1. What is behavior therapy?

Some basic issues
Is it possible to give an adequate definition of behavior therapy? Why do we need one? How will we know if we have found one?

First, we will sketch (in Section I) a brief history of the behavior therapy movement and then describe (in Section II) techniques used by behavior therapists. We will then be in a better position to say (in Section III) what behavior therapy is. (Discussion of theoretical issues begins in Section III; the reader familiar with the behavior therapy literature may want to start with this section.)

I. Origins

Although it would be arbitrary to fix the beginning of the behavior therapy movement at any precise time, there is some reason to trace its origins to the work of John Watson. In several important works, Watson developed and defended principles of behaviorism, including the ideas that psychologists should use experimental techniques and not rely on introspection and that they should study behavior and not the mind (Watson, 1913, 1919, 1924). His opposition to any kind of mentalistic psychology was sometimes based on the premise that the mind does not exist and sometimes on methodological arguments. The methodological arguments have had an important impact on the development of twentieth-century psychology, at least in the United States, and have influenced many leading behavior therapists.

Besides founding the school of behaviorism, Watson, together with his wife Rosalie Raynor Watson, performed a famous experiment in which a fear of rats was induced in a child, little Albert (Watson & Raynor, 1920). Albert initially displayed no fear of rats, but subsequently did so after a loud noise and a rat were presented together. The repetition of this pairing soon made the child afraid of the rat alone. Later, the child reacted fearfully to a white rabbit and to other furry objects.
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Watson and Raynor did not prove, of course, that all neurotic behavior results from conditioning, but they did demonstrate how one phobic reaction could develop in this way. Furthermore, their work suggested that in a case of this kind a counterconditioning technique, such as the use of an extinction procedure, could eliminate the problem. One of Watson's students, Mary Cover Jones, was one of the first to use such a technique, in treating a small-animal phobia (Jones, 1924, 1975).

There is an obvious analogy between the procedure of Watson and Raynor and the techniques used by Pavlov to condition a salivating response in dogs. This is not surprising, because Pavlov's work influenced Watson and his colleagues. For this reason and because of his work on the production of neuroses in dogs, Pavlov's contribution to the behavior therapy movement is significant (Franks, 1969).

Despite the work of Watson, Pavlov, and others, the behavior therapy movement did not develop in any significant way until the 1950s. Some of the important papers in this period are Lindsley, Skinner, and Solomon's (1953) study of operant conditioning in the treatment of psychotic patients; Wolpe's (1958) report of a successful treatment of neurotics by the use of (what is now called) systematic desensitization; and Lazarus's (1958) paper entitled "New Methods in Psychotherapy: A Case Study." In the 1960s two behavior therapy journals were begun: Behaviour Research and Therapy (begun in 1963) and The Journal of Applied Behavior Analysis (begun in 1968). In addition, several influential anthologies were published, including Eysenck's Behavior Therapy and the Neuroses (1960) and Experiments in Behavior Therapy (1964) and Ullmann and Krasner's Case Studies in Behavior Modification (1965). The work reported in these journals and anthologies in the 1960s is somewhat varied, but the following points seem particularly salient:

The influence of modern principles and theories of learning is strong. One may or may not agree with Wolpe's (1976) contention that behavior therapy has been based on principles and paradigms of learning, but even a cursory review of the relevant literature shows that there are several important connections between the practice of behavior therapy and research on learning. Some of these connections are explored later (Chapter 3).

Some writers contend that the development of behavior therapy has been strongly influenced by the philosophy of behaviorism (Krasner, 1971a; O'Leary & Wilson, 1975). This contention is dif-
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It is difficult to prove by appeal to any direct historical evidence, but it is plausible. One difficulty in obtaining direct evidence is that before 1970 there is relatively little discussion of behaviorism in the behavior therapy literature. Some writers referred to themselves as behaviorists but did so because they accepted the learning theories of Thorndike, Pavlov, Hull, or Skinner; they were not necessarily endorsing any particular philosophical doctrine. It is true, however, that in behavior therapy research and practice, the primary focus was on behavior, not on the mind or psyche. That has been one of the important differences between behavior therapy and traditional psychotherapy. It is not implausible to think that this difference was partly due to the influence of behaviorism. What exactly is meant by behaviorism and how it relates to behavior therapy is discussed in more detail in Chapter 2.

