Psychological Paradigms and Behavior Modification[1]

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Abstract

The application of Kuhn's concept of "incommensurable paradigms" to the science of psychology is discussed. Two such paradigms, the behaviorist or behavior analytic paradigm and the cognitive/mentalist paradigm, are distinguished. It is suggested that the choice of paradigm will depend on the method of behavior modification to be employed. If behavior is to be modified by stimulus control and contingency management, a version of the behaviorist paradigm will be selected. If behavior is to be modified by changing the individual's self-directed verbal behavior, the mentalist/cognitive paradigm is to be preferred.

In his book *The Structure of Scientific Revolutions*, T. S. Kuhn (1962) has introduced the notion of a "paradigm" as an explanatory concept in the interpretation of the history of science. A paradigm, as Kuhn conceives it, is a coherent tradition of scientific theory, practice, research and application which pervades and makes sense of all the scientific activities of a particular scientific community within a given scientific discipline over a particular stretch of time. It is not to be confused with a particular scientific hypothesis or theory which is open to empirical confirmation or disconfirmation; it is rather the basic framework of fundamental concepts and unquestioned assumptions within and in terms of which the problems of scientific research are stated.

As Kuhn conceives it, all the day to day work and research of what he calls "normal science" is conducted entirely within the framework of a particular scientific paradigm. Nevertheless we frequently encounter different groups of workers within what is nominally the same scientific discipline, who are using quite different and apparently incompatible paradigms at one and the same time. Moreover, when what Kuhn calls "a scientific revolution" occurs, the whole scientific community constituted by the workers within a given discipline will abandon a previously dominant paradigm, or a number of pre-existing rival paradigms, in favour of a new and supposedly better paradigm, which provides the basis for normal science until such time as a further scientific revolution takes place, whereby it is in its turn superseded by yet another paradigm.

Now when two or more rival paradigms co-exist within a single scientific discipline, these paradigms are said by Kuhn to be "incommensurable" one with another. Kuhn explains what he means by this in the following passage: "The proponents of competing paradigms are always at least slightly at cross purposes. Neither side will grant all the non-empirical assumptions that the other needs in order to make its case. Like Proust and Berthollet arguing about the composition of chemical compounds, they are bound partly to talk through each other. Though each may hope to convert the other to his way of seeing his science and its

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problems, neither may hope to prove his case. The competition between paradigms is not the sort of battle that can be resolved by proofs." (Kuhn, 1962, p. 148). Needless to say, Kuhn has made no attempt to interpret the history of psychology in these terms. Moreover it is doubtful whether, were he to do so, he would be willing to allow the existence within psychology of anything corresponding to the different paradigms to which he has drawn attention in the older and more developed sciences. For one thing there are few coherent traditions of psychological theory, research, and practice which have yet managed to survive the death of the founding father or fathers of a particular "school" and the dispersal of the group of students who originally comprised it. Then there is the remarkable ability of psychology to develop a plethora of evanescent fads and fashions, few of which manage to provide the basis for a concerted programme of "normal science" in Kuhn's sense for longer than a generation.

Nevertheless the situation which Kuhn describes in terms of the incommensurability of competing paradigms in which scientists operating within different theoretical frameworks talk at cross purposes with one another and where their differences cannot be resolved either by argument or by empirical test, is one with which psychologists are only too painfully familiar.

Perhaps the most striking example of this kind of incommensurability within psychology is to be found in the area of psychological research which, depending upon the paradigm to which you subscribe, you will call "psychotherapy", behavior therapy" or "behavior modification". The inability of those who subscribe to different paradigms to communicate effectively with one another could not be more vividly illustrated than by the fact that although behavioral engineers, behavior therapists and psychotherapists are manifestly wrestling with the same basic problems and working with much the same population of clients or patients, there is no description of the problems with which they are all concerned and no description of the population involved on which the different varieties of specialists can agree.

