

the Behavior Therapist

**SPECIAL
ISSUE**

PSEUDOSCIENCE in Mental Health Treatment

Special Issue Editor: R. Trent Codd, III

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Introduction to the Special Issue: Pseudoscience in Mental Health Treatment: What Remedies Are Available • 1

David N. Rapp and Amalia M. Donovan

The Challenge of Overcoming Pseudoscientific Ideas • 4

David Trafimow

The Scientist-Practitioner Gap in Clinical Psychology: A Social Psychology Perspective • 12

William O'Donohue

Science and Epistemic Vice: The Manufacture and Marketing of Problematic Evidence • 19

William C. Follette

Pseudoscience Persists Until Clinical Science Raises the Bar • 24

Clara Johnson, Shannon Wiltsey-Stirman, and Heidi La Bash

De-implementation of Harmful, Pseudoscientific Practices: An Underutilized Step in Implementation Research • 32

Stuart Vyse

What's a Therapist to Do When Clients Have Pseudoscientific Beliefs? • 36

Dean McKay

The Seductive Allure of Pseudoscience in Clinical Practice • 39

Scott O. Lilienfeld, Steven Jay Lynn, and Stephen C. Bowden

Why Evidence-Based Practice Isn't Enough: A Call for Science-Based Practice • 42

Lisa A. Napolitano

Pseudotherapies in Clinical Psychology: What Legal Recourse Do We Have? • 47

Monica Pignotti

Exposing Pseudoscientific Practices: Benefits and Hazards • 51

Introduction to the Special Issue

Pseudoscience in Mental Health Treatment: What Remedies Are Available?

R. Trent Codd, III, Cognitive-Behavioral Therapy Center of WNC, P.A.

MANY MENTAL HEALTH professionals deliver interventions that are unsupported by science. These interventions range from inert to harmful. In addition, many consumers of psychological services espouse confidence in scientifically unsound theories and their associated interventions. The behavioral consequences of such confidence is frequently consumer pursuit of unhelpful treatment, often to the exclusion of treatments with empirical support. Clinician and consumer allegiance to unsubstantiated treatments is a major barrier to the optimal care of persons with psychological difficulties.

An example of how pseudoscience has interfered in my own clinical practice is instructive. There is widespread agreement in the scientific community that exposure and response prevention (ERP), which has been available for decades, is the gold-standard treatment for obsessive-compulsive disorder (OCD). Yet, it is unclear whether most persons with OCD receive ERP rather than treatments not indicated or even contraindicated in the treatment of OCD. Many anecdotes illustrative of this problem are available for sharing. Also available are examples of patients involved in ERP who simultaneously received competing advice that undermined their treatment and did not comport with the scientific database pertaining to OCD. One salient anecdote involves a former patient of mine with particularly severe OCD symptoms. During my attempt to deliver ERP to him, this patient was variously advised to

[Contents continued on p. 2]

[continued on p. 3]

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Contents, continued

At ABCT

- Classified • 54
- Call for Nominations for ABCT Officers • 55
- Awards & Recognition Ceremony, 2017 • 56
- Call for Award Nominations, 2018 • 58

52nd ANNUAL CONVENTION

- Preparing to Submit an Abstract • 60
- Understanding the ABCT Convention • 61
- Call for Continuing Education Sessions • 62
- Call for Papers (General Sessions) • 63
- ABCT and Continuing Education • 64

INSTRUCTIONS for AUTHORS

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- Feature articles that are approximately 16 double-spaced manuscript pages may be submitted.
- Brief articles, approximately 6 to 12 double-spaced manuscript pages, are preferred.
- Feature articles and brief articles should be accompanied by a 75- to 100- word abstract.
- Letters to the Editor may be used to respond to articles published in *the Behavior Therapist* or to voice a professional opinion. Letters should be limited to approximately 3 double-spaced manuscript pages.

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seek chiropractic care, neurofeedback, and even allergy shots! For clarity, those interventions were all recommended specifically for his OCD. Sadly, I was not particularly persuasive and, despite my recommendation not to do so, this individual pursued each of these interventions, one after the other, as each failed in turn. Notably, the patient neglected ERP as he worked through this sequence of treatments. Also notable is that this patient's OCD symptoms were so impairing that he was unable to maintain employment and thus he struggled financially. He was not able to compensate with monetary assistance from his family because they did not possess robust financial resources. However, his financial obstacles did not impede his pursuit of the recommended interventions. Although all of these interventions were expensive, the allergy shots were particularly costly because they entailed travel costs (e.g., airfare, accommodations) as the provider of this intervention resided out of state. This patient never returned to me for treatment, so his terminal outcome is unknown. However, my belief, based on the science, is that the odds of treatment success with ERP at my office were favorable.

The problem of pseudoscience in mental health treatment is not new, unfortunately. Scientifically minded practitioners have directed their attention to this problem. One of the primary approaches to addressing this problem involves the application of critical analyses to various pseudoscientific methods followed by the dissemination of these analyses to consumers and professionals. The hope, of course, is that these analyses will impact the behavior of practitioners and their clientele. Whether this approach is effective is dubious, yet it seems to be the dominant strategy pursued historically. For example, when soliciting manuscripts for this special issue, even a well-known pseudoscientific treatment debunker had difficulty imagining how he could contribute without "taking down certain approaches" specifically by name. This seems to be the common way of approaching this problem among well-intentioned scientists.

My objection to the debunking model is not a moral one. Rather, given the abundance of pseudoscience, it seems safe to conclude that a debunking model isn't particularly effective. Even if it were successful, it's not a practical solution because there are simply too many pseudoscientific interventions to address one-by-one. If one considers the rate at which new pseudoscientific

treatments seem to propagate, the debunking model can only result in an endless game of whack-a-mole. Other strategies have been tried too, of course, including various forms of advocacy, education campaigns, and legislative efforts. Yet, the problem remains.

The primary objective of this special issue is to explore alternatives to the pure debunking model. The contributors' academic disciplines differ, affording fresh perspectives stemming from their unique and varied vantage points. Experimental psychologists Rapp and Donovan (this issue) open the issue with a presentation of an experimental literature that can inform the construction of interventions targeted at the remediation of pseudoscientific beliefs. Next, Trafimow (this issue) provides a social psychological perspective and addresses two main areas. First, he suggests improvements in the science of clinical psychology, an area also emphasized by other contributors to this special issue. Second, he recommends a line of research focused on practitioner behavior change using the Reasoned Action Approach (Fishbein & Ajzen, 2010).

O'Donohue (this issue) and Follette (this issue) focus on research methods in clinical psychology. More specifically, O'Donohue introduces the concept of epistemic virtue and suggests that it has not received adequate attention in CBT research. He then underscores its importance and provides recommendations for improving its presence in CBT science. Follette (this issue) argues that the historical emphasis on efficacy studies in clinical psychology to the exclusion of tests of mechanisms of change has allowed pseudoscientific interventions to persist by claims of effectiveness.

Johnson, Wiltsey-Stirman, and La Bash (this issue), coming from the vantage point of dissemination and implementation researchers, discuss de-implementation or the discontinuation of previously implemented practices. They consider the generalization of de-implementation models for addressing the problem of pseudoscientific practices.

Next, behavior analyst Stuart Vyse (this issue) addresses the problem of clients who are committed to non-evidence-based therapies. He offers several strategies for reasoning with these types of clients based on recent research on effective discrediting of misinformation.

This is followed by McKay (this issue), who contemplates why mental health professionals may be particularly susceptible

to pseudoscientific psychotherapy and offers some recommendations for remediation. Lilienfeld, Lynn, and Bowden (this issue) then note that evidence-based practice (EBP) has not been particularly successful in impeding the spread of pseudoscience in psychotherapy. Consequently, they introduce and argue for science-based practice as an alternative to EBP.

Then, Napolitano (this issue), trained in both clinical psychology and law, approaches the problem from a legal perspective. She makes the case that professional associations and government agencies have been ineffective in protecting consumers and the mental health professions from the negative impact of pseudotherapies. Consequently, she emphasizes the value of exploring legal options and suggests a specific legal strategy.

Finally, Pignotti (this issue) provides us with an account of her efforts in exposing harmful practices and the high personal and professional costs of her having done so. There's much to be learned, as well as admired, from a reading of this history. She concludes by providing her reflections of what might be learned from her experience.