The so-called “disease model” of mental illness is often explicitly rejected (Ullmann & Krasner, 1969, 1975; Bandura, 1969; Rimm & Masters, 1974). There are several reasons. The conceptual arguments of such provocative nonbehavior therapists as Szasz (1961) were purporting to show that the idea of a “mental disease” is incoherent. Doubts about the utility of the medical classifications traditionally used to categorize so-called mental illnesses were becoming increasingly prevalent in clinical circles. A rejection of the medical model accorded well with ideas about behaviorism and modern learning theory. Although a behaviorist can agree to the use of certain mentalistic concepts, it is not surprising if he is somewhat skeptical of classifying behavioral problems as mental illnesses. He is also not likely to see such problems as illnesses if he believes that laws of learning can explain the origin and maintenance of so-called abnormal as well as normal behavior. All these ideas are discussed and employed in Ullmann and Krasner’s (1969, 1975) defense of a psychological model for behavior therapy. The defense of a psychological model and a rejection of the medical model are discussed in Chapter 4.

An important characteristic of most early behavior therapy writings is a skepticism about psychoanalysis. One of the main grounds for this skepticism was Eysenck’s influential reports (1952, 1966) that the percentage of remissions of symptoms in untreated neurotics after two years was at least as great as that in patients treated by psychoanalysts or eclectic psychotherapists. Eysenck’s arguments are now controversial. They have been vigorously criticized (Bergin, 1971; Bergin & Suinn, 1976) and defended (Rachman, 1971), but during the 1960s they served as stimulants to behavior
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therapists to develop alternative therapeutic techniques (Ullmann & Krasner, 1975).

It would be a mistake to assume that all early behavior therapists agreed about modern learning theory, behaviorism, and the belief that mental illness did not exist; there has always been some disagreement about these matters. More recently there have even been attempts to reconcile behavior therapy and psychoanalysis (Wachtel, 1977). However, there has been at least one doctrine that appears to be acceptable to all behavior therapists, early and late: Claims of therapeutic effectiveness need to be subjected to rigorous experimental tests. Behavior therapists have not been alone in agreeing to this demand, but in the fields of clinical psychology and psychiatry they have been more consistent than any other group in urging the need for experimental rigor.

Conclusion. The preceding discussion of origins is intended only to highlight certain important events; it is obviously not an adequate historical account of the behavior therapy movement. More detailed accounts can be found in Franks (1969); Ullmann and Krasner (1975); Krasner (1971a); Yates (1970); and Eysenck and Beech (1971). We should not expect, however, to gain too much even from a relatively complete account of the study of origins. It would be an egregious logical mistake to derive a conclusion about the nature of behavior therapy from a description of its origins. Behavior therapy might, for example, have originated out of learning theory experiments on rats, cats, dogs, and pigeons without having any logical ties to learning theory; it might have had behavioristic beginnings and yet not be, in any interesting sense, behavioristic. A better guide to the nature of behavior therapy would be a description of behavior therapy techniques. It is to this subject that we now turn.

The descriptions that follow are brief, elementary, and incomplete in important respects. Their function is to help determine the sorts of techniques we discuss in Section III. More complete descriptions can be found in standard behavior therapy textbooks (e.g., O'Leary & Wilson, 1975; Rimm & Masters, 1974).

II. Techniques

In attempting to describe therapeutic techniques, we must be careful not to beg two important theoretical questions: (1) Is there such a thing as a behavior therapy technique? (2) If question 1 has an affirmative answer, which techniques qualify? To most behavior
therapists it will seem obvious that there are behavior therapy
techniques, but not everyone agrees (Bergin, 1970). One possi-
bility is that the techniques used by behavior therapists have no
theoretically interesting properties in common, except possibly
for their being used and studied by therapists who share common
methodological and philosophical assumptions. To avoid ruling
out this possibility prematurely, the term behavior therapy
technique will be used in the present section to refer to techniques
commonly described as such by behavior therapists; no judgment
will be made now about the accuracy of this description.

