

the Behavior Therapist

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Instructions for Authors

The Association for Advancement of Behavior Therapy publishes *the Behavior Therapist* as a service to its membership. Eight issues are published annually. The purpose is to provide a vehicle for the rapid dissemination of news, recent advances, and innovative applications in behavior therapy.

■ **Feature articles** that are approximately 16 double-spaced manuscript pages may be submitted. ■ **Brief articles**, approximately 6 to 12 double-spaced manuscript pages, are preferred. ■ **Feature articles** and **brief articles** should be accompanied by a 75- to 100-word abstract. ■ **Letters to the Editor** may be used to respond to articles published in *the Behavior Therapist* or to voice a professional opinion. Letters should be limited to approximately 3 double-spaced manuscript pages. Please contact the Editor or any of the Associate Editors for guidance prior to submitting series, special issues, or other unique formats. All submissions should be in triplicate and formatted according to the *Publication Manual of the American Psychological Association, 5th edition*. Prior to publication, authors will be asked to provide a 3.5" diskette containing a file copy of the final version of their manuscript. Authors submitting materials to *the Behavior Therapist* do so with the understanding that the copyright of published materials shall be assigned exclusively to the Association for Advancement of Behavior Therapy. **Please submit materials** to the attention of the Editor: George F. Ronan, Ph.D., Department of Psychology, Central Michigan University, Mount Pleasant, MI 48859.

Open Forum

SPECIAL SERIES

Future Directions in Behavior Therapy: Evolve or Die?

Daniel J. Moran, *Valparaiso University*

Selectionistic sciences account for complexity and changes found in nature by using the three-step process of variation, selection, and retention. Although these principles are used more familiarly with evolution, they are also basic principles in the science of behavior. A person's behavior will vary, the consequences of this variation may reinforce or "select" this behavior, and the behavior will be retained if certain contingencies remain in place. Behavior therapy can be thought of as applied selectionistic science. Practitioners assess the client's behavioral variation (from the norm and from their own baseline) and influence behavioral variation (by changing the client's environment). The therapist also delivers reinforcing consequences for clinical gains, which helps select these functional behaviors, and these more functional responses are retained when the therapist helps the client generalize his or her behavior to different situations. Ultimately, success will depend on whether the client's therapeutic gains are maintained by the natural environmental selections.

Behavior therapy itself is subject to selectionism principles. At first, behavior therapy was an exceptional variation from the norm of psychotherapy. Its omnibus effectiveness and empirical support were selected, and the widespread need for effective, economical, "managed care-approved" therapies was a decisive factor in its retention.

Although behavior therapy has found a rich ecological niche, it must also find new environments in which to thrive, maintain its strength over competition for resources, and produce new

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generations of replicants that function in
other environs. If, as Lucretius states,
"change is the only constant," behavior
therapy needs to continue to "evolve or
die." What directions should behavior
therapy take in order to remain fit and sur-
vive? Three AABT members who have
made significant contributions to the field
were asked to make suggestions to pro-
mote the longevity of cognitive-behavior
therapy. I posed three questions to Richard
Suinn, past president of APA and AABT,
which he discussed during our World
Congress of Behavioral and Cognitive
Therapies roundtable, and his answers fol-

low. Robert Kohlenberg, co-author of
Functional Analytic Psychotherapy and pro-
moter of clinical behavior analysis and
functionally enhanced cognitive therapy,
discusses the potential problems that cog-
nitive-behavior therapy faces and how
they can be resolved. Mitchell Schare, past
AABT Student Membership chair and act-
ing chair of the Hofstra University
Clinical/School Ph.D. program, makes a
number of suggestions for the future inte-
gration of behavioral technologies. Taken
together, these suggestions provide a pre-
liminary blueprint for developing future
objectives for behavior therapy. ✍

Answering Questions Regarding the Future Directions in Behavior Therapy

Richard M. Suinn, *Colorado State University*

I appreciate the invitation to present
some views about what is needed in the
future, at least from one person's per-
spective. At the end of this article, I will
share a bibliography for those interested in
general readings on various topics.

What does cognitive behavior therapy need to do to maintain longevity?

1. Maintain the course. Cognitive
behavior therapy is based upon the inte-
gration of research and practice. A proce-
dure must show proven efficacy before it is
adopted by practitioners. This, in turn, has
led to a focus on outcomes research.

To assure longevity, we must maintain
this integration. A current threat has been
managed care. But cognitive behavior
therapy has been unique in that documen-
tation is available to present the outcomes
expected from treatment, to offer a treat-
ment rationale, to identify concretely the
behavioral changes anticipated, and to
offer information on how to track clients'
progress. (For information on approaches
to assessing efficacy and effectiveness of
therapy, see Kendall and Chambless,
1998).

2. Keep the blighters out! Some of the
reputation of cognitive behavior therapy
has been soiled by interventions that claim
to be based on cognitive behavior therapy
principles. We must continue to clearly
define what are and what are not cognitive
behavior therapies in order to protect the
longevity of our approaches. A recent
series of analyses by Dr. Jeffrey Lohr at the
University of Arkansas has appeared in
the Behavior Therapist (see, for example,
Meunier, Parker, and Kline, 2001). He
argues that some interventions purporting

to be cognitive behavior therapies are
indeed making false claims. Although
some may consider his conclusions contro-
versial, he does provide clear arguments
for his positions.

3. Get to the masses. We need to get
the word out to the masses, both the gen-
eral public and students. I am referring to
"marketing," normally considered a bad
word. . . . But to use more acceptable lan-
guage: *We need to do a better and more active
job of educating the public.*

Cognitive behavior therapy has been
successful in treating a variety of human
conditions, and the public needs to be
aware of this so that they can advocate for
greater choice of therapies. For instance,
there are firm data showing the value of
cognitive behavior therapy for many phys-
ical disorders, ranging from heart disease
to pain management to cancer. Regarding
the latter, it is my fondest hope that all
cancer treatment teams will automatically
include a psychologist. But it will take
pressure from an educated public to bring
this about.

Students also need to take a more
active role. In a recent half-million-dollar
study supported by the Pew Foundation,
graduate students were asked what was
needed to improve graduate education for
the 21st century. A common answer was
that greater diversity of training was need-
ed. During my year as president of the
American Psychological Association, I
traveled across the U.S. listening to many
psychologists in independent practice.
Those who were still doing well despite
managed care had one piece of advice: Be
competent with diverse skills for diverse
populations and diverse goals. Cognitive
behavior therapy offers a set of such skills

and underlying principles that permit such diversity.

What research areas will be needed in the future?

1. Certainly research is needed on diverse populations. The research in the U.S. on ethnic populations today is abysmal. Perhaps our international colleagues will help show the way. However, it still needs to be recognized that ethnic minority groups in each country possess some cultural differences from their ancestral heritage. So, for example, Asian Americans within the United States may show some differences from their Asian counterparts in Asia. See the American Psychological Association (2001) for guidelines on multicultural proficiency and Shiang, Kjellander, Huang, and Bogumill (1998) for a specific example regarding Chinese clients.

2. Process variables must continue to demand our attention in research, such as dose-response, the interaction between medication and cognitive behavior therapy, and response prevention.

3. Cost offset data must be added to research measures. We must be better able to identify the value of cognitive behavior therapy not only in terms of efficacy, but also in terms of the relative financial costs. (Some procedures are discussed in Yates, 1995).

How can we improve on the practice of cognitive behavior therapy?

1. Students need more emphasis on the interviewing/assessment phase of treatment, especially on case conceptualization and planning. I have been impressed by practitioners in the communication disorders programs (i.e., speech and hearing therapists). They do what I would refer to as "lesson plans" for each session, based upon assessment information and the progress of the client. Although such an approach is common for the first cognitive behavior therapy session, we need to emphasize reviewing the proposed plan of action for each subsequent session for each client. If a strength of cognitive behavior therapy is its preciseness and its basis on clear principles, then we should be able to state these in a planful way.

2. Training should be broad, not narrow. Some practicum training in graduate programs emphasizes the special research population interests of the program's faculty. The advantage is that the student learns in depth how to work with this type of client. The disadvantage is that the students have no breadth in either types of clients or in intervention approaches. My own choice would be for breadth rather than depth, although I recognize the importance of depth. Perhaps we need to leave it to the internship agencies to provide the needed depth—or, heaven forbid, even consider establishing residencies?

3. Another way of improving on practice is by the development of practice guidelines; I recommend reading Hayes

and Gregg (2001) for more information regarding this topic.

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AABT: On the Precipice of Becoming Dysfunctional?

Robert J. Kohlenberg, *University of Washington*

When my clients ask for an explanation for why they persist in carrying out distressing, self-defeating, dysfunctional behavior, here is the answer I give them: At one time, earlier in their lives, the very same behavior that is now so troubling was adaptive. In that earlier context (usually in their families), their problematic behavior worked—that is, led to their psychological survival, family harmony, and the successes they now enjoy. Unfortunately, I go on to explain, circumstances have changed, and those very same behaviors that were once so adaptive are now self-defeating and causing their current distress and failures.