The problem of pseudoscience in mental health treatment is significant. Please do not read these articles and then fall into inaction. Allow these articles to stimulate action: Share them widely, execute the actionable items they suggest, and/or initiate a new line of empirical work based on their content. Numerous suffering human beings are counting on you.

Reference

- Fishbein, M., & Ajzen, I. (2010). *Predicting and changing behavior: The reasoned action approach*. New York, NY: Psychology Press (Taylor & Francis).

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The Challenge of Overcoming Pseudoscientific Ideas

David N. Rapp and Amalia M. Donovan, *Northwestern University*

PEOPLE HOLD MANY different kinds of beliefs. Some are rooted in direct experiences, such as that at the end of the day the sun will set, and in the morning the sun will rise in the sky. Others are derived from explanations and evidence communicated by outside sources, such as learning in school that the world is round. The hope is that our direct experiences with the world, and the knowledge provided by others, will converge and be accurate, such that we can use what we have learned to make decisions and solve problems successfully in the future. The problem, unfortunately, is that our direct experiences can encourage beliefs that are incorrect (diSessa, 1993; Vosniadou & Brewer, 1994), and information provided by others can be wrong (Garrett, Weeks, & Neo, 2016; Rapp & Braasch, 2014).

For example, consider standing on the edge of a beach, peering out at the water. In the distance you can see the horizon. This perceptual experience can suggest that the world just ends; it isn't curved, but rather seems to drop off at some distance far away. What we are seeing does not accurately inform us as to the actual shape of the Earth. Also consider that there are groups that subscribe to the incorrect idea of a flat Earth, presenting the view with anecdotes and personal tests intended to raise skepticism that we do not live on a spherical planet (e.g., the Flat Earth Society). Our direct experiences, and the information supplied by other people, as exemplified in this case, can inform inaccurate beliefs about the world.

This case also provides an illustrative example of pseudoscience, which we can define as a set of claims, beliefs, and practices that invoke notions of scientific investigation but that are actually based on misunderstandings and misapplications (sometimes intended and sometimes not) of the scientific method. Pseudoscientific conjectures lack and often run counter to scientific claims derived from accumulated and generally accepted evidence (Lobato, Mendoza, Sims, & Chin, 2014). Some pseudoscientific beliefs have their bases in naïve preconceptions about biology, physics, and chemistry (e.g., Vosniadou & Brewer, 1992). Advocates of pseudoscientific beliefs

also often reject wholesale the need for science by disregarding consideration of experimental controls, the importance of accumulated evidence, and the theoretical supports underlying empirically based claims. In efforts to reject scientific consensus and to promote their beliefs as valid alternatives, these advocates often contend that nobody can actually know the truth, that evidence and experiments can be biased (sometimes invoking conspiratorial stances), and that school-supplied understandings of the world are derived from book claims rather than from what experience tells us (Lewandowsky, Gignac, & Oberauer, 2013; Lewandowsky, Oberauer, & Gignac, 2013). Contemporary concerns about the growth of pseudoscience are becoming increasingly worrisome, linked to recent sociopolitical events, the ease of publishing information through online sources, and concerns about journalistic investments and integrity (Kahne & Bowyer, 2017; Lewandowsky, Ecker, Seifert, Schwarz, & Cook, 2012).

Pseudoscientific beliefs can have important consequences for everyday behaviors and decisions, including our health and well-being. Consider one particular pseudoscientific belief—the notion that mental illnesses are contagious maladies that you can catch from another person, similar to the cold or the flu. We highlight this particular belief for three reasons. First, the topic connects with the theme of this special issue as considered in other articles in this volume. Second, this belief has received extended examination in the psychological literature, as accounts attempt to highlight factors associated with possessing it, as well as potential outcomes associated with such thinking (Marsh & Shanks, 2014). Third, this belief is one of a series of incorrect assertions that we have explicitly tested in our own research focused on the consequences of exposure to inaccurate information. Focusing on this belief helps highlight the broader consequences of learning about false information as identified in empirical projects (e.g., Marsh, Meade, & Roediger, 2003; Rapp & Braasch, 2014), and is situated with awareness of and respect for work on mental health treatment. In our analysis, we show how expo-

sure to this pseudoscientific claim can have problematic consequences. Our discussion then focuses on processes of memory and learning that should, under most circumstances, support successful comprehension, but that can also result in uptake and reliance on inaccurate information. Articulating the contributions of these processes for comprehension helps identify conditions and activities that may help reduce reliance on inaccuracies (Rapp, 2016). We conclude by outlining other factors that, in concert with these processes, contribute to the pervasive effects of pseudoscience. Our work attempts to identify these contributions so as to inform theoretical accounts of pseudoscientific thinking, and to support the design of interventions intended to combat the acquisition and persistence of inaccurate beliefs.

Consequences of Exposure to Inaccurate Information

To begin, consider the following excerpt from a story in which a conversation between two characters, Dane and Brad, turns to the topic of mental illness:

As quickly as Brad had become excited, he calmed down . . . "Well," he said, "if I'm crazy, it's only because you were crazy first and you keep breathing on me all the time — I caught it from you."

Dane laughed and said, "I bet you think you're being funny."

"Right now, I'm just being brain-dead."

Dane forged ahead: "No, really, there's now evidence that you can catch some forms of mental illness from your friends and loved ones. . . . I was really amazed when I read this stuff. . . . They now have shown that there are some mental troubles that are passed through the air."

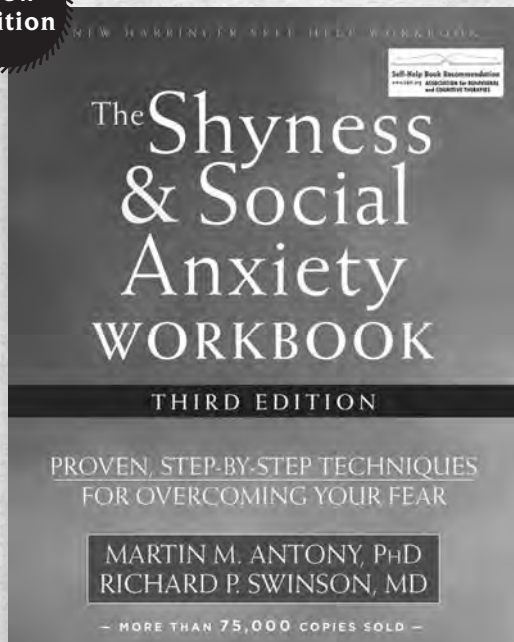
"Mental troubles?"

"Sure — paranoia, hallucinations, fits. All the good stuff. You never know what you'll breath in nowadays. You could catch almost anything just by being breathed on by the wrong person. It's amazing that more people aren't aware that mental illness can be highly contagious."

In a series of experiments (Rapp, Hinze, Kohlhepp, & Ryskin, 2014), participants

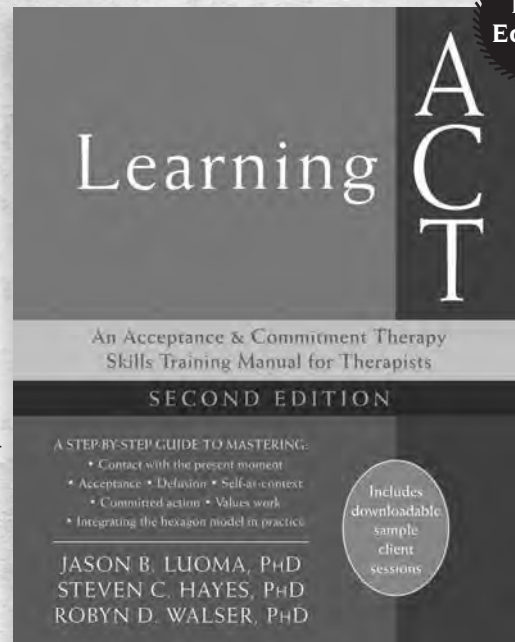
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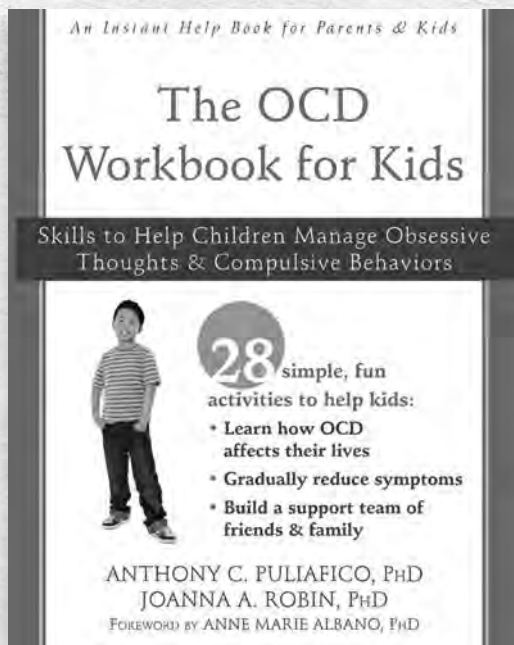


