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The neurotic paradox: Progress in understanding and treating anxiety and related disorders by David H. Barlow

Epigram

The Neurotic Paradox

In 1950, O. Hobart Mowrer described a mystery:

> [It is] the absolutely central problem in neurosis and therapy. Most simply formulated, it is a paradox—the paradox of behavior which is at one and the same time self-perpetuating and self-defeating!...Common sense holds that a normal, sensible man or even a beast to the limits of his intelligence, will weigh and balance the consequences of his acts: if the net effect is favorable, the action producing it will be perpetuated; and if the net effect is unfavorable, the action producing it will be inhibited, abandoned. In neurosis, however, one sees actions which have predominantly unfavorable consequences; yet they persist over a period of months, years, or a lifetime. (p.486)

Introduction

I began graduate school in clinical psychology in 1964. Now, as I write this introductory chapter I am 50 years into a stimulating and rewarding career. Thus, when presented with an opportunity by my publisher to recap the highlights of this career in the form of a strategic selection of publications I can only think that it is rare privilege and pleasure indeed for several reasons. First, is the fact that I am still here with my cognitive processes relatively intact (an observation that some would dispute). Second, this opportunity affords me the chance to temporarily pull out of the day to day demands of a busy ongoing program of clinical research as well as the usual and customary academic duties to reflect back on the major trends and developments that have occurred during the last 50 years. In addition to reflecting on these trends and any small part I may have played in developments over this period of time, the hope is, that close observation of the path and process of progress in various areas in which I have worked will be useful in providing some perspective to current and future generations of clinicians and clinical scientists on the nature of scientific advancement in our field. Accordingly I have organized this retrospective by choosing publications from four major areas.

The first area focuses on fundamental **methodology in clinical research**, particularly in the area of **idiographic, single case experimental methods** in the development of psychological treatments as reflected in two papers written almost 30 years apart. The second section carries on with another major theme in my research on going for the full 50 years, **the development of new treatments for anxiety and related disorders**. The third section covers research delving more deeply into the nature of anxiety and negative affect with implications for **the study of the etiology, diagnosis, and classification of anxiety and related disorders**. In the final section I turn more towards policy with a description of the trials and tribulations of **promoting and**

disseminating evidence based psychological treatments, a process in which I have been heavily involved for the past 25 years. But before describing the publications chosen to represent developments in each of these four areas, it would seem useful to say something about my own odyssey and the intellectual and experiential influences that provided the context and, at least in part, determined the substance of my contributions over the decades.

Boston: the Early Years

My undergraduate years were spent in an idyllic fashion at the University of Notre Dame where I was deeply immersed in the academic and athletic culture of what was then an all-male University. But due to its strong Catholic heritage a gaping hole in Notre Dame's academic portfolio was the absence of a department of psychology. As with many Catholic universities, those responsible for the curriculum at Notre Dame assumed that the principles of psychology were adequately covered in the myriad philosophy courses available. As it turned out, Notre Dame was one of the last Catholic universities to correct this omission. This was not terribly concerning to me at first since I loved to read and became deeply immersed in literature, including fiction but also biography. And I soon realized that the most fascinating books I encountered involve the psychoanalytic study of literary characters! With my long standing interest in how people behave (one of my less than endearing traits was setting up practical jokes to see how people reacted to improbable situations) and a perception by my friends that I was a good listener, I realized that I was very strongly interested in psychology.

Knowing that I wanted to major in psychology, but also that I did not want to leave Notre Dame, I began taking advantage of psychology courses that were offered, mostly in the sociology department (my official major) and accumulated sufficient credits to apply to graduate

school with one exception – a laboratory course in experimental psychology. To meet this requirement, I enrolled at Boston College for an intensive course in experimental psychology with accompanying laboratory experiences during the summer of 1963 prior to my senior year. That course happened to have been taught by Joseph R. Cautela. During that intensive 6 week experience immersed as we were everyday in the classroom and lab, Cautela made an indelible impression persuading me that only through a reliance on the slow but inexorable process of science could the applications of psychological principals to human problems truly advance. This was a very different take on psychology from my initial interests in the in-depth exploration of personality and also a very different message (I was to learn) from that delivered by other clinical faculty at Boston College, who believed that the scientific method was incapable of unraveling the complexities of the human spirit, an activity that required a different more introspective way of knowing.

I began graduate school in the fall of 1964 at Boston College in the M.A. program in clinical psychology. For me, the program at Boston College not only afforded me a chance to return to my home town of Boston, but also to continue working with Cautela. Cautela was an experimental psychologist as were most of the early adherents to the fledgling movement of behavior therapy or behavior modification, but he maintained a clinical practice mostly in the evenings. This was not uncommon in those days since professional and national guidelines articulating very clear boundaries between clinical psychology and non-clinical psychological training had not yet been developed; indeed, it was common for most experimental psychologists to have some exposure to applied issues during training and to engage in some sort of consulting experience in addition to academic and research duties. Cautela was very interested in what was then called “modern learning theory” comprised mostly of principles of classical and operant

conditioning. The previous summer, pursuing this interest in a more clinical context, he had attended a 2 week summer training institute with the South African psychiatrist Joseph Wolpe in Philadelphia.

Wolpe was already recognized as one of the "fathers" of behavior therapy based on his 1958 book "Psychotherapy by Reciprocal Inhibition." He developed this point of view early in his training after becoming disillusioned with the prevailing psychoanalytic views and, looking for alternatives, became deeply interested in the work of Pavlov. At that point Wolpe decided to do a doctoral thesis as part of his psychiatric residency, an option then available in the British system of medical training in South Africa. In a very strange twist of fate he then came under the influence of an American psychologist, Leo Reyna, who had recently received his PhD from the University of Iowa working with Kenneth Spence and who was spending a few years in South Africa, the only place he could find employment. Reyna went on to direct Wolpe's thesis on reducing fear (in cats) through a counterconditioning process. After leaving South Africa, Reyna ended up at Boston University where he soon began mentoring another doctoral student, Joe Cautela!

After escaping South Africa where he was in danger of arrest to do his strong and activist anti-apartheid views, Wolpe spent some time in England and in the United States before moving to Temple University Medical School and the Eastern Pennsylvania Psychiatric Institute in the early 1960s, where he began conducting his summer institutes. Cautela, having been introduced to Wolpe by his mentor, Leo Reyna, came back from the Institute inspired to begin administering more "learning theory" based approaches with his patients. Cautela had observed Wolpe's technique of "Systematic Desensitization," (SD) in which patients with phobias and anxieties would imagine their phobic situations arranged on a hierarchy based on patient ratings of

intensity of fear while deeply relaxed under the therapist's direction. Patients would slowly move up a hierarchy of these imaginal situations from least fear provoking (e.g. looking at a spider across the room) to most fear provoking (holding the spider) contingent on their fear diminishing and, as the theory went, the anxiety would be "reciprocally inhibited". In Wolpe's view this process occurred at a very basic peripheral neurological level but, it wasn't long before psychologists pointed out that the physiological components of the theory were not tenable scientifically, and that the process almost certainly was best described as counterconditioning. This was an important early example to me that the scientific process could, in fact, direct and guide the development of knowledge in a clinical context.

In any case, following this institute Cautela became very intrigued not only with the application of principles of learning to the clinic but also with the process of utilizing images in therapy. With his background in both operant and classical conditioning (which comprised at the time most of what we knew about learning) he began incorporating behavioral principals *in imagination* utilizing terms such as "covert" reinforcement (imagine engaging in a desired behavior and being rewarded etc.) and "covert" sensitization (for someone with alcohol abuse, imagine engaging in a pleasurable but undesirable behavior such as reaching for a bottle and suddenly beginning to feel very nauseous with the nausea intensifying as the bottle comes closer (e.g. Miller & Barlow, 1973). He would discuss his cases during class as well as during casual meetings outside of class and it would seem, anecdotally of course, that he was achieving some considerable success with his patients. During informal conversations and private meetings Cautela would actually demonstrate some of these procedures to me. It is hard to appreciate how extraordinarily radical this was at the time, since the ubiquitous prevailing approach was psychoanalytic and deviations from this approach were viewed at heretical and even unethical.

Cautela went on to become one of the early presidents of the Association for the Advancement of Behavior Therapy (AABT), now the Association for Behavioral and Cognitive Therapy (ABCT), in 1972.

During the summer of 1966 after finishing my master's degree Cautela arranged for me to spend several months with Joe Wolpe in Philadelphia whose offices were at the Eastern Pennsylvania Psychiatric Institute. Wolpe ran a very active clinical unit and since he had something new and different to offer, many patients were referred who had already failed to benefit from long term psychodynamic psychotherapy or medications available at that time. As a graduate student this was a very exciting time since, under Wolpe's supervision, we would utilize many behavioral procedures including systematic desensitization, assertive training (which was a specialized form of helping people function better in demanding interpersonal situations) as well as deep muscle relaxation techniques, then one of the bedrock strategies in behavior therapy used in SD and other applications. Visitors from around the world came to see what this new "Behavior Therapy" was about, and we would engage in stimulating and interesting discussions of these sometimes difficult cases. Once a week I would accompany Wolpe over to the main Department of Psychiatry at Temple where he would treat a patient using behavioral techniques such as SD in an ongoing seminar format in front of 15 or 20 of assorted psychiatric residents and faculty. Wolpe simply sat in the middle of a room with the patient with everyone seated in a circle around him. As the weeks went by the patient would often improve dramatically which would be surprising and even shocking to the residents and faculty deeply steeped in psychoanalytic thought who would then spend the rest of the session trying to interpret what Wolpe was doing in psychoanalytic terms. At one point after a particularly skillful session a resident approached me saying "isn't it just the case that Dr. Wolpe is wonderful at

forming alliances and that this patient's progress is simply due to the effects of the alliance and resulting transference?" Already a "true believer" I found myself replying "no" that any success could not be due to Wolpe's skill as a clinician because we often interchanged therapists with the same patient during the course of treatment (which was true) and that it really was the new behavior therapy techniques that were important! In later years I would recount this anecdote to Wolpe and how I found myself in the position of renouncing to some extent his therapeutic skills (feeling something like a "Judas") because, in fact, Wolpe was a wonderful, warm, and supportive therapist.

