1 A BRIEF HISTORY OF COGNITIVE BEHAVIOR THERAPY: ARE THERE TROUBLES AHEAD?

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In its beginnings, behavior therapy was linked to learning research in an inextricable and unique manner. I will refer to this period in the history of behavior therapy as ''first-generation behavior therapy.'' First-generation behavior therapy was a scientific paradigm that resulted in important solutions to a number of clinical problems (Task Force on Promotion and Dissemination of Psychological Procedures, 1995). For various reasons, however, many behavior therapists and researchers lost touch with developments in conditioning research and theory. Over the last three decades, behavior therapists turned their attention to topics such as therapies based on ''clinical experience'' (e.g., Goldfried & Davison, 1976), techniques seen independently from underlying behavioral principles (Hayes, Rincover, & Solnick, 1980), cognitive experimental psychology, cognitive accounts not based on experimental cognitive psychology (e.g., Ellis & Harper, 1975), and integrating or borrowing from other therapeutic approaches (Lazarus, 1969; but see O'Donohue & McKelvie, 1993). I will collectively refer to these developments as ''second-generation behavior therapy.''

Often, the argument in second-generation behavior therapy for this widening of influences was that ''some clinical problem has not yielded to a conditioning analysis; therefore, other domains need to be explored for solutions.'' This is a reasonable argument, as it is imprudent to restrict behavior therapy to conditioning if there are important resources in other domains. However, there are grounds for concern because second-generation behavior therapists may have relied too heavily on these other domains to the extent that contemporary learning research extends older research, contradicts older research, or has discovered completely new relationships and principles. Clinical problems may be refractory to behavioral treatment simply because the behavior therapist is not using the more powerful regularities uncovered by recent learning research. It is possible that one of the core ideas—extrapolating results from learning research—of first-generation behavior therapy still remains a useful animating principle for contemporary therapy.

However, many contemporary behavior therapists still look to conditioning principles and theory developed in the 1950s and 1960s for solutions to clinical problems. In this chapter, *third-generation behavior therapy* is called for. Third-generation behavior therapists should extrapolate contemporary learning research to understand and treat clinical problems. Third-generation behavior therapy should rely on regularities found in modern accounts of classical conditioning, latent inhibition, two-factor theory, response-deprivation analysis of reinforcement, behavioral regulation, matching law, other models of choice behavior, behavioral momentum, behavioral economics, optimization, adjunctive behavior, rule-governed behavior, stimulus equivalence, and modern accounts of concept learning and causal attribution.

FIRST-GENERATION BEHAVIOR THERAPY

Prior to the 1960s, the founders of behavior therapy extrapolated laboratory learning results to clinical problems. For example, John Watson and Rosalie Rayner (1920) attempted to demonstrate that a child's phobia could be produced by classical conditioning. Mary Cover Jones (1924a, b) showed that a child's fear of an animal could be counterconditioned by the pairing of the feared stimulus with a positive stimulus. O. Hobart Mowrer and Willie Mowrer (1938) developed a bell and pad treatment for enuresis that conditioned stimulus for sphincter control and the inhibition of urination.

Despite the initial promise of these early extrapolations, these efforts were generally ignored in clinical practice. Psychotherapists of the period were largely interested in psychoanalysis, a paradigm with a much different focus. Behavior therapists had to compete with the many offshoots of psychoanalysis. Andrew Salter (1949) shows some of the antipathy that many behavior therapists had toward psychoanalysis:

> It is high time that psychoanalysis, like the elephant of fable, dragged itself off to some distant jungle graveyard and died. Psychoanalysis has outlived its usefulness. Its methods are vague, its treatment is long drawn out, and more often than not, its results are insipid and unimpressive. Every literate non-Freudian in our day knows these accusations to be true. But we may ask ourselves, might it not be that psychotherapy, by its very nature, must always be difficult, time-consuming, and inefficient? I do not think so. I say flatly that psychotherapy can be quite rapid and extremely efficacious. I know so because I have done so. And if the reader will bear with me, I will show him how by building our therapeutic methods on the firm scientific bed rock of Pavlov, we can keep out of the Freudian metaphysical quicksands and help ten persons in the time that the Freudians are getting ready to ''help'' one. (p. 1)

In the 1950s, Joseph Wolpe (1958) attempted to countercondition anxiety responses by pairing relaxation with the stimuli that usually elicited anxiety. Wolpe's work represents the real beginnings of modern behavior therapy, as his work comprised a sustained research program that affected subsequent clinical practice. The earlier work of Watson, Jones, and others was not as programmatic and for whatever reasons did not disseminate well. Wolpe's desensitization techniques and his learning account of fears generated dozens of research studies and clinical applications over the following decade. The reader is referred to Kazdin's (1978) excellent history of behavior therapy for additional examples of early learning-based therapies.