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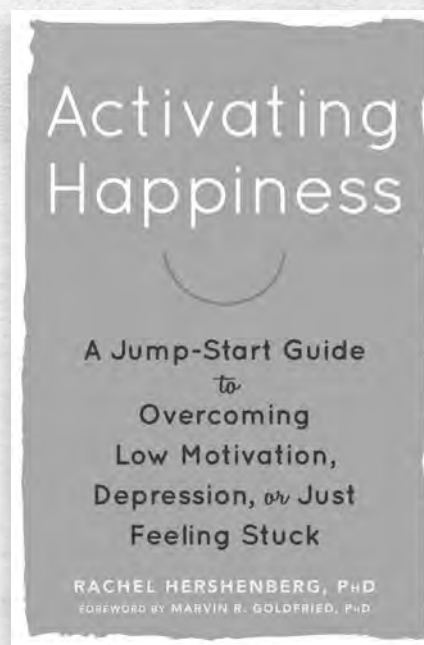
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were asked to read a 19-page story, almost 8,000 words in length, that potentially contained this excerpt, as well as other conversations between characters, none of which were integral to the plot. Some of the conversations contained inaccurate assertions about the world, as in this example, while other conversations offered more valid assertions. Two versions of the story were constructed, with participants assigned to read one or the other, to counterbalance the 16 presented assertions for accuracy (i.e., 8 of the assertions in each version were presented in an inaccurate form, with the remaining 8 presented in an accurate form). After reading one version of the story, participants completed a distractor task to discourage rehearsal and reflection on the story contents. Finally, participants were presented with a series of statements and asked to indicate whether each statement was true or false. This validity judgment task included statements that referenced ideas offered in the 16 critical assertions, and was administered as a mean of assessing whether the story content influenced participants' postreading considerations of assertion content. Two versions of the validity judgment task were created such that half of the statements were presented as true and the remaining half were presented as false.

With respect to our example, half of the participants read the story, including the above excerpt (as well as 15 other assertions), while the other half read a version that rejected the notion of mental illness as being contagious with similar linguist content (and again, along with 15 other assertions). During the judgment task, for half of the participants one of the test items asked them to determine whether the statement, "Most forms of mental illness are contagious" was true or not, while the other half of the participants were asked to judge the statement, "Most forms of mental illness are not contagious." Again, this is only one of a range of test statements included for all participants in the task.

The results indicated that participants who previously read inaccurate assertions were more than twice as likely to make incorrect validity judgments, regardless of the kind of test statement they were asked to evaluate (i.e., true or false), as compared to participants who previously read accurate assertions. Specifically, if participants read the earlier excerpt, they showed greater difficulty rejecting the claim, "Most forms of mental illness are contagious," as well as greater difficulty accepting the statement, "Most forms of mental illness are not

contagious," as compared to participants who read a version of the story in which the assertion about mental illness being contagious was discussed by the characters as being obviously wrong. What is noteworthy about this finding is that the assertions used in these experiments had been previously normed with members of the population from which participants were sampled (i.e., undergraduate psychology students at Northwestern University), which indicated they should have been familiar with and known which version of the claim was accurate. Yet despite their accurate prior knowledge concerning the potential transmission of mental illness, participants' decisions were contaminated by what they read.

These results have been replicated a variety of times and emerge across the different assertions used in the texts (e.g., Seat belts do/do not save lives; Brushing your teeth can lead to/prevent gum disease; Aerobic exercise strengthens/weakens your heart and lungs; e.g., Gerrig & Prentice, 1991; Prentice, Gerrig, & Bailis, 1997). Besides assertions, similar problematic effects emerge when participants are presented with inaccurate declarative statements (e.g., The Pilgrims sailed to America on the Mayflower/Godspeed; The scientist who discovered radium was Curie/Pasteur; Abraham Lincoln was assassinated by Booth/Oswald), which can subsequently be used to answer related questions (e.g., Hinze, Slaten, Horton, Jenkins, & Rapp, 2014; Marsh 2004). The accumulated results indicate being exposed to inaccurate information negatively impacts people's attempts to make decisions and answer queries involving that same information, even when they should know better.

Mechanisms That Influence Reliance on Inaccurate Information

Recent work has articulated underlying cognitive processes associated with memory, language, and comprehension that contribute to people's reliance on patently inaccurate information (Marsh, Cantor, & Brashier, 2016; Rapp & Braasch, 2014; Rapp & Donovan, in press; Rapp, 2016). To be clear, these processes support the development of accurate understandings, as they facilitate the encoding and retrieval of correct information people have experienced. The challenge is that these processes operate generally, with problematic consequences when people are exposed to inaccurate information. To exemplify this issue, we discuss here two

features of routine cognition that contribute to these effects.

Fluency

Our judgments about what we know, and the degree to which what we know is accurate or requires additional contemplation and consideration, is influenced by a host of factors. One factor that has received substantial empirical investigation is the ease with which people feel they can access information from memory. This is defined as fluency, and our feelings as to how fluently we can retrieve our existing understandings and recall what we have experienced also informs expectations as to how valid we consider that information (Oppenheimer, 2008). Feelings of fluency are often useful as information that we are more familiar with and have thought more about is often information we should indeed feel confident in accepting and reporting. Information that is easily retrieved from memory is often considered to be more true than is information for which we have to exert effort and deliberately search memory to consult (Fazio, Brashier, Payne, & Marsh, 2015). Accurate information should be more easily available than inaccurate, inappropriate information; it should be the information we can quickly deliver and apply when we need it.

The challenge is that a variety of cues can confer feelings of fluency that inappropriately invoke such confidence (Reber & Schwarz, 1999; Unkelbach, 2007). As such, information that is retrievable can be mistakenly believed to also be true, or more modestly, is less likely to be submitted to careful evaluation and rejection. Consider, for example, having recently read a text promoting the claim that mental illness is contagious. Memory traces for that recently encoded information are now more available for retrieval than other, less recently experienced ideas and events. People can misattribute the phenomenological feeling that the information is easily available as an indicator that the information is valid. This misattribution process is thus a potential contributor to people's use of inaccurate information, as well as a routine consequence of the normal operation of memory.

We might expect that fluency effects based on the recency with which we have experienced information would fade, as encoded information, when unrehearsed, becomes more difficult to retrieve after going unconsidered for some time. While this is a reasonable inference, it would

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necessitate individuals never being exposed to the inaccurate information again so as to allow those earlier acquired memory traces to decay or at least be less accessible given less attention. This may never actually happen in the real world, though, despite being a condition that could be usefully set up and studied in a lab setting. In the real world, people are often exposed and reexposed to inaccurate information. For example, imagine reading a post on social media presenting a pseudoscientific argument for mental illness being contagious. Such information is often repeatedly reposted by others, making it more likely that it will not just be seen once but several times. Repeated exposure to inaccuracies can bring with it increased feelings of validity in at least two ways. First, repetition helps ensure the information remains recently experienced. Second, repeated exposures can make the information feel more familiar, with ready familiarity also conferring feelings of fluency (McGlone & Tofiqbakhsh, 2000). The consequence is that repeated experience with an inaccuracy can be misattributed as meaning the information is more true, or that it should be relied upon, or that it might be recruited in future considerations about the same topic.

To summarize, feelings of fluency can convey information about the validity of information in ways that are inappropriate. Those feelings can be driven by the recency with which we have experienced information, and the degree to which we have been repeatedly exposed to that information. Political groups and news agencies often take advantage of these feelings, sometimes intentionally and sometimes without awareness of the consequences. These cues are also often explicitly associated with techniques that advertisers, lawyers, and authors, among other groups, rely upon to convince, persuade, and entice their audiences (Johar & Roggeveen, 2007; Sundar, Kardes, & Wright, 2015). All of these cases could involve pseudoscientific claims. These cues are often useful for informing feelings as to whether information should be trusted and whether it might be true, of course. However, in many circumstances, those cues are at best uninformative and at worst misleading.