This leads to our second question: Because behavior therapists
disagree in their classifications, which techniques should we in-
clude? Some writers, for example, include cognitive techniques
(Rimm & Masters, 1974), but others do not (Beck, 1970). If we take
sides on this issue now, we are likely to bias our inquiry into the
nature of behavior therapy. To avoid doing that, let us distinguish
between paradigmatic and nonparadigmatic behavior therapy
techniques. The former term will be applied to techniques concern-
ing which there has been widespread agreement and little or no
disagreement about their classification, techniques that have been
most thoroughly researched by behavior therapists and that are
often used to illustrate what is meant by behavior therapy. Using
these criteria, at least four procedures qualify as paradigmatic in-
stances of behavior therapy techniques: (1) systematic desensitiza-
tion; (2) aversion therapy; (3) operant conditioning; and (4) mod-
eling. This list may not be exhaustive, but no harm will result if
some techniques that might qualify as paradigmatic are classified
as nonparadigmatic.

**Paradigmatic techniques**

**Systematic desensitization**

When using systematic desensitization (or reciprocal inhibition
therapy), a therapist usually describes a set of threatening situ-
tions, sometimes called an anxiety hierarchy. With the help of the
client, the potential situations are ranked from least threatening to
most threatening. For example, someone with a snake phobia
might rate the picking up of a live snake as more threatening than
simply being in a room with a snake, which in turn might be consid-
ered more threatening than seeing a snake in the zoo. The next
step is to train the patient to relax by using a relaxation training
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procedure, such as that developed by Jacobson (1938), although
some therapists consider this step unessential (Paul, 1969). The
subject is then told to imagine as vividly as possible the least
threatening scene in the anxiety hierarchy. If the patient remains
relaxed, he moves up the hierarchy. If anxiety is exhibited at a
certain level, either the scene is repeated or the patient goes back
to imagining a less threatening situation. The goal is to have the
patient run through the hierarchy in a relaxed state. One impor-
tant advantage of using imagined rather than actual scenes is that
the therapist is given greater flexibility. For example, if a patient
has a fear of flying, the therapist cannot bring an airport or a jumbo
jet into his office, but he can ask his patient to imagine these
things.

Desensitization has been used to treat a wide variety of prob-
lems, including agoraphobia, claustrophobia, social anxiety, reac-
tive depression, fear of public speaking, stuttering, frigidity, fear
of atomic attack, impotence, and asthma (Paul, 1969; Wolpe,
1973). Although most of the original studies of desensitization
were uncontrolled case reports, Lang and Lazovik published a con-
trolled study, the first of its kind, in 1963. Subsequent studies,
some having an extremely sophisticated experimental design, pro-
vided fairly firm evidence for the effectiveness of desensitization,
at least for certain types of disorders and for certain types of pa-
tients (Lang, 1964; Paul, 1966; Davison, 1968; Moore, 1965).
Paul (1969) carefully reviews these and other studies and con-
cludes that at least eight are sufficiently well controlled to estab-
lish a causal relation between the therapy and the therapeutic
benefits, the first time in the history of psychological treatment,
according to Paul, that this has been accomplished.

Many of the subjects in the studies discussed in Paul’s review
were college students with rather mild phobias, but that is not true
of some of the more recent studies. In one well-controlled study
(Gelder et al., 1973), desensitization and another behavior
therapy technique, flooding, were used with psychiatric outpa-
tients having a mean age of thirty-five. Some of these patients had
relatively severe phobias. The authors concluded that both sys-
tematic desensitization and flooding were effective. In another
controlled study comparing various techniques, Hedberg and
Cambell (1974) found desensitization to be “highly effective” in
treating alcoholics, although not as effective as another behavior
therapy technique described as “behavioral family counseling.”
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Assessment. It would be extremely difficult in a brief space to give an adequate analysis of the evidence for the effectiveness of either desensitization or the other behavior therapy techniques to be discussed later. The following is not intended to be such an analysis, but it may highlight some of the main issues.

