SOME NOTES ON BEHAVIOR THEORY, BEHAVIOR THERAPY AND BEHAVIOURAL COUNSELING

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There have been a number of critiques of behavior theory as applied to counseling and psychotherapy (Breger and McGaugh, 1965, 1966; Kiesler, 1966; Murray, 1963; Weitzman, 1967), whose purpose has been, to some extent, to cut away the underpinnings. While these critiques have in turn been criticized (Rachman and Eysenck, 1966; Wiest, 1967; Yates, 1970), questions still remain regarding the theoretical and experimental bases of behavior therapy or behavioral counseling (these terms are used interchangeably in this paper). However it is not proposed here to reiterate these criticisms, or to question the (demonstrated) effectiveness of what is called behavior therapy. Rather, the purpose here is to raise questions regarding the nature of what happens in behavior modification, behavior therapy, and behavioral counseling, and to suggest that this area of activity is not as simple and clearcut as is often assumed to be the case, and that it is not as different from so-called "traditional" counseling or therapy as it has often been made to appear to be. Limitations of space prevent the detailed documentation of the points discussed. Further documentation of many of the points will be found in Murray and Jacobson (1970).

My comments will therefore not be restricted to Hosford's paper, which is elementary and avoids the issues and problems involved in behavior therapy. I shall pass by the use of the word "revolution" with only the comment that if the use of this term enhances the self-concept of behavior therapists, I am willing to allow them to use it. Hosford provides no justification for the use of the word, acknowledging that behavior modification goes back to Jones in 1924.

IS BEHAVIOR THERAPY THE ONLY SCIENTIFIC THERAPY?

Behaviorists emphasize that their approach is based upon "scientific research findings" (is there also nonscientific research?); that it is "laboratory-based", uses "experimentally-derived methods," and is based on "modern learning theory." The implication clearly is that other methods differ in these respects. The simple facts are that: (1) Other methods are supported by research; (2) The procedures used by behaviorists are not always based upon prior research demonstrating their effectiveness (not that this should necessarily be the case) but are often developed on the basis of clinical experience; (3) The research evidence for the validity of their methods is far from conclusive, and in fact, as more research data have accumulated, the more complex the apparently simple methods appear to be; (4) The methods are not necessarily explainable only by so-called modern learning theory (whatever that is) but can be rationalized in other ways. The recognition that counseling or psychotherapy is a learning process existed before the current revival of behavior theory, and other explanations involving learning theory have been proposed (Shoben, 1949; Dollard and Miller, 1950). Furthermore, the methods of behavior

therapy are not invariably successful. In spite of the recognized fact that negative results are underreported in professional and scientific publications, there are reports of failures, which may even be increasing--a phenomenon common as any new method begins to be used by other than its early, enthusiastic proponents. This will be referred to later in another connection.

Contrary to the impression often given that the methods are simple and clearcut, and that their methods of operation are clearly understood, it is becoming evident that they are highly complex and not clearly understood, as some behaviorists are willing to admit. Moreover, there is no integrating theory to tie together the many methods or techniques. "While there are many techniques, there are few concepts or general principles involved in behavior therapy" (Ullmann and Krasner, 1969, p. 252). Weitzman (1967) suggests that behavior therapy is actually "a nontheoretical amalgam of pragmatic principles." The behaviorists make a virtue out of necessity in expressing their willingness to try anything that seems to work or that might work. Behaviorists will try anything, and of course, sometimes (particularly under the right conditions, which will be considered later), with some clients, anything will work. Thus specific techniques are being tried and recommended and accepted on a superstitious basis until extinguished after enough failures. Wolpe (1968) refers to a case of Guthrie's in which a girl was locked in a car and driven around until her phobia of riding in cars was "apparently" overcome. This, of course is a good "common sense" approach, analogous to throwing a nonswimmer in the water to teach him how to swim. This would be claimed by many behaviorists to be inconsistent with learning theory as exemplified in desensitization (Hogan and Kirchner, 1967; Hogan and Kirchner, 1968; Levis and Carrera, 1967; Murray and Jacobson, 1970; Stampfl and Levis, 1967; Wolpin, 1966 Wolpin and Raines, 1966).

There is no general agreement on the nature and conditions of learning, and thus no generally accepted, proven principles or methods which can be automatically applied in behavior therapy. An examination of desensitization (see, e.g., Weitzman, 1967, and Murray and Jacobson, 1970) as well as of other techniques indicates that they cannot be reduced to the simple principles advanced by the behaviorists.

ARE BEHAVIOR THERAPY GOALS SPECIFIC?

