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Personal Perspectives on the Development of Behavior Therapy and Cognitive Behavior Therapy

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In June 1999 I was privileged to be invited to a two-day conference at the University of Nevada, Reno to participate in presentations by colleagues regarded by the organizing committee (William T. O’Donohue, Deborah A. Henderson, Steven C. Hayes, Jane E. Fisher, and Linda J. Hayes) to be founders of behavior therapy. Most of the other speakers and those, having passed, their surrogates, were people whose seminal writings I had learned from as a graduate student and as a young researcher, teacher, and practitioner. The task set for each of us was to describe what we saw as the strongest influences in our professional lives and to discuss a publication that we believed had had some importance in the development of (cognitive) behavior therapy. We were also encouraged to engage in something which occasionally happens only in personal settings with friends and perhaps also with colleagues and students, namely autobiographical material that each of us believed had had bearing on our intellectual and professional development. The assumption of the conference organizers was that this framework would provide a context for a deeper understanding of the field than is available in published materials and, if one is lucky, from direct and extended contacts with a small handful of senior colleagues. There was laughter and, yes, there were tears as each of us learned something hitherto unknown about our colleagues and how these intimate details were seen by them as pivotal to their professional achievements. Articles based on these speeches were edited and published in O'Donohue et al. (2001).

It was similarly daunting and humbling to have been asked by the editors of this volume to write my reflections on behavior therapy's past as I have experienced it. I hope that the following account can place behavior therapy and cognitive behavior therapy into a useful perspective. We all know that this kind of material is almost never allowable in our publications. The focus in scientific writings is on hypothesis-testing, research methodology, and statistical analysis. But where did the ideas come from? How did the investigators come to spend inordinate amounts of time, energy, and grant money on asking questions in a controlled, scientific fashion? For myself, if there's a main theme to my story and, I believe, to the evolution of behavior therapy/modification, which is rhetorically derived from "modern learning theory", to cognitive behavior therapy and thence to the "third wave," it's the centrality of cognition – how people construct their world and how therapeutic efforts to alter their constructions can improve their lives as well as the human condition.

Earliest Influences

I've been fortunate – damned lucky, to put it more bluntly – to have done my Ph.D. work at Stanford in the early 1960s and to have had the opportunity to learn from four giants in the field: Albert Bandura, Walter Mischel, Perry London, and Arnold Lazarus. My sense of good fortune is enhanced by the fact that I went to Stanford after college to study dissonance theory with Leon Festinger (Festinger, 1957). As it turned out, he had just changed his research interests from his pioneering work in cognitive dissonance to basic laboratory work in eye movements (and I don't mean EMDR). My boundless admiration of him as a leading and creative social psychologist was exceeded only by my lack of interest in his newfound research focus, and so I wandered a bit my first year only to end up with having Bandura as my advisor.

These happy accidents are, I believe, instructive in how one might view behavior therapy's past (with major considerations for its present and its future). For at its core, what we call behavior therapy, behavior modification, and more recently cognitive behavior therapy has its essence in a desire to apply as rigorously as possible various scientific methods to studying the exceedingly complex challenges in helping people achieve changes in thinking, feeling, and behaving that will ease their suffering and perhaps enrich their lives.

I entered graduate school in 1962 uncertain of what specialty I would pursue (beyond designing clever analogue deception-laden dissonance experiments with Festinger[1]), but I was

[1] A valued colleague and good friend at Stony Brook, where I had my first academic position after graduate school, the late Jerome E. Singer, once commented that the most interesting aspects of dissonance theory experiments were the cover stories. This wry observation came from the co-author of the famous Schachter-Singer study on the centrality of cognition in how

certain of one thing, namely that my specialty would not be clinical psychology. The reason was that the only kind of clinical I had been exposed to during my undergraduate days at Harvard was psychoanalytic and its variations. I just couldn't accept the epistemology. When is a cigar just a good smoke? After my disappointment at Festinger's radical change in direction and after immersing myself for a few months in physiological psychology in J. A. Deutsch's (Deutsch, 1960) lab (and having to confront my serious allergies to animal dander), I learned that there was this young full professor teaching ways to help people psychologically without forsaking one's interest in and commitment to testable theorizing and hard-nosed experimentation. This person was the above-mentioned Albert Bandura.

In the olden days, when I had to trudge uphill in the biting cold to go to school and then uphill in the afternoon to return home, one could actually read everything that had been published in what was called "behavior therapy" or "behavior modification." One could also master the experimental animal learning literature that was boldly asserted to be the firm foundation for these startlingly new therapeutic techniques rested. I did just that as part of my Ph.D. qualifying exams.[2]

There were a handful of books that were pivotal in the early 1960s –Andrew Salter's "Conditioned Reflex Therapy" (1949), Hans Eysenck's edited "Behavior Therapy and the Neuroses (1960)," Joseph Wolpe's "Psychotherapy by Reciprocal Inhibition" (1958), Arthur and Carolyn Staats's "Complex Human Behavior" (1963), and an almost poetically crafted and little known gem by a former mentor, Perry London (1964), "The Modes and Morals of Psychotherapy." Noteworthy as well, of course, was B.F. Skinner's (1953) "Science and Human Behavior". As Marv Goldfried and I reviewed in the first chapter of our 1976 book, "Clinical Behavior Therapy," (Goldfried & Davison, 1976) there were also other earlier books and articles that are seldom cited and appreciated, most especially Julian Rotter's "Social Learning and Clinical Psychology" (1954), George Kelly's "Psychology of Personal Constructs (1955) , and a 1961 Psychological Bulletin article by my Doktorvater, Albert Bandura. The books by London, Rotter, and Kelly are rightly seen as foundational in the evolution of behavior therapy into cognitive behavior therapy, but my reading of the CBT literature seldom references and discusses these earlier seminal writings. Of course no deep understanding of and appreciation for

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people understand their autonomic arousal. Talk about a cover story! Among the many classic studies pertinent to cognitive behavior therapy that younger cohorts would enjoy and benefit from, none is more significant for earlier generations than this article.

[2] There's an old joke about a computer nerd rising from his cumbersome PC on a day in 1996 to exclaim with great satisfaction that he had just finished viewing every website on the World Wide Web. After completing my Ph.D. qualifying exams in the fall of 1964, I had the same feeling about behavior therapy – I had read all that had been published plus a great deal of in press articles as well as related material, like the aforementioned animal avoidance learning literature. I doubt that anyone has been able to say this about behavior therapy for at least the past fifty years.

CBT is possible without the seminal contributions of Albert Ellis (e.g, 1962) and Aaron T. Beck (e.g.,1967). I believe it's useful to mention these foundational publications because, unfortunately, they are seldom even read or taught these days in our continuing efforts to stay abreast of the explosion of books, chapters, and articles that are generally seen as seminal.[3]

My exceedingly enjoyable and stimulating years at Stanford were immeasurably enriched by the visit of Arnold Lazarus from Johannesberg, South Africa during my second year, 1963-1964. Another totally unpredicted and unexpected bit of luck. With a few of my fellow clinical students, I sat in on therapy sessions that Lazarus had with patients from the area south of San Francisco who eagerly sought help from a highly touted clinical psychologist who was one of the few clinicians in the world widely acknowledged to be an expert in this new thing called behavior therapy. I spent 10 to 15 hours a week from September to May sitting in with Lazarus. It's hard to put into words how important that year was in my intellectual and professional development. Anticipating a theme that I will develop later in this chapter, I came to appreciate the complexities of the clinical interaction, and gradually the abstract concepts and experimental research that I was immersed in through courses with Bandura and Mischel[4] came to life. I had the unique and priceless opportunity of watching how a master clinician implemented what behavior therapy was at that time, described and explained in Wolpe and Lazarus's 1966 book, "Behavior Therapy Techniques". I was stunned by the improvement of most of Lazarus's patients but also dumbfounded by how much more there was to actual clinical work than was evident in the extant body of theory and research. Lazarus referred to these factors as "non-specifics," which I later came to appreciate as deriving largely from Rogers's client-centered therapy (Rogers, 1942), in particular the importance of a trusting therapeutic relationship marked by empathy and mutual respect. The importance of the therapeutic relationship loomed much larger than was discussed in the behavior therapy literature of the time. I had the opportunity to formalize and elaborate on these factors in my book with Goldfried (Goldfried & Davison, 1976) after I had gained some "seasoning" in the clinical world, especially in my teaching and clinical supervision in my first job at Stony Brook beginning in 1966.

Cognitive Factors in Behavior Therapy – The Development of Cognitive Behavior Therapy

[3] I've often considered offering an elective seminar that would involve a study of these and other early works, but I seriously doubt that it would meet minimum enrollment.

[4] I have found that many younger cohorts of students and colleagues are sometimes unfamiliar with Walter Mischel. In the current context, he was what Thomas Kuhn would have called a paradigm-buster. In a painstaking and creative analysis of how well traits assessed by personality tests predicted behavior in various situations and over time, Mischel argued that more situational analyses were more useful and valid. In this way he made seminal contributions to behavior therapy's emphasis on functional analysis.

As Lazarus and I have written in several chapters and articles (e.g., Lazarus & Davison, 1971; Davison & Lazarus, 1995), case studies occupy an honored place in the developing science of clinical psychology. Their heuristic value is probably their key importance and will be addressed throughout this chapter. I hope it will be informative to illustrate this essential point by discussing in some detail an early publication on what I called "cognitive restructuring" and which, for me, took me into the hybrid field of cognitive behavior therapy (Davison, 1966b).

Context: During my postdoctoral clinical internship at the Palo Alto Veterans Administration Hospital in 1965-1966, I treated a middle-aged patient who had been presented at a Grand Rounds by a psychiatry resident for treatment of paranoid delusional beliefs centering around spirits communicating through some cysts on his forehead. He described these as "pressure points". Though psychodynamic in orientation, the resident had fulfilled the patient's request by convincing the surgery department to remove the cysts based on the belief that this would eliminate his delusion. The delusions did not abate[5].

Possessed with a nascent cognitive behavior therapy fervor, I asked the patient during the Grand Rounds Q&A if he experienced his spirits communicating with him at any particular times. He replied that it tended to happen when he had to make a decision, even a very minor one. I queried the patient further, operating with the very tentative hypothesis that he was very anxious about making mistakes and that, for some reason, he had developed the paranoid delusion about helpful spirits as way to cope with his decisional anxieties. This kind of situational assessment had not been a part of his prior sessions with the psychiatry resident. The patient's answers prompted me to seek and obtain the approval of my clinical supervisor to volunteer to have some sessions with the patient. I believe that it will be informative to quote extensively from this brief publication:

"Mr. B's psychiatric problems seemed to begin 4 yr. prior to hospitalization, with the suicide of his only brother. It was during this time that he began to be preoccupied with 'pressure points' over his right eye, which he interpreted as being caused by a spirit either inside or outside his body, helping him make decisions. His marriage of 3 yr. held, from the very outset, little more than continuous arguments with his wife and her family. He would often be squelched by being called a 'mental case.'

"Upon admission his speech was described as tangential, with loose associations and grandiose schemes and persecutions by others, but centering around information from his pressure points. There was no evidence of hallucinations.

"In the first session [with me], when asked to relate as many instances as he could of their occurrence, the patient brought up several situations which were clearly anxiety-provoking, e.g., losing his way on the freeway [he was a truck driver], being late with a truckload of goods, and then, along with severe anxiety, receiving 'messages' of which turns to take. In every case Mr.

[5] Your tax dollars at work, I thought ruefully to myself at the time.