I believe AABT stands in danger of being in the same position as my clients.

Certain aspects of its amazingly successful and adaptive patterns that have worked so well in the past have the potential to become dysfunctional and self-defeating.

Before turning to what these incipient dysfunctional behaviors are, I want to speak to how successful they have been. At the very first AABT meeting I attended in the mid 70s, there were about 100 people in attendance. The papers were mainly reports about increasing or decreasing targeted problematic behaviors in children and institutionalized patients and using desensitization and relaxation to reduce anxiety. The attendees were a group of mavericks and outsiders to the existing mental health establishment. They spoke about the importance of collecting data and empirically testing the effects of their

treatments. Of equal importance, they were united against a common enemy—an overwhelmingly powerful mental health establishment, dominated by psychiatrists and psychologists, that had accepted an account of maladaptive behavior that was incompatible with cognitive-behavior therapy (CBT). This account was primarily informed by psychodynamic theory but also included the medical model (Ullman & Krasner, 1965).

Well, things have changed in the last 25 years. Membership has grown to 4,000 and AABT is now a significant part of the establishment. The former enemy has capitulated and attends meetings and workshops. They want to learn how to follow the treatment manual instructions and do empirically validated treatments that many third-party payers require. In my experience, most therapists now say they "use" behavior therapy techniques. At the last AABT workshop I attended, half the attendees were psychiatrists.

As the outsiders in the 70s, however, the behavior of our founders was shaped by some powerful negative influences. For

instance, the establishment accused the fledgling cognitive-behavior therapists of only treating symptoms and harming their clients by making them vulnerable to symptom substitution. The potentially harmful, treatments, according to the establishment, included procedures such as homework assignments, extinction, contingency management, behavioral contracts, and focusing on the behavior (rather than the underlying disease). Early behavior therapists, in self-defense, found it necessary to reject the establishment—their theories, claims, and techniques. Although this rejection paved the way for the development of effective therapies and eventual growth of AABT, there were some unintended effects that may not be serving us well now.

The rejection of the dominant model took two forms. First, our founders rejected psychodynamic and medical-model assumptions and offered instead an account of clinical problems based on learning. Second, rejection of the medical/psychodynamic model also included defining CBT interventions in terms of their differences from the establishment view. This counterattack definition of CBT was articulated in Ullman and Krasner's 1965 landmark volume *Case Studies in Behavior Modification*. They specifically defined CBT as an approach that dismissed the medical model and Freudian assumptions along with such techniques as "strengthening egos" and "expressive" treatments. Eysenck (1968) suggested that behavior therapy rejects the disease model and its emphasis on uncovering repressed memories to treat the clinical problem. Rather, behavior therapy conceptualizes clinical problems as behaviors that are learned and endure as dysfunctional habits. Behavior therapy treatment involves using learning principles to establish more functional behavior. In contrast to the passive therapists of the opposition, CBT was defined by its active approach to treatment with a therapist who set agendas, made direct suggestions, manipulated contingencies, and assigned homework. The defense against charges of harming clients and producing symptom substitution was extremely effective. CBT was committed to empiricism—cognitive behavior therapists collected data! They supported the effectiveness of their anti-establishment techniques with empirical findings that showed that it worked, and symptom substitution did not occur.

So far, so good. But I contend that the considerable benefits accrued by CBT for not doing what the old mental health establishment advocated has led CBT to overlook some useful techniques and concepts, which, in turn, may be responsible for potentially dysfunctional behavior in

current-day practice. I will discuss two such possibilities.

Potential Problem I: CBT's Rejection of the Morbid View of the Human Condition

One such effect is the rejection of any form of the somber psychoanalytic view of the nature of the human condition. As articulated by Messer and Winokur (1980), the human condition inherently involves ambivalence, irresolvable conflict, and suffering. Psychoanalyst Hartman (as cited in Messer & Winokur) stated that "A healthy person must have the capacity to suffer and be depressed." Along these lines, Freud's classic definition of mental health was the ability "to love and work" and, notably, did *not* refer to being happy and devoid of negative feelings. This view can be contrasted with the dominant CBT view characterized by Ellis and Harper's (1975) contention that negative emotions are neurotic and based on irrational thought and Lazarus's comment that "The control or absence of unpleasant emotions coupled with an increase in positive feeling is a most worthy goal" (as cited in Messer & Winokur). If, on the other hand, it turns out that "true" human nature is more like the psychoanalyst's view than that of the cognitive-behavior therapist's, then a CBT

that doesn't take this into account will not be able to fully address the range of problems that our clients present. Needless to say, a therapeutic system that fails to address the problems of the client can be termed dysfunctional.

Perhaps it could be said that "true human nature" is not a notion that can be addressed within the realm of CBT. The psychoanalysts arrived at this proposition based on a model involving nonbehavioral entities—a mental "seething cauldron" kept in check by other mental mechanisms such as the ego and superego. How, then, do we explain that two past presidents of AABT arrived at systems of therapy that are much more consistent with the view of human nature espoused by psychoanalysts than mainstream cognitive-behavior therapists? Marsha Linehan and Steve Hayes focus on the importance of accepting the fact that people suffer and that therapy should include a way to accept these negative feelings rather than ameliorating them. In contrast to the traditional CBT view that negative emotions must be ameliorated, consider the comment by Hayes, Strosahl, and Wilson (1999, p.1): "The single most remarkable fact of human existence is how hard it is for humans to be happy." My thesis that CBT rejects the notion that the human condition might entail suffering is

further corroborated by Linehan (personal communication, 2002), who reported that she asked Terry Wilson in the early 1980s if her therapeutic system was behavior therapy. According to Linehan, his response was that it was, except for the part about tolerating negative feelings.

Potential Problem II: Overrejection of Transference

Similarly, early CBT dismissed any notion that hearkened to transference. Transference, an unconscious, erroneous reenactment of the client's neurosis via distorted perceptions of the therapist, was a concept theoretically abhorrent to the learning theory of AABT members. Further, psychoanalysts used the theory of transference to discredit many of the features of the change process that are hallmarks of CBT (an active therapist, a focus on structuring therapy session, assigning homework, and short-term treatment). Thus, it is no accident that most cognitive-behavioral therapists do not include the term transference, nor do they even consider transference-like features, in their conception of the therapist-client relationship.

Kohlenberg and Tsai (1991), however, have argued that the occurrence of transference-like phenomena is an almost inevitable conclusion of a behavioral functional analysis of the CBT client-therapist interaction. In particular, the therapeutic relationship is (a) an environment that can evoke "clinically relevant behaviors"—the client's daily life problems (and improvements) that actually occur during the session in relationship to the therapist, and (b) the therapist's activities of "doing therapy" can inadvertently strengthen or weaken (immediately reinforce or punish) these problematic behaviors. Given the well-accepted maxim that in-vivo treatment is maximally effective, Kohlenberg and Tsai view the occurrence of clinically relevant behavior as providing extraordinary opportunities for therapeutic change.

Although all CBT therapists stress the importance of the client-therapist relationship, the transference-like quality of a here-and-now occurrence of the client's presenting problem is rarely mentioned, much less used during treatment. As reviewed by Kohlenberg, Tsai, and Kohlenberg (1996), the major cognitive-behavior texts view the therapeutic relationship as ancillary to technique or something that produces collaboration in the service of enhancing the therapist's technical influence over the client. There are some notable exceptions, including Safran and Segal (1990), Young, (1990), and Goldfried and Davison (1976). Further, in my experience, most CBT therapists also say they attend to in-vivo

occurrences of the client behavior. Notwithstanding this claim of practicing clinicians and the exceptions noted above, the data in a recent study by Kohlenberg, Kanter, Bolling, Parker, and Tsai (in press) show that qualified, experienced cognitive-behavior therapists rarely attend to clinically relevant behavior. An exception, however, is Linehan's dialectical behavior therapy (DBT), which explicitly emphasizes the therapeutic use of clinically relevant behavior.

On the Precipice or Not?

Now, what is the evidence that AABT is or is not on the precipice of becoming dysfunctional? One bit of evidence is the declining membership of AABT. In spite of phenomenal growth in the last 25 years, it has been declining in the last 4. Although a myriad of economic and social factors contribute to this decline, one factor might be that we are not attracting clinicians who place importance on the curative potential of intense, involving therapeutic relationships and/or the therapeutic importance of accepting and tolerating pain and distress. There also appears to be some resistance to innovation (Corrigan, 2001; Hayes, 2002), particularly to acceptance and commitment therapy (Hayes, Strosahl, & Wilson, 1999), functional analytic psychotherapy (Kohlenberg & Tsai, 1991), and DBT (Linehan, 1993).

On the other hand, there are innovative treatments associated with AABT that address the issues discussed above. In addition to those already cited, other examples are mindfulness (Teasdale et al., 2000) and the Cognitive Behavioral Analysis System of Psychotherapy (CBASP; McCullough, 2000). The future of AABT depends on the development of innovative treatments that address the voids of the type discussed above and ultimately the data on their effectiveness.