The Vermont Years

After an exhilarating summer studying and working with Wolpe in 1966 and newly married to Beverly Colby I began my doctoral studies at the University of Vermont. At that point in time it was very clear to me that I wanted to pursue a more scientific approach to clinical training and the nascent field of behavior therapy seemed provide the clearest path. The University of Vermont seemed an ideal setting in many ways because of the presence of a young assistant professor who himself had only begun his career several years earlier, Harold Leitenberg, as well as a young associate professor of psychiatry, Stewart Agras, who had recently teamed up with Harold to pursue clinical research. Harold Leitenberg trained at Indiana University with James Dinsmoor, who focused on operant conditioning in the animal laboratories very much in the tradition of B. F. Skinner. But Leitenberg at the time had little or no clinical experience. Stewart Agras, on the other hand, who was trained in London with a residency at McGill University in Montreal, brought a British empirical approach to psychiatry with an emphasis on careful observation and measurement and diagnostic precision that was very

unusual in those days in North America. What I bought to the table was direct training and experience in the practice of behavior therapy such as it was in 1966 after training with Cautela and Wolpe.

Working within an operant conditioning paradigm, and, under Leitenberg's guidance, we began pursuing what at the time was an innovative to approach to clinical research. This approach emphasized repeated measurement and functional analysis in individual participants that came to be called single-case experimental designs. Our particular contribution as a new research team was translating these designs as utilized in the operant animal laboratories to the clinic resulting in a series of early research studies such as reprinted as article number 3 in this volume (Agras, Leitenberg, & Barlow, 1968). These studies, in turn led to a paper describing this methodology published in the Archives of General Psychiatry (Barlow & Hersen, 1973) and ultimately the first edition of a book on the same topic (Hersen & Barlow, 1976).

What made this research possible was the collaboration between Leitenberg and Agras (and myself) since Agras, from his position in the psychiatry department, had direct access to clinical populations and clinical facilities that was very rare those days in psychology departments. Since we were pursuing a systematic program of clinical research we managed to secure several inpatient beds in a dedicated federally funded clinical research unit that was open to the whole medical school but was relatively underutilized by other departments such as medicine and surgery. Thus, for example, the severe house bound patients described in article number 3 (Agras, Leitenberg, & Barlow, 1968) spent several weeks in the hospital participating in very intensive daily treatment along with careful frequent measurement of progress and the beginnings of some experimental analysis of treatment components. To accomplish these goals our research team, by then consisting of several additional young psychiatrists as well as

additional doctoral students in psychology, would meet daily for two hours going over new data as they came in. It was during in depth discussion in these meetings that the translation of basic experimental methodologies to the clinic were further developed. During this period of time research focus was not limited to what would now be called anxiety disorders such as agoraphobia (we had not yet come to recognize panic attacks or panic disorder at that point in time) but rather extended to any type of very severe psychopathology, mostly treatment resistant patients who had not benefited from other treatment approaches and who were looking for something that might be effective. Thus, in these years we published papers on tightly controlled analyses of treatments for conversion disorder, claustrophobia (Agras, Leitenberg, Barlow, & Thomson, 1969), anorexia nervosa (Agras, Barlow, Chapin, Abel, & Leitenberg, 1974), and other presentations in addition to agoraphobia.

It was also the case in that period that diagnosis was an imprecise and very much undervalued activity in psychiatry and clinical psychology. The second edition of the Diagnostic and Statistical Manual (DSM-II) or the 7th edition of the International Classification of Diseases, (ICD-7), the contemporary nosological systems, offered only very loose global definitions of disorders focused on presenting symptoms such as paranoid delusions or agoraphobia. The prevailing catch all category for most non-psychotic disorders, particularly disorders presenting with strong negative affect was “neurosis” which presumed a specific psychoanalytically based etiological process with the resulting symptoms considered only superficial and relatively trivial manifestations of that process. Because of this, patients were typically identified by sometimes unreliable descriptions of their most prominent presenting symptoms or personality features and formal diagnostic categories were widely ignored.

Although our intensive idiographic analyses ranged across much of psychopathology, for my dissertation I returned to my focus on anxiety by examining some of the mechanisms of fear reduction in procedures we were using at that time. Many of us in those years found a convenient analog of phobia in the very common presentation of fear of snakes among college women. Recruiting participants for the studies was very easy, and so many of us did it that it came to be said that the great snake phobic epidemic of the 1960s was all but eliminated by energetic doctoral students attempting to finish their dissertations! In my case I focused on the slippage in fear reduction during the process of desensitization that occurred when the image of the snake was presented in imagination versus in reality despite the fact that participants worked up the same fear hierarchy at approximately the same speed (Barlow, Leitenberg, Agras, & Wincze, 1969). Another study focused on the effects of actively shaping approach behavior to the snake as opposed to passively moving the snake closer to the participant who sat in a chair watching (Barlow, Agras, Leitenberg, & Wincze, 1970).

But it was also during this time that I undertook what has come to be from my own personal point of view the most regrettable initiative in my clinical research career. Specifically, with my expertise acquired from Joe Cautela in the administration of covert sensitization I began treating and evaluating the effects of treatment in individuals with what came to be called paraphilias but what was then called sexual deviation (Barlow, 1974a). While our focus was mostly on pedophilia (e.g. Barlow, Leitenberg, & Agras, 1969), the aggressive behavior of rapists (e.g. Abel, Barlow, Blanchard, & Guild, 1977), and other paraphilias (e.g. Hayes, Brownell, & Barlow, 1978), included in this series of studies were participants presenting with same-sex arousal patterns with consenting adults. (e.g., Barlow, Leitenberg, & Agras, 1969). At that time homosexuality was considered a disorder in all systems of nosology and, under extreme

pressures from society and the associated stigma, these individuals sought out treatment; so very few clinicians even gave it a second thought. But by the mid-1970s several individuals began questioning these treatment goals. One of the first mental health clinicians to do so in a shining example of groundbreaking ethical thinking was the then president of AABT, Jerry Davison, in his presidential address in 1974. Later in the decade the American Psychiatric Association "declassified" homosexuality as a disorder, and these events began what most observers of culture regard as the most rapid shift in cultural attitudes and behavior ever to occur. Looking back on that period from today's vantage point it is very hard to even conceive how we could not have realized the inherent conflicts in attempting to treat harmless consenting adult behavior involving love and affection. But, the lesson learned by most of us is that definitions and classification of psychopathology do not represent qualitatively different entities but rather are embedded in the continually shifting landscape of cultural values and mores and that these ethical and moral issues must be transparent, debated, and occupy a central role in all of our endeavors (Barlow & Durand, 2014).

In 1969, after three years at the University of Vermont and with my Ph.D. in hand Stewart Agras had decided it was the time in his career to seek a position as chair of a psychiatry department. He invited me to join him in the search for the appropriate place with a view towards continuing our collaboration. This search was a much more difficult task than might be imagined. Although Agras had clearly been very productive in clinical research and, with his CV, would be highly valued as a chair at any psychiatry department in the country under present circumstances, in psychiatry departments and clinical psychology training programs in those days, empirical research was at best a secondary activity and at worst discouraged. Furthermore, actually influencing a mature established department in any meaningful way deeply immersed,

as they were, in intensive (psychoanalytic) training and clinical work would have been very difficult if not impossible. But Stewart, in the process of his search, visited a small department in Jackson, Mississippi that had few established programs and therefor was in a position to hire a relatively large number of people in a short period of time. Surprised though I was as a New Englander at the location, a visit persuaded me of the excellent opportunity that existed. Thus, in the fall of 1969 we both settled a very long way from New England in the Department of Psychiatry and Human Behavior at the University of Mississippi Medical Center.

The Mississippi Years

From lowly graduate student in the previous months I was suddenly Chief of Psychology in the Department of Psychiatry and Human Behavior and Director of the Psychology Internship Program (now called the Psychology Residency Program)[1]. There were very few psychologists on site and psychology interns had yet to be admitted. In addition, when the Chief of Psychology at the adjacent Veterans Administration hospital, closely affiliated with the medical center, moved on several months after I arrived, I found myself acting Chief of Psychology at the VA as well. With this surfeit of titles but little or no substance to the programs under my direction my first task was to continue to organize our clinical research efforts along with Stewart Agras who, although devoting a fair amount of time to administration, retained his active role in research. My second was to initiate a clinical psychology internship program, and my third was to begin to fill many of the open positions with likeminded psychologists with a strong research and scientific approach to clinical work, something of a rarity in those days. Fortunately in those very

[1] In fact, a small internship program had existed briefly in the 1960's but had been dormant for years.

early years of behavior therapy and behavior modification several outstanding opportunities presented themselves. First, Ed Blanchard, then at the University of Georgia after having completed his Ph.D. at Stanford with Al Bandura, showed some interest in joining us to help build new programs. We were fortunate to successfully recruit him with his strong expertise in psychophysiological methods and in the emerging field of biofeedback. Second, after consultation with the central office of the VA we identified Michel Hersen who at the time was looking to relocate from a state hospital in Connecticut. With Michel's experience and a recommendation from VA headquarters, he was quickly appointed as Chief of Psychology at the VA. Michel also became Associate Director of the Psychology Internship Program since the plan was to integrate the internship with stipends provided by both the VA and the Medical Center. Other strong psychologists recruited over the next several years into the program included Peter Miller with expertise in addictions and Dick Eisler who had been a colleague of Michel Hersen's during graduate school days at SUNY Buffalo. And in the fall of 1970 we welcomed the first group of three interns.