But while the incommensurability, the failure of communication, stares us in the face, the shape and character of the conflicting paradigms between which the failure of communication arises is more opaque. It is true that on one side of the fence, that occupied by the behavior modifiers and behavior therapists we do find a reasonably coherent tradition of scientific theory and practice with a common theoretical language. The tradition which begins with stimulus- response behaviorism and culminates in contemporary behavior analysis, despite a certain amount of fragmentation into different sub- varieties, has retained a remarkable degree of coherence and integrity, as it has evolved from the work of Thorndike, Pavlov and Watson at the beginning of the century through Guthrie, Hull and Mowrer, down to the later work of Skinner and his students. Similarly, despite the differences of attitude to the problem of individual differences and the role of statistical procedures epitomized by the approach of Sidman (1960) on the one hand and Eysenck (1953) on the other, there is a substantial measure of agreement within this tradition on matters of scientific methodology. Here, at least, we have something that is recognizable in Kuhnian terms as a paradigm.

On the psychotherapeutic side of the fence, by contrast, the situation is much more confused. It is true that in psycho-analysis we have a theoretical tradition which, despite a certain amount of ossification, has preserved its integrity over the generations that separate us from the death of Freud. Psycho-analysis in its original form, however, commands the allegiance of only a small minority of those who would call themselves psychotherapists. Here the fragmentation into different schools of thought and practice is far more marked than amongst the behaviorists. Nevertheless within this babel of different tongues, it is possible to discern two common features which distinguish the psychotherapeutic tradition as a whole from that of behavior therapy and behavioral engineering and which, in my view, may be regarded as constituting a psychotherapeutic paradigm in Kuhn's sense. These two common features which, as I shall argue, are closely connected with one another, are (a) the use of a conceptual framework which is "mentalistic" or, to use the currently fashionable jargon, "cognitive" in character and (b) the use of verbal interactions between the patient or client on the one hand and the therapist on the other or, as in the case of group psychotherapy, verbal interaction between members of the therapeutic group, as a means of modifying the verbal behavior emitted by the patient or client, when talking about him or herself and his/her life situation.

The complexity and subtlety of these verbal interactions is so great as to defeat any systematic attempt to apply the experimental and statistical methodology advocated by the behaviorists except in a very

crude and superficial way. Such attempts, moreover, are regarded by most psychotherapists with understandable suspicion. But, since they have failed to suggest any coherent alternative to, what is after all, the standard methodology of empirical science, it becomes very difficult to substantiate the claim that psychotherapy and the various theoretical formulations in terms of which it is described and interpreted constitute a body of scientific theory and practice, in the sense in which that term is understood in the biological and physical sciences.

In this paper, however, I do not propose to discuss the failure of communication which arises between behaviorists and psychotherapists on the issue of scientific method. I am concerned solely with the failure of communication which arises from differences in the kind of theoretical language that is employed by the two groups and the relationship between these theoretical languages and the techniques employed by the two groups in trying to influence, control or modify the behavior of the individual patient, client or subject.

In particular I shall argue

(1) that, whatever may be true as far as the differences in methodology are concerned, the mentalist or cognitive language in terms of which the psychotherapist construes both the problems of the patient and his or her own procedure in dealing with those problems is at least very much more like the theoretical language of a standard scientific paradigm than either its behaviorist opponents or its philosophical advocates are usually willing to admit,

(2) that it is the only feasible and convenient way of construing the determinants of behavior in a case such as psychotherapy where we are trying to influence and modify the behavior of a human being by interacting verbally with him/her,

(3) that when, as in the case of behavior modification and behavior therapy, we are trying to modify an individual's behavior through the non-dominant rather than the dominant hemisphere of the cerebral cortex, we likewise have no real alternative but to construe the determinants of behavior in terms of some version of the non- cognitive behaviorist paradigm. If such a theoretical language had not already been available for this purpose, we would have had to invent it or something very like it.

What I am suggesting is that instead of wasting time trying to convince the other party that one's own preferred paradigm is the only genuinely scientific way to construe behavior or that it is the only way of construing the human personality which does justice to the richness, subtlety and variety of its unique individuality, we should recognize that each of the two paradigms has its own proper sphere of application, and that the choice between them is a matter of the technique of behavior modification that is to be adopted rather than a matter of deciding which offers the most scientifically accurate account of or the most revealing insights into the behavior of human beings.