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A Need for the Behavioral Integration of Technology (BIT): Back to the Future

Mitchell L. Schare, *Hofstra University*

We must come to the realization that some time over the last decade or so our baby, behavior therapy, has grown up. From its conception at around the time of the Boulder conference to its birth in the late 1950s, we fed it with research, played with different therapies, and clothed it in empiricism. As with all healthy children, behavior therapy has come unto its own. As parents, we continue to nurture and direct but with a sense that we do not have as much impact as we once did. This is how I view the current state of behavior therapy.

What was once alien has now become mainstream. Behavior therapy no longer needs to defend itself against paranoid accusations of being “brainwashing” and mind-control treatment, or complaints that it is mechanistic, anti-thought, and unconcerned with “real” emotions. Rather, behavior therapy, in its multitude of forms, is practiced by mental health professionals of many varieties and is found in hospitals, clinics, and schools around the world. The public, mass media, government agencies, medical community, and even the insurance industry have accepted behavior therapy. So why am I worried? A loving parent


still looks out for the health and welfare of his or her children.

Behavior therapy has become large in many ways. Behavior therapy and behavioral principles have entered mainstream education in most psychology department curricula. Any sampling of undergraduate texts for introductory, abnormal, or personality psychology would reflect this point. At the graduate level, a perusal of the APA's annual listing of accredited training sites reflects an ongoing growth of new programs, many of which are behaviorally oriented. This is a good thing: After all, who would be against spreading our knowledge and techniques to help alleviate human suffering? Yet in order to have more programs, we need more professors, who produce more research, who need to publish in order to get tenured. Test, book, and journal publishers seem to be happy to take up the cause. Every organization and division I belong to also has a journal (or two) devoted to its membership and their interests. Am I the only person experiencing information overload through this seemingly endless proliferation of new journals devoted to psychological specialties, subspecialties and sub-subspecialties?

I am not trying to be cynical here—I am trying to make a point that behavior therapy has become large and, in many ways, big business.


In order for behavior therapy to survive, we need to control its destiny. Whether it is accomplished by Division 12, AABT, or Nathan and Gorman (2002), empiricism must dictate which therapeutic procedures are viable and which should be dismissed. In getting large, the world of behavior therapy has experienced a proliferation of therapies from which to choose. From this alphabet soup of ABA, ACT, AMT, CT, DBT, EMDR, ERP, IT, REBT, SD, ECT (no, that's etc.), one can see some of the more popular and/or recent therapeutic approaches in BT or CBT. We need to know what treatments work, with whom, and in what time frames. We all know that techniques come and go; does anyone practice neuro-linguistic programming anymore? The trend toward evidence-based treatments is crucial in determining the future of our field. We are the empiricists studying our science. Therefore, we, and not the economics of insurance, must determine which treatments should be offered and how they are to be undertaken.

We must do a better job of truly educating the public about what we do and how we do it. The 10-session “miracle” cures of the 1960s to 80s outcome-study literature has resulted in a public actually believing that many of their problems could be cured through such quick fixes.



April is AABT election month.
Watch for your ballot in the mail. Please sign the return envelope *or* tear card (or your ballot won't count). It must be post-marked April 30.

ote.



By the 1990s, insurance companies, when turning their scalpels in our direction looking for costs to cut, bought our promises, demanded our goal statements, and gave us 8 sessions. Evidence-based treatments help to define what we do and how to do it. We need to reeducate the public. Unfortunately, behavior therapists have a poor track record of accurately presenting themselves—and what they do—to the public, which still expects, “Please, lie on my couch and tell me your dreams.”

As an educator, I am very much concerned with what my students need to be taught to have productive careers as clinicians and how to accomplish this. (So many therapies, so little time . . .) However, something else happened while behavior therapy grew larger: It became very specialized. We often don’t speak of or research generic therapy techniques. We tend to talk about Beck’s cognitive therapy for depression, Barlow’s treatment of panic disorder, or Linehan’s dialectical behavior therapy for borderline personality disorder. Specific techniques get attached to specific disorders. While one could argue that this is a healthy sign for the maturation of behavior therapy, it creates great dilemmas for the teaching faculty. Do we produce a generalist student who then specializes on internship or postdoc, or must training programs advertise what specializations they teach to potential students in advance of their enrollments? When was the last time a good general graduate-level text existed for the teaching of behavior therapy? One great book, *Clinical Behavior Therapy* (Goldfried & Davison, 1976), was mildly revised in

1994 and is still in demand and print. Ask yourself, Why?

I propose that we need to conduct new research combining a couple of interesting movements from our recent past. The psychotherapy integration movement, loosely traced back to Wachtel’s (1977) seminal work, searches for commonalities across therapies. I strongly believe that we need to start addressing this same issue but within behavior therapy. Over the years we have developed so many techniques (maybe too many) that resemble others by stealing a piece here and a bit there. However, this direction of future research should also be based upon Lazarus’s notion of technical eclecticism, in which he advocated the use of a variety of therapy techniques, provided they were empirically derived. In this regard, I propose that we begin to adhere to a new research philosophy called the Behavioral Integration of Technology (BIT). The implication of the BIT philosophy is that by identifying technical commonalities across therapy techniques, we can overcome the multitude of therapeutic approaches offered by behavior therapy. Furthermore, we need to assess what works across different avenues of psychopathology, getting out of this trap of one problem—one technique. By using the BIT philosophy, behavior therapy will move forward by better defining what it does. BIT is not meant to suggest that other, new approaches to treatment cannot or should not be developed. In fact, new methodologies would benefit from an even stronger empirical base on which to build. We need to know what works and why. Let’s stop dressing up and packaging every new behavioral technique. A propri-

etary approach to developing therapy, even if not intended as such, is simply not good science.

If my proposal for a philosophy of BIT sounds unnecessary to you, it is probably because you are either too young or haven’t read original work of Wolpe, Eysenck, Lazarus, Bandura, Marks, Rachman, Stampfl, Yates, Ullman, Krasner, Mowrer, Agras, Meyer, Azrin, Ayllon, Brady, and so on. To appreciate the BIT philosophy is to understand the true empirical nature of behavior therapy, which experienced an empirically driven, bottom-up development. Our baby has become bloated and somewhat full of itself. We need to return to the basics from which behavior therapy was initially derived, *the laws of learning*, and make it healthy once again.

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Open Forum

Increasing Diversity in Psychology: A Call for the Involvement of Ethnic Minority Graduate Students

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A recent issue of *the Behavior Therapist* dedicated a special series to the lack of ethnic minorities in the psychological sciences. The series addressed increasing the number of ethnic minority therapists trained in behavioral and cognitive-behavioral techniques (Caraway, 2001), the importance of diversity in training (Iwamasa, 2001), as well as sexual minority issues in training (Hart, 2001; Martell, 2001). The articles provided an

accurate depiction of the discrepancy between the relative heterogeneous nature of the general population that we serve as therapists and therapists in training and the homogeneous nature of current behavioral and cognitive-behavioral therapists (Safren, 2001). In the spirit of that special series promoting discussion and action in increasing diversity in behavior therapy, this brief article offers a perspective that calls for the active involvement of ethnic

minority students in the recruitment of other minority students into the field of psychology.

Caraway (2001) identified several initiatives that have been implemented to increase diversity in the training of psychologists. Some of these initiatives include the implementation of accreditation processes, boards and task forces, as well as scholarships and other incentives. These initiatives have resulted in a 19% increase in APA minority affiliation; of course, this is an “increase” to a total of 5%. A 19% increase is commendable, although a 19% increase to a total of 5% is surely nothing to write home about. As such, the obvious question may not be why are these initiatives not working, but why are they not working as well as they should? Upon inspection it would seem that the majority of the proposed interventions devoted to increasing minority involvement in the psychological sciences come after the fact. The issue that war-

rants attention in the not-too-distant future may not be how many ethnic individuals are actually in the field of psychology, but how many are actually applying to the field of psychology. Of course, both of these issues are related, though it could be argued that the number of people applying to the field may be an indicator of the number of ethnic populations drawn to the field.

Logic would assume that if we are not getting many minority students involved in various specialized aspects of the field of psychology (i.e., cognitive-behavioral training), then it is highly likely that not many minorities are applying to get into the field to begin with. What this suggests is that we have to address the problem at its root. It does not seem particularly efficient to establish initiatives to increase minority affiliation in specialized areas when the number of minorities in the field in its entirety is minimal. Programs that are established to increase minority affiliation in psychological sciences may have to be implemented during phases in which interest begins to develop in terms of career goals and options, not after the fact.

Initiatives promoting minority affiliation need to be applied very early, not when people are relatively set in their ways in terms of jobs and career direction. Early intervention might be the key to increasing the diversity of behaviorally trained clinicians. Accordingly, an early intervention approach aimed at increasing diversity in psychology will likely consist of initiatives directed at the undergraduate and, more importantly, the high school level. Other professions (i.e., medical, law, engineering) have implemented recruitment programs for minority students at the high school level. However, the high school level appears to be a relatively untapped resource in terms of the recruitment of ethnic minority students into the field of psychology. Recruitment for student affiliation into various psychological organizations more or less targets undergraduates and graduates. As a general practice, this approach may need to be reconsidered, given that high school is a critical period in terms of career development—perhaps the most practical time to make students of diversity aware that psychology is a real and achievable career option. Targeting high school students of diversity will likely involve providing education about the profession of psychology in general and, more specifically, about its various career options. It is surprising how many high school students, or, for that matter, undergraduates, do not know exactly what psychologists do (I know I didn't!). This early intervention approach may also consist of organizing workshops, career fairs, and presentations related to

the profession of psychology at both the undergraduate and high school levels.