Two points may be made about goals: (1) the behaviorists have no monopoly on goals which are specific; and (2) the behaviorists are interested in general, nonspecific, or what I have called "ultimate" goals (Patterson, 1970). Other counselors or therapists are of course concerned about specific behaviors of their clients, but the question which the behaviorists ignore or minimize is the meaning or significance of specific behaviors. The choice, or acceptance, of particular specific goals involves a value decision. The question is, what is the criterion which the behaviorists use in determining the appropriateness of specific goals? Michael and Meyerson (1962) say that this question has been decided by society. But one might well question whether society has made the decisions which they claim it has. At any rate, the criterion problem must ultimately be faced and resolved by the behaviorists. Ullmann and Krasner appear to be caught up in the problem of goals and values. On the one hand they state that "The stress throughout this volume has been that any behavior by itself is neither good nor bad" (Ullmann and Krasner, 1969, p. 589). Yet, they recognize that this is not so in any real or social sense: "if, on the other hand, the role of the therapist is to directly and actively change behavior (which in itself is

neither normal nor abnormal) and to arrange environmental contingencies, then what the therapist considers socially appropriate becomes crucial" (p. 593). The need for a criterion is obvious. Miller (1969), in his 1969 APA Presidential Address dealing with the question of the role of psychology in human welfare, states that "Changing behavior is pointless in the absence of any coherent plan for how it should be changed.... Too often, I fear, psychologists have implied that acceptable uses for behavior control are either self-evident, or can be safely left to the wisdom and benevolence of powerful men." A solution to this problem has been suggested in terms of an ultimate goal of self-actualization (Patterson, 1970).

Now before this goal is rejected out of hand by the behaviorists, two points should be considered. The first is that although self-actualization cannot yet be adequately measured, in principle it is measurable. As a matter of fact, considerable progress has been made in this direction (Patterson, 1970). The behaviors constituting the process of self-actualization can be defined objectively, and can be observed and measured.

It might be noted here that the behaviorists at least imply, if they do not state specifically, that nonbehavioral counselors ignore the problem for which the client seeks help. One might be equally justified in arguing that it is the behaviorists who refuse to accept the client's stated problem, and insist that he reduce it to the kind of problem with which the behaviorist prefers to deal.

Secondly, the behaviorists are interested in broad general goals, although they apparently consider these as side-effects. Yet the explicit consideration of side-effects has been ignored by the behaviorists. Pavlov noticed that, in addition to the specific conditioning which he produced in his dogs, other changes in their behavior occurred. He recognized that the total organism was affected by the conditioning procedure. Most current behaviorists are not aware of or have forgotten this aspect of Pavlov's work. They focus their attention upon a single specific result and ignore the possibility that significant other effects (or side effects) may occur. It has been suggested that one possible such effect of a highly structured, counselor-therapist-directed treatment may be increased dependency in the client. Behaviorists could, of course, respond that, if greater independence is desired, they could achieve this. But the point is that the behaviorists have not been concerned about possible effects other than the specific one with which they are concerned. In addition, there appears to be an inconsistency between methods of behavior modification and independence which should be explored. Moreover, it has not been demonstrated that behavior modification techniques are more effective for achieving independence or responsible behavior than other approaches such as client-centered or relationship therapy.

Side-effects need not, of course, be unfavorable, though this is the usual connotation of the term. There appears to be some evidence that behavior therapy, in addition to its specific effects, has general, or less specific effects, which are favorable, and these effects are accepted by behavior therapists as evidence of their effectiveness. Wolpe, for example, used as criteria of improvement not only symptom removal, but "increased productiveness, improved adjustment and pleasure in sex, improved interpersonal relationships and ability to handle psychological conflict and reasonable stresses" (Wolpe, 1958, p. 200). He does not explain how these results were measured, how they were achieved, or how they are related to the specific technique of behavior

therapy. Murray and Jacobson (1970) note that "rather than the symptom substitution feared by dynamic therapists, published and unpublished reports by the therapists indicate that when a symptom is removed, it is often followed by a general, nonspecific improvement in the individual's daily life." They cite a study by Gelder et al. (1969), in which it was found that systematic desensitization "not only decreased the main phobias and related fears more effectively than individual and group therapy, but had generalized effects in areas that had little connection with the focus of treatment. Thus, the systematic desensitization group showed greater improvements in adjustment to work and leisure time activities than either of the other two treatments, and as much improvement in general social relationships as those patients in group therapy" (Murray and Jacobson, 1970). The concept of symptom substitution is relevant here, and we now turn to this.

DO BEHAVIORISTS TREAT SYMPTOMS?

The issue of what behaviorists treat is clouded by the concept of "symptoms" used in a medical sense. If the results of behavior therapy go beyond the specific behaviors about which the behaviorists make so much fuss, just what is it that they are treating? The specificity of effect which many behaviorists insist on is apparently a myth.

The behaviorists have an explanation. It is that the change in specific behaviors of the client leads to changes in the way in which he is responded to by others in his environment, which leads in turn to changes in other aspects of the client's behavior. This is, of course, no doubt the case. But it also appears that the client's behavior may change in other respects prior to his experiences with others, and in ways which cannot be accounted for as generalization from the specific behavior effect. Such generalization, in the strict sense of the term, is recognized by the behaviorists as limited, even in terms of phobias other than the one treated.