First-generation behavior therapists not only utilized learning principles to formulate interventions, but also used learning principles to develop accounts of the origins and maintenance of problems in living. Abnormal behavior was judged to develop and be maintained by the same learning principles as normal behavior (e.g., Ullmann & Krasner, 1969). Problems in learning or problems in maintaining conditions resulted in a variety of behavior problems. Ullmann and Krasner's (1969) textbook on abnormal behavior is a useful compendium of first-generation learning-based accounts of the development and maintenance of changeworthy behavior.

Most of the initial behavioral studies were influenced by Pavlovian principles, particularly simultaneous and forward classical conditioning. This is not surprising, as some of these predated Skinner's work on operant conditioning. However, in the 1950s, another stream of behavior therapy emerged: applied behavior analysis or behavior modification. These interventions relied on operant principles. In one of the first studies to explicitly use operant principles, Lindsley, Skinner, and Solomon (1953) initiated this stream when they operantly conditioned responses in schizophrenics, demonstrating that psychotic disorders did not obviate basic conditioning processes. Another important early operant researcher, Sidney Bijou (e.g., Bijou, 1959) investigated the behavior of both normal and developmentally delayed children through the use of functional analyses and schedules of reinforcement. Baer, Wolf, and Risley (1968) in the first issue of the *Journal of Applied Behavior Analysis* highlighted the importance of the systematic and direct

application of learning principles for the future of applied behavior analysis:

> The field of applied behavior analysis will probably advance best if the published descriptions of its procedures are not only precise technologically but also strive for relevance to principle. . . . This can have the effect of making a body of technology into a discipline rather than a collection of tricks. Collections of tricks historically have been difficult to expand systematically, and when they were extensive, difficult to learn and teach. (p. 96)

These cases of first-generation behavior therapy exhibit several important commonalities:

- The clinical scientists had extensive backgrounds in basic learning research. They could reasonably be described as learning researchers seeking to understand the generalizability of laboratory research as well as examining the practical value of this research by helping to solve problems involving human suffering.
- They were applying *what was then current learning research* to clinical problems.
- The results of their clinical research were by and large positive, although the methodological adequacy is problematic by today's standards.
- They saw their particular research as illustrating a much wider program of research and therapy. That is, their research did not exhaust the potential for the applicability of learning principles to clinical problems, but merely illustrated a small part of a much wider program.

During this period, behavior therapy was often defined by a direct and explicit reference to learning principles. For example, Ullmann and Krasner (1965) defined behavior modification as ''includ[ing] many different techniques, all broadly related to the field of learning, *but learning with a particular intent, namely clinical treatment and change*'' (p. 1; italics in the original). Wolpe (1969) stated, ''Behavior therapy, or conditioning therapy, is the use of experimentally established principles of learning for the purpose of changing maladaptive behavior'' (p. vii). Eysenck (1964) defined behavior therapy as ''the attempt to alter human behavior and emotion in a beneficial manner according to the laws of modern learning theory'' (p. 1). Franks (1964) stated, ''Behavior therapy may be defined as the systematic application of principles derived from behavior or learning theory and the experimental work in these areas to the rational modification of abnormal or undesirable behavior'' (p. 12). Furthermore, Franks (1964) wrote that essential to behavior therapy is a ''profound awareness of learning theory'' (p. 12).

Although by and large these early behavior therapists agreed that learning principles should serve as the foundation of behavior therapy, the behavior therapy they advocated was not homogeneous. There was a significant heterogeneity in this early research. These researchers did not draw upon the same learning principles, nor did they subscribe to the same theory of learning. Skinner and his students emphasized operant conditioning principles; Watson, Rayner, and Jones, Pavlovian principles; and Wolpe and others, Hullian and Pavlovian. Moreover, within these broad traditions, different regularities were used: Some used extinction procedures, others excitatory classical conditioning; some differential reinforcement of successive approximations, others counterconditioning. However, each of these is a canonical illustration of behavior therapy of this period because each shares a critical family resemblance: an extrapolation of learning principles to clinical problems.

A related but separate movement occurred during this period. This movement did not gather much momentum and has largely died out. It is best represented by the work of Dollard and Miller (1950). In their classic book, *Personality and Psychotherapy*, these authors attempted to provide an explanation of psychoanalytic therapy techniques and principles based on learning principles. Dollard and Miller attempted to explain psychoanalytic techniques by an appeal to Milian learning principles. This movement should be regarded as separate from the first movement described earlier because the connection between conditioning and a therapy technique in this movement is post hoc. That is, first, therapeutic principles are

described with no direct connection to learning principles, and this is followed by an attempt to understand these by learning principles. In the first movement, initially learning principles are discovered, and this is followed by the development of treatment procedures.

