

the Behavior Therapist

Contents

President's Message

Robert L. Leahy

Sunk Costs: Backward-Looking Decisions • 137

Series on Technology

Michael D. Anestis and Joye C. Anestis

Using Social Media Tools in Clinical Psychology:
The Experience of Psychotherapy Brown Bag • 140

Research Forum

Bruce E. Wampold, Zac E. Imel, and Scott D. Miller

Barriers to the Dissemination of Empirically Supported
Treatments: Matching Messages to the Evidence • 144

Book Review

Reviewed by Chad E. Drake

Roemer & Orsillo (2009), *Mindfulness- and Acceptance-Based
Behavioral Therapy in Practice* • 156

Multimedia Journal

Maureen Whittal

Looking to the Future of *Cognitive and Behavioral Practice* • 157

Letter to the Editor

Patti Lou Watkins and George A. Clum

Empirically Evaluated Self-Help Therapies • 157

At ABCT

Kristene A. Doyle

To Lead or Not to Lead? • 159

Virginia Rutter

Three Recipients of the Neil S. Jacobson Research Award
for Outstanding and Innovative Clinical Research • 160

CALL for AWARD NOMINATIONS • 161

WELCOME, NEW MEMBERS • 162

President's Message

Sunk Costs: Backward-Looking Decisions

Robert L. Leahy, *American Institute for
Cognitive Therapy*



We are all familiar with the following: You pay good money for a suit or dress, take it home, look at it, and hang it in the closet. Years go by, you take it out and look at it and say, "It's not me—I'll wear something else." You can't seem to throw it out although you really know you won't wear it again. You say, "I can't throw it out. It's hardly been used. I paid good money for it." Or, more significantly, you have been stuck in a dead-end relationship that has dragged on for years. You know—"rationally"—it makes sense to get out, but you can't. Your friends urge you to look at the costs and benefits of staying versus getting out. You know rationally they are right, but you can't pull the plug. In both cases, you are "honoring" the sunk costs of prior decisions. You can't abandon the sunk cost because you believe you have to justify why you have stayed in so long. You say, "If I left it would mean I wasted all that time"; "I can't stand the feeling of loss"; "If I left it would prove I am a failure"; or "You don't understand. There really are good things there. I just have to wait for things to turn around." In fact, your reasons for staying may continue to change—because you are highly motivated to prove that you are not wrong in staying in the first place. Your decision is "backward-looking," attempting to justify what you have done in the past. Ironically, the longer you stay in, the greater the sunk cost and the greater the need to justify your decision to stay. You are making decisions looking back-

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The Association for Behavioral and Cognitive Therapies 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.
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- 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.

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Drew A. Anderson, Ph.D.
SUNY-Albany
Dept. of Psychology/SS369
1400 Washington Ave.
Albany, NY 12222

ward to past investments and not making decisions based on future utility.

Rational decision-making models argue that we make choices based on future utility, but evidence for sunk costs suggests that we are often trapped by past commitments and investments. Indeed, the greater the sunk cost, the greater the escalation of commitment. There are endless examples of sunk costs. Along with your out-of-style jacket or dress, there are sunk costs in relationships, careers, purchases, and even in foreign policy. The Vietnam War is a much-agreed-on sunk cost, but when the United States was engaged in that war a significant majority of Americans supported the war. An entire nation at times was committed to honoring sunk costs. President Johnson in fact made the bold sunk-cost justification for staying, claiming that we couldn't give up because we had lost so many men. Sunk costs are common in behavioral finance where investors double-up on a losing investment to "get their money back." We often "ride a loser."

Humans are the only animals who honor sunk costs (Arkes & Ayton, 1999). Laboratory rats may show a burst of activity as they face extinction trials when reinforcements have been eliminated, but they quickly learn to look somewhere else for rewards. Why are rats "smarter" than humans? Or are we too smart for our own good? Unlike the "rational" rat, humans appear condemned to continually reflect on their past decisions, attempting to make "sense" of them and to justify their future decisions by reference to the past. Honoring sunk costs can be explained by loss aversion (Wilson, Arvai, & Arkes, 2008), commitment theory (Kiesler, Nisbett, & Zanna, 1969), cognitive-dissonance theory (Festinger, 1957, 1961), prospect theory and loss frames (Kahneman & Tversky, 1979), fear of wasting (Arkes, 1996; Arkes & Blumer, 1985), attribution processes (for example, Jones & Davis, 1965), and inaction inertia (Gilovich & Medvec, 1994; Gilovich, Medvec, & Chen, 1995). In each case it is the absence of reward that makes this puzzling until we recognize that it is the "interpretation" of change and the "need to explain" the past that keeps us trapped.

How can we liberate ourselves from the sunk cost trap? First, standard cognitive therapy inquiries can be used, but may lead nowhere. For example, one can ask about the costs and benefits of continuing in the course of action. Patients often say, "I know it's irrational, but I can't get out." Second, educating the patient about sunk costs (the jacket in the closet) immediately helps to

make the trap more familiar (Leahy, 2000). Third, you can divide (or bifurcate) the decision: "If you had never gotten into this behavior, would you make a decision to get into it now?" Fourth, the patient can examine the justifications and challenges to these rationalizations: "I have too much invested to walk away"; "I now have a responsibility to make it work out"; or "I'm not frivolous—I don't walk away from my commitments." These assumptions may be examined utilizing cognitive therapy techniques: "What if you looked at your prior investments as *lost costs* that you can never recover? How would putting more of yourself into this help you achieve your ultimate goals?" Fifth, you can externalize the decision by asking, "What if your friends had to make the decision for you? What would they decide?" This helps *decouple the decider from the decision*. Sixth, you can identify the fear of "wasting," which often underlies the fear that walking away from the sunk cost is an admission of having wasted time and resources. This fear can be addressed by recognizing that losses (or wasting) are always involved in decision making, but the self-interest strategy would be not to throw good money after bad. Seventh, many people stay in sunk costs because of the fear of humiliation: "I would be telling everyone that I was wrong and they were right." This concern can be addressed by recognizing that most friends will be happy to have you agree with them and happy that you are out of your misery but, in the event that they use this as a reason for criticism, it may be a price worth paying to cut your losses. Eighth, some people fear the flood of negative feelings following abandoning a sunk cost. This can be addressed by an analogy of pulling a splint from a toe. It hurts until it stops hurting and then it is followed by relief. Being stuck in a sunk cost is the ultimate helplessness and is a guarantee for depression, anger, and hopelessness (Leahy, 2000, 2004).

We often get trapped by our need to justify the decisions that have continued our misery—looking backward to the past for justification, rather than committing to actions for future utility. Focusing on valued goals rather than valuing our past can help liberate us from commitments whose pay-offs have turned into deficits. Our "rationality" may be less logical than we think and more determined by rationalizing the past rather than pursuing a better future.

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Correspondence to Robert L. Leahy, Ph.D., American Institute for Cognitive Therapy, 136 E. 57th St., Suite 1101, New York, NY 10022; Leahy@CognitiveTherapyNYC.com

Using Social Media Tools in Clinical Psychology: The Experience of Psychotherapy Brown Bag

Michael D. Anestis and Joye C. Anestis, *Psychotherapy Brown Bag* and *Florida State University*

EDITOR'S NOTE: *This article marks the first of a series of articles looking at potential uses of new technologies for ABCT members. If you have any ideas or suggestions for articles, please send me an email.*
—D.A.

As a pair of advanced graduate students trained in the clinical science tradition, the struggles of disseminating empirically supported treatments (ESTs) in a world full of misinformation has often been at the forefront of our minds. In fact, we have at times been downright demoralized that all of this great research and these effective treatments exist, yet so few people know about them. We knew we wanted to do something about it, but found ourselves unsure how to make a difference. After years of brainstorming ways in which we could effectively sing the praises of ESTs to a wide audience while we are on graduate student wallets (which are slim) and schedules (which are full), we decided to stick our toes into the world of blogs (although we tend to say “online magazine”) and launched Psychotherapy Brown Bag (<http://www.psychotherapybrownbag.com>) on March 1, 2009. The central aims of PBB are to disseminate research findings in clinical psychology and information on ESTs for mental illness to the general public (both clinicians and consumers, thus ostensibly helping to bridge the research-practice gap) and to help individuals find local clinics that provide such services.

The actual creation of PBB was far simpler than we ever anticipated. In fact, the most difficult part of the process was coming up with a plan for continuously updating the website and selecting a name. Once those decisions were made, the rest was easy. We selected a blogging service, purchased a domain name, and PBB was born! In all, about 3 weeks lapsed between the day we began researching this endeavor and the launching of the website. We did have to spend time researching the best blog host-

ing tools (e.g., Typepad versus Blogger) and the best way to obtain a domain name, and we spent a considerable amount of our early PBB days brainstorming on the design of the site—but these activities were actually quite enjoyable, as they marked the beginning of an exciting new business venture in which we wholeheartedly believe and to which we are fervently devoted. In our first 6 months, we have incurred some costs, as we invested in professional services to design a banner for the page and optimize our visibility in search engines (e.g., Google), but we counter these costs through the use of advertisements and the creation of an online store run in cooperation with Amazon through which we recommend research-based books, DVDs, and Kindle products about psychology.

PBB is updated every weekday, excluding holidays and vacations, and we take turns supplying articles. One of the lucky things about running a business with your spouse is that your business partner always knows your work availability, so it is easy to distribute tasks between us! The average article takes us approximately 1 to 3 hours to write. For some this might seem like too much time outside of our academic responsibilities, but to us it is definitely worth it. We also welcome articles from anyone who wants to write one, as long as the information discussed is consonant with the goals of PBB. On the first weekday of every month, we post a guest article from a distinguished member of the clinical psychology community—a professor, researcher, or clinician who is willing to contribute. Past featured contributors have included Robert Leahy, the current president of ABCT, as well as Craig Bryan of the United States Air Force, Jill Holm-Denoma of the University of Denver, Sarah Fischer of the University of Georgia, and several other notable professionals. Perhaps our greatest obstacle has been our own anxiety about approaching others to write for the site. As graduate students in a demanding program, we under-

stand that our colleagues are extremely busy and that our invitations could serve as a bit of a burden, so we have had to develop tactful, understanding approaches to making such offers in order to entice contributors without becoming a bother. Our efforts to develop a steady list of guest contributors are still ongoing and, as such, the workload in these early days of the business is fairly high as we take on the vast majority of the writing.

As with any new online business, one of our initial priorities was to maximize traffic to the site and general knowledge of our goals. We found ourselves somewhat unsure about how to develop a following for the site, as there did not seem to be an obvious method by which to alert a wide audience to our project. Granted, a significant portion of the global population suffers with or knows an individual suffering with a mental illness, but the general knowledge regarding ESTs for mental illness and research findings in clinical psychology is limited enough that a website with our goals must seek out readers rather than simply scoop up an already existing audience. The constraints of the graduate student wallet prevented us from utilizing high-cost, more traditional advertising opportunities. As such, we needed to find an alternative way to promote our services in a cost-effective manner that would enable us to reach as broad an audience as possible, encompassing both professionals and nonprofessionals. This is where social media tools became crucial in our efforts.

Our first attempt at publicity was simple word of mouth. Through conversations and e-mails, we alerted family, friends, and colleagues. This resulted in substantial encouragement, but only a small flurry of visits to the site. Our next step was to create a presence on Facebook (www.facebook.com), a social media website that allows individuals to follow the lives of others as well as to receive news regarding causes, individuals, and organizations of interest (e.g., political figures, celebrities, support groups for particular illnesses). We already had experience with Facebook, having maintained personal pages for several years and being involved with several organizations via the website (including ABCT). Creating a Facebook presence for Psychotherapy Brown Bag was the next logical step. We created two types of pages: a group (285 members as of 9/8/2009) and a fan page (150 fans as of 9/8/2009). Facebook has provided an avenue through which individuals we might not otherwise meet could discover PBB, glance at links to our articles, recommend

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the site to others, and communicate with us. After we publish a new post on Psychotherapy Brown Bag, we alert individuals following us through Facebook by posting a link to our new article. Each week, we email all Facebook users following our group page and provide a list of the week's articles as well as any news pieces of interest. With just a small amount of effort on our part and at absolutely no cost, Facebook almost does the work for us, as fans of PBB alert their friends to our pages and services and those friends pass along information again, and so on.