In arguing the case for the retention of both these two rival and incommensurable paradigms on purely practical grounds, I propose to begin with a discussion of what I am calling the "mentalist" or "cognitive" paradigm. My reason for focusing attention on this paradigm in the first instance is that, whereas the practical virtues of construing behavior in terms of the concepts and principles of behavior analysis need no argument as far as readers of this journal[2] are concerned, the case for a mentalistic or cognitive approach to the explanation of behavior has usually been argued by psychologists, as well as by philosophers, on philosophical and ethical grounds rather than in relation to the practical considerations of applied psychology or applied behavior analysis.

Another reason for concentrating in the first instance on the mentalistic in preference to the behaviorist paradigm is that mentalistic or cognitive explanations of behavior have been seriously misunderstood, not only by the behaviorists who have systematically opposed the use of explanations couched in these terms for scientific purposes, but equally by those who have advocated and defended their use.

This misunderstanding of and confusion about the mentalistic paradigm is not due to any unfamiliarity with it, on the part either of its advocates or its opponents. Indeed, there is no theoretical

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[2] Editorial note: probably this revised version of the article was not accepted by the journal to which it was sent. The name of the journal is unknown to me (TP).

language in the field of psychology with which all of us, whatever our psychological persuasion, are more familiar than we are with this way of interpreting our own and other people's behavior. For the mentalistic paradigm is nothing more or less than the body of concepts which we employ in every day life whenever we set about predicting, explaining or trying to influence the behavior of other people in ordinary non-technical contexts. It is a theoretical language in the use of which we are all highly fluent and proficient and yet about which we are able to maintain some remarkable misconceptions and delusions.

These misconceptions arise because we are misled by our own familiarity with and facility in the use of this system of explanatory concepts into overlooking the very considerable logical complexity of this particular "language game", as Wittgenstein would have called it.

The various theoretical languages employed by psychotherapists, although they incorporate many distortions and misconceptions which have been superimposed by a long and confused tradition of philosophical and psychological theorizing, all have a recognizable common origin in the psychological language of ordinary discourse and common sense. That psychotherapists should prefer to adopt a theoretical language which, however distorted, is still recognizably based upon the psychological language of everyday life is understandable when we consider that the psychotherapist is committed to influencing or modifying the patient's behavior by means of a two- way process of verbal communication. In this two-way communication process the client or patient must be able to understand the interpretations or comments which the therapist offers of his or her verbal and non-verbal behavior. Likewise, however much the therapist may suspect the client of rationalizing and repressing the real reasons for his or her behavior, the therapist must be able to incorporate the patient's own account of those reasons into the theoretical reinterpretation which he/she (the therapist) is trying to develop. If the therapist were to employ a conceptual system for interpreting the client's behavior which was radically different from that which the client him/herself employs, not only would he/she be faced with the problem of translating the theoretical interpretation into the ordinary language psychological concepts which the client understands before putting it to the client, he/she would also be faced with the problem of translating what the client says into the therapist's theoretical language before evaluating its significance in terms of that conceptual system. Consequently to adopt a theoretical language quite different from that of common sense and ordinary language would be to place a barrier which would seriously impede the all-important flow of communication between therapist and client in both directions. Indeed it is arguable that in the interests of maintaining communication with as wide a range of clients as possible, psychotherapists would have been better advised to stick more closely in their theoretical formulations to the psychological language of common sense than in practice they have done.

But it is not only the fact that the psychological language of ordinary discourse is the language that the patient or client uses and understands which makes this way of interpreting behavior the most appropriate one to select for purposes of psychotherapy; the psychological language of ordinary discourse is also the theoretical language which is the most useful and appropriate in any situation where an attempt is made to influence the behavior of an individual by communicating verbally with him/her, whether it takes the form advice, instruction, argument, persuasion, teaching, indoctrination or psychotherapy. The reason for this is that the so-called mentalist language of ordinary discourse provides us with a technique for explaining and predicting the behavior of the individual *not*, as has been traditionally supposed by the behaviorists and their opponents alike, in terms of the individual's private conscious experience, but in terms of the consistent and rational relationship which normally holds between what is said to the individual by others and what he or she says both to others and, more importantly but less accessibly, to him or herself on the one hand and what he or she otherwise does on the other.

The evidence that this is what we are doing when we explain behavior in mentalist/cognitive terms comes from the observation that such explanations consist typically of sentences in which a so-called psychological verb is followed by an embedded sentence in *oratio obliqua* or indirect reported speech. These embedded *oratio obliqua* sentences are of three kinds: (1) indicative, (2) interrogative and (3) imperative.