B. volunteered that he had been extremely tense and upset in these situations. Towards the end of the hour, I suggested to him that, while he had his own ideas about the nature of these sensations, he entertain another notion. At this point I requested him to extend his arm, clench his fist, and slowly bend his wrist downwards so as to bring the closed hand toward the inside of the forearm. A definite feeling of severe muscle tension was thereby produced in the forearm, at which time he smiled slightly and muttered that it felt very much like a 'pressure point.' I then suggested that perhaps these sensations [his "pressure points"] were purely natural phenomena, a consequence of his becoming very tense in particular kinds of situations. To appeal to his interest in philosophy and anthropology (which may account for his construction of the sensations), I cited Malinowski's (1948) discussion of how Man's need to explain phenomena probably gives rise to mystical explanations in areas where scientific, naturalistic explanations are lacking. To test my hypothesis, I asked him to undergo training in deep muscular relaxation, designed to reduce his generally high level of anxiety and especially to determine the nature of the pressure points and perhaps to control them. The first session ended with a half hour's training in relaxation [by means of tapes I made based on Edmund Jacobson's original work but modified by Lazarus to involve quick tension and release rather than Jacobson's gradual tensing and relaxing of muscles]. After completing the relaxation exercises, the patient reported spontaneously: 'I feel relaxed inside like I haven't felt in a long time.' I deemed this as a very favorable and promising initial outcome and had the patient practice with the tape on his own between subsequent sessions.

"There were eight additional sessions over a 9-wk, period. During these meetings, Mr. B. was instructed in differential relaxation (Davison, 1965), in order to enable him to eliminate pressure points when they arose, as well as to reduce his maladaptively high levels of anxiety. He began to report on the occurrence of pressure points at the hospital, all of which confirmed the hypothesis that we were testing; he was also succeeding in reducing them markedly by relaxing. After 1 mo. he began to refer to them as 'sensations,' and his conversation generally was losing its paranoid flavor.

"In the fourth session I initiated a game of black-jack with him, feeling that it would provide the occasion for a pressure point. This, indeed, turned out to be the case, and being able actually to produce the sensation in a manner analogous to real life and then to eliminate it by relaxing provided further evidence, for both of us, as to the utility of both the hypothesis and the therapy.

"During a week-long leave of absence at home, Mr. B. began to assert himself to his wife and in-laws, as had been suggested; the favorable effects of this behavior, in terms of clarifying some misunderstandings, were augmented by his feeling significantly more at ease. He also reported significant relief from the realization that his 'crazy,' 'sick' behavior in the past could be fruitfully interpreted in terms of quantitatively different reactions to situations, rather than in terms of a 'mental illness,' which notion had placed him in a most unfavorable, 'one-down' position at home.

“For the remaining 3 wk. of his hospitalization we spoke often about the effects which our behavior has on others; how these effects can in turn influence our own feelings; about the advisability of asserting oneself in the appropriate situation so as to avoid the buildup of tension and often the subsequent, sometimes ‘crazy’ outbursts; and especially about the benefits to be derived from the control of one's tensions through differential relaxation.

“A follow-up of [only] 6 wk. was obtained by letter. Mr. B. reported that the ‘pressure points’ (his quotation marks) were far less frequent, fairly amenable to relaxation, but most importantly, of no concern to him. He has been far less tense generally and has managed to complete a correspondence road-building course which he had been able to work on very little the previous 2 yr. His marital relationship has also shown continued improvement.

“It would appear that improvement was due, in greatest part, to the combination of differential relaxation and cognitive restructuring of the pressure points. In addition, the general use of relaxation is assumed to have made the patient less tense overall and perhaps also to have occasioned "in vivo desensitization" of various aversive stimuli (Davison, 1965; Lazarus, Davison, & Polefka, 1965). The reduction of tension and the shift of ideational and verbal behavior from socially unacceptable to socially approved patterns seem to have consolidated the improvement by changing the reactions of others to him, thereby setting the stage for still further gains.

“In this short report one [with very limited follow-up] can only allude to earlier work with paranoid cases. In spite of radically different orientations, such workers as Cameron (1959), Salzman (1960), and Schwartz (1963) seem to agree strikingly with the present therapy to the extent that the paranoid' s constructions of the world should be subtly challenged, with alternate explanations being offered.

“Is this ‘behavior therapy?’ Surely an answer depends on one’s definitions. As techniques derived from ‘modern learning theory’ (cf. Eysenck, 1960), especially from studies in classical and operant conditioning, this certainly is not the case. The intentional appeal to cognitive processes points to this therapy as being perhaps "neobehavioristic," in the sense used by Peterson and London (1965), who report the first case in the behavior therapy literature which explicitly extends the therapist's concerns into cognition.” (Davison, 1966b,pp. 177-178).

Of course a case study is very limited in strictly scientific terms. Many other factors were operating here, among them the nature of the trusting and respectful relationship that I managed to establish, which apparently reduced the usual negative reaction that paranoid people have when their delusional beliefs challenged. I was able to encourage this hospitalized patient to question his paranoid beliefs and subject them to experimental analysis. On the other hand, a variety of other therapeutic interventions had been attempted without success, which is consistent with the strong possibility that my sessions with him had specific desirable effects. Efforts to apply variations of CBT to people with serious mental disorders have become an active area of research and application (e.g., Beck, Grant, Inverso, Brinen, & Perivoliotis (in press).

The Person as an Active, Thinking Agent

In 1966 and in 1973 I published two articles that relate to the current conception of people as active and thinking participants in their lives. This seems pretty unremarkable to any sentient human being and has for years been an underlying assumption in many specialties in our field, especially in social psychology, where choosing and deciding and wanting and demurring have underlain decades of theory and research. Festinger's dissonance theory, for example (Festinger, 1957), would have no meaning without the core assumption that people can freely choose and that their attributions for their choices matter. (My own work in attribution is described in a later section.)

However, I would argue that this was not a formal part of behavior therapy in its early days.

Recall that behavior therapy was defined in the late 1950s into the mid-1960s as based on "modern learning theory," which for all intents and purposes referred to Pavlov, Skinner, and to some extent Hull. People were characterized in theory -- though doubtless not in practice -- as passive objects of environmental events. In my view, the earliest experiments and position statements in behavior therapy did not state or imply that animals or humans played an active role in their relearning/"reconditioning." Like Pavlov's dogs, Skinner's pigeons, and Wolpe's cats, people were acted upon by environmental manipulations. Relax them, provide stimuli to them, observe their responses, reward them etc. Mediating states themselves were viewed a la Mowrer (1939) and Miller (1948) as "little r's" subject to the same stimuli and reinforcing events as overt behavior.

<u>Relaxation as an Active Cognitive Process</u>

Radical behavioristic theorizing was exemplified by the pioneering research and clinical work of a physician, Edmund Jacobson (1929). He published numerous investigations of training in deep muscle relaxation, painstaking exercises to relax muscles to the extent that proprioceptive stimuli were eliminated and *therefore*, he asserted, all affect and ideation. Per John Watson's radical behaviorism (Watson, 1913), it was the reduction of proprioceptive stimuli that effected a reduction in anxiety and even thought. This led Wolpe to build systematic desensitization on muscle relaxation as a functional substitute for the eating that Mary Cover Jones had employed as an anxiety-inhibiting "response" in eliminating little Peter's fear of rabbits (Jones, 1924).

This was the context for two events, one based on clinical observation, the other on some reading I did during my short period in Tony Deutsch's physiological psychology laboratory.

On the clinical side, recall the many hours I was fortunate to sit in with Arnie Lazarus during his visiting year at Stanford, 1963-1964. Watching him conduct training in muscle relaxation with anxious patients, I was struck by the emphasis on alternate tensing and relaxing of muscles. Of particular interest was the "letting go" part of the exercises, the softly spoken instructions to the patient that they actively release the tension that they had just created in a group of muscles (for transcripts of such exercises, see pp. 82-98 in Goldfried and Davison [1976]). It was a very active process. The reduction in tension, the reduction of proprioceptive input from the muscles, was created by the patient releasing the tension.

I saw a possible connection between Jacobson's peripheralistic conception of thought and feeling with research in curare, a drug that is sometimes used to prevent anesthetized surgery patients from moving their bodies in ways that would interfere with the surgery. Their striate muscles are rendered flaccid via a blocking of excitatory efferent nerve impulses at the neuromuscular junction. In plain language, messages from the brain don't get translated into contraction of the muscle. Experiments with curarized rats as reviewed by Solomon and Turner (1950) showed, though, that avoidance learning is possible when the musculature is rendered flaccid by curare, supporting the presence of anxiety under total curarization.

These animal findings were confirmed in a remarkable article by Smith, Brown, Toman, & Goodman (1947). One of the co-authors, a biologist, had himself paralyzed with the drug without being rendered unconscious. *He found it an absolutely terrifying experience.* Even though he was on a ventilator and in good hands medically with professionals he trusted, he found it alarming not to be able to move his muscles. Hardly surprising! He did, though, try mightily to do so, which strongly suggests that his cortex was sending efferent messages to his muscles to tense up. Ergo, reduction in proprioceptive feedback from muscles is not at all inconsistent with cognition and anxiety, contrary to the Watsonian theorizing of Edmund Jacobson and Joseph Wolpe.

I summarized the implications of these animal and human studies as follows:

> "… there seems to be an important difference between relaxing one's own muscles and having them relaxed by a paralytic drug, quite aside from one's subjective reactions. In both states there is a virtual elimination of proprioceptive feedback from the muscles. If one looks beyond the elimination of *a*fferents, he might ask whether *e*fferent activity offers a clue. Quoting from Ruch, Patton, Woodbury, and Towe (1961), 'Reduction of a skeletal muscle is accomplished by inhibition within the spinal cord of the motorneurons which excite **it** (p. 221).**'** Therefore**,** it would seem that, in a person relaxing his own musculature, the efferent activity from his cortex would be quite different from that during muscle contraction, i.e., it would entail inhibitory efferents which would block activity in the actual efferents that innervate muscles." (Davison, 1966a, p. 446)

And with respect to awareness or cognition, I pointed out that:

> "Briefly stated, if we are to suggest that *letting go* of one's muscles is the crucial factor in the use of Jacobsonian relaxation in systematic desensitization, a

> question to ask might be whether, over above the afferent feedback we usually get as a result of an efferent, we can be aware of our efferents. By means of an experiment on spatial visual localization in humans, Festinger and Canon (1965) have been able to show that we do, in fact, make use of 'outflow information' ." (p. 446).

Putting it all together – the clinical practice of teaching deep muscle relaxation and the human experience of abject terror when the muscles are relaxed by a drug ---led me to conclude that awareness (cognition) and agency by the person had to be incorporated into behavior therapy, a self-evident truth that I'm sure was not lost on practicing behavior therapists but was not incorporated into the "conditioning therapies" behavior therapy paradigm of the 1950s and 1960s. Obviously people are active controllers and deciders rather than passive organisms being acted upon by the environment. This fact needed to be formally integrated into behavior therapy.

Countercontrol

The other stream regarding agency and cognition arises from a paper I gave at the annual international Banff Conference on Behavior Modification in Banff, Canada in the spring of 1972 and published in a volume edited by the conference organizers (Hamerlynck, Handy. & Mash, 1973). I decided to talk about a topic I had been discussing with Stony Brook colleagues for a few years, namely countercontrol (Skinner, 1953).

Of course this concept, referred to as resistance for many decades, is an integral part of psychoanalytic thinking, indeed a very important defensive maneuver by the patient's unconscious to avoid examination of repressed problems that needed to be explored to effect improvement. And the similar concept of "reactance" had been a focus in social psychology since Brehm (1966). So the idea was nothing new. But in spite of Skinner's (1953) discussion, there was little if any serious attention paid to countercontrol in the early behavior therapy literature, to the best of my knowledge. In the Banff paper I described many ways that patients could resist behavior therapy treatment. For example, if a patient undergoing systematic desensitization does not generate a fearsome image when asked by the therapist to do so, there is no way that imaginal exposure to an instantiation of the person's fear is going to happen. And regardless of the change mechanisms hypothesized to be operating that underlie the efficacy of the procedure – which I explored at length with one of my first Ph.D. students, Terry Wilson (Wilson & Davison, 1971; Davison & Wilson, 1973)—nothing was going to happen if the patient didn't follow some basic procedural requirements. Not that our clinical pioneers were unaware of the need for patients to follow directions, but it took the form of what I found to be rather casual instructions like "Have the patient lean back in a comfortable chair;" "Ask the patient to imagine scenes that you present to her;" "Be sure to have the patient raise a finger when she feels even the slightest degree of anxiety and then to stop imagining the aversive image." Nothing startling here except that words like "have" as in "have the patient stop imagining" and other such cooperative rule-following were lightly glossed over and not fully addressed conceptually within a behavioristic paradigm in which the imagining of a fearsome event is the functional equivalent of sounding a tone that had been previously associated with an electric shock in an experiment with rats. (The example here is systematic desensitization but the principle applies across the board.)