Murray and Jacobson (1970) suggest that such changes are the result of changes in the belief system of the client, so that he believes, or has confidence that, he can cope with the situation, and other situations as well. In effect, there has been a change in his self-concept (see also Wolpin, 1968).

Murray and Jacobson (1970) refer to Leff's review of studies utilizing operant techniques with children, in which there were nonspecific effects which could not be accounted for, by either the authors or the reviewer, in terms of the procedures used, or in terms of stimulus generalization. They suggest that "it is difficult to see how such widespread effects can be attributed to the specific reinforcement procedure employed unless one assumes that it resulted in some basic change in the patients' attitudes and conceptions about themselves and their interpersonal environment." They also refer to the study of Valins and Ray (1967), in which a decrease in overt phobic behavior occurred following the giving of falsified information to the subjects about their autonomic reactions to snakes. They suggest that "the study indicates dramatically that the critical factor in systematic desensitization therapy is the change in beliefs about the self-similar to that occurring in traditional therapy - rather than the mechanics of relaxation, hierarchies, images, and so on."

These general changes in the client have significance for the position of the behaviorists with regard to the symptom removal controversy as well as the specificity of results. If there are general changes in the client, this suggests, if it does not actually prove, that the client's problem goes beyond the specific complaint which the behaviorists treat, and includes a general low level of "adjustment," for want of a better term. Thus, in a nonmedical sense, the complaint is a "symptom," or an indicator, of a more general problem. This is not surprising, since the individual is an integrated whole, whose parts are all interrelated, so that a change in one part--or one aspect of behavior--leads to or induces changes in others. The behaviorists appear to ignore the wholeness and integrity of the organism, although it is an obvious factor in explaining the generality of their results and an important theoretical support for their approach. Weitzman (1967) notes that the concept that "systematic desensitization does, as a technique, in some way affect the total psychological matrix, has not been given due theoretical consideration by behavior therapists or psychotherapists." In terms of a systems or cybernetic approach to human behavior, the entrance into or the modification of the system at any point leads to changes in the total system. However, this view also suggests that entering the system at the point, or level, of the belief system would also be useful or effective.

Many behaviorists, of course, accept the interaction of behaviors within the individual, and do not reject a causal interpretation, although their emphasis is upon external causes (i.e., in terms of contingencies). Yates (1970, pp. 398-399) indicates that even "some notion corresponding to the term 'symptom substitution' is acceptable to most (but not all) behavior therapists, though the use of a different term would probably be preferable...." He refers with approval to Cahoon's (1968) concept of symptoms and symptom substitution, a concept similar to that used here.

It is not our purpose, again, to question the results or the effectiveness of behavior therapy, but to question whether the explanations of the behaviorists are adequate to account for the nature and kinds of changes which appear to occur.

COUNSELING OR GUESSING GAME?

The narrowness of the concept of learning accepted by many behavioral therapists or counselors is illustrated by the methods used in many studies. In these studies, the counselor-or behavior modifier-actually sets up and engages in a guessing game with the client-or subject. This is essentially what Krumboltz and Thoreson (1964) did in their highly regarded study which demonstrated the effectiveness of reinforcement of information-seeking behavior with eleventh grade students. It would be interesting to compare the results of such treatment with simply telling "clients" what is it you wish them to do, such as "If you want to obtain educational-vocational information, you should seek such information by talking with people who are working in the occupation, look up materials in the library, visit schools and places of employment, etc." Such an approach would appear to be more simple and efficient than a reinforcement approach. In fact, there is some evidence that suggests that such a cognitive structuring method is as effective as reinforcement (Baker, 1966); Maurath, 1966; Rubin, 1968; Serber, 1967). Gilbert (1968, p. 32) presents an interesting anecdote relevant to this comparison. A psychology student in a mental hospital decided to attempt to eliminate by aversive conditioning certain antagonizing behavior which consisted of the patient sticking his tongue out at the staff. After several interviews the patient said: "Say Doc, if you're trying to get me to stop

sticking my tongue out, just tell me and I'll be glad to." As Murray and Jacobson (1970) put it, "if the critical factor in verbal conditioning is the communication of information, it would appear that there are more efficient means of achieving this goal than the protracted process of response shaping and extinction."

The problem of awareness in conditioning studies obviously arises here. It will probably come as a surprise to most behavioral counselors that the effects of operant conditioning do not occur without awareness on the part of the subject. Ericksen, evaluating the research on human operant conditioning, concluded that "in none of these situations is impressive or unequivocal evidence of learning without awareness obtained" (Ericksen, 1962, p. 11). If awareness is necessary for such learning then it is fair to ask whether it is conditioning in the strict sense of the term, or whether it is not, in fact, cognitive learning. If the latter is the case, as certainly it seems to be, the conditioning paradigm is inappropriate, and indeed, inefficient for such learning. Certainly the question may be raised, in the light of negative results in studies of conditioning without awareness, as to whether human beings are susceptible to operant conditioning. Parenthetically, the recognition of awareness as a cognitive aspect of what has been called conditioning leads to a suggested cognitive explanation for the resistance of behavior established by intermittent reinforcement to extinction, which has never been adequately explained by conditioning principles. Apparently intermittent reinforcement sets up expectations which persist through nonreinforced trials. Reinforcement, rather than operating in a mechanical manner, is essentially informational feedback.