In addition to opportunities for research one of the very attractive features of the department under the extraordinarily forward looking leadership of Stewart Agras was the notion that psychology was fully an equal partner with psychiatry in both clinical and research endeavors. Thus, psychologists were afforded principal responsibility for patients in both inpatient and outpatient units and were on call to the emergency room for psychiatric consultation nights and weekends. As the training program began this opportunity was also afforded to psychology interns who would alternate this duty with psychiatry residents (Barlow, 1974b). It was also noteworthy that the clinical psychology internship was the first in the country

to orientate itself as an empirically based training program. An ad that appeared in the journal *Behavior Therapy* stated the philosophy very clearly:

> The psychiatry department of the University of Mississippi Medical Center offers an approved pre-doctoral internship in clinical psychology. The focus is on a behavioral analysis- behavior modification approach to the variety of clinical problems found in community settings, outpatient and inpatient settings, experimental preschools, and a general hospital emergency room. Diagnostic testing requirements are minimal. Applied clinical research using single case experimental designs is encouraged and taught in most clinical settings. (Vol. 4, p. 627, 1973)

One of the key sentences in this advertisement was “Diagnostic testing requirements are minimal.” Thus, the program moved very far away from the prevailing mode of training focusing on detailed projective and related personality and cognitive testing of patients with results in the form of a psychological report forwarded on to the psychiatrist who would then incorporate the report into a treatment plan. In this way, clinical psychology programs within psychiatry departments operated very much as a laboratory similar to clinical laboratories analyzing blood samples. Of course objective psychological testing was conducted if indicated, but the elimination of projective testing as well as the routine “psychological battery” administered to every patient was a relatively radical move at the time that prompted the predictable backlash in the community and to some extent nationally.

Early in this phase of program development I received a notice from the VA that, based on a report from a committee of regional directors of training they wished to come and investigate developments at the VA hospital in Jackson. Of course, it turned out that politics was behind this notification since one of the members of the committee was friends with the previous

Chief of Psychology who had, in fact, been “pushed out” by the Chief of Staff of the VA hospital who was not impressed with the way he conducted his small department. The Chief of Staff cleverly used our arrival to ask if we were going to retain this individual and, after a few brief conversations, we reported back that he probably would not fit in with our developing plans. The Chief of Staff was obviously delighted and moved him out at the first possible moment, but the blame fell on us and our new “behavior modification” approach and the customary implication in those days that what we were doing was very radically different and probably unethical. A visit from Central Office of the VA quickly dispelled any misconceptions and the recommendation was to hire Hersen as the new Chief of Psychology.

Fortunately there was enough support in the psychological community that, with strong backing of the chair, Stewart Agras, the training program not only survived this and other early attacks but prevailed and flourished. With strong and effective leadership over the ensuing years it is gratifying to note that the core concepts that we established back in 1969 have been retained in the form of a broad and deep scientist practitioner model of training, and the Psychology Residency Program at Mississippi continues to be one of the strongest research oriented internships in the country.

But our principal focus remained on clinical research. As Stewart Agras has observed it is noteworthy that the productivity of the whole department of psychiatry in Mississippi prior to 1969 averaged one published paper per year whereas four years later close to 100 papers per year were published (Agras, 2012). As in internship training the focus was on behavioral analytic single case approaches to clinical problems although not to the exclusion of other more nomothetic approaches. While some of this idiographic research focused on anxiety disorders including phobias and obsessive-compulsive disorders, the emphasis continued to be on

idiographic approaches rather than any one content area. For example, selected publications from this era included work on severe speech disorders (Pineda, Barlow, & Turner, 1971); spasmodic torticollis, (Bernhardt, Hersen, & Barlow, 1972); and, and the measurement and modification of incestuous behavior, (Harbert, Barlow, Hersen, & Austin, 1974). Research meetings continued every single day and in 1971 I was fortunate to obtain my first NIMH grant to study psychological aspects of sexual dysfunctions and paraphilia.

In 1974 I was promoted to full professor of psychiatry, but some of the very radical innovations in the Department came under attack. A political fight over the control of a local community mental health center waged between the Medical Center and a community hospital provided an opportunity for some in the mental health community to rail against the evils of "behavior modification" as propagated by Stewart Agras and the faculty. During this period of somewhat nasty politics Stewart was offered a much more felicitous professorship at Stanford University relocating in 1974 where he remains to this day (early 2015) as productive as ever at the age of 85! Approximately six months later and missing New England I accepted a position as Professor of Psychiatry and Psychology at Brown University and Director of Education and Training and Psychology at Butler Hospital in Providence Rhode Island where Beverly and I, now with two children, relocated in January of 1975.

The Brown Years

The new chairman of the department of psychiatry at Brown, Ben Feather was trained as a psychoanalyst, as were almost all psychiatrists in those days, but also had received some training in behavior therapy and biofeedback and was interested in the variety of different clinical approaches and how they could be integrated for training purposes. As the founding chair of the

new Department of Psychiatry at Brown.[2] Ben was creative and bright but faced a number of administrative hurdles as he attempted to build the fledgling department. First, like many New England medical schools Brown University did not "own" its own teaching hospital. Rather it affiliated with a number of existing community hospitals. The freestanding private psychiatric hospital in Rhode Island, Butler Hospital, became the principal seat of the Department of Psychiatry. On the basis of an administrative arrangement worked out between the university and the various hospitals, the Medical Director of Butler Hospital would also become the Chair of Psychiatry at Brown. But other hospitals also provided some mental health services and they had to be integrated into the larger department, no small undertaking. The Bradley Hospital was the child psychiatric hospital with its own Medical Director. Some of the other general hospitals such as the Rhode Island Hospital also had psychiatric components and potential slots for hiring faculty. But, the Chair of Psychiatry did not actually control the budgets or salary lines in these other hospitals, with the exception of Butler Hospital, requiring that any negotiations to build up the department of psychiatry depended on the personal persuasiveness of the chair and an overarching sense on the part of the hospitals that being affiliated with Brown University was a good thing. Nevertheless, conflicts soon arose over expending hospital monies on training activities since this did not generate revenues for the hospitals to pay the bills. On the contrary, it cost the hospitals money and any reimbursement from Brown University did not begin to cover the time lost.

While Ben Feather was very interested in beginning to build a clinical research program with external support, he was more immediately interested in setting up exemplary training programs. He saw the internship program in clinical psychology at the Medical Center in

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[2] Although a medical school existed at Brown in the 19th century it had been on "hiatus" for a number of decades until resurrected in the late 1960's.

Mississippi, which he had visited the previous year, as an exemplar of what could be set up at Brown, and creating this internship became my first task. Having arrived in January of 1975, and with internship acceptances due to be mailed out in early February there was little time to be lost and all recruiting had to be done by phone. In those days with few formal procedures in place this was somewhat easier to do (it would not be possible today) and I was able to recruit five students mostly through contacts with directors of clinical training at universities with whom I had built a relationship while in Mississippi. Nevertheless these five students were taking a large risk accepting admission to a program that did not yet exist. The five students who comprised the first internship class at Brown in the fall of 1975 included Kelly Brownell, formerly Professor of Psychology and Epidemiology and Public Health at Yale and currently Dean of the Sanford School of Public Policy at Duke University, Steve Hayes, Foundation Professor at the University of Nevada, Reno and well known for his creative accomplishments, Toy Caldwell-Colbert, formerly Provost and Chief Academic Officer at Howard University before her untimely passing, Peter Monti, who went on to become Professor of Psychiatry and Human Behavior and Medical Sciences and the Donald G. Miller Distinguished Professor of Alcohol and Addiction Studies at Brown and ran the internship program himself for a number of years, and Carol Landau, who became Clinical Professor of Psychiatry and Human Behavior and Clinical Professor of Medicine at Brown. I was also fortunate to be able to recruit my colleague from graduate school days at Boston College and the University of Vermont, John Wincze, then at Dalhousie University, to become chief of the Providence VA Hospital and Associate Director of the Psychology Internship. John remained at Brown for the rest of his career. His son Jeff would later become one of my doctoral students.

With a heavy focus on training, a number of publications from that period of time focused on the integration of science and practice still something of a new endeavor from a more practical point of view (e.g. Barlow, 1981; Barlow, Hayes, & Nelson, 1984). Research on sexuality (e.g. Abel, Barlow, Blanchard, & Guild, 1977) and anxiety (e.g. Barlow, Mavissakalian, & Schofield, 1980) continued at Brown as well, but research productivity diminished somewhat due to heavy administrative responsibilities. The Chair of Psychiatry, Ben Feather, unexpectantly resigned in the summer of 1975. Thus, for a period of four years the several of us who were department heads at the hospital were responsible for both the administration of the hospital while another group of us ran the Department of Psychiatry. But it was the hospital, with its large budget and the myriad of issues attendant with its mission that consumed most of my administrative time and effort. This left less time for research than was desirable from my point of view and in 1979, with the internship program firmly established, but the politics in the department continuing to be unsettled, it seemed time to relocate to a setting that allowed a more complete focus on clinical research. This decision was solidified by the arrival of a new Chair of Psychiatry from Canada of whom those of us on the search committee knew less then we thought we did. He proved to be a difficult, autocratic chair who, in addition, was not supportive of psychology. Within the year all of the full professors and chiefs of psychiatry recruited by Ben Feather at the various hospitals had left, and it was time for me to move on also. Thus, in the fall of 1979 I accepted a position as Professor of Psychology at the State University of New York (SUNY) at Albany joining my old friend from Mississippi days, Ed Blanchard who had been recruited to rejuvenate the clinical psychology program at that university two years previously.