A few years later Marv Goldfried and I (Goldfried & Davison, 1976) elaborated on the concept of resistance in our chapter on the therapeutic relationship, suggesting various ways that clinicians might reduce the patient's reluctance and lack of cooperation or actually to use it to enhance therapeutic change. Many of our proposals can be seen in later developments in what has come to be referred to as "third wave" behavior therapies, such as dialectical behavior therapy (Linehan, 1993) and Acceptance and Commitment Therapy (Hayes, Strohsal, & Wilson, 1999).[6]

Attribution

You may recall that I went to Stanford primarily to work with Leon Festinger in dissonance theory. Though I switched to clinical during my first year, social psychology remained an area of great interest. And why not? Social psychologists concern themselves with humans as "the social animal", the title of Eliot Aronson's charming and engaging introduction to the field (Aronson, 1972). This metatheoretical perspective was the theme of an important book by a Arnold Goldstein, Kenneth Heller, and Lee Sechest (1966) "Psychotherapy and the Psychology of Behavior Changed". In this scholarly and prescient book, a strong case was made for the emerging science of clinical behavior change to encompass theory and research in social psychology. And an inherent part of social psychology is of course cognitive in nature.

[6] My countercontrol paper had a section aimed at behavior therapy colleagues who were enthused primarily about operant conditioning and who interpreted Skinner as discouraging, even forbidding, inferences about mediators like thoughts, feelings, and willing. While colleagues more knowledgeable about Skinner than I regard such rejection of internal states as a misinterpretation of Skinner, it was a guiding assumption at least during the earliest stages of what was called "behavior *modification*." But, I asked in my presentation, what if patients change their overt behavior due to any manner of contingency management without changing their actual feelings and thinking, domains which of course constitute the focus of *C*BT? I then semi-facetiously proposed "the Kol Nidre Effect" to describe this possibility. For my non-Jewish colleagues: Kol Nidre is a Jewish prayer chanted on the evening of Yom Kippur, the day of atonement. It means "all vows" and it is believed to have originated over a thousand years ago but is usually associated with the forced conversion of Jews to Catholicism during the Spanish Inquisition in the 15th century. The prayer asks for God's forgiveness for having behaved contrary to Jewish beliefs, that is, changing only one's overt behavior just to keep from getting killed. So I suggested that even if one obtained the collaboration/cooperation of the patient, a focus only on overt behavior might well not be enough to effect meaningful and enduring change.

My growing interest in bringing cognition into behavior therapy, spurred on no doubt by the Goldstein et al. book, developed further during my fledging days at Stony Brook when I began an exhilarating collaboration with Stuart Valins, a Stanley Schachter Ph.D. Given the very environmentalistic and openly manipulative stance of early behavior therapy, it occurred to us that the reasons we give to why we have changed, in other words our attributions for change, might be important in how that change would be maintained once formal therapy sessions are terminated. How do patients view the reasons for their improvement? The usual relapses following the termination of drug therapies – if indeed they are ever terminated – are consistent with the hypothesis that people who attribute their improvement to an external source like a drug are less likely to maintain their therapeutic gains than patients who attribute their change to something internal to themselves.

Valins and I decided to examine this issue in a laboratory analogue of drug treatment. The study was described as an evaluation of a new drug[7] that increased people's ability to tolerate pain. Subjects (a) underwent a pain threshold and shock tolerance test, (b) ingested what they believed was a drug (really a placebo), and (c) repeated the test with the shock intensities surreptitiously halved. All subjects were thus led to believe that a drug had changed their tolerance for pain. Half of the subjects were then told that they had actually received a placebo, whereas the other half continued to believe that they had received a true pain-reducing drug. It was found that subjects who attributed their behavior change to themselves (i.e., who believed they had ingested a placebo) subsequently perceived the shocks as less painful and tolerated significantly more than subjects who attributed their behavior change to the drug (Davison & Valins, 1969).

I then did a conceptual replication (Davison, Tsujimoto, & Glaros, 1973). Undergraduate and graduate students suffering from insomnia participated in a controlled field experiment in which beneficial change was brought about in falling to sleep via a treatment package composed of 1,000 mg of chloral hydrate per night and modified Jacobsonian (ref)relaxation procedures as well as regularizing when Ss were to get into bed for sleep. Following treatment, half of the Ss were told that they had received an optimal dosage of the sleep aid, while the others were informed that the dosage they had received was too weak to have been responsible for any improvement. All Ss were then instructed to discontinue the drug but to continue with the relaxation and scheduling procedures during a post-treatment week. As predicted, greater maintenance of therapeutic gain was achieved by those who could not attribute their changes to the drug. Participants were also asked how often they had continued their relaxation exercises and sleep-scheduling during the week following their being told whether they had received an optimal versus an inadequate dose of the sleep aid. No differences in their self-reports emerged.

[7] As an ode to Schachter and Singer's "Suproxin", we called our drug "Parataxin".

Taken together, I concluded that these two experiments on analogue and actual drug treatment had important practical and conceptual implications for behavior therapy and contributed to the new field of *cognitive* behavior therapy. As an extension of general experimental psychology, behavior therapy was essentially environmentalistic, looking to external variables for the control and alteration of "abnormal" behavior. The literature of the time was marked by little if any concern about how the individual so manipulated *perceives the reasons for their changing.* Early behavior therapy – in its theorizing though probably not in its practice -- seemed to be consistent with the assumption that behavior therapy clients construe the reasons for change to be *outside* themselves, that is, that therapeutic improvement is to be attributed primarily if not entirely to external influence. It seemed to me that especially the operant approaches would pose problems for the maintenance of behavior change once the artificially imposed contingencies are withdrawn (cf. Davison, 1969); and the difficulty of maintaining therapeutic change might be accounted for at least in part by the notions of attribution proposed by Valins and myself. If a person realizes that his behavior change is totally dependent upon an external reward or punishment, there is no reason in the patient's mind for his new behavior to persist once the environmental contingencies change. The external contingencies assumption, widely held in the 1960s and even for decades letter, can be seen in the belief that maintaining desired changes had to be effected through trying to ensure that patients would receive reinforcement from the social environment in which they were living, rather than to working to make changes in internal processes like beliefs, schemata, and attributions, the foci of cognitive behavior therapy. And it is noteworthy, I believe, that this shift coincided with the psychotherapy integration movement of the 1970s with my 1976 book with Marv Goldfried and the 1977 book by Paul Wachtel, discussed below. But before we get to that, I'd like to discuss another relevant theme.

Perceived control

As part of my interest in cognitive factors is an experiment the idea for which grew out of one of my graduate school specialty examinations. As a behavior therapy warrior and enthusiast for all things Wolpean as well as for the animal avoidance learning experiments and scholarly writings in the 1940s and 1950s inspired by O. H. Mowrer and Neal Miller (Mowrer, 1939; Miller, 1948), I was intrigued by a 1948 rat experiment by Mowrer and Viek entitled "An Experimental Analogue of Fear from Sense of Helplessness." Interestingly it was published in the *Journal of Abnormal and Social Psychology*, a journal that very, very seldom published studies using non-human models. Because the concept of control and most especially *perceived* control has become important in social psychology (Taylor, 1983) and in CBT, I'd like to provide some background and details.

The avoidance learning literature, which was the foundation of most of early behavior therapy's appeal to "modern learning theory, was almost entirely with rats. (Wolpe's creative experiments in the early 1950s employed cats (Wolpe,1952), perhaps because he was using

Masserman's earlier experiments involving cats (Masserman,1943) as an animal model.) It was for this reason that I channeled my youthful energies into a diligent and comprehensive study of the animal avoidance learning literature as part of my doctoral specialty exams at Stanford in the summer and early fall of 1964. My growing interest in and respect for Wolpe was enriched by the increasingly cognitive interests of two of my primary instructors, Albert Bandura and Walter Mischel, and most importantly by my "clinical apprenticeship" with Arnold Lazarus. These cognitive influences went back a couple of years to my undergraduate mentorship under Jerome Bruner at Harvard, to be discussed below. For all these reasons, at least as I reflect retrospectively over the past 55+ years, I began to chafe under the early behavior therapy constraints of "modern learning theory" and I returned to my undergraduate appreciation that a useful understanding of the human condition had to include explicit and careful attention to cognitive factors. If this sounds naïve and dated, that is totally understandable. But it was diametrically opposed to the foundational behavior therapy mantra against what Perry London aptly termed "the insight therapies," which included all that had come before, principally psychoanalytic/psychodynamic and humanistic-existential approaches.[8]

In the aforementioned experiment by Mowrer and Viek, laboratory rats were trained in an instrumental response to obtain food. Then they were shocked when eating the food reinforcer. Randomly selected rats were then assigned to one of two conditions. One group was able to terminate the shock by jumping. Each member of this "control" group was paired with/yoked to a rat in the "no control" group, for whom the shock was terminated not by anything it was doing when shocked but when its partner in the "control" group made the movement that terminated the shock. Thus, nothing that the "no control" rats did had any bearing on how long they had to endure the shock; that was determined by its partner's behavior in the "control" group. The rats whose jumping terminated the shock later exhibited less fear than the group that had no actual control over the shock.

These experimental findings were consistent with prior (e.g., Rotter, 1954) as well as with subsequent clinical and anthropological observations of people's reactions to fearsome events over which they have no actual control. For example, in a 1957 anthropological report by

[8] The contempt for insight-oriented paradigms, in particular psychoanalysis and its variants but also the humanistic-existential tradition of Rogers and Maslow, can be appreciated by the colorful first paragraph of Andrew Salter's classic *Conditioned Reflex Therapy*: " It is high time that psychoanalysis, like the elephant of fable, drag 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" (Salter, 1949, page 1). This kind of mantra was common in the earliest behavior therapy/modification literature of the 1950s and 1960s.

Richter, entitled “On the Phenomenon of Sudden Death in Animals and Man”, it was reported that “A Brazilian Indian condemned and sentenced to death by a so-called medicine man is helpless against his own emotional response to this pronouncement – and dies within hours…. In New Zealand a Maori woman eats fruit that she only later learns has come from a taboo place. Her chief has been profaned. By noon of the next day she is dead (Basedow, 1925, cited in Richter, 1957, p. 191).”

Reports like this abound in the anthropology literature. Similar observations can be found in our own society. Bettelheim, for example, a concentration camp survivor, wrote as follows (I am citing him despite the controversies that swirl around him. The following quote is consistent with numerous other reports): “Prisoners who came to believe the repeated statements of the guards – that there was no hope for them, and that they would never leave the camp except as a corpse – who came to feel that their environment was one over which they could exercise no influence whatever… these prisoners were in effect walking corpses… they had given the environment total power over them (Bettelheim, 1960, pp. 151-152).”

My aforementioned case study about paranoid delusions (Davison, 1966b), my collaboration with Stu Valins on attribution (Davison & Valins,1969), my clinical experience and clinical supervision during my first few years at Stony Brook --- I think that all these factors underlay how I began to interpret the Mowrer-Viek study. With a measure of unabashed anthropomorphism, I hypothesized that for humans it might not (entirely) be the objective measure of control that was important, rather it might be the *perception* of control. That is, perhaps stress can be reduced in humans if the belief is induced that it is under their control even if it not. I began brainstorming with another Stony Brook colleague, James Geer, and, together with a promising undergraduate, Robert Gatchel, we designed an analogue experiment with humans to address the issue.