Again, the question is not whether learning is involved in behavior modification, but whether the learning which takes place can be accounted for in the simple model of conditioning which the behaviorists propose. Counseling is not simply operant conditioning in an interview situation. The learning principles accepted by the behaviorists are limited and neglect or minimize cognitive, emotional, and social learning. In this respect, behavior modification is not current with the state of learning theory and research (Murray and Jacobson, 1970).

COUNSELING OR INSTRUCTION?

Related to the confusion of "behavioral counseling" with the use of reinforcement in an interview situation is the confusion of counseling with teaching. I have recently reviewed, as an editorial board member, several studies which involve this confusion. A study involving the comparison of different approaches to dealing with study problems will illustrate this. Such a study might involve a group in which group discussion (not counseling) with reinforcement of positive statements and attitudes toward study would be one treatment. A control group might be designated a "placebo" group, in which undirected discussion would be permitted (in one such study this group was called a "nondirective counseling group"). The criterion might consist of a test of knowledge about study habits and methods. When, as obviously would result, it is found that the first group does better than the second group the conclusion could be (and was in a similar study) drawn that behavioral counseling is more effective than nondirective or client-centered counseling! Such studies exhibit ignorance of what over 50 years of research in educational psychology has clearly demonstrated--that direct discussion and teaching in a subject matter area results in better scores on tests of achievement in that area than discussion not so directed or limited. It should be apparent that a measure of the acquisition of specific information

or behavior is not a fair test to compare an approach specifically directed to the development of such behavior with one which does not deal with such behavior. Yet this is typical of many studies purporting to compare behavioral counseling with "traditional" counseling.

In addition to the obvious illegitimacy of such conclusions, there is the matter of the confusion of didactic instruction with counseling. Behavioral counselors engage in such instruction and call it counseling. It is not possible to examine this problem in detail here, but if there is a distinction between counseling and teaching, even though both are learning, then behavioral counselors should be clear about what they are doing. This failure to make a distinction is the source of confusion. In fact, much of what behaviorists call counseling or psychotherapy is direct instruction or teaching, or in some cases, indirect teaching through others. Bijou (1946), for example, uses the term counseling to refer to the latter kinds of activities. Goldiamond and Dyrud (1968, p. 70) note the possibility of confusion when they say that "Whether the appropriate programming [or treatment] is to be academic [education] or therapeutic, or both, depends on the nature of the problem."

Even where the instruction is not direct, the cognitive aspects of behavior therapy are more important than is recognized. Ullmann and Krasner (1969, p. 189n) appear to reject the concept of cognition as a factor in abnormal behavior, in favor of "specific stimuli contingent upon behavior." Nevertheless, they recognize that the therapist teaches (p. 593), although they separate education and psychotherapy, even though they eventually merge (p. 597).

While the experimental psychologists have increasingly recognized the importance of cognitive, emotional (personality) and interpersonal factors in conditioning, behavior therapists have in general ignored these factors, although they are now beginning to discover their existence.

IS MODELING BEHAVIOR THERAPY?

One of the methods widely used in behavior therapy which would appear to involve cognitive learning is modeling. Modeling is a highly complex, yet universal form of learning, which cannot be reduced to simple S-R learning. It was not derived from so-called "modern learning theory," but was appropriated by the behaviorists when it was recognized as an effective method of behavior modification. Bandura's work (e.g., 1965) has indicated that modeling is not a simple process and that social (interpersonal) factors are extremely important.

It is interesting to note that modeling is turning out to be perhaps the most useful and most important method of behavior therapy. It has recently been found to be more effective than desensitization in the treatment of phobias, for which desensitization has been felt to be the specific treatment. This leads to the question of specificity of methods.

IS BEHAVIOR THERAPY SPECIFIC?

It has been noted in passing that there is no single method of behavior therapy. In fact, behavior therapy has appropriated or incorporated every conceivable method or technique in addition to those methods generally associated with it, that is, desensitization and various forms of conditioning and reconditioning. In addition to modeling, these include simple relaxation,

presentation of information, hypnosis, direct instruction, encouragement, suggestion advice giving, persuasion, and indoctrination, often in various combinations. Klein, Dittman, Parloff and Gill (1969), after observing Wolpe and Lazarus, noted: "From our acquaintance with the literature, we knew intellectually "at least that behavior therapists do not work in a unitary fashion and indeed take pains to vary their approach from case to case. We were surprised to find, however, that within most cases, too, a number of manipulations were routinely employed... It is appropriate...to note the apparent contradiction between the proliferation of methods in behavior therapy and the popular conception, based partly on hope, partly on the behavior therapists' writings, that this is a simple and straightforward treatment for the neuroses."