The Albany Years

Shortly after arriving in Albany, and in a department of psychology rather than a department of psychiatry and associated hospital for the first time, it seemed necessary to create a venue to carry on clinically meaningful research since there was no ready supply of patients. While some psychology departments in those years had established training clinics usually called "psychological service centers" as had Albany, the setup of these training clinics along with their mission made the conduct of clinical research difficult if not impossible beyond simply collecting a few questionnaires etc. Indeed much of the research on going in psychology departments in the 1970's could be characterized as "analog" research focusing on personality traits or fears mostly in college students. Research from my own dissertation described earlier focusing as it did on modifying relatively normal fears of snakes in college sophomores remained the prototype for clinical research approaches in psychology departments despite the fact that findings from these research paradigms produced few results that were generalizable to more severe psychopathology.

Thus, Ed Blanchard and I together initiated the Center for Stress and Anxiety Disorders which grew to become a large federally funded research clinic. This Center was, in turn, organized around the Phobia and Anxiety Disorders Clinic housing my research and the Stress Disorders Clinic for Ed's research on biofeedback for such conditions as hypertension and migraine headaches. We shared administrative space and a reception area, and over the years flexibly allocated space based on grant portfolios. Here my knowledge of organizing and supervising large outpatient clinics in departments of psychiatry and hospitals came in handy and we purposely set up the Phobia and Anxiety Clinic to serve the general public rather than college students. We also marketed the clinic as offering specialized effective brief psychological

treatments for people with anxiety and related disorders, and patients paid for these services. This communicated the fact that the clinic was squarely in the realm of mental health care delivery with a focus on accurate diagnosis and effective treatment (in other words getting people better) rather than a psychology lab where patients paid nothing in return for being a "guinea pig" in a research project. We also saw that the waiting area and offices were comfortable and well appointed and that staff and receptionists dressed and acted professionally. The functioning of this clinic, as it evolved, is described in Barlow (1992, article #8). Treatment was provided by myself and other young faculty members collaborating on setting up clinical research programs such as Rick Heimberg who began a program for social phobia and Gerry O'Brien working in the area of agoraphobia and the newly created disorder (in DSM-III) of panic disorder. Services were also delivered by doctoral students working on the team who were supported by one or more NIH grants focused on developing psychological treatments for anxiety disorders. Despite marketing ourselves as a fully functioning clinic serving the public in order to attract sufficient numbers of patients, we did not lose sight of our principal mission as a center for clinical research. With this structure in place, and taking advantage of every media invitation to make the public aware of our programs, the clinic developed a reputation of providing effective clinical services in a professional manner and positive word of mouth assured steady growth in the number of referrals. This growth necessitated several moves in those first few years to accommodate expansion and by 1983 we finally settled in our own building leased by the university on a busy road just off campus. That year we recruited Wendy Silverman to the faculty and she organized a Child and Adolescent program. When Wendy left for Florida in 1990 a new post-doc, Anne Marie Albano took it over. In 1987 Bonnie Brown, who had been with us

in more junior positions became administrator of the anxiety clinic, and when she finished her nursing degree became Nurse-Administrator.

Our research focus on anxiety in this center was effectively launched with the awarding of an NIMH contract for a conference on behavioral approaches to anxiety disorders that was held in Albany on April 28th-30th, 1980. This conference, attended by 20 of the leading clinical researchers in the world at that time noted that psychologically based treatments, particularly exposure therapies for phobia, were proving to be an important advance in effective treatment which had been dominated up until that time by pharmacological treatments. But there were sizeable disagreements among clinical investigators on the appropriate directions for research in this area. A report of the consensus conclusions of this conference published in 1981 (Barlow & Wolfe, 1981, article # 4) recommended general research strategies, new approaches to assessment and classification, ideas for fruitful process and outcome research, as well as the beginnings of dissemination strategies. Among the interesting recommendations from the conference participants at that time was that there was no further need for outcome studies utilizing exposure therapies for phobias since these procedures were well established. However, important outcome research should be initiated in the newly conceptualized areas of generalized anxiety disorder, social phobia, and panic disorder. Furthermore, all funded research should be carried out with clinical populations "whether the goal was to uncover basic mechanisms of action of psychological treatment or to pursue future outcome goals" (p. 449). Thus the committee suggested that analogue research had probably run its course and that priority should be given to studies in the context of the more commonly encountered clinical disorders. Recommendations were also made to study the generalization and maintenance of changes as a function of psychological treatments, to study epidemiological and natural histories of the

anxiety disorders and, not surprisingly, that a greater use of single case experimental designs teasing out mechanisms should prove fruitful. It is interesting to contrast knowledge of the anxiety disorders as it existed in 1980 represented by the conference recommendations with what we have learned in the past 35 years.

In 1981 Peter DiNardo who was on the faculty of nearby SUNY Oneonta joined us for a year-long sabbatical and together we collaborated on writing a much needed semi-structured diagnostic interview focusing exclusively on anxiety and related emotional disorders. Modeled after the Schedule for Affective Disorders and Schizophrenia (SADS) we named this instrument the Anxiety Disorders Interview Schedule (ADIS) which proved to be an extremely useful instrument and became the focus of our new project on classifying anxiety disorders.

With a venue in place and the focus squarely on anxiety and related disorders the major themes of my research became more fully elaborated. Early research on the nosology of anxiety and mood disorders resulted in explorations of the reliability of these new DSM-III categories as well as new conceptualizations of generalized anxiety disorder and panic disorder (e.g. Barlow, 1985; Barlow, 1987; Barlow, Blanchard, Vermilyea, Vermilyea, & Di Nardo, 1986). These developments were communicated through my membership on the anxiety disorders work group for DSM-III-R, and later on the Task Force for DSM—IV. Our long running NIMH grant on the classification of anxiety disorders first funded in 1984 and taken over by Tim Brown as Principal Investigator in 2000, continues to this day.

At the same time experimental psychopathology research focused on the nature of anxiety as evidenced in men, and later in women, presenting with sexual dysfunction. First funded by NIMH in 1979, the major paradigms used in this grant afforded an easily quantifiable output of the influence of cognitive and affective components of anxiety in the form of

psychophysiological measures of sexual arousal which could be manipulated, sometimes without patients' awareness. This program of research resulted in a model of the process of anxiety manifested in sexual dysfunction (Barlow, 1986a, article # 12).

In 1985 Michelle Craske arrived to spend a post-doctoral year after finishing up her PhD at the University of British Columbia with Jack Rachman. During this period my colleagues and I, particularly Michelle and Ron Rapee, also spending a postdoctoral year after getting his degree at the University New South Wales in Sydney developed new treatments for anxiety and related disorders, most notably a new psychological approach to treating panic disorder that was positively evaluated and widely accepted (Barlow, 1986b, article #5; Barlow, Gorman, Shear, & Woods, 2000; Klosko, Barlow, Tassinari, & Cerny, 1990; Barlow & Cerny, 1988). We also extended treatment development efforts to generalized anxiety (e.g. Rapee & Barlow, 1991). Fortunately for me Michelle and Ron ended up staying approximately five years contributing substantially to a very productive period of clinical research.

Programmatic research on the nature, classification, and treatment of anxiety and its disorders during the 1980's resulted in a book integrating basic and applied research on anxiety from a variety of different perspectives (Barlow, 1988). The several years it took to write this book proved to be a crucial step in my research as it deepened and broadened my knowledge of anxiety and I became acquainted with the literature, such as it was, in emotion science. This, in turn, led to the first statement of a transdiagnostic approach to treatment in the emotional disorders based on effective principles of change as described below.

By 1990, evidence based psychological treatments had been evaluated positively from emerging clinical trials such that sufficient evidence for efficacy was available for at least some disorders, and around this time we turned our attention to dissemination and implementation.

The first step was made possible by the necessity in well conducted clinical trials of writing treatment manuals describing the administration of treatments undergoing evaluation in some detail. This allowed multiple therapists to administer these treatments in a reliable manner thus ensuring the existence of an independent variable. It became apparent to us that these manuals, that had been restricted to internal use among research teams up until that point, would be valuable to clinicians more generally wishing to incorporate these treatments into their practice. Thus, in the late 1980's Michelle Craske and I wrote up our treatment for panic disorder and self-published it through my own publishing company set up for this purpose, Graywind Publications, INC. (Barlow & Craske, 1988). When it quickly became apparent that there was considerable demand for this product we published additional materials such as a therapist guide (Craske & Barlow, 1990) as well as programs for treating generalized anxiety disorder (Craske, Barlow, & O'leary, 1992, Zinbarg, Craske, & Barlow, 1993) and began publishing manuals used in other clinical trials by other investigators for a variety of different disorders as well. This was a new concept at that time that did not fit comfortably with traditional publishing companies since we were making available an integrated treatment program to address specific disorders, and it seemed a new and different marketing and dissemination strategy was needed.