Today, there is little work that follows the second paradigm. Few are attempting to uncover the learning mechanisms underlying Rogerian and Gestalt techniques, object-relations therapy, and the like. This is probably because today, unlike the 1950s, there is more doubt regarding whether there is anything to explain. This movement attempted to explain, for example, how psychoanalysis worked (the conditioning processes involved). However, if there is little reason to believe that these other therapies are effective, then there is little reason to explain how they work. Moreover, this movement failed to produce any novel treatment techniques. In its emphasis on attempting to understand existing therapy techniques, it produced no useful innovations.

However, the model of moving from the learning laboratory to the clinic proved to be an extraordinarily rich paradigm. In the 1960s, numerous learning principles were shown to be relevant to clinical problems. Learning research quickly proved to be a productive source of ideas for developing treatments or etiological accounts of many problems in living. The development of psychotherapy had been a quasi-mysterious process before this point. Psychotherapies were usually developed by the unique clinical observations of the person who would become the leader of the school. Psychotherapists were no longer dependent on the ''revelations'' of insightful and creative seers who founded their schools. For the first time, psychotherapists could do Kuhnian (Kuhn, 1970) normal science because it is considerably more straightforward to extrapolate extant learning principles to clinical phenomena than it is to understand how, say, Freud formed and revised his assertions. ''Extrapolate learning principles'' is a clear and useful heuristic for the context of discovery.

Six books were critically important in extending the learning-based therapy paradigm. Wolpe's (1958) *Psychotherapy by Reciprocal Inhibition;* Eysenck's (1960) *Behavior Therapy and the Neuroses;* Franks's (1964) *Conditioning Techniques in Clinical Practice and Research;* Eysenck's Experiments in Behavior Therapy (1964); and Krasner and Ullmann's two volumes, *Case Studies in Behavior Modification* (1965) and *Research in Behavior Modification* (1965). All contained an extensive set of case studies, research, and conceptual analyses that greatly extended the paradigm. Conditioned reinforcement, modeling, generalization and discrimination, satiation techniques, punishment, the effects of schedules of reinforcement, and token economies were investigated. Moreover, these principles were applied to a greater number and variety of clinical problems. Eating, compulsive behavior, elective mutism, cooperative responses, disruptive behavior, anorexia, hysterical blindness, posttraumatic anxiety, fetishism, sexual dysfunction, stuttering, tics, school phobia, tantrums, toilet training, social isolation, teaching skills to people with mental retardation, and hyperactive behavior were all addressed by learning-based treatments in these books. The matrix involving the crossing of learning principles by kinds of problematic behavior resulted in a rich research and therapy program.

Due to the initial successes in applying learning principles to clinical problems, another trend emerged. First-generation behavior therapists started working in the other direction: they began with a clinical problem and then attempted see to what extent it yielded to an analysis based on learning principles. Thus, a reciprocal relationship between the clinic and the learning lab emerged. This movement was important because behavior therapists can also be interested in uncovering basic learning processes in humans and can have a useful vantage point for generating and testing hypotheses concerning basic processes.

However, there is some danger with this approach. Unfortunately, it could be quite attractive to the behavior therapist who knew much more about clinical presentation than about learning research. This may have been the beginnings of the reliance of behavior therapists on something other than a thorough and faithful knowledge of current learning theory and research. With the success of behavior therapy came a new kind of professional: one who was

first trained to be a clinical behavior therapist rather than a learning researcher.

Care must be taken not to lose sight of another important dimension of first-generation behavior therapy: its commitment to science and research. This scientific commitment, although not unprecedented in the history of psychotherapy, was more thoroughgoing. In 1952, after more than a half-century of the dominance of psychotherapy by psychoanalysis, Eysenck correctly pointed out that there was little properly controlled research that demonstrated it was more effective than a placebo treatment. Part of Eysenck's thesis was that it may be the case that effective therapies had yet to be discovered. However, another part was that existing therapies had not been adequately evaluated with properly controlled designs. Psychotherapists were doing an inadequate job as clinical researchers by not evaluating the efficacy of their therapies.

Admittedly, many of the early reports of behavior therapy were largely uncontrolled case studies that merely demonstrated its potential utility. Behavior therapists, however, quickly began to conduct unprecedentedly well-controlled research. Paul's (1966) study of the effectiveness of systematic desensitization can properly be regarded as the first research in history that was sufficiently well controlled to demonstrate that a form of psychotherapy was more effective than placebo and no treatment.

The research orientation of behavior therapists may have emanated from the school's roots in conditioning theory and research. Many then-extant forms of therapy had a much different heritage: the founder of the particular school made what were taken by some as astute clinical observations (witness Freud, Perls, Rogers; see O'Donohue & Halsey, 1997) and somehow formed this clinical experience into a more or less systematic school of therapy. It is easier to be ''looser'' when one is not extrapolating from a basic science. In contrast, the learning researchers/behavior therapists who composed the first wave of behavior therapy did not give up their experimental orientation when turning their attention to clinical problems. Behavior therapy from its beginnings valued science. The epistemological principles from their backgrounds in experimental psychology remained with them and became an important part of the metascience of behavior therapy. Behavior therapists were interested in process research because they had a strong prior set of expectations (i.e., learning principles) of what these process variables might look like.

