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An Analysis of Systematic Desensitization and Covert Modeling: A Theoretical Comparison

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AN ANALYSIS OF
SYSTEMATIC DESENSITIZATION AND COVERT MODELING:
A THEORETICAL COMPARISON

by
Frank G. Barefield, Jr.

A Thesis
Submitted to the
Faculty of the Graduate College
in partial fulfillment
of the
Degree of Master of Arts

Western Michigan University
Kalamazoo, Michigan
December 1976
ACKNOWLEDGEMENTS

I would like to express heart-felt gratitude to P. G. B., without whose constant encouragement and support this thesis would not have been written.

Frank Barefield, Jr.
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I have mind myself and recognize
Mind when I meet with it in any guise.
No one can know how glad I am to find
On any sheet the least display of mind.

-- Robert Frost
A COMPARATIVE ANALYSIS OF
SYSTEMATIC DESENSITIZATION AND COVERT MODELING

INTRODUCTION

The aim of this paper is to demonstrate that a counter-conditioning model of systematic desensitization (SD) is not adequate to account for the success of the clinical procedures and to outline an alternate explanation of SD based upon a cognitive mediational model and modeling theory. SD as an extinction procedure will also be discussed and weaknesses in this analogue will be indicated. Finally, the clinical technical advantages accruing to a conceptualization of SD as a modeling procedure will be examined and some suggestions will be made for future research.

According to Krasner (1971), the term "behavior therapy" was first used by Lindsley, Skinner and Solomon, in 1951, to describe the use of operant procedures to modify the behavior of psychotic patients. Since that time, this term has come to be applied to a wide range of techniques derived, according to most proponents of behavior therapy, though there are some notable exceptions (e.g., London, 1972), from "modern learning theory". A major argument advanced in support of behavior therapy as a more viable approach to behavior change than traditional psychotherapy is that behavior therapy techniques are based upon experimentally validated laws of learning (Eysenck, 1966; Rachman, 1973; Wolpe, 1973, 1961). Modern learning theory, i.e.,
conditioning or reinforcement theory, is not, however, without its critics. Breger and McGaugh (1965) discuss what they consider to be fatal flaws in the modern learning theory account of neurosis and psychotherapy. Though they criticize classical conditioning and operant learning theories in general, the theoretical and technical position represented by Eysenck and Wolpe, especially SD and counter-conditioning, are their main targets.

According to Breger and McGaugh, neither classical conditioning nor operant theory yields a convincing account of neurotic behavior or psychotherapeutic learning. They suggest that to answer the question, what is learned, in terms of cognitive mediation and learned strategies allows for a more complete account of observations from the laboratory, from behavior therapy, and from psychotherapy, than can be developed from either a classical conditioning or operant perspective.

Rachman and Eysenck (1966) and Weist (1967) defend behaviorism, arguing that the Breger and McGaugh attack is a misrepresentation of and irrelevant to the behavioristic position. For example, Weist writes,

"Misrepresentation occurred ... in the statement that "the use of the terms stimulus and response [by behavior therapists] is only remotely allegorical to the traditional use of those terms in psychology ..." To support this view, Breger and McGaugh referred to Bandura's ... review of Wolpe's methods and states,"'The imagination of a scene' is hardly an objectively defined stimulus ..." The quoted statement is misleading because it confuses stimulus and response and attributes the confusion..."
Imagination is a form of behavior, not a stimulus ... pp. 215-216.

Even aside from the fact that calling imagination of a scene the response rather than the stimulus just shifts the problem rather than solves it (i.e., taking imagination of a scene to be an objectively defined response "is only remotely allegorical" etc.) Weist's analysis does not completely clarify Wolpe's position. Wolpe (1961) writes of "presenting to the imagination of the deeply relaxed patient" an anxiety-provoking stimulus. Wolpe does not make clear whether the imagination of the scene is the response, the imagined scene, the stimulus, or the imagination of which the scene is presented some sort of psychic structure within the patient.

If imagination is taken as behavior, problems remain, since the introduction of this sort of behavior into behaviorism might require major adjustments within the paradigm. Lazarus (1971b) points out that symbolic ability might make human neuroses qualitatively different from animal neuroses: "The images and ideas that a patient undergoing desensitization therapy may report cannot readily be dismissed or subsumed under a facile analogy to experiments with cats" (p. 370). Wolpe (1973) introduces, perhaps inadvertently, this same point, by suggesting that cognitive abilities of the human animal provide a source of technical innovations in the practice of behavior therapy not available to experimenters working in laboratories with
with less brainy creatures. He writes,

In human subjects ... the treatment of neuroses may be complicated ... However, endowed with language, we can unravel most webs and our very complexity gives to human behavior therapy the possibility of a large repertoire of techniques. For example, some boys who have had frightening experiences at the stoves might lose their fears of similar chests of drawers to which the fears had generalized by verbal explanation and physical demonstration of the differences between the two kinds of objects -- one kind of solution not available for the neuroses of animals [pp. 55-56].