Caraway (2001) suggests encouraging faculty of color to play a more active role in the recruitment of ethnic minority students. This appears to be a practical approach that surely warrants further consideration. But it may be more practical to incorporate minority graduate students into the active recruitment of other minority students at the undergraduate and high school level. In line with the early intervention perspective, a task force of current minority graduate students may be assembled to establish initiatives in the recruitment of undergraduate and high school students to the field of psychology. This approach may also have minority graduate students serve as ambassadors to undergraduate universities and high schools in their area. In this role, minority graduate students may then serve as mentors to minority students who develop interest in the field.

This early intervention approach is likely to be more effective for several reasons. First, for minority students, or for anyone, becoming a graduate student may be perceived as more of an achievable goal than becoming a psychologist. The first goal, then, should be to interest more minorities in the field of psychology, then get them into the field at ground level. Being a graduate student in the field of psychology is surely ground level. Essentially, the incorporation of minority graduate students into the recruitment process will make the short-term goal of becoming a graduate student more realistic. This will then set the stage for the long-term goal of becoming a psychologist. Second, the generation gap in some instances and the cultural gap in most instances between minority graduate students and minority students under recruitment is considerably less. This increased sense of similarity and familiarity between minority graduate students and minority students under recruitment may make the transfer of information as to the benefits of a career in psychology more receivable. Third, as a result of the increased sense of similarity and familiarity, graduate students may serve as more compatible role models and mentors.

Caraway (2001) made a rather interesting observation. He suggested that walking through the halls of an AABT conference, the faces more or less look the same. This is an observation that I definitely agree with and it is surely an observation that is not specific to AABT conferences. This lack of diversity in the field of psychology warrants immediate attention from the field as a whole and especially from young ethnic minorities within the field. It goes without saying that the process of efficiently increasing

diversity in psychology as a whole will require a great deal of energy and effort. Younger generations of minority psychologists in training need to provide this energy and effort and provide it early. Maybe then we can come to a day in which faces at an AABT conference do not resemble one another. Now wouldn't that be nice!

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A Modified Computer Version of the Paced Auditory Serial Addition Task (PASAT) as a Laboratory-Based Stressor

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To simulate psychological stress in laboratory studies of psychopathology, researchers have used a variety of stressful tasks that have included exposure to challenging problems (e.g., mental arithmetic, puzzles; Pike, Smith, Hauger, & Nicassio, 1997; Sharpley & Gordon, 1999; Sharpley, Power, Mollard, & Parsons, 1993), time pressure (e.g., reaction time; Light & Sherwood, 1989; Schneider, Julius, & Karunas, 1989; Sharpley & Gordon), physical exercise (e.g., deep knee bends, hand grip; Emmons, Weidner, & Collins, 1989; Schneider et al., 1989), aversive stimuli (e.g., cold pressor task, electric shock, upsetting video footage, loud auditory noise; Ader & Tatum, 1961, 1963; Allen & Crowell, 1989; Butler, Wells, & Dewick, 1995; Lejuez, O'Donnell, Wirth, Zvolensky, & Eifert, 1998; Van Gemmert & Van Galen, 1997; Weisse et al., 1990), and/or social performance demands (e.g., presentation of a speech, social interaction with strangers; Breslin & Wilson, 1992; Stoney & Hughes, 1999). Within this large body of research using stressors, however, a lack of consistency in the procedural details of these studies necessarily limits across-study comparisons. Further, whereas most tasks have particular strengths, few allow for the flexibility to comprehensively examine responses across behavioral/motor, cognitive/self-report, and physiological response modes in an experimentally precise manner. To address

this need, we present a modified version of the Paced Auditory Serial Addition Task (PASAT), referred to here as the PASAT-C.

PASAT

The PASAT, originally developed for the assessment of information processing and capacity in patients with head trauma (Gronwall, 1977), is widely used as a neuropsychological test that indexes sustained attention (Cohen, 1993), divided attention (van Zomeran & Brouwer, 1994), information processing (Gronwall; Gronwall & Wrightson, 1981), and mental tracking (Lezak, 1995). The task involves the participant adding a verbally presented digit to the previous verbally presented digit. After verbally answering with the sum, the participant must then ignore that sum and add the following digit to the previous digit. For example, the correct answers to the series of 7, 8, 4, 5, 1 would be 15, 12, 9, and 6.

Despite evidence for the utility of the PASAT as a neuropsychological assessment device (Deary, Langan, Hepburn, & Frier, 1991; Gronwall & Wrightson, 1981), the task also has been criticized for these purposes because it produces elevated levels of stress (Deary et al., 1994; Holdwick & Wingenfeld, 1999; Lezak, 1995; Roman, Edwall, Buchanan, & Patton, 1991). Although the production of stress may be undesirable when the primary goal is the assessment of neuropsychological functioning, this feature also suggests the potential utility of the task as a laboratory inducer of psychological stress. Given these findings, we developed the PASAT-C to serve as a laboratory inducer of psychological stress. In this context, we have utilized particular modifications to exacerbate reactions (e.g., unpleasant auditory feedback for incorrect answers) and to increase precision and control (e.g., computerized execution of exact latencies between number presentation, as well as computerized determination of participant response accuracy and latency). Although these modifications likely limit any reliable neuropsychological

assessment, we present this modified version of the PASAT as an experimentally rigorous measure capable of precise assessment of behavior across behavioral/motor, cognitive/self-report, and physiological response modes.

Description of the Modified Task

The PASAT-C (see Figure 1) is a modified computer version of the standard PASAT used to assess neuropsychological functioning. The version described below is that used most frequently in our laboratory, yet most of the parameters can be modified (e.g., number of levels, latency of stimulus presentation) to better suit particular experimental needs. The basics of the task are taken from the standard PASAT, with a few exceptions. First, whereas stimuli in the standard version are presented orally, the stimuli on the modified task are presented in a 70-point bold font within a 7.62 cm (w) × 3.81 cm (h) rectangle on the upper middle area of the computer screen. Also, unlike the standard version in which participant responses are provided orally, the modified version requires answers to be provided via a computer mouse click on a keypad (including the numbers 1 through 20) pictured in the lower middle area of the computer screen. The numbers are presented in an 18-point bold font within a 1.25 cm (w) × 1.9 cm (h) box (1 through 10 on the top row and 11 through 20 on the bottom row). Each box is separated by .5 cm. Finally, the participant's score, presented in 32-point bold font, is continuously updated within a 3.2 cm (w) × 1.9 cm (h) box.

The version of the PASAT-C described here consists of three levels. As shown in Table 1, Level 1 provides a 3-s latency between number presentations (i.e., low difficulty). Level 2 provides a 1.5-s latency between number presentations (i.e., medium difficulty). Level 3 provides a 1-s latency between number presentations (i.e., high difficulty). Level 1 continues for 3 min and then transitions without warning to Level 2, which lasts for 5 min. The seamless transition is especially useful if the experimenters are interested in sensitivity to subtle experimental manipulations. A 2-min break separates Level 2 and 3, during which self-report ratings and other desired assessment data may be collected.

Immediately prior to the start of Level 3, a 15-s warning period is used to alert the participant that the session will soon resume. Specifically, the screen displays the message "get ready" for the first 5 s, "get set" for the second 5 s, and "go" for the final 5 s. Following the warning period, Level 3 lasts up to 10 min, with the participant given an explicit "escape" option. Participants are informed about

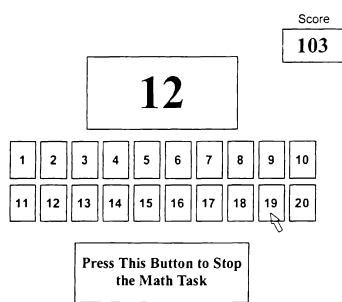


Fig. 1. The screen presentation for the computerized version of the Paced Auditory Serial Addition Task (PASAT).

this escape option prior to the start of the task. Specifically, they are told that at some point in the experiment, a box labeled QUIT will appear below their answer keypad and that clicking the mouse anywhere on this box will terminate exposure to the task. The dimensions of the box are 9.5 cm (w) × 3.2 cm (h). To provide incentive to complete the task to the best of their abilities, participants also are told that they will receive a gift certificate if their total number of points earned is greater than the average score of the other participants. It is further explained that this “target” score cannot be determined until the end of participation, but in the event that the participant’s score exceeds the target score, the gift certificate will be sent in the mail about 4 weeks after participation. We used a small amount (i.e., \$5) to produce some incentive for continuing the task without creating a ceiling effect in the duration of endurance across participants.