Source Monitoring

Another issue relevant to people's experiences with information is that they do not seem to be particularly adept or systematic at tagging information as accurate or inaccurate. In the best of situations, people

should carefully evaluate information, skeptically contemplate what they read, see, or hear, and recruit relevant knowledge to reject information that is incorrect. Outside of the issue that people often do not have the appropriate knowledge to conduct such evaluations, they also do not routinely engage in careful appraisals of information content or of the sources providing that content even when they should. And if they do engage in such activity, the products of their evaluations are not guaranteed to lead to careful encodings of credibility or accuracy (Isberner & Richter, 2014).

For example, when participants are presented with information from a source that should not be considered reliable, unless they receive instructions, repeated reminders, and guidance to reflect upon and base decisions on source credibility, their subsequent understandings do not seem to include an acknowledgement of a lack of credibility (Sparks & Rapp, 2011). Several studies have shown that readers do not outright reject information from unreliable sources, unless the credibility of those sources is explicitly identified and associated with performance concerns or repeated reminders (Andrews & Rapp, 2014). Source monitoring, the process by which individuals encode information about the person or group providing information, does not seem to be a routine activity during comprehension (Johnson, Hashtroudi, & Lindsay, 1993). Tagging information as credible or unreliable would be useful for guiding subsequent judgments that invoke retrieval of that information. But lacking such tags, information that was encoded as false can still be retrieved for subsequent use.

The seeming negligence to engage in such tagging can emerge for a variety of reasons, but one important explanation relates to the allocation of people's limited cognitive resources. In our efforts to comprehend information, we apply mental resources to determine meaning, build inferences, rehearse content, and derive interpretations (along with a host of other processes). This leaves fewer resources available for other processes that are not necessarily critical to building meaning in the here-and-now, such as source monitoring. As a consequence, information encoded into memory can be jumbled together without an effective indexing of which information is accurate and reliable, and which information is inaccurate and should be discounted for further use (Schwarz, Sanna, Skurnik, & Yoon, 2007). Engaging in more careful evaluation neces-

sitates overcoming the routine, heuristic processing we engage in and that is often effective and efficient for everyday reasoning (McNeil, personal communication, September 1, 2017).

Added to this issue, information in memory is, at least initially, reactivated through a process some researchers have identified as automatic and unguided (Cook, Halleran, & O'Brien, 1998; O'Brien, 1995), meaning without being strategically retrieved. When a particular cue provokes retrieval in memory, concepts broadly associated with that information become activated, with some of those concepts rising above threshold to be brought into conscious awareness. The challenge is that concepts broadly associated with a retrieval cue can become activated, including closely related and indirectly related information. Given that activated memories are likely not effectively tagged, a routine consequence of retrieval is that inaccurate information might become available for use.

In sum, comprehension involves encoding information into memory for subsequent retrieval. Because people may not routinely add tags to those encodings that reflect the credibility or validity of what has been experienced, retrieval can involve reactivating inappropriate, inaccurate concepts. Activated inaccurate information in memory, including pseudoscientific claims, even after they have been debunked, can thus have effects on subsequent comprehension and decision making.

Discouraging the Use of Inaccurate Information

Given these processes are routinely recruited in the service of comprehension, and when enacted on accurate information are supportive and necessary for building effective understandings, determining ways of "correcting them" when information is faulty is both challenging and potentially misguided. Much of the information we routinely encounter is, after all, worth relying upon. With this in mind, a variety of recent experimental findings from our lab have revealed situations in which people are more effective at rejecting inaccurate information. These findings highlight important features of memory and language processing that delineate the allure and influence of pseudoscientific claims.

When confronted with information that is patently inaccurate, people may nevertheless encode the information into

memory, despite being aware it is wrong. And once that information is encoded, it can potentially be reactivated later to influence comprehension. To combat this, we have instructed participants to carefully edit what they are reading as a text unfolds (Rapp, Hinze, Kohlhepp, et al., 2014). For example, when a participant encounters pseudoscience that sounds dubious, they might note skepticism about that information, or annotate correct ideas that are not being reported. These kinds of edits are likely effective because they encourage an encoding of the accurate information that is already known, rehearsing that knowledge as participants retrieve it and write it down as they edit. This helps ensure the accurate information will be available later, and discourages encoding the inaccurate information into memory.

Sometimes text content itself can reduce reliance and enhance evaluation, as has been shown when participants encounter false information that is implausible (Rapp, Hinze, Slaten & Horton, 2014). For example, if people read an account contending that mental illness is contagious associated with a particularly outlandish set of claims (e.g., involving patho-

genic spirits and demons), the likelihood they might consider that idea later is greatly reduced, in contrast to when the account is more plausibly motivated (e.g., other people's behaviors might inform how we should behave). Some individuals might still endorse even implausible ideas in efforts to support their existing world-views, but implausible information often calls attention to explicit inconsistencies, discrepancies, and logical leaps that mark information as inappropriate. These qualities can encourage careful evaluation and tagging of that information as wrong.

In both of the above cases, individuals must be given the motivation to carefully consider the validity and plausibility of what they read, as well as the appropriate tools for engaging in evaluation. These are skills that people differentially possess, and that they opt to apply in different contexts to varying degrees (Gottlieb & Wineburg, 2012). The upshot is that explicit training on evaluation and media literacy may be beneficial in helping people overcome the allure of inaccurate information. This training could, for example, and as relevant to pseudoscience, involve exposure to the scientific methodologies and practices that

are involved in developing and testing ideas. Increased awareness and familiarity with applying a scientifically based perspective should help readers call into question the kinds of unsubstantiated claims and false information commonly presented in pseudoscientific discourse.

Concluding Thoughts

The cognitive factors discussed above that support attention to and reliance on inaccuracies are one set of contributors to people's use of pseudoscientific claims such as a flat earth and mental illness as being contagious. But these are far from the only contributors to such problematic acquisition and reliance. There are a host of other considerations that, in concert with the routine operations of human memory and language, can lead to surprising and problematic endorsements. People's naïve theories as to how the world works often invoke simple, intuitively appealing explanations that can connect to claims associated with pseudoscience (Vosniadou & Brewer, 1992). A lack of familiarity with scientific investigations, including the tools and practices of scientists, can lead to dis-



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trust and confusion that may make other seeming explanations appealing and viable (Miller, 2004). The filter bubbles that people routinely place themselves in, exposing themselves to and accessing information that aligns with their beliefs without considering alternative perspectives and contradictory evidence, can help drive pseudoscientific thinking (Lewandowsky, Ecker, & Cook, in press; Lewandowsky & Oberauer, 2016). The unmoderated content available through social media, blog postings, and Internet articles can make false ideas and claims available to audiences that may not have the time, energy, or inclination to evaluate that content carefully (Del Vicario et al., 2015; Kumar & Geethakumari, 2014). Contemporary concerns about “fake news” make this last notion even more worrisome, as individuals who promote pseudoscientific claims often like to call into question whether we can truly know anything, to support arguments that their view, lacking evidence, is just as reasonable as any other (Lazer et al., 2017). Even the routine use of terms like “theory” and “hypotheses,” detached from their more rigorous implementations to instead be synonymous with the terms “opinion” and “viewpoint,” have consequences for how people might opt to think about the claims that underlie pseudoscientific conjectures.

Understanding and combatting the influence of “fake news,” inaccurate information, and pseudoscience requires a concerted, interdisciplinary effort. This will require leveraging theoretical understandings of cognition and behavior, as derived from the social and medical sciences, with applied understandings derived from practices including journalism and educational design, as well as from domains studying topics such as persuasion, media literacy, and critical evaluation (to name a few relevant fields and topic areas). The goal is to encourage more careful evaluation on the part of readers, which hopefully will beneficially lead to a reduction in the promotion of and reliance on pseudoscientific discourse.

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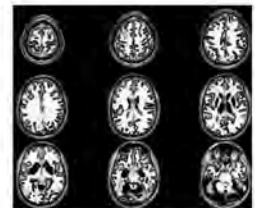
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The Scientist-Practitioner Gap in Clinical Psychology: A Social Psychology Perspective

David Trafimow, *New Mexico State University*

MUCH LITERATURE ATTESTS to the existence of a large gap between the science of clinical psychology and how it is practiced (Garb & Boyle, 2015; Gaudiano, Dalrymple, Weinstock, & Lohr, 2015; Katz, 2001; Lilienfeld, Lynn, & Lohr, 2015; McFall, 1991; Nunez, Poole, & Memon, 2003; Poole, Lindsay, Memon, & Bull, 1995; Polusny & Follette, 1996; Tavis, 2015). To gain an idea of some researchers' perceptions of the gap, consider that Tavis likened it to that which separates the Israelis and the Arabs. Assuming the desirability of bridging the gap, we might inquire as to the reasons for its existence to gain clues about what to do about it. One possibility is that practitioners believe the science of clinical psychology is so badly flawed or irrelevant that there is no point in basing their clinical practices on it. Alternatively, practitioners might believe that the science is neither badly flawed nor irrelevant, but that they are not capable of learning it or applying it to their practices. Of course, there are many other possibilities too.