Wolpe's statement implies that cognition can function in a mediational capacity, and it seems a violation of the claim that behavior therapy techniques are derived directly from laboratory experiments.

Breger and McGaugh (1966), in a reply to Rachman and Eysenck, argue that Rachman and Eysenck do not address the original issues and that their position is a misrepresentation of and irrelevant to the position outlined by Breger and McGaugh, rather than the other way around as Rachman and Eysenck claim.

Bolles (1972) also questions the utility of an automatic reinforcement view of learning and proposes that reinforcement is neither necessary nor sufficient for learning and explains behavior in terms of cognitive expectancies. Bandura (1974) rejects automatic reinforcement, saying,

Contrary to popular belief, the fabled reflexive conditioning in humans is largely a myth. Conditioning is simply a descriptive term for learning through paired associations, not an explanation.
of how the changes come about ... So-called conditioned reactions are largely self-activated on the basis of learned expecta-
tions [p. 859],

and,

... there is little evidence that rewards function as automatic strengtheners of human con-
duct [p. 860].

Before further reviewing recent evidence and opinion, a critical examination of Wolpe's original experiment will perhaps help to set the stage for a reconceptualization of the theoretical base of SD.
WOLFE'S EARLY WORK

Wolpe (1958) was the first to outline clinical SD procedures and writes of them, "Of all the methods of therapy considered in this book, the present parallels most closely the experimental procedure of feeding cats in the presence of increasing 'doses' of anxiety-evoking stimuli" (p. 139). This suggests that the strength of claims for a scientific base for SD rests on the degree of correspondence between clinical SD and Wolpe's experimental procedure. The validity of these claims is a reflection of how definitively Wolpe's cat experiment rules out other explanations of experimental neuroses that might be offered as alternatives to the explanation Wolpe derives from classical conditioning theory.

In an early review of experimental neurosis studies, Cook (1939) isolates procedures which successfully elicit from subjects behaviors termed neurotic. Restraint of the subject in the experimental chamber while requiring the subject to make a difficult discrimination or to inhibit a response in the presence of an excitatory stimulus seem sufficient to produce experimental neuroses. Most of the studies reviewed by Cook used classical conditioning procedures. Cook concludes that none of the explanations offered by the authors of the studies reviewed is adequate to account for all the data.
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Wolpe (1952) reviews many of these same studies, analyzing them within a framework which he offers as adequate to explain the production of all experimental neuroses. Like Cook, Wolpe implicates confinement as "a potent factor in neurosis production." He then divides all experimental neuroses into two groups: those produced by ambivalent stimulation and those produced by noxious stimulation. His basic thesis is that either of these variables will generate anxiety in the subject and that this anxiety, under conditions of confinement, will elicit those behaviors called neurotic. Most of Wolpe's attention, clinically and experimentally, has been directed toward neuroses produced by noxious stimulation, SD procedure and rationale being based upon this model of neurotic behavior. Because Wolpe's (1952) cat experiment plays such an important part in the later development of SD, a critical examination of this experiment seems in order. This experiment had a two-fold purpose: one, to demonstrate that conflict, as Masserman (1942) proposed, is not necessary for the production of neuroses, and two, that subjecting a confined animal to noxious stimulation is sufficient to produce neurotic behavior. Wolpe's claims notwithstanding, his experimental design did not allow a definitive test of either of these hypotheses.

Wolpe's (1952) procedure included a replication of Masserman's (1942) experiment in which cats were trained
to eat from a foodbox upon presentation of a signal; then, whenever the cats made an approach response toward the foodbox, a shock was paired with the signal. Wolpe's conflict group was composed of those cats. A no-conflict group was also included; these cats were exposed to shock preceded by a "hoot". Experimental neuroses were obtained in the conflict group after two or three shocks and in the no-conflict group after an initial series of five to ten shocks followed by a second series of shocks one to three days later. During the deconditioning phase, neurotic behaviors in the no-conflict group faded more quickly than in the conflict group. These differences seem to weaken Wolpe's claim that conflict is not implicated in the development of experimental neuroses. Wolpe's account also leaves unexplained the efficacy of feeding the cats prior to putting them in the experimental chamber for reducing the neurotic behavior (Masserman, 1942).