Data Supporting the Utility of the Task

To be an effective psychological stressor, it is necessary that the task be shown to produce effects across motor/behavioral, cognitive/self-report, and physiological response channels (Lang, 1971). In pilot data from our laboratory we exposed 32 individuals to the PASAT-C. Of the participants, 87% were Caucasian, 50% were male. These participants averaged 13.3 years of education ($SD = 2.0$) and the mean age was 44.3 years ($SD = 9.6$).

Cognitive/self-report. The use of self-report measures built into the PASAT-C allows for the on-line assessment of subjective effects produced by the task. Although any combination of variables can be examined, we have focused on the assessment of variables thought to be indicative of psychological stress (i.e., anxiety, difficulty concentrating, and irritability). As a validity check, we also measured the effects of the task upon self-reported bodily discomfort, a variable unlikely to be affected by the task. Self-report assessments were taken at baseline and again following the completion of Level 2. Level 2 was chosen over Level 1 and Level 3 because we were concerned about the limited degree of difficulty in Level 1 and the differences in duration across participants in Level 3 due to the termination option.

Self-report of anxiety, difficulty concentrating, bodily discomfort, and irritability were completed on the computer via a 0 (*none*) to 100 (*extreme*) visual analog scale with a mouse-manipulated marker. Using repeated-measures Analyses of Variance (ANOVAs) to compare reports at baseline and at the end of Level 2, anxiety, $F(1, 31) = 10.51, p = .003$, difficulty concentrating, $F(1, 31) = 26.5, p < .001$, and irri-

tability, $F(1, 31) = 18.82, p < .001$, all increased significantly. Additionally, self-report of bodily discomfort, a measure unlikely to be affected by the PASAT-C, did not significantly increase from the baseline to the experimental period, $F(1, 31) = 2.52, p > .10$.

Physiological arousal. The design of the PASAT-C allows for the assessment of overall physiological responding. Additionally, the use of transition and rest periods allows for more precise analyses of habituation within each level (i.e., a systematic decrease in arousal as the novelty of the task decreases) and potentiation across levels (i.e., a spike in arousal during the transition from one level to the next), as well as anticipatory effects during the clearly defined warning period that signals the impending start of Level 3. Further, the additional baseline measurement provided during the rest period between Level 2 and Level 3 allows for more accurate change scores for Level 3 responses than that available using the preexperimental baseline assessment.

To address physiological responding we used a Biopac MP 100 system to digitally record skin conductance level (SCL) and heart rate (HR) data on-line at a sample rate of 10 samples/s across all channels using Biopac’s Acqknowledge Software. SCL (in microsiemens) was obtained using the Biopac GSR100B electrodermal activity amplifier with the TSD103A Ag-AgCl electrodes placed on the middle segment of the middle and ring fingers. Raw electrocardiogram data were collected using the Biopac ECG100B Electrocardiogram amplifier, with disposable Ag/AgCl electrodes aligned in a standard configuration (right and left of sternum just below the clavicle). These raw data were converted to obtain HR in beats per min.

The PASAT-C effectively increased participants’ physiological arousal, with the most robust effects evidence in SCL (see Figure 2). Compared to baseline values, SCL increased during the first 10 s of Level 1, $F(1, 31) = 16.1, p = .0004$, Level 2, $F(1, 31) = 20.6, p < .0001$, and Level 3, $F(1, 31) = 32.7, p < .0001$. Further, these effects remained throughout both Levels 1 and 2, and actually a slight increase in SCL was evidenced by the last 10 s of each these levels. Finally, anticipatory effects also were evident. Specifically, SCL significantly increased from the baseline period compared to the 15-s warning period immediately prior to Level 3, $F(1, 31) = 31.6, p < .0001$.

Regarding HR, the pattern of data was somewhat consistent with that found for

SCL, yet considerably less robust.¹ Specifically, a significant increase from baseline levels was found at the first 10 s of Level 1, $F(1, 31) = 10.0, p = .004$, but not at Level 2 or 3. Additionally, anticipatory effects were found; HR significantly increased from the baseline period compared to the 15-s warning period immediately prior to Level 3, $F(1, 31) = 4.32, p = .047$.

Motor/behavioral. On a motor/behavioral level, there are several useful dependent measures to assess both performance and persistence on the PASAT-C. We examined task performance as a function of number of correct responses. As expected, performance decreased as the number presentation latency decreased. During Level 1, scores ranged from 1 to 57 ($M = 21.4; SD = 12.14$) out of a possible 59 (36% correct). Performance dropped considerably in Level 2 with scores ranging from 0 to 103 ($M = 21.93; SD = 21.4$) out of a possible 199 (11% correct). Level 3 scores ranged from 0 to 87 ($M = 19.56; SD = 22.79$). The variation in termination durations limited the utility of comparing these scores to an absolute total score for the entire 600 s, but adjusting the total score based upon the average termination latency resulted in an average maximum score of 332 (6% correct).

Several factors should be considered when interpreting exactly what is being measured by performance. Based upon data from the standard version of the PASAT and the simplicity of the computations, it is unlikely that intelligence or mathematical ability were affecting score on the PASAT-C (Deary et al., 1991). In contrast, attention and reaction time are factors that may be especially relevant because of the established attentional component of the standard PASAT (Deary

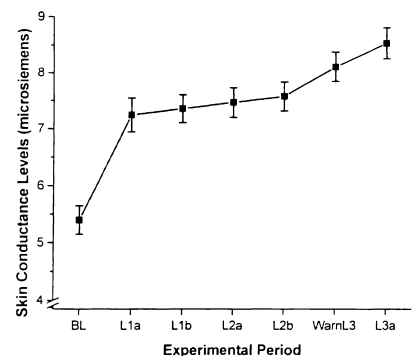


Fig. 2. The pattern of Skin Conductance Level (in microsiemens) changes across various experimental periods of the computerized version of the Paced Auditory Serial Addition Task (PASAT). X-axis labels are abbreviated as follows. BL = preexperimental baseline; L1a = first 10 s of Level 1; L1b = last 10 s of Level 1; L2a = first 10 s of Level 2; L2b = last 10 s of Level 2; WarnL3 = the 15 s warning period prior to Level 3; L3a = first 10 s of Level 3. The vertical bar through each symbol represents standard error of the mean.

¹The less robust HR findings are not surprising given that many researchers have questioned the use of HR changes as an index of anxiety and psychological stress (Fowles, 1983).

et al.; Gronwall & Wrightson, 1981). To reduce the effect of any mouse skill-related effects, a touch screen could be used instead of a mouse. However, it should be acknowledged that the psychological stress produced by the task might be reduced in the absence of the extra motor requirement of the mouse, even if number presentation latencies are reduced to accommodate the less complex response. This caution is especially true when utilizing a slower mouse speed, which often serves the effect of preventing participants from answering quickly enough to get credit despite their clear knowledge of the correct answer.

In addition to performance, the PASAT-C allows for the assessment of persistence/frustration tolerance, indexed by latency to terminate the PASAT. In our sample, we found a wide distribution of termination latencies. Specifically, the average termination time was 354.9 ($SD = 245.8$) out of a possible 600 s, with 8 (25%) participants terminating the task in the first 100 s and 12 (37.5%) participants completing the entire 600 s. Because within most samples a subset of the participants complete the entire 600 s, the distribution of scores is unlikely to be normal and thereby requires a statistical transformation or the use of statistics for which normalcy is not a requirement. Alternatively, participants could be grouped by the presence or absence of a termination response as opposed to the duration of termination latency, with the resulting data analyzed using a chi-square analysis. As a fourth option, we chose to analyze our data using continuous time survival analysis as implemented in SAS using PROC PHREG. This method allows for analysis of participants who never experience an index event (in this case-task termination) during a given period of time.

Results of survival analyses indicated that age, education, gender, and a self-reported index of familiarity with a computer mouse were not related to termination latency, $ps < .30$. However, performance on the PASAT, as indexed by score on the first two levels, did predict termination latency, with higher scores being associated with lower relative risk of task termination across the 10-min trial (Relative Risk [RR] = .95, $p < .018$). This relation raises the possibility that participants' proficiency at the task (potentially including attention level and motor speed), and not other factors related to persistence, may have determined termination latency.

Although the potential confound of performance and persistence appears problematic, features of the current data suggest that these variables may be acting at least somewhat independently. Indeed,

it should also be noted that despite the association between performance and termination latency, neither self-report ratings at baseline nor following Level 2 were correlated with performance, whereas relations between these reports and termination latencies were found. Specifically, controlling for performance, higher self-reported irritability at baseline ($RR = 1.03$; $p = .021$) and after Level 2 ($RR = 1.02$; $p = .011$) was associated with relatively greater risk of task termination during Level 3. Also, difficulty concentrating after Level 2 was associated with greater risk of task termination ($RR = 1.02$; $p < .018$). Difficulty concentrating at baseline as well as anxiety and bodily discomfort at both baseline and Level 2 were not significantly associated with survival to task termination, $ps > .05$. These results suggest that cognitive and affective processes, unrelated to performance, may influence termination latencies. In particular, the relation between termination latencies, as well as both baseline and within-session levels of irritability, suggests that a construct such as frustration tolerance may be of interest.

