED

Sigmund Koch and the "Breakdown" of Behaviorism

It is Hullian S-R theory to which the criticism of Koch and Taylor (aside from his comments on teleology) most aptly apply. Hull did indeed try to develop a 'hypothetico deductive' theory with variables and postulates tied to observation. This system however broke down not because logical positivism is invalid but more importantly because the system does not work. Hullian S-R theory was an extreme position in behaviorism, but it is not all behaviorism, nor has it ever been to any degree descriptive of what went on in behavioral psychology at the laboratory level. Behaviorism is almost by definition 'positivist' in a general sense. This meant a number of things; the rejection of mentalism, the restriction of propositions to clearly testable claims, the emphasis on measurable data, the search for causal-functional relationships between data as a basis of explanation and finally the operational definition of explanatory non-observable variables. It is really only at this last point and its connection with criteria for theory construction that it has 'broken down'.

Koch claims that this breakdown has come about as a result of a too strict requirement of meaningfulness. In speaking about science in general he claims that particular discriminations and their corresponding propositions may only have meaning for those who have developed a fine sensitivity through communication and training with a very few people, in a very limited environment. Therefore "any formulation of the meaning criterion demanding a wider consensus group for *admission* of a term as meaningful (the position he sees in behaviorism) would eliminate much meaning either from our universe of approachable data, or from that of the scientifically sayable." To place this sort of demand on psychology as the behaviorist positivism does is, according to Koch, to arrive at "an extraordinary degraded formulation of the psychological questions of potential interest to human beings."[1]

The essential objection here seems to be that a demand for public observability of subject matter precludes the study of anything like private events in psychology. However, it is not clear just how widening of a consensus group (assuming "group" means two or more) entails this.

Though Koch's comments were made twenty years after the peak of interest in Hullian theory, he does not elaborate on what exactly this 'breakdown' of behaviorism has meant.

There have certainly been changes in behavioral research and one can of course point out that behaviorists are not now using Hullian postulates or a host of intervening variables operationally defined. But they are still avoiding mental talk, sticking to measurable data, plotting functional relationships between observable variables, engaging in experimental work to test and confirm hypothesis and all the time keeping intervening variables to a minimum. They still take little interest in psychoanalytic theory, cognitive psychology or phenomenological psychology.

Charles Taylor: Epistemology and the Rejection of Purposive Explanation

Though Taylor's criticism is similar to Koch's it is more explicitly directed at what he takes as behaviorism's inability to deal with intentional behavior and teleological explanation. According to Taylor, "Operationism does not allow for any psychological concepts"—to the extent they are empirical they deal with non-psychological events, and to the extent they deal with psychological events, they are inadequately 'interpreted' and therefore non-empirical."[2] It follows from this that any explanation in terms of consciousness and intentionality cannot be meaningfully stated and the "question seems to be decided a priori, and one view rejected as meaningless."[3]

According to Taylor:

> cogent reasons have yet to be given for considering the mind as an inaccessable inner locus of events. But until these are produced, there is no reason to accept the operationist premiss that psychological statements are unverifiable unless interpreted in non-psychological terms. *The epistemological ruling to this effect, then seems to be simply another attempt to legislate a priori on the question whether explanation by purpose is the mode which obtains with animate organisms.*[4]

Taylor's assumption here, regarding purposive explanation, is only partly correct. *Intentions*, for instance, do not enter into behavioristic accounts; but some behaviorists do deal with actions as a class of goal or consequence directed events. We have shown that purposive behavior, behavior directed to a goal, has in fact been a major concern of behaviorists, and though some, Watson, Hull, Guthrie, tried to give an account of such behavior in molecular S-R terms, others, e.g. Tolman, Skinner (and also the philosopher, Perry), have begun with a description of behavior as goal directed and tried to give an explanation of the etiology of that behavior as the outcome of a history of organism-environment interaction. This seems to *accord* with Taylor's view that explanation by reference to purpose is *not*, most significantly, explanation by reference to mental predicates.

The Meaning of Behavioristic Concepts and the Question of Context

Taylor and Hamlyn have given more detailed criticisms which I now want to consider. In particular they have

questioned the meaningfulness of some basic concepts in behavioristic analysis.

"Stimulus" it is argued is a problematic term which can not be defined environmentally or physiologically. According to Taylor:

> Animals can be trained to react differentially to all sorts of features of the environment, not only to its elements, but to its configuration, to the number of elements rather than their shape, size, etc. and even to complex relations. Thus the animal's reponse to a given environment is not a function of the same elements all the time, but the way in which it is relevant to his behavior can change or, as we would say in ordinary speech, the way the environment is seen by the animal changes.[5]

This according to Taylor, indicates along with the intentional nature of environmental interaction, that some simple mechanism, i.e. the behaviorist's, will not account for discrimination learning in situations involving complex perception. In a sense this is true. One need only, for instance, point to the complexity of the brain as a less than simple mechanism involved in learning. However, it does not follow from this that one cannot describe the relationship between behavior change, stimuli and reinforcement in discrimination learning, *nor* does it follow that *this sort of description*, particularly as it points out functional relationships and triggering events *will not* be explanatory. As was pointed out earlier, explanations are always practical in the sense that they are answers to

particular questions and that not all necessary conditions can be dealt with. There may well be a limit to the extent to which a behavioral analysis can deal with problems of perception. But, to use Taylor's argument, this limit can not be set a priori.

If one wants to know what chain of events in the environment result in a pigeon now responding to a red visual cue rather than a green one, then one can assume for instance, that neural mechanisms of perception are functioning. This becomes a necessary condition for the event under consideration and moves into the background of information and assumption. That this may be a problem is not simply a matter of the logic of explanation but also a practical problem for research. This does not mean that behavioral research is misguided. It does mean that, as in practical research everywhere, one should be clear about assumptions rather than cavalier about what is ignored.

'Response' according to Taylor is another problematic term for it cannot be treated as 'colorless movement'. It must be defined in terms of its goals.

> It is obvious that animals have a more or less flexible repertoire, and that they can substitute one behavior route for another to the same goal, if the first is inappropriate; so that to say that an animal has 'acquired a response' is often to say he has learned to bring about some outcome.[6]

But surely, as we have shown, not all behaviorists have tried to account for response as 'colorless movement'.

Operant conditioning studies and behavioral analysis do treat many behaviors, including actions, as goal-directed. Here, they often are importantly *so described*; but the *description* is not an *event* explanation. It doesn't explain the etiology of the behavior or action. It specifies what is *to be* explained.

Intentional Explanation, Behavioristic Explanation, and Characterization of the Explanandum

Part of the difficulty here is due to the different ways in which actions as objects of explanation are characterized for different types of explanation and, in turn, the difference in specificity or target relevancy that results. Consider the following example which falls somewhere between the behaviorist's paradigm case of operant conditioning, the rat in a Skinner box, and the moral philosopher's paradigm of action, the stealing of a loaf of bread after careful deliberation about one's plight and one's values. A parent and child are in a supermarket and the parent has just refused to buy candy for the child. The child now 'acts' in the following way—He yells, "I want candy!" and continues to repeat this act many times. Now the general question arises—"Why is the child yelling for candy?"

A simple intentional analysis will point out that the child is shouting in order to get his parent to buy candy.

The analysis becomes more sophisticated when it points to beliefs and desires that the child has and the way in which these beliefs and desires make this sort of action sensible or at least understandable. Here the explanation need not go beyond the immediate situation and the state of the child in that situation.

A tentative behavioral analysis of the same situation, i.e. an operant conditioning analysis, will start with a description of the event as the child yelling for candy in this *particular* situation and will go no further except on the assumption that this is one instance of a series of the same type of behaviors, or is the last of a series of successive approximations (a case of shaping). On this assumption then, the situation appears to fit a pattern of operant conditioning in which the parent has rewarded the child in similar situations thereby making the behavior more probable according to the general rule that when some preferred activity (eating candy) is contingent upon a particular behavior that behavior will be increased.

The behavior is now seen as a dependent variable, the parent giving in as the schedule and eating the candy the reinforcement. This is a *tentative* explanation and may be completely wrong. All this may be tested with co-operation of the parent. One can search for other independent

variables, e.g. particular cues, patterns of scheduling and other reinforcements. Variables can be manipulated and results charted.