Briefly stated, male undergraduate volunteers underwent a series of 6-second painful electric shocks – levels set at mildly painful for each subject -- at baseline while their stress (spontaneous GSR fluctuations) and reaction times to turn off each shock were measured. Then half the subjects were told that if their reaction times to a second series of shocks were quick enough, the duration of the shocks would be reduced in length from 6 seconds to 3 seconds. The other half were simply told that their second series of shocks would be reduced to 3 seconds in duration. In fact, the second series of shocks were reduced from 6 seconds to 3 seconds for *all* subjects, the key difference being that “control” Ss *believed* that they were exerting control over the duration of the second series of shocks. As predicted, those subjects who *believed incorrectly* that they were exerting control over aversive stimulation reacted with less stress than those who did not. We considered these findings all the more significant since other research had shown that our perceived control Ss might have been more on edge during the second series of shocks because they were performing a demanding task, trying to reduce their reaction times, in order to achieve a goal, namely reducing their discomfort. The important role of belief in control moved us to end the article with a reference to the anthropologist Malinowski (1949) to the effect that "Man creates his own gods to fill in gaps in his knowledge about a sometimes terrifying environment.[9] Perhaps the next best thing to being master of one's fate is being deluded into thinking at he is (Geer, Davison, & Gatchel, 1970, pp. 737-738)."[10]

Of course, reality bites. Nonveridical perception, like primary process thinking a la Freud, has its limits[11], but the concept of perceived control has become a focus of great interest among in both social and clinical psychology.

Science and Practice: A Two-Way Street

As a new assistant and then associate professor at Stony Brook, I was invited in 1968 to co-author a chapter with Arnold Lazarus entitled "Clinical Innovation in Research and Practice." It was to be included in the weighty "Handbook of Psychotherapy and Behavior Change" that Allen Bergin and Sol Garfield were putting together. The list of contributors was impressive and I felt almost giddy about being asked, especially since I would be co-authoring the piece with my mentor, teacher, and soon-to-be best friend Arnie Lazarus. It turned out to be a piece that managed to annoy as many people as it pleased. In this chapter we tried to lay out the intricate relationships, dialectics if you will, between applied and scientific work in clinical psychology and other mental health disciplines. Among the unique characteristics of clinical work that we deemed essential was the following:

"While it is proper to guard against ex cathedra statements based upon flimsy and subjective evidence, it is a serious mistake to discount the importance of clinical experience per se. There is nothing mysterious about the fact that repeated exposure to any given set of conditions makes the recipient aware of subtle cues and contingencies in that setting which elude the scrutiny of those less familiar with the situation. Clinical experience enables a therapist to recognize problems and identify trends that are usually beyond the perceptions of novices,

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[9] The reader may recall my using Malinowski in persuading the paranoid patient to entertain a more naturalistic explanation of his delusional beliefs. Clearly I still am influenced by his book, which was part of a sophomore tutorial seminar.

[10] I'm sure that the later Walter Mischel, my old teacher and mentor, would not have minded my recounting the following. When he was editor of the *Journal of Personality and Social Psychology*, I was visiting at Stanford in 1969-1970 and was officed across the hall from him. He had received reviews of our manuscript and had decided to accept it. In what is surely the rarest of experiences, he came into my office smiling and gave me the good news. But he then asked if I would be prepared to drop the quote from Malinowski. "We don't usually have articles that end in a poem", he said with that twinkle in the eye that marked his delightfully impish sense of humor. "But I really like it," I replied, "It makes an important point." One of my few wins with MIschel.

[11] It also has possible strengths, like enhancing a sense of self-efficacy (Bandura, 1877) and encouraging persistence and efforts to achieve certain goals.

regardless of their general expertise. It is at this level that new ideas come to the practitioner and often constitute breakthroughs that could not be derived from animal analogues or tightly controlled investigations. Different kinds of data and differing levels of information are obtained in the laboratory and the clinic. Each is necessary, useful, and desirable" (Lazarus & Davison, 1971, p. 199).

The importance we placed on clinical work was anathema to some of our behavior therapy colleagues, who inveighed against the role that on-the-ground experience had in developing a scientific approach to etiology, assessment, and intervention (which included not only what community psychologists call tertiary prevention but also primary and second prevention, efforts to prevent clinical problems in the first place and efforts to keep developing problems from getting worse, respectively). It may not be a controversial issue these days, but those who were not around 50-plus years ago might benefit from appreciating that it was a major kerfuffle. Behavior therapy was trying mightily, some would say frantically, to be taken seriously as a scientific approach to intervention. Arguing that the more scientific people were limited if they were not experienced in applied settings was troublesome and viewed as a risk to the scientific respectability of our approach.

Controlled research (as defined by a community of knowledge-generators at a given place and time) can be informed by clinical experience about which phenomena are worthy of study. In fact, as stated above by Lazarus and myself, relevant clinical science *requires* such applied experiences. Clinical observations have primarily heuristic value; scientific research tests the ideas and hypotheses emanating from the applied setting. The interactions -- two-way street as we put it initially and as I renamed it later, dialectics – are mutually enriching. Both components are essential to a clinical psychology that is both scientifically based and professionally relevant.

Since I was a young pup in graduate school, behavior therapy was an exemplar of this interaction between and blending of research and practice. Indeed, we were doing "evidence-based practice" long before the term and variations thereof became a mantra in mental health fields. But is there a gap between research and practice? Absolutely, and this has for years been the subject of discussion in education and training circles, though we have found an appreciation of the applied side primarily among colleagues who are experienced in clinical work themselves and/or in clinical supervision.[12]

The original position of behavior therapy was that it was the application of "modern learning theory" to the modification of abnormal behavior. This definition was, it always seemed to me, more aspirational than actual – as I learned in graduate school, controversies

[12] The interested reader might like to look at my analyses of APA's report on "empirically-based practice in psychology" published in *The Clinical Psychologist* during my year as president of APA's Division 12, The Society of Clinical Psychology (Davison. 2006a, 2006b, 2006c).

abound in the field of learning and memory. But setting this aside for the moment, Lazarus and I put the challenge this way:

"The clinician… approaches his work with a given set, a framework for ordering the complex data that are his [or her] domain. But frameworks [paradigms, theories, hypotheses, hunches etc.] are insufficient. The clinician, like any other applied scientist, must fill out the theoretical skeleton. Individual cases present problems that always call for knowledge beyond basic psychological principles (Lazarus & Davison, 1971, p. 203)."

This dialectical interplay between theory and research, on the one hand, and practice on the other is where the rubber hits the road. This is true not only in clinical psychology but in every specialization that employs experimental methods. Consider the following from the esteemed Handbook of Social Psychology, a chapter by esteemed social psychologists Eliot Aronson and Merrill Carlsmith:

"In any experiment, the investigator chooses a procedure which he intuitively feels is an empirical realization of his conceptual variable. All experimental procedures are 'contrived' in the sense that they are invented. Indeed it can be said that the *art* [italics added] of experimentation rests primarily on the skill of the investigator to judge the procedure which is the most accurate realization of his conceptual variable and has the greatest impact and the most credibility for the subject (Aronson & Carlsmith, 1968, p. 25).

Principles of Change not Treatment Packages

Consistent with the very beginnings of behavior therapy, my focus has always been on principles and mechanisms rather than techniques and certainly not on treatment packages that are often vigorously marketed in workshops and sold in books. When Albert Bandura, my Doktorvater, published his classic and daunting tome, *Principles of Behavior Modification* in 1969, my delight was surpassed only by my lack of surprise. What was far more important than extant therapeutic procedures or therapies named after their founders/promoters was the underlying mechanisms. An example of this was my 1965 dissertation, the publication of which was entitled "Systematic Desensitization as a Counterconditioning Process" (Davison, 1968).

But, in my view, the focus shifted in the 1980s to comparing treatment package with each other. A landmark effort was by Sloane, Staples, Cristol, Yorkston, & Whipple (1975), followed by the famous NIMH Treatment of Depression Collaborative Research Program (Elkins, Parloff, Hadley, & Autry [1985]) which cost many millions of dollars and which provided material for many scores of articles, each of them seeing in the voluminous data reasons to feel good about Beck's version of CBT, Klerman's psychodynamic therapy (Klerman, 1990), and even the venerable placebo effect.[13]

[13] In my teaching I've sometimes referred to the findings as a giant Rorschach test.

The seeds for a welcome return to basic science can be seen in this excerpt from Goldfried's and my Preface to *Clinical Behavior Therapy*, to wit:

"…. We have attempted to describe the way behavior therapists analyze clinical problems and *move from general principles to clinical applications* [italics added]… We hope that the book will serve a heuristic purpose in helping the reader generate clinical innovations within a broad behavioral framework." (Goldfried & Davison, 1976, pp. vi-vii)."

This principles-focussed conception of CBT (and of any science-based approach to therapeutic change, which was certainly characterized by Rogers and indeed by Freud), has emerged in recent years as a more productive strategy than the treatment package approach of comparing treatment X with Y in what some have called the gold standard for research in psychotherapy. Obviously, I and others have never agreed with that. e.g.,Bandura, 1969; Davison, 1994, 1997, 2000; Davison, Goldfried, & Krasner, 1970; Goldfried, 1980; Rosen & Davison, 2003). I went further almost 20 years ago in proposing a research strategy that turns therapy research on its head:

> "Several years ago I commented on the role of basic research in clinical psychology (Davison, 1994) and had occasion to develop the argument further during a conference sponsored by the National Institute on Drug Abuse (NIDA) concerned with untapped opportunities to use basic research in developing clinical procedures de novo (Davison, 1997). Simply put, searching for change mechanisms in existing effective techniques is to work after the fact, and although such process research is very important . . . , working in the other direction may be even better . . . : Moving from experimentally established principles of change to novel and effective clinical application . . . is an inadequately explored strategy for developing new therapeutic procedures that, from the outset, will have known mechanisms of change (because such research begins with principles of change. (Davison, 2000, p. 581)

Abnormal Psychology Textbook

The complex and vital dialectical tension between science and practice played a role in my collaborating in the writing of an abnormal psychology textbook with my late Stony Brook colleague and friend, John Neale. After teaching the undergraduate course for five years, I came to realize that there wasn't a textbook whose leitmotif was the interplay that I had come to recognize in my clinical work and teaching. I had been using a very fine book by Brendan

Maher (1966) and then for one year the textbook by Leonard Ullmann and Leonard Krasner (Ullmann & Krasner (1969). Maher's book was excellent in its scientific approach to the subject matter but, in my view, didn't emphasize enough the applied side of things. Ullmann and Krasner appealed to my behavior therapy interests but was too extreme in trying to apply operant conditioning to the entire gamut of psychopathology and treatment.

For these and other reasons, I began discussing with Neale in the fall of 1971 whether we could co-author a textbook that would truly integrate science and the clinical application. I saw it at the time as an incarnation of the Boulder Model (Raimy, 1950), with a heavy emphasis on hard-nosed analysis blended with the humanity and complexity of intervention. Reflecting this focus, the subtitle of the first edition was "An Experimental-Clinical Approach (Davison & Neale, 1974)."

Since I was by that time strongly identified with CBT, the book was seen by many as a cognitive-behavioral one integrated with a strong emphasis on biological factors. It was actually by no means limited to CBT, and, especially in succeeding editions, the importance of non-cognitive-behavioral perspectives was explored at length and in depth. Our primary audience was the so-called upper-tier undergraduate market and, to some extent, beginning graduate students in the mental health disciplines. For me, the book and its many succeeding editions constituted the most intense and challenging scholarly activity of my entire career.

It is gratifying to observe that the book was well-received. I had the responsibility and the opportunity to describe and critically discuss the kinds of issues in CBT that are covered in this chapter. In my more than 55 years of teaching, I have never worked harder than when I had to explain the complexities of psychopathology, science and practice, CBT and of psychotherapy generally in this book and in my hundreds of hours in the classroom. It has been said that you never really understand a topic until you've explained it adequately to (motivated) undergraduates and to graduate students. I can attest to that simple truth.[14]

Clinical Complexity and Psychotherapy Integration

Based on our respective clinical supervisory experiences, my good friend and distinguished Stony Brook colleague, Marvin Goldfried, and I began collaborating in 1972 on "Clinical Behavior Therapy" (Goldfried & Davison, 1976). I believe that this book was seminal in what was then the somewhat heretical notion that we (cognitive) behavior therapists might have something to learn from our non-behavioral colleagues and vice versa. Coupled with Wachtel's classic 1977 book, "Psychoanalysis and Behavior Therapy: Toward a Rapprochement," I believe we helped create a fruitful dialogue with theorists, researchers, and

[14] Having studied during a Fulbright year at the University of Freiburg, it was quite thrilling to be told by German colleagues that Davison/Neale has been a staple for decades in the Staatsexamen in psychology, required for licensure and professional recognition.

clinicians who began both to feel uneasy about the limitations of their respective approach and to believe that there might be something of value in other approaches.