From here, our next move was to create a presence on a similar social media site, albeit one with which we had less personal experience: Twitter (<http://www.twitter.com>). Twitter allows users to post links and comments and to follow the comments of other individuals. In doing so, individuals can also follow global conversations on particular topics (e.g., depression, a particular celebrity, a sporting event) or simply engage in conversations with an individual or group of individuals on a smaller scale. Each message is brief—limited to a total of 140 characters—so each post essentially serves as a headline. It was our belief that by entering these conversations and posting links to our articles, we could increase the degree to which individuals know about, visit, and interact with our site. This belief has thus far proven to be true. We were recently named as one of the top 100 science resources on Twitter by OnlineCourses.org and, as of September 8, 2009, we have more than 1,800 individuals and organizations following our daily updates. That's nearly 2,000 people who are receiving daily information on ESTs—quite a success, in our opinion! Additionally, Twitter has developed a social code by which individuals interact with one another. When one individual finds another individual's post interesting, he or she "retweets" the comment. Retweeting is a slang term that refers to making the same comment as another poster, crediting the original poster for the comment (including a link to that poster's profile), and thus making the comment visible to all people following his or her feed. As such, a single comment can be made visible to thousands of individuals in mere moments. This has proven to be a highly valuable form of free advertising. Additionally, because individuals only follow the comments of people they choose, the site provides an opportunity for like-minded individuals to network and develop professional connections. In this sense, we have found several individuals who have expressed an interest in or begun

the process of writing guest articles for our site on areas that are outside of our areas of expertise but consistent with the overall goals of PBB. So just like Facebook, Twitter has afforded us an opportunity to reach a global audience with little effort and no cost on our part.

Our third and final significant interaction with social media tools has been with Reddit (<http://www.reddit.com>). Individuals sign up for Reddit and post links either to the general site or to one of the subheadings. In our case, the psychology subheading (<http://www.reddit.com/r/psychology>) has proven to be the most useful. As of September 8, 2009, the psychology subheading of Reddit had over 8,600 subscribers. Subscribers click on links and evaluate them by providing a score, either a plus or minus one. Links thus receive a total score based upon the summed opinions of readers. The higher the score, the further up on the list of headlines it appears, thus increasing the likelihood that it will be seen and read by a greater number of individuals (although there are separate tabs for "disliked" and "controversial" links as well for individuals who wish to search for articles not necessarily popular with other readers). Subscribers also have the option of posting comments regarding the link, thus providing an additional opportunity to interact with readers, clarify any confusion, and assess which topics might interest readers more than others. Thus far, readers have been directed to our site through Reddit from every continent except Antarctica, with particularly heavy traffic in Europe. As such, we have managed to develop new connections with individuals we would otherwise not have had any opportunity to meet while simultaneously accomplishing our goal of disseminating accurate, research-based information on psychopathology and psychological treatments. Again, we reiterate this was low-effort and entirely free advertising.

Overall, our experience with social media tools has been positive. Traffic on our website continues to increase substantially, enabling us to reach hundreds of readers per day. We remain optimistic that these tools will continue to help our company grow and to reach a broader global audience. We have also begun experimenting with additional social media tools such as Digg (<http://www.digg.com>) and StumbleUpon (<http://www.stumbleupon.com>) and anticipate experimenting with others as we learn about them. It remains to be seen whether these sites are as useful as traditional advertising methods, but without question, they

are a quick and cost-effective means through which to increase awareness of a cause, issue, company, or service and to target a particular audience likely to interact with whatever is being marketed. Perhaps most importantly, utilizing social media tools can help a website like ours or even a therapist in private practice to keep up with current trends in the field, issues that are gaining significant interest, and prominent names receiving attention on particular topics. Although the goal of our site is to provide articles on a comprehensive variety of psychology-related topics, we also strive to ensure that readers are able to find a multitude of articles on topics directly related to their interests. By interacting with readers on social media websites, we have been able to develop a greater sense of what topics currently spark the most interest amongst the widest array of individuals, thus helping to capture a larger audience likely to return to the site and engage with other materials we publish.

Despite all of the positive aspects of social media tools, we remain cautious with respect to how we present ourselves on these websites, as we believe there are several potential risks that must be considered. Perhaps the greatest of these risks is the potential for individuals in crisis to misinterpret our services and seek emergency interventions through our website or our profiles on various social media tools. As such, we are careful to clearly state that Psychotherapy Brown Bag is an educational tool, not a therapy provider, and to provide resources such as the National Suicide Prevention Lifeline (1-800-273-TALK) for individuals in crisis. This is an equally important consideration for therapists considering utilizing social media tools, as it is entirely possible that individuals seeking immediate help will stumble upon a website and mistakenly interpret it as a source of help when, in fact, better alternatives exist for their immediate situation. An additional risk—one more unique to the services provided by Psychotherapy Brown Bag—is the potential that individuals will misinterpret our postings in a manner that would result in the proliferation of misinformation or, on a more personal level, hurt feelings. Translating complex research findings published in fairly esoteric psychology journals into prose more consistent with what one would read in a popular magazine can be difficult, as some topics require a significant amount of background information in order to be explained clearly. As such, we take care to provide several different explanations for findings that might be difficult

to interpret and links to alternative sources of information that might explain the topic in a manner more easily understood by the reader. In fact, we are not the only website out there promoting mental health care and we frequently link to such resources as we learn about them. Given the sensitive nature of much of the material covered by clinical psychologists, whether they work as therapists, researchers, or educators, such considerations are pivotal, otherwise readers will be hurt and, in all likelihood, turn to other sources for information that might rely less upon research and, as such, be more likely to encounter misinformation. A final consideration for professionals considering incorporating social media tools into their lives is the issue of personal privacy. We understand from discussions with colleagues (for example, subscribers to the ABCT list-serve) that many professionals are nervous about having an online presence because of the potential for blurred therapist-client boundaries. This is a legitimate concern, but one that can be avoided by being smart and careful in one's use of these technologies. For example, on Facebook, we have been able to maintain both professional and personal identities by taking advantage of the many privacy options that Facebook provides. We have limited what the public can see on our personal sites, while keeping the Psychotherapy Brown Bag pages open to the public (see the Facebook blog post, "10 Privacy Settings Every Facebook User Should Know" for details on how to do this: <http://www.allfacebook.com/2009/02/facebook-privacy/>). Other social media sites can also be navigated and protected in a similar manner.

In weighing the costs and benefits of utilizing social media tools, there is one final point worthy of consideration: regardless of whether or not a particular psychologist chooses to utilize these tools, other individuals will make the choice to do so. Unfortunately, many of those individuals will offer ineffective therapeutic interventions or attempt to perpetuate misinformation. These individuals will, in large part, not do so out of malice, but rather ignorance. Whether this represents a lack of education regarding research methods and data analysis, a philosophical devotion to a particular therapeutic modality not supported by research, or a bad personal experience with an otherwise effective treatment approach, the end result is the same: a voice, widely accessible to the public, making claims that contradict empirical facts. Readers—consumers of psychological services and information—lack sure-fire

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methods by which to determine who is providing accurate information and who is providing false hope. As such, if there are more individuals utilizing social media tools who are peddling misinformation and doing so on a large stage in a charismatic manner, consumers will be defenseless and the public will remain in the dark regarding data-driven conceptualizations of mental illness and the most effective means for treating it.

In this sense, regardless of whether or not a clinical psychologist finds him- or herself interested in social media tools, it could be argued that it is beholden upon them to engage in this new source of knowledge in an effort to provide a voice to research and ESTs for mental illness. Psychotherapy Brown Bag is lending its voice to the debate, and we hope others will join in the effort.

Correspondence to Michael D. Anestis or Joye C. Anestis, Florida State University, 953, Parkview Dr., Tallahassee, FL 32311
mikeanestis@psychotherapybrownbag.com
joyeanestis@psychotherapybrownbag.com

Research Forum

Barriers to the Dissemination of Empirically Supported Treatments: Matching Messages to the Evidence

Bruce E. Wampold, *University of Wisconsin–Madison*, Zac E. Imel, *VA Puget Sound Healthcare System—Seattle Division and University of Washington*, and Scott D. Miller, *International Center for Clinical Excellence*

Given the complexity of the therapeutic endeavor, it is not surprising that interpreting the *evidence* is complex—if it were not, the debate surrounding empirically supported treatments (EST) would be inconspicuously absent. Evidence is not simply observation of phenomena, regardless of whether the observations were derived in experimentally manipulated environments (e.g., randomized controlled trials [RCTs]) or naturalistic settings. Rather, evidence involves the inferences that flow from observations. What constitutes evidence is ultimately decided by a confluence of two factors—the phenomenon itself and people (Hacking, 1983; Latour, 1999). The phenomenon, under various environmental conditions, is observed by people (i.e., the scientists), who then draw conclusions about the phenomenon. The road from observation to conclusion is saturated with social influences on the scientist. Ultimately, it is the scientific community that decides which conclusions are valid and disseminated.

Accordingly, publications are, in some sense, rhetorical devices, the purpose of which is to influence the scientific community about what is the “proper” evidence (Latour, 1999). With respect to the ongoing EST debate, Raymond DiGiuseppe (2007) recently lamented that “efforts to disseminate empirically supported treatments (ESTs), and especially cognitive-behavioral

treatments, have been limited by perceptions that all psychotherapies are equally effective [the Dodo Bird verdict], and ... ‘that common factors, therapist, and relationship variables account for the majority of the variance in therapy outcome studies’” (as quoted in Siev, Huppert, & Chambless, 2009, p. 69). DiGiuseppe offered three alternatives: “Either we rebut these conclusions, conduct new research to show they are wrong, or we accept them and change our message” (as quoted in Siev et al., p. 69).

Siev et al. (2009) have accepted the premise that barriers to the dissemination of ESTs are due to the rhetorical talents of the dodo birders and have chosen to rebut what they believe are misguided conclusions about the nature of psychotherapy. Although their article purports to “respond to these contentions and to present an update on recent research bearing directly on the Dodo Bird verdict” (p. 69), it mostly recapitulates old criticisms and ignores contradictory, and at times vast, evidence. Barriers to the dissemination of ESTs are not the result of sophistry, but a rational reaction to an interpretation of the evidence. We take this opportunity to present evidence that has been omitted and address several important questions raised by Siev et al.

Psychotherapy Treatments and a Possible Future

The history of psychotherapy is characterized by efforts to promote particular theoretical perspectives (Cushman, 1992; Fancher, 1995; Wampold, in press). The claims of the superiority of one method over another were endemic from the origins of psychotherapy, as Freud and his disciples vehemently argued about theory and practice (Makari, 2008). The disputes proliferated as the behaviorists criticized the psychoanalysts, the humanists took a different tack altogether, cognitive constructs were accommodated by behavioral theories (Fishman & Franks, 1992), a third wave of “acceptance based” behavioral treatments emerged (Hayes, 2004), and integrationists attempted to reconcile deficiencies in unitary theories (Norcross & Goldfried, 2005). These efforts have resulted in hundreds and hundreds of approaches to psychotherapy. Norcross and Newman (1992) said it aptly:

Rivalry among theoretical orientations has a long and undistinguished history in psychotherapy, dating back to Freud. In the infancy of the field, therapy systems, like battling siblings, competed for attention and affection in a “dogma eat dogma” environment... Mutual antipathy and exchange of puerile insults between adherents of rival orientations were much the order of the day. (p. 3)

A seemingly reasonable way to settle disputes between rival schools is to sift and winnow the various approaches based on their efficacy—treatments that produce demonstrable benefits should be preferred to others. Indeed, such logic gave birth to and guided the EST movement (Chambless & Hollon, 1998; Task Force on Promotion and Dissemination of Psychological Procedures, 1995). Unfortunately, agreement about both the type and meaning of the evidence has proven to be more complex than anticipated.

The difficulty in using evidence from what many consider the “gold standard” of sources—RCTs of psychological treatments—is illustrated by a taking a hypothetical visit to the future.

A Hypothetical Future: The Ascendance of Affect

In 2020, due to advances in affective neuroscience (see Davidson, Sherer, & Goldsmith, 2003), Federico Perla developed a new psychotherapy, which he named affect-centered psychotherapy (ACP). The premise of ACP is that affect evolved to regulate social interactions in nonhuman and eventually human animals. The cognitions generated by individuals are post-hoc explanations of experience that interfere with the encoding and decoding of emotion, which in turn leads to dysfunction. ACP involves a systematic program to emphasize primary emotional responding and minimize cognitive involvement. Similar to other therapies, ACP contains a number of elements that are considered important and necessary but not constitutive of the treatment (Grünbaum, 1981), such as a relationship with a therapist, a cogent explanation, therapeutic actions, and expectations for change. Perla, a charismatic and persuasive scientist-practitioner, having successfully treated many anxiety disorders with ACP, designed three successive RCTs to establish the efficacy and specificity of ACP.

Programmatic Research in ACP

The first study involved a comparison of ACP to a waitlist condition. Using four of his therapists, Perla randomized 30 patients with generalized anxiety disorder (GAD) to one of the two conditions. Not surprisingly, ACP was superior to no treatment on targeted measures (i.e., measures related to GAD) and on other measures (e.g., measures of depression and quality of life).