(1) Where the embedded sentence is in the indicative or declarative mood, following verbs like *know, remember, decide, expect, be afraid, believe, be angry*, etc., it is typically introduced in English by the pronoun *that*. In these cases the embedded sentence expresses "the gist or upshot" (Geach 1957) of the

answer the agent regularly gives or would give, if asked, when giving his or her honest opinion on the matter to which the sentence relates and which he or she would, in Elizabeth Anscombe's (1958) words, use "as a ... premise in [his or her] practical reasoning." Locutions of this type serve to characterize what philosophers are accustomed to refer to as the agent's "propositional attitudes."

(2) Embedded interrogative sentences are introduced by an interrogative pronoun, such as *who, which, what, when, where,* whether, *how* or *why,* following verbs like *know, remember, decide, see, wonder,* etc. They are used to characterize what the agent can or cannot do by reference to the questions he or she is able or unable to answer as the case may be.

(3) Embedded imperative sentences are introduced by the preposition *to* following verbs like *know, remember, decide, expect, be afraid, want,* etc., and are used to account for the agent's behavior by reference to a rule or instruction which he/she was following in doing what he/she did.

The advantage of using this device is that it allows us to group together all the behavior that is controlled either by a particular sentence or, more commonly, by a class of approximately equivalent sentences regardless of whether those sentences

(1) have been generated by the agent him/herself on the basis of previous encounters with the contingency they specify,

(2) are derived from information, instructions or arguments supplied by another speaker or from a written or printed text, or

(3) have been inferred by the agent from other sentences derived from either or both these sources.

It goes without saying that from the standpoint of someone who is trying to control behavior by persuading the agent to alter the sentences which are controlling that behavior, this is the way that the determinants of behavior need to be laid out. For it is only in so far as they are laid out in this way that the teacher, salesman, politician, advocate, negotiator or therapist can see at a glance what it is that needs to be changed, if behavior is to be modified by changing the way the agent talks. For these purposes there is simply no alternative to construing the behavior of the agent whose behavior is to be modified in terms of some version of what we have been calling "the mentalist/cognitive paradigm".

My second point, therefore, is that the mentalistic/cognitive language of common sense and ordinary discourse contains a conceptual scheme whose primary function is to enable us to predict and explain *ex post facto* the actions of other people in controlling or trying to control the environment from a knowledge of the kind of verbal behavior they exhibit when answering questions or otherwise commenting on the current and probable future state of that environment and their own goals and objectives with respect to it.

This system of explanation depends for its effectiveness on the fact that it is usually quite easy to discover what people are inclined to say about the situation confronting them either by asking an appropriate question or by discovering what information has been supplied by other people whether by word of mouth or through books, newspapers and the other so-called media of education, indoctrination and communication. It is also possible to infer with some degree of reliability what an individual will say by observing the various inputs of a non-verbal kind to which he/she has been exposed on the principle that when the conditions of observation are straightforward and unambiguous two people with normal sensory discrimination abilities will give broadly similar and non-contradictory accounts of what they observe, from which it is reasonable to predict that if I make certain observations in such a situation, you will do likewise. Moreover since one cannot effectively learn a language without learning to draw the more obvious inferences which follow logically from the various propositions expressed in terms of it, and since no coherent course of action can be based on propositions that are mutually contradictory, it is usually safe to infer that an individual will be inclined to assert whatever follows as an obvious logical consequence of other things he/she says or is inclined to say.

Given that we know or can reasonably infer what people are inclined to say about their situation and that there is a consistent and rational relationship between what they say and what they do, we are now in a position to predict within reasonable limits of confidence what they are or are not likely to do in adapting their behavior to that situation, without having observed their behavior when confronted with similar situations in the past. This, I suggest, is what we are doing when we predict what someone will do or explain

what they have already done in terms of what they know or believe about the situation in which they find themselves and their desires or aversions with respect to it.

There is, of course, no law of nature by virtue of which there exists this consistent and rational relationship between what people say and what they otherwise do on which mentalistic predictions and explanations of human behavior depend. Human beings sometimes talk in one way and act in another, in other words they tell lies. Nevertheless if lying were the rule rather than the exception and if lying were not usually detectable when it occurs, not only would our standard method of predicting and explaining behavior, which presupposes a consistent relationship between what people say and what they do, be undermined, the whole system of verbal communication on which human society, culture and civilization depends would collapse. It is not surprising therefore, to find that there are powerful social sanctions which operate so as to ensure that this consistency between what is said and what is done is maintained and can in general be relied upon.