Wolpe (1958, 1952) defines neurosis as persistent unadaptive behavior acquired by learning, usually characterized by anxiety (anxiety always being part of the casual sequence). The immediate reactions of the experimental cats to the shock ("... rushing hither and thither, getting up on the hind legs, clawing at the floor, roof and sides of the experimental cage, crouching, trembling, spitting ..., etc.) do not qualify as neurotic, given Wolpe's definition. His position on this point is unclear; though
not stating explicitly that these behaviors are neurotic, Wolpe does refer to them as symptoms. The initial responses were not persistent and for two reasons should not be considered unadaptive. One, the design of the experiment did not allow the emergence of any adaptive behavior during the shock phase, i.e., behavior which led to the reduction of pain, and, two, the topography of the initial responses suggests that they might be considered escape attempts. Had the cats not been confined, their responses would have removed them from the aversive situation. Wolpe (1952) does specify three lasting behaviors which he clearly considers neurotic: "(1) resistance to being put into the experimental cage; (2) signs of anxiety when inside the cage (muscular tension and myriasis were invariable); (3) refusal to eat meat pellets anywhere in the cage even after one, two or three days' starvation" (p. 255). Of these, only refusal to eat seems even remotely unadaptive, given the empirical association of shock with the experimental cage.

To decondition the cats to the experimental chamber, they were fed in one of three rooms which seemed "to the human eye and ear, to have decreasing degrees of resemblance" to the experimental room, beginning with the most dissimilar room and proceeding to the experimental room. The cats were moved from one room into the next only after no anxiety was observed in the more dissimilar room.
Wolpe reasoned that the reeding response inhibited the anxiety and that this reciprocal inhibition provided the therapeutic potency of the procedure. As noted above, clinical SD procedures are based upon this experiment. Though there is a superficial resemblance between the experimental and clinical procedures, the analogy from experiment to clinical application breaks down in several places.

Physical restraint of the experimental subject is widely accepted as an essential condition for eliciting an experimental neurosis. Wolpe (1952) assumes that "the force of habits previously learned" is equivalent to confinement as it is used in experimental neurosis studies. Without the support of experimental data, this seems a weak correspondence. Wolpe does not provide this evidence and Davison (1968) writes,

... the author has sought vainly in the experimental literature for paradigms which illustrate the acquisition of stable fear responses in human beings under conditions bearing even a remote resemblance to what would likely hold in real life [p. 98].

The lack of congruence between "presenting to the imagination of the deeply relaxed patient" an anxiety-provoking scene and feeding cats in a room which seemed "to the human eye and ear" to resemble a room feared by the cats has been commented upon above. Finally, the experimental subjects were held in each of the deconditioning rooms until there was no indication of anxiety; in SD, clinical subjects
are instructed to signal anxiety, upon which the scene is immediately withdrawn. Wolpe's claim that SD closely parallels experimental procedures seems highly tenuous. An evaluation of other evidence demonstrates that Wolpe's cat experiment is a definitive statement neither of the conditions under which experimental neuroses occur nor of the essential elements for treatment, and casts further doubt on the claim that human neuroses can be adequately understood in terms of experimental evidence generated by modern learning theory.
OTHER CONSIDERATIONS

Solomon and Wynne (1953) and Solomon, Kamin, and Wynne (1955) varied the parameters studied by Wolpe. Dogs placed in a shuttle box were presented with a CS and ten seconds later shocked. The shock continued until the dogs jumped over a barrier into the safe (non-shock) chamber. This behavior quickly established as an avoidance response, often occurring less than two seconds after the presentation of the CS. This behavior was totally resistant to ordinary extinction, the dogs continuing to jump after hundreds of no-shock trials, each dog developing a ritualistic, stereotyped set of behaviors which were performed in addition to the jumping response. To eliminate the jumping behavior, a three second shock, administered when the dog jumped into the previously safe chamber from the original shock chamber, was introduced. Though this did disrupt the ritual behavior somewhat, the dogs acting as if they were anticipated or expected the shock to occur as a result of jumping, the jumping was not eliminated.

This behavior satisfied Wolpe's criteria for experimental neuroses; it is persistent, unadaptive, and learned. However, in this case the neurosis is not generated by pairing an aversive stimulus with restraint, but by pairing shock with the performance of an avoidance response while providing no opportunities for the dogs to learn alternative avoidance strategies. As in the case of Wolpe's
cats, the dogs' behaviors appear to be experimental analogues of human neurosis, in this case obsessive-compulsive behavior. But a point-by-point comparison reveals some weaknesses. Solomon (1964) writes,

My guess at the moment is that experimental neurotic disturbances arise often in those cases where consummatory behavior or instinctive behavior is punished, and punished under nondiscriminatory control [p. 251].