Encouraged by the results and convinced that the focus on emotion was critical to the successful treatment of GAD, Perla designed a trial that would control for the common factors. The control condition in this case was called *rational counseling* (RC), which involved an engaged therapist who probed the patient to express how they *thought* about events in their lives; the therapist reinforced the verbalizations about cognitions, but did not convey any value about the rationality of the thoughts (i.e., did not discriminate between adaptive and maladaptive thoughts). To establish internal validity (i.e., control for potential focus on

affect in the rational condition), the therapist redirected the patient to their cognitions about an event if the patient expressed emotion. Additionally, therapists were proscribed from using language that referenced emotion. The patient was encouraged to explore their thoughts about events in their lives and the therapist was instructed not to focus on particular themes, but rather allow the patient to talk about what he or she thought was important. Perla trained the four therapists used in Study 1 to conduct RC as well as ACP and supervised all treatment. One therapist was removed prior to the study because she could not adhere to the requirement to ignore affect in the RC condition. Patients were randomly assigned to conditions and adherence measures showed adequate fidelity. It was found that ACP was superior to RC on measures of anxiety but was not definitively superior on secondary measures of depression and quality of life.

Perla, having shown that ACP was superior to a treatment that contained the common factors (i.e., a relationship with a therapist), was convinced that the focus on affect was critical to the successful treatment of GAD, sought to show that it was superior to a well-accepted treatment, namely cognitive-behavioral treatment (CBT) of GAD, a well-established EST for GAD (Chambless et al., 1998). However, to ensure that CBT did not work through any mechanisms related to the encoding or decoding of emotion, the therapists administering CBT, like those in the prior study using RC, were proscribed from engaging in any conversations about emotion, or using any affective language or display of emotional responses, and, finally, instructed to redirect any affect-laden material arising in session to cognitions. The basic CBT protocol was otherwise left unchanged. A waitlist control group was included as well. Additionally, the same three therapists conducted both ACP and CBT. Because Perla received training in the United States by a prominent CBT therapist, he trained the therapists in CBT and supervised both therapists in both modalities. Patients were randomly assigned to both treatments.

As in the prior studies, fidelity measures showed the treatments were delivered according to their respective protocols. On measures of GAD symptoms, ACP was superior to CBT ($d = 1.02$); there were few significant differences between ACP and CBT on ancillary measures, including depression and quality of life. Both treatments were superior to the no-treatment condition on all measures.



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Subsequently, several other groups compared ACP to CBT and found divergent results. In one study, CBT was superior to ACP on GAD measures, in two studies there were no significant differences, and on a fourth, ACP was superior to CBT on GAD measures, although the size of the effect was smaller than in Perla's study (viz., $d = .32$ vs. $d = 1.02$, respectively). No differences in studies were found on ancillary measures. Perla reasoned that in the studies in which CBT was equivalent to ACP, CBT worked because the therapists were not proscribed from discussing affect and that the affective components of CBT were responsible for the benefits of CBT. Moreover, he claimed that when CBT was superior to ACP, ACP was not delivered in an adequate way. A meta-analysis of these studies showed that the aggregate effect size for the five studies was significantly larger than zero ($d = .36$) for GAD measures with no differences on other variables of psychological functioning. Based on the totality of the results, it was concluded that (a) ACP should be designated as an EST, (b) ACP works through the specific ingredient of attending to affect, (c) ACP is preferred as a treatment of GAD to other treatments.

Problematic Conclusions

The history and conclusions about ACP closely resemble developments in modern clinical practice—and are equally problematic. Anyone familiar with research on psychotherapy will not find the results of the first trial, where ACP proved superior to no treatment, surprising. Every reasonable treatment administered to patients seeking treatment has been found to be effective relative to no treatment, including such treatments as eye movement desensitization and reprocessing and present-centered therapy for PTSD (e.g., McDonagh et al., 2005; F. Shapiro, 1989). More importantly, studies comparing a treatment to a no-treatment control do not indicate whether the supposed “active” or specific ingredients of the particular treatment are responsible for the change (Wampold, 2001). In the case of ACP, the benefits may be due to the fact that ACP delivers a treatment that has a cogent rationale, creates positive expectations, involves a relationship with a skilled therapist who collaborates with the patient to set treatment goals and involves reasonable tasks related to those goals (i.e., the components that form the working alliance)—all factors known to be related to successful psychotherapy (Anderson, Lunnen, & Ogles, in press; Imel & Wampold, 2008).

The second clinical trial, which compared ACP to RC, is also problematic. Over the years, the RCT has become the “gold standard” of research in psychotherapy and medicine. As most know, an essential element of the RCT in medicine is the double blind; both the patient and the provider of treatment are unaware of whether they are providing the real or sham treatment. To point out the obvious, RCTs in psychotherapy are not double blinded (Seligman, 1995; Wampold, 2001). Indeed, as Seligman has noted, “Whenever you hear someone demanding the double-blind study of psychotherapy, hold on to your wallet” (p. 965). Unlike medicine, where a pill that looks, tastes, and even mimics the side effects of the experimental drug makes it difficult for providers to tell the difference between the experimental and control treatments, control psychotherapies are easy to identify. In the ACP versus RC comparison, the therapists knew about both treatments they were delivering (i.e., the design was crossed). In addition, the therapists were knowledgeable of the hypothesis regarding the two therapies, had an allegiance to one of those approaches (viz., ACP), and were trained and supervised in

both treatments by the developer of ACP. This failure of the double-blind is further exacerbated by the fact that the comparison treatment, RC, is easily identified as a sham. For reasons of internal validity rather than quality of care, the therapists in the RC condition were proscribed from actions that would be reasonable to most therapists (e.g., could not use language that referred to affect). What consumer of mental health services would seek out a practitioner in any profession who believed the services being offered were bogus? And yet, this is exactly the case with the RC condition—therapists *knew* they were delivering a service that was not intended to be therapeutic. As such, the finding that ACP was *more* effective is hardly surprising!

The third trial, in which it was found that ACP was superior to CBT, would reasonably elicit the skepticism of CBT researchers. There are several consequential problems here. First, as was the case previously, the therapists had a clear allegiance to ACP—they worked with Perla, were trained by Perla, and were supervised by Perla. Second, the CBT protocol was altered to obviate any work on emotions, which, while justifiable as a research operation intended to tighten internal validity, an emotionless CBT does not represent treatment as it would be delivered by a competent clinician. Moreover, the only significant differences found were in the area of GAD symptoms—both treatments were equally effective in terms of addressing depression and quality of life, which raises the issue of whether ACP is truly superior to CBT or whether, as operationalized in this study, ACP is more focused on GAD symptoms, raising the specter of reactivity of the measures (i.e., ACP was more focused on symptoms of GAD).

The superiority of ACP to CBT was based on only five studies and a reanalysis of these trials shows that the effect size for GAD symptoms was due primarily to the one study conducted by Perla comparing ACP and CBT (i.e., the Perla result was an outlier). Indeed, when the Perla study was removed from the meta-analysis, the aggregate effect size was not significantly different from zero. Thus, in terms of GAD symptoms, it would be difficult to conclude that ACP was superior to CBT. The equivalence of ACP and CBT for ancillary measures adds strength to the conclusion that there may be negligible differences between ACP and CBT. This meta-analysis provides little evidence to suggest that CBT is *not* indicated for GAD. In practical terms, there is insufficient evidence to support the con-

clusion that a focus on affect is a specific ingredient in the treatment of GAD.

Having briefly visited the past and future of research on psychological treatments, we now return to the present. In the following sections, we apply the lessons learned to the evidence reviewed by Siev et al. (2009) and others (e.g., Crits-Christoph, 1997; Howard, Krause, Saunders, & Kopta, 1997).

The Complete Evidence Base: The Dodo Bird Redux

Siev et al.’s (2009) criticism of the dodo bird conclusion that psychological treatments are equally effective is based largely on two studies: (a) a meta-analysis published almost 13 years ago (viz., Wampold, Mondin, Moody, Stich, et al., 1997) that corroborated the dodo bird conjecture, and (b) a new meta-analysis (viz., Siev & Chambless, 2007) that purportedly shows the dodo bird conclusion to be false. Briefly, the study by Wampold, Mondin, Moody, Stich, et al., utilized a test of effect size homogeneity to meta-analytically review all direct comparisons of psychotherapies and found that the results were consistent with the dodo bird conclusion: There was not sufficient evidence to reject the null hypothesis that the effect size for the comparison of various treatments was zero. Said another way, no evidence for differences in efficacy between psychological treatments were found. For many, particularly the advocates of ESTs and, for that matter, anyone who believes that the specific ingredients of particular treatments are responsible for the benefits of psychotherapy, this is a challenging conclusion.

Although the conclusions of the Wampold, Mondin, Moody, Stich, et al. (1997) meta-analysis were consistent with previous ones that examined the dodo bird conjecture (Grissom, 1994; D. Shapiro & Shapiro, 1982a, 1982b; Smith, Glass, & Miller, 1980; see Wampold, 2001, for a review), a number of important issues that limited the generalizability of the results were raised. In their article, Siev et al. (2009) combined these earlier criticisms of the Wampold, Mondin, Moody, Stich, et al. study with findings from the more recent meta-analysis by Siev and Chambless (2007), identifying four specific methodological flaws with the evidence in support of the dodo verdict: (a) meta-analyses that aggregate data across disorders masks differences between specific treatments for specific disorders; (b) meta-analyses that aggregate primary and secondary outcome

measures preclude the discovery of any specific effects for targeted treatments; (c) the similarity between the treatments included in the supporting meta-analyses accounts for the failure to find differences in outcome; and (d) the way these same meta-analyses classify treatments as bona fide is circular, eliminating treatments that are not efficacious. These issues are not new and have indeed been helpful in guiding and structuring a program of systematic research conducted over the last decade. As will be discussed below, Siev et al. failed to cite this body of research, thereby presenting an incomplete picture of the evidence. We now respond to each of the points raised by Siev et al., citing prior published responses to earlier criticisms, and presenting existing evidence.

Meta-Analyses Were Conducted Across Disorders

Wampold, Mondin, Moody, Stich, et al.'s (1997) conclusion about the lack of treatment differences was based on examining direct comparisons of treatments published between 1970 and 1995 in six journals that typically publish psychotherapy RCTs, regardless of the disorders being treated. Siev et al. (2009) raised what was, at the time the study appeared, an important point to consider (Crits-Christoph, 1997; DeRubeis, Brotman, & Gibbons, 2005), namely that ignoring disorder may well mask differences between treatments for particular disorders. DeRubeis et al. (2005) articulated this quite clearly: Ignoring disorder "is akin to asking whether insulin or an antibiotic is better, without knowing the condition for which these treatments are to be given.... Alternatively, researchers should begin with a problem and ask how treatments compare in their effectiveness for that problem" (p. 175).

There are two responses to this argument, however. First, the trials examined by Wampold, Mondin, Moody, Stich, et al. (1997) were, in each case, two treatments *for a particular disorder*—they were not simply two treatments selected at random or arbitrarily. Second, and more to the point, there have been numerous subsequent meta-analyses that addressed the criticism of disorder heterogeneity that have found no differences among treatments for particular disorders, meta-analyses that Siev et al. (2009) did not mention. These meta-analyses span a number of disorders, including alcohol use disorders (Imel, Wampold, Miller, & Fleming, 2008), posttraumatic stress dis-

order (PTSD; Benish et al., 2008), depression (Wampold, Minami, Baskin, & Tierney, 2002), GAD (Siev & Chambless, 2007), and pediatric disorders (Miller, Wampold, & Varhely, 2008, when allegiance was controlled, which is discussed in a following section; Spielmanns, Pasek, & McFall, 2007).

The new meta-analysis cited by Siev et al. (2009) as evidence that "broad judgments about the relative importance of technique ... can be misleading" (p. 75) was conducted by Siev and Chambless (2007). Briefly, the study compared CBT and relaxation therapy (RT) for panic disorder without agoraphobia and GAD. No differences were found between CBT and RT for GAD. However, for panic disorder, CBT was superior to RT for panic-related symptoms but not for other symptoms of anxiety or depression. Based on these results, Siev and Chambless concluded, "The finding that CT and RT do not differ in the treatment of GAD, but do for PD [panic disorder], is evidence for the specificity of treatment to disorder, even for 2 treatments within a CBT class, and 2 disorders within an anxiety class" (p. 513). Importantly, they arrive at such a conclusion despite the fact that (a) no a priori prediction of such a pattern of results was made, and (b) no description is given of the specific mechanism believed to be present in CBT for panic that is responsible for the superiority for panic symptoms only. Exactly what is the specific ingredient of CBT that makes it more effective in reducing panic-related symptoms in patients with panic disorder but has no effect on patients with GAD and depression? Finally, it is ironic that specificity based on a GAD/panic disorder distinction is critical to promoting and disseminating CBT when a perspicuous effort in CBT is to develop protocols that are effective across the range of emotional disorders, based on a common diathesis of such disorders (see, e.g., Moses & Barlow, 2006).