There appears to be some evidence that all of these methods lead to behavior change. However, the claims of some behaviorists that specific methods are most effective for specific results have not been demonstrated. In fact, evidence to the contrary seems to be accumulating. Although efforts are made in research to isolate specific techniques for study, most studies involve the confounding of several methods which makes it difficult to determine to just what the results may be attributed. The problem of relationship factors is especially noted here, since few, if any, studies have adequately controlled these factors. While it may be laudable for a therapist to do anything which may appear to be necessary or desirable to help the client, it then becomes impossible to attribute results to any specific treatment.

TECHNIQUES OR RELATIONSHIP?

The matter of the relationship in behavior therapy has long been a topic of dispute. Most behavior therapists admit the existence, even the necessity, of the relationship, but view it as a nonspecific factor. It is becoming clear, however, that the relationship is much more significant than has been admitted, and some behaviorists are recognizing this (Wolpe and Lazarus, 1966; Wilson, Hannan and Evans, 1968).

The relationship factor is extremely complex. An aspect of this is the so-called placebo. While it is not possible to examine this effect in detail, it appears that there is a difference of opinion regarding the desirability of eliminating the placebo effect from therapy. Some behavior therapists appear to feel that it should be avoided. Others, however, would accept it and exploit it (Krasner and Ullmann, 1965, p. 44).

Suggestion is a main aspect of the placebo effect. Klein, Dittman, Parloff and Gill (1969) comment: "Perhaps the most striking impression we came away with was of how much use behavior therapists make of suggestion .and of how much the patient's expectations and attitudes are manipulated." They quote Lazarus as stating: "Both Wolpe and I have explicitly stated that relationship variables are often extremely important in behavior therapy. Factors such as warmth, empathy, and authenticity are considered necessary but often insufficient." He continues later: "If suggestion enables the person to attempt new responses, these may have positive effects. One thus endeavors quite deliberately to maximize the 'placebo effect'." And he agrees that "even the results of a specific technique like systematic desensitization cannot be accounted for solely in terms of graded hierarchies and muscle relocation."

Efforts have been made in research to eliminate the effects. Lang, Lazovik and Reynolds (1965) conducted a study from which they concluded that suggestion and the relationship were not responsible for the results. However, they note that "the possibility of error in a procedure can never be completely discounted." While they found no relationship between suggestibility and results, it is quite possible that suggestibility operated (differentially perhaps) on all subjects. Grossberg (1964) notes that "It is not logically possible to prove the absence of an effect such as suggestion." The attempt to eliminate the relationship by using a device for automatic desensitization (DAD) (Lang, et al., no date) is also open to criticism. Although the desensitization process can be handled mechanically with a programmed tape, at least three elements of an interpersonal relationship were present. First, the tape utilized a human voice. Even if the usual printed tape were used, the likelihood of the subjects perceiving the machine as a person, or attributing personal characteristics to it by the process of anthropomorphism is present (Schwitzgebel and Traugott, 1968). Second, the total experiment involved relationships with the experimenters. Third, it is possible that the use of a machine enhances the expectation of help, which is a psychological or "placebo" effect.

The work of behaviorists with psychotic and autistic children (Leff, 1968; Lovaas, 1968a, 1968b) has involved intense personal relationships, clearly using the personal interest, concern and attention of the therapists as reinforcers. Incidentally, the non-specific results cannot be attributed to stimulus generalization or any other clear cause. Lovaas (1968a), referring to these changes, states: "As children improve, and their behaviors become increasingly complex, requiring increasingly complex environments, we face certain methodological and theoretical problems which we have been unable to handle within the present structure. Ricky, for example, can now verbalize that he makes 'crazy faces' to scare away fears. Such verbalization demands empathy, and I see no easy way, at the present time, to handle this interaction within a reinforcement theory paradigm" (see also Lovaas, 1968, p. 119). It is also interesting to speculate whether, if the therapist's behavior were based upon reinforcement in terms of results of his efforts, he would have persisted so long with no apparent effects. The total context of the situation, with its interpersonal factors, seems to be ignored by the behaviorists. While the Hawthorne effect is well known, and while experimental psychologists have become sensitive to what have been called the demand characteristics of the experiment (Orne, 1962; Rosenthal, 1966), the behaviorists, in both their research and practice, appear in general to be oblivious to the effect. (Kanfer, 1965; Krasner, 1965 and Sarason, 1965) are exceptions.) Hunt and Dyrud (1968, pp. 144-145) write: "In general, the social psychology of the situation receives too little attention [in behavior therapy research]. Placebo control are comparatively rare, and even more rarely do we have a firm basis for knowing how 'believable' the placebo controls were to the subject. Also, 'Hawthorne effects' need to be guarded against, as expectations and responsiveness may be enhanced when the subjects are given the opportunity to participate in an experiment on a promising new treatment."