The relationship between punishing consummatory or instinctive behavior in animals and the conditions under which human neuroses occur is unclear. Not only is consummatory or instinctive behavior difficult to specify in the etiology of human neuroses punishing stimuli which might be hypothesized to account for a particular neurosis are seldom as clearly defined as an electric shock of a given intensity and duration. Note also that these conditions are not those outlined by Wolpe (1952).

More recent research indicates that aversive stimuli have effects determined by the context in which they occur. Holz and Azrin (1962, 1961) report that experimental neuroses can be obtained in rate by shocking them while they are restrained. If the same shock is made a discriminative stimulus for reinforcement, however, no neuroses develop. It might be said that neuroses do not develop when the aversive event is made part of a predictive relationship with other environmental events.

These two examples should make it clear that Wolpe did not adequately define the parameters which influence a sub-
ject's response to the experimental conditions he manip­
ulated (shock and restraint) and that one must look else­
where for an adequate account of these and Wolpe's findings.

Bolles (1972, 1970, 1967) postulates the existence of
species specific defense reactions (SSDR's), interpreting
escape and avoidance in terms of these reactions. His basic
thesis is that in escape and avoidance experiments, the sub­
jects learn, not a discrete response, but an expectance,
i.e., a predictive relation between aversive environmental
events and which SSDR is effective in escaping or avoiding
them. Assuming the validity of this thesis (successful
tests of hypotheses derived from Bolles's theory are re­
ported by Peterson and Lyon (1975) and Weisinger, Parker
and Skarupsk (1974); indirect supporting evidence is re­
ported by Azerin, Hutchinson, and Hake (1967), Azrin and
Norman (1964), and Ulrich and Azrin (1962). The experi­
mental neuroses obtained by Wolpe in his cats can be view­
ed as thwarted escape behaviors. This integrates restraint
into the theoretical picture, rather than just listing it
as a prior condition necessary for the generation of experi­
mental neuroses as Wolpe does. Bolles's hypotheses also
make sense of the change observed in the topography of the
cats' behaviors, providing a rationale for expecting the
more passive behavior Wolpe reports as a lasting effect
of the shock/restraint procedure to follow the active "es­
cape" attempts reported as immediate effect. This suggests
that the cats learned not a collection of discrete responses as Wolpe supposes, but helplessness, i.e., that no response was effective for escaping or avoiding the shock. This serve a better account of Wolpe's (1952) findings than classical conditioning can provide.

These same concepts can account for the findings reported by Solomon, et al. (1953). The dogs learned that the CS predicted the occurrence of interminable shock and responded to the expectancy by jumping the barrier to avoid this shock. When the three second shock was introduced, the dogs gave every indication of learning this second expectancy, that jumping is followed by shock, but continued to jump in response to the original expectancy of interminable shock. This interpretation is partially supported by a third procedure employed to eliminate jumping. A glass partition was installed across the escape hatch to prevent the dogs from jumping upon presentation of the CS. This procedure more effectively reduced jumping on trials when the partition was removed than did the three second shock procedure, with Solomon, et al. predicting that had it been continued longer, it would have been totally effective. Solomon (Rescorla and Solomon, 1967) later concluded that the important feature of avoidance is learning the predictive relationship between the CS and the aversive stimuli.

The above explanation also accounts for Holz and
Azrin's (1962, 1961) findings. If an aversive stimulus is made a discriminative stimulus, its meaning to the subject changes. The subject reacts to it as a way of manipulating the environment, rather than with helplessness to a punishing and unresponsive environment.

The analysis of the studies reviewed above suggests that Wolpe's cat experiment is a definitive statement of neither the conditions under which human neuroses develop nor the elements necessary for their treatment, and casts doubt on the relevance of much of the research generated by modern learning theory for understanding human neuroses. This does not mean that there is nothing of use to the clinician to be learned from these studies. The question, what is learned, is raised by this analysis; certainly cognitive factors which have been largely ignored by proponents of conditioning and reinforcement theories are implicated. Bandura (1974, 1971, 1969) has integrated these factors into social learning theory and derived from this theory a highly effective behavior change technique. A consideration of those aspects of social learning theory necessary for a reconceptualization of SD as a modeling procedure is presented below.
SOCIAL LEARNING THEORY:
A SHORT SUMMARY

Bandura's (1974, 1971, 1969) basic thesis is that a behavior is determined by the anticipated consequences of performing that behavior. That is, what is learned is a symbolic representation of behavior and the associated consequences of that behavior, rather than a peripheral S-R or S$^D$-R-S$^R$ connection. ("Originally, conditioning was assumed to occur automatically. On closer examination, it turned out to be cognitively mediated" (Bandura, 1974, p. 859).) Social learning theory has three important distinguishing characteristics: (1) no response is required by the theory for learning to take place; (2) learning can occur in the absence of reinforcement; and (3) covert processes can be manipulated to produce behavior change. Each of these are discussed below.