Despite the relatively weak evidence for specificity, it could be claimed that the superiority of CBT to RT for the treatment of panic seems to be a clear counter-example to the dodo bird conjecture determined by meta-analytic methods. Let's examine this result on which so much rests. In the meta-analysis conducted by Siev and Chambless (2007), the conclusion that CBT was superior to RT for panic-related measures and not for generalized anxiety or depression was based on five studies. For the panic symptom measures, one study had an effect that was very large in favor of CBT ($d = 1.02$) while the remaining four studies pro-

duced small to moderate effects in favor of CBT ($d = .38, .32, \text{ and } .20$) or small effect in favor of RT ($d = -.20$). Indeed, there was relatively large heterogeneity among the effects but the hypothesis of homogeneity was not rejected because five studies yields a drastically underpowered test of homogeneity (Siev and Chambless calculated 37% of the variability in effects was due to between study variability, although we calculated 50%).

But here is what is very important: The advantage to CBT in the Siev and Chambless (2007) meta-analysis was entirely accounted for by the one study that found a large effect for CBT ($d = 1.02$), a study conducted by Clark et al. (1994) more than 15 years ago. When that single study is eliminated, the aggregate effect size for CBT versus RT is not statistically different from zero and the between-study variability goes from 37% (50% in our calculation) to 0%. In other words, the Clark et al. result is, statistically speaking, an outlier, without which there is no difference between CBT and RT for any class of measures. *The basis of Siev et al.'s claim that the dodo bird conjecture is false and that there is substantial evidence for specificity rests solely on this one study that found that CBT was superior to RT.*

Some would argue that, rather than being an anomaly, an outlier might reveal an important phenomenon that is obscured in other studies. With that possibility in mind, it is worth taking a closer look at this one study, as so much rests on its validity. The one outlier in favor of CBT is a study comparing CBT, RT, and imipramine for the treatment of panic disorder (a no-treatment condition was also included) conducted by Clark and colleagues (1994). There are several aspects of the Clark et al. study that are important to note when considering the results. The relaxation treatment in this study was Öst's (1987) applied relaxation, which was modified in two ways for the comparison with CBT. The first modification was changing the rationale presented to the patients to exclude mentioning that the premise of Öst's applied relaxation involved "a vicious circle in which the physical symptoms of anxiety are augmented by negative thoughts" (Clark et al., p. 761) and instead used a behavioral explanation that excluded mentioning that thoughts were involved. We would assume this was done to increase internal validity (thereby preventing excessive discussion of thoughts) and not to improve the quality of RT.

The second modification, more consequential, was that in Öst's development of RT, exposure to anxiety-provoking stimuli was not introduced until *after* training in relaxation was completed (8 to 10 sessions), whereas in Clark et al.'s version, exposure was begun after four sessions—that is to say, Clark et al. exposed patients to the feared stimuli before they had learned to relax. Thus, it is quite possible that Clark et al. may have actually conditioned panic symptoms (i.e., patients were exposed to the feared stimulus without skills to cope with fear) whereas Öst's protocol correctly desensitized the patients because they would have the coping skills to reduce anxiety in the presence of the feared stimuli.

Clark et al.'s (1994) modifications make it difficult to argue that this trial was a comparison of CBT and RT, as *RT has been developed, administered, and tested*. Recall the discussion of hypothetical model of the future. Perla's comparison of ACP and CBT was flawed because Perla modified CBT to increase the validity of the comparison with ACP, but in the process the CBT delivered in Perla's comparison was *not* CBT as designed and delivered by CBT therapists. Indeed, it is not unusual that when a cognitive or behavioral treatment is found to be inferior to another treatment (e.g., an insight-oriented treatment), the claim is made that the CBT was not delivered faithfully or skillfully, despite adequate fidelity ratings (see Jacobson, 1991).

Another consequential aspect of the Clark et al. (1994) study was also foreshadowed in the Perla story—namely, the therapists involved in the trials. The therapists in the Clark et al. trial were Ann Hackmann and Paul M. Salkovskis, widely regarded as accomplished CBT therapists, co-authors of the article, and closely allied with Clark. Indeed, Salkovskis, along with Clark, developed the CBT used in this trial and Ann Hackmann has published many articles on CBT and has been involved in the development of various cognitive-behavioral treatments. Both therapists were supervised by David Clark. Despite Siev et al.'s (2009) contention that the trials in the meta-analysis were fair because therapists “were crossed with treatment condition,” the therapists in the Clark et al. study were clearly aligned with and had allegiance to CBT. Perhaps there is a clinical implication of this study: A consumer seeing either of these *two* therapists would rather receive CBT than RT, as administered in this trial. However, the same consumer seeing a therapist who has an allegiance to RT and who followed Öst's protocol may well attain an

equivalent benefit. Or, alternatively, the consumer may well benefit from psychodynamic therapy, a treatment recently shown to be efficacious in the treatment of panic disorder (Milrod et al., 2007).

So, to a large extent, the claim that the dodo bird conjecture is false rests on the Siev and Chambless (2007) meta-analysis—a study that found no differences between treatments for GAD but did find differences for the treatment of panic disorder, but only on panic measures, but not anxiety and depression. More importantly, these results are based entirely on a single study (viz., Clark et al., 1994), one which severely, if not fatally, modified RT and for whom the clinicians delivering the treatments had a distinct allegiance to the superior treatment. If for some, the dodo bird conjecture is to fall because CBT was superior to RT in the Clark et al. study, so be it. The similarities to Perla and the conclusion that ACP is superior to CBT, which was worrisome scientifically, are unmistakable. The (hypothetical) future is the present.

It Is Inappropriate to Aggregate Primary and Secondary Measures

Siev et al. (2009) emphasized that meta-analyses should not aggregate across outcome measures because certain patterns of results would fail to be revealed—an argument that echoes criticisms made by Crits-Christoph (1997), who raised a similar argument when Wampold, Mondin, Moody, Stich, et al. (1997) was initially published. Siev et al. failed to reference that, in response to Crits-Christoph, the trials meta-analyzed by Wampold, Mondin, Moody, Stich, et al. were re-analyzed by Wampold and his students (Wampold, Mondin, Moody, & Ahn, 1997) and it was found that *the dodo bird conclusion applied to targeted as well as secondary variables*. As well, we have published meta-analyses that now segregate analyses for primary measures, and in these meta-analyses no differences are found among treatments for primary variables for a number of disorders (e.g., Benish et al., 2008; Imel et al., 2008). Moreover, there is strong evidence that primary and secondary measures are not conceptually or psychometrically distinct (Flückiger, Regli, Grawe, & Lutz, 2007; Krueger, 1999; McGlinchey & Zimmerman, 2007; Meyer, Pilkonis, & Krupnick, 2002; Moses & Barlow, 2006; Tanaka-Matsumi & Kameoka, 1986). This suggests that making a distinction between these two classes is not appropriate and raises the specter that changes in measures

specific to a disorder (e.g., diagnostic criteria) may be due to the focus of a treatment on these particular symptoms.

Of more clinical importance, the issue of primary and secondary variables attenuates the importance of psychological functioning of patients and emphasizes diagnostic criteria instead. Siev et al. (2009) make a point to show that targeted measures in panic disorder are quite large compared to all other effects (see panel 3 of their Figure 1). Their results, as displayed in Figure 1, show that CBT is superior to RT with regard to panic symptoms, but not for other quite important measures of psychological well-being—generalized anxiety and depression. It is disappointing that psychotherapy research has come to the point that demonstrating relative advantage on targeted symptom measures but having little impact on important measures of the quality of patients' lives is viewed as compelling evidence from which to argue for specificity. Removing symptoms but not benefitting patients generally is not a desirable outcome to many—most importantly to patients.

To illustrate the imprudence of focusing exclusively on the symptoms of a particular disorder, consider a trial comparing CBT and present-centered therapy for PTSD (McDonagh et al., 2005). Present-centered therapy is based on a problem-solving model and similar to condition in the Perla example; RC and present-centered therapy were both purposefully developed so as not to contain any specific ingredients of the experimental treatment (e.g., exposure or cognitive restructuring) believed to be remedial to the disorder. In the McDonagh et al. study, the *only* significant difference in outcomes between the CBT and PCT was for the completers in terms of proportion of patients meeting diagnostic criteria at follow-up. Said another way, significantly fewer patients who completed CBT met diagnostic criteria for PTSD at the follow-up assessment—a difference was not significant in the intent-to-treat sample. There were no differences between the two treatments in terms of anxiety, depression, dissociative experiences, hostility, anger, and quality of life. What conclusion can one make about a treatment that impacts the criteria used to make the original diagnosis and offers little other relative advantage? Combined with the fact that over 40% of patients in CBT dropped out versus less than 10% in present-centered therapy, one has to question the emphasis placed on meeting criteria for a disorder and ignoring measures of psychological functioning and

well-being and the acceptance of the treatment (see, as well, Schnurr et al., 2003, which found a similar result). Certainly, psychotherapy can do better. And certainly we have to be cautious about disseminating CBT prematurely, given the high dropout rate in this study.

Preponderance of CBT and Behavioral Treatments in Meta-Analyses

With regard to the preponderance of CBT and behavioral treatments, Siev et al. (2009) again noted earlier criticisms of the Wampold, Mondin, Moody, Stich, et al. (1997) meta-analysis without considering published responses or recent studies that addressed these criticisms. Crits-Christoph (1997) first suggested that the similarity of treatments in the Wampold, Mondin, Moody, Stich, et al. meta-analysis was responsible for lack of difference in outcome. Siev et al. did not mention that Wampold, Mondin, Moody, Stich et al. showed that the dissimilarity of the treatments compared was not correlated with the effect size (i.e., comparisons of more dissimilar treatments did *not* produce larger effects). Neither did Siev et al. cite an extensive discussion of this criticism by Wampold and colleagues (Wampold, Mondin, Moody, & Ahn, 1997). As just one example, Crits-Christoph (1997) classified many treatments as cognitive behavioral when clearly they were not (e.g., Greenberg's emotion-focused therapy). Finally, there are several subsequent meta-analyses containing treatments with diverse theoretical bases that have found no differences in outcome. For example, the meta-analysis of PTSD that showed no differences contained treatments as variable as CBT, eye-movement desensitization and reprocessing, hypnotherapy, present-centered therapy, psychodynamic therapy, and exposure (Benish et al., 2008). Similarly, with regard to the alcohol use disorders, inclusion of treatments based on cognitive-behavioral principles, motivational interviewing, and 12-step principles did not reveal significant differences among treatments (Imel et al., 2008).

If It Works Then It's Bona Fide

One of the features of Wampold, Mondin, Moody, Stich, et al.'s (1997) meta-analysis was that the comparisons involved only treatments that were intended to be therapeutic, a hypothesis proposed by Lambert and Bergin (1994): "Research carried out with the intent of contrasting two or more bona fide treatments show surprisingly small differences between the out-

comes for patients who undergo a treatment that is fully intended to be therapeutic" (p. 158). The intent of Wampold and colleagues' meta-analysis was to examine the relative efficacy of various psychotherapies that were plausibly therapeutic and to exclude treatments that were shams created for the purpose of controlling for common factors.

As was clear in the Perla scenario, some comparison treatments, such as rational counseling, are designed to control for some common factors, but are not legitimate treatments. Such comparison treatments, often called supportive counseling, common factor controls, or psychological placebos, do not have cogent psychological rationales, proscribe the therapist from actions that most therapist would consider fundamental and therapeutic, and often have no coherent actions intended to be therapeutic. In the trials that use these treatments, the therapists delivering them know full well they are not intended to be therapeutic (as was clear in the Perla scenario), which demonstrates the pernicious effects of not being able to blind psychotherapy trials.