These social sanctions however are aimed, not only at discouraging deliberate misrepresentation or lying, they are also aimed at discouraging any kind of loosening in the consistency and rationality of the connection between what is said and what is done on which our common sense mentalistic predictions and explanations of behavior depend. Human beings are discouraged by their fellows, not only from lying or breaking their promises, not only from talking unintelligibly, but also from behaving in ways for which they can give no rational justification in terms of their beliefs about and desires and aversions with respect to the situation in which the behavior in question occurs. Anyone who moves out of line in this respect is liable to incur what is for most people the highly aversive social stigma of being classified as a madman or lunatic, that is to say, the kind of person who is rejected by society because he/she fails to maintain the rational and consistent relationship between what is said and what is done on which our ability to predict behavior in common sense terms depends.

These social pressures to maintain a consistent and rational relationship between what is said and what is done help to explain the phenomenon of rationalization to which the psycho-analysts have drawn our attention in which the individual constructs a verbal account of his/her behavioral situation *ex post facto* in order to justify behavior which has already occurred or for which a behavioral disposition already exists independently of the individual's verbal construal of the relevant behavioral situation. Although in such a case the way the individual construes the situation in verbal terms has no causal role in the occurrence of the behavior in question, this does not prevent us from using the rationalizations as a basis for predicting and explaining the individual's behavior, provided that the behavior is in fact consistent with it. In practice, of course, it is only when we notice some inconsistency between the behavior and the reasons given for it, that rationalizations are detected. For most practical purposes the question whether the verbal formulation of the behavioral situation precedes or follows the behavior which it justifies is of no importance, so long as we can understand and predict the behavior in terms of it. So convenient is this method of explaining and predicting behavior and so deeply is it ingrained in our way of thinking and talking about the behavior of ourselves and other people, that we find no great difficulty in construing the behavior of organisms like animals or human infants, who do not talk, in terms of what they would say if they did. Moreover, when a psycho-analyst explains and interprets the patient's "neurotic symptoms" in terms of hypotheses about that individual's "unconscious mental processes", he is trying to explain aspects of the patient's behavior which are not in fact subject to verbal control in terms of the sort of verbal description of the behavioral situation which would be required to justify and make sense of that behavior, if it were.

I take it that the reasons for adopting this somewhat unusual explanatory strategy are a matter of clinical heuristics rather than scientific accuracy. The object is to bring aspects of the patient's behavior which are not in fact under verbal-conceptual control under that control (a) by undermining the patient's rationalizations by demonstrating inconsistencies between the behavior and the account which the patient gives of it and (b) by suggesting the kind of verbal formulation which would be required if the symptom behavior in all its details were to be consistently and rationally justified and selected in verbal terms.

Now it may not matter very much for the practical purposes of everyday life whether the person concerned would actually say the kinds of things which are implicitly attributed to them in explaining and predicting their behavior in this way, provided that the fiction that they would so talk yields a consistent

explanation and an effective prediction of their behavior. Nor, provided that consistent explanations and effective predictions can be derived from them, is it of any practical consequence if the reasons that an individual gives for acting as he/she does are in reality *ex post facto* rationalizations of pre-existing behavioral inclinations rather than considerations which were causally instrumental in the formation of the intention so to act. In the context of a scientific enquiry, on the other hand, where the object of the exercise is to discover and disentangle the actual causal relationships governing the phenomena under investigation such questions cannot be a matter of indifference. Metaphors and fictions, however convenient for practical purposes, can only be tolerated on a strictly provisional basis, as a deliberate oversimplification of a complex problem which will ultimately give way to a precise description of the actual causal relationships involved.