Orne and Scheibe (1964) have suggested that expectancy factors on the part of the subjects have been involved in the results of sensory deprivation studies, and they concluded that "the effects of sensory deprivation are almost universally interaction phenomena;" i.e., the result of the nature of the relationship. Similarly, it might be maintained that the results of the techniques of behavior modification are essentially or mainly interaction phenomena, involving the demand characteristics of the situation including beliefs, expectancies and enthusiasm of the

experimenter or therapist on the one hand, and the set, expectancies, belief and faith of the subject or client on the other. Perhaps it does not matter too much what you do, as long as these factors (and other relationship factors) are present. As Ullmann and Krasner (1969, p. 408) note, "With tact, sensitivity and genuine respect for the person as an individual, there is little that is not possible; without these, little can be accomplished." Reich's orgone box was effective, and so are the methods of so-called quacks in medicine and psychology, because of these factors.

The criticism of behavior modification as mechanical, dehumanizing, and coldly manipulative is mistaken. It is just because behavior therapy is not impersonal that its claimed reasons for success, that is, its specific techniques, must be questioned.

Charcot recognized the role of suggestion in hypnosis but minimized it, insisting that hypnosis was a neurological phenomenon. The behaviorists recognize the role of suggestion (and more broadly the relationship) in behavior therapy, but minimize it, insisting that behavior therapy is a simple, objective learning phenomenon. History is likely to show that they, like Charcot, are wrong. Ullmann and Krasner (1969, p. 124n), commenting on Charcot's hypothesis, note that the null hypothesis that hypnosis is due to neural weakness cannot be proven. "All one can do is illustrate that every hypnotic phenomenon can be instigated without reference to trance or neurological weakness." Similarly, one cannot prove that the results of behavior therapy are not due to the specific mechanical techniques. All one can do is show that every result can be instigated without reference to these specific techniques. Evidence is accumulating that this is indeed the case. The emergence of modeling as the most important method of behavior change, a method in which complex relationship factors are present, supports this conclusion.

The evidence seems to indicate that, as the importance of relationship factors becomes evident, behavior therapy is moving closer to traditional therapy. The common element in all the various methods of the behaviorists is the relationship. The relationship itself can be viewed as a reinforcer for the kinds of human behavior which appear to be desirable as outcomes of counseling or psychotherapy. Ullmann and Krasner (1964, p. 56) refer to relationship factors as generalized reinforcers which "may be the most important factor in the control of behavior because they are so powerful." But these reinforcers are powerful only when they are genuine, and are not artificially offered and withdrawn or withheld. One of the reasons that the behavioral approach died out prior to its recent revival may be that the providing of praise, attention, interest and concern were used as techniques. The old motto that you can fool some of the people all of the time and all of the people some of the time, but you can't fool all of the people all of the time applies here. It might even be questioned whether you can fool anyone all of the time.

The behaviorists have suggested that traditional therapy uses or involves reinforcement, but have insisted that it is unsystematic and that the behaviorists are systematic in their use of reinforcement. Ullmann and Krasner (1965) write that "the traditional therapist does influence his patients and we think that he should do so consciously and systematically (p. 38)....However, the procedure [of the traditional or expressive therapist] is not overtly systematic (p. 37)."

IS BEHAVIOR THERAPY MORE SYSTEMATIC THAN TRADITIONAL THERAPY IN THE USE OF REINFORCEMENT?

The behavior therapists are, presumably, systematic in the application of specific reinforcers. But it might also be said that relationship therapists are systematic in the application of generalized reinforcers--interest, concern, warmth' respect, understanding.

In fact, it might be questioned how systematic the behaviorists actually are. Research on the conditioning of verbal behaviors, such as positive self-references, indicates that it is difficult to establish such conditioning. It has been contended that this is because the time periods in such therapy analog studies are brief, and that not enough reinforcements are provided to establish the conditioned response. But another factor appears to be present. Experimenters (including some of my own students) who have worked on such studies have noted how difficult it is to identify each member of the response class to be conditioned and to reinforce every occurrence of the response (or operant). This indicates that such conscious reinforcement is not continuous, that is it is not highly systematic. Skinner recognizes this: "In the experimental study of learning it has been found that the contingencies of reinforcement which are most efficient in controlling the organism cannot be arranged through the personal mediation of the experimenter....Mechanical and electrical devices must be used.... Mechanical help is also demanded by the sheer number of contingencies which may be used efficiently in a single experimental session....Personal arrangement of the contingencies and personal observation of the results are quite unthinkable" (pp. 21-22).

This may be true when one is concerned with reinforcing a highly specify response or response class on an artificial basis or even in the classroom, the situation to which Skinner is referring here. On the other hand, it is possible that the relationship therapist, in concentrating upon his own behavior and upon the general rather than highly specific behavior of the client (client self-talk, self-exploration rather than particular kinds of self-talk), is highly systematic in providing the generalized reinforcers of his approach. As Ullmann and Krasner note, "The therapist has a 'theory' that helps him respond rapidly to patient behavior" (1965, p.37).