A good example of a treatment not intended to be therapeutic is the supportive counseling condition designed by Foa as a control for a CBT/exposure treatment for PTSD with women who had experienced childhood sexual abuse (Foa, Rothbaum, Riggs, & Murdock, 1991). To rule out cognitive components and exposure, the therapists providing supportive counseling were not allowed to explore or discuss patients' attributions about their abuse or their current problems and the therapists could not allow patients to discuss their abuse, as the latter would involve covert exposure and desensitization. Rather, their patients were taught a general problem-solving strategy. Therapists were allowed to respond indirectly and with unconditional support. Critically, however, whenever a patient in the study tried to talk about the sexual assault they had experienced, therapists in the supportive control condition were limited to redirecting the discussion to everyday problems. What therapist would provide such a treatment, much less believe in the efficacy of such a treatment? Some might not believe in repeated and prolonged imaginal exposure to the trauma as the primary curative ingredient of trauma therapy, but we know of few therapists who would advocate prolonged, intentional, and systematic redirection away from discussion of the very topic that brought the patient to therapy. If merely talking about the trauma

could lead to therapeutic exposure, then questions are raised about the necessity of prolonged and repeated exposure to the trauma memory in the form of PE (e.g., listening to tapes, etc.). Conversely, if merely talking about the trauma is not sufficient, why restrict the conversations? Supportive counseling in this instance is as credible as Perla's rational counseling. How are we to interpret the meaning of differences between the two conditions? Are they the result of curative power of exposure, or perhaps some other factor? We believe the answer is ambiguous at best.

To identify those treatment conditions that were *not* intended to be therapeutic, Wampold and colleagues (Wampold, Mondin, Moody, Stich, et al., 1997) used the following criteria to designate a treatment as bona fide:

First, the treatment must have involved a therapist with at least a master's degree and a meeting with a patient in which the therapist developed a relationship with the patient and tailored the treatment to the patient. Thus, any study that used solely tape-recorded instructions to patients or a protocol that was administered regardless of patient behavior (e.g., a progressive relaxation protocol that was not modified in any way for particular patients) was excluded. Second, the problem addressed by the treatment must have been one that would reasonably be treated by psychotherapy, although it was not required that the sample treated be classified as *clinically dysfunctional*. For example, treatments to increase time that a participant could keep a hand submerged in cold water would be excluded because cold-water stress would not reasonably be considered a problem for which one would present to a psychotherapist. However, any treatment for depression was included whether the participants met diagnostic criteria for any depressive disorder or scored below standard cutoffs on depression scales. Finally, the treatment had to satisfy two of the following four conditions: (a) a citation was made to an established approach to psychotherapy (e.g., a reference to Rogers's, 1951, client-centered therapy), (b) a description of the therapy was contained in the article and the description contained a reference to psychological processes (e.g., operant conditioning), (c) a manual for the treatment existed and was used to guide the administration of the psychotherapy, and (d) the active ingredients of the treatment were identified and citations provided for those ingredients. Accordingly, any treatments designed to control for com-

mon or nonspecific factors, such as placebo control groups, alternative therapies, or nonspecific therapies, were excluded. (pp. 206-207)

The criteria for differentiating the bona-fide treatments from shams are objective, and can be applied to any treatment, regardless of how effective the treatment is generally thought to be, based on clinical experience or past research. Importantly, in the meta-analyses conducted to date, coders applied the criteria for determining whether a treatment was intended to be therapeutic or not while remaining *blind* to the results of the individual trials being assessed. Coders were given only the description of the treatment provided in the method section of the trial in which the treatments were described.

Despite blind ratings and objective criteria, Siev et al. (2009) object to the classification scheme, contending that the criteria are circular:

Wampold and colleagues (e.g., Ahn & Wampold, 2001; Messer & Wampold, 2002) conclude that treatment outcome studies are futile because comparisons between bona fide treatments yield clinically insignificant differences and those between bona fide treatments and controls yield uninteresting differences. This contention is somewhat circular, however, because categorization as a bona fide treatment is both a criterion for inclusion in, and an implication of, the results of clinical experience and treatment outcome research (and meta analyses that synthesize multiple such studies). (p. 72)

To summarize Siev et al.'s (2009) argument, they believe that a bona fide treatment is synonymous with an effective treatment. They suggest that if it became known that treatments that exclude cognitive or behavioral components are less effective than cognitive/behavior treatments, it would be impossible to design a therapy without these components that would not be considered inferior. How they arrived at such a conclusion is unclear and a misinterpretation of Wampold and colleagues' method. Stated succinctly, designating a treatment as bona fide has nothing to do with either the efficacy of the treatment or whether it contains ingredients that are believed to work. The equivalency of bona fide treatments is an implication, *not* an a priori requirement of Wampold and colleagues' research. To be sure, quibbles can be made about the specific criteria, but that is a very

different criticism than the issue of circularity. For example, RT would not be excluded as a bona fide treatment for depression because it is not currently an EST for depression or because clinical experience suggests it does not work. It would be excluded because the researchers that used it intended for it to be a control condition not fully meant to work (i.e., no credible explanation for its effectiveness was given) and the therapists in the trial were aware that it was not intended to be therapeutic. It seems plausible that some dedicated researcher could develop a bona fide RT for depression or any other disorder, but we are not aware of any such attempts.

To illustrate the nuances of determining whether a treatment is designed as a control for common factors (i.e., is not intended to be therapeutic) and a treatment that is plausibly therapeutic, contrast the supportive counseling condition used by Foa et al. (1991) discussed earlier and present-centered therapy, a treatment designated as "intended to be therapeutic" by Benish et al. (2008). Present-centered therapy was described by McDonagh et al. (2005) in the following way:

The first two sessions of PCT were spent establishing rapport, giving an overview of the treatment, presenting the psychoeducational material, and establishing a treatment plan based on the client's choices of problems to address. The framework used to assist in understanding the ways in which CSA trauma can impact the client's current life was that of traumagenic dynamics. Traumagenic dynamics organizes the consequences of the experience of CSA into four categories—a sense of betrayal, powerlessness, stigmatization, and traumatic sexualization (Finkelhor, 1987). Clients were guided in noticing these dynamics in current life difficulties and factoring them in as information in the problem-solving model. The problem-solving model was a modification of systematic problem solving, which has demonstrated efficacy in the treatment of depression (DeShazer et al., 1986; D'Zurilla & Goldfried, 1971; Nezu et al., 1989). Therapists were encouraged to provide empathy, unconditional regard, and genuineness in their sessions (Meador & Rogers, 1973). The foci of subsequent sessions were determined by the participants' choice of current issues to address with the problem-solving skills. Homework was designed to assist clients in consolidating the information conveyed in sessions, writing about their problem solving efforts and, tracking those efforts in a journal to be re-

viewed with the therapist. A full description of PCT is available in the therapy manual. (p. 518)

Both supportive counseling and present-centered therapy involved contact with a reasonably skilled therapist, both were designed to exclude cognitive and exposure ingredients, and both involve aspects of problem solving as a coping mechanism. Present-centered therapy, however, had a cogent rationale, incorporated and referenced the psychological bases of the treatment, involved tasks reasonably related to the patient's distress, contained aspects of treatment related to the particular patient's problems, and contained homework that was integrated into the treatments. Moreover, the therapists in the supportive counseling condition were trained by one of the two authors of the study, who had an allegiance to the two CBT conditions and were supervised by Foa, the first author and developer of one the treatments. On the other hand, the therapists in the present-centered therapy condition were trained and supervised by the developers of the present-centered manual. The present-centered therapy met Wampold and colleagues' (Wampold, Mondin, Moody, Stich, et al., 1997) criteria of a treatment intended to be therapeutic, whereas supportive counseling did not.

The goal of designing RCTs that compare an EST to an alternative is not to design comparison treatments that might formally meet criteria for treatments intended to be therapeutic, but rather to find an adequate comparison treatment that will provide evidence about which treatment is most efficacious or evidence about the mechanisms of change. Poorly designed alternative treatments do neither. When comparison treatments are better designed, it appears that they are as effective as the evidence-based treatments to which they are compared (Baskin, Tierney, Minami, & Wampold, 2003).

As an aside, it is important to note just how well supportive counseling type and other stripped-down interventions can be—especially given the deficiencies in these treatments vis-à-vis what therapists would find to be reasonable treatments. Cottraux and colleagues (2008) compared CBT to Rogerian therapy for the treatment of PTSD in an RCT. Rogerian therapy was more focused than many alternative treatments but the description remained quite regimented, naïve, and obsolete compared to what modern experiential or humanistic therapists would employ currently (cf.

Elliott & Greenberg, 2007; Ellison & Greenberg, 2007; Greenberg, 2008; Pascual-Leone & Greenberg, 2007). The therapists in this study were CBT therapists, Cottraux is an advocate of CBT, and the trial was conducted in centers known for delivering CBT (the patients may well have desired CBT). Nevertheless, there were no differences in outcomes for the completers (and few in the intent-to-treat samples), although CBT retained more patients in treatment. Certainly, such findings as this (see also McDonagh et al., 2005; Schnurr et al., 2003) must pique the interest of those who claim specificity of treatment: How can Rogerian therapy for the treatment of PTSD be as, or even nearly as, effective as CBT?

Allegiance Effects

A confound alluded to in this critique of RCTs and Siev et al.'s (2009) conclusion is researcher allegiance. Perla and Clark, in our hypothetical and real-world examples, have an allegiance to a particular treatment. Allegiance is not simply a hypothetical conjecture—the effects are demonstrable (see Luborsky et al., 1999; Wampold, 2001). Apparent differences among treatments are often explained by differences in allegiance (e.g., Imel et al., 2008; Robinson, Berman, & Neimeyer, 1990)—the researcher's allegiance to a treatment increases the likelihood that this treatment will be shown to be superior to alternatives.

Although the presence of researcher allegiance effects is apparent, its causes have not been well determined (Leykin & DeRubeis, 2009; Luborsky et al., 1999; Wampold, 2001). On the one hand, allegiance may be manifest by the design of comparison groups (e.g., by creating comparison treatments that have no cogent rationale, a lack of focus on the patient's problems, and proscribe therapists from actions that most therapists would think therapeutic) or by therapist beliefs that one of the treatments is preferred and more effective. On the other hand, researcher allegiance may result in better outcomes for a specific treatment because the therapists have particular expertise; for example, Perla's therapists may be extraordinarily skilled at providing ACP and the extraordinary results (*vis-à-vis* either RC or CBT) demonstrate the efficacy of ACP when delivered by therapists who have allegiance and competence to deliver ACP (Leykin & DeRubeis, 2009). Whatever the explanation, the impact of allegiance on outcome demands that RCTs control for it in order to

be able to make valid inferences about the efficacy of treatments. Allegiance is best controlled when therapists are nested within treatment (each therapist gives only one treatment) such that the treatment is delivered by therapists who have an allegiance to the treatment they provide and are trained and supervised by experts in the respective treatments (see Wampold, 2001). For example, in a trial of process-experiential therapy versus CBT for depression, Watson, Gordon, Stermac, Kalogerakos, and Steckley (2003) ensured that their allegiance to process-experiential therapy was controlled by having experts in the two respective therapies train, supervise, and conduct the respective therapies and CBT was not altered in any way. RCTs and meta-analyses need to control for the allegiance effects if one is ever to sort out the nature of treatment differences.

Common Factors: Alliance and Therapist Effects

Having criticized the conclusion that there are negligible treatment differences, Siev et al. (2009) go on to argue that the evidence related to the common factors is flawed as well. Two common factors, alliance and the therapist, were called into question. Although Siev et al. highlight potentially important limitations to common factor research, we will show that their criticisms fail to acknowledge extant research evidence and minimize the evidence that supports the alliance and therapists.

Alliance. Siev et al. (2009) echo a variety of critiques of the alliance literature (see particularly DeRubeis et al., 2005), correctly noting that various meta-analyses have found that the alliance, measured early in therapy, is correlated with final outcome, with an estimate in the neighborhood of .22, which indicates about 5% of the variability in final outcome is associated with the alliance. What Siev et al. fail to mention is that Wampold determined that differences among treatments account for *at most* 1% (Wampold, 2001), so the 5% figure is quite impressive. Indeed, there is nothing—absolutely no other variable, except initial severity—that can be measured so early in therapy that correlates so highly with outcome. Siev et al. (2009) then go on to cite a few studies that demonstrate an effect that supports their argument and denigrate meta-analyses as means to understand the evidence of a corpus of studies on the alliance.

The evidence is clear: The alliance has been shown to be robustly predictive of out-

come across treatments, including psychopharmacology (Horvath & Bedi, 2002; Martin, Garske, & Davis, 2000; Wampold, 2001). Moreover, there is research that indicates that the alliance is not a result of early symptom change (Baldwin, Wampold, & Imel, 2007; Klein et al., 2003), although the evidence is not entirely conclusive. Without a doubt, alliance is difficult to study because levels of the alliance cannot be experimentally manipulated, but that does not preclude the possibility that the alliance is causal to outcomes in psychotherapy.

Interestingly, medicine is increasingly interested in relationship factors. For example, relationship factors between physician and patient have been found to reduce the duration of the common cold (Rakel et al., 2009). No therapist, whether psychodynamic, CBT, or humanistic, would suggest that their relationship with the patient and their collaboration around the goals and tasks of therapy, the elements of the therapeutic alliance, is unimportant. Clinical psychology will not be well served by minimizing the importance of relationship and collaboration. Let us agree to differ on whether the alliance is the primary cause of change or is necessary for the delivery of specific ingredients, as that is a question worthy of research effort.