In the present article, I use the literature cited above as providing two starting points. First, there is a large science-practitioner gap. Second, it is important to bridge the gap. These starting points suggest at least two possibilities: the gap can be bridged by inducing clinical scientists to move in the direction of practitioners or by inducing practitioners to move in the direction of clinical scientists. From the point of view of evidence-based practice, it is more desirable for practice to move in the direction of clinical science than for clinical science to move in the direction of practice. There doubtless are institutional changes that could aid in moving practitioners in the direction of clinical science, but these will not be discussed here. Instead, consistent with a social psychology focus, I take the goal as that of inducing practitioners to change their behaviors to be more in line with clinical science. An important step in changing such behaviors is to diagnose the reasons why practitioners perform them or fail to perform desirable behaviors. The most widely researched social psychology program for understand-

ing and predicting behavior is the reasoned action approach (see Fishbein & Ajzen, 2010, for a comprehensive review). As will be explained at some length, there are different routes to behavior, and these different routes might imply quite different interventions to induce practitioners to change their behaviors.

There are three main sections. The first section includes a brief discussion of two flaws in the actual science. I wish to make clear that although I support practitioners using the science of clinical psychology (indeed, this is the point of the present article), there can be little doubt that there is much wrong with the science itself that researchers should fix. The argument that practitioners should attend to the science of clinical psychology would be augmented by improvements in that science. The second section explains the reasoned action approach and what it implies about possible reasons for the science-practitioner gap in clinical psychology. The third section discusses implications for how to design research to investigate behaviors relevant to reducing the science-practitioner gap.

The Science of Clinical Psychology

Because the science of clinical psychology is a subset of the larger field of psychology, it is plagued with some of the problems that plague psychology more generally. For example, clinical psychology depends on the null hypothesis significance testing procedure (NHSTP). But the procedure has come under much fire for being logically invalid (e.g., Bakan, 1966; Carver, 1978, 1993; Cohen, 1994; Grice, Cohn, Ramsey, & Chaney, 2015; Kass & Raftery, 1995; Kline, 2015; Meehl, 1967, 1978, 1990, 1997; Rozeboom, 1969, 1997; Schmidt, 1996; Schmidt & Hunter, 1997; Trafimow, 2003, 2006 Trafimow & Marks, 2015, 2016; Valentine, Aloe, & Lau 2015). In short, the fact that a finding is unlikely given the null hypothesis fails to justify an inverse infer-

ence that the null hypothesis is unlikely given the finding. The editors of *Basic and Applied Social Psychology* (Trafimow & Marks, 2015) banned the NHSTP for the simple reason that low p -values (less than .05) fail to provide a logically defensible justification for rejecting null hypotheses and accepting alternative ones. The American Statistical Association (Wasserstein & Lazar, 2016), though stopping short of supporting the ban, admitted that the NHSTP fails to provide a sufficient reason for rejecting null hypotheses or drawing any conclusions whatsoever other than the tautological one that if p is a low value, the probability of the finding is low given the null hypothesis. As the primary "evidence" for the efficacy of treatments comes from statistically significant p -values, a skeptic has clear grounds for his or her skepticism. Researchers should consider alternatives (e.g., Trafimow, 2017; Trafimow & MacDonald, 2017).

Another problem concerns the failure of researchers to distinguish cleanly between theoretical assumptions and assumptions that are auxiliary to the main theory (hereafter, auxiliary assumptions), but are nevertheless necessary to derive treatments. The importance of auxiliary assumptions comes from a more general concern in science than clinical psychology, or even psychology more generally. Philosophers of science long have recognized that theories contain nonobservational terms (e.g., Duhem, 1954; Lakatos, 1978). In clinical psychology, "anxiety" might be considered a nonobservational term. But every science uses nonobservational terms. In fact, the Nobel Laureate Leon Lederman (1993) pointed out that Newton used *mass* as a nonobservational term that even lacks an independent definition!¹ Despite this lack, Newton's equation, $force = mass \times acceleration$, is possibly the most important equation in the history of physics. Because theories contain nonobservational terms, there is no way to derive empirical predictions from them except by using auxiliary assumptions that link the nonobservational terms in theories to the observational terms in empirical hypotheses. Haley used Newton's theory to predict the reappearance of the comet that now bears his name, in conjunction with auxiliary assumptions about the present position of the comet, gravitational influ-

¹The nonobservational term "mass" should not be confused with the observational term "weight." That these are different can be seen easily merely by considering that an object of the same mass would weigh different amounts on different planets.

ences, and so on. In clinical psychology, there is no way to derive treatments from theories, except in conjunction with auxiliary assumptions. A theory may lead to an excellent treatment when used in conjunction with one set of auxiliary assumptions and a failing treatment when used in conjunction with another set of auxiliary assumptions. Thus, the success or failure of a treatment need not provide a strong case for the worth or lack of worth of the theory. Unfortunately, researchers in clinical psychology have not been careful about spelling out the auxiliary assumptions that lead from theory to treatment. This is a major strike against the science of clinical psychology and researchers should remedy it if they wish practitioners to take the science more seriously.

A third issue concerns level of measurement. Suppose that a proper experiment is conducted that shows that a particular treatment group does better than the control group with respect to, say, a depression

index. And let us even suppose that the effect size is reasonably large, there was a sufficient sample size, and so on. Can we conclude that practitioners should use the touted treatment? It depends, in part, on whether one believes that the depression index is at least at the interval level of measurement (Stevens, 1946). Without an assumption of at least an interval level of measurement (a ratio level would be even better), the effect size calculation is meaningless. In fact, several researchers have questioned whether typical indexes in psychology really are at the interval level or ratio level of measurement to justify the usual calculations upon which researchers base their conclusions (e.g., Barrett, 2003; Michell, 1997, 2000, 2008a, 2008b; Morris, Grice, & Cox, 2017). Unfortunately, although the mathematical basis for making this determination was worked out in the 1970s (Kranz, Luce, Suppes, & Tversky, 1971; also see Luce, Krantz, Suppes, & Tversky, 1990; Roberts, 1979; Suppes,

Kranz, Luce, & Tversky, 1989), researchers in clinical science have not taken the trouble to test whether their indexes, such as that which measures depression, actually are at the interval level or ratio level of measurement. Ironically, just as clinical scientists accuse practitioners of failing to attend to the relevant literature in clinical science, it is possible to accuse clinical scientists of failing to attend to the basic mathematics underlying the assumed quantitative nature (or lack thereof) of their indexes.

Despite the foregoing criticisms of the science of clinical psychology, there is no intent to declare the science to be worthless. There have been gains, too, and the fact of shortcomings provides a poor justification for practitioners being unaware of the science of clinical psychology. Although the present section can be considered a slight indictment against how the science of clinical psychology has been conducted, the remainder of this article assumes that practitioners nonetheless



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should attend to it and be influenced in the conduct of their clinical practices.

The Reasoned Action Approach

The major goal of the reasoned action approach (Ajzen, 1988, 1991; Ajzen & Fishbein, 1980; Ajzen & Fishbein, 2005; Fishbein, 1963; Fishbein, 1967, 1980; Fishbein & Ajzen, 1975; Fishbein & Ajzen, 2010) is to understand and predict behavior. Consequently, the easiest way to comprehend the theory is to work backwards from behavior to its determinants. The immediate determinant of behavior is behavioral intention; people do what they intend to do and not what they intend not to do. There are complications to be discussed later, but these can be ignored for now.

In turn, behavioral intentions are determined by attitudes and subjective norms. Attitudes are people's evaluations of the behavior (how much they like or dislike to perform it) and subjective norms are people's opinions about what most others who are important to them think they should do or not do. Because a person cannot know for sure what others think, this is the subjective part of subjective norms. Any particular behavioral intention might be influenced more by attitudes or more by subjective norms: that is, a behavior might be more under attitudinal control or more under normative control. In addition, Trafimow and Finlay (1996) showed that people also can be more under attitudinal or more under normative control, across a wide range of behaviors.