Now there are areas of psychological research where a mentalist explanation of an individual's behavior involves either, as in the case of animals and infants, the fiction that organisms that cannot talk can talk, or, as in the case of neurotic symptoms, the fiction that the individual in question would say things that he/she demonstrably would not say. In these areas, so it seems to me, there is an unanswerable case from the scientific point of view for the development of an alternative way of constructing explanations and predictions of the behavior of organisms which does not depend, as does the mentalistic paradigm, on the metaphor or fiction of a rational and consistent relation between what people say and what they otherwise do. Moreover, although they did not construe the objection to mentalistic explanations in quite these terms, it was the scientific objections to the use of mentalistic explanations of the behavior of animals and infants which led Thorndike, Pavlov and Watson to create the alternative stimulus-response-reinforcement paradigm in the first place.

Nor is the fact that it dispenses with the fiction that non- speaking organisms speak, the only reason for preferring an account of the behavior of such organisms in terms of the concepts and principles of behavior analysis to a mentalistic explanation. The development in recent years of techniques of behavior modification in terms of stimulus control and the management of contingencies has provided us with a powerful method of influencing, modifying and controlling behavior which does not depend, or depends to a much lesser extent, on verbal communication than do the traditional methods of advice, argument, persuasion, preaching, indoctrination and psychotherapy. To my mind, this method of predicting, explaining and controlling the behavior of organisms has proved itself beyond all reasonable doubt, not only as a rationale for the procedures adopted by the animal trainer, but also in such fields as the behavior of infants and children in the classroom, the behavior of the developmentally retarded, of "chronic psychotic" patients and many of the irrational symptom behaviors of so-called "neurotics".

It is notable, however, that in those cases where this approach to behavior modification has proved most effective, we are dealing either with organisms like animals, infants, or the developmentally retarded who have never acquired the verbal abilities of the normal human adult, with people like the chronic psychotic in whom the normal rational relationship between speech and behavior has broken down, or finally with aspects of the behavior of otherwise normal adults which are inaccessible to influence and control by the usual processes of argument, persuasion, preaching and indoctrination. On the other hand there is little evidence to suggest that in those areas where the traditional methods of control by verbal communication *are* effective there is much to be gained from introducing techniques of behavior modification based on stimulus control and contingency management and hence, from construing such behavior in terms of an explanatory paradigm which, I am suggesting, is only appropriate in so far as such methods of influencing behavior are to be attempted.

It may, of course, be argued that the process whereby behavior is controlled through the medium of verbal communication is itself susceptible to analysis in terms of the stimulus-response-reinforcement paradigm. Thus, although his book *Verbal Behavior* (Skinner 1957) has little to say about the control exercised by verbal stimuli over the behavior of the listener or, as in the case of self- directed verbal behavior, over that of the thinker, Skinner has made an important contribution to our understanding of the control that is exercised by verbal behavior over behavior of other kinds in his "operant analysis of problem solving" which forms Chapter 6 of *Contingencies of Reinforcement* (Skinner 1969).

In that chapter, Skinner introduces the well-known distinction between behavior that is "rule-governed", i.e., controlled by a verbal stimulus which "specifies" the relevant contingency, and behavior that

is "shaped" to the actual contingency by repeated exposure to it in the past. Given this distinction between "rule-governed" and "contingency-shaped" behavior, we can express the view we are considering by saying that the proper sphere for the application of mentalist/cognitive type explanations is to behavior that is rule-governed, while the proper sphere for the application of explanations couched in terms of the concepts and principles of behavior analysis is to behavior that is contingency-shaped.

To say that, however, is to assume (a) that rule-governed and contingency-shaped behavior form two distinct and non-overlapping classes of behavior and (b) that, just as mentalist/cognitive explanations have no legitimate application in the case of contingency-shaped behavior, so the concepts and principles of behavior analysis have no legitimate application in the case of rule-governed behavior. Both these assumptions have recently been challenged by Professor Skinner himself (Skinner 1984) in responding to a previous exposition (Place 1984) of the proposed connection between mentalism and rule-governed behavior. All behavior, according to Professor Skinner, is contingency-shaped. Some human behavior is rule-governed, i.e., it is verbally controlled. But what that means is simply that its initial appearance in the agent's behavioral repertoire is controlled by a verbal specification of the relevant contingency. It is still subject to subsequent shaping through actual exposure to the contingency itself and, therefore, to the same behavior analytic principles as any other form of contingency-shaped behavior.