Learning occurs first as a central event:

When a person observes a model's behavior, but otherwise performs no overt responses, he can acquire the modeled responses while they are occurring only in cognitive, representational forms [Bandura, 1969, p. 133].

Social learning theory thus distinguishes between acquisition and performance. During acquisition, patterns of behavior come to be represented cognitively in verbal and imaginal form. Bandura implies that cognitive acquisition always precedes, except in the case of reflexive behaviors, the overt performance of any behavior. Learning is not the acquisition of a response,
the acquisition of a response, but the cognitive representation of a predictive relationship (see also, Bolles, 1972).

Reinforcement (used here as a generic term to cover all response consequences), though it may facilitate learning, is not taken to be necessary for learning. Bandura (1974) writes, "The capacity to represent modeled activities symbolically enables man to acquire new patterns of behavior observationally without reinforced enactment" (p. 863). This does not mean that reinforcement is inconsequential. In accounting for performance, as opposed to acquisition, reinforcement serves two major functions in social learning theory. First, reinforcement conveys information to the observer regarding the potential outcome of any given behavior, thereby allowing the observer to develop sets of expectancies or predictive relationships between behavior and its consequences for the actor. Thus, if one observes an actor punishes for a particular behavior, one is less likely to perform that behavior even though the requisite skills are possessed. Second, reinforcement functions as a motivational influence. Behaviors acquire incentive value for the observer as a result of the consequences which are observed to follow from them. Thus, though learning can occur without reinforcement, the learned skills are not likely to be performed unless appropriate reinforcement has been observed to follow from their performance.

It is clear from this discussion that reinforcement
does not have the same meaning in social learning theory as it has in traditional instrumental learning or operant theory. Bandura distinguishes social learning reinforcement from traditional conceptions of reinforcement by emphasizing its vicarious nature. This shift from direct to vicarious reinforcement changes the meaning of the concept. Vicarious reinforcement is not an event which directly affects the strength or frequency of the behavior which preceded it; it involves a judgment by the observer: "If that's what people get for acting that way, I will (or will not) do that." Vicarious reinforcement is then a synonym for reward/punishment in the vernacular and implies awareness.

If learning, in the form of symbolic and representational cognitive coding, can occur without reinforcement and prior to any overt response, and if this learning can guide subsequent behavior, then, of course, if the cognitive coding is changed, the behavior will change. There is ample evidence that modeling procedures developed by Bandura do work (see Marlatt & Perry, 1975 and Rimm and Masters, 1974, for discussions of the clinical application of these procedures). Cautela (1971, cited in Kazdin, 1973) has reasoned that if behavior is represented verbally and imaginally, then altering these directly by covert means would eliminate the need for overt modeling of the desired behavior. Cautela terms the procedure he developed to accomplish this "covert Modeling." Before discussing this
procedure, recent attempts to demonstrate that SD is a cognitive, rather than counter-conditioning therapy are reviewed and their relationship to covert modeling discussed.
SYSTEMATIC DESENSITIZATION AND COGNITION

There have been several recent efforts to interpret SD as a cognitive procedure. This literature is reviewed below and an attempt is made to abstract from these studies those common features which suggest viewing SD as a variation of covert modeling.

Beck (1970) discusses the issues raised by Breger and McGaugh (1965), but from a different perspective, being more concerned with the logical implications of behavior therapy techniques than with the theoretical adequacy of classical conditioning and operant models. Therapeutic procedures are divided into behavioral and cognitive categories on the basis of whether, in Beck's judgment, a tapping of cognitive processes is implied by the procedure. Judged along this dimension, SD is placed in the cognitive group. Bergin (1970) and Locke (1971) agree, Bergin saying, "It is difficult... to imagine how desensitization can be considered to be a 'behavioral' procedure in any definitive sense (p. 206), and Locke writes, "The procedures could hardly be called behavioristic in view of the fact that they all pertain to the patient's conscious experience" (p. 321).

Valins and Ray (1967) and Conger, Conger and Brehm, (1976) report success with cognitive relabeling procedures, reducing avoidance behavior by relabeling subjects' perceptions of their own reactions to feared objects. By leading subjects to believe that they do not react
physiologically to feared stimuli, avoidance of these stimuli can be reduced. These authors interpret this finding as support for the position that SD operates in this manner ("If I am relaxed, I cannot be frightened of the object I am imagining."). SD may work through this sort of mechanism but the experimental procedures used by Valins and Ray or by Conger, et. al. do not demonstrate this. The findings do support the notion that behavior can be changed by changing cognitions.