Nevertheless, the link between performance and persistence makes intuitive sense and always should be considered when interpreting data using this task. Along these lines, one advantage of the current task compared with other similar tasks in which performance is not assessed is that the effect of performance on termination latencies can be addressed statistically. That is, when using the PASAT-C to assess between-group differences, score can be used as a covariate to determine if group differences in termination latency exist independent of performance.

Conclusions

In conclusion, we present the PASAT-C as a tool that may be used to produce psychological stress in laboratory examinations of experimental psychopathology. Most importantly, this task allows for the comprehensive examination of behavioral/motor, cognitive/self-report, and physiological response modes without sacrificing experimental precision and control. Further, the procedural details can be easily manipulated to best suit the particular research question.

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TABLE 1. Procedural Details for the PASAT-C

Level	Duration	Presentation Latency	Possible Correct	Preceded by	Followed by
Level 1	3 min	3.0 s	59	10-min baseline	Seamless transition to Level 2
Level 2	5 min	1.5 s	199	Seamless transition from Level 1	2-min assessment period 15-s warning period
Level 3	10 min	1.0 s	599	2-min assessment period 15 s warning period	End of task

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So You Call Yourself a Doctor?

Frank M. Dattilio, *Harvard Medical School*

You've heard it a thousand times: Kids say the darnedest things. I guess it's that sweet innocence that relinquishes them from the constraints that hold most of us back from saying what we really think.

I recall many years ago, my wife and I were hosting a dinner party at our home when one of our guests, a colleague of mine, asked my son Michael, then age 7, "So, what do you want to be when you grow up?" Michael replied sheepishly, "Eh, a doctor, I guess." "Oh, like your dad?" my colleague offered. "Nah," my son said, "I want to be a real doctor." Needless to say, everyone in the room roared with laughter. Needless to say, little Mikey saw an early bedtime that evening. Well, lucky for him he had no trouble falling asleep, because I certainly tossed and turned that night. I don't know what it was that irked me most about my son's comment, but maybe it was that "out of the mouths of babes" thing, and this was out of "my babe." I was never one to take my education lightly; but, on the other hand, I never corrected anyone who referred to me as Mister, even in such formal circumstances as a lecture or in court. I always preferred a first-name basis with both my students and my patients, and I certainly avoided making any comments that might be construed as condescending. But my son's statement ate at me, probably because it wasn't the first time I had heard it. So I got to thinking about what actually constitutes a doctor, why some doctors are doctors according to most everyone, no question; and why others are, well, a matter of opinion.

Many believe that "doctor" should be reserved exclusively for the physician, the one and true healer. Yet, there are many double messages as far as usage. Some nonphysician professionals are referred to as "doctor" in certain contexts, but not in others. For example, during a deposition or when testifying in a court of law, the judge, attorneys, tip staff, and even the shoeshine man in the courthouse address me respectively as Doctor, yet the newspapers tell me that they only use the title when the individual is a physician. That is, of course, unless you die. The dead person with a doctoral degree is a "Doctor

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Person" on the obituary page, even if simply a "Mr. Person" in life. A "posthumous courtesy" I guess.

Merriam-Webster's dictionary defines the term doctor as "a person qualified to practice medicine, Dr., the title of a medical practitioner, the title of a holder of the highest academic degrees (as a Ph.D.) conferred by a university, 2. v.t. to give first aid to/to adulterate, doctor drink/so as to improve/to alter so as to falsify, 3. A person who restores or repairs things." If you check some other dictionaries, doctor is defined as "teacher or healer."

The World Book Encyclopedia defines doctor as "a degree awarded to a person by a college or university." Physicians have the Doctor of Medicine (M.D. or D.O.). Many scientists and teachers have the Doctor of Philosophy (Ph.D.) degree. Other professionals also hold doctoral degrees, such as a Doctor of Science (D.Sc.).

In some countries, the word doctor is used quite loosely. For example, in parts of South America, if a professional is highly respected, then he or she may be referred to as "doctor," even though he or she may not possess a formal doctoral degree of any kind. That is interesting because, in this country, even those who do possess a doctoral degree don't always get the title either socially or professionally. Such is the case of most attorneys who hold a juris doctorate. While technically a doctor, these attorneys are rarely referred to as such in this country. In Brazil, however, they are. In some countries in Europe, particularly in Eastern Europe, where physicians are regarded with less esteem, they are referred to as simply Mr. or Ms. or sometimes *Professore*. China, on the other hand, holds Ph.D. degrees in much higher esteem than the M.D.s., referring to them by rote as "doctor."

After my dictionary research, just for the fun of it, I decided to look up the name Doctor in my local phone book and, sure enough, there was one individual with this unusual last name. I called him and found out that he wasn't a real doctor either, but a retired plumber who had inherited the name Doctor from his father who was also a plumber. When I asked him how people addressed him, he replied, "Just Doc."

I even found somebody in the APA directory with the last name of Doctor. He had a Ph.D. in psychology. I called him to ask how his patients and colleagues addressed him professionally and he said, "Why, Doctor of course." "Dr. Doctor or Dr. Ron?" I chided. "Hey," he replied, "it works for me."

So back to the issue of what constitutes a real doctor. I remember I went back and asked my son the morning after the dinner party what he meant by, "I want to be a 'real doctor'." He answered, "I want to be

like Dr. Toff." "Oh," I said, "So I guess that's what a real doctor is, one who practices medicine like your pediatrician?" I caught him rolling his eyes, but he responded, "I don't know, it's confusing, Dad—leave me alone. There are just different kinds of doctors. Like fake doctors, they're not really doctors." After that conversation, I was even more bewildered.

My next step was to ask people on the street at random what they thought. Half of them told me that doctors were physicians; others told me that some doctors were physicians, and still others were just doctors in different areas—like people who teach at a university, they're doctors too. One guy told me that if I gave him a dollar, he'd be happy to call me anything I wanted.

A colleague who teaches at a medical school once told me that when the department chair would issue memos, he used "Dr." only before the names of those who were physicians. The Ph.D.s were simply addressed by name. When my colleague inquired as to why, the chair of the department, who was an M.D., responded outright, "Only physicians deserve the title Doctor."

And what about all those other doctors? Podiatrists with a D.P.M., chiropractors with a D.C., nurses who hold a D.Sc., and ministers with a D.Min. who are referred to as "Very Reverend Doctor"—are they really doctors? Not to mention those who hold honorary doctorates . . . what is the appropriate title for them?

In a telephone interview with John Solomon of the Associated Press in Washington, DC, he informed me that the AP follows the guidelines set forth by their "style bible," which directs all journalists to adhere to these rules:

doctor Use Dr. in first reference as a formal title before the name of an individual who holds a doctor of dental surgery, doctor of medicine, doctor of osteopathy, or doctor of podiatric medicine degree: Dr. Jonas Salk

The form Dr., or Drs., in a plural construction, applies to all first-reference uses before a name, including direct quotations.

If appropriate in the context, Dr. also may be used on first reference before the names of individuals who hold other types of doctoral degrees. However, because the public frequently identifies Dr. only with physicians, care should be taken to assure that the individual's specialty is stated in first or second reference. The only exception would be a story in which the context left no doubt that the person was a dentist, psychologist, chemist, historian, etc.

In some instances it also is necessary to specify that an individual identified as Dr. is a physician. One frequent case is a story reporting on joint research by physicians, biologists, etc.

Do not use Dr. before the names of individuals who hold only honorary doctorates.

Do not continue the use of Dr. in subsequent references. (Goldstein, 1996).


So let's recap: If the person in a news story holds a doctoral degree, "Dr." should be followed by the type of doctorate he or she holds so that the public does not become confused as to the person's profession. For example, Dr. Jones, a physician, or Dr. Smith, a clinical psychologist, etc. Okay, so what of my friend Dr. Doctor, who is not really a doctor because he has a Ph.D. and not an M.D.? He might give that well-worn style bible a reason for revision.

As you can see, my research didn't get me very far. What's in a title anyway? you may ask. Is it so important to be called Doctor? I guess that the issue at hand is really one of respect. But in my opinion, respect should not be accorded based on one's credentials. So, lately, if someone asks, "Are you a doctor?" I reply, "No, I'm not dead yet!"

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Saul D. Raw, *Weill Medical College of Cornell University*

Comparative Study of Theoretical Orientations and Employment Settings for Clinical and Counseling Psychologists

A study published in *The Clinical Psychologist* (Bechtold et al., 2001) compares theoretical orientations and employment settings of clinical and counseling psychologists who are members of the American Psychological Association's Divisions 12 and 17, respectively. Questionnaires were mailed to 6,000 randomly selected members of these divisions and returns were received from 1,389 psychologists, comprising a 23% response rate. For both divisions, 29% of the psychologists endorsed the eclectic/integrative orientation while 26% endorsed the cognitive orientation. Division 12 (Clinical) members more frequently favored the behavioral tradition while Division 17 (Counseling) members more frequently embraced humanistic-existential theories. Private practice and university settings were the employment venues for 60% of each division, although clinical psychologists were more frequently employed in private practice and hospital settings whereas counseling psychologists were more frequently employed in universities and other settings.