Suppose that a behavior is more under attitudinal than normative control. To intervene, it is desirable to know the determinants of attitudes, which are behavioral beliefs and evaluations of those beliefs. Behavioral beliefs are judgments about the likelihood of the consequences that might arise from performing a behavior whereas evaluations are judgments about how good or bad each of the consequences would be if they were to happen. In the reasoned action tradition, attitudes are a function of each behavioral belief-evaluation product, summed across all products ($A = \sum_{i=1}^j b_i e_i$). Analogously, subjective norms are determined by normative beliefs and motivations to comply with normative referents. A normative belief is a judgment about the likelihood with which a specific normative referent believes one should or should not perform the behavior, and these are paired with how much one is motivated to comply with what that person thinks. Thus, subjective norms are a function of normative

belief-motivation to comply products, summed across all products: ($SN = \sum_{i=1}^k n_i m_i$).

There is also a measurement model that accompanies the substantive theory. The basic principle, sometimes called the "principle of correspondence" or the "principle of compatibility," is that all behaviors have four elements and these elements must correspond across all reasoned action constructs. That is, each behavior has a target, action, time, and context. For example, the behavior of "eating a chocolate bar at 3:00 on Friday in my office" has the following elements: target (chocolate bar), action (eating), time (3:00 on Friday), and context (in my office). To perform well at predicting behaviors; measures of behavioral intentions, attitudes, subjective norms, behavioral beliefs, evaluations, normative beliefs, and motivations to comply; all should mention the same four elements of target, action, time, and context. Research performed in the 1970s (e.g., Davidson & Jaccard, 1975, 1979), specifically on the measurement principle, supports that excellent prediction is obtained when it is complied with fully, but that a mismatch on even one of the four elements is problematic.

Thus far, we have the received view from the 1970s (e.g., Ajzen & Fishbein, 1980; Fishbein, 1980; Fishbein & Ajzen, 1975), but Ajzen (1988) added the notion of perceived behavioral control. The original theory only was meant to apply to behaviors that people are capable of performing, but Ajzen wanted to extend the theory to behaviors that people might not be capable of performing. Although there is no way to measure actual control over a behavior, it is possible to measure people's perceptions of their degree of control; hence, the notion of perceived behavioral control came into being. Usually the concept is measured by having participants respond to items referring to how much control they have over the behavior and how easy or difficult the behavior would be for them to perform. But Trafimow et al. (2002) argued that "control" and "difficulty" are different concepts that should be kept distinct for the sake of precision. To back up this claim, Trafimow et al. showed that it is possible to perform manipulations that influence perceptions of control without influencing perceptions of difficulty, and to perform manipulations that influence perceptions of difficulty without influencing perceptions of control. Thus, rather than use what has been demon-

strated to be an imprecise concept of perceived behavioral control, it is possible to substitute the more precise concepts of perceived control and perceived difficulty. And to go with perceived control and perceived difficulty, there also are beliefs about the factors that render a behavior under one's control or not (control beliefs) and about the factors that render a behavior easy or difficult to perform (difficulty beliefs).

Finally, Fishbein (1980) argued strongly that attitude only consists of a cognitive evaluation, and also criticized the factor analytic approaches that indicated an affective component too. While agreeing with Fishbein's criticisms of the factor analytic work up to that time, Trafimow and Sheeran (1998) performed a set of experiments that demonstrated that affect and cognition nevertheless need to be separated. They also showed that "affective" beliefs can be distinguished from "cognitive" beliefs. Thus, the reasoned action tradition is much richer in the 21st century than it was in the 1970s. On the negative side, this increased richness comes at a price in parsimony.

Defining the Behavior

The long description of the theory was necessary so that the reader could appreciate some important ambiguities. One of these concerns the behavior of interest. To reiterate, the presenting problem is that clinical practitioners fail to consider the scientific evidence that is relevant to their practices. But it is not clear what we mean by this. Do we mean that practitioners should read the scientific literature? If so, how often should they read it, when should they read it, and in what context should they read it?

Or do we mean that clinical practitioners should apply the scientific literature to their own practices? If so, when should they do it, to what extent should they do it, and in what context should they do it?

What we might mean is that we wish for practitioners to perform a set of behaviors that will result in evidence-based practice. This is fine, but we need to specify the set of behaviors we wish to change in a precise manner.

Multiple Pathways to Behavior

Another ambiguity pertains to how to get to behavior. In the original version of the theory, there was an attitudinal and normative pathway. To that, researchers have added perceived control and perceived difficulty. We might even consider

affect to be a fifth pathway, though some would argue that it is part of the attitude construct. Before any sort of intervention is likely to work, it is necessary to figure out which pathway predominates for most practitioners, assuming, of course, that one has specified a behavior or set of behaviors of interest. For example, there is no point in intervening at the normative level if the behavior is mostly under attitudinal control.

The usual method for determining control is to use multiple regression with behavior or behavioral intention regressed onto the other variables. In the traditional version of the theory, a large attitude and small subjective norm beta-weight is taken as indicating that the behavior is more under attitudinal control than normative control whereas the reverse pattern of beta-weights is taken as indicating that the behavior is primarily under normative control. To make use of the more recent literature, it is desirable to measure perceived control and perceived difficulty too. Although strong beta-weights and correlation coefficients do not prove causation from a precursor construct to behavior, they support that some precursor constructs are better candidates than others for intervention.

The Belief Level

We have seen that the constructs that are precursors to behavioral intentions and behaviors have, in turn, their own precursors. And these precursors are beliefs of various types, augmented by evaluations or motivations to comply. But remaining with beliefs, we have behavioral beliefs, normative beliefs, control beliefs, difficulty beliefs, and affective beliefs. Which of these are relevant to the scientist-practitioner gap? Consider some plausible possibilities.

- It could be that practitioners believe that using clinical science will not actually have positive consequences (behavioral belief);
- Practitioners may believe their colleagues think they should not use clinical science (normative belief);
- Practitioners may believe they do not have the ability to learn the clinical science (control belief);
- Practitioners may believe that it would be difficult for them to learn the clinical science, or might take too much time and effort (difficulty belief);
- Practitioners may simply have negative affective reactions to the clinical science, possibly because of being

reminded of unpleasant aspects of graduate school (affective belief).

To change the behaviors of practitioners towards reading relevant clinical science, changing their own practices in accordance with relevant clinical science, and so on, it is necessary to know which of the foregoing beliefs, or other beliefs not mentioned in the bullet list, determine the behavior or behaviors of interest. For example, if the main obstacle for practitioners is a belief that their learning the relevant clinical science will not result in positive consequences for their patients, then an intervention designed to educate them to see how relevant clinical science can result in positive consequences for their patients is likely to be effective. However, if the problem is at the level of a control or difficulty belief, such education likely will be ineffective. And to make the problem more complex, I stress that the bullet-listed beliefs compose only a small set of the potentially relevant ones.

Zeroing in on an Intervention

There are at least three stages to zeroing in on an intervention. First, there are two preliminary studies that the researcher must complete. Second, the researcher should use the data to find out which beliefs are good candidates for intervention, and design the intervention. Third, it is desirable to perform a third study to evaluate the effectiveness of the intervention. I'll present more details of the two preliminary studies as clinical psychologists are less likely to know these. And I will say very little about evaluating the effectiveness of the intervention because readers of this journal are likely to know this already.

How to Conduct Two Preliminary Studies

Although there are many candidates for relevant beliefs, they fall into four categories.² These are beliefs about consequences that determine attitudes, normative beliefs that determine subjective norms, control beliefs that determine perceived control, and difficulty beliefs that determine perceived difficulty. I recommend that researchers conduct two studies to zero in on an intervention, but first, as I

mentioned earlier, it is necessary to specify the behavior or set of behaviors of interest.

Once a behavior of interest is chosen, the researcher can conduct a two-part study. In the first part, the researcher can measure behavioral intentions (and actual behaviors, too, if that is feasible), attitudes, subjective norms, perceived control, and perceived difficulty. It is important to keep the principle of correspondence in mind for all measures. By determining which of the four precursor constructs (attitudes, subjective norms, perceived control, or perceived difficulty) are good predictors of behavioral intentions (or better yet, behaviors), and which precursor constructs are not, it may be possible to narrow matters down substantially. For example, suppose that attitudes do an excellent job of predicting behavioral intentions (or behaviors) but that subjective norms, perceived control, and perceived difficulty do not. In that case, the researcher would not have to deal with the latter three precursors in the subsequent study, and also would not have to deal with normative beliefs (or motivations to comply), control beliefs, or difficulty beliefs.