Other studies have demonstrated the importance of cognitive expectancy for therapeutic behavior change (Brown, 1973; Lott & Murray, 1975; Tori & Woreil, 1973). Usually these studies proceed by exposing different groups to different treatment instructions, then administering SD or some variation thereof, and finally measuring outcomes of the different groups. Though these studies are often interpreted as support for a cognitive interpretation of SD, these interpretations are misguided; they demonstrate the effects instructional set can have on procedures which follos, but these studies are not tests of the theoretical rationals underlying the procedures (Evans, 1973). As with the relabeling studies, the results are suggestive -- if cognitive expectancy can effect the outcome of therapy, perhaps therapy is just a way of changing expectancy -- but they are no more than suggestive. These studies support the hypothesis that different pretreatment procedures designed to manipulate cognitive expectancy do affect the outcome of therapy,
but these studies are not tests of a counter-conditioning explanation of SD. The author could find only one study which reported expectancy alone to be sufficient for producing change. Marcia, Rubin & Efron (1969) compared outcomes of SD and "a technique (T-scope therapy) embodying the expectancy manipulating features of systematic desensitization, but without the technical elements of the procedure" (p. 382). No significant differences in outcome were obtained between the two therapies. The authors point out that of theoretical importance is that none of the T-scope patients got worse, though a classical conditioning model predicts such an outcome.

Wilkins (1971) reviewed several SD studies and summarizes well the position of the proponents of a cognitive mediational approach to behavior change. He writes,

... in light of investigation reported predominately in the last two years, the original theoretical formulations appear to be inadequate in providing an understanding of the nature of the learning involved in desensitization. Strong trends in the recent evidence indicate that neither relaxation training nor hierarchy construction are [sic] necessary for successful therapy outcome. It appears that the only necessary element of the desensitization procedure is the cognitive element of instructed imagination of fear-relevant scenes.

It appears that the cognitive and social aspects of the therapeutic situation... are critical variables for successful therapeutic outcome. [pp. 315-316].

Though the studies reported here and those reviewed by Wilkins are suggestive of this hypothesis (of the cognitive relabeling and expectancy studies discussed here, all have
imagination of fear-relevant scenes in common), none are definitive tests of a cognitive explanation of SD. There are, however, other studies which lend additional support to the position that imagination of fear-relevant scenes is the crucial variable.

In a study of non-specific factors in behavior therapy, Gelder, Bancroft, Gath, Johnston, Mathews & Shaw (1973) found a non-specific control treatment which included imagination of fear-relevant scenes to be almost as effective as SD in reducing avoidance behavior. Flooding, which consisted almost exclusively of imagination of fear-relevant scenes, was the most effective treatment. The apparent advantage of SD over the non-specific control could perhaps be accounted for by the concentration of expectancy effects in the SD and flooding groups. Other studies which have imagination of fear-relevant stimuli in common though other parameters of SD are varied include studies of free associating to feared stimuli while relaxed (Wilson & Smith, 1968), directed discussion of hierarchy items while relaxed (Wollersheim, 1974), and temporally standardized presentation of hierarchy items without regard to anxiety signalling (Ross & Proctor, 1973), successful procedures all.

Lazarus (1971a) working within a clinical framework, has expressed dissatisfaction with the "old method" of SD and suggests that it can be made more effective if a wide
range of cognitive and affective variables is taken account of and incorporated into the therapeutic procedures.

Not all writers agree about the usefulness of cognitive concepts. Wolpe (1973) reviews many of these same studies and rejects them as support for a cognitive interpretation of behavior therapy, citing Sushinsky and Bootzin (1970) and Kent, Wilson & Nelson (1972) as examples of research that is methodologically superior to the cognitive studies and that support classical conditioning theory. Kent, et. al. do support Wolpe, but Sushinsky and Bootzin question only the technical contribution made by the cognitive workers, while agreeing that there is evidence to indicate that cognitive factors might be important for therapeutic change. Davison (1968), at one time, reported "strong support for the hypothesis that behavior change produced by systematic desensitization reflects a counter-conditioning process" (p. 96). More recently, writing with Valins (Davison & Valins, 1969) and with Lazarus (Lazarus & Davison, 1971), Davison assumes a favorable position toward the hypothesis that therapeutic behavior change can sometimes be attributed to cognitive variables.