While this dissemination strategy was effective to a point, it became clear that broader efforts were needed to make these treatments better known and available in order to provide some semblance of choice to people suffering from these disorders most of whom were restricted to available medications from their providers. In 1993 when I was elected President of the Division of Clinical Psychology (now the Society of Clinical Psychology) of the American Psychological Association, I organized a task force (one of the prerogatives of the president each year) and titled it "Promotion and Dissemination of Psychological Interventions". I was fortunate

to have my first choice for chair of this task force, Diane Chambliss, accept this responsibility and, working together we recruited diverse members from research and practice settings. In order to implement promotion and dissemination initiatives it became necessary to first survey the state of the evidence for psychological treatments ascertaining which interventions could be judged to be empirically supported or evidence-based, but no a priori criteria existed for making these determinations. The Task Force settled on a criteria consisting of a minimum number of studies showing efficacy which was admittedly arbitrary but formed a starting point for what proved to be a very important debate on this topic. This was followed by the listing of psychological interventions that met these new criteria, and the active process of communicating the empirical support of psychological treatments to psychologists, other mental health professionals, and the public at large began. These ideas merged with the growing mandate emanating from medicine for evidence based practice and it became clear that this was an idea who's time had come (Barlow, 1996, 2004).

Boston: The Later Years

In 1996 after 17 productive and fulfilling years in Albany I was presented with an exciting opportunity to rebuild a clinical psychology program in my hometown, and Beverly and I, after an absence of 30 years, returned to Boston where I became Professor of Psychology and Psychiatry, Director of Clinical Training, and Director of the Center for Anxiety and Related Disorders at Boston University (CARD). CARD was actually the Phobia and Anxiety Disorders Clinic of the Center for Stress and Anxiety Disorders at Albany which, by that time, had grown to approximately 30 people including staff and students, almost all of whom made the trek down the Mass Pike from Albany to Boston. Notable among them were Tim Brown and Stefan

Hofmann, who had taken over the Social Phobia Program from Rick Heimberg, David Spiegel, the clinic psychiatrist, and, of course, Bonnie Brown, our Nurse-Administrator. Construction of our new quarters in Kenmore Square, very close to Fenway Park proceeded quickly and we were open for business in the fall of 1996. The design of the new clinic was greatly assisted by David Spiegel who, prior to attending medical school, had completed a degree in engineering, skills he put to good use in designing and adapting space to suit our needs. Shortly after arriving Donna Pincus joined us at CARD and took over the Child and Adolescent program. In the years to follow Pincus, Brown, and Hofmann would all cross over from soft research funded positions to tenured professorships at B.U.

CARD was principally a clinical research operation supported by five different NIMH grants in 1996. The complexity of the transfer prompted the Provost to equate efforts to recruit us with the recruitment of a large physics lab. But once again, as had happened at Brown University, one of the principal objectives of my recruitment by the administration at Boston University along with research initiatives was to revitalize the clinical psychology training program. This program, one of the original 12 programs approved by APA in 1948, retained many existing elements from those early years including a very heavy focus on practice and theory. As with most of the early programs, research was not emphasized and training in research methods was introduced only late in the training sequence. There were no existing externally funded research grants in the clinical program and the university made it clear that unless the program became more scholarly and research based they would consider closing it down. Initially, we focused largely on revamping the curriculum, hiring or promoting more research oriented clinical faculty, and making research training an integral part of the clinical program experience beginning in the first year. Very ably assisted by a new graduate of the

clinical program, Lynn Bufka who was intimately familiar with the inner workings of the training program, the department, and the university, and with the cooperation of the faculty who were, for the most part, on board with the necessity of change if not necessarily the specifics of what needed to change, the transition went relatively smoothly. During this transition Lynn performed superbly as Associate Director of Clinical Training with her strong interpersonal skills and the ability to relate to both faculty with whom she was popular and respected, and students who were, after all, just a few years younger than she. Lynn was able to smooth out many of the inevitable bumps in the road during this rebuilding process. This allowed a more complete focus on clinical research at CARD then I had anticipated. By 2004 after eight years of running the clinical program the transitions were complete and I stepped down.

As I write this chapter it has now been over 18 years since I arrived in Boston and my program of research and writing has continued to focus on the nature and treatment of anxiety and related disorders of emotion, as well as policy issues involved in dissemination and implementation. In 2004, recognizing the plethora of treatment manuals that had been developed for anxiety and related emotional disorders, each targeting a very narrow slice of psychopathology represented by DSM-IV categories, we returned to the approach first articulated in 1988, identifying a common set of principles of change applicable to all disorders. We referred to this approach as a Unified Transdiagnostic Treatment for Emotional Disorders in the first article on this topic (Barlow, Allen, & Choate, 2004, article # 10).

Moving more deeply into this area I began to realize, based on our ongoing research on the classification and nature of emotional disorders with my colleague Tim Brown, that fundamental temperamental aspects underlying anxiety, mood, and related disorders seemed more central to the nature of these disorders than did the symptom presentations that were their

defining feature in DSM-IV and DSM-5. In 2009 Tim Brown and I proposed the conceptual outlines of a new hybrid dimensional-categorical classification system based on these shared features and began to spell out implications for assessment and treatment (Brown & Barlow, 2009, article # 16). This led in turn to a greater focus on the underlying temperament of neuroticism along with related temperaments such as positive affect, and a modification of existing models of etiology to encompass the nature and development of neuroticism itself with implications for diagnosis and treatment (Barlow, Sauer-Zavala, Carl, Bullis, & Ellard, 2014, article # 17).

At the same time we continued to focus on dissemination and implementation with policy based articles outlining the status of some of these fledgling efforts (McHugh & Barlow, 2010 article #19) and a more general update on the status of evidence based psychological treatments with some predictions of necessary future steps (Barlow, Bullis, Comer, & Ametaj, 2013, article #21).

By 2009 CARD had morphed into something much more than my laboratory which it had been during the Albany years and the early years in Boston, to a university wide resource that was used for training not only for doctoral level psychology students at BU, but also psychiatric residents, social work students, as well as students from other clinical psychology programs in and around the city of Boston. Research projects and programs have multiplied and CARD is currently staffed with over 70 individuals including a number of principal investigators pursuing different aspects of adult and child emotional disorders. Over 500 patients are admitted to the clinic (after a telephone screen for appropriateness) in a typical year with about half of them triaged into ongoing clinical research projects. The other half, including those who refuse participation in research for one reason or another are assigned to staff and students for

treatment. In 2009 I gave up all remaining administrative roles in the department and clinic and a full time director was appointed to administer CARD. In view of the complexities of running this large research and teaching unit the decision was made to appoint someone on a non-tenure-track line who was not also pursuing research. Dr. Lisa Smith, the former clinical director of CARD was appointed and she in turn reports to a steering committee of principal investigators leaving me time to focus more fully on the above mentioned strands of research.

With this description of the external influences and contexts that led to the various strands of my program of research now complete, it is time to return once again to the major themes outlined at the beginning of this chapter and to consider briefly the articles I have chosen to illustrate the development of these themes beginning with methodology and clinical research.

Methodology and Clinical Research

The first paper in this section (Barlow, 1980) represents my presidential address to AABT (now (ABCT). The address was actually delivered in November of 1979 at a time when behavioral and cognitive interventions were beginning to be seen in a more favorable light. This followed a decade ranging through the mid to late 1970's when behavior modification or behavior therapy received very bad press often described as nothing more than unethical brain washing as famously represented in the popular book and movie of the time *Clockwork Orange*. This poor image was best represented by a survey of articles in the *New York Times* during the mid 1970's demonstrating that approximately 50 percent of the articles addressing the subject equated behavior modification with such procedures as brainwashing, psychosurgery, sensory deprivation, and Chinese water torture! Nevertheless, by 1980 both the NIMH and the American Psychiatric Association along with *Science Magazine* and the President's Commission on Mental

Health strongly endorsed further research into behavioral interventions noting preliminary evidence for their efficacy. But what I noted in that address was that our science was becoming shallow. Basically this was due to an overemphasis on broad nomothetic comparisons of a treatment to no treatment with results analyzed in terms of statistical rather than clinical significance "…instead of asking why does a treatment work and what are the ingredients of a given technique that are truly effective, the question is often how much of a chance is there that this package treatment as it currently exists might work with some broadly defined problem. And any probability at all is usually good enough" (pp. 319) In this report, and following the tenets of applied behavioral analysis I advocated for "…a fine grained analysis of an individual's behavior or a series of individuals' behavior, with attention to technique building, repeated measurement, and social rather than statistical significance; an approach to science that in its very nature attends to our failures" (p. 322) In this article I go on to hazard some predictions about how this focus on a more idiographic analysis might come about.

It is interesting to fast-forward 30 years to the second paper in this section (Barlow & Nock, 2009). The same plea is made, but it is noted that more idiographic single case experimental analyses had not been as widely adopted as I predicted they might be in 1979. The principal reason for this (in retrospect) seemed to be the beginning of the funding of large randomized clinical trials (RCTs) by the National Institute of Mental Health (NIMH). These trials very clearly became the gold standard for establishing the efficacy of treatments and the outcomes deeply influenced healthcare policies and practices. Without questioning the enormous contribution of this methodology, it is interesting to note that the NIMH in its recent policy pronouncements has proposed abandoning large clinical trials that simply compare treatment A to treatment B. Rather, any new trials must undertake a deeper analysis of active mechanisms in

these treatments, along with a focus on discerning appropriate targets of any intervention integrating biological and psychological factors that might reflect important causal mechanisms in psychopathology. The NIMH, with its recent launch of the Science of Behavior Change (SOBC) initiative, seems to be pursing the very same objectives noted as desirable in these articles. In Barlow and Nock (2009) we hazard a suggestion as to how this technology can be better integrated into current clinical science initiatives.