First-generation behavior therapy resulted in unprecedented progress in psychotherapy. If we somewhat arbitrarily say that the modern era of psychotherapy began in roughly 1900 with Freud, then we can agree with Eysenck (1952) in that the first 50 years of psychotherapy resulted in little progress. No treatments were developed that effectively resolved the problems they attempted to address. In contrast, during the early years of behavior therapy, significant progress was made with enuresis, phobias, other anxiety problems, child management problems, skill deficits of developmentally disabled individuals, self-injurious behavior and stereotypic behavior of autistic and schizophrenic individuals, and social and verbal problems of schizophrenia. These all were no longer completely refractory to ameliorative attempts. Moreover, as Salter (1949) described, behavioral treatment was also much quicker and less costly. In the span of a little over a decade, psychotherapy made progress that it failed to make in the preceding five decades. Surely, any reasonable observer could see that there was something special about this new movement. Today, if one looks at the Task Force on Promotion and Dissemination of Psychological Procedures (1995), this first-generation behavior therapy still accounts for a significant percentage of what are now considered ''validated treatments.''

The success of early behavior therapy should not have come as a complete surprise. Psychotherapists for the first time began using a strategy that had proved successful in other domains. For nearly a century, physicians had relied on experimental physiology and microbiology, and by extrapolating from the results of the basic biological sciences they had made significant clinical progress. Engineers relied on the basic sciences of physics and chemistry and made remarkable progress solving many applied problems. The strategy used by these groups was enticing: Extrapolate

antecedently validated principles from basic research to applied problems.

For the first time in the 1950s and 1960s, psychotherapy began to use the same strategy: first nomothetics were discovered through basic research, and then these were applied to practical problems. In the learning laboratory, learning researchers derived principles applicable to human behavior. The animals used in their research were largely chosen for convenience rather than because of any strong interest in understanding the behavior of that particular species. Evolutionary theory supported some behavioral continuity across species, which further justified the study of infrahuman animals. The laboratory and the animal preparation allow control that is not possible in naturalistic studies of humans. Variables can be controlled and isolated, and thus false hypotheses can more easily be refuted. Regularities emerging from the learning laboratory have relatively good epistemic credentials and a reasonable potential for revealing clinically useful regularities. The epistemic credentials of the laboratory-derived first-generation behavior therapy were far superior to the epistemic credentials of principles or regularities alleged by the clinical observers who initiated competing schools of therapy. The number of possible therapy techniques is, of course, indefinitely large, and therefore it is useful to have antecedent evidence on which to judge which are worthy of investigation (Erwin, 1978).

An additional, somewhat more subtle, factor may also have contributed to the success of first-generation, learning-based therapies. This paradigm may have met with such unprecedented success because of felicitous correspondences between the core objects of both programs. Learning researchers attempt to uncover how experience changes behavior. In fact, a common definition of learning is that learning is experience that results in relatively enduring changes in behavior. This focus precisely addresses the general question involved in the enterprise of psychotherapy: How can therapists structure experience so that relatively enduring changes occur in the client's behavior? Thus, this paradigm might have been successful because of the confluence of the aims of these two pursuits.

Two further confluences might have accounted for the success of operant approaches. Skinner criticized research utilizing group designs. He argued that group averages are a confused and confusing scientific variable. Instead of group comparisons, Skinner argued for the intensive experimental analysis of the behavior of an individual organism. The goal was to find the controlling variables of the individual's behavior by manipulating environmental conditions to see if these were functionally related to subsequent behavior. Again, this emphasis is highly consistent with the clinician's problem situation. The clinician is rarely concerned with group averages, but rather is concerned with the behavior of an individual client. Moreover, clinicians aim to find manipulable conditions to bring about desirable changes in the client's behavior.

A final confluence was that in conducting these single-subject designs, Skinnerians eschewed statistical analysis. They wanted to show that they had identified controlling variables due to the reliable, high-magnitude changes produced in the dependent variable. Although some learning researchers statistically analyzed group designs in order to find ''statistically significant'' differences, operant researchers wanted to demonstrate differences that would be readily apparent in any graphical display. This is fortuitous because clinicians generally want or need dependent variables to undergo large changes. The work coming out of the operant lab showed that these large changes were possible. Work coming out of group designs showed that with large enough sample sizes, small differences (that were statistically significant but often not clinically significant) were possible.