Nawas (1970) and Ullman (1970) also express some reluctance to accept a cognitive view of behavior therapy procedures, though neither suggests that cognitive concepts have no place in psychology. Ullman proposes that if cognitions are treated as behavior, the behavioral/cognitive conflict
would be resolved. Allowing that covert behavior can influence overt behavior has implications beyond those discussed by Ullman. Cognitive behavior, as was pointed out above, may be qualitatively different from overt behavior, that is, it may mediate overt behavior, and follow different laws. Simply assuming that human cognitive behavior is analogous to overt behavior obtained in an animal laboratory solves no problems. Nawas also suggests that cognitions be treated as behavior but seems more liberal than Ullman. He defends the S-R perspective as, for the present, the most profitable model to follow, but says, "Cognitions are not irrelevant; and once empirical and fecund evidence is presented to confirm such a view, only the inept or the bigot would dismiss it" (p. 368).

The research reviewed here, though not a clear-cut test of a cognitive interpretation of SD, clearly supports the position that cognition is, indeed, not irrelevant. Instructed imagination has been offered as a relevant parameter for affecting therapeutic change, but the problem of fitting this into a theoretical framework remains. Two SD studies remain to be reviewed which provide clues that are helpful for solving this problem.

Burgraff (1974) found SD as effective as traditional sorts of speech therapy for reducing stuttering. Unless stuttering can somehow be shown to be a learned avoidance reaction, there seems little reason why SD should function
to reduce it. That is, the topography of the stuttering situation is only metaphorically similar to the experimental neurosis situation through which Wolpe's cats suffered. Stuttering, however, could be a behavioral expression of anxiety, SD succeeding by teaching the stutterer to relax in those situations which elicit this particular unadaptive response. If the SD hierarchy is "a list of stimulus situations to which a patient reacts with graded amounts of anxiety," and if stuttering is a behavioral expression of anxiety, stuttering should increase as anxiety increases. No such relation was discovered in this study. Perhaps these results could be interpreted as due to the opportunity provided by the SD situation for imaginal self-modeling of normal speech.

Mann (1972) reports equal success with three techniques for treating test anxiety. One group of students was shown a videotaped SD session of a fellow student being treated for test anxiety and instructed to follow the therapist's instruction and to imitate the client. A second group saw the same session but was instructed only to concentrate on the content of the session. The third group saw only the client progressing through the hierarchy and told to concentrate on the content of the items. Features required by a counter-conditioning model are absent for this third group (there is no competing response nor anxiety signal). Standard modeling did not occur, as Mann suggests, there
being no modeling of test taking behavior. Again, imagi- nal modeling seems one likely candidate.

Covert modeling (CM) is the instructed imagination of a model engaged in the performance of some target behavior. Though it developed independently of SD research, deriving from social learning theory, the rationale underlying CM seems to fit recent findings from SD experiments very well. The element which SD research seems to suggest as necessary and sufficient for therapeutic success, instructed imagination of fear-relevant scenes, is included in CM procedure, and CM has been demonstrated successful in reducing avoidance behavior (Cautela, Flannery & Hanley, 1974; Kazdin, 1974a, 1974b, 1973), though there have been no direct comparisons of CM and SD.

Though a full discussion is beyond the scope of this paper, a short note regarding SD and extinction models seems in order. The typical classical conditioning extinction generally pairs an unconditioned stimulus with a conditioned stimulus (such as a light with an electric shock) until the subject comes to respond to the conditioned stimulus as it initially responded to the unconditioned stimulus. During the extinction phase, the conditioned stimulus is presented repeatedly without the unconditioned stimulus until it no longer elicits the response. This paradigm would seem to explain very well what occurs during SD except that the results from avoidance learning experiments do not support extinction as a rapid method to eliminate
avoidance behavior (Solomon, 1964), unless response prevention or forced exposure is used (Masserman, 1942). However, as seen above, if these procedures are introduced into the extinction paradigm, the choice between a classical conditioning and cognitive expectancy explanation of the subsequent reduction in avoidance behavior is not so clear as when extinction is used alone. Classical conditioning predicts that once an avoidance response is firmly established, it should immediately begin to extinguish, since the subject is no longer experiencing the aversive stimulus. Except when extremely powerful aversive stimuli are used, this does occur. However, clinical phobias of very long standing can be rapidly eliminated with SD. Response prevention or forced exposure can speed up the extinction process, but SD does not incorporate these procedures, avoidance being freely available to the subject in the form of the anxiety signal. Since avoidance can occur, the question remains of why SD "extinction" proceeds more rapidly than the normally expected extinction observed in most avoidance behavior experiments. It can be argued that the competing response functions to speed up the extinction. As was pointed out above, neither relaxation nor the anxiety signal is necessary for successful desensitization with SD. If these are removed from the SD procedure, the remaining treatment resembles flooding, a treatment that is a much better analogy to respondent extinction than is SD. However, experimental support for flooding as the treatment
of choice over SD is weak (Rimm & Masters, 1974), leaving the question of the role played by respondent extinction in SD open.