Treatment of Anxiety and Related Disorders

Articles in this section span most of my career and trace an interesting story in the development of our conceptions of treatment predating even exposure as we know it today. The first article in this section, published in 1968 in the *Archives of General Psychiatry*, but reflecting treatments that occurred a year or two earlier seems naïve in its conception as one might expect from an effort over 40 years old. As a doctoral student at the time I was the therapist for these patients which meant I saw them on a daily basis for a number of weeks during these intensive interventions. These patients suffered from very severe agoraphobia such that they could not leave their homes and had to be transported to the clinical research unit in the hospital via ambulance while heavily sedated. Treatment consisted of setting up a walking course of about a mile in length that proceeded into an ever more crowded area in Burlington, Vermont. Patients would venture out as far as they could away from the hospital and would receive enthusiastic praise (from me) for successfully meeting behavioral targets for the day. The choice of selective positive reinforcement was based on strong results from the operant laboratories as a well established procedure for effecting behavior change. At that time of exposure procedures themselves were not considered central to any therapeutic approach including the new behavioral

approaches. Wolpe had been using SD, particularly for specific phobia, in which patients would be gradually exposed in imagination to feared situations along a hierarchy while deeply relaxed based on the idea that relaxation would inhibit anxiety as long as the anxiety was not too intense. So the principal goal was to inhibit or decrease the anxiety. This also reflected a theme present in both psychoanalytic and behavioral theorizing of the day that experiencing intense anxiety could produce a very dangerous outcome up to and including a psychotic break. In psychoanalytic theory the purpose of defense mechanisms was to prevent intense anxiety from happening. In behavioral theorizing based on Pavlov if anxiety was too intense the organism could enter a state of paralysis described by Pavlov “transmarginal inhibition” (Barlow, 1988). So all anxiety reduction procedures of the day were carried out very gradually indeed. That our interventions were successful even with very severe patients was something of a surprise to us, but did engender a number of questions on active mechanisms of change. It was not long before we discovered that the exposure procedures themselves, particularly in vivo exposure as was happening in this study was the principal and most powerful mechanism of change, and that reinforcement simply motivated this new behavior (Barlow, 1988).

Other articles in this section detail the accumulation of knowledge over the decades as we developed ever more successful treatments. The aforementioned NIH conference that occurred at SUNY, Albany in 1980 summarized the state of the art at that time (article #4). By the mid 1980’s we had conceptualized a new approach to treating panic attacks based on the notion that these attacks represented the conditioning of anxiety to internal somatic (interoceptive) cues and that the required treatment would involve the “twist” of directly exposing patients suffering from panic attacks to these interoceptive cues in a procedure that came to be called interoceptive exposure (Barlow, 1988). This early conceptualization is presented in Barlow (1986b, article #5).

In 1988, and as noted above, while attempting to integrate everything we knew about anxiety and its disorders at the time, I wrote a chapter “The process of fear and anxiety reduction: Affective therapy” (Barlow, 1988, p. 285-318, article #6). I called it “affective therapy” since I drew on principals of emotion science as the basis for new and continuing developments in treating disorders of emotion. Particularly important strategies gleaned from this analysis included changing action tendencies associated with pathological emotions, along with interoceptive exposure and altering attributions and appraisals about the emotional experience. This was actually the first statement of what would develop 15 years later into an integrative transdiagnostic treatment for emotional disorders.

In the meantime clinical trials and had already become the gold standard for evaluating treatment efficacy and our clinic benefited from a number of NIMH grants supporting trials for one disorder or another. One of my students, Janet Klosko with support from what was then the Upjohn Pharmaceutical Company conducted the first direct comparison of our new treatments for panic disorder incorporating interoceptive exposure with the most popular drug of the day Alprazolam (Xanax) a high potency benzodiazepine (Klosko, Barlow, Tassinari, & Cerny, 1990, article #7). The somewhat surprising result was that what we then called “panic control treatment” actually outperformed the drug, an outcome quite surprising to many. “Surprising” because there was little evidence at that time for the efficacy of psychological interventions for what was thought by many, particularly biological psychiatrists, to be a biologically based disorder. But this finding led directly to a large multisite clinical trial finally published in 2000 in *JAMA* comparing panic control treatment with the tricyclic antidepressant imipramine as well as their combination (Barlow, Gorman, Shear, & Woods, 2000, article #9) This study broke new ground at NIMH since they had not yet had an application for a study from multiple principal

investigators acting independently at four different sites (as opposed to one site subcontracting to another). This forced the NIMH to create a new grant mechanism to accommodate this collaboration which included two sites with a pharmacological approach to anxiety and panic (Gorman and Woods) and two sites better known for a psychological approach (Barlow and Shear). This innovation, intended to handle the obvious confound of allegiance effects at any one site, was widely praised and, given the success in obtaining funding, widely imitated in years to come.

The activities ongoing in our anxiety disorders clinic in the 1990's referred to above in the discussion of "the Albany years" are detailed in a brief article published in 1992 (Barlow, 1992, article #8). Finally, as a logical extension of thinking detailed in my 1988 book on "Affective Therapy" we published in 2004 the first statement describing a Unified Transdiagnostic Treatment for Emotional Disorders consisting of five core therapeutic procedures thought to be widely applicable to all anxiety, mood, and related disorders (Barlow, Allen, & Choate, 2004, article #10). In many ways this was a culmination of work begun back in the 60's that reflected our deepening understanding of the nature of anxiety and its disorders and the development of effective psychological interventions targeting these underlying mechanisms. It certainly will not be the last statement since clinical science will continue to advance, but it does form the current focus of our efforts in the twilight of my career.

The Nature, Diagnosis and Etiology of Anxiety and Related Disorders

Upon arriving at SUNY Albany in the fall of 1979 two NIMH grants were simultaneously awarded; the first focused on the treatment of agoraphobia and panic disorder with a particular emphasis on including a significant other, usually the spouse, in the treatment

process (see Barlow, O'Brien, & Last, 1984). The second was focused on elucidating the nature of anxiety and arousal in sexual dysfunction (as mentioned above and described below). In 1982 a third NIMH sponsored clinical trial began focused on evaluating new treatments for generalized anxiety disorder. As the principal investigator (PI) on three NIMH awards we discussed with NIMH staff the desirability of applying for a Center grant that would support the infrastructure of the anxiety clinic making each of the treatment outcome grants less expensive. Due to fluctuations in policies regarding Center grants at that time this particular application was not encouraged, but working with NIMH we decided that an additional grant application focused on detailed assessment, diagnosis and classification of all patients coming into the clinic would serve the same purpose. That is, all patients could then be worked up in considerable detail and triaged to the appropriate clinical trial focused on treatment outcomes thereby saving the clinical trials the expense of this initial screening. The grant, titled "Classification of Anxiety Disorders" was funded in April of 1984. In September of 2000 after relocating to Boston the PI status was transferred to Tim Brown and this grant is now in its 31st year of continual funding.

The first article in this section "The phenomenon of panic" (Barlow, Vermilyea, Blanchard, Vermilyea, DiNardo, & Cerny, 1985, article #11) emanating from early work on this project provides detailed descriptive data on patients coming into our clinic. In this article it was demonstrated for the first time that panic attacks were a ubiquitous process occurring across all of the anxiety disorders and depression, and therefore from a nosological point of view could not be restricted to what was then called panic disorder. We also developed in this article for the first time a scheme for categorizing panic attacks based on whether triggers or cues for the panic attack were recognized by the patient (as in a phobic situation) or not and whether the attack was expected or unexpected. A panic attack categorized as uncued, and unexpected by the patient

was meant to replace the prevailing more biologically based conception of a "spontaneous" panic attack, a term that was judged to be unscientific since nothing was truly "spontaneous" in nature. We also made clear that whether the attacks were cued and expected or not was a construction of the patient rather than a biologically based phenomenon since the different types of attacks presented all but identically in terms of symptom clusters etc. This terminology and the ubiquity of panic attacks then began to make its way to the DSM process beginning with DSM-III-R published in 1987.

The second article on "Causes of sexual dysfunction: The role of anxiety in cognitive interference" (Barlow, 1986a, article # 12), ultimately provided the theoretical underpinning for a new conception of the nature of anxiety described in more detail in my book *Anxiety and its Disorders* published in 1988. The gist of this article was a fundamental refutation of the then widely accepted notion from Masters and Johnson that anxious arousal was the cause of sexual dysfunction. What was demonstrated in this article was that introducing anxiety in the form of shock threat while "normal" males (and later females) without sexual dysfunction were watching erotic content not only did not diminish sexual arousal objectively measured in the form of changes in penile circumference, but actually enhanced sexual arousal. Indeed, what seemed to differentiate sexually functional from dysfunctional males in this paradigm was the type and focus of cognitive activity in an erotic context, and the extent to which this sexual arousal was perceived to be under the participants control.

A sense of control became a major theoretical underpinning in my conceptions of anxiety (and later neuroticism itself) and the origins of perceptions of uncontrollability and anxiety were traced to early developmental experiences first in 1988 (Barlow, 1988) and then, in considerably

more detail, in a Psychological Bulletin article first authored by one of my students at the time, Bruce Chorpita, (Chorpita & Barlow, 1998, article #14).

In 1991, anticipating later work attempting to identify common underpinnings of all emotional disorders and building on theoretical work developed in Barlow (1988), I published an article “Disorders of Emotion” (Barlow, 1991, article #13) extending our conceptions of the etiology of panic disorder to other emotional disorders such as depression, stress and anger, and mania (excitement). Ten years later during a very productive year at the Center for Advanced Study and Behavioral Sciences with my colleagues Mark Bouton, and Sue Mineka, we wrote a paper updating in some detail the theory of the etiology of panic disorder integrating new findings from cognitive science and neuroscience (Bouton, Mineka, & Barlow, 2001, article # 15).