Mental Illness as a Workplace Cost and Legal Protections for Workers With Psychiatric Disabilities

A series of pre-September 11 articles in *The Wall Street Journal* (Tanouye, 2001a, 2001b, 2001c) highlights some of the difficulties and costs engendered by mental illness in the workplace. Mental illness in the workplace is said to take a toll as high as \$70 billion per year, including medical costs and lost workplace productivity. In one survey, conducted in 2000 by Watson Wyatt Worldwide, a consulting firm, and Washington Business Group on Health, an employer group, 70% of large employers indicated a concern with rising costs for psychiatric claims. Companies reported to view psychiatric claims as costs to be minimized.

Bank One Corp, a banking and credit card company based in Chicago, did a comprehensive analysis of employee health data in the mid-1980s to examine factors accounting for rising health costs, and found that treating depressive disorders cost the company's self-insured plan \$931,000 in 1991, almost as much as the

\$1.2 million cost to treat heart disease. The actual costs may have been closer, but the reimbursement rates for psychiatric disorders are lower than for other medical claims.

The article notes that the indirect costs, such as lost productivity and absenteeism, were even more pronounced. Depressed employees had longer short-term disability leaves than employees with other "common" illnesses and were said to suffer from high relapse rates. Depressive illness led to 10,859 lost workdays for Bank One employees over a 2-year period compared to 947 lost days for high blood pressure and 795 days for diabetes. The article notes that the high proportion of female employees at Bank One could account for some of these findings, given higher rates for depression in women in the general population.

In evaluating employee access to appropriate mental health treatment, the bank determined that many employees were not receiving adequate treatment either from the self-insured plan or from the several HMOs that it offered. The company responded by reducing employee out-of-pocket costs for the first 12 visits in its self-insured plan and stressing early intervention in an effort to avoid the high costs of hospitalization. Only 30% of employees were in the self-insured plans and the HMO care was reportedly "disappointing," despite pressure from the company for better treatment. HMO-treated patients were said to frequently receive medication and short-term therapy and, although they tended to return to work more quickly, suffered from high rates of relapse. Bank One's efforts to aggressively treat depression led in part to unexpectedly high numbers of employees taking disability leaves for this disorder. The 7.2 per 1,000 figure in 1999 was said to be four times the 1989 rate.

The article notes that it is ultimately difficult to know if that rate is "abnormally" high because most businesses do not truly know what drives their disability costs. Bank One's awareness of the problem of depression may well have contributed to the jump in disability leave. The company's awareness programs encouraged employees to seek treatment and, in some cases, to finally seek disability leave.

The Wall Street Journal article noted that, under the Americans with Disabilities Act, employees and employers have certain rights and responsibilities

(<http://www.usdoj.gov/crt/ada/adahom1.htm>). The ADA was designed to protect the rights and interests of workers and citizens with a whole range of disabilities: Employees with psychiatric disabilities qualify for protection if their disability substantially limits a major life activity; they must disclose their disability to their employer in order to be eligible for protection under the law; they can request reasonable accommodations from their employer to aid in their adapting to the work environment; and they are eligible to file a lawsuit or charges with state and federal agencies if they feel as if their rights have been violated. Employers, under the law, are not permitted to ask a job applicant about any psychiatric disabilities before making a job offer. Employers can require a pre-employment medical exam or inquiry after making a job offer, if such is required of all employees. They can require that an employee seeking accommodations provide medical documentation of disability. Finally, employers must keep all information about an employee's psychiatric condition or history confidential. This information must be kept separately from personnel records.

Popular Book Cited as Detrimental to Improving Relationships

Men Are From Mars, Women Are From Venus, the popular self-help book by John Grey, which has sold millions of copies worldwide, was cited in an article in the *Journal of Marital and Family Therapy* (Zimmerman, Haddock, & McGeorge, 2001) as running counter to "best practices" in family therapy. The study, which utilized a thematic analysis and feminist critique, categorized Grey's work into major themes and assumptions.

According to the study, Grey's work is said to promote gender differences in communications, stress response, and desire for intimacy. Grey's central thesis is that men and women are "extremely" different and that men and women must accept these differences to foster successful relationships.

The authors maintain that Grey's recommendations encourage power differentials between men and women and run "counter to a growing body of research that underscores the importance of shared power for achieving an intimate and effective relationship."

Will Physicians in California Leave the State en Masse?

The Wall Street Journal (Rundle, 2001) reports on an admittedly unrepresentative pole of California physicians in which half of 2,300 survey respondents stated that they planned to quit, retire, or move out of

the state within the next 3 years. The survey, published by the California Medical Association, is entitled, "And Then There Were None: The Coming Physician Supply Problem." The implication is that physician frustration with "low pay and . . . managed care" was in danger of leaving California with an undersupply of physicians.

California Medical Association President Frank E. Staggers stated, "We need to look at ways to ward off a potential exodus of physicians from this state." The California Medical Board noted that the number of physicians licensed and practicing in California had actually climbed slightly in recent years.

The survey was released in the midst of an ongoing struggle between the physicians and the managed care industry over a proposed law that would increase the bargaining power of physicians within the state by exempting them from antitrust laws. They would, under the proposed bill, be eligible to jointly negotiate fees and other contract items. The bill already passed the state assembly and was awaiting a vote in the state senate.

Bill Wehrle, chief lobbyist for the California Association of Health Plans, questioned the timing of the release of the survey, but a California Medical Association spokesperson stated that this was just a coincidence and that the study had been in preparation since February.

California has been noted to set standards for managed care legislation, and any decisions there will be closely watched by the health care industry and provider groups. Similar legislation to exempt health care providers from antitrust statutes has been introduced in other states and also on the federal level.

Study Charges Clinical Guides Often Hide Doctors' Ties to the Pharmaceutical Industry

The New York Times (Stolberg, 2002), citing a survey of experts who prepare guidelines for the treatment of medical and psychiatric disorders, found that nearly 90% of them have ties to the pharmaceutical industry, and that the ties are almost never disclosed. Although it has long been known that pharmaceutical industry ties can influence prescribing patterns and the course of medical research, a small study conducted by the University of Toronto is the first to "document the extent to which the industry may influence so-called clinical practice guidelines." The article notes that such guidelines, which are typically published in medical journals, "set standards that are followed by countless doctors."

The survey, published in the *Journal of the American Medical Association*, ques-

tioned 192 medical experts who had been involved in the writing of 44 sets of practice guidelines, including those for asthma, depression, diabetes, hypertension, and other disorders. Out of the 100 who responded, 87% had some kind of link with the pharmaceutical industry, including research support, speaking, travel, and consulting fees. Fifty-nine percent had relationships with the drug companies whose products were considered in the guidelines that they authored. Ninety-six percent of these relationships predated the creation of the particular guidelines.

Eleven of the 44 practice guidelines were underwritten by the pharmaceutical industry, and this was so stated in the guideline presentation, but out of the 44 guidelines considered, only 1 reported a potential conflict of interest. Interestingly, only 7% of the doctors in the study believed that their ties to the industry influenced their conclusions, but 19% believed that the recommendations of their colleagues had been influenced by industry ties.

Opinions about these findings varied, with some stating that the industry helps to educate doctors and others stating that, at the least, complete transparency and disclosure were called for.

Mandatory New York Debriefing Called "A Waste of Time"

A news item appearing in the on-line edition of the *British Medical Journal* cites an article by Professor Peretz Lavie, director of the sleep laboratory at the Technion-Israel Institute of Technology in Haifa, Israel. In the article, Professor Lavie criticizes the plan for mandatory debriefing of 55,000 police and firefighters who participated in the aftermath of the attacks on the World Trade Center.

According to Lavie, such debriefing will not prevent anyone from developing PTSD and notes that people who have endured serious psychological trauma "may not recover faster if forced to relive their memories." Lavie also takes issue with the conventional view that trauma victims suffer from insomnia. He believes that these people actually lose less sleep than they think.

Lavie stated that "trauma patients who claim they can't sleep often confuse their fear of going to sleep with an inability to fall asleep." His conclusions were said to be based on "decades" of study of trauma victims, including Holocaust survivors, and survivors of terrorism and missile attacks.

Professor Lavie recommends behavioral therapies, including progressive muscle relaxation, stimulus control, sleep restriction, and, if needed, sleeping pills rather than debriefing to treat these patients.

Dr. Danny Brom, director of the Israel Center for the Treatment of Psychotrauma in Jerusalem, commented that "it is becoming increasingly clear that (debriefing) is not an effective treatment for trauma or for preventing the onset of posttraumatic stress disorder, although it is helpful in identifying people at high risk for the disorder."

CIGNA Cut Reversed after National Association of Social Workers (NASW) Intervention

NASW News (Vallianatos, 2002) reports that CIGNA Behavioral Health, one of the large national managed care companies, reversed a decision to lower reimbursement rates to social workers after a meeting with the Massachusetts NASW chapter.