It seemed to us that the more hands-on actual clinical experience one had, the less certain one was with the hegemony of one's preferred theoretical orientation. To be sure, one way that science progresses is for scientists to be dogged about their paradigm or theory as a way to test the limits. Researchers seldom forsake an hypothesis or, or a grander scale, their theory or paradigm the first time that an experiment doesn't work out or, in clinical settings, when one's preferred approach does not yield the hoped-for outcome. It's a tricky business to know when to give up on an idea and when to stay with it by pursuing additional research or clinical innovations.[15]

Goldfried and I held then, and hold now, that when clinicians of *any* theoretical persuasion engage deeply in actual practice and/or thoughtful clinical supervision, they recognize the limits of their preferred paradigm.   Sometimes techniques can be imported and assimilated into one's applied and conceptual efforts (cf. the technical eclecticism of Arnold Lazarus and the theoretical integrative efforts of Paul Wachtel), sometimes not. Certainly we see in the mindfulness and acceptance approaches of the past three decades a willingness to look outside of what can reasonably be regarded as a cognitive-behavioral paradigm, and to develop techniques and theories that take us far afield from "the mother ship." This "third wave" is discussed in other chapters of this volume.

It may be instructive to quote from the Preface of Goldfried and Davison:

"A colleague of ours [Paul Wachtel] once alluded to a 'therapeutic underground' among clinical workers of various orientations. He struck a resonant chord, for we are continually impressed by the distance between written descriptions of behavior therapy and what occurs in practice. In *Clinical Behavior Therapy*, we have tried, within the constraints of the written word, to describe in details the complexities inherent in effective and humane intervention into the lives of others.

"As behavior therapists, we are ever-mindful of the importance of tying our clinical procedures to our data base. Whenever possible, we present material that is consistent with available research. But as any knowledgeable student of behavior therapy can appreciate, more is required of the behavioral clinician that familiarity with well-established principles and procedures. Much of what you will find in this book will necessarily be based on clinical experience, our own and that of our students and colleagues. While some readers may be uncomfortable with an appeal to clinical experience, for the time being this seems to be the most straightforward way of talking about clinical behavior therapy and, most important, communicating our thinking to others. A special virtue of the behavior therapy approach is that

[15] For careful and incisive arguments *against* integration, see inter alia Haaga (1986).

we are answerable to data, and are prepared to alter or give up entirely any suggestion contained in this book that is found wanting in the light of controlled research.

"…. We have attempted to describe the way behavior therapists analyze clinical problems and move from general principles to clinical applications… We hope that the book will serve a heuristic purpose in helping the reader generate clinical innovations within a broad behavioral framework." (Goldfried & Davison, 1976, pp. vi-vii)."

The foregoing is meant to convey a few things of relevance and, I hope, interest. First and foremost, it's the focus we had – and still have, along with many colleagues – on the gap between science and practice and on the exquisitely complex challenges clinicians confront at every moment with a patient. How do I intervene right now and in the future in a way that has the most scientific evidence behind it while at the same time making sense for this particular patient at this particular time? This was a question Lazarus and I had framed a few years earlier, as described above.

This science-practice gap is hardly specific to cognitive behavior therapy, but I think it is especially pertinent for us because our core foundational assumption is that we can apply findings from controlled research, usually analogue in nature, to messy real-life situations, the complexity of which are never more profound and at times more daunting than when dealing with behavior considered to be abnormal and at least worthy of professional change efforts.

Related to this central theme of Goldfrief/Davison is a feature discussed next that was designed to try to make vivid the great complexity and intellectual challenge of clinical work.

The Therapist's Thoughts and Feelings During Interactions with Patients

Our cognitive-behavioral perspective and our intensive involvement in both clinical supervision and hands-on clinical work, along with my own leadership of Stony Brook's unique postdoctoral program in behavior therapy, established in 1966, the same year that saw the first graduate students entering our as-yet-unaccredited clinical Ph.D. program – all of these factors blended into a feature that I believe Goldfried and I innovated in *Clinical Behavior Therapy* (1976, 1994). We wanted to share with the reader the reasoning behind the on-the-ground implementation of change principles. So we included numerous transcripts throughout the book but with a novel pedagogical device that we believe is the core of good clinical supervision. This was basically a think-aloud strategy that for me evolved from my senior thesis with Bruner and anticipated my development of a research paradigm to be described below. This pedagogical technique can be seen in the following excerpt from the third session of a course of therapy. It illustrates how a cognitive behavior therapist worked to reconceptualize the patient's complaints into a behavioral framework, giving her problems what we called "a behavioral twist", a particular conceptualization of the patient's complaints. The italicized text in brackets are the thoughts of the therapist:

“Therapist: I’d like now to give you an idea of the problem as I see it, and then you can tell me whether or not I’ve missed anything, and whether or not it agrees with the situation as you see it. *[I think I’ve pretty much covered the major problem areas. It’s time for me to present a summary statement (a la Sullivan) so that she can fill in any gaps or change any misconceptions I may have about the presenting problem. It can also help me communicate to her that I’ve been listening to what has been said so far and that I’m trying to understand her.]* The primary problem that you want to have dealt with involves your nervousness and anxiety in social situations, primarily new situations, and particularly when you feel you are being evaluated by others. This may involve being at a party, presenting a talk, and other similar situations. Does that sound accurate?

“Client: Yes, that’s about it. The most important problem in my day-to-day life is really my anxiety when I’m with people….

“Therapist: *[Based on what she said earlier, I think her problem reflects more of an inhibition than an actual behavioral or skill deficit…I’m going to have to check it out further within the next session or so….]* There was one other thing you had mentioned. You said that when you are in social situations, you know what to say, and you know what to do, but you feel too nervous to say or do it. Is that right? You become immobilized?

“Client: Well, yes, but I wouldn’t say I’m immobilized, though I think I should certainly be much better than I am. I do know what to do. I’m just afraid (Goldfried & Davison, 1976, pp. 68-69).”

We were trying to provide a glimpse into the inner world of the therapist, the kind of monologue that all sentient beings engage in as they negotiate their way. A common element in clinical supervision is not only observing and discussing what one’s supervisee did with a particular patient at a particular time during a session but *why* the student-clinician did it. This teaching strategy is, I opine, no different from the general cognitive-behavioral approach, only in the present context it entails both the supervisor and the supervisee attending carefully to the thoughts and feelings coursing through the clinician’s mind and using this information to understand the reasons for what the therapist does.

Furthermore, attending to actual problem-solving in concrete applied situations makes one less doctrinaire, I believe. It’s necessary to have principles and a theoretical framework when doing applied work, but abstractions are not enough. As APA’s report of “empirically based practice in psychology” (APA, 2006) suggested, idiographics matter, and when one is faced with the challenge of applying abstractions, one inevitably ventures out of one’s particular conceptual framework, however rough and crude as it may be, to put meat on the theoretical skeleton, to use the metaphor Lazarus set forth in our 1971 effort.

The Phenomenological Essence of CBT

In recent years I have been teaching a first year required course in University of Southern California's clinical science program entitled "Clinical Interviewing and Professional Issues." For much of the semester, we practice Rogerian interviewing, something which, in my halcyon graduate school days at Stanford, was ignored or even derogated as an unnecessary element of "insight therapy," one of the betes noir of the brave new movement. I began to see the undesirability of this extreme focus when I spent much of my second year sitting in on numerous clinical sessions conducted by Arnold Lazarus, as noted earlier. Watching him for hundreds of hours, I noticed that what he called "the nonspecifics" were really not non-specific at all, rather they involved the kind of empathic listening that is the foundation of Carl Rogers's work. I began to see these strategies as a way both to establish a trusting working relationship with the patient and also, most importantly, as a means to get relevant information that was essential to designing and implementing a behavioral intervention. Empathic listening helps fill out the familiar functional-analytic framework for determining what Bandura called the "controlling variables" necessary for devising and implementing a science-based intervention.

And yet, as I have argued for many years, CBT has much in common with humanistic perspectives because it is at its core phenomenological. As I put it 40 years ago:

"All cognitive behavior therapists heed the mental processes of their clients…. They pay attention to the world as it is perceived by the client. It is not what impinges on us from the outside that controls our behavior, the assumption that has guided stimulus-response psychology for decades. Rather our feelings and [overt] behavior are determined by how we view the world. [As often cited by Albert Ellis] The Greek philosopher Epictetus stated it in the first century, 'Men are not disturbed by things, but by the views they take of them'. Thus behavior therapy is being brought closer to the humanistic therapies. A central thesis of therapists like Rogers and Perls is that the client must be understood from the client's own frame of reference, from his or her phenomenological world, for it is this perception of the world that controls life and behavior.

"From the philosophical point of view, such assumptions on the part of those who would understand people and try to help them are profoundly important. Experimentally minded clinicians and researchers [i.e., cognitive behavior therapists and researchers] are intrigued by how much the new field of cognitive behavior therapy has in common with the humanists and their attention to the phenomenological world of their clients. To be sure, the *techniques* used by the cognitive behavior therapists are usually quite different from those of the followers of Rogers and of Perls. But as students of psychotherapy and human nature, these surface differences should not blind us to the [conceptual] links between the two approaches (Davison & Neale, 1982, pp. 616-617)."

This is an important point, so allow me to elaborate.

The phenomenological core of humanistic and existential therapies, which is essential to CBT, is, I believe, evident in the fact that Rogers and his followers did not restrict their empathic work to what is obvious in the client's verbal and nonverbal expressions. This was spelled out more clearly in Gerald Egan's "The Skilled Helper" (Egan, 1975). Here's an example I have used often in my teaching of both undergraduates and graduate students:

"Client: I don't know what's going on. I study hard, but I just don't get good marks. I think I study as hard as anyone else, but all of my efforts seem to go down the drain. I don't know what else I can do.

"Counselor A: You feel frustrated because even when you try hard you fail [primary empathy].

"Counselor B: It's depressing to put in as much effort as those who pass and still fail. It gets you down and maybe even makes you feel a little sorry for yourself [advanced empathy]. (Egan, 1975, p. 135)"

Bear in mind that therapists operating both within a humanistic-existential framework and a cognitive-behavioral one assume that the client views things in an unproductive way, as evidenced by the psychological distress that has brought the client into therapy. At the primary empathic level, the therapist accepts this view, understands it, and communicates to the client that it is appreciated and respected. But *at the advanced or interpretive level, the therapist offers something new, a perspective that he or she hopes is more productive and implies new modes of action*. Advanced empathizing builds on the information provided over a number of sessions in which the therapist concentrates on making primary-level empathic statements.

The client-centered therapist, operating within a phenomenological perspective, *must* have as the goal the movement of a client from his or her present phenomenological world to another one, hence the importance of the advanced-empathy stage. *Since the core belief of both the humanists and cognitive-behavioral clinicians is that people's emotions and actions are determined by how they construe themselves and their surroundings—by their phenomenology—those who are dysfunctional or otherwise dissatisfied with their present mode of living are in need of a new phenomenology.* From the very outset, then, all phenomenological therapies concentrate on clients adopting frameworks *different from* what they had when they began treatment. Merely to reflect back to clients their current phenomenology cannot in itself bring therapeutic change. *A new phenomenology must be acquired*.

Thus, the core of CBT is essentially the same as all the phenomenological therapies – what matters most is how people construe their world. And I would propose also that the essence of Freud since his second theory of anxiety has been that the perception, the recollection that people have of their past fearsome events, is more important that what may actually have happened. This is worlds away from original behavior therapy, whereby the person responds to stimuli and is either reinforced or not. That's an oversimplified picture of course but it is not inaccurate. What Rotter, Kelly, Mischel, Bandura, and even myself brought into the picture was the centrality of how patients view the world, the meaning they attach to what is going on in and around themselves. The defining feature of the CBT paradigm has always been that these constructions of the world can change the person's emotional and behavioral reactions in enduring ways.