Measurement reliability and validity are extremely important. For well over a century (Spearman, 1904), it has been known that reliability sets an upper limit on validity. If one imagines two variables, X and Y , that have "true scores" according to classical true score or classical test theory (Gulliksen, 1987; Lord & Novick, 1968; Spearman, 1904), the following equation shows how the correlation one might expect to observe (ρ_{XY}) is decreased from the true correlation ($\rho_{T_X T_Y}$), depending on the reliabilities of the measures of X (ρ_{XX}) and Y (ρ_{YY}):

$$\rho_{XY} = \rho_{T_X T_Y} \sqrt{\rho_{XX} \rho_{YY}}$$

As an example, suppose that the true correlation is .7 and that the reliabilities of the two measures are .7 and .7, respectively. In that case, the observed correlation can be expected to come out at .49 rather than at the true level of .7.

Fortunately, because all of the reasoned action variables are very precisely defined, it is possible to capture most of the meaning with very precise items. In fact, Trafimow and Finlay (1996) showed that—in

² For the sake of brevity, I am skipping a possible fifth category, pertaining to affective reactions to learning or using the science of clinical psychology. However, researchers who seriously want to pursue this issue might wish to consider this as a possibility too, that should be investigated.

violation of a standard rule of scale construction—even single item measures do well if designed with care, both with respect to test-retest reliability and predictive validity. Nevertheless, I recommend using three to five items to measure each construct, remembering, of course, to obey the principle of correspondence, without which there will be a lack of validity. Ajzen and Fishbein (1980, Appendix A) contain example items and demonstrate how to create items that obey the principle of correspondence.

But it also is necessary to obtain relevant beliefs, which leads us to the second part of the first study, which depends on open-ended questions. Specifically, the researcher should obtain behavioral beliefs by asking participants to list the advantages and disadvantages of performing the behavior. Again, the principle of correspondence needs to be obeyed even at the level of beliefs. Moving to normative beliefs, the researcher should ask participants to list the people whose opinions are relevant to their performing the behavior. The researcher can obtain control beliefs by asking participants to list specific reasons why the behavior might be under their control or might not be under their control. Finally, the researcher can obtain difficulty beliefs by asking participants to list specific reasons why the behavior might be easy or difficult for them to perform.

The design of the second study depends on the results obtained in the first study. If luck is with the researcher, all but one of the precursor constructs can be eliminated, which implies that only one category of beliefs is relevant. With less luck, the researcher might find that two or three categories are relevant. Suppose, for example, that only attitudes do a good job of predicting behavioral intentions (or behaviors). In that case, it is important to find out the relevant behavioral beliefs. Happily, these can be obtained from the open-ended list of advantages and disadvantages of the behavior obtained in Study 1. Assuming a reasonable sample size in Study 1, many behavioral beliefs may be listed, and it may take some judgment to decide how many people need to have listed a particular behavioral belief for it to deserve to be used in Study 2. Ajzen and Fishbein (1980) suggested a 70% rule (item listed by 70% of the participants), but this is arbitrary and may not fit any particular case at hand. Once the researcher decides to include a particular behavioral belief, participants can be asked to respond on a scale ranging from “extremely likely” to “extremely unlikely”

that indicates participants’ perceptions of the likelihood of the consequence if they were to perform the behavior. An evaluation item can be paired with it, asking to what extent it would be “extremely good” to “extremely bad” if the consequence were to happen. At the risk of sounding like a broken record, I reiterate that the principle of correspondence must be followed even at this level. For example, if the behavior is “to read an average of three clinical science papers per week for the next year,” and a consequence is that “I will get bored,” then the behavioral belief item might be as follows: “How likely or unlikely would you be to get bored if you read an average of three clinical science papers per week for the next year?”

Although attitudes tend to be the most important construct for predicting most behavioral intentions or behaviors, this is not always so. It may turn out that subjective norms, perceived control, or perceived difficulty also are important, and may even be more important than attitudes for predicting a particular behavior. In that case, normative beliefs, control beliefs, or difficulty beliefs might be important too, and should be included in Study 2. As always, the principle of correspondence should be obeyed.

It also might be useful to replicate the first part of Study 1, concerning behavioral intentions (or behaviors), attitudes, subjective norms, perceived control, and perceived difficulty. A benefit of the replication is that the researcher can be more certain about which precursor constructs matter and which do not.

In the end, though, interventions will be at the level of beliefs, and so it is important to find the ones that matter. This can be done with simple correlations. Remaining with attitudes as the most important construct, for example, how well do each of the behavioral beliefs correlate with attitudes? As a complication, recall that it is the sum of belief-evaluation pairs that determine attitudes. Consequently, it also might be worth computing each belief-evaluation product separately, to investigate which product terms best predict attitudes.³ These will be excellent candidates for intervention. As a more general check, the sum of the belief-evaluation products also can

be used to predict attitudes. If the researcher can find a small number (hopefully one or two) of belief-evaluation products that account for almost all of the variance in attitudes that the sum of the products accounts for, those are excellent candidates for intervention, especially if they do a good job of predicting behavioral intentions (or behaviors) too. Alternatively, it might be that a different construct matters. My preliminary bet would be on perceived difficulty as an important construct. That is, beliefs having to do with time, effort, and so on devoted to learning relevant clinical science literature might be likely to perform well as predictors of, say, learning the clinical science literature.

Designing the Intervention

Designing the intervention is the most difficult part. The foregoing two preliminary studies can be performed in a rather “automatic” way, following the principle of correspondence, and it is practically a certainty that the result will be a few, or several, beliefs that are good candidates for intervention. Based on both an extensive literature (see Fishbein & Ajzen, 2010, for a review) and my own experiences, I can say with confidence that, up to this point, failure is extremely unlikely provided that the researcher complies carefully with the principle of correspondence. But from here, matters are no longer straightforward. The theory does not tell the researcher how to intervene, only how to find the beliefs that matter most for the behavior.

As an example, suppose that as a result of the two preliminary studies, the behavioral belief pertaining to “being bored if I read an average of three clinical science papers per week for the next year” turns out to be critical. At one level, the solution is obvious: change that belief! But at another level, it is far from obvious how to intervene to change the belief. How do you convince someone that an activity they consider to be boring is not boring? Or, failing that, how do you convince someone not to evaluate being bored so poorly? Perhaps a solution might be to introduce a journal with the goal of filtering and translating important advances in clinical science so that useful information is provided with a minimum of boring statistical detail (espe-

³According to traditional reasoned action thinking, belief-evaluation products should be used. Arguably, belief measures or evaluation measures are not at a ratio level, in which case it might be best not to use products after all. This would constitute an argument that beliefs should be correlated directly with attitudes and intentions, directly.

cially *t*-tests, *F*-tests, and resulting *p*-values that are invalid anyhow).

On the other hand, there are other sorts of beliefs that might pose less of a problem for intervention. For example, suppose that an important belief is as follows: "There would be no benefit to my clients if I were to read an average of three clinical science papers per week for the next year." It may be possible to cite data showing that their patients likely would benefit after all, especially if reading the literature results in actual change in practices to more effective ones.

The problem can be considered more abstractly. Whenever a theory is applied to make an empirical prediction or an application, it is necessary to make auxiliary assumptions, as I explained in the first section of the present article. With respect to the preliminary studies described in the foregoing subsection, the requisite auxiliary assumptions have been worked out in great detail, thereby reducing the creative load on the researcher. In contrast, when it comes to interventions, relevant auxiliary assumptions have not been worked out, and so the researcher is thrown on his or her own ingenuity and creativity.

Evaluating the Intervention

Because readers of this journal are already knowledgeable about evaluating interventions, this section can be kept brief. But it seems useful to make the following points. First, it is important to evaluate intervention effects with means other than *p*-values. As I pointed out earlier, not only have these come under much criticism, but even aficionados of *p*-values admit that they fail to indicate how well an intervention works. Most statistical authorities recommend effect sizes. For example, Cohen's *d* gives the distance between means of two conditions, in standard deviation units. However, it is possible to argue that even Cohen's *d* is problematic because it confounds variation due to randomness and systematicity. Provided that the researcher has obtained good reliability estimates of the dependent variables, Trafimow (in press) demonstrated that it is possible to distinguish the variance due to randomness, the independent variable, and systematic effects due to variables not considered. Using this tripartite distinction, it also is possible to obtain more focused effect sizes that control for either randomness or for systematic effects that are not of interest (due to variables not considered).