Despite the considerable advantages provided by this basic science/applied science model, it has one serious disadvantage. The limits of the basic science place limits on the applied science. Learning research was (and still is) unsettled. Pavlovians, Ruffians, and Skinnerians, among others, engaged in debates concerning fundamental issues. Much of the behavior of the organism remained unaccounted for. There was a clear need for further basic research to fill the many lacunae in the learning account. At

times, behavior therapists were stymied because they relied on incorrect information, incomplete information, regularities that were weak, and regularities whose initial conditions or boundary conditions were poorly understood.

SECOND-GENERATION BEHAVIOR THERAPY

In the 1970s, behavior therapy's heterogeneity increased. Systematic desensitization, implosion therapy, and two-factor accounts of anxiety disorders were examples of the continuing influence of Pavlov, Hull, and Mowrer, respectively. Those influenced by Skinner sometimes tried to distinguish themselves from those influenced by nonoperant principles and particularly from those influenced by nonconditioning factors. Operantly inclined behavior therapists sometimes called what they did *applied behavior analysis* or *behavior modification*. These terminological distinctions have not always been clear, but at times they function as code words for background allegiances regarding favored learning principles. The increasing diversity of behavior therapy should not be surprising, as the seeds for the growth of a heterogeneous discipline were present from its beginning. For example, Ullmann and Krasner (1965) described behavior therapy as ''treatment deducible from the *sociopsychological model* that aims to alter a person's behavior directly through the application of general psychological principles (p. 244, italics added). These prominent, early behavior therapists viewed behavior therapy as also relying on many social–psychological domains such as role theory, small-group research, demand characteristics, labeling, and conformity. Ullmann and Krasner attempted to set a learning-influenced behavior therapy in the larger context of a psychology of behavior influence.

Gerald Patterson (1969), another prominent early behavior therapist, agreed with the emphasis on social–psychological principles:

> It seems to me that future trends will of necessity involve a greater reliance upon principles available from social learning. The term social learning as used here refers to the loosely organized body of literature dealing with changes in learning, or performance, which occur as a function of contingencies which characterize social interaction. . . . Many of the mechanisms which have been described as bringing about these changes have been based upon principles from social psychology rather than learning theory: these would include such processes as persuasion, conformity, and modeling. (p. 342)

Arnold Lazarus, a student of Wolpe's, was probably one of the earliest and most significant forces for turning behavior therapists' attention to areas other than learning. Lazarus argued that learning principles were helpful but insufficient. Lazarus (1968) stated:

> Why should behavior therapists limit themselves only to ''experimentally established principles of learning against the background of physiology'' and ignore other areas of experimental psychology such as studies on perception, emotion, cognition, and so forth? And why should behavior therapists avoid using such techniques as self-disclosure, dyadic interactions, and other methods, as long as they can be reconciled with reinforcement principles? Finally, one might inquire to what extent Wolpe's reference to a ''stimulus–response model'' is a vague and meaningless abstraction. If the current upsurge of interest in behavior therapy is to expand and mature, we must beware of oversimplified notions, limited procedures, and extravagant claims which would conceivably undermine our efforts. (p. 2)

Following this line of thought, Lazarus (1969) stated that the multimodal behavior therapist is ''free to employ any technique, derived from any system, without subscribing to any theoretical underpinnings which do not have the benefit of empirical support'' (p. 5).

Bandura's description and analysis of modeling and vicarious learning was another important influence on the development of behavior therapy during this period. Bandura (1969) stated that:

> . . . research conducted within the framework of social-learning theory demonstrates that virtually all learning phenomena resulting from direct experiences can occur on a vicarious basis through

> observation of other persons' behavior and its consequences for them. Thus, for example, one can acquire intricate response patterns merely by observing the performances of appropriate models; emotional responses can be conditioned observationally by witnessing the affective reactions of others undergoing painful or pleasurable experiences; fearful and avoidance behavior can be extinguished vicariously through observation of modeled approach behavior toward feared objects without any adverse consequences accruing to the performer; inhibitions can be induced by witnessing the behavior of others punished; and finally, the expression of well-learned responses can be enhanced and socially regulated through the actions of influential models. Modeling procedures are therefore ideally suited for effecting diverse outcomes including elimination of behavioral deficits, reduction of excessive fears and inhibitions, transmission of self-regulating systems, and social facilitation of behavioral patterns on a groupwide scale. (p. 118)

Together, these authors argued that social psychology and experimental learning psychology were relevant to behavior therapy. It is also fair to say that many of those influenced by the social-learning perspective relied most heavily on learning principles. Growing from these early seeds, in the second generation, behavior therapy became more broadly defined. Instead of defining behavior therapy as the application of learning principles, behavior therapy came to be defined as the application of principles from experimental and social psychology (e.g., Davison & Neale, 1974; Rimm & Masters, 1974; Franks & Wilson, 1975). This, of course, included learning principles, but it also included a lot of other material.