The chapter also objected to what it termed "micromanagement" of outpatient mental health benefits. CIGNA had reduced its initial authorization of six outpatient visits to four but agreed to restore the initial authorization to six visits after the meeting. The chapter also got CIGNA to agree to family visits without a child client having to be present.

Chapter Clinical Issues Director Carol Trust also noted that many had difficulties with CIGNA's reimbursement practices and noted that the chapter was still working with CIGNA on several outstanding issues. In a blunt letter to CIGNA before the negotiations, the Massachusetts chapter noted, "So, first CIGNA deviates from industry standards with a micromanaging style that just serves to alienate the clinicians it says it wants to partner with. Then it rewards the most cost-effective clinicians by lowering their rate to the lowest in the industry."

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Book Review

Connors, G. J., Donovan, D. M., & DiClemente, C. C. (2001). *Substance abuse and the stages of change: Selecting and planning interventions*. New York: The Guilford Press.

Reviewed by Peter Vik and Tony Cellucci, *Idaho State University*

Most comprehensive approaches to substance abuse treatment now recognize the importance of considering clients' process and stages of change. Prochaska and DiClemente (1983; Prochaska, DiClemente, & Norcross, 1992) introduced a model to describe a client's readiness to change problem behavior. This approach to change has inspired motivational treatment approaches intended to elicit commitment and ultimately active effort to change a problem behavior (e.g., Miller & Rollnick, 2002). Now, Connors, Donovan, and DiClemente (2001) have authored *Substance Abuse Treatment and the Stages of Change*, a succinct yet comprehensive presentation of the change model and its implications for therapy. The authors target key elements of substance abuse treatment and then demonstrate how the stage-of-change model enhances each element. The book begins with a review of the model and its empirical support. In the heart of the book, the authors demonstrate therapeutic applications of the model, including assessment, treatment planning, and individual, group, couples, and family therapies. They round out the volume with chapters on special populations, relapse, and topics for future study.

The first part of the book establishes a foundation for the subsequent therapeutic application of the stages-of-change model. Chapter 1 provides a quick and clear introduction to the model that is suitable for those who are unfamiliar with the concept of stages of change. Details of the change

model are presented in chapter 2. For each stage, the authors provide a detailed description, an accompanying table listing characteristics of that stage, and a case example. Chapter 3 transitions from theoretical discussion to practical application by introducing the role and function of the stages of change when assessing substance-using clients. The comprehensive scope of this chapter is a useful introduction to substance abuse assessment for students and beginning practitioners, and a convenient reference to the broad array of assessment approaches, tools, and resources for seasoned therapists. As with all chapters in the book, bulleted summaries highlight the key points of the chapter, providing readers with a quick and useful checklist of important ideas.

The heart of the text (chapters 4 through 7) integrates the model into therapy. Attention is given to treatment planning and individual, group, couple, and family treatment approaches. Chapter 4 is one of the strongest in terms of clinical utility and training. It provides a thorough description of the treatment planning process and steps to construct an individualized treatment plan. In keeping with the theme of the book, the authors use the stage-of-change model to formulate goals that the patient can embrace and achieve. The authors also summarize motivational enhancement and illustrate specific plans for two cases. (We recommend this chapter to our practicum students for instruction in writing functional treatment plans for their substance abuse patients.)

In the chapter on individual treatment (chapter 5), the authors emphasize the assessment of stage status, discuss conceptual and central tasks of each stage, and suggest intervention strategies. Stage of change is a dynamic process. Shifts and transitions within and between stages are as important to monitor as a patient's urges and current substance use behaviors. The change process is unique to each patient; nevertheless, a general pathway toward positive change can be described. Patients generally move from pre-action (characterized by doubt about the problem, contemplation, and decision) to action (develop and commit to an action plan), to finally solidify life changes (maintenance). Connors et al. reveal their clinical acumen as they discuss the need to strengthen commitment in the early stages of change when patients experience loss and discomfort, then they remind clinicians to attend to referral needs during maintenance and stabilization. Observations such as these make reading the text worthwhile, even for experienced therapists. Perhaps the most significant point for new trainees is this broader realization: Often, treatment contact with persons abusing substances is a single episode in a larger process of change. From a stage perspective, the clinician's role is to facilitate and respect the patient's journey.

Group work is a primary mode of substance abuse treatment, and chapter 6 discusses recent efforts to extend the stage model to this treatment modality. After reviewing the advantages and curative factors of group therapy, the authors describe early phase recovery groups as largely psychoeducational but providing the opportunity for personal assessment of problems, exploration of the pros and cons of continued use, and possible resources for change. Several resolution-enhancing exercises are described. Action phase groups focus on developing skills, providing training in general problem-solving, and supporting and encouraging change efforts. There is a sensitive discussion of how groups should respond to lapses, with an emphasis on building self-efficacy and maintaining optimism.

Another strength of this volume is chapter 7, which addresses the treatment of family members. The idea that family

members go through a similar change process is an intriguing way to reframe family efforts to cope. Behavioral marital therapy, behavioral contracting, and the community reinforcement approach are highlighted as effective approaches, but self-help groups are also supported for their ability to help family members reduce negative affect and improve self-esteem.

We commend Drs. Connors, Donovan, and DiClemente for writing a book that translates the stage-of-change model into practical guidance in keeping with the vicissitudes of therapeutic practice. As the authors note early in chapter 1, the stage-of-change model has evolved as research findings specific to the model have accumulated.

Consistent with criticisms of the model (e.g. Joseph, Breslin, & Skinner, 1999), we see the model describing phases in a patient's motivation as opposed to strict stages.

Evolution based on empirical findings is the final test of a good model, and there is much to learn regarding the application of this model to treatment approaches. In the final chapter, the authors review many areas for future research, including increasing and maintaining commitment to action, the effect of relapse on the change process, assessing an individual's stage, how stage transitions relate to long-term outcomes, the relationship between treatments and the process of change, and intrinsically versus extrinsically motivated change. We recommend this book for teachers and practitioners both new and seasoned.

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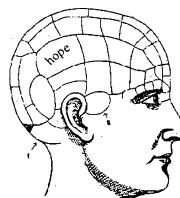
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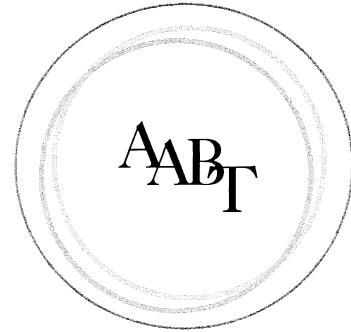
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DBT for Suicidal Clients Meeting Criteria for Borderline Personality Disorder

Marsha M. Linehan, *University of Washington, Seattle*

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Marsha Linehan, master clinician and founder of Dialectical Behavior Therapy, demonstrates techniques used to persuade clients to refrain from harmful behaviors during the course of treatment. Linehan demonstrates successful negotiating and contracting for nonsuicidal behaviors, techniques to strengthen commitment to therapy, and emphasizes ways for therapists to treat clients with borderline personality disorder as humans rather than patients.

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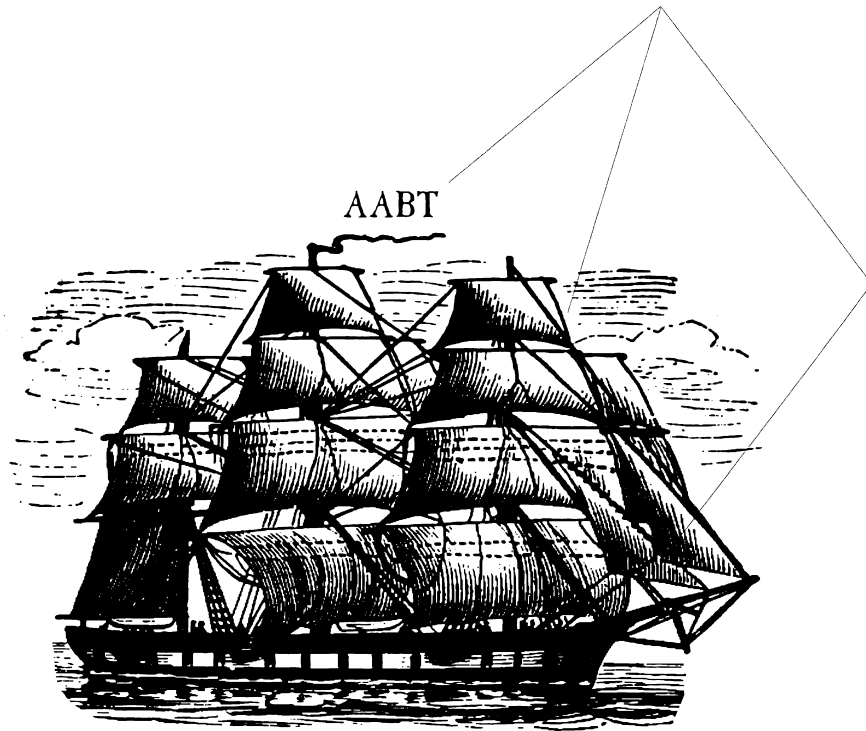
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