Operant extinction experiments typically proceed in the following manner. A particular response is followed by a reinforcing stimulus (e.g., a bar press followed by a food pellet). Once this response is occurring regularly, the reinforcing stimulus is withdrawn, resulting in the extinction of the response. In SD, if the avoidance response is taken as the operant to be extinguished, an operant extinction paradigm does not fit the SD procedure. The client is not required to make the response which is to be extinguished and it is unclear exactly what reinforcing stimulus is being withheld. Operant extinction is strongly influenced by the individual history of the subject (Skinner, 1953), so that unless similar developmental histories relating to the acquisition of phobic responses can be demonstrated across subjects, the success of SD suggests the operation of processes other than operant extinction. Kazdin (1973) has compared an imaginal operant extinction procedure to CM and found it ineffective, though it should be pointed out that the temporal parameters manipulated by Kazdin might be outside the range required for effective extinction. Nevertheless, it appears that, as with respondent extinction, the role of operant extinction in SD remains open.
A PROPOSAL

Interpreting SD as a covert modeling procedure appears reasonable and recent SD research supports this interpretation. Unless an experimental test can be made of this interpretation, it remains largely speculative. The following are some suggestions for making this comparison.

Subjects would ideally be drawn from a clinical population (Bernstein & Paul, 1971), discuss the dangers of using volunteer subjects to test clinical procedures, but in order to standardize assessment procedures and hierarchy content across subjects, volunteer subjects who report having fear of an easily specifiable stimulus (e.g., harmless snakes) are acceptable for an initial test. Subjects should be screened using an instrument such as the Fear Survey Schedule to insure high fear levels. The screening instrument should not be part of the pre-treatment test battery.

Subjects should be randomly assigned to two groups; one group to undergo pre-testing, the second group to receive treatment without pre-testing. From each of these groups subjects could be assigned to an SD group, a CM group, and an exposure-control (E-C) group. This procedure results in a 3 x 2 analysis of variance design.

Initial instructions for all groups are the same, stating that the procedure used is widely accepted as effective for reducing fears and avoidance behavior and explaining
the rationale for using imaginal processes. However, the
S-C group is told that there will be a waiting period dur­ing which they will discuss their fears with a therapist,
the subjects being allowed to control the content of the
discussion. The most effective treatment should be made
available to them later.

All groups should be given relaxation training and
should construct hierarchies.

The SD group receives standard SD. CM and E-C sub-
jects should be yoked to the SD subjects to control for
time in therapy. CM subjects are given no instructions to
relax nor is an anxiety signal used. Hierarchy items are
presented to them in random order, each subject being asked
to imagine a model performing the behaviors specified in
the scene, and to imagine the scene as if they are there
with the model. The E-C group meets with a therapist and
discusses the hierarchy items, again arranged into random
order.

Post-test assessment (identical to the pre-test)
should include a behavioral approach test, and might also
include such items as measures of self-competence and in-
ternal-external locus of control (see Bandura, 1975, for
discussion of a comprehensive assessment plan). Ideally,
follow-up testing should be included.

A counter-conditioning model predicts that the SD
group will improve. This model also predicts that the CM
group will get worse, because anxiety provoking stimuli
are presented without a competing response, and that no change will be observed in the E-C group, because this procedure includes procedures associated with neither desensitization nor sensitization (i.e., though fear relevant scenes, in general, are being discussed, avoidance can operate since the subjects are in control of the specific content of the session).

Social learning theory predicts that both the CM and the SD groups will improve, because both procedures include imaginal modeling of the phobic behavior. No change in the E-C group is predicted since the relevant therapeutic variable is not being manipulated.

This experiment could provide supporting evidence for rejecting a counter-conditioning account of SD in favor of a social learning interpretation.
CLINICAL IMPLICATIONS

Assuming experimental support for the position taken here, some important implications for clinical practice should be noted. Some of these implications, while important to the clinician, are only tangentially related to applied matters. For example, a wide range of techniques might come to be subsumed under the same theoretical rationale; in addition to SD, procedures such as role reversal, role playing (self-modeling), and psychodrama, all seem likely candidates as modeling procedures. Because social learning experiments require a shorter "analogic leap" to reach the clinical side than do traditional classical conditioning or operant experiments, the credibility of the claim by behavior therapists for experimental support would be much less strained than at present. Some contributions of this sort of research would be more directly related to applied concerns. If guides to behavior are cognitively coded in verbal form as social learning theory suggests, verbal therapy might be made more effective by teaching therapists to model appropriately verbal representations of target behaviors until the client can comfortably replicate this performance (see Goldiamond and Dryud, 1968, for an interesting discussion of the "influence exerted by verbal behaviors over other behavior in general.")
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