For any complex therapy, it is possible to distinguish at least three separate questions: (1) “Is it effective in producing therapeutic benefits?” (2) “Exactly which components of the therapy contribute to the production of such benefits?” (3) “For those components that do make a causal contribution, why are they productive?” If we know that a certain therapy is not effective, there may be no point in asking questions 2 or 3; but suppose that that is not true. Suppose, for example, that we have some reason to believe that psychoanalysis is effective. We may then be motivated to ask if specific components, such as the use of free association or dream analysis, make any difference, singly or in combination, to therapeutic outcome. If the answer is positive for both components, then we may go on to ask question 3: Does psychoanalytic theory or some rival theory best explain why these components make a difference?

The preceding distinctions should be kept in mind when discussing systematic desensitization. With respect to this therapy there have been attempts to answer our second and third questions (Rosen et al., 1972; Gaupp et al., 1972; Murray & Jacobson, 1971; Davison & Wilson, 1973; Bandura, 1977), but the evidence is still inconclusive. For this reason we will deal only with question 1: Is the therapy effective? Even this question may be too difficult to answer, depending on how it is interpreted. To circumvent some of the problems, we can stipulate the following. In asking if the therapy is effective, we will mean: “Does its use sometimes make a significant causal contribution to the production of beneficial therapeutic change?” (We could also specify what is meant by “significant” and “beneficial,” but we will assume that these terms are sufficiently well understood to get on with the discussion.) If we answer yes to question 1, we will be making a relatively weak causal claim; one that does not imply that in any given therapy situation, it is the systematic desensitization alone that accounts for any therapeutic change, nor that the therapy will be effective with any clinical problem, any client, or any therapist.

Understood in this minimal way, the claim that systematic de-
sensitization (hereafter, SD) is effective is widely accepted and is said to have been empirically demonstrated (Paul, 1969; Franks &
Wilson, 1975, p. 66). Has it been empirically demonstrated? De-
spite a wide body of supporting research, there are still some
grounds for skepticism. For example, Wolpe (1977) has recently
raised an issue about misconception-based fears and has concluded
that because of a neglect of the issue of hypothesis testing, studies
of SD lack the significance that is commonly attributed to them.
Wolpe’s point depends on the seemingly plausible assumption that
phobic patients will respond differently to different treatments,
depending on whether their phobia stems from a belief that the
object of their fear is dangerous or arises from an automatic reac-
tion to the phobic object. It seems plausible to think that some
type of cognitive therapy will be more effective if a false belief is at
the root of the client’s problem and that systematic desensitization
will work better where the fear response is automatic. If that is
right, then there is need to control for the source of the client’s
problem when testing therapeutic claims; most studies have not
done that. For example, suppose we find that a greater percentage
of clients treated with SD improve compared to a control group
receiving a pseudo-therapy. If cognitive-based problems are more
likely to be affected by nonspecific factors, such as the expectation
of being cured, then the mean differences between the two groups
might have arisen because more clients in the treatment group
have problems that are cognitively based. Wolpe (1977) is not
suggesting that all previous research on SD is worthless; but he
does contend that many of the experiments will have to be done
again because of a failure to control for differences in the bases of
phobias. The exact importance of this variable is difficult to
determine without hard evidence about how clients with
misconception-based fears react to different treatments, but there
is some reason to be concerned that some of the evidence com-
monly presented for the effectiveness of SD is irretrievably con-
taminated.

A second, and more important, reason to be skeptical concerns
the operation of factors in the therapeutic situation that are not
specifically part of the desensitization package. One of the most
important of these is the client’s expectation that he will be helped
by the therapy. There is evidence that this variable can account for
some of the improvement that typically follows the use of SD (Ro-
zen, 1974; Tori & Worrel, 1973; Lick, 1975). Can it account for all
such change? It might seem that the answer is clearly no: There
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have been many controlled experiments in which a SD treatment group has shown superior improvement to a pseudo-therapy group in which expectancy for improvement was induced by the experimenter (Paul, 1966; Lick & Bootzin, 1970; Miller, 1972; Davison, 1968; Hyman & Gale, 1973; Nawas, Fishman, & Pucel, 1970). However, much of the research in this area is difficult to interpret because of doubts as to whether or not the pseudo-therapy and SD were equally credible as viewed by the subjects (Borkovec & Nau, 1972; Nau, Caputo, & Borkovec, 1974). Newer studies have attempted to solve this problem by using more adequate methods to assess the subjects' expectation of therapeutic gain; the findings, however, have been inconsistent. Some of these studies (Brown, 1973; Steinmark & Borkovec, 1974) have found SD to be superior to an equally credible placebo therapy, but others have not (Lick, 1975, McReynolds et al., 1973).