By that time DSM-5 was already in the works as noted above, and Tim Brown and I began speculating on what new findings from the classification of anxiety disorders grant would portend for classification in DSM-5 and beyond. Taking a unified transdiagnostic perspective, we published in 2009 an invited paper proposing a new hybrid dimensional- categorical classification system based on the shared features of anxiety and mood disorders (Brown & Barlow, 2009, article # 16). This work continues to be the major focus of the classification grant under Tim’s direction. Finally, and as also mentioned briefly above, following the substantial honor of being awarded the James McKeen Cattell award from the Association for Psychological Science in 2012 our team fashioned a paper based on portions of my award address entitled “The Nature, Diagnosis, and Treatment of Neuroticism: Back to the Future” in which we proposed that earlier ideas from Barlow (1988) on the origins of anxiety required refocusing to higher order dimensions of temperament, specifically neuroticism itself (Barlow, Sauer-Zavala, Carl, Bullis,

& Ellard, 2014, article # 17). We note in that article that neuroticism may be more malleable than previously thought and would ideally be the target of direct therapeutic intervention. This constitutes the very heart of our research approach at the current time.

The Ascendance of Evidence-Based Psychological Treatments

As mentioned in the section describing the Albany years, I served as president of the Society for Clinical Psychology (Division 12 of the American Psychological Association) in 1993 and created a task force on the "Promotion and Dissemination of Psychological Interventions". This initiative grew out of efforts in the late 1980's alluded to above when it had become very clear from a public policy point of view that the prevailing pharmacological approaches to mental disorders were being widely adopted and recommended in emerging health care policy statements. Most of these policies were targeting accountability under the relatively new concept of evidence based practice while at the same time addressing the spiraling cost of health care. These policies often took the form of clinical practice guidelines. As noted above, medications were deemed the treatment of choice for panic disorder despite the fact that our data had already indicated that new psychological interventions were at least as effective if not more effective (e.g. Klosko, Barlow, Tassinari, & Cerny, 1990; Barlow & Cerny, 1988).

Since these treatments were not readily available to clinicians or the public, and there were no efforts to make them available other than the occasional workshop, we formed a small company "Graywind Publications, INC." to disseminate new treatments. With unbridled optimism and a firmly established illusion of control over how easy this was going to be, we incorporated our company, invested money in printing several hundred copies of our treatment for panic disorder entitled "Mastery of Your Anxiety and Panic" and set up shop in the basement

of our house in 1988 with my wife Beverly running the show. After obtaining several mailing lists and testing the waters the initial investment was recouped in the first month and the company became profitable. The company expanded quickly, was moved into commercial space, and additional employees were hired. This experience convinced us that the demand existed for these programs, and that the problem was little or no infrastructure for dissemination. After five years the business required an infusion of substantial cash for expansion, a much advanced computer system for fulfillment, and a CEO to actually run the business, all steps that neither Beverly nor I were willing to take due to other commitments and a bit of fatigue from the very long hours and inevitable set of problems involved in any rapidly growing business. So we sold the business with it ultimately ending up in the hands of Oxford University Press where the series is known as "Treatments that Work".

But these early experiences made apparent the necessity of further promoting the existence of what we then called "empirically validated treatments" and exploring methods of dissemination and implementation In the first article in this section published in 1996 (Barlow, 1996, article # 18), I identified emerging policies, the status of research, and the necessity of responding to developing clinical practice guidelines such as they were at that time. By 2010 dissemination and implementation had become its own field of endeavor with appropriate funding mechanisms and emerging methodologies. Along with my student at the time, Kate McHugh, we detailed the status of those efforts offering judgments on where the field was lacking and what additional research was needed (McHugh & Barlow, 2010, article # 19). This topic was expanded into a book published in 2012 (McHugh & Barlow, 2012) and in 2013 our team once again updated emerging directions in research on evidence based psychological

treatments with suggestions for more broad based future efforts (Barlow, Bullis, Comer, & Ametaj, 2013, article # 21).

To improve dissemination, one of those recommendations focused on taking advantage of the power of direct to consumer marketing of psychological interventions, a strategy that has proved enormously successful for the large pharmaceutical companies. Initial demonstrations of the potential of this strategy for psychological interventions were detailed in a special series (Santucci, McHugh, & Barlow, 2012, article # 20). Notably, another of my students Katlin Gallo, conducted an important dissertation on this topic only recently published (Gallo, Comer, & Barlow, 2013; Gallo, Comer, Clarke, Antony, & Barlow, in press).

Thus, the articles in this last section representing as they do substantial advances in policy is perhaps one of the more remarkable developments over the course of my career since we literally began with nothing . From that humble beginning we have now reached a point, detailed in some of the above publications, where governments and health care policy makers around the world including the National Health Care System in the U.K. and the Veterans Health Administration in the United States are spending billions of dollars to make evidence based psychological treatments more readily available. While we have a very long way to go, I believe we can all be gratified by this progress.

Routledge World Library of Mental Health: *The neurotic paradox: Progress in understanding and treating anxiety and related disorders by David H. Barlow*

Methodology

1. Barlow, D. H. (1980). Behavior therapy: The next decade. *Behavior Therapy*,*11*(3), 315-328.
2. Barlow, D. H., & Nock, M. K. (2009). Why can't we be more idiographic in our research?. *Perspectives on Psychological Science*, *4*(1), 19-21.

Treatment of Anxiety and Related Disorders

3. Agras, S., Leitenberg, H., & Barlow, D. H. (1968). Social reinforcement in the modification of agoraphobia. *Archives of general psychiatry*, *19*(4), 423-427.
4. Barlow, D. H., & Wolfe, B. E. (1981). Behavioral approaches to anxiety disorders: A report on the NIMH-SUNY, Albany, research conference. *Journal of Consulting and Clinical Psychology*, *49*(3), 448-454.
5. Barlow, D. H. (1986). Behavioral conception and treatment of panic. *Psychopharmacology Bulletin*, *22*(3), 802-806.
6. Barlow, D. H., (1988). The process of fear and anxiety reduction: Affective therapy. In D. H. Barlow, *Anxiety and its disorders* (pp. 285-318). Guilford press. New York, NY.
7. Klosko, J. S., Barlow, D. H., Tassinari, R., & Cerny, J. A. (1990). A comparison of alprazolam and behavior therapy in treatment of panic disorder. *Journal of consulting and clinical psychology*, *58*(1), 77-84.
8. Barlow, D.H. (1992). The development of an anxiety research clinic. In D.K. Freedheim (Ed.), *History of psychotherapy: A century of change.* (pp. 429-431). WA: American Psychological Association.
9. Barlow, D. H., Gorman, J. M., Shear, M. K., & Woods, S. W. (2000). Cognitive-behavioral therapy, imipramine, or their combination for panic disorder: A randomized controlled trial. *Jama*, *283*(19), 2529-2536.
10. Barlow, D. H., Allen, L. B., & Choate, M. L. (2004). Toward a unified treatment for emotional disorders. *Behavior Therapy*, *35*(2), 205-230.

Nature, Diagnosis and Etiology of Anxiety and Related Disorders

11. Barlow, D. H., Vermilyea, J., Blanchard, E. B., Vermilyea, B. B., Di Nardo, P. A., & Cerny, J. A. (1985). The phenomenon of panic. *Journal of Abnormal Psychology*, *94*(3), 320-328.
12. Barlow, D. H. (1986). Causes of sexual dysfunction: The role of anxiety and cognitive interference. *Journal of Consulting and Clinical Psychology*, *54*(2), 140-148.
13. Barlow, D. H. (1991). Disorders of emotion. *Psychological inquiry*, *2*(1), 58-71.
14. Chorpita, B. F., & Barlow, D. H. (1998). The development of anxiety: the role of control in the early environment. *Psychological bulletin*, *124*(1), 3.
15. Bouton, M. E., Mineka, S., & Barlow, D. H. (2001). A modern learning theory perspective on the etiology of panic disorder. *Psychological review*, *108*(1), 4-32.
16. Brown, T. A., & Barlow, D. H. (2009). A proposal for a dimensional classification system based on the shared features of the DSM-IV anxiety and mood disorders: Implications for assessment and treatment. *Psychological assessment*, *21*(3), 256-271.
17. Barlow, D. H., Sauer-Zavala, S., Carl, J. R., Bullis, J. R., & Ellard, K. K. (2014). The Nature, Diagnosis, and Treatment of Neuroticism Back to the Future. *Clinical Psychological Science*, *2*(3), 344-365.

The Ascendance of Evidence-Based Psychological Treatments

18. Barlow, D. H. (1996). Health care policy, psychotherapy research, and the future of psychotherapy. *American Psychologist*, *51*(10), 1050-1058.
19. McHugh, R. K., & Barlow, D. H. (2010). The dissemination and implementation of evidence-based psychological treatments: a review of current efforts. *American Psychologist*, *65*(2), 73-84.
20. Santucci, L. C., McHugh, R. K., & Barlow, D. H. (2012). Direct-to-consumer marketing of evidence-based psychological interventions: Introduction. *Behavior therapy*, *43*(2), 231-235.
21. Barlow, D. H., Bullis, J. R., Comer, J. S., & Ametaj, A. A. (2013). Evidence-Based Psychological Treatments: An Update and a Way Forward. *Annual review of clinical psychology*, *9*, 1-27.

References

Abel, G.G., Barlow, D.H., Blanchard, E.B., & Guild, D. (1977). The components of rapists' sexual arousal. *Archives of General Psychiatry, 34,* 895-908.

Agras, W.S. (2012). The Mississippi years (1969-1974). *Behavior Modification*, *36*, 436-443.

Agras, W.S., Barlow, D.H., Chapin, H.N., Abel, G.G., & Leitenberg, H. (1974). Behavior modification of anorexia nervosa. *Archives of General Psychiatry, 30,* 279-286.

Agras, W.S., Leitenberg, H., & Barlow, D.H. (1968). Social reinforcement in the modification of agoraphobia. *Archives of General Psychiatry, 19,* 423-427.