A second consideration is that the intervention attempted in an experiment might

have to be at some distance from how it actually would be implemented on a large scale. As an example, suppose that researchers find that to handle the most predictive beliefs, it is necessary to do something at an organizational level, such as founding a journal whose purpose is to translate important clinical science papers from jounalese into language that is interesting and easy to understand. Short of actually founding the journal and evaluating its effects, a preliminary intervention study necessarily will be somewhat different. For example, practitioners might be randomly assigned to read specific articles tailored in this direction in the experimental condition, but not in the control condition, to determine whether the behavior of concern is influenced, and by how much. To what extent the findings from such a preliminary intervention study will support broader conclusions about the likely effect of founding a journal may depend on a variety of factors, such as how close the tailored articles in the experiment would be to the real articles in the founded journal, the extent to which it would be easy for practitioners to access the founded journal, and many others. My point is not that researchers should not conduct such research, only that they should be aware that a single study is unlikely to be definitive.

Conclusion

There has been much complaining on the part of those knowledgeable about the science of clinical psychology about the fact that practitioners mostly are uninfluenced by that science. Certainly, from the point of view that therapy should be based on evidence, this is a deplorable state of affairs. But what has been lacking from the scientists themselves is (a) an admission that there is much wrong, as well as much that is right, with the science of clinical psychology; (b) strong efforts to fix what is wrong to provide a better case that practitioners ought to be influenced; and (c) effort devoted to finding out why practitioners fail to do what scientists think they should do. Let me emphasize this last point. If researchers do not know what determines the behaviors that practitioners perform or fail to perform, efforts to change practitioners' behaviors are likely to fail. The point of the present article is to focus on how to find out that which is relevant and that which is irrelevant, to provide a starting point in the right direction.

But although I obviously believe in the direction advocated in the foregoing comments, it is important to be up front about the difficulties. The first difficulty, as I emphasized earlier, is to figure out precisely what the behaviors of concern should be. This includes specifying the target, action, time, and context of each behavior but it also includes specifying corresponding target, action, time and context for all precursor variables. Although the two preliminary studies are reasonably straightforward, and are practically guaranteed to provide useful information, there also are complications with respect to performing and evaluating intervention studies. For example, what are the auxiliary assumptions that allow the researcher to traverse the distance from the nonobservational terms in the theory to the observational terms used in the experimental hypothesis? Another problem is that the researcher needs to figure out which type of effect size to use to index the size of the effect of the intervention. Although researchers may be in the habit of using a particular sort of effect size for a particular experimental paradigm, the issue is not automatic, and researchers should consider it carefully before coming to any conclusions. Finally, even if an intervention is quite successful in an experiment, there might be quite a distance between the laboratory context and how the intervention actually would be implemented with real practitioners in real practice sorts of contexts.

Although I have attempted to be up front about the ambiguities that face the researcher to whom change in practitioners' behaviors is an important concern, this focus should not be taken too pessimistically. I am not arguing that the effort is not worthwhile. Nor am I arguing that the evidence has to come from a single, definitive, study. What I am saying, however, is that changing practitioner behavior is likely to prove extremely difficult, with an enormous amount of psychological inertia to overcome. If the reader nevertheless would like to move in this direction, the fight will be difficult and protracted. Still, very little that is worthwhile is obtained without a fight, and so I hope and anticipate that scientific clinical psychology researchers will not allow themselves to feel too discommoded.

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Science and Epistemic Vice: The Manufacture and Marketing of Problematic Evidence

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McFall (1991) in his classic “Manifesto for a Science of Clinical Psychology,” suggested that science is the only warrant for evaluating knowledge claims in clinical psychology. Certainly science has given rise to both an unprecedented growth in knowledge as well as powerful technologies that allow humans to apply this knowledge for their desired ends. In addition, professional expertise is founded on epistemic duties—a duty to know.

However, there are at least six problems in this otherwise generally rosy picture regarding science: (1) philosophers of science and others engaging in the study of science do not agree on how to define science or even if there is a single scientific method (Feyerabend, 1975; O’Donohue,

2013); (2) the social sciences, including clinical psychology, have seemed to enjoy much less scientific progress (e.g., discovery of scientific laws) than the natural sciences (Meehl, 1978); (3) not all of science has produced beneficial results—for example, certain technologies have had harmful effects on the environment and weapons of mass destruction have been produced; (4) there has been a grab bag of vexing and embarrassing problems in psychology, such as replicability failures, fraudulent data, concerns that business interests such as those of Big Pharma add considerable noise to the literature (see for example, Antonuccio et al., 1999), such that at times what may appear to be proper science is actually pseudoscience (see, for example,

Herbert et al. [2000], on EMDR, and O’Donohue, Snipes, & Soto [2016a & b] on Acceptance and Commitment Therapy—but also see Gregg & Hayes [2016] for a rejoinder); (5) there are unresolved issues regarding how disparate individual studies can be properly and fairly aggregated and summarized to accurately produce certain useful summary statements such as “empirically supported treatments” (e.g., see Chambless & Hollon, 1998); and (6) there are longstanding concerns about whether scientific epistemology is complete or whether other ways of knowing are also needed to complete our knowledge (see, for example, Hempel, 1965, on ethics and Houts, 2009, on religious beliefs).

These are all important problems and concerns relevant to the work of the cognitive behavior therapist. However, this paper will focus on several key issues described in point 4 above, which may be summarized by the problem of pseudoscience. The basic notion is it is possible for research to be conducted in a way that appears to be scientifically sound but actu-

ally misses some essential characteristic of science, so that one must conclude that proper science actually has not been conducted but rather what has occurred is pseudoscience—literally false science. The Nobel Laureate Richard Feynman (1974) picturesquely called this cargo cult science:

In the South Seas there is a cargo cult of people. During the war they saw airplanes land with lots of good materials, and they want the same thing to happen now. So they've arranged to imitate things like runways, to put fires along the sides of the runways, to make a wooden hut for a man to sit in, with two wooden pieces on his head like headphones and bars of bamboo sticking out like antennas—he's the controller—and they wait for the airplanes to land. They're doing everything right. The form is perfect. It looks exactly the way it looked before. But it doesn't work. No airplanes land. So I call these things cargo cult science, because they follow all the apparent precepts and forms of scientific investigation, but they're missing something essential, because the planes don't land. (p. 7)

A key underlying problem is that if philosophers of science have not produced a consensual characterization of what science is, it can be somewhat difficult to identify some missing essential property of epistemically sound science (O'Donohue, 2013). For example, a popular candidate for an essential feature of science is the maximization of criticism (Bartley, 1962). In this view good scientific research is an attempt to expose cherished beliefs to severe criticism in order to efficiently identify errors in one's web of belief. Genuine science is not a craving to be correct, but rather a craving to efficiently learn where our beliefs are wrong so that our errors can be eliminated. The prominent philosopher of science Sir Karl Popper (1959), for example, suggested that it is only through such error elimination that knowledge grows.

Thus, the best and most efficient way of rooting out error in our beliefs is to expose these to severe criticism through empirical tests that can efficiently uncover error. To give a general picture of the distinction between severe vs. nonsevere tests, suppose one wanted to test the belief, "My minister never swears." The researcher could collect verbal samples from her sermons, from her speeches in front of community groups,

and from her teaching Sunday school and so on. One could then examine these samples to see if these refute the proposition "My minister never swears" by finding an instance or instances of swearing. This would be a test of the proposition—even an empirical one—but not a severe one.

Alternatively, the researcher could sample from the minister's golf games, after she stubs her toe, when she is intoxicated, when someone cuts her off in traffic, or when she is in a heated argument. Both studies could count as a test of the belief: but it is only the latter that counts as a severe test; it is simply much more likely to expose the potential falseness of belief under test. It is a more risky test. The question then becomes, How severe have tests such as random clinical trials been in cognitive behavior therapy? To what extent are behavior therapists designing and conducting tests that actually place their cherished beliefs at risk—or to what extent are they practicing "cargo cult"—science in which there are "tests" but there is very little risk of their cherished belief being shown as false? Are they looking at sermons for swearing or after toe stubbing? It will be argued that the general answer is twofold: first, research in cognitive behavior therapy generally has not been