During this period, applied behavior analysts appeared to become less attentive to the underlying learning principles. Hayes, Rincover, and Solnick (1980) found that in early volumes of the *Journal of Applied Behavior Analysis* it was nearly always the case that the articles contained references to behavioral principles. However, in an analysis of later volumes, Hayes et al. found:

> Overall the data show that applied behavior analysis is becoming a more purely technical effort, with less and less interest in conceptual questions. To answer these technical questions we are using relatively simple experimental designs which determine if the technique had a reliable effect, or if it is better than another technique, with little interest in the components producing the effect or the parametric boundaries of the techniques. (p. 281)

THE RISE OF COGNITIVE BEHAVIOR THERAPY

Behavior therapy is not insulated from events happening outside it. The ''cognitive revolution'' in psychology occurred in the 1960s, and by the 1970s many behavior therapists influenced by it began to call what they did ''cognitive behavior therapy.'' Wilson (1982) stated:

> During the 1950s and 1960s, the behaviour therapies developed within the framework of classical and operant conditioning principles that had originally served importantly to distinguish behaviour therapy from other clinical approaches. Over the course of the 1970s, this conceptual commitment to conditioning theory peaked out—some would say even waned. In part this change reflected the shift to more technological considerations governing the increasingly broad application of behavioral techniques that had been developed and refined during the previous period of growth. Moreover, as psychology ''went cognitive'' during the 1970s, cognitive concepts inevitably were drawn upon to guide and explain treatment strategies. (p. 51)

Mahoney, an early leader in cognitive behavior therapy, stated a similar theme (1984):

> By the late 1970s it was clear that cognitive behavior therapy was not a fad; indeed it had its own special interest group in the AABT. It had become a more frequent topic at conventions, in journals, and in research, and it had become more pervasively integrated into behavioral psychotherapies. Behavior therapy, like psychology in general, had ''gone cognitive.'' (p. 9)

Part of this movement argued that learning research was still relevant but the research that should influence second-generation behavior

therapy was *human* learning research that examined cognitive mediators of learning. The argument was that conditioning in humans is not automatic and direct, but rather is mediated by the person's verbal and cognitive abilities. Awareness, attention, expectancy, attribution, and linguistic representation were constructs thought to be necessary to account for learning. The argument was that animal conditioning models were inadequate for the study of human learning because these neglected to include the unique abilities of humans such as verbal abilities. Thus, these animal conditioning models needed to be supplemented or replaced by cognitive accounts.

Not all behavior therapists ''went cognitive.'' Most applied behavior analysts continued to practice first-generation behavior therapy. These and others argued that the so-called cognitive revolution was in part a retreat to folk psychology rather than a progressive scientific movement. Critics were quick to point out that the new cognitive techniques generally had, at best, a rather loose connection with experimental cognitive psychology. This was serious epistemically because, to the extent that this criticism was true, no longer were behavior therapists extrapolating antecedently tested principles.

It does appear that during this period, behavior therapists developed treatments that had a looser relationship with conditioning: self-reinforcement, behavioral rehearsal, covert sensitization, and thought stopping all were clinical techniques that were not derived from basic animal learning research. Conditioning principles became more of a rough heuristic during the second generation of behavior therapy. Admittedly, these techniques have a family resemblance to conditioning procedures, but their actual connection is much more ephemeral. Claims that there was a shift in regard for basic animal research have some empirical support. Poling et al. (1981) found through a citation analysis that sources that report work with nonhuman subjects have been referenced increasingly infrequently since 1965 by clinical authors.

It also may have been the case that the success and credentials of behavior therapy attracted many individuals, some of whom were relatively unfamiliar with learning principles. Psychotherapists and clinical researchers trained in other paradigms ''converted'' to behavior therapy during this period. However, such conversion rarely entailed an extensive training in learning research. Rather, it more typically included training in behavior therapy techniques themselves. This trend could have hastened the view of these techniques as being more autonomous from the basic learning principles. For this group of behavior therapists, when difficulties were encountered, it was more likely that learning principles were not drawn upon. It is easier for the potential of learning principles to be seen as exhausted when one does not have an exhaustive knowledge of them.

I also conjecture that these less faithful, less accurate extrapolations from basic learning research had a higher likelihood of leading to failures. To the extent that these failures were attributed to the inadequacy or insufficiency of learning principles to gird clinical practice, a movement away from learning and toward other domains occurred. Many behavior therapists have had the experience of hearing psychotherapists say that their failed attempts at what they see as behavior therapy support their conclusions that behavior therapy is a bad form of therapy. I recall an avowed eclectic therapist telling me that behavior therapy failed her because she tried to reinforce an academically underperforming adolescent by rewarding him with a minibike at the end of the semester if he received all As and Bs. If she had even a cursory understanding of operant conditioning, she would have known that:

- One does not reinforce organisms, but rather responses.
- Reinforcement of successive approximations is usually a more effective strategy for producing high-magnitude changes.
- A large, distant reinforcer often needs to be supplemented by more proximate reinforcers.
- Receiving a good grade is not a response.
- A more careful functional analysis of competing behaviors and reinforcers needed to be done to understand controlling variables. Too often during this period, people began to

practice ''behavior therapy'' in a superficial and rather incompetent manner.