This refocus on the internal has not been easy fit, and I have interacted over the years with many CBT colleagues who object to being in bed with theoreticians and therapists whom we have actively and sometimes poetically (cf. Salter, supra at footnote 7) vilified. But at the end of the day, I believe that is where we have found ourselves since at least the mid-1960s, with the seeds on this paradigmatic shift being discernible in people not usually regarded as part of the CBT family (e.g., George Kelly and Julian Rotter).

The foregoing is most assuredly not new to today's cognitive behavior therapy. And that's the point, for these ideas and practices were either poo-pooed by behavior therapy's leading lights in the 1950s and 1960s or were assigned to the realm of "clinical know-how" or "non-specifics", which was intellectually honest but not conducive to searching and sober analysis of psychosocial assessment and intervention.

Early involvement in basic cognitive research

The "cognitive revolution" in CBT of the past 4 decades has another thread for me that I have alluded to above and believe would be useful to describe in greater detail. This takes us back to my undergraduate days. As I wrote in the abnormal textbook with John Neale beginning with the first edition in 1974, *C*BT really represents a return to earlier periods in experimental psychology, for example the research of Duncker on problem-solving (Duncker, 1926). My own extended and intensive exposure to the study of cognition was during my undergraduate years as a research assistant to and then a senior honors thesis advisee to Jerome S. Bruner, one of the pioneers of the so-called "new look" in perception that germinated soon after the second world war. Together with George Miller and other colleagues, Bruner's prolific theoretical and experimental publications (e.g., Bruner, Goodnow, & Austin, 1956) demonstrated the central importance of cognition in understanding the human condition, a general perspective which I saw at the time as a response to his Harvard colleague, B.F. Skinner, and his behavioristic focus on reinforcement contingencies with no inference to internal cognitive and affective processes.

Pivotal for my entry into CBT years before the concept even existed was doing my honors thesis with Bruner in 1955-1957. The purpose of my thesis was to explore Duncker's concept of "functional fixedness" -- familiarity with a cognitive challenge can interfere with rather than facilitate one's solving it if one cannot shake an hypothesis that is not proving fruitful. Changing one's mind is often very difficult. Under Bruner's supervision, I adapted Jean Piaget's (Piaget, 1954) recording of children talking to themselves while they solved problems. I had undergraduate subjects verbalize their hypotheses about what was in pictures that were shown to them gradually coming into focus, beginning with presentations in which each picture was so blurry that virtually no one could accurately identify it. Participants' words were tape-recorded, transcribed, and then content-analyzed.

For example, one of the pictures I used was of a black puppy standing in sunlight on grass. When the photo is very much out of focus, nearly everyone sees it as some kind of heavy dark object like a sofa, a fat pig, some other kind of heavyset thing. But as the photo becomes clearer, the shadow underneath the puppy's stomach becomes discernible as separate from the animal's stomach. There is a sliver of light between the tummy and the shadow, thus rendering the heavy dark thing as not so heavy and fat, leading to the "aha" experience of its being a slimmer puppy. I coined the term "constraint set" for the underlying assumption that tied together all the pre-recognition hypotheses. The research participants *seemed* to be changing their minds as the visual information improved, but, at a more basic level, they were not. Like scientists operating within a theory or paradigm, their perception was, I proposed, constrained by their general assumption of what the dark object was. And their hypotheses, guesses actually, were almost always wrong because the poor focus of each picture kind of seduced them into adopting a constraint set that was inconsistent with the actual visual stimulus. They had to free themselves from their earlier underlying assumption as the focus improved.

The analogy I draw when I discuss this experiment with my students as an analogue to scientific thinking is the old joke about the inebriated man crawling around under a streetlamp at midnight. "What are you doing?" asks a suspicious police officer. "Lookin' for my keys," mutters the drunken man. "Well, do you remember where you lost them?" inquires the police officer, now trying to be helpful. "Over there," says the man, gesturing to a dark area several yards in the distance. "Why are you looking for the keys *here?* " asks the officer incredulously. "Because there's light here from the lamp." I tell students that if they get the joke, they have some understanding of the nature of paradigms and theories in science.

Thus, in addition to replicating earlier research that prior exposure to suboptimal visual stimuli interferes with accurate perception, my content analysis of participants' pre-recognition hypotheses suggested a reason for this delay. When people are trying to understand something that is complex and murky, they usually have unspoken (unconscious, actually) assumptions of what it could be. When the data are poor, their initial ideas are probably wrong. And these ideas, though they may be changing as the information improves, are usually within a restricted domain of which they are seldom even aware. People usually get attached to these underlying assumptions even when additional and improved data become available. This has been a theme in psychology from its very beginnings as a science, cf. the Wurzburg School's concept of "unbewusste Einstellung," or "unconscious set", in the early 20th century, a concept applied mostly to perception. and more recently elaborated in the study of implicit bias in social prejudice (Greenwald & Banaji, 1995; 2017).

Even as a newly minted B.A. in 1961 – or maybe *because of my youth* – I boldly suggested that my analogue experiment had implications far beyond looking at fuzzy pictures gradually being brought into focus. I ended that first publication of mine (Davison, 1964) with the proposal that my analysis of the findings could be viewed as the way scientific hypotheses and theories function to both facilitate discovery and to discourage it. Not to be limited to scientific inquiry, my imagination took flight to propose that a societal-cultural Weltanschauung (world view) could be fruitfully understood as a massive constraint set that helps make sense of the world but that can also interfere with new and possibly more useful perspectives.[16]

Cognitive assessment

Over the past 40 years or so there has been increasing interest in assessing the thoughts and feelings, both overtly expressed and implied, as people go about their daily lives. My think-aloud work with Bruner guided me to design a procedure that could, I thought, enable us to assess thoughts and feelings in a situational context consistent with the functional analytic behavioral paradigm.

In my original experiment on what we (Davison, Robins, & Johnson, 1983) called the "Articulated Thoughts in Simulated Situations paradigm" (ATSS), subjects are instructed and coached into immersing themselves imaginally in audiotaped complex interpersonal situations, like being criticized, and verbalizing what is going through their minds (cf. my discussion below of my undergraduate think-aloud research with Jerome Bruner). To facilitate accessing their thoughts and feelings in a non-retrospective and very situational fashion, our fictional scenarios are divided into segments of between five and ten seconds in length. After each seconds-long segment is presented, there is a pause and a signal to talk out loud about what is passing through their minds in reaction to what they have just heard. After about 30 seconds to permit thinking aloud, another signal tells them to listen to the next segment and imagine some more, and so on through a number of segments that comprise the story. The raw data can then be content-analyzed in an infinite number of ways depending on one's theoretical focus.

Since the publication of the first article in 1983, dozens of subsequent experiments, both in my lab and elsewhere, have investigated the cognitive components of a wide range of human problems such as social anxiety, depression, hate crimes, fear of flying, marital anger and aggression, and withdrawal from smoking (for reviews see Davison, Coffman, & Vogel, 1997; and Zanov & Davison, 2008). Psychometrically the ATSS has been shown to possess good content, concurrent, predictive, and construct validity; and a variety of coding schemes have been applied with a very high measure of interrater reliability. The ATSS is part of the growing interest in situational cognitive assessment, such as Ecological Momentary Assessment (Stone & Shiffman, 1994) and related approaches.

[16] Imagine my excitement when I read Thomas Kuhn's analysis of scientific paradigms in terms of perceptual set (Kuhn, 1962).

Ethics and psychotherapy of all kinds

My doctoral education in behavior therapy, as rigorous and sophisticated as was available at the time, eschewed careful consideration of ethical issues, specifically, what the goals of intervention were and how they were decided upon. Not that goals were regarded as unimportant! The very nature of behavior therapy required a clear sense of the directions that therapy would take. Not only was this fair to the patient but it was essential in any attempts to evaluate the success of assessments and intervention -– one needs a dependent variable to do experimental research, after all.

But the kinds of changes that a behavior therapist and the client focused on were seen as separate from the theories and findings being applied in therapeutic change efforts. And in a way the two issues are. But responsible application cannot properly eschew ethical considerations. This issue often brings to mind what I have described as the Will Rogers "aw shucks" model. It goes something like this: "I'm just a simple technician. I have techniques that I can use to help you move from Point A to Point B. Point A is where you are right now. Point B is the goal of treatment. The latter is your choice entirely. I'm not going to make that judgment for you. Assuming that getting you to Point B is not illegal and/or unethical – I won't desensitize you to any anxieties around murdering someone, for example – you can hire me to help you get there."

Most of the time this works, that is, there is nothing to worry about. We are usually children of the same culture; agreement on values is more the rule than the exception. The earliest patients that Wolpe and Lazarus reported on were clearly suffering from anxieties and depression that were unwarranted, even diagnosable in the DSM of the period, and which, if alleviated or eliminated, would allow the patient to live a happier, more productive life. Surely a person who avoids social interactions because of debilitating fears of negative evaluation deserves the benefit of the evolving behavior therapy armamentarium of techniques that have been demonstrated to be effective and that, at least metaphorically and rhetorically, derive from theory and experimental data, the essential lifeblood of behavior therapy.

I had no problems with this perspective and system of (implicit) beliefs in graduate school and in the first few years of my academic and professional life. But things began to shift, perhaps beginning with the following experience in the small, part-time clinical practice that I have had for most of my career. One day in the spring of 1970 I was consulted by a very accomplished professional woman who, knowing of my expertise in anxiety-reduction via systematic desensitization, asked me to help her eliminate the extreme anxiety she felt about her husband cheating on her. Possible anxiety hierarchy items, it was readily determined even in the first session, included sitting alone at her kitchen table, a lovingly prepared dinner for two getting cold, with the time approaching 10:00 pm, and her husband not yet home. The anxiety was usually accompanied by anger and/or feelings of hopelessness and depression. Other anxiety-provoking scenes could readily be determined as I listened to her tearful account.

However, I felt uncomfortable with her request. Running through my mind were questions as to whether, in *my* system of values and ethics, a spouse *should be* or *has every right to be* anxious and angry about their partner showing all sorts of signs of being unfaithful. So perhaps half an hour into the initial session, I decided to share my ethical concerns and, while allowing for people's intimate relationships to be highly variable, I said (gently but unequivocally) that I would not be comfortable working toward her stated goal of being able to tolerate her husband's infidelity. Then I engaged her in a discussion about her own perspectives on marriage. I no longer have my notes on this session of more than 50 years ago but I do clearly recall her *relief* that I was not prepared to meet her stated wishes. She was eager to schedule several more sessions to discuss the problems in her marriage and how she might try to make changes in the relationship rather than within herself in an effort to remain married. As things turned out, I learned a few years after termination that the marriage had been dissolved.[17]

I'm certain that other therapists have had similar experiences, many of them preceding my own. My point is that, despite what I consider to have been very good education and training in behavior therapy in the early 1960s, I cannot recall these issues being thoroughly explored.[18]

My concerns about ethics and behavior change took an unexpected and rather cataclysmic turn when I became president-elect of the Association for Advancement of Behavior Therapy in 1972. As may be familiar to some readers, I argued against offering sexual reorientation therapy to gay people in my 1974 AABT presidential address. I had been inspired by remarks of Charles Silverstein (1972).[19] The core of my speech (published two years later, Davison, 1976) was that the values and biases of therapists inevitably influence the way they construe problems and which goals they work towards; that goals are determined much more by the therapist than by the patient; that therapists never make decisions about goals outside of a political and moral context; and that change-of-orientation programs should be stopped, even when gay patients request them, because prejudice and often physical attacks have made it highly unlikely that "voluntary" change requests are in fact self-determined. Several years later, I offered the following fantasy to try to encapsulate the situation of gays in therapy as of the 1970s (that this argument may seem belabored and unnecessary in the 2020s speaks to how much things have changed in many segments of North American society and indeed around the world):

"API (Apocryphal Press International). The governor recently signed into law a bill

[17] The reader may have a different conception of marriage. It might be religious – marriage is sacred and divorce must be avoided at all costs. This possibility proves my point.