A point which students raise here is that not reinforcing every response is not a problem because intermittent reinforcement is more effective than continuous reinforcement. This is an illustration of the lack of understanding of the conditioning process shown by many who accept and attempt to use it. Skinner answers that simply: "If you devise an apparatus which reinforces every hundredth response, and put an organism in it, the organism will starve because it will not make the required one hundred responses. But if you reinforce every response, then every second response, then every fifth, then every tenth, twenty-fifth, fiftieth, and hundredth, you can get a pigeon to go on indefinitely, responding one hundred times for each small measure of food. Actually, you can build up to ten thousand responses for each small measure of food. But it takes programming. You can't reach the final stage without going through intervening stages. In most studies of learning, organisms are plopped down into terminal contingencies of reinforcement and allowed to struggle through. They may reach the terminal behavior or they may not" (Evans, 1968).

SUMMARY

The purpose of these comments is not to question the effectiveness of those techniques grouped under the terms behavior therapy, behavioral counseling, or behavior modification, nor to question that the process involved is learning. However, the following points appear to be in order:

1. The learning process involved in the methods of behavior therapy is more complex, and perhaps even different, than the conceptualization of the behaviorists. "Research has begun to make it increasingly clear that the learning occurring in the behavioral therapies involves complex cognitive, emotional and motivational changes operating within a social context" (Murray and Jacobson, 1970), or in an interpersonal relationship.

2. Conditioning as an automatic, mechanical process without the awareness or without involving cognitive activities on the part of the subject, is rare or nonexistent in human behavior.

3. The existence of methods or techniques which are specific treatments for specific problems is questionable.

4. Much of what is called behavior therapy or behavioral counseling is simply standard teaching or instruction, applying principles and methods which are neither new nor unique.

5. Experience and research have indicated that the relationship variables, including those referred to as the placebo effect, the Hawthorne effect and demand characteristics of the experiment or situation, are much more important in behavior therapy than has been generally admitted.

6. Perhaps a major difference between behavior therapy and "traditional" or relationship therapy is that the relationship therapist has more confidence in the ability of the client to resolve his own problems--assuming that he is not lacking in information, knowledge or skills. Behavioral methods are useful where these are lacking and are essentially teaching or instruction. An apparent conflict exists between the view of the behaviorists that the client learns only when his learning is directed or controlled from without, and the view that the client is capable of learning on his own.

The early workers in behavior therapy oversimplified the approach, overgeneralized, and were overconfident and overoptimistic. Equating behavior therapy with learning techniques, they claimed the whole area of psychotherapy as a technology of learning. More recent work indicates that the situation is much more complex and less clearcut than a simple application of laboratory techniques. Behavior therapists are rediscovering the complexity of learning of which the experimental psychologists in learning have long been aware, and are also discovering the significance of human relationship factors.

7. The recognition of the significance of the relationship, involving the complexities of social learning, brings behavior therapy close to the so-called "traditional" therapies.

8. This provides the basis and opportunity for analyzing problems in learning in terms of those for which the relationship alone is the necessary and sufficient condition and those which require, in addition, information, direct instruction or training. The former, it is suggested, is psychotherapy, while the latter is teaching.

REFERENCES

Alexander, F. (1963). The dynamics of psychotherapy in the light of learning theory. *American Journal of Psychiatry,* 201, 440-448.

Baker, J. N. (1966). *Reason versus reinforcement in behavior modification*. Unpublished doctoral dissertation, University of Illinois.

Bandura, A. (1965). Behavioral modification through modeling procedures. In L. Krasner and L. P. Ullmann (Eds.), *Research in behavior modification.* New York: Holt, Rinehart and Winston. Pp. 310-340.

Bandura, A. (1968). On empirical disconfirmations of equivocal deductions with insufficient data. *Journal of Consulting and Clinical Psychology,* 1968, 32, 247-249.

Bandura, A. (1961). Psychotherapy as a learning process. *Psychological Bulletin,* 58,143-159.

Bijou, S. W. (1966). Implications of behavior science for counseling and guidance. In J. D. Krumboltz (Ed.). *Revolution in counseling*. Boston: Houghton Mifflin. Pp. 27-48.

Breger, L. and McGaugh, J. L. (1965). Critique and reformulation of "learning theory" approaches to psychotherapy and neuroses. *Psychological Bulletin,* 63, 338-358.

Breger, L. and McGaugh, J. L. (1968). Learning theory and behavior therapy: A reply to Rachman and Eysenck. *Psychological Bulletin,* 65,170-173.

Cahoon, D. D. (1968). Symptom substitution and the behavior therapies: Reappraisal. *Psychological bulletin,* 69, 149-156.

Dollard, J. and Miller, N. E. (1950). *Personality and psychotherapy*. New York: McGraw-Hill.

Ericksen, C. W. (D.). (1962). *Behavior and awareness*. Durham: Duke University Press.