One reaction to evidence about expectancy is to say that it bears not on the efficacy of SD, but on the explanation of why the therapy works; that is, it is relevant to answering our second or third questions but not the first (Lick & Bootzin, 1975, p. 928). This raises an interesting conceptual issue. Suppose that the use of the therapy is especially effective in establishing and maintaining expectancies that are therapeutic. Should we say (1) that the therapy causes the therapeutic change but does so through the operation of cognitive factors or (2) that the cognitive factors cause the change? The second option seems more plausible, although it may not matter to the client which option is correct. Suppose, for example, that psychoanalysis is useless in treating phobias but that psychoanalysts can be convinced that they will be helped with their own phobias only if they are given genuine psychoanalytic therapy; if we give them a pseudo-therapy resembling psychoanalysis, they will detect the difference and lose faith in the therapy.

Assuming that belief in the therapy is efficacious, it seems plausible to say that the psychoanalysis is not causing the therapeutic change, even though undergoing it is a necessary condition for the expectancy factor to become operative in phobic psychoanalysts. If this is right, then even if SD is particularly useful in inducing therapeutic expectancies, it might still be false that the therapy itself is effective. In any event, it is not clear that SD is more useful than certain pseudo-therapies in inducing an expectation of cure. In studies by Wilson (1973), Lick (1975), and McReynolds et al.
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(1973), there was evidence that the pseudo-therapies and SD were seen as equally credible.

Assuming that expectancy of cure can sometimes facilitate therapeutic change, how it does so is still unclear. One possibility is that the client who believes he is being helped may be motivated to test his belief by exposing himself to the phobic stimulus; another possibility is that the subject is “under more demand” to show improvement than is a client who does not expect to be helped (Nisbett & Valins, 1971; Lick & Bootzin, 1975). There are other possibilities. For some it may be more plausible to say that the therapy is effective but works partly because of the client’s beliefs; for others, that the belief and not the therapy causes the therapeutic change. Even without knowing how expectancy of cure works, however, we can say this: If none of the improvement that follows SD is caused by the therapy, if all of it is caused by the operation of nonspecific factors, including the client’s belief that he will be helped, then the therapy is not effective; the “nonspecific” factors, by definition, are not part of the therapy. It is not true, then, that data concerning expectancy do not bear on the issue of the effectiveness of SD. On the contrary, if it is true, as argued by Kazdin and Wilcoxon (1976), that the overwhelming majority of SD studies have failed to rule out differences in expectancies of success in clients in treatment and control groups, and if the relatively few studies that have done this have produced inconsistent results, then an important rival hypothesis has not yet been ruled out: that the apparent success of SD is caused by nonspecific factors. If this is true and if we add in the uncertainty generated by the almost universal failure to control for the source of the client’s problem, then there is good reason to doubt that the effectivess of SD has been empirically demonstrated. Although some of the uncertainties are likely to be removed by research that is already in progress, it is still sobering to realize that in 1976 (the year in which Kazdin and Wilcoxon’s report was published), reasonable doubts could still be raised about the most heavily researched of all behavior therapy techniques. The issues have simply proved to be much more complex than was once thought. The doubts, however, concern not the effectiveness of the therapy, but the demonstration of that effectiveness. First, the total evidence provides no support for the hypothesis that SD is not effective. There have been some reports of SD failure (e.g., Gelder & Marks, 1966), but they are counterbalanced by hundreds of apparent successes (Paul, 1969; Wolpe, 1973; Paul & Bernstein,