[18] The exception was Lazarus's ethical view that sometimes efforts to "save a marriage" can be not only unsuccessful but even destructive and demeaning.

[19] A documentary film, "Conversion", has recently been released that portrays Charles Silverstein's and my seminal roles in the movement against conversion therapy..

prohibiting discrimination in housing and job opportunities on the basis of membership in a Protestant Church. This new law is the result of efforts by militant Protestants, who have lobbied extensively during the past ten years for relief from institutionalized discrimination. In an unusual statement accompanying the signing of the bill, the governor expressed the hope that this legislation would contribute to greater social acceptance of Protestantism as a legitimate, albeit unconventional, religion.

"At the same time, the governor authorized funding in the amount of twenty million dollars for the upcoming fiscal year to be used to set up within existing mental health centers special units devoted to research into the causes of people's adoption of Protestantism as their religion and into the most humane and effective procedures for helping Protestants convert to Catholicism or Judaism. The governor was quick to point out, however, that these efforts, and the therapy services that will derive from and accompany them, are not be imposed on Protestants, rather are only to be made available to those who express the voluntary wish to change. 'We are not in the business of forcing anything on these people. We only want to help,'" he said (Davison, 2001).

When a lead article based on my speech was published two years later in *Journal of Consulting and Clinical Psychology* (Davison, 1976)[20] , there were invited critiques by Seymour Halleck, Hans Strupp, and Irving Bieber. You can imagine Dr. Bieber's paper. Since publication my article has become part of a growing and influential literature on dealing with human problems that homosexuals can have rather than the alleged problem of homosexuality that had to be "fixed." Beginning in the 1980s there have been far fewer requests for sexual orientation change. Indeed, 19 states and several countries have made it illegal to offer sexual reorientation treatment at least for minors. I expect this will be extended to people of all ages in the next decade or two.

It merits mention that the argument that such programs *can* succeed if more effort is put into them (e.g., Sturgis & Adams, 1978) is irrelevant. In an invited response to their article, I pointed out that the decision is an ethical and political one, not an empirical one. "Not Can But Ought" was the title of my rejoinder to Sturgis and Adams (Davison, 1978).

Over the years, I have extended the argument against sexual conversion therapies to the entire gamut of assessment and intervention. As articulated in a

[20] When I first submitted my paper to the *American Psychologist*, I received a brief letter from the editor saying that he was rejecting it without sending it out for review because he did not consider it of general enough interest to the APA membership. I was dumbfounded by this editorial gatekeeper decision so I sent it to JCCP. As before, I received a thin envelope a week later which, I feared, was the same negative decision. To my delight (though not surprising, knowing the editor, Brendan Maher), the decision was also not to send it out for review but to accept it right away and, if I agreed, to invite critiques. My first recommendation was Irving Bieber, who I knew would assert an opposing position. I was not disappointed.

behavioral medicine handbook a few years ago:

"Often, the most important and influential forces in our immediate world are those that we think little about in our day-to-day life. If we are fish, our values are the water that surrounds us. They guide our thoughts, our questions, and our behaviors. They inform us if we are doing something "right" or "wrong" and can sway us in different directions, like the waves of an ocean. While this guidance, of which we are usually unaware, can be good in many ways, our values feel so natural to us – to the extent that we even think of them – that they can sometimes be mistaken for absolute truths.

"We – both scientists and non-scientists – take certain values for granted, not even considering them an issue. For example, we can safely assume that most individuals would not tolerate a child banging her head against the wall. In fact, in certain situations such as working with children diagnosed with autism, health professionals have gone to great lengths, including heavy sedation and/or physical restraints, to prevent this behavior. Why? Well, it has to be because we as a society value keeping the human brain as undamaged as possible. But why do we value this? The reason has to be that we place a high value on children benefitting from life experiences that require as undamaged a brain as possible. These value choices sometimes result in our being prepared to take drastic measures to protect human brains. As social scientists and human beings, we certainly agree with this position, but it is a values-laden position, not an empirical one (Davison & Feng, 2018, p. 1053).[21]

[212121]21 In my teaching I have tried to drive home the simple truth of "not can but ought" by telling students that I have a cure for all human problems. It's inexpensive, direct, and sure-fire. After getting their attention, I announce that my cure is a bullet in the head. It's been my experience that many students are shocked, even scandalized, by this. I encourage that reaction and use it to make vivid that we don't always do what it is in our capacity to do! Health professionals swim in these waters all the time, but like the proverbial fish who don't know that they are swimming in water, they don't realize their political, legal, and moral constraints until they are brought to their attention.

Clinical Problems as Clinicians' Constructions

In writings since my 1976 article, influenced importantly by my teaching and clinical supervision as well as my activities as a clinician, the conversion issue evolved into a social constructivist epistemology. What most of our patients come to us with are vague complaints, signs, and symptoms that are subject to an infinite array of interpretations/diagnoses. The questions we ask and the methods we use to make our assessments are determined ahead of time by our paradigms and other biases. Therapists, goes my argument, don't simply do what their patients ask them to do. Our decisions about treatment are guided both by legal constraints and, most importantly, by scientific and personal biases about what a problem is and how it might be treated.

Most human psychological problems, then, are constructed by the clinician in ways that are more or less useful. An example is the following discussion of hierarchy construction in systematic desensitization:

"Another aspect to the assessment situation [in considering and designing a regimen of desensitization] is the notion of a basic theme as a *conceptualization* of the therapist. We have long ago stopped asking ourselves whether we have 'truly' isolated a basic anxiety dimension of our clients. Rather, we ask ourselves how best to construe a person's difficulty so as to maximize his gains. In other words, rather than looking for the 'real hierarchy,' we look for the *most useful* hierarchy. This has important implications, not the least of which is the freedom to attempt to reconceptualize various client problems in terms amenable to desensitization. An … example [is] how one might fruitfully construe a problem of depression in terms of an anxiety/avoidance gradient, where desensitization would be appropriate. The clinician must ask himself what the implications are likely to be should a particular desensitization actually succeed. For instance, will a person depressed about her lack of meaningful social contacts be happier if her inhibitions about talking to people are reduced by desensitization? Looked at in this way, the clinician would seem to have both greater freedom and greater challenge in isolating anxiety dimensions" (Goldfried & Davison, 1976, p. 115)."

Put differently, and this is how my position against conversion therapy blends with my social constructionist perspective:

"… clients seldom come to mental health clinicians with problems as clearly delineated and independently verifiable as what patients often bring to physicians. A client usually goes to a psychologist or psychiatrist in the way described by Halleck (1971). That is, the person is unhappy; life is going badly; nothing is meaningful; sadness and despair are out of proportion to life circumstances; the mind wanders and unwanted thoughts intrude, etc. The clinician *transforms* [italics in original] these often vague and complex complaints into a diagnosis or functional analysis, a set of ideas of what is wrong, what the controlling variables are, and what might be done to relieve the suffering and maladaptation. My argument, then, is that psychological problems are for the most part *constructions* of the clinician. Clients come to us in pain, and they leave with a … problem or set of problems that we *assign* to them (Davison, 2001, p. 347)."[22]

[22] This social constructivist argument seems far less appropriate for psychological problems that have or are believed to have a biological basis, cf. Paul Meehl's (1999) "carving nature at its joints".

Conclusion

"Behavior therapy" used to be synonymous with "the conditioning therapies" as articulated by our innovative and intellectually courageous pioneers – people like Joseph Wolpe, Andrew Salter, Cyril Franks, Hans Eysenck, Arthur Staats, Albert Bandura, Walter Mischel, and

Arnold Lazarus. Most of them, in my view, evolved in a cognitive direction in the mid- to late-1960s, some of them perhaps influenced by the seminal writings of Albert Ellis and Tim Beck. I had the dumb luck of being a part of this by virtue of having entered the Ph.D. program at Stanford in 1962 with the express purpose of specializing in anything but clinical psychology. I believe I was able to contribute to the evolving cognitive directions of behavior therapy in the ways described in this paper – cognitive restructuring of a paranoid delusion, arguing for agency in deep muscle relaxation and in countercontrol, attribution in the maintenance of behavior change, perceived as contrasted with actual control, integrating humanistic elements into CBT, the complexities of the science-practice dialectic, calling attention to the essential phenomenological nature of CBT, providing insights into therapists' thinking through a pedagogical innovation in explaining clinical applications, social constructivism in clinical assessment, innovating with a laboratory-based think-aloud cognitive assessment paradigm, and the ethics and politics of conversion therapy for gay people.

I conclude now by offering for your consideration, whether you are a student, practicing professional, or an academician, some general comments about interdisciplinarity, breadth within the field of psychology, and the role of the liberal arts. As I wrote in an earlier article:

> ".... a liberal arts education provides undergraduate psychology majors – who account for the vast majority of applicants to our doctoral programs – with a suitably broad historical, social, and philosophical context for their specialty study of psychology. But ... when students apply to graduate psychology programs, the primary focus of admissions committees is, I believe, on statistics, research methods, psychology content courses, and especially involvement in psychological research to the virtual exclusion of non-psychology work and intellectual interests that can provide ... [a] broad context [for understanding the human condition] ....
>
> " Once they enter a doctoral program in clinical or counseling psychology, the de-emphasis on topics not tightly linked to psychology becomes even stronger. When Ph.D. programs required comprehensive examinations, including history and systems, there was some assurance that students would gain a modicum of exposure to the larger historical, social, and epistemological context of the study of the human condition. But [I believe that] students are not being encouraged or required to appreciate the macro factors that influence their subject matter (Davison, 2005, p. 1062)."

And with respect specifically to clinical psychology programs:

“I have long believed in the importance of a solid liberal arts education as the foundation for all fields of graduate and postgraduate specialization. [But the liberal arts are especially important to clinicians, and perhaps particularly for cognitive behavior therapists.] Whether it makes the more hard-nosed amongst us uncomfortable or not, both researchers and clinicians – to the extent that there are sharp differences between them – have to be *Menschenkenner*, people who know and understand people, including themselves. I believe that a broad education -- in addition, no doubt, to some inborn abilities of empathy and interpersonal sensitivity -- can contribute to the ability to figure out the vagaries of human conduct and how most effectively to devise ethically proper methods of change. (Davison, 2006b, page 3).”

I have for years disagreed with most of APA/s standards and procedures for accreditation, but on one issue I have always believed they have it right, namely the importance of *breadth*, that is, the need for clinical psychologists (and other mental health professionals of course) to engage in graduate level study of history and systems, social, developmental, neuroscience, quantitative, research methods, and cognition and learning. And that preferably they study these specialties with faculty who are content experts, which usually means faculty who are not in clinical programs. I do not believe, for example, that the cognition and learning requirement be satisfied by taking a cognitive behavior therapy course with someone like myself.

As both a scientific endeavor and a profession offering effective, humane, and morally sound interventions, cognitive behavior therapy has a heavy responsibility. I hope that this journey of my own development will prove useful to the reader.

References

American Psychological Association. Evidence-based practice in psychology. *American Psychologist, 61*, May-Jun 2006, 271-285

Aronson, E. (1972). *The Social Animal.* San Francisco: Freeman.

Aronson, E., & Carlsmith, J.M. (1968). Experimentation in social psychology. In G. Lindzey & E. Aronson (Eds.),*The Handbook of Social Psychology: Vol. 2, Research Methods.* Menlo Park, PA: Addison-Wesley.

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

Bandura, A. (1969). *Principles of Behavior Modification.* New York: Holt, Rinehart and Winston.

Bandura, A. (1977) Self-efficacy: Toward a unifying theory of behavioral change. *Psychological Review, 84,* 191-215.

Basedow, H. (1925). *The Australian Aboriginal*. Adelaide, Australia: FW Preece

Beck, A. T (1967). *Depression: Clinical, Experimental, and Theoretical Aspects*. New York: Harper and Row.

Beck, A.T., Grant, P., Inverso, E., Brinen, A.P., and Perivoliotis, D. (2020). *Recovery-oriented Cognitive Therapy for Serious Mental Health Conditions*. New York: Guilford Press.