Siev et al. (2009) raised an interesting point when they suggest that when therapists address the alliance, the “very focus on alliance becomes a treatment technique” (p. 74). This raises a critical issue in understanding common factors and specific ingredients. Common factors models as early as Jerome Frank's discussion of healing (Frank, 1961) have emphasized that a cogent rationale and therapeutic actions are essential ingredients of any healing practice (Wampold, 2007). Indeed, there are treatments for which work on the relationship with the therapist is the focus of treatment (e.g., Safran, Muran, Samstag, & Stevens, 2002). Common factor models posit that a cogent rationale and therapeutic actions of some type are necessary and Siev et al. seem to agree: Focusing on the alliance will be therapeutic if that is the focus of the treatment, in the same way that CBT is therapeutic by focusing on cognitions, ACP is therapeutic by focusing on affect, and dynamic therapies are therapeutic by focusing on the unconscious and attachment histories. We agree with Siev et al. that therapeutic techniques are needed, when they are endemic to a cogently constructed treatment that the therapist believes will be therapeutic. This is exactly why we have so

much difficulty with the poorly constructed “supportive counseling” type control groups—it seems the goal of these therapies is for the therapist to do their very best to do nothing (or not do something) while still providing some modicum of benefit to the patient. They do not control for important ingredients common to psychotherapy and they cannot be faithfully executed by therapists.

Therapist effects. Therapist effects, which are well established as noted by Siev et al. (2009), is an area where all psychotherapists, researchers, and patients covertly agree—the person of the therapist makes a difference. Rarely, if ever, are therapists randomly selected to deliver treatments in RCTs. Perla used effective therapists to deliver ACP, and the same is true of Clark et al. (1994). The important issue, and the one on which Siev et al. (2009) focuses, is determining the characteristics and actions of effective therapists. Siev et al. speculate about therapist differences: “Some therapists are likely more adept than others at using some techniques, formulating treatment plans, encouraging their patients to do difficult exposures, etc., even within CBT” (p. 74). In some ways, the evidence does not support this contention, as adherence to and competence in a particular treatment have not been particularly strong predictors of outcome (Schnurr et al., 2003; Shaw et al., 1999; Wampold, 2001). However, Siev et al.’s suggestion that effective therapists are skilled in persuading patients to follow the treatment protocol is well taken—this is exactly part of the alliance involved in collaborative agreement about the tasks and goals of therapy. That is, effective therapists are able to form alliances across a range of patients. This is remarkably consistent with Baldwin, Wampold, and Imel (2007), who found that it was the therapist’s contributions to the alliance that predicted outcomes and that the therapist’s ability to form an alliance (i.e., form a bond and *collaborate on the tasks and goals of therapy*) accounted for the therapist differences in outcomes. A recent study has shown that interpersonal skill of the therapist also accounts for therapist differences (Anderson, Ogles, Patterson, Lambert, & Vermeersch, in press).

If one is serious about studying therapist effects, then attention must be paid to the proper design of RCTs to provide valid estimates of these effects (Serlin, Wampold, & Levin, 2003; Wampold, 2001; Wampold & Serlin, 2000). If, as Siev et al. (2009) suggest, the goal is to use evidence from RCTs to improve the quality of routine care,

which is the basis of attempts to disseminate ESTs, then it is necessary for the RCTs to provide evidence that is generalizable to such settings. There is a reasonable debate to be had about whether clinical trials produce evidence that is generalizable (Stirman & DeRubeis, 2006; Westen, 2006), but what is clear is that the operations of the study must be sufficient to make generalizations—and this has unambiguous consequences for designing RCTs and estimating therapist effects. To be generalizable to therapists in routine care, therapists must be considered a random effect and should be representative of therapists to whom one wishes to generalize (Crits-Christoph & Mintz, 1991; Serlin & Lapsley, 1985; Serlin et al., 2003; Wampold & Serlin, 2000). Using a select group of therapists, as Perla and Clark have done in their trials, limits dramatically the inferences that can be made to how therapists in routine care might deliver the treatment (i.e., the notion of dissemination), particularly because in clinical trials the training, supervision, and monitoring of therapists would be extraordinary in routine care (clearly dissemination involves training, but not nearly at the level provided in clinical trials). Moreover, to estimate therapist effects, there needs to be sufficient numbers of therapists—they are considered to be drawn from a population of therapists. Just as the case for number of patients in a trial, there must be a sufficient number of therapists (at least 10) to make a reasonable estimation of the variability in outcomes. The partition of variance for Clark et al. (2006) in Siev et al.’s (2009) pie chart is problematic because the Clark trial contained only 4 therapists—nobody would make conclusions about a treatment with only four patients. The generalizability of any trial that uses an extraordinarily small number of therapists, particularly if the therapists were selected in a way that makes them unusual (e.g., they developed the treatment), received extraordinary training, supervision, and monitoring, is severely limited. Testing therapist effects in a study with less than 10 therapists is unlikely to be illuminating; ignoring therapist effects (i.e., not estimating therapist effects) in such studies does not improve the situation (actually, it makes it worse; see R. C. Serlin et al., 2003; Wampold & Serlin, 2000).

Conclusions

For a moment, let us talk about the points the various sides in the common versus specific factors debate agree on. It might

be safe to say that we agree that psychological treatments are effective. There is also agreement that to be effective, a treatment must have a cogent rationale, actions consistent with the rationale, and collaborative work on the goals and tasks of therapy—treatment is not simply listening to a patient. In Siev et al.’s (1990) terminology, techniques are necessary. There is also an agreement that it is imperative to improve the quality of services in routine practice. Such efforts can certainly involve continued work to develop, refine, and test treatments. Not surprisingly, there is also agreement that measuring outcomes in practice and using that information to improve the quality of services is a strategy that spans multiple perspectives on psychotherapy (Duncan, Miller, Hubble, & Wampold, in press; Lambert, Harmon, Slade, Whipple, & Hawkins, 2005; Miller, Duncan, & Hubble, 2005).

So, where is the divergence? Siev et al. (2009) and other EST advocates make some claims that not only appear to be contrary to the evidence, but also diverge sharply from the experience of therapists to whom they wish to disseminate the ESTs. First, the claim that treatments are specific rests on criticisms of previous meta-analyses that have been addressed, and on one meta-analysis that showed one treatment to be superior to another treatment for symptom measures only. And the one result that is offered to support specificity rests on a single and flawed study—not a reassuring bit of flotsam in a sea of evidence.

To add to the evidence, when control treatments without active ingredients are well designed with a rationale and therapeutic actions, they often are as, or nearly as, effective as “first-line” ESTs (Baskin et al., 2003; Cottraux et al., 2001; McDonagh et al., 2005). Moreover, dismantling studies rarely if ever identify a particular ingredient that is necessary for the efficacy of a treatment (Ahn & Wampold, 2001). Cognitive therapy for depression does not need the cognitive components (Jacobson et al., 1996), cognitive processing therapy for PTSD does not need the cognitive processing or the writing components (Resick et al., 2008), and CBT for PTSD does not need cognitive restructuring (Foa et al., 2005).

Establishing specificity requires the detailed display of system-specific demonstrations of mechanisms (Wampold, 2007) as well as a simple superiority of one treatment over another. Interestingly, there is evidence that CBT provides patients with coping skills that are vital to lasting benefits. In a

study of 35 clients who responded to CBT for depression, it was found that those clients who acquired cognitive coping skills and displayed evidence that they were using CBT principles had lower relapse rates during the year following treatment (Strunk, DeRubeis, Chiu, & Alvarez, 2007). This type of research is needed to clearly understand mechanisms of change in an EST.

Minimizing the importance of the alliance and of therapist effects, despite the evidence that exists, seems unnecessary. One can accept the importance of the alliance and therapists and remain committed to developing and improving treatments. To say that “if the goal of psychotherapy research is to determine the best ways to relieve suffering for the most people, researchers need to continue to focus on the areas that are most manipulable, such as technique” (Siev et al., 2009, p. 74) is to deny that there are ways to improve outcomes by focusing on areas *other* than technique. Indeed, Siev et al. go on to say that using efforts that “integrate ... the importance of the technique, alliance, and therapist factors” are needed (p. 74)—and we agree. It would be informative to know how therapists use feedback to improve performance: Do they alter techniques, address ruptures in the alliance, or engage patients in an examination of progress?

Instead of insisting that therapists learn and deliver ESTs, perhaps we should insist that therapists attain a reasonable benchmark regardless of the treatment they deliver (Minami et al., 2008). Therapists who are achieving outcomes comparable to or in excess of those achieved in clinical trials of ESTs might understandably be resentful of efforts to mandate the type of treatment they deliver.

For us, the issue—the driving force behind our work—is proper attention to the entire body of evidence. To be cast in the role of prevailing “in the broad court of professional opinion” on the basis of presenting a case “more aggressively to wide spread audiences” (Siev et al., 2009, p. 75), rather than on the basis of solid research evidence, is not a compliment many would want.

DiGiuseppe offered three alternatives: “Either we rebut these conclusions, conduct new research to show they are wrong, or we accept them and change our message” (as quoted in Siev et al., p. 71). Siev et al. have neither rebutted nor presented sufficient new research to reject the conjectures that (a) all treatments with cogent psychological bases, therapeutic actions consistent with the rationale of the treatment (i.e., techniques in Siev et al.’s language) delivered by

competent therapists who believe in the treatment, to patients seeking treatment, are equally effective, or (b) the therapeutic alliance and therapists are potent therapeutic ingredients. Therefore, consideration should be given to the DiGiuseppe’s third option, in light of the evidence.

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Correspondence to Bruce E. Wampold, Ph.D., University of Wisconsin-Madison, Department of Counseling Psychology, 244 Rust-Schreiner Halls, Madison, WI 53715-1150; wampold@education.wisc.edu

ABCT FAQ

What are the registration hours at the ABCT Annual Meeting in NYC?

- Thursday preregistration pick-up: 11:00 A.M. – 8:00 P.M.
- On-site registration: 3:00 P.M. – 8:00 P.M.
- Friday: 7:30 A.M. – 3:00 P.M.
- Saturday: 8:00 A.M. – 3:00 P.M.
- Sunday: 8:00 A.M. – 11:30 P.M.

CLASSIFIED

POSTDOCTORAL FELLOWSHIP IN ALCOHOL RESEARCH AT THE UNIVERSITY OF WASHINGTON. The fellowship will provide training for individuals who wish to pursue a career in alcohol research, with an emphasis on the etiology and prevention of problem drinking and alcohol dependence. For more information please see our website: <http://depts.washington.edu/cshrb/newweb/postdoc.html>

Book Review

Roemer, L., & Orsillo, S. M. (2009). *Mindfulness- and Acceptance-Based Behavioral Therapy in Practice*. New York: The Guilford Press.

Reviewed by Chad E. Drake, *University of South Carolina-Aiken*

Roemer and Orsillo have generated a thoughtful overview of acceptance-based behavioral therapies (abbreviated as “ABBTs” throughout the book) that the reader may find difficult to classify. It is not a textbook, though it provides a synthetic view of theory and frequently cites research findings when relevant. It is not a treatment protocol, though it presents a comprehensive guide for treatment activities and includes numerous case examples and transcripts. Rather, the book reads like a guided meditation on the authors’ combined knowledge and experience in practicing behavior therapy with mindfulness as a central treatment component.

The book is transtheoretical, drawing from and at times using terminology and techniques from Acceptance and Commitment Therapy, Dialectical Behavior Therapy, Mindfulness-Based Cognitive Therapy, Integrative Behavioral Couple Therapy, and Mindfulness-Based

Relapse Prevention, among others. The authors maintain a firm foothold in behavioral practice, consistently relying on a functional analysis of psychopathology. No particular diagnosis is given favor—the book provides a broad, comprehensive view of treatment that is readily adaptable to a variety of clinical presentations. An emphasis on theoretically driven processes and empirically supported treatment components is balanced with acknowledgment of limitations in the available data on these matters.

Their synthetic approach makes for a book with broad appeal, especially for those who are still in training or wishing to expand upon their training experience. Cognitive and behavioral practitioners will receive a thorough introduction to mindfulness techniques and interventions, while enthusiasts of mindfulness who lack a strong background in evidence-based treatment will benefit from solid behavioral theory and case conceptualization. Chapter topics

cover the gamut of concerns for a clinician. Chapters 1 through 3 discuss case conceptualization, assessment, and treatment planning. Chapters 4 and 5 focus on the relationship between therapist and client and the development of a therapeutic contract. Subsequent chapters expand upon the implementation of acceptance strategies (Chapter 6), values clarification (Chapter 7), the promotion of mindful and values-consistent behavioral activation (Chapter 8), and the monitoring of progress and termination of treatment (Chapter 9). Chapter 10 weaves in the relevance of knowledge and techniques from cognitive and behavioral traditions while Chapter 11 discusses the advantages of cultural sensitivity when conducting ABBTs with diverse clients. The book also contains an Appendix of recommended resources.