Now it is perfectly true that behavior which is initiated either on the basis of information supplied by another person or on the basis of a verbally formulated hypothesis contrived by the agent him/herself is subject to modification or rejection in the light of subsequent exposure to the contingency *in vivo*. However, there is an increasing body of empirical evidence (Lippman and Meyer 1967, Lowe 1979; 1983, Lowe and Higson 1983, Lowe, Beasty and Bentall 1983, Bentall, Lowe and Beasty 1985) which shows beyond reasonable doubt that, where the effect of subsequent exposure to the contingency is mediated by a change in the way the contingency is specified verbally, the resulting behavior differs in a number of important respects from the behavior which results from the kind of contingency-shaping which is *not* mediated by a verbal specification of the contingency involved.

In other words, although the principles of rule-governed behavior are sufficiently similar to those of contingency-shaped behavior to allow us in a rough and ready way both to explain contingency-shaped behavior in mentalistic terms and at the same time to explain rule- governed behavior in terms of the principles of behavior analysis, neither procedure can be justified in the light of a detailed examination and comparison of the two types of behavior in question.

On the other hand, Professor Skinner is undoubtedly right in so far as he is suggesting, as I have taken him elsewhere (Place 1985), to be suggesting, that, whereas the principles of contingency-shaped behavior cannot on pain of circularity be deduced from those of rule-governed or verbally-controlled behavior, it is reasonable to suppose that the principles of rule-governed behavior will ultimately prove to be deducible from those of contingency-shaped behavior. After all, rule-governed behavior is behavior under the control of verbal stimuli, whether those stimuli are generated by another speaker or by the thinker him/herself in the course of problem solving; and there is no reason to suppose that the principles governing this form of discriminative stimulus control will ultimately prove to be any different from those governing other forms of discriminative stimulus control.

Nevertheless, the claim that we can understand the principles governing the control of behavior by verbal stimuli in terms of the principles that govern the control of behavior by discriminative stimuli of other kinds is only an assumption. It is not a matter of demonstrable fact. On the face of it, there are more than enough differences between verbal stimuli and stimuli other kinds to give, at least, some initial plausibility to the view which has influential support from Noam Chomsky (1959, etc.) and his followers and which holds that language is a domain where the rules and principles are quite different from those that apply to other aspects of behavior.

From the standpoint of behavior analysis, perhaps the most persuasive argument in favour of this view is the observation that the sentence or "rule", as Skinner calls it, which is the verbal stimulus unit required in order to provide effective stimulus control over the behavior of the listener differs from other discriminative stimuli in that it is able to alert the listener to the presence of a contingency despite the fact

that neither the sentence, the contingency for which it acts as a discriminative stimulus, nor the combination of the two have ever previously been encountered by the listener.

I personally believe that these difficulties are not insuperable and that a satisfactory account, consistent with the principles of behavior analysis, can be provided of the way sentences which the listener has never previously heard come to act as discriminative stimuli for contingencies which the listener has likewise never previously encountered. This can be achieved, in my view, by invoking the principle which I have referred to elsewhere (Place 1983) as "behavioral contingency semantics". This is the principle whereby a sentence acts as a discriminative stimulus for a contingency by virtue of an isomorphism between the content and structure of the sentence on the one hand and the content and structure of the contingency which it thereby specifies on the other.

With the help of a principle such as this we can hope to understand how the behavior of the listener or thinker is controlled by verbal stimuli in the form of rules or sentences of other kinds[3] in the same terms that we use to explain contingency-shaped behavior; but we are still a very long way from being able to dispense with mentalist/cognitive type explanations of the behavior that is thereby controlled. The reason for this, I suggest, is that behavior analytic explanations of particular items of contingency-shaped behavior are explanations which rely on an understanding of the relation between the behavior in question and the "reinforcement history" of similar behavior in the recent experience of the organism concerned. But because sentences are made up of individual words which have never occurred in that precise combination before, it follows that in order to give an explanation of the listener's response to a particular sentence in behavior analytic terms, we would need a separate reinforcement history both for the particular sentence type and for each of the individual words comprising the sentence in question. Such a reinforcement history, even if it were obtainable which, beyond the earliest years of an unusually well documented childhood it would not be, would undoubtedly conceal more than it revealed under a mass of irrelevant detail. For in the case of behavior that is "rule- governed", in Skinner's sense of that term, what determines the behavior that is selected is the nature of the rule or sentence and the contingency which it specifies. As we have seen, there are a number of different ways in which an agent can come to adopt a particular sentence as a basis for action. If we already know what rule or sentence is controlling an agent's behavior, nothing is added to our prediction of how he/she will behave by supplying details of the particular reinforcement history whereby the agent's ability to construct and respond to the sentence has been acquired and maintained and the sentence adopted by the agent as a basis for action.