This is not to say simply that the growing schism between behavior therapy and basic learning research can be understood entirely by the behavior of behavior therapists. During this period, basic learning research moved on as well. It admittedly became more esoteric, more technical, and thus there were more barriers to entry to those who wanted to acquaint themselves with contemporary learning research. The difficulty of contemporary learning research helps to explain why many behavior therapists failed to keep up. If one picks up a current issue of, for example, *Journal of the Experimental Analysis of Behavior* and attempts to read one of the articles, it is likely that one will understand little. Learning research became more insular as it grew more technical, quantitative, and specialized. Learning researchers stopped writing for general psychologists and wrote increasingly for their scientific microcommunity. Learning researchers began to experience problems in knowledge utilization and dissemination—topics that are of intellectual interest in their own right.

The advent and success of behavior therapy also created certain interpersonal and professional tensions. Behavior therapists were often critical of the lack of evidence for the efficacy of other schools of therapy, of the lack of scientific commitment of these schools, of the lack of evidence that these schools' favored process variables actually were important, and of the way these schools defined *abnormality*. Part of the general ethos of the psychotherapy movement is to have good interpersonal relationships. But behavior therapists were increasingly critical, skeptical, and unaccepting of many of the claims of other schools, and, frankly, claimed to be practicing a superior form of therapy. These tensions were at least partly relieved when behavior therapists became more eclectic, less stridently learning based, and accepting of techniques from other schools. If one looks at some of the external forces on the development of behavior therapy, one problem behavior therapists had to face was this sort of ''foreign relations.'' Some sought appeasement by compromise. Eclecticism may be understandably more satisfying in certain political and interpersonal contexts.

Probably the most radical critique of first-generation behavior therapy during this period was the criticism that behavior therapy techniques were not derived from basic laboratory principles of learning. For example, Breger and McGaugh (1965) stated, ''When we look at the way conditioning principles are applied in the explanation of more complex phenomena, we see that only a rather flimsy analogue bridges the gap between such laboratory defined terms as stimulus, response, and reinforcement and their reference in the case of complex behavior'' (p. 344). Erwin (1978) also argued that behavior therapy techniques were not derived from learning principles. For example, the argument was that in systematic desensitization, Wolpe used an imagined scene as a conditioned stimulus but that this conditioned stimulus did not have properties that laboratory conditioned stimuli have—for example, public observability, direct control by the experimenter, and invariance. Thus, the claim was that animal laboratory research often could serve as a heuristic or useful analog but that behavior therapy techniques were not derived from basic animal learning research.

This argument presents a restrictive view of the relationship between basic and applied research. It is an elementary methodological point that laboratory research trades off external validity for internal validity. Laboratory protocols simplify in order to isolate and improve control of independent variables, and to improve the accuracy of measurement of dependent variables. In doing this, the laboratory preparation often becomes idealized and removed from naturalistic phenomena. However, after regularities are discovered in the lab, the next step is to examine whether they can be extrapolated to related (but not identical) variables in the natural environment. Similar relationships can be found in laboratory preparations and naturalistic phenomena in physiology and medicine, for example. Moreover, it is not clear if it is necessary for a logical entailment between laboratory preparations and behavior therapy

techniques to exist. Rather, the behavior therapy technique simply needs to be ''covered'' (Hempel, 1966) by regularities discovered in the lab. Most competent contingency management procedures are subsumed under general operant principles and procedures. A particular behavior therapy technique may represent a widening of laboratory-derived regularities. This may be the case in Wolpe's systematic desensitization.

These factors contrived to create a heterogenous behavior therapy with more tenuous or even often nonexistent roots in animal learning. Kazdin (1978) stated:

> By now [the mid- to late 1970s] behavior modification is so variegated in its conceptualization of behavior, research methods and techniques that no unifying schema or set of assumptions about behavior can incorporate all the extant techniques. Many of the theoretical positions expressed within behavior modification represent opposing views about the nature of human motivation, the mechanisms that influence behavior and the relative influence of such factors, and the most suitable focus of treatment for a given problem. (p. 374)

TOWARD THIRD-GENERATION BEHAVIOR THERAPY

It is clearly legitimate for behavior therapy to explore all areas of experimental and social psychology. However, it seems prudent that behavior therapists do this in a way that preserves the basic science/applied science relation. Extrapolating regularities found by basic researchers has epistemic advantages as described above. As previously mentioned, there is reason to be somewhat pessimistic about the usefulness of certain areas of basic psychology. Some of these areas do not share any of the three important confluences: (1) a shared search to understand how experience changes behavior; (2) a shared use of single-subject methodologies; and (3) a mutual reliance on large, ''clinically significant'' change.