Overall, the book is readable, comprehensive, and aptly titled. Trainees and practitioners who have an interest would be hard-pressed to find a better resource than *Mindfulness- and Acceptance-Based Behavioral Therapies in Practice*.

Correspondence to Chad E. Drake, Ph.D.,
University of South Carolina, Aiken, 471
University Parkway, Aiken, SC 29801
ChadD@usca.edu

ABCT Annual Convention



Intensive Learning Opportunities

2 CE Hours Master Clinician Seminars

FRIDAY

8:15 a.m. – 10:15 a.m.

1. DBT Skills Training With Adolescents and Families: Teaching Points Dos and Dont's (Alec Miller & Jill Rathus)

10:30 a.m. – 12:30 p.m.

2. Treating GAD: Evidence-Based Strategies (William Sanderson)

12:45 p.m. – 2:45 p.m.

3. Cognitive Therapy for Paranoia (Neil Rector)

3:00 p.m. – 5:00 p.m.

4. Ending on a Positive Note: Mastery of the Ethics and Practice of Termination (Denise Davis)

SATURDAY

8:15 a.m. – 10:15 a.m.

5. Mindfulness for Two: An ACT Approach to Mindfulness in Psychotherapy (Kelly G. Wilson)

10:30 a.m. – 12:30 p.m.

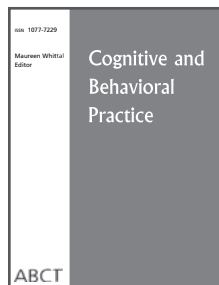
6. Introduction to Parent-Child Interaction Therapy (Cheryl McNeil)

12:45 p.m. – 2:45 p.m.

7. The Reality of Conducting VR Exposure Therapy: Expectations, Techniques, and Limitations (Mitchell Schare & Allen Grove)

3:00 p.m. – 5:00 p.m.

8. “Why Would I Ever Want to Feel That?” Overcoming Emotional Avoidance in Cognitive Therapy (Stephen Holland)



Looking to the Future of *Cognitive and Behavioral Practice*

Maureen L. Whittal, Editor, *Cognitive and Behavioral Practice*, and *University of British Columbia*

Communicating the complexity of treatment strategies can be difficult using the written word. However, technology today offers us the opportunity to enhance our learning experiences through the use of multimedia platforms such as video. The addition of video provides the advantage of an alternate avenue to learn, similar to attending a training workshop without leaving your office or home. Imagine being able to access video segments that illustrate how to roll with resistance, setting up and debriefing behavioral experiments, or how to respond to ruptures in the therapeutic alliance. Given the potential advantages, I am pleased to introduce the opportunity to augment *C&BP* manuscripts with video components.

The first two issues of Volume 17 in 2010 will contain invited contributions to illustrate the various ways in which this new application can be utilized. Similar to making reference to Tables and Figures in the body of a manuscript, authors will now have the opportunity to embed video clips at key points in the paper. In the first issue of 2010, the *C&BP* editorial team, including myself, Associate Editors Steve Safren and Joaquin Borrego Jr., and Book Editor Sabine Wilhelm, along with our colleagues will be presenting manuscripts that include the use of video. In the second issue, outgoing Publications Coordinator Phil Kendall and Jack Rachman and their colleagues will be presenting their recent work, augmented with video components.

The purpose of these video components is to concretize a concept or to illustrate the specifics of a treatment strategy. The focus of the video should be the clinician and not the patient. Secondary to confidentiality, we ask that real patients not be used. As you will see in the invited manuscripts, the videos are shot with actors or manuscript collaborators. The number of clips will be up to the author, but the length of each video should not exceed 7 minutes. Similar

to manuscripts, the video components will also undergo review. This review will be handled by the action editor upon acceptance of your manuscript.

To introduce the use of this technology, a video component is available and accompanies the electronic version of this manuscript at www.sciencedirect.com. If you are interested in submitting a manuscript that includes video clips, look for detailed instructions in the "Guide for Authors" section on the *C&BP* web pages at Elsevier, www.elsevier.com/locate/cabp. I believe that *C&BP* is the first psychology journal to make use of multimedia. It is my hope that it will enhance the visibility and utility of this unique journal.

In closing, I would like to thank you for your continued interest in the journal. I encourage you to continue submitting manuscripts (with or without videos) and reading the work of your colleagues. If you have feedback regarding the journal, be it positive or negative, we are always happy to hear it. Our ultimate goal remains the same: to be an enduring resource for scientist-practitioners interested in empirically supported approaches. With the imminent availability of multimedia manuscripts, we hope to stay on the cutting edge, for at least a little while!

...
Correspondence to Maureen Whittal, Ph.D., Vancouver CBT Centre, 708-777 W. Broadway, Vancouver BC V5Z 4J7 Canada whittal@interchange.ubc.ca.

Empirically Evaluated Self-Help Therapies

Patti Lou Watkins, *Oregon State University*, and George A. Clum, *Virginia Tech*

The June 2009 issue of *the Behavior Therapist* announced the advent of a "Seal of Merit" system to be applied to self-help books "that are consistent with CBT principles and that incorporate scientifically tested strategies for overcoming these difficulties" (p. 110). In line with the overarching theme of the June 2009 issue, this system seems designed to promote improved dissemination of empirically validated self-help programs to practitioners and the general public alike. As researchers in this field, we laud ABCT's move to educate consumers and promote the use of self-help materials with demonstrated efficacy, especially given the plethora of self-help products of dubious quality (Rosen, Barrera, & Glasgow, 2008; Watkins, 2008). We would also like to bring attention to another resource that aims to spur critical thinking about available self-help approaches and facilitate the dissemination of approaches that have scientifically demonstrated effectiveness: *Handbook of Self-Help Therapies* (Watkins & Clum, 2008).

As stated in the first chapter, which provides an overview of definitions, history, advantages, and limitations of self-help, "The lack of empirical evaluation of self-help materials is, in fact, the impetus for this text" (Watkins, 2008, p. 15). Following initial chapters describing various considerations in using self-help therapies as well as the theoretical underpinnings of this modality, the *Handbook of Self-Help Therapies* contains 11 chapters, each detailing the empirical evidence for self-help methods to treat psychiatric disorders such as depression and sexual dysfunctions as well as behavioral health problems such as cigarette smoking and weight management. In fact, over half of the inaugural titles that have received ABCT's "Self-Help Seal of Merit" are discussed and referenced in these chapters. The text also contains a chapter specifically addressing the integration of self-help ther-

apies in primary care settings. For a critical review of the *Handbook of Self-Help Therapies*, please see Wolleat (2008).

Indeed, lack of knowledge regarding which self-help interventions are effective and appropriate for a given problem is a barrier that has kept many clinicians from implementing this form of therapy. More alarmingly, many practitioners prescribe self-help books to their clients that advocate treatment techniques with no empirical basis (Adams & Pitre, 2000). Rosen et al. (2008), however, remind us that treatment techniques with empirically demonstrated efficacy, when implemented by a therapist in individual or group modalities, may not be similarly efficacious when translated into self-help formats. Thus, ongoing research of self-help programs, even those based on scientifically tested treatment strategies in other venues, is necessary. Wilson (2003) underscores the need for continued research, voicing a concern that the self-help industry, which is laden with nonvalidated products, may cast aspersions on the validated products that do exist. She likens this industry to a tumor, warning that it has “used the mental health profession as a host, but it has metastasized in ways that threaten to displace the profession itself” (p. 425).

In the final chapter of the *Handbook of Self-Help Therapies* (Clum & Watkins, 2008), we provide recommendations for future research that include both large-scale experimental designs as well as a return to single-case designs on which the field of be-

havior therapy was based, but seem to have given way to the former in recent years (Ollendick, 2006). These recommendations are consistent with Becker, Nakamura, Young, and Chorpita’s (2009) call in the June issue of *the Behavior Therapist* for practice-based evaluation of interventions. Lastly, our hope with the *Handbook of Self-Help Therapies* is “to achieve praxis, providing a resource that is both helpful to practitioners working directly with clients and inspiring to researchers seeking to extend the data base in this area” (Watkins & Clum, 2008, p. xii). As such, and again in line with Becker et al.’s recommendations, we envision this text as a potential resource in graduate training so that scientist-practitioners might become as well-versed on self-help interventions as they are in individual and group treatment modalities.

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Correspondence to Patti Lou Watkins, Ph.D., Women Studies Program, 200 Gilkey Hall, Oregon State University, Corvallis, OR 97331; pwatkins@oregonstate.edu

Call
for

WORKSHOP SUBMISSIONS

44th Annual Convention | November 18–21, 2009
San Francisco

Please send a 250-word abstract and a CV for each presenter to:

Carolyn M. Pepper, Ph.D.
University of Wyoming
Dept. of Psychology, Dept. 3415
16th and Gibbon
Laramie, WY 82071
or email: cpepper@uwyo.edu

DEADLINE for submissions: February 1, 2010

On Our Website

Meet ABCT’s Featured Clinicians of the Month

- Who are their mentors?
- How do they avoid burnout?
- What are they reading?
- Do they have any other talents?
- How do they stay current?
- Most important, what do their waiting rooms look like?

Don’t miss these in-depth interviews on ABCT’s home page. A different therapist is highlighted each month.

<http://www.abct.org>

To Lead or Not to Lead? That Is the Question

Kristene A. Doyle, *Albert Ellis Institute, Chair, Leadership & Elections Committee*

Believe it or not, nominating yourself or a colleague for office in ABCT is easier than getting to attend all that you want to at the Annual Convention! These are exciting times for ABCT. Edwin H. Friedman once said, "Leadership can be thought of as a capacity to define oneself to others in a way that clarifies and expands a vision of the future." Make this the year you take steps to guide your professional home and make a worthy contribution. If you ask members who have previously served in office, you'll find that many share similar reasons for doing so—they wanted to make a difference, and they did. So could you or someone you know. In addition to the inherent satisfaction achieved from contributing to ABCT, you have the opportunity to develop new friendships while reconnecting with old ones.

Those members who receive the most nominations will appear on the ballot. In April, full and new professional members in good standing vote on the candidates of their choice to serve for 3 years. The candidate elected as President serves as elect, sitting, and past. The candidate who wins the

Representative-at-Large position in the 2010 election serves as the liaison to Academic and Professional Issues. The individuals elected serve a year as "elect" to allow for all of the important particulars to be learned by the incoming officer. Once every 3 years a strategic planning meeting is held to assure that all elected members participate in at least one planning session during their term of office. The next scheduled strategic planning retreat will be held in 2011 to accommodate ABCT's cosponsorship of the 2010 World Congress with Boston University's Center for Anxiety Disorders and School of Social Work.

Last year marked the first time ABCT members could vote electronically. Feedback indicated that, overall, the process was convenient and effortless. Whether or not you run for office, we encourage you and your colleagues to vote for the individuals you believe will do the best job as soon as you receive your ballot.

How to get nominated. If you or someone you know possesses the skills, vision, and dedication to ABCT and its mission, then stop by the membership sign-up booth

at this year's convention and drop your nominations in the CALL FOR NOMINATIONS box. You can also mail in your form to ABCT's Central Office, or fax it to (212) 647-1865. We can't accept email nominations as original signatures are a requirement. All full members in good standing are eligible to be nominated. There is no limit to the number of members you may nominate.

You'll find the call for nominations form below, and in both the Winter and January issues of *tBT*. A more thorough description of each of the position appears in ABCT's bylaws at www.abct.org.

The individual elected as President-Elect (2010-2011) will serve as ABCT's President from 2011 to 2012. The Representative-at-Large candidate will serve November 2010 through November 2013.

The annual meeting of the Board takes place the Thursday of the convention, with monthly conference calls the remaining 10 months of the year. (There is no conference call in August.) The President, Immediate Past-President, President-Elect, and the Secretary-Treasurer comprise the Executive Committee. Conference calls are scheduled on an as-needed basis to ensure the continued efficiency of ABCT. It is expected that candidates have knowledge of ABCT's mission, its bylaws, strategic plan, and existing priorities. If you want this year to be the year you (or a colleague) give it a shot, please nominate yourself or a colleague.