Agras, W.S., Leitenberg, H., Barlow, D.H., & Thomson, L.E. (1969). Instruction and reinforcement in the modification of neurotic behavior. *American Journal of Psychiatry, 125,* 1435-1439.

Barlow, D.H. (1974a). The treatment of sexual deviation: Towards a comprehensive behavioral approach. In K.S. Calhoun, H.E. Adams, & K.M. Mitchell (Eds.), *Innovative treatment methods in psychopathology*. New York: John Wiley & Sons.

Barlow, D.H. (1974b). Psychologists in the emergency room: Implications for training. *Professional Psychology, 5,* 251-256.

Barlow, D. H. (1980). Behavior therapy: The next decade. *Behavior Therapy*,*11*(3), 315-328.

Barlow, D.H. (1981). On the relation of clinical research to clinical practice: Current issues, new directions. *Journal of Consulting and Clinical Psychology, 49,* 147-156.

Barlow, D.H. (1985). The dimensions of anxiety disorders. In A.H. Tuma & J.D. Maser (Eds.), *Anxiety and the anxiety disorders.* Hillsdale, NJ: Lawrence Erlbaum Associates.

Barlow, D. H. (1986a). Causes of sexual dysfunction: The role of anxiety and cognitive interference. *Journal of Consulting and Clinical Psychology*, *54*(2), 140-148.

Barlow, D. H. (1986b). Behavioral conception and treatment of panic. *Psychopharmacology Bulletin*, *22*(3), 802-806.

Barlow, D.H. (1987). The classification of anxiety disorders: In G.L. Tischler (Ed.), *Diagnoses and classification in psychiatry: A critical appraisal of DSM-III* (pp. 223-242). Cambridge University Press.

Barlow, D.H. (1988). *Anxiety and its disorders: The nature and treatment of anxiety and panic*. New York: The Guilford Press.

Barlow, D. H. (1991). Disorders of emotion. *Psychological inquiry*, *2*(1), 58-71.

Barlow, D.H. (1992). The development of an anxiety research clinic. In D.K. Freedheim (Ed.), *History of psychotherapy: A century of change.* (pp. 429-431). WA: American Psychological Association.

Barlow, D. H. (1996). Health care policy, psychotherapy research, and the future of psychotherapy. *American Psychologist*, *51*(10), 1050-1058.

Barlow, D.H. (2004). Psychological treatments. *American Psychologist, 59*(9), 869-878.

Barlow, D.H., Agras, W.S., Leitenberg, H., & Wincze, J.P. (1970). An experimental analysis of the effectiveness of "shaping" in reducing maladaptive avoidance behavior: An analogue study. *Behaviour Research and Therapy, 8,* 165-173.

Barlow, D. H., Allen, L. B., & Choate, M. L. (2004). Toward a unified treatment for emotional disorders. *Behavior Therapy*, *35*(2), 205-230.

Barlow, D.H., Blanchard, E.B., Vermilyea, J.A., Vermilyea, B.B., & Di Nardo, P.A. (1986). Generalized anxiety and generalized anxiety disorder: Description and reconceptualization. *The American Journal of Psychiatry, 143,* 40-44.

Barlow, D. H., Bullis, J. R., Comer, J. S., & Ametaj, A. A. (2013). Evidence-Based Psychological Treatments: An Update and a Way Forward. *Annual review of clinical psychology*, *9*, 1-27.

Barlow, D.H., & Cerny, J.A. (1988). *Psychological treatment of panic*. New York: The Guilford Press.

Barlow, D.H., & Craske, M.G. (1988). *Mastery of your anxiety and panic*. Albany, NY: Graywind Publications.

Barlow, D. H., & Durand, V. M. (2014). *Abnormal psychology: An integrative approach* (7th ed.). Belmont, CA: Wadsworth, Cengage Learning.

Barlow, D.H., Gorman, J.M., Shear, M.K., & Woods, S.W. (2000). Cognitive-behavioral therapy, imipramine, or their combination for panic disorder: A randomized controlled trial. *JAMA, 283,* 2529-2536.

Barlow, D.H., Hayes, S.C., & Nelson, R.O. (1984). *The scientist-practitioner: Research and accountability in clinical and educational settings.* New York: Pergamon Press.

Barlow, D.H., & Hersen, M. (1973). Single case experimental designs: Uses in applied clinical research. *Archives of General Psychiatry, 29,* 319-325.

Barlow, D.H., Leitenberg, H., & Agras, W.S. (1969). Experimental control of sexual deviation through manipulation of the noxious scene in covert sensitization. *Journal of Abnormal Psychology, 74,* 596-601.

Barlow, D.H., Leitenberg, H., Agras, W.S., & Wincze, J.P. (1969). The transfer gap in systematic desensitization: An analogue study. *Behaviour Research and Therapy, 7,* 191-197.

Barlow, D.H., Mavissakalian, M.R., & Schofield, L.D. (1980). Patterns of desynchrony in agoraphobia: A preliminary report. *Behaviour Research and Therapy, 18,* 441-448.

Barlow, D. H., & Nock, M. K. (2009). Why can't we be more idiographic in our research?. *Perspectives on Psychological Science*, *4*(1), 19-21.

Barlow, D.H., O'Brien, G.T., & Last, C.G. (1984). Couples treatment of agoraphobia. *Behavior Therapy, 15,* 41-59.

Barlow, D. H., Sauer-Zavala, S., Carl, J. R., Bullis, J. R., & Ellard, K. K. (2014). The Nature, Diagnosis, and Treatment of Neuroticism Back to the Future. *Clinical Psychological Science*, *2*(3), 344-365.

Barlow, D. H., Vermilyea, J., Blanchard, E. B., Vermilyea, B. B., Di Nardo, P. A., & Cerny, J. A. (1985). The phenomenon of panic. *Journal of Abnormal Psychology*, *94*(3), 320-328.

Barlow, D. H., & Wolfe, B. E. (1981). Behavioral approaches to anxiety disorders: A report on the NIMH-SUNY, Albany, research conference. *Journal of Consulting and Clinical Psychology*, *49*(3), 448-454.

Bernhardt, A.J., Hersen, M., & Barlow, D.H. (1972). Measurement and modification of spasmodic torticollis: An experimental analysis. *Behavior Therapy, 3,* 294-298.

Bouton, M. E., Mineka, S., & Barlow, D. H. (2001). A modern learning theory perspective on the etiology of panic disorder. *Psychological review*, *108*(1), 4-32.

Brown, T. A., & Barlow, D. H. (2009). A proposal for a dimensional classification system based on the shared features of the DSM-IV anxiety and mood disorders: Implications for assessment and treatment. *Psychological assessment*, *21*(3), 256-271.

Chorpita, B. F., & Barlow, D. H. (1998). The development of anxiety: the role of control in the early environment. *Psychological bulletin*, *124*(1), 3.

Craske, M.G., & Barlow, D.H. (1990). *Therapist guide for the mastery of your anxiety and panic program*. Albany, NY: Graywind Publications.

Craske, M.G., Barlow, D.H., & O'Leary, T. (1992). *Mastery of your anxiety and worry: Client workbook*. Albany, NY: Graywind Publications.

Gallo, K. P., Comer, J. S., & Barlow, D. H. (2013). Direct-to-consumer marketing of psychological treatments for anxiety disorders. *Journal of Anxiety Disorders, 27*(8), 793-801.

Gallo, K.P., Comer, J.S., Clarke, R.N., Antony, M.M., & Barlow, D.H. (in press). Direct-to-consumer marketing of psychological treatments: A randomized controlled trial. *Journal of Consulting and Clinical Psychology.*

Harbert, T.L., Barlow, D.H., Hersen, M., & Austin, J.B. (1974). Measurement and modification of incestuous behavior: A case study. *Psychological Reports, 34,* 79-86.

Hayes, S.C., Brownell, K.D., & Barlow, D.H. (1978). The use of self-administered covert sensitization in the treatment of exhibitionism and sadism. *Behavior Therapy, 9,* 283-290.

Hersen, M., & Barlow, D.H. (1976). *Single case experimental designs: Strategies for studying behavior change*. New York: Pergamon Press.

Klosko, J.S., Barlow, D.H., Tassinari, R., & Cerny, J.A. (1990). A comparison of alprazolam and cognitive-behavior therapy in treatment of panic disorder. *Journal of Consulting and Clinical Psychology, 58* 77-84.

McHugh, R. K., & Barlow, D. H. (2010). The dissemination and implementation of evidence-based psychological treatments: A review of current efforts. *American Psychologist*, *65*(2), 73-84.

McHugh, R. K., & Barlow, D. H. (Eds.). (2012). *Dissemination and implementation of evidence-based psychological interventions*. New York: Oxford University Press.

Miller, P.M., & Barlow, D.H. (1973). Behavioral approaches to the treatment of alcoholism. *Journal of Nervous and Mental Disease, 157,* 10-20.

Mowrer, O. H. (1950). *Learning theory and the personality dynamics*. New York: Arnold Press.

Pineda, M.R., Barlow, D.H., & Turner, B.B. (1971). Treatment of a severe speech disorder by behavior modification: A case study. *Behavior Therapy and Experimental Psychiatry, 2*, 203-207.

Rapee, R.M., & Barlow, D.H. (1991). *Chronic anxiety, generalized anxiety disorder, and mixed anxiety depression.* NY: The Guilford Publications, Inc.

Santucci, L. C., McHugh, R. K., & Barlow, D. H. (2012). Direct-to-consumer marketing of evidence-based psychological interventions: Introduction. *Behavior therapy*, *43*(2), 231-235.

Zinbarg, R.E., Craske, M.G., & Barlow, D.H. (1993). *Mastery of Your Anxiety and Worry: Therapist Guide*. Albany, NY: Graywind Publications.