Ericksen, C. W. (1962). Figments, fantasies, and follies: A search for subconscious mind. In C. W. Ericksen (Ed.), *Behavior and awareness*. Durham: Duke University Press, Pp. 3-26.

Evans, R. (1968). B. F. Skinner: The man and his ideas. Excerpted in *Psychiatry and Social Science Review,* 12(2), 12.

Gelder, M. G., Marks, J. M. and Wolff, H. M. (1967). Desensitization and psychotherapy in the treatment of phobic states: A controlled study. *British Journal of Psychiatry*, 1113, 53-73.

Gilbert, W. Discussion. In J. M. Whitely (Ed.). (1967) *Research in counseling.* Columbus, Ohio: Merrill. Pp. 30-35.

Goldiamond, I. and Dyrud, J. E. (1968). Some applications and implications of behavior analysis for psychotherapy. In J. M. Shlien (Ed.), *Research in psychotherapy*, Vol. III. Washington: American Psychological Association, Pp. 54-89.

Goldstein, A. P. (1962). *Therapist-patient expectancies in psychotherapy*. New York: Pergamon Press.

Grossberg, J. M. Behavior therapy: A review. (1964). *Psychological Bulletin*, 62, 73-88.

Hogan, R. A. and Kirchner, J. H. (1968). Implosive, eclectic verbal and bibliotherapy in the treatment of fears of snakes. *Behavior Research and Therapy*, 6, 167-171.

Hogan, R. A. and Kirchner, J. H. (1967). Preliminary report of the extinction of learned fear in a short-term implosive therapy. *Journal of Abnormal Psychology*, 72,106-109.

Hunt, H. F. and Dyrud, J. I. (1968). Commentary: Perspective in behavior therapy. In M. Shlien, (Ed.) *Research in psychotherapy*, Vol. III. Washington American Psychological Association. Pp. 140-154.

Kanfer, F. H. (1965). Vicarious human reinforcements: A glimpse into the black box. In L. Krasner and L. P. Ullmann (Eds.), *Research in behavior modification*. New York: Holt, Rinehart and Winston. Pp. 244-267.

Kiesler, D. J. (1966). Some myths of psychotherapy research and the search for a paradigm. *Psychological Bulletin*, 65, 110-136.

Klein, Marjorie, Dittman, A. J., Parloff, M. B. and Gill, M. M. (1969). Behavior therapy: Observations and reflections. *Journal of Consulting and Clinical Psychology*, 33, 259-266.

Krasner, L. (1965). Verbal conditioning and psychotherapy. In L. Krasner and L. P. Ullmann (Eds.), *Research in behavior modification.* New York: Holt, Rinehart and Winston. Pp. 211-228.

Krasner, L. and Ullmann, L. P. (Eds.). (1965). *Research in behavior modification*. New York: Holt, Rinehart and Winston.

Krumboltz, J. D. and Thoreson, C. E. (1964). The effect of behavioral counseling in group and individual settings on information seeking behavior. *Journal of Counseling Psychology*, 11, 324-333.

Lang, P. J. (1966), The transfer of treatment. *Journal of Consulting Psychology*, 30, 375-378.

Lang, P. J., Lazovik, A. D. and Reynolds, D. J. (1965). Desensitization, suggestibility, and pseudotherapy. *Journal of Abnormal Psychology*, 70, 395-402.

Lang, P. J., Melomed, Barbara G. and Hart, J. (No Date). *Automating the desensitization procedure: A psychophysiological analysis of fear modification*. Mimeographed paper.

Wiest, W. M. (1967). Some recent criticisms of behaviorism and learning theory with special references to Breger and McGaugh and to Chomsky. *Psychological Bulletin*, 67, 214-225.

Wilson, G. T., Hannon, A. E. and Evans, W. I. M. (1968). Behavior therapy and the therapist-patient relationship. *Journal of Consulting and Clinical Psychology*, 32,103-109.

Wolpe, J. (1958). *Psychotherapy by reciprocal inhibition*. Stanford: Stanford University Press.

Wolpe, J. (1968). Some methods of behavior therapy. In A. B. Mills (Ed.), *Behavior theory and therapy.* California Mental Health Research Symposium, No. 2. Sacramento: Bureau of Research, Department of Mental Hygiene.

Wolpe, J. and Lazarus, A. A. (1966). *Behavior therapy techniques.* New York: Pergamon.

Wolpin, M. (1968). Guided imagining in reducing fear and avoidance behavior. In A. B. Mills (Ed.), *Behavior theory and therapy*. California Mental Health Research Symposium, No. 2. Sacramento: Bureau of Research, Department of Mental Hygiene. Pp. 37-49.

Wolpin, M. and Raines, J. (1966). Visual imagery, expected roles and extinction as possible factors in reducing fear and avoidance behavior. *Behavior Research and Therapy*, 4, 25-37.

Yates, A. J. *Behavior therapy.* New York: Wiley, 1970.