NOMINATE the Next Candidates for ABCT Office

I nominate the following individuals
for the positions indicated:

PRESIDENT-ELECT (2010-2011)

REPRESENTATIVE-AT-LARGE (2010-2013)

NAME (printed)

SIGNATURE (required)

2010 Call for Nominations

Every nomination counts! Encourage colleagues to run for office or consider running yourself. Nominate as many full members as you like for each office. The results will be tallied and the names of those individuals who receive the most nominations will appear on the election ballot next April. **Only those nomination forms bearing a signature and postmark on or before February 1, 2010, will be counted.**

Nomination acknowledges an individual's leadership abilities and dedication to behavior therapy and/or cognitive therapy, empirically supported science, and to ABCT. When completing the nomination form, please take into consideration that these individuals will be entrusted to represent the interests of ABCT members in important policy decisions in the coming years. Contact the Leadership and Elections Chair for more information about serving ABCT or to get more information on the positions.

Please complete, sign, and send this nomination form to Kristene Doyle, Ph.D., Leadership & Elections Chair, ABCT, 305 Seventh Ave., New York, NY 10001.

Three Recipients of the Neil S. Jacobson Research Award for Outstanding and Innovative Clinical Research

Virginia Rutter, *Framingham State College*

Three talented young clinical researchers—Katherina Hauner, Sally Moore, and Eddie Selby—will receive the Neil S. Jacobson Research Awards for Outstanding and Innovative Clinical Research at 5:00 p.m. on Friday, November 20, 2009, during the awards and recognition ceremony at the ABCT Convention in New York City. The awards coincide with the 10th anniversary of Neil S. Jacobson's death—and with his 60th birthday earlier this year.

Neil Jacobson made original contributions in three areas of clinical psychology: couple therapy, depression treatment, and intimate partner violence. Much of his professional and personal sense of accomplishment, though, came from mentoring and training outstanding graduate students. These awards honor his contributions by recognizing and funding three innovative young scholars in clinical psychology.

The award recipients, Katherina Hauner (Northwestern University), Sally Moore (Seattle Veterans Administration Medical Center and University of Washington), and Eddie Selby (Florida State University), were selected from 50 submissions. The Awards Committee—consisting of Andrew Christensen (UCLA), Sona Dimidjian (University of Colorado, Boulder), Steven Hollon (Vanderbilt University), Bob Kohlenberg (University of Washington), and Virginia Rutter (Framingham State College)—sought to identify work that showed the kind of deep thinking about important problems and novel methodology that characterized Neil's early career.

About the Recipients

Katherina Hauner is a doctoral candidate in clinical psychology at Northwestern University, and is currently on clinical internship at the University of Illinois at Chicago. She received her B.S. from the University of Chicago in psychology, with a specialization in biopsychology. Her proposal, "Neuroanatomical Substrates of Fear Extinction During Exposure Therapy," fo-

cus on how the brain changes in response to effective psychotherapy. For this dissertation project (supervised by Sue Mineka at Northwestern University), Ms. Hauner will employ neuroimaging to examine the extinction of fear, by observing the brain changes that are associated with successful exposure therapy for spider phobia. During the study, participants who have lifetime diagnoses of spider phobia will receive a single, 2-hour session of exposure therapy; before and after the therapy, participants' neural response to spider images will be observed via fMRI, and the differences in the observed brain activity (pre- and post-therapy) will be compared. Ms. Hauner's research has been supported by grants from Northwestern University and a Society for the Science of Clinical Psychology Dissertation Award.

Sally Moore is a postdoctoral research fellow at the Seattle VA Medical Center's Mental Illness Research, Education, and Clinical Center. In 2008, she received her Ph.D. in clinical psychology from the University of Washington, where she studied memory difficulties associated with PTSD (under the mentorship of Lori Zoellner). Her graduate research was funded by a National Research Service Award from NIMH. Her primary research interests are in memory processes and emotion regulation difficulties associated with PTSD, exposure-based therapies, and treatment development. Dr. Moore will be working on "Specific Memory Retrieval Practice in Veterans With PTSD and Depression" (Tracy Simpson at the Seattle VA Medical Center is her sponsor for this study). In this project, Dr. Moore addresses how, in addition to intrusive memories of trauma, individuals with PTSD have difficulties retrieving specific memories of non-traumatic life events when they are intentionally trying to do so. This difficulty appears to contribute to the maintenance of PTSD symptoms over time and may represent a vulnerability factor for the development of PTSD. Dr. Moore's study will examine whether brief training to address

PTSD-related impairment in autobiographical memory specificity leads to short-term changes in memory specificity, PTSD and depression symptoms, and vulnerability factors for the disorder. Results of this research may suggest novel treatments, treatment components, or preventative interventions for PTSD that involve improving specificity of retrieval.

Eddie Selby is a Ph.D. candidate in clinical psychology at Florida State University; he received B.A. and B.S. degrees in psychology and physiology/neuroscience at the University of Wyoming in 2005. He is primarily interested in emotion dysregulation as it relates to borderline personality disorder, suicidal behaviors, and eating disorders. His work on emotional cascades and behavioral dysregulation has been published in the *Journal of Abnormal Psychology*, *Review of General Psychology*, and *Behavior Research and Therapy*. Mr. Selby's project is "A Real Time Evaluation of Emotional Cascades and Dysregulated Behaviors in Borderline Personality Disorder" (supervised by Thomas Joiner at Florida State University). Mr. Selby, mindful that individuals with borderline personality disorder (BPD) may engage in many impulsive/dysregulated behaviors as a way of inhibiting emotional cascades and rumination can amplify negative emotion to an unbearable state in order to get some relief, devised a study that explores the relationship between emotional cascades and dysregulated behaviors in individuals with BPD. The study will use experience sampling, in which participants complete records of interpersonal experiences, emotions, thoughts, and behaviors over a period of multiple days using Palm Pilots. It is expected that intense fluctuations of rumination and negative affect will arise from interpersonal stressors, and that these fluctuations will significantly predict dysregulated behaviors in those with BPD.

These recipients will be at the ABCT meeting to receive their Neil S. Jacobson Research Award for Outstanding and Innovative Clinical Research. In addition, Ms. Hauner will be presenting her work on fear extinction in a symposium on Sunday, November 22, at 11:30 a.m.; Dr. Moore will be chairing and presenting at a symposium on information processing in PTSD on Saturday, November 21, at 3:00 p.m.; and Mr. Selby will be presenting a symposium on self-injury in BPD on Sunday, November 22, at 9:45 a.m. Please join us in honoring these young scholars, and for remembering Neil S. Jacobson and his many contributions. ■

Call

for Award Nominations

The ABCT Awards and Recognition Committee, chaired by Shelley Robbins of Holy Family University, is pleased to announce the 2010 awards program. Nominations are requested in all categories listed below. Please see the specific nomination instructions in each category.

Please note that award nominations may not be submitted by current members of the ABCT Board of Directors.

Outstanding Contribution by an Individual for Research Activities

Eligible candidates for this award should be members of ABCT in good standing who have provided significant contributions to the literature advancing our knowledge of behavior therapy. Past recipients of this award include Alan E. Kazdin in 1998, David H. Barlow in 2001, Terence M. Keane in 2004, and Thomas Borkovec in 2007. Please complete an on-line nomination form at www.abct.org. Then, e-mail the completed forms to srobbins@holyfamily.edu. Also, mail a hard copy of your submission to ABCT, Outstanding Researcher, 305 Seventh Ave., New York, NY 10001.

Outstanding Mentor

This year we are seeking eligible candidates for the Outstanding Mentor award who are members of ABCT in good standing who have encouraged the clinical and/or academic and professional excellence of psychology graduate students, interns, postdocs, and/or residents. Outstanding mentors are considered those who have provided exceptional guidance to students through leadership, advisement, and activities aimed at providing opportunities for professional development, networking, and future growth. Appropriate nominators are current or past students of the mentor. The first recipient of this award was Richard Heimberg in 2006, followed by G. Terence Wilson in 2008. Please complete an on-line nomination form at www.abct.org. Then, e-mail the completed forms to srobbins@holyfamily.edu. Also, mail a hard copy of your submission to ABCT, Outstanding Mentor, 305 Seventh Avenue, NY, NY 10001.

Student Dissertation Awards:

- The Virginia A. Roswell Student Dissertation Award
- The Leonard Krasner Student Dissertation Award

Each award will be given to one student based on his/her doctoral dissertation proposal. The research should be relevant to behavior therapy. Accompanying this honor will be a \$1,000 award to be used in support of research (e.g., to pay participants, to purchase testing equipment) and/or to facilitate travel to the ABCT convention. Eligible candidates for this award should be student members who have already had their dissertation proposal approved and are investigating an area of direct relevance to behavior therapy, broadly defined. A student's dissertation mentor should complete the nomination. Please complete an on-line nomination form at www.abct.org. Then, e-mail the completed forms to srobbins@holyfamily.edu. Also, mail a hard copy

of your submission to ABCT, Student Dissertation Awards, 305 Seventh Ave., New York, NY 10001.

Distinguished Friend to Behavior Therapy

Eligible candidates for this award should NOT be members of ABCT, but are individuals who have promoted the mission of cognitive and/or behavioral work outside of our organization. Applications should include a letter of nomination, three letters of support, and a curriculum vitae of the nominee. Past recipients of this award include Jon Kabat-Zinn, Nora Volkow, John Allen, Anne Fletcher, Jack Gorman, Art Dykstra, and Michael Davis. Please complete an on-line nomination form at www.abct.org. Then, e-mail the completed forms to srobbins@holyfamily.edu. Also, mail a hard copy of your submission to ABCT, Distinguished Friend to BT Award, 305 Seventh Ave., New York, NY 10001.

Career/Lifetime Achievement

Eligible candidates for this award should be members of ABCT in good standing who have made significant contributions over a number of years to cognitive and/or behavior therapy. Applications should include a letter of nomination, three letters of support, and a curriculum vitae of the nominee. Past recipients of this award include Albert Ellis, Leonard Ullman, Leonard Krasner, Steve Hayes, and David H. Barlow. Please complete an on-line nomination form at www.abct.org. Then, e-mail the completed forms to srobbins@holyfamily.edu. Also, mail a hard copy of your submission to ABCT, Career/Lifetime Achievement Award, 305 Seventh Ave., New York, NY 10001.

NOMINATIONS FOR THE FOLLOWING AWARD ARE SOLICITED FROM MEMBERS OF THE ABCT GOVERNANCE:

Outstanding Service to ABCT

Members of the governance, please complete an on-line nomination by visiting www.abct.org. Then, e-mail the completed forms to srobbins@holyfamily.edu. Also, mail a hard copy of your submission to ABCT, Outstanding Service to ABCT Award, 305 Seventh Ave., New York, NY 10001.

Questions? Contact: Shelley Robbins, Ph.D., Chair, ABCT Awards & Recognition Committee; e-mail: srobbins@holyfamily.edu

Nominate on line: www.abct.org
Deadline for all nominations:
Monday, March 2, 2010

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 Kathryn DeYoung
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 Ashley Marie Smith
 Jonathan P. Stange

Students

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 Dominic C. Mocerri
 Oswaldo Moreno

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Hillary E. Nammack
Aaron Martin Norr
Kathleen A. O'Malley
Adjoa T Osei
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Invited Panel
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Overcoming the Glass Ceiling—Lessons Learned and Lessons to Give: A Conversation With the Trailblazers

MODERATORS: Lata K. McGinn, *Yeshiva University*, and Michelle Newman, *Pennsylvania State University*

PANELISTS: Dianne Chambless, Edna Foa, Robin Jarrett, Marsha Linehan, Barbara McCrady,
Susan Mineka, Rosemary Nelson, Patricia Resick, Antonette Zeiss

Friday, November 20 | 2:45 P.M. | Broadway North

This unique panel will address the following topics:

- The impact of being a woman, including decisions made and challenges or adversities faced during participants' career building years;
- Ways in which participants prevented or overcame any adversities or challenges faced;
- Potential challenges faced by participants and women professionals in general today and how they may differ from early challenges;
- Lessons that emerging female professionals can learn from experiences faced by participants;
- Discussion of strategies to further break the glass ceiling: (e.g., mentorship of younger female professionals, development and maintenance of informal networks (an old girl's club), raising awareness of subtle biases faced by women today).



the Behavior Therapist
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● INTERNSHIP PANEL

Internship Training Site Overview | *Jeanette Hsu & Justin Nash*

Friday, Nov. 20, Manhattan Ballroom, 8:45 a.m.

● POSTGRADUATE PANEL

Postdoctoral Paths for Professional Development | *Richard Seime & Antonette Zeiss*

Friday, Nov. 20, Marquis B & C, 10:30 a.m.

● MEMBERSHIP PANEL DISCUSSION

What Every Graduate Student, Postdoc, and Early Career Professional Needs to Know About the NIHM Loan Repayment Programs | *Borrego et al.*

Friday, Nov. 20, Cantor/Jolson, 1:00 p.m.