If, in the light of these considerations, we conclude, that there is no possibility of our dispensing with either the behavior analytic or the mentalist/cognitive paradigm, so long as we need to retain the two types of behavior modification to which they relate, and if, as I would also want to argue, there is no serious possibility of our being able, sometime in the future, to replace both of these paradigms by some other paradigm, for example, a conceptual scheme based on a molecular analysis of behavior in terms of information theory, the computational model or neurophysiology[4], we shall be faced for the foreseeable future with the problem of having to live with two incommensurable paradigms both equally adapted in their different ways to the purpose of explaining and predicting the behavior of the individual at the molar level. If the analysis I have given is correct, the existence of these two incommensurable paradigms should present no great problem in those areas of psychological research and practice where we have no choice in the method of behavior modification to be adopted. In construing the behavior of animals, infants, cases of severe mental handicap and chronic psychosis where the control of behavior through verbal communication has little or no application, we shall inevitably find ourselves using some version of the behavior analytic paradigm. In construing the behavior of normal adults who are liable to regard conditioning procedures and

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[3] I suggested in an earlier paper (Place 1983) that all sentences, and not just those that qualify as "rules" in Skinner's (1969) sense, act as discriminative stimuli for a particular contingency by virtue of specifying one or more of its terms or "legs", as I prefer to call them. But, whereas the kind of sentence which Skinner (1957) calls "a mand" specifies the behavior to be performed without specifying the consequences of so behaving, a rule in Skinner's sense specifies both the behavior *and* its consequences.

[4] An argument for the view that the *explicanda* of molecular explanations do not overlap with those of molar explanations is given in Place (1981) pp. 19-23.

the manipulation of reinforcement contingencies as an insult both to their intelligence and to their freedom of choice and decision, we shall be compelled to retain the mentalistic paradigm that goes with the verbal-communication method of behavior modification. The problem of either choosing between, or somehow reconciling, the two paradigms will continue to be acute in those areas, where it already presents a serious problem, namely, in areas where the choice between verbal and non-verbal methods of behavior modification is not predetermined for us and where it may well be, not only advantageous, but essential to combine both methods in a single educational or therapeutic program rather than rely on one method to the exclusion of the other.

There are two areas of research and practice which spring particularly to mind in this connection. One is in dealing with the educational and behavioral problems of children who have learnt to speak and respond to language, but who have not yet acquired the verbal concepts of the adult. The other is in dealing with the behavior problems of neurotic and less seriously deteriorated psychotic adults. In these areas we must expect to find teachers, psychologists and therapists constantly needing to switch between these two different ways of construing the behavior of those they are trying to help, as they switch from one method of handling the problem to the other. As I see it, this is as problem to which there is no easy solution. For although there are many features of human behavior which can be equally well described and explained in terms of either paradigm and although what is asserted in terms of the concepts of the one is not inconsistent with what is asserted in terms of the concepts of the other, the two accounts are not equivalent; nor is there any simple way of translating what is said in the language of one paradigm into that of the other. In order to move fluently and effectively between paradigms, we need to understand the logical structure of both theoretical languages and the logical connections and the differences between them. For this purpose, I suggest, we need to acquire and make use of the skills of the logician and the linguistic philosopher.

Unfortunately, those philosophers and logicians who have interested themselves in the problems of the explanation of behavior in recent years have, almost without exception, adopted a position on this issue which is uniformly hostile to any form of behaviorism and regard the mentalist/cognitive language of ordinary discourse as the only acceptable paradigm for the explanation of human action. One can only hope that as the virtues of behavior modification techniques based on the concepts and principles of behavior analysis become more widely appreciated, the very important contributions which have been made by philosophers in recent years to our understanding of the mentalistic paradigm will be put to a more constructive and practical use in assisting those who are daily confronted with this conceptual problem in their clinical practice to handle the incommensurability of these two paradigms without being forced to abandon either of them.

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