We may be able to modify the child's behavior through operant conditioning, perhaps reduce to zero the rate of this particular behavior. This would be *evidence* that the original behavior was a case of implicit conditioning; but it wouldn't be compelling evidence. The fact that behavior change may be brought about through conditioning does not show that any particular behavior is an instance of conditioning. One needs to examine the history of the organism and the behavior in question. It may be that in the particular case *no* significant support, no history of anything resembling conditioning, can be found. But, the problem of giving the history of a particular phenomenon is an explanatory problem for *psychology in general* not just behaviorism. Here I have only meant to indicate the way in which a behavioristic analysis *might* be meaningful.

Let us re-examine the original question in the light of these two different approaches. One way of getting at the different meanings of the question is to begin with the following translation: "What facts explain the child's shouting for candy?" Now note the kind of facts pointed to by the different explanations. The intentional analysis

holds up as explanatory the state of the child as he is acting. The behavioral analysis says almost nothing about the state of the child as he is acting, instead it focuses attention on the learning history of the child in which behavior changed as a function of environmental change.

In the first case the child is said to act because he is in a certain state and because he has certain beliefs. In the second he is said to act because of a series of events in the past. Are these two explanations really at odds? In what sense does the second deny the first? Surely if we ask 'Why is the child shouting for candy?', and mean "what reason does he have?" our question will be answered by being told "He believes that if he keeps it up he will get the candy." However if we want to know how he acquired the response, this answer will not do, while the operant conditioning analysis does provide an explanation which is plausible and amenable to testing. The intentional explanation is simply not addressed to a question about the etiology of the behavior.

This suggests that an intentional explanation when limited to either rationalization or state description is compatible with an operant conditioning analysis of the etiology of action. This is not analysis of a 'colorless' movement. It is the analysis of learning to perform a

certain behavior in order to attain what is contingent upon that behavior. Here the behavior, the action, is defined, delineated, and understood in relation to its reinforcing effects.

Underlying Connections

Hamlyn and Taylor both claim that a molar psychology (behaviorism) unlike neural psychology will always be non-explanatory becuase it makes no reference to underlying connections. According to Hamlyn, a behavioristic analysis will never be explanatory so long as it does not go beyond noting correlations between factors. He has said—what studies of conditioning

> seem to have in common, is that there takes place some process whereby a connection *appears* to be set up between two factors in the situation, between stimulus and response, between response and reinforcement, or between stimulus and image. I say 'appears to be set up' because strictly speaking it is merely a matter of hypothesis that there is such a connection. All that can be observed is that response, or whatever it is, takes place and becomes more probable in the given situation, whether this is due to an underlying connection and of what kind is another matter. THAT is where theory comes in.[7]

We can certainly agree that some behaviorists, Skinner in particular, have given little attention to 'underlying connections'. This does not mean that their findings, therefore, have 'no explanatory value'. We may judge explanations in terms of effectiveness in bringing about prediction and control, in terms of their meeting the requirements of an

explanatory model or by the degree to which they help to provide *understanding* in a number of different senses. But their explanatory value must be determined relative to the particular questions being asked. For any particular action, or event, some characteristic of which is wanting explanation, an underlying connection may not be at issue.

Behaviorism, most centrally, is simply not about underlying connections. It is about the relationship between behavior, in particular the patterns of learning behavior, and patterns of change in the environment. Psychological behaviorism (as distinct from philosophical behaviorism) requires only a methodological position concerning underlying connections, be they mental, mechanical, physiological, or intentional. That methodological position is essentially to refrain, wherever possible, from consideration of underlying connections.

According to Skinner

> The practice of looking inside the organism for an explanation of behavior has tended to obscure the variables which are immediately available for a scientific analysis. These variables lie outside the organism, in its immediate environment and in its environmental history.[8]

The behaviorist may infer that some sort of *necessary* connection is to be found in the pattern of changing events and behavior which he is examining. This need only be stated in terms of dependent-independent variable relationships

and/or changing probabilities, not in mechanistic terms.

In discussing the difference between physicalism of the Vienna Circle and behaviorism as philosophy, Norman Malcolm claims: "Carnap asserted that: a sentence about other minds refers to physical process in the body of the person in question. On any other interpretation the sentence becomes untestable in principle, and thus meaningless."

Malcolm states:

> Skinner could say, and I should agree with him, that this is a *non sequitur*. If the statement that a certain person is *discouraged* about something refers to his behavior, and also to external circumstances which 'control' that behavior, then the statement is testable; and it is far more testable than if if referred to processes in his body. Thus Skinner's brand of behaviorism explains psychological concepts in terms of outward behavior and circumstances rather than inner physiology.[9]

It is in the above sense that behaviorism is explanatory but not reductionistic.[10] It is, in a large part, a search for law-like relationships between patterns of behavior change and change in the environment. Individual actions as explanandum events are generally shown to be part of a pattern and explained with the explanation of the pattern.

The only thing that seems to come close to the *underlying connection* that Hamlyn would like to see is to be found in the functional, *adaptive* characteristics of behavior. This is certainly an important matter for behaviorism. But it is a matter which is no more inside than outside the

behaving organism and it is not clear that it would be most appropriately described as *underlying* learning phenomenon.

Daniel Dennett: A Case of Misunderstanding Skinnerian Behaviorism

In my explanation of behaviorism in Chapters 3 and 4, I noted the following concerning behavioral psychology. It is concerned to give an account of action and it is about a variety of behavioral phenomena other than simple stimulus-response relationships. It is primarily about learning and the acquisition of patterns of behavior. It is concerned to discover the boundaries of learning including the limits of response generalization. The central object of study in *operant conditioning* experiments is the *rate* of response as a function of changes in the reinforcement schedule. The particular questions that a behavioristic explanation is meant to answer are all concerned with the functional relationship between environmental change and behavior change. No 'physiological' or 'intentional' system need be at issue. Whatever these last two terms refer to they fall into a background of conditions for the behavioral factor being sought as explanation.

In his article "Intentional Systems"[11] Daniel Dennett misses most of these points when criticizing Skinnerian behaviorism, and thus his remarks fall short of genuine criticism.

At this point in his essay he is arguing that intentional explanation is valuable but insufficient for psychology. True, deeper explanation is to be found in "whatever mechanistic regularities there are in the functioning of internal systems whose design approaches the optimal (relative to some end)." He offers Skinner's approach as an example of the wrong tack in dealing with the insufficiency of intentional explanation. In particular, he considers the problem of making predictions about behavior and takes up, as an example, the case of the lever box in operant conditioning studies. Speaking of Skinner he says:

> To provide a setting for non-intentional prediction of behavior, he invented the Skinner box, in which the rewarded behavior of the occupant—say a rat—is a highly restricted and stereotypic bodily motion—usually pressing a bar with the front paws.[12]

Now, whatever might be done with this apparatus it was clearly not Skinner's objective in developing it to make non-intentional predictions of behavior. Skinner states that at that time he was simply 'looking for lawful processes in the behavior of organisms'.[13] His discussion of the development of the lever or 'Skinner' box does not mention the prediction of behavior at all. It does mention his desire to construct a controlled setting in which various relationships between behavior change and changes in reinforcement could be accurately and efficiently recorded over relatively long periods of time.[14] Thus Dennett has misconstrued the behaviorists'

project from the beginning. However, it is instructive to continue with an examination of Dennett's remarks.

Dennett argues that when an animal is trained in a Skinner box and a regularity develops between non-intentionally characterized events this is due to a property of the Skinner box and not the occupant.[15] But how is this meant to be a criticism?

In a sense it was just the effects of changes in the controlled environment (the apparatus) on behavior that Skinner was wanting to chart. The importance of the animals' internal system is not being denied here, it is being ignored. However, note that one might run experiments with the *same* apparatus and the *same* changes in control. For example, variations in schedule of reinforcement may be set up but with different sets of animals, and different species or different age groups, and, thereby, age or species specific patterns of learning *due to the nature of the organism* may be discovered.

Dennett seems particularly concerned with the restrictive nature of the Skinner box and I agree this is *a* problem but not the problem that Dennett sees. He asks us to consider the situation of a mathematician strapped in to an appropriately sized Skinner box equivalent, in which he is restricted to two possible responses, either pushing button

marked '35' or pushing button marked '34'. Some reinforcing system is in place and the 'controlling stimulus' is the question "What is 7 x 5?" Now Dennett claims that to *predict* that the subject will push #35 in a significant number of cases *on the basis* of a description of the situation in terms of conditioning, in which intentional vocabulary is eschewed, is not satisfactory scientific prediction at all. It is simply disguised intentional prediction.[16]

But does this criticism bear upon psychological behaviorism? I think not. Dennett's caricature of a conditioning experiment apparatus is just that—a caricature of the apparatus, not the experimental analysis of behavior. What sort of lawful process might a researcher be looking for in the situation Dennett depicts? Would any psychologist qua researcher, engage in this sort of prediction? Dennett is not addressing the issues of just how and what a behavioral analysis is meant to explain. Again, note that in a similar situation one might experiment with variations in reinforcement, stimulus control and responses of the mathematician. For example an interesting project might be to try and discover what effect variations in reinforcement had upon the limits of discrimination learning with smaller and smaller print size. And here it is not clear that an intentional prediction would be useful.

Dennett claims that a problem for behavioristic explanation is that it is 'action and not merely motion' that one predicts in the Skinner box situation.

> Suppose a mouse were trained in a Skinner box with a food reward, to take exactly four steps forward and press a bar with its nose; if Skinner's laws truly held between stimuli and response defined in terms of bodily motion, were we to move the bar an inch farther away, so four steps did not reach it, Skinner would have to predict that the mouse would jab its nose into the empty air rather than take a fifth step.[17]

But this is not the case at all. As we have shown operant analysis is most centrally an analysis of action (at least of a limited range of action). The operant in the Skinner box is pressing the bar far enough to activate the recorder. If the mouse had learned to press the bar and this required four steps then 'Skinner's Laws' would predict that if you moved the bar a small distance away the mouse would continue to press it. However, if the mouse had been trained to discriminate between the bar being four steps away and a number of other distances (e.g. the bar was presented at various distances and the mouse was punished for pressing it at other than the four step position, at which it was rewarded), then the behaviorist's prediction might be quite different. (These suppositional 'experiments' have their limits of usefulness, at some point it becomes appropriate to go and examine the research.)

Dennett now considers the sort of objection I have raised as a 'variation of the Skinnerian theory'. But this aspect

of Skinnerian theory far from being a variant is at the very centre of Skinner's approach. However, let us consider his objection to this 'variation'. He argues that even if this were the case certain problems of perception would result in wrong behavioristic predictions and correct intentional ones. According to Dennett a man trained to push a button in selecting the longer of two displays would be immune to the Müller-Lyer arrowhead illusion. This would be the wrong conclusion, while an intentional description, in accordance with our belief that the man will select[18] the display that *seems* longer, will be the right one.

Dennett's criticism would be more significant here if it were not the case that behavioristic research can and does assume that there are boundary conditions to learning phenomena. Both the behaviorist and the intentionalist might make the same predictions here but both on the same ultimate basis, past experience, or knowledge of past experience, with subjects and the Müller-Lyer arrowhead illusion. The Müller-Lyer arrowhead illusion, like all optical illusions, is an anomaly in psychology, and it is not clear how Dennett's comment here is meant to apply to operant conditioning in general.

Dennett summarizes the situation by stating that Skinner's experimental design is supposed to eliminate the

intentional but it merely masks it'.[19] But what does this mean? What is the intentional to be eliminated from? Skinner does not deny that there are thoughts, feelings, emotions and intentions. He excludes reference to them in *his explanation.* Whether or not this makes his explanation inadequate depends on what those explanations are meant to do. (And whether or not Skinner's criticism of intentional explanation is valid is a separate issue.) I have argued that behavioristic explanation is often appropriate given the right context and the right question and that it is only having seen this that we can understand the logic of that explanation. Dennett has not addressed this issue and consequently can criticise only the caricature of Skinnerian behaviorism, and this is not a significant criticism at all.

CHAPTER 7

COMPATIBILITY AND APPROPRIATENESS

I have argued that behavioristic and commonsense, intentional accounts of action are often compatible because of differences in context dependency. This is at least the case where these accounts are *restricted*, i.e. where intentional explanations are limited to explication of the state of the organism and its orientation to the environment prior to and at the time of the action. And where, in the case of human action, reasons, wants, beliefs and intentions are simply appealed to in rationalizing action. While at the same time we have restricted our discussion of behavioristic accounts to descriptions of the functional relationships or patterns of interaction between environmental variables and behavior. Here particular actions are found as instances in the pattern of behavior or behavior change being examined. I want now to show how extension of this analysis helps to elucidate some confusion about compatibility and controversy with respect to behaviorism and the explanation of action.

A. Goldman: Compatibility and a Causal Account of Intentional Action

In chapter five of *A Theory of Human Action* Alvin Goldman has argued that in so far as commonsense explanation of action

entails an account of wants and beliefs as causes of action, it is compatible with Skinnerian behaviorism. This is however suppositional, for he doubts that 'a system such as Skinner's, as it stands at present, will actually succeed in accounting for all or even a significant portion of human behavior'.[1] Goldman's argument for compatibility is worth examining because its inadequacy indicates the value of the kind of analysis I have tried to develop. It is inadequate because he does not distinguish between two different questions of compatibility and because he has not addressed the issues of explanatory adequacy and context dependency.

Goldman argues specifically for the compatibility of what he refers to as 'wants-and-belief' explanation with Skinnerian behaviorism (Skinner's position he classifies as stimulus-response psychology with no attention to Skinner's claim to a distinction between his position and stimulus-response behaviorism). With respect to those events we ordinarily think of as human action he asks: "Would a Skinnerian explanation of such events rule out a want-and-belief explanation?"[2]

He notes that in everyday discussions particular purposive explanations are challenged, but not 'on the grounds that there are no such things as reasons, wants, or purposes'.[3] He then offers comments of J. B. Watson and B. F. Skinner as representative of the behaviorist's rejection of

want-and-belief explanation. Watson argues that consciousness and related concepts are subjective, indefinite and unusable in science. And Skinner says that the fictional nature of 'inner causes' and their lack of observability make them too readily available for ad hoc explanation which cannot be dealt with in science.[4]

Though Goldman asserts that his purpose is "to examine the extent to which scientific *explanations* are compatible or incompatible with purposive *explanations* of behavior,"[5] it is important to note that his analysis of this question is ontological and not primarily concerned with either epistemology or the logic of explanation. He tries to make clear that behavioristic claims do not entail a picture of the causal chains in which we find action such that there is no room for wants-and-beliefs as *causes* of action. The most plausible analysis, he claims, is that if the behavioristic account *were* successful then it would be the case that there exist causal chains in which 'Skinnerian events' cause wants-and-beliefs which in turn cause actions.

Two Questions of Compatibility

Now there are at least two general questions of compatibility here, one ontological and the other concerned with the logic of explanation. Goldman addresses the first one directly, and the second only implicitly, if at all. The

two questions may be distinguished as follows:

First, the *ontological question*: can both the following be true?

(a) Physiological functioning and interaction with the environment (e.g. conditioning) cause action;

and (b) Having certain wants, beliefs (and abilities) *cause* action.

Second, the *logical-explanatory question*: can both the following be true?

(a) Human action can be adequately explained *without* reference to wants, beliefs, and purpose (e.g. it can be explained in terms of a history of conditioning);

and (b) Human action can be adequately explained by reference to wants, beliefs, and purpose.

It is not clear that compatibility in the logical explanatory sense either follows from, or is dependent upon, a positive answer to the ontological question. At issue here will be criteria of adequacy in the different types of explanation, causal, intentional and behavioristic; and just what is entailed by a claim of compatibility. These points and the significance of the distinction between the two questions of compatibility will be brought out in a further examination of Goldman's analysis.

Goldman first presents an argument for his causal chain analysis as a reasonable possibility. One can fit wants and beliefs into an operant conditioning analysis, and this, according to Goldman, is compatible with the analysis of behaviorists, Miller and Dollard, who make much use of the concepts of 'drive' and 'cue'. According to Goldman however, the most difficult problem here is that there is another possibility and that is that "Skinnerian events may directly cause behavior and at the same time incidentally cause wants and beliefs."[6] He thinks that both possibilities match the topography of the subject matter but his causal chain analysis is the most plausible. It is this highly evident plausibility which is the main evidence for compatibility.

In making the case for plausibility of the causal chain analysis he puts forward a number of questionable arguments. The first, as he admits, is obviously question-begging and that is to *define* 'action' as behavior caused by wants and beliefs. The second argument reads as follows: "since it is part of the very notions of wanting that wants tend to cause acts it would be very surprising should there be large numbers of wants none of which cause acts."[7] But as I hope to show, this is *without considerable qualification very tenuous* and not substantial enough to support the compatibility claims.

One can argue that acting and wanting are often *found together*, and that specific acts often *follow* specific

desires, one might also accept a distinction between say standing wants (or beliefs) and occurrent wants (or beliefs). But clearly wanting is not generally causally *sufficient* for action. We often experience wants appropriate to certain actions and the actions do not follow. It might be 'very surprising should there be large numbers of wants none of which cause any acts', but still this might well be the case.

Sometimes we are said to engage in certain actions *without wanting* to do so, e.g. unconsciously or out of habit. It may be argued that such cases are not cases of action, but, however this question is resolved, appropriate wants and beliefs appear not to be *necessary* in bringing about a type of behavior which we would sometimes classify as action. My point here is simply that Goldman's claim that "it is part of the very notion of wanting that wants tend to cause acts" does not tell us much about the purported causal relation.

His two claims, he notes, "depend very heavily on the specific explications (he) has given of the terms 'act' and 'want'"[8] and they might well be challenged or denied. (They also depend very heavily on the meaning of "tend" in the claim "wants tend to cause acts." This is discussed below.) However, he argues that if we "consider it a purely empirical matter . . . whether wants and beliefs are causally relevant to behavior,"[9] and then simply look at what goes on, our basic intuitions and understanding will show that wants and

beliefs *do cause* action. This approach will also show that the supposition that operant conditioning causes behavior *directly* is absurd.[10]

Goldman's argument here is dependent on his analysis of wants, beliefs and action in chapter three and four of the same text. A brief examination of some points in that analysis may help to clarify his position. He notes that, "no precise, predictively adequate law is known that correlates wants and beliefs with the performance of acts" but such is not necessary "to justify the statement that wants and beliefs *cause* acts" for "most of our knowledge of singular causal propositions is not based on knowledge of precise universal laws."[11] The explanation of action, he claims, is a case in point and "we certainly know that wants and beliefs result in acts."[12] But there is, according to Goldman, an important limit to this sort of knowing and that is that "although we can say *ex post facto* that certain desires and beliefs caused act A, we could not have predicted that that act would be performed."[13]

Goldman's analysis from this point on proceeds from this position which is little more than an assumption or intuition that wants and beliefs 'cause' acts. He then goes on to explicate what follows from this position for the understanding of action.

In chapter four he takes up the question of the logical status of the want-act relationship and argues that the logical relationship does not preclude a causal relationship. In fact it *ensures* it. This is because "the concept of an occurrent want is the concept of a mental event that tends to result in behavior," and "(it) is, therefore, a logical truth that wants tend to cause action." However given the difficulty of making sharp analytic/synthetic distinctions he would not claim that "the tendency to cause acts is *unquestionably* tied to the notion of a want."[14] But, he states "it is instructive to adopt this assumption," that it is a *logical* truth, "in order to make sure that its being a logical truth is not incompatible with the claim that wants cause acts."[15] (He offers as support for this position what he sees as analogous situations with magnets and acids and their properties of attraction and turning litmus paper red. "It is a logical truth that anything which is an acid has a tendency to cause litmus paper to turn red."[16] In conclusion he notes:

> There are many occasions on which an action-want does not cause any action at all because some stronger want is present which 'overrides' the former one. We may say then, that wants have a tendency to cause acts, but we cannot say that every want causes some act or other.[17]

Goldman's analysis then is dependent upon at least two superficially analysed and tenuous claims: first, that wants do cause actions and second that this in turn is a logical

truth; on these assumptions he demonstrates the meaningfulness of a causal account of intentional action. There need be no strong objection to this tack provided one does not lose sight of just where the analysis began.

This, however, does seem to be a problem for Goldman's discussion of the compatibility issue, for that problem was a problem about the compatibility of *the explanations*, and in scientific explanation, the sort of explanation the behaviorist is aiming for, universal predictive laws (and the methodology of establishing such laws) play a central role. There, 'causal' relationships are defined, in part, by reference to such laws, while Goldman's analysis, of the 'causal' relations in question, asserts that no such reference is necessary. But surely some further analysis is required if we are to answer the compatibility question he has raised.

Goldman has a partial answer to this objection but one that I hope to show is not satisfactory:

> The difficult question of course, is: precisely what is this "characteristic" mode of causation by which wants and beliefs cause intentional action? To this question, I confess, I do not have a fully detailed answer. But neither do I think it is incumbent on me, *qua* philosopher, to give an answer to this question. A complete explanation of how wants and beliefs lead to intentional acts would require extensive neurophysiological information, and I do not think it fair to demand of a *philosophical* analysis that it provide this information. I think we are aware, intuitively, of a characteristic manner in which desires and beliefs flow into intentional acts. Certainly we can "feel" a difference between a voluntary movement and an involuntary one, and this feeling, I think is symptomatic of certain causal processes. But a detailed delineation of the causal process that is characteristic of intentional action is a problem mainly for the special sciences.[18]

Now two things can be said immediately in response to this position. First, so far as it is correct it may well indicate limits for purely philosophical analysis that Goldman has not taken into account. And second, that it is not incumbent upon Goldman to engage in neurological research does not save him from the need to give a more detailed conditional account of the purported causal relationships that his analysis rests upon. These points will now be expanded in the explication of some comments of Donald Davidson's in his article "Freedom to Act."[19]

Davidson has outlined some *qualifications* to the kind of causal analysis of intentional action that Goldman, and Davidson himself, have presented. He does not, though, believe that these qualifications deny a basic claim that both he and Goldman agree upon, viz. that wants and beliefs do cause actions. His argument here appears to be based on the distinction he makes "between knowing there is a law 'covering' two events and knowing what that law is."[20] The qualifications Davidson offers in "Freedom to Act" do seem to undercut a causal analysis of the relationship of wants and beliefs to action if part of what we mean by "causal analysis" is delineating appropriate covering laws. However, according to Davidson, to assert the truth of a singular causal statement is only to suppose there is such a covering law and not to know what that law is.[21]

It is not clear that Davidson is correct on this point, at least not with respect to singular causal statements *in a scientific context*. For often the problem there is to identify 'the cause' from a number of post hoc ergo propter hoc claims; and it is in part by establishment of, or appeal to, a covering law that the 'cause' is identified. On Davidson's account it is not clear what criteria, beyond intuition, exist to confirm the identification of a causal relation in situations where singular causal statements are made without reference to covering laws. However the qualifications he has presented in "Freedom to Act" provide valuable insights into the problems of giving causal, intentional explanation. Let us consider three of his conclusions in this regard, then offer some explanation of them, and finally show their connection with Goldman's causal analysis of compatibility.

> These things, then stand together: a law stating conditions under which agents perform intentional actions; an analysis of freedom to act that makes it a causal power; and a causal analysis of intentional action. They stand together, or they fall together. In my opinion, they fall together if what we want are explicit, non-questionbegging analysis, or laws without generous caveats and *ceteris paribus* clauses.[22]

> Unavoidable mention of causality is a cloak for ignorance; we must appeal to the notion of cause when we lack detailed and accurate laws. In the analysis of action, mention of causality takes up some of the slack between analysis and science.[23]

With respect to the possibility of a law of intentional action he says:

> If the terms of the antecedent conditions were to remain mentalistic (for example) by using such concepts as those of belief

> and desire, the law would continue to seem constitutive or analytic. But the attempt to be more accurate, to eliminate appeal to questionbegging notions like 'overriding' desires, would, I think, suggest that a serious law would have to state the antecedent conditions in physical, or at least behavioristic terms.[24]

A full explication of Davidson's arguments for these conclusions would be out of place here, but the following considerations seem most relevant to our concern: According to Davidson, a causal analysis of freedom to act entails finding states or events which are causal conditions of *intentional actions*, but which are not themselves actions or events about which the question whether the agent can perform them can intelligibly be raised. Beliefs and desires of an agent that rationalize an action are the best candidates here. This in turn requires an analysis of intentional action which yields criteria for identification of action:

> To say when an agent is free to perform an action intentionally (i.e. with a certain intention) is to state conditions under which he would perform the action; to explain the performance of an action with a certain intention is to say the conditions are satisfied. Of course, this style of analysis and explanation works only if the satisfaction of the conditions (which we are assuming are causal) *always* lead to the performance.[25]

Davidson now considers three attempts to provide the appropriate law or analysis, all of which run into difficulty. In reference to Paul Churchland's proposed law in "The Logical Character of Action Explanations":[26]

> *If* A wants Ø and believes X-ing is a way to bring about Ø and that there is no better way to bring about Ø, and A has no overriding want, and knows how to X, and is able to X, *then* A x's.

he says:

> The notion of 'overriding want' is treacherous, since it is unclear how a want is shown to be overriding except by the fact that it overrides. The more general difficulty, however is this: all the conditions may be satisfied, and yet they fail to ignite an action.[27]

He sees a similar problem with Goldman's analysis. He first considers Goldman's distinction between act types and particular actions and argues that this does not allow him to get around the difficulty here:

> The trouble is that wanting to do something of type x even if x-ing is completely in his power, and he knows it, may not cause an agent to do anything of type x. And a worse trouble is that wanting to do something of type x may cause someone to do something of type x, and yet the causal chain may operate in such a manner that the act is not intentional.[28]

The problem for all such analyses and the one they fail to cope with, according to Davidson, is just *how* wants and beliefs cause intentional action. However, even if we accept that wants and beliefs do cause actions, and that this is a claim of some substance, there may still be what he calls "wayward causal chains" by which the action is brought about. A sudden desire and realization may, for instance, so shock you into doing something that you do it before you have formed any intention, though you would have done so had things been less shocking. From this it follows that "we cannot say that if attitudes that would rationalize x cause an agent to do x, then he does x intentionally." Davidson's single example may help to make things less confusing:

> A climber wants to rid himself of the weight and danger of holding another man on a rope, and he might know that by loosening his hold on the rope he could rid himself of the weight and danger. This belief and want might so unnerve him as to cause him to loosen his hold, and yet it might be the case that he never *chose* to loosen his hold, nor did he do it intentionally.[29]

Davidson's comments help to make clear the *limitations* of the sort of intentional analysis he has been considering in which wants and beliefs are held to be causes of action. In Goldman's case, he (Goldman) has not provided an analysis of the relation of wants and beliefs to actions which shows how they might cause action, and just what 'cause' would refer to in this case; at least he has not provided an analysis which would allow us to decide whether such an account would match up with a behavioristic account. A minimal requirement here would be to give a *conditional analysis* of our common-sense notion of the causal relationships in intentional explanation. This in turn would need to be compared with a similar analysis of behavioristic explanation with its search for predictive laws. But to say as Goldman does, that wants *tend* to cause actions only masks the issue.

In our everyday conversation it may be useful and unproblematic to talks of wants and beliefs 'causing' actions, but *this* is not causal analysis. When we do engage in causal analysis it may well be problematic to do so without a clear notion of what we mean by "cause". To ignore this matter raises others which Goldman's analysis cannot meet: Why can we only say that wants *tend* to cause actions? What does it

mean to say that a want sometimes causes an action? How are we to identify a particular want-act relationship as causal? With respect to the problem of showing compatibility in a causal chain analysis it would seem that a necessary condition would be to show that 'cause' meant something like the same thing in both behavioristic and wants and beliefs explanation.

I suggested earlier that, in consideration of the question of comaptibility, Goldman has lost sight of two underlying premises that his argument is dependent on, i.e. that wants do cause actions and that this is a logical truth. We can now see how this is problematic for his compatibility thesis, not just as a result of the questionbegging nature of that analysis but also because of a significant difference in style of explanation which begins with those premises.

The behaviorists, Skinner in particular, purport to be engaged in a descriptive science seeking functional, predictive 'laws' of learning, developed on the basis of repeated observations, and singular, causal propositions which cannot be supported by laws are not to be a part of that science. Goldman's overall thesis is an analysis of what it would *mean* for singular causal propositions relating wants and beliefs to actions to hold true. His position that "although we can say *ex post facto* that certain desires and beliefs caused a particular act A, we could not have predicted that the act would be performed" is at odds with the requirements of

adequacy in behavioral psychology.

It is just *ex post facto* 'causal' analysis, in reference to non-ascertainable factors, that behaviorists insist is, by itself, inadequate explanation. Without the means of a determination of the causal factors, in large part, by reference to laws, such explanations, for the behaviorist, will be ad hoc and cannot be adequate scientific explanation.

Goldman's account of wants and beliefs as causes of action is incompatible with a behavioristic account not just because the appropriate causal chains cannot be explicitly envisioned but also because he has not resolved the issue of that account not meeting the same criteria of adequacy that the behaviorists have adopted.

It might be argued that I have tried to make too much of Goldman's thesis; that what he was trying to show *quite simply* was that *if* wants and beliefs cause action and *if* 'Skinnerian events' cause wants and beliefs *then* commonsense intentional explanations of action and behavioristic explanations are compatible. But with respect to the ontological question this claim would only be trivially correct, and with respect to the logical explanatory question of compatibility the argument is a non sequitur, because the questions of adequacy, of meaning and of specificity of question have not been met.

Appropriateness and Context-Dependency

The logical explanatory question is a meta-psychological one. It asks about the compatibility of the explanations per se, not the factors those explanations refer to. Can it be true that both behavioristic accounts and commonsense intentional accounts provide adequate explanations of action? The two claims contained here would be incompatible if some requirement of one denied the truth, validity or methodological significance of the other (or entailed such denial). We may proceed then by asking, is there something about behaviorism, i.e. something about the *research of behavioral psychologists*, that denies the truth, validity or significance of wants and beliefs explanations in general? And is there something about commonsensical explanation of action which is at odds with behavioral research?

Clearly the answer to these questions will depend to a large extent on what we mean and what we claim in the two positions. I neither want to defend all that is called commonsense nor all that is called behaviorism. My claim is that *psychological* behaviorism, the study of animal and human behavior in relation to its environment, is legitimate and compatible with commonsense intentional explanation. By 'commonsense intentional explanation' I mean the *description* of action in terms of its function and reasons (including

reference to wants and beliefs) which helps us to understand that a particular action takes place *in order to* bring about certain ends. The question of the 'causal' nature of relationships here remains in abeyance. If I see a man cutting up boards and am told he is building a bird house this explains what he is doing and no causal question is at issue.

At the same time it is my claim that psychological behaviorism need only take a methodological position with minimal *ontological* claims about the subject matter of psychology. This is the claim that whatever is going on, whatever all the factors turn out to be, we may chart changes in environment and behavior and give *an* adequate explanation of behavior based on that study and without reference to intentions and internal states.

It can be seen that sometimes the question of compatibility does not arise or at least no controversy arises, and this is where explanations are being offered to clearly different questions, i.e. when the question for instance, that the behavioristic explanation is addressed to is not of the type that is normally answered with an intentional explanation. Let us consider just two examples here: we may note an increase in the number of times each day a healthy infant cries and explain this by the increased attention the infant's mother has been giving the child when it cries and the way she ignores the child when it is calm. This 'behavioristic'

account will not be at odds with commonsense. If we find a friend changing a fan belt, he may explain his action by saying "I noticed it was badly worn and want to avoid having to deal with it tomorrow when I am off on my holiday." This is an acceptable explanation even, I think, for the behaviorist, yet no reference is made to neurophysiology, stimulus control, or learning history.

Now one might still raise questions about the adequacy or correctness of these explanations, but they *are uncontroversial*. I mean this as an empirical claim about how people carry on. In so far as this is the case, why is it so? What is it about these explanations that make them acceptable? Why does the question of compatibility not arise? A partial answer is that they are both *appropriate* answers to the puzzlement of the person seeking explanation. They provide information which fills in a gap in our understanding of a situation.

When I see a child crying *for his mother's attention* and wonder why, it is no use to tell me he is crying in order to get her attention. When the man changes the fan belt and I wonder why, I do not want, or need, to know anything about his learning history. I want to know what it is about his plans and the conditions of the fan belt that makes changing it sensible at this time.

Compatibility and Responsibility

That the explanations are appropriate is again only part of the answer. Another part is that these explanations do not raise questions of responsibility. It is in those situations where we are trying to assess responsibility or when our assumption of responsibility affects our judgements and recommendations that both controversy and in turn the question of compatibility arise.

School children are sometimes said to misbehave or not get their work done because they are lazy or have a poor attitude. A man knowingly exceeds the speed limit because he has little respect for the law, trusts his own driving abilities and has judged that it is worth the risk. A man is a success because he has intelligence, drive, and ambition. He fights for his country because he believes in freedom and justice. These comments are all, at first gloss, compatible with, and part of what we mean by, people being responsible for their actions. A behavioristic account which attempts to show that these actions are explainable as the outcome of learning history, and genetic and environmental determinants seems to deny responsibility. From this point of view the actions are not most importantly the result of agency; they are the result of what has happened to a person.

It would seem then that a behavioristic account of action is incompatible with agency and responsibility while a commonsensical intentional account is. Therefore, the two accounts cannot be compatible. This is an interesting argument and variations of it can I believe be found which vary greatly in sophistication. However, which sense of 'compatibility' will be at issue in that debate? Not, I think, the logical explanatory one, at least not most importantly. One need not take a stand on the question of responsibility before going further with analysis of the explanatory issues.

Clearly, the debate about behaviorism touches the problem of freewill and determinism, but it is *not* that problem. Determinism, according to its proponents, is just as compatible with wants and beliefs explanations as it is with behaviorism. And in so far as psychological behaviorism is a descriptive science, is non-mechanistic, is not reductionistic and is simply about behavior, it need not be incompatible with agency. As I have argued earlier, neither approach is dependent upon an explanation of all relevant causal chains. Both types of explanations, to an important degree, *stand on their own* as they fit the facts and help to make actions understandable.

P. F. Strawson, with respect to the notion of responsibility, has analysed a distinction which in a number of ways parallels to some degree the one I have made here between

intentional and behavioristic explanation.[30] This is the distinction between, on the one hand, our attitude toward others as we *participate* in normal adult relationships and, on the other hand, our *objective* attitude towards people who, for a variety of reasons, we believe are temporarily, or permanently, incapacitated in terms of their ability to participate in normal inter-personal relations, e.g. an idiot, or a man severely mentally deranged. This objective attitude is also available for consideration of normal people in normal times. It entails seeing their actions as a function of physiological or environmental factors, e.g. as studies in the social sciences. Such an attitude, he claims, may be useful in making manipulations of our environment, but it is a characteristic of human nature that it cannot be sustained. More important for our analysis here he points out that although, in particular cases, it is appropriate to adopt an objective attitude, we do not do so "as a result of theoretical conviction of the truth of determinism." It will be "for different reasons in different cases" that we abandon the "ordinary inter-personal attitudes."[31]

He claims, and I think rightly so, that our inter-personal attitudes are part of the general framework of human life. (However, he puts the case too strongly when he says that our commitment to ordinary inter-personal attitudes can not "come up for review within that general framework.") The

interesting question here is just what recommendation for adoption of ordinary inter-personal (participatory) attitudes would follow *rationally* from such a review in which determinism of some sort were assumed to be the case. Strawson's comments on this question are germane, not only to the question of inter-personal attitudes and objectivity, but also to the *rational* acceptance of social *science* in general:

> if we could imagine what we cannot have, viz. a choice in this matter, then we could choose rationally only in light of an assessment of the gains and losses to human life, its enrichment or impoverishment; and the truth or falsity of a general thesis of determinism would not *bear* on the rationality of *this* choice.[32]

Now much can be made of Strawson's comments here, but one thing that seems to follow from this limited analysis of the connection of the determinism issue to explanation of human action is that it will often be a matter of *judgement* what explanation is *appropriate*. And this will always be, to a large degree, a contextual matter in accordance with our analysis of context dependency.

Behaviorism, Determinism and Commonsense

Before completing this discussion of compatibility and appropriateness I want to try and reduce the apparent distance between behavioristic and commonsense explanation of action. First, by examination of some remarks of behavioral psychologist, Albert Bandura, and then by indicating some of the ways in which learning and environmental influence are a

part of commonsense intentional explanation.

Albert Bandura and "Reciprocal Determinism"

Albert Bandura has argued that a two-way control process operates between organism and environment, or person and environment and that the influences operate concurrently.[33] He claims that within psychological research it is an optional selection of paradigm to focus attention, (as Skinner does) on just one direction of control. However, such a focus *may* sometimes be justified for practical reasons.

In his theoretical, methodological position he has given attention to the interdependent nature of environmental and personal factors (defined as the combination of psychic state and behavior) and their 'reciprocal determination'. This is to note, in part, that while much of what we do may be accounted for as response to environment, what particular environment we are in, and the features of that environment are often a function of our behavior. We have, to some degree, created it. A similar interdependence is to be found between behavior and certain "internal personal factors."

> To take one example, people's expectations influence how they behave, and the outcomes of their behavior change their expectations.[34]

> Environments have causes, as do behaviors. It is true that behavior is regulated by its contingencies, but the contingencies are partly of a person's own making. By their actions, people play an active role in producing the reinforcing contingencies that impinge upon them.[35]

In other words (and this is what some behaviorists, Watson in particular, missed) higher organisms are engaged in *activity* in their environment and this cannot be fully captured by the concept of behavior as response. Nor can it be captured by a *simple* intentional analysis which fails to take account of the degree to which behavior or aspects of consciousness are shaped by our environment. This is not to say that a study of simple stimulus-response relationships is wrong or that simple intentional explanation is wrong. Nor is it to say that more complex intentional or behavioristic explanation is always in order. However, what follows from this, for Bandura, seems to accord with the concern developed here for context dependency, appropriateness of explanation and compatibility.

> The image of people's efficacy that emerges from psychological research depends upon which aspect of the reciprocal influence system is selected for analysis.[36]

> although the reciprocal sources of influence are separable for experimental purposes, in everyday life two-way control operates concurrently. In on going interchanges, one and the same event can thus be a stimulus, a response, or an environmental reinforcer depending on the place in the sequence at which the analysis arbitrarily begins.[37]

I do not want to defend Bandura's notion of determinism. What is important for my purpose is that he takes an essentially *explanatory* position in the face of the determinism issue, without trying to resolve the larger question of determinism, and in so doing, he suggests a kind of

behaviorism much closer to our commonsense understanding of action than that of early behaviorism or of the contemporary Skinnerian position.

Intentional Explanation, Learning and Environment

Let us reconsider commonsense explanation of action. For the sake of discussion, we said we explained actions adequately by reference to wants, beliefs, and purposes. This is however, a rather simplistic view of commonsense explanation. Commonsensically we also make reference to learning history and environmental interaction, or, we have an understanding of such as a background to explanation.

Our choices are limited or facilitated by physical, economic, legal, and social factors. They are often based on our abilities, knowledge and emotional responses which we know were acquired through experience. We often act within situations which are only partly of our own making. Reference to physical health, energy level and interaction with our social environment that results in stress, irritation or pleasure are often part of our commonsensical explanation.

Thus we may say that someone has gone to bed or has decided to do so, because they are tired and they are tired because they slept little last night and worked hard today. We do not mean, then, that the act of going to bed is non-

intentional, that the person had no choice, or that his *belief* about beds and his *wanting* to go to bed are irrelevant to his action. But clearly environmental determinants and the person's *learning* to go to bed when he is tired are important to our commonsense understanding. Just, as I argued in chapter three, psychological behaviorism has had to deal with a class of behaviors whose defining features were their intentional characteristics, so also, commonsense intentional explanation cannot ignore environmental influences in giving a full account of action.

I said earlier that behavioristic and commonsense intentional explanation would only be incompatible if some requirement of one denied the truth, validity or methodological significance of the other. In our discussion, two major points arise relative to this problem. The first is that, *in general*, the two types of explanation are compatible; the second is that in particular cases what appears to be a question of compatibility is really a matter of appropriateness.

For any action either a behavioristic or an intentional explanation may be given, at least in theory. However, if we start by defining intentional explanation as that which denies the significance of environmental influences or if we mean by behavioristic explanation that which denies that the intentional state of the organism, or person, is important in determining action, then we do have a case of incompatibility.

But as I have indicated, neither type of explanation *needs* to make these claims in order to get on with either intentional explanation or behavioral research.

The importance of the notion of 'appropriateness' has arisen from three findings which were brought out at the beginning of this analysis. First, the failure to develop a defining, logical model of what scientific explanation *must* be like was also the discovery that judgement about adequacy of explanation is always, in part, an empirical matter. One has to go out and see if, and in what way, the explanation of an event does explain it. This requires being very clear as to the nature of the event to be explained and to the background of conditions in which we find the explanation. Second, our discussion of target relevancy, in the explanation of an event, suggests that, while an explanation may be about that event, correct and genuinely explanatory, it may be wrong because it is not addressed to our specific concern, e.g. we may be concerned about meaning, or about function, or about etiology. Third, psychology is a practical matter. In so far as it is a science and psychologists are attempting to get *scientific results* (along with 'scientific' speculations) such as prediction and control of phenomena, factor analysis and accurate measurement of variables, then explanations and theoretical positions must be judged, in part, by their *usefulness* in getting those results. Thus the two types of

explanations, while in general may be compatible, in particular cases may *not* both be *appropriate*. And this may, without close attention, look like incompatibility. Sometimes one or the other will be useful while the other is not, i.e. will provide the information required or provide it most efficiently. (Consider my fan belt example where learning history is not to the point.) Commonsense explanations may often be inappropriate because they cannot be tested in a scientific fashion, but then, and I think this is a point often missed, scientific explanations are also sometimes inappropriate because for practical reasons they cannot be applied or used in the context of everyday life. At other times, one may want to know a person's intentions, desires or relevant beliefs. It is just this sort of information which *will* explain his action. I may, however, want to know how it came to be that he generally acts *this way* in these situations and I may want to know what has *happened to him* such that he behaves this way. Here a behavioristic account may explain his action, but in either case the explanation will be limited.

Skinner claims that one reason for the rejection of intentional accounts is that they are too available for ad hoc explanation. But surely operant analysis is problematic in a similar way. Daniel Dennett has argued this point at some length. If Skinner's operant analysis *means* that every case of *action*, as distinct from mere response, is a case of

operant conditioning then those cases where we do not find operant conditioning but do find action only show the shortcomings of our research, not our analysis:

> When Skinner speculates about the past history of reinforcement in a person in order to explain some current behavior, he is saying in effect, "I don't know which of many possible equivalent series of events occurred, but one of them did, and that explains the occurrence of this behavior now." But what is the equivalence class Skinner is pointing to in every case?"
>
> In the end Skinner is playing the same game with his speculations as the cognitivist who speculates about internal representations of information.[38]

Such 'explanations' are of course ad hoc or question-begging. This does not mean that the explanation *type* is therefore wrong, and in this case unscientific. In these situations we would have been better to say *reference* to operant conditioning is ad hoc. The explanation type is being *misused*.

Similarly when appeals are made to intention, wants and beliefs in explaining action such explanations will in some situations not be useful and not be scientifically confirmable. But again this is not because there is something wrong with that explanation *type*. It does have its role. What is wrong is the use of that particular type when it cannot possibly tell us what we want to know.

When is an explanation type being properly used? When do the facts fit the model? Which approach provides the correct methodology to ascertain the information we want?

One can sketch out areas of concern here but it would appear that we will very often have to answer these questions only as we proceed to deal with particular problems of action in psychology and in everyday life. This will always be in large part, a matter of judgement.

NOTES

Chapter 1

[1]For an outline of philosophical behaviorism see: Arnold S. Kaufman, "Behaviorism," in *The Encyclopedia of Learning*, edited by Paul Edwards (New York: Macmillan, 1967), pp. 268-273.

[2]See Howard Rachlin, *Introduction to Modern Behaviorism*, 2nd edition (San Francisco: W. H. Freeman, 1976).

[3]Roger Fellows and R. Lindley, *What Philosophy Does* (London: Open Books, 1978), p. 72.

[4]Noam Chomsky, "Psychology and Ideology," in *The Philosophy of Society*, edited by R. Beehler and A. Drengson (London: Methuen, 1978), p. 111. In this article Chomsky criticizes B. Skinner's "Speculations on Human Behavior," but notes that these "are to be clearly distinguished from his experimental investigation of operant conditioning."

[5]Sigmund Koch, "Psychology and Emerging Conceptions of Knowledge as Unitary," in *Behaviorism and Phenomenology*, edited by T. W. Wann (Chicago/London: University of Chicago Press, 1964).

[6]Ibid, p. 9.

[7]Ibid., pp. 9-15.

[7a]Ibid., p. 8-9

[8]Ibid., p. 9.

[9]Ibid., p. 10.

[10]Ibid., p. 11.

[11]Ibid., p. 13.

[12]Ibid., p. 5.

[13]Charles Taylor, *The Explanation of Behavior* (London: Routledge and Kegan Paul, 1964), p. 3.

[14]Ibid., p. 5.

[15]Ibid.

[16]Charles Taylor, "The Explanation of Purposive Behavior," in *Explanation in the Behavioral Sciences*, edited by R. Borger and F. Cioffi (Cambridge: Cambridge University Press, 1970), p. 55.

[17]Ibid., p. 50.

[18]Ibid., pp. 54-62.

[19]Ibid., p. 58.

[20]Ibid., p. 59.

[21]Ibid., p. 60.

[22]Ibid., p. 61.

[23]Ibid., pp. 61-62.

[24]Ibid., pp. 62-66.

[25]D. W. Hamlyn, "Conditioning and Behavior," in *Explanation in the Behavioral Sciences*, edited by R. Borger and F. Cioffi (Cambridge: Cambridge University Press, 1970).

[26]Ibid., p. 139.

[27]Ibid., pp. 141-143.

[28]Ibid., p. 144.

[29]Ibid., p. 142.

[30]Hamlyn, p. 146.

[31]Ibid., p. 147.

Chapter 2

[1]For a similar position relative to sociology, see Alan Garfinkel, *Forms of Explanation* (New Haven/London: Yale University Press, 1981), Chap. 1 and Introduction.

[2]See C. G. Hempel, *Aspects of Scientific Explanation, and Other Essays in the Philosophy of Science* (New York: Free Press, 1965), especially pp. 297-303 for a concise outline of Hempel's views of nomological explanation.

[3]J. L. Mackie, "Causes and Conditions," *American Philosophical Quarterly*, Vol. 2, No. 4 (Oct. 1965), 245.

[4]John Anderson, "The Problem of Causality," in *Studies in Empirical Philosophy*.

[5]Mackie, p. 249.

[6]See Hempel, p. 297 and Peter Achinstein, "Can There Be a Model of Explanation?", *Theory and Decision*, Vol. 13, No. 3 (Sept. 1981), 201-227.

[7]Achinstein, p. 215.

[8]B. A. Brody, "Towards an Aristotlelian Theory of Scientific Explanation," *Philosophy of Science*, Vol. 39 (1972), 27, cited by Achinstein, ibid., p. 221.

[9]C. G. Morgan, "Archaeology and Explanation," *World Archaeology*, Vol. 4, No. 3 (Feb. 1973), 265.

[10]Ibid.

[11]Achinstein, p. 212.

Chapter 3

[1]Anthony Kenny, *Action, Emotion and Will* (London: Routledge and Kegan Paul, 1963), p. 184.

[2]Descartes, cited in R. J. Herrnstein and E. G. Boring (editors), *A Source Book in the History of Psychology* (Cambridge, Mass.: Harvard University Press, 1965), pp. 266-272 and Rachlin, Chap. 1.

[3]Rachlin, p. 7.

[4]Ibid., p. 11.

[5]See the selections in Herrnstein and Boring, especially "George Berkeley on Arbitrary Connection Among Ideas, 1733," p. 340; and "John Locke, on Disorders of the Mind, 1700," from an essay concerning Humane Understanding.

[6]Rachlin, pp. 26-28.

[7]Ibid., p. 26.

[8]Ibid., p. 32.

[9]See Erwin A. Esper, *A History of Psychology* (Philadelphia: W. B. Saunders, 1964). Espir has argued that Sechnov's concerns with psychology were strongly influenced by the British Empiricists. He points out that as a medical student Sechnov "spent much time on the works of the German philosopher Friedrich Edvard Beneke (1798-1854), who played an important role in introducing British empiricism and associationism into German psychology" (p. 318). He also quotes Sechnov directly from "Reflexes of the Brain" where he makes reference to Beneke as his source of understanding of systematic psychology. (Though I find no reference to Sechnov in the work of Watson or other early behaviorists, he is clearly important by way of his influence on Pavlov who was a significant contributor to behaviorism, although perhaps not by desire.)

[10]Rachlin, p. 35.

[11]Ibid.

[12]I. M. Sechenov, "Reflexes of the Brain," pp. 264-323, cited in Herrnstein and Boring (editors), p. 308.

[13]Ibid., p. 318.

[14]Ibid., p. 321.

[15]Sechenov's account is mechanistic. It presents a picture of a hard-wired system with no suggestion of how patterns of response changed, or how complex responses may be learned. Esper, in noting similarities between Sechenov's approach to psychology and that of the early behaviorists, states that both are continuous with 19th century positivism: "they are both philosophical essays rather than analysis related to past and projected research on the behavior of human beings of which of course there was little or none to the purpose" (Esper, p. 325).

Just as for Sechenov success in applying objective methods to exploration and conformation of theory in physiology led him to speculate about the extension of such an approach to voluntary behavior, so later, Watson's success in applying objective methods in studying functional relationships between environmental changes and animal behavior, led him to speculate about the extension of the approach to human action.

[16]I. P. Pavlov, "On Conditioned Reflexes," cited in Herrnstein and Boring (editors), p. 564.

[17]See Rachlin's discussion of 'Molarism' in psychology, p. 39.

[18]Ibid., p. 54.

[19]Ibid., p. 54.

Chapter 4

[1]See Rachlin for a brief discussion of early experimental psychologists whose methodology was to a large extent introspective (e.g. E. H. Weber, Johannes Müller and Herman Ebbinghaus), pp. 14-24.

[2]J. B. Watson, *Behaviorism* (New York: W. W. Norton, 1924), p. 3.

[3]J. B. Watson, "Psychology as the Behaviorist Views it," *Psychological Review*, Vol. 20 (1913), 159 and 167.

[4]I. P. Pavlov, "Lectures on Conditioned Reflexes," in *History of Psychology, A Source Book in Systematic Psychology*, edited by W. S. Sahakian (Itasca: F. E. Peacock, 1968), p. 444.

[5]Herrnstein and Boring (editors), p. 534.

[6]B. F. Skinner, *The Behavior of Organisms* (New York: Appleton-Century-Crofts, 1938), p. 5.

[7]Taylor, *The Explanation of Behavior*, p. 112.

[8]Taylor, ibid., p. 114, citing Hull, *Principles of Behavior*, pp. 25-26.

[9]R. F. Kitchener, "Behavior and Behaviorism," *Behaviorism* (Fall 1977), pp. 11-71.

[10]Watson, *Behaviorism*, p. 7.

[11]Kitchener, p. 14.

[12]See Donald A. Dewsbury, *Comparative Animal Behavior* (McGraw-Hill Book Company, 1978), p. 14.

[13]Kitchener, citing W. S. Hunter, "The Psychological Study of Behavior," *Psychological Review*, Vol. 39, 1-24.

[14]Kitchener, p. 24.

[15]Ibid., p. 25.

[16]Ibid., p. 25, citing E. Holt, "Response and Cognition," *Journal of Philosophy*, Vol. 12 (1915), 371.

[17]R. B. Perry, "Docility and Purpose," *The Psychological Review*, Vol. 25, No. 1 (Jan. 1918), See also Kitchener's discussion of Perry's work.

[17a]Andrew Woodfield, *Teleology* (Cambridge: Cambridge University Press, 1976), p. 201. Woodfield has made similar claims about our understanding of the concept of teleology as it has grown to cover developments in science and technology. In his study of teleology he says:

> In order to analyse our present notion of goal directed behavior it was found necessary to postulate a mentalistic core-concept, and to hypothesize that our present concept, which applies to machines as well as to animals, was derived from it by analogical extension. . . . The class of goal-directed systems is not, however, a class of equals. The members of it can be arranged in a rough sort of order according to their degree of sophistication.

[18]Dewsbury, p. 191.

[19]E. C. Tolman, *Behavior and Psychological Man, Essays in Motivation and Learning* (Berkeley/Los Angeles: University of California Press, 1961; first pub. 1951), p. 36, in his essay "Behaviorism and Purpose" first published in *The Journal of Philosophy* (Jan. 15, 1925).

[20]Ibid., p. 33.

[21]Ibid., p. 36.

[22]Ibid., p. 35.

[23]Ibid., p. 47, in the essay "Purpose and Cognition," first published in *The Psychological Review* (July 1925).

[24]E. R. Guthrie, *The Psychology of Learning*, revised edition (Glouchester, Mass.: Peter Smith, 1960), p. 7.

[25]Ibid., p. 28.

[26]E. R. Guthrie, *The Psychology of Human Conflict* (New York: Peter Smith, 1950), p. 33.

[27]E. R. Hilgard and G. H. Bower, *Theories of Learning*, 3rd edition (New York: Appleton-Century-Crofts, 1960), p. 146.

[28]Ibid., chapter 6.

[29]B. F. Skinner, "Are Theories of Learning Necessary?" *The Psychological Review*, Vol. 57 (1950), 192-216; reprinted in *Cumulative Record* (New York: Appleton-Century-Crofts, 1959), p. 41.

[30]Ibid.

Chapter 5

[1]Skinner, *The Behavior of Organisms*, p. 19.

[2]Ibid., p. 20.

[3]Ibid.

[4]Ibid., p. 6.

[5]B. F. Skinner, *About Behaviorism* (New York: Alfred A. Knopf, 1974), p. 55.

[6]Finley Carpenter, *The Skinner Primer* (New York: The Free Press, 1974), p. 5.

[7]This outline of operant conditioning is based most directly on the descriptions in Rachlin, *Introduction to Modern Behaviorism*, Chapter 3, and Dewsbury, *Comparative Animal Behavior*, Chapter 16.

[8]See Rachlin, Chapter 4, and Dewsbury, Chapters 16 and 17.

[9]Dewsbury, p. 325.

[10]I have dealt only with the behaviorists' *general* concern for classical and operant *conditioning*. It should be pointed out that behaviorists do concern themselves with phenomena which are better characterized in other ways. There is also considerable difference of opinion and concern among behaviorists over a variety of issues. Behaviorists have, for instance, been concerned with the following phenomena: imitation and modelling in social learning (A. Bandura), personality (H. J. Eysenck) and species difference in learning (see Rachlin, 1970).

Chapter 6

[1]Koch, p. 29.

[2]Taylor, *The Explanation of Behavior*, p. 86.

[3]Ibid., p. 87.

[4]Ibid., p. 95.

[5]Taylor, "The Explanation of Purposive Behavior," p. 65.

[6]Ibid., p. 66.

[7]Hamlyn, p. 139.

[8]Skinner, *Cumulative Record*, p. 31.

[9]Norman Malcolm, "Behaviorism as a Philosophy of Psychology," in *Behaviorism and Phenomenology*, edited by T. W. Wann (Chicago: University of Chicago Press, 1964), pp. 141-162.

[10]Note, however, that according to Malcolm, behaviorism *is* reductionistic and its main fallacy arises from the non-reducibility of psychological sentences in the first-person present-tense. But Carnap's reductionism concerns the *reference* of psychological terms. This need not be an issue for the psychological behaviorist at all. He simply restricts himself to talking about *behavior* per se, some of which will be the verbal behavior, utterances of first-person present-tense psycholgical sentences. This points to the way in which behaviorism is limited. (For further discussion of this debate see Malcolm and also W. F. Day, "On Skinner's Treatment of the First-Person, Third-Person Psychological Sentence Distinction," *Behaviorism*, Vol. 5, No. 1 (1977), 33-37.

[11]Daniel C. Dennett, *Brainstorms, Philosophical Essays on Mind and Psychology* (Bradford Books, 1978), p. 3.

[12]Ibid., p. 15.

[13]Skinner, "A Case of History in Scientific Method," in *Cumulative Record*, p. 80.

[14]Ibid., passim.

[15]Dennett, p. 14.

[16]Ibid.

[17]Ibid.

[18]Ibid., p. 15.

[19]Ibid., p. 15.

Chapter 7

[1]Alvin I. Goldman, *A Theory of Human Action* (Princeton: Princeton University Press, 1976; first published 1970), p. 157.

[2]Ibid.

[3]Ibid., p. 126.

[4]Ibid., pp. 127 and 128.

[5]Ibid., p. 129.

[6]Ibid., p. 153.

[7]Ibid.

[8]Ibid., p. 154.

[9]Ibid.

[10]Ibid.

[11]Ibid., p. 72.

[12]Ibid., p. 73.

[13]Ibid.

[14]Ibid., p. 112.

[15]Ibid., p. 113.

[16]Ibid.

[17]Ibid.

[18]Ibid., p. 62.

[19]Donald Davidson, *Essays on Actions and Events* (Oxford: Clarendon Press, 1980), p. 62.

[20]Ibid., p. 160; Chapter 7, "Causal Relations (1967).

[21]Ibid., p. 18, Chapter 1, "Actions, Reasons and Causes" (1963).

[22]Ibid, p. 76.

[23]Ibid., p. 80.

[24]Ibid. Note: Davidson goes on to say "the attempt to find such a law would also end in frustration." However, he does not discuss the matter in this essay. For further discussion see "Mental Events" and "Psychology as Philosophy" in this book.

[25]Ibid., p. 76.

[26]Paul Churchland, "The Logical Character of Action Explanation," *Philosophical Review*, Vol. 79 (1970), 214-236, cited by Davidson, p. 77.

[27]Davidson, p. 77.

[28]Ibid., p. 78.

[29]Ibid., p. 79.

[30]P. F. Strawson, *Freedom and Resentment and Other Essays* (London: Methuen, 1974), pp. 6-13.

[31]Ibid., p. 12.

[32]Ibid., p. 13.

[33]Albert Bandura, *Social Learning Theory* (Englewood Cliffs, N.J.: Prentice-Hall, 1977), Chapter 6.

[34]Ibid., p. 195.

[35]Ibid., p. 203.

[36]Ibid.

[37]Ibid., p. 204.

[38]Dennett, p. 70.

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