The potential or actual usefulness of other areas of basic psychology does not reduce the relevance or importance of contemporary learning research. Nothing that occurred during the second generation of behavior therapy obviated the usefulness of conditioning research. However, learning is not a settled area. *Behavior therapists need to keep up with the evidential status of learning principles*.

An example may provide a clearer idea of what third-generation behavior therapy would look like. Third-generation behavior therapy suggests new ways of analyzing and intervening with clinically relevant behaviors. First-generation behavior therapists would examine individual contingencies to find controlling variables. However, third-generation behavior therapists would not view the behavior of the organism as controlled by a single contingency but rather as under the influence of multiple contingencies. Thus, the behavior therapist needs to understand the organism's behavior as an example of choice behavior, and as being influenced by competing contingencies. The matching law dictates an analysis of multiple sources of reinforcement, not just the simple, single contingency on which the first-generation behavior therapist would focus. McDowell (1982) argued:

> Hernstein's equation is considerably more descriptive of natural human environments than Skinner's earlier view of reinforcement. It is not always easy to isolate Skinnerian response reinforcement units in the natural environment. Hernstein's equation makes efforts to do so unnecessary and, moreover, obsolete. The equation can help clinicians conceptualize cases more effectively and design treatment regimens more efficiently. It also suggests new treatment strategies that may be especially useful in difficult cases. (p. 778)

The matching law would predict that reducing the reinforcement of competing responses should increase responding in the other contingency. Somewhat counterintuitively (at least to first-generation behavior therapists), the frequency of a behavior can be altered not only by manipulating the contingency the behavior is involved in, but also by the contingency of a competing behavior.

As a further example, behavior therapists often wish to identify reinforcers to influence the behavior of their clients. First-generation behavior therapists used Skinner's empirical law

of effect, which renders reinforcer identification a post hoc process: Reinforcers are stimuli that, when presented contingently on some response, increase the frequency of that response. Third-generation behavior therapists could rely on response deprivation/free operant analysis (Timberlake, 1995) to more accurately, more fully, and antecedently identify reinforcers. Using a free operant analysis, behaviors that occur within the system can be identified as reinforcers. Further, any behavior that occurs in the situation can be deprived and function as a reinforcer. This more contemporary analysis is useful because it:

- Can *antecedently* identify what will function as a reinforcer
- Can uncover ''natural'' reinforcers that occur within the system
- Precisely describes the conditions needed to produce a reinforcer (deprivation is transformed from an unclear initial condition in the empirical law of effect to having an explicit and clear role)
- Indicates that there is no special and unique class of reinforcers
- Describes a wider range of reinforcers
- Indicates why something will function as a reinforcer

Moreover, third-generation behavior therapists can rely on further behavioral principles to greatly augment the analysis of client behavior. Staddon's behavioral-regulation account of the preservation of ''bliss points'' can be used to make point predictions of response change under the influence of constraints such as contingencies. This analysis suggests that the organism attempts to preserve responses in fixed proportions. This can be further augmented by behavioral economics. The notion of elastic versus inelastic demand (or Staddon's defense variable and Rachlin's research on substitutability) is also relevant and potentially important.

It is hoped that learning researchers will be more mindful of dissemination and utilization issues and more frequently write in an accessible manner so that applied psychologists can more routinely access their important work. It is also hoped that learning researchers will conduct basic human conditioning studies to more clearly investigate the relevance to humans of their initial studies with animals. Often, basic researchers are best equipped to understand how protocols may need to be modified or augmented when applied to significant responses of humans. This would greatly aid behavior therapists' extrapolations to clinically significant behaviors.

Part of the excitement and promise of first-generation behavior therapy was that behavior therapists were not simply technicians. They knew how to faithfully execute procedures but also understood the underlying principles on which these were based. The first-generation behavior therapists understood the basic learning principles and could creatively and opportunistically apply them. Their repertoire was complex and led to many innovative and faithful applications. It is hoped that the subsequent chapters in this book will help reinstate this deep and faithful understanding of learning principles. As Kalish (1981) stated:

> The inclination to regard the methods of intervention in behavior modification as a collection of standardized techniques is especially misleading. It tends to obscure one of the most important contributions to the understanding of behavior change made by the advent of behavior modification procedures: namely, that for every so-called technique, there is a more fundamental and more general principle of behavior derived from research with animals and/or humans which can be applied to the solution of a problem in human functioning. This means, among other things, that those who intend to use behavior modification to help solve human problems should be aware of these principles and resourceful enough to propose treatment strategies which fit the case after a thorough analysis of the conditions which initiate and maintain the behavior. (p. 3)

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