Bettelheim, B. (1960). *The Informed Heart: Autonomy in a Mass Age.* Mankato, MN: Free Press

Brehm, J.W. (1966). *A Theory of Psychological Reactance.* New York: Academic Press.

Bruner, J.S., Goodnow, J., & Austin, G.A. (1956). *A Study of Thinking*. New York: Wiley.

Davison, G.C. (1964). The negative effects of early exposure to suboptimal visual stimuli. *Journal of Personality, 32*, 278-295.

Davison, G.C. (1966a). Anxiety under total curarization: Implications for the role of muscular relaxation in thedesensitization of neurotic fears. *Journal of Nervous and Mental Disease, 143,* 443-448.

Davison, G. C. (1966b). Differential relaxation and cognitive restructuring in therapy with a paranoid schizophrenic or paranoid state. *Proceedings of the 74th Annual Convention of the American Psychological Association*. Washington, DC: American Psychological Association.

Davison, G. C. (1969). Appraisal of behavior modification techniques with adults in institutional settings. In C. M. Franks (Ed.), *Behavior Therapy: Appraisal and Status*. New York:McGraw-Hill. Pp. 220-278.

Davison, G. C. (1973). Counter control in behavior modification. In L. A. Hamerlynck, L. C. Handy, & E. J. Mash (Eds.), Behavior change: Methodology, concepts, and practice (pp. 153-167). Champaign, IL: Research Press.

Davison, G. C. (1976). Homosexuality: The ethical challenge. *Journal of Consulting and Clinical Psychology, 44*, 157-162.

Davison, G. C. (1978). Not can but ought: The treatment of homosexuality. *Journal of Consulting and Clinical Psychology, 46*, 170-172.

Davison, G. C. (1994). Issues in psychotherapy as the practice of psychology. Journal of Psychotherapy Integration, 4, 39-45.

Davison, G. C. (1997). The mutual enrichment of basic and applied research in psychological science and practice. Psychological Science, 8, 194-197.

Davison, G. C. (2000). Stepped care: Doing more with less? Journal of Consulting and Clinical Psychology, 68, 580-585.

Davison, G.C. (2001). Values and constructionism in psychological assessment: Some historical and personal perspectives on behavior therapy. In W.O. O'Donohue, S.C. Hayes, J.E. Fisher, D. Henderson, & L.J. Hayes (Eds.), *A History of the Behavioral Therapies: Founders' Personal Histories* (pp. 337-357). Reno, NV: Context Press.

Davison, G.C. (2005) Some critical observations on twenty-first century graduate education in clinical psychology. *Journal of Clinical Psychology, 61*, 1061-1066. Davison, G.C. (2006a). An invitation to auseindersetzen about the Evidence-Based Practices in Psychology task force report. *The Clinical Psychologist, 59* (Spring), 1-4.

Davison, G.C. (2006b). Graduate education in clinical psychology and the Evidence-Based Practices in Psychology report. *The Clinical Psychologis, 59* (Summer), 1-5.

Davison, G.C. (2006c). Politics, ethics, and goal-setting in psychological interventions: Limitations in the Evidence-Based Practices in Psychology report. *The Clinical Psychologist, 59* (Winter), 1-6.

Davison, G. C., & Feng, M.C. (2018). Values in behavioral medicine. In E. Fisher, L.D. Cameron, A. J. Christensen, U. Ehlert, Y. Guo, B. Oldenburg, & F. Snoek (Eds.), *Principles and Concepts of Behavioral Medicine: A Global Handbook.* New York: Springer.

Davison, G. C., Goldfried, M. R., & Krasner, L. (1970). A postdoctoral program in behavior modification: Theory and practice. *American Psychologist, 25,* 767-772.

Davison, G. C., & Lazarus, A. A. (1995). The dialectics of science and practice. In S. C. Hayes, V. M. Follette, T. Risley, R. D. Dawes, & K. Grady (Eds.), *Scientific Standards of Psychological Practice: Issues and Recommendations* (pp. 95-120). Reno, NV: Context Press.

Davison, G.C. & Neale, J M. (1974). *Abnormal Psychology: An Experimental-Clinical Approach*. New York: Wiley.

Davison, G.C., &, Neale, J.M. (1982) *Abnormal Psychology, 3e.* New York: Wiley.

Davison, G. C., Robins, C., & Johnson, M. (1983). Articulated thoughts during simulated situations: A paradigm for studying cognition in emotion and behavior, *Cognitive Therapy and Research, 7,* 17-40.

Davison, G. C. & Wilson, G. T. (1973). Processes of fear-reduction in systematic desensitization: Cognitive and social reinforcement factors in humans. Behavior Therapy, 4, 1-21.

Davison, G. C., Tsujimoto, R., & Glaros, A. (1973). Attribution and the maintenance of behavior change in falling asleep. *Journal of Abnormal Psychology, 82*, 124-133.Davison, G. C., & Valins, S. (1969). Maintenance of self-attributed and drug-attributed behavior change. *Journal of Personality and Social Psychology, 11*, 25-33.

Davison, G. C., Vogel, R. S., & Coffman, S. G. (1997). Think-aloud approaches to cognitive assessment and the Articulated Thoughts in Simulated Situations paradigm. Journal of Consulting and Clinical Psychology, 65, 950-958.

Deutsch, J.A. (1960). *The Structural Basis of Behavior. Chicago*: University of Chicago Press.

Duncker, Karl (1926). A qualitative experimental and theoretical) study of productive thinking (sSolving of comprehensible problems). *Pedagogical Seminary and Journal of Genetic Psychology. 33*, 642–708.

Ellis, A. (1962). *Reason and Emotion in Psychotherapy*. New York: Lyle Stuart.

Eysenck, H.J. (Ed.), (1960). *Behavior Therapy and the Neuroses*. New York: Pergamon Press.

Festinger, L. (1957). *A Theory of Cognitive Dissonance.* Stanford, CA: Stanford University Press.

Festinger, L., & Canon, L.K. (1965). Information about spatial location based on knowledge about efference. *Psychological Review*, *72*, 373-384.

Geer, J. H., Davison, G. C., & Gatchel, R. (1970). Reduction of stress in humans through non-veridical perceived control of aversive stimulation. *Journal of Personality and Social Psychology, 16*, 731-738.

Goldfried, M. R., & Davison, G. C. (1976). *Clinical Behavior Therapy*. New York: Holt, Rinehart & Winston.

Goldfried, M. R., & Davison, G. C. (1994). *Clinical Behavior Therapy*. *Expanded edition.* New York: Wiley-Interscience.

Goldstein, A.P., Heller, K., & Sechrest, L.B. (1966). *Psychotherapy and the Psychology of Behavior Change.* New York: Wiley.

Greenwald, A. G., & Banaji, M. R. (1995). Implicit social cognition: Attitudes, self-esteem, and stereotypes. *Psychological Review, 102*, 4-27.

Greenwald, A. G., & Banaji, M. R. (2017). The implicit revolution: Reconceiving the relation between conscious and unconscious. *American Psychologist, 72,* 861–871.

Haaga, D. A. (1986). A review of the common principles approach to integration of psychotherapies. *Cognitive Therapy and Research, 10*, 527 538.

Halleck, S. L. *The Politics of Therapy.* New York: Science House.

Hayes, S. C., Strosahl, K., & Wilson, K. G. (1999). *Acceptance and Commitment Therapy: An experiential approach to behavior change*. New York: Guilford Press.

Jacobson, E. (1929). *Progressive Relaxation*. Chicago: University of Chicago Press.

Kelly, G. A. (1955). *The Psychology of Personal Constructs*. New York: Norton.

Kuhn, T.S. (1962). *The Structure of Scientific Revolutions.* Chicago: The University of Chicago Press.

Lazarus, A. A., & Davison, G. C. (1971). Clinical innovation in research and practice. In A. E. Bergin & S. L. Garfield (Eds.), *Handbook of Psychotherapy and Behavior Change* (pp. 196-213). New York: Wiley.

Linehan, M.M. (1993). *Cognitive-Behavioral Treatment of Borderline Personality Disorder: The Dialectics of Effective Treatment.* New York: Guilford Press.

London, P. (1963), *The Modes and Morals of Psychotherapy.* New York: Holt, Rinehart & Winston.

Masserman, J.H. (1943). Experimental neuroses and psychotherapy. *Archives of Neurology and Psychiatry, 49*, 43-48.

Meehl, P.E. (1999). Clarifications about about taxometric methods. Applied and Preventive Psychology, *8*, 165-174

Mischel, W. (1968). *Personality and Assessment*. New York: Wiley.

Miller, N.E. (1948). Studies of fear as an acquirable drive: I. Fear as motivation and fear-reduction as reinforcement in the learning of new responses. *Journal of Experimental Psychology, 38*, 89-101.

Mowrer, O.H. (1939). A stimulus-response analysis of anxiety and its role as a reinforcing agent. *Psychological Review, 46*, 553-565.

Mowrer, O.H., & Viek, P. (1948). An experimental analogue of fear from a sense of helplessness. *Journal of Abnormal and Social Psychology, 43*, 193–200.

O'Donohue, W.O., Hayes, S.C., Fisher, J.E., Henderson, D., & Hayes, L.J. (Eds.), *A History of the Behavioral Therapies: Founders' Personal Histories*. Reno, NV: Context Press.

Peterson, D.R., & London, P. (1964). Neobehavioristic psychotherapy: Quasi-hypnotic suggestions and multiple reinforcement in the treatment of a case of post-infantile dyscopresis. *Psychological Record, 14,* 469-474.

Piaget, J. (1954). *The construction of reality in the child.* New York: Basic Books.

Richter, C.P. (1957). On the phenomenon of sudden death in animals and man. *Psychosomatic Medicine, 19*, 191-198.

Rogers, C. R. (1942). *Counseling and Psychotherapy*: *New Concepts in Practice*. Boston: Houghton Mifflin.

Rotter, J.B. (1954). *Social Learning and Clinical Psychology*. Englewood Cliffs, NJ: Prentice-Hall.

Ruch, T.C., Patton, H.D., Woodbury, J.W., & Towe, A.L. (1961). *Neurophysiology.* Philadelphia: Saunders.

Salter, A. (1949). *Conditioned Reflex Therapy.* New York: Creative Age Press.

Silverstein, C. A. (1972). *Behavior Modification and the Gay Community.* Paper presented at the Annual Convention of the Association for Advancement of Behavior Therapy, New York, October.

Skinner, B.F. (1953). *Science and Human Behavior*. New York: Macmillan.

Staats, A.K., and Staats, C.K. (1963). *Complex Human Behavior: A Systematic Extension of Learning Principles.* New York: Holt, Rinehart & Winston.

Stone, A.A., & Shiffman, S. (1994). Ecological Momentary Assessment (EMA) in behavioral medicine. *Annals of Behavioral Medicine*, *16,* 199-202.

Sturgis, E. T., & Adams, H. E. (1978). The right to treatment: Issues in the treatment of homosexuality. *Journal of Consulting and Clinical Psychology, 46*, 165–169.

Wachtel, P. L. (1977). *Psychoanalysis and Behavior Therapy: Towards a Rapprochement,* New York: Basic Books.

Watson, J.B. (1913). Psychology as the behaviorist views it. *Psychological Review, 20*, 158-177.

Wilson, G. T., & Davison, G. C. (1971). Processes of fear reduction in systematic desensitization: Animal studies. Psychological Bulletin, 76, 1-14.

Wolpe, J. (1952). Experimental neuroses as learned behavior. *British Journal of Psychology, 43*, 243-261.

Wolpe, J. (1958). *Psychotherapy by Reciprocal Inhibition*. Stanford: Stanford University Press.

Wolpe, J., & Lazarus, A.A. (1966). *Behavior Therapy Techniques*. New York: Pergamon.

Zanov, M.V., & Davison, G.C. (2010). A conceptual and empirical review of 25 years of cognitive assessment using the Articulated Thoughts in Simulated Situations (ATSS) think-aloud paradigm. *Cognitive Therapy and Research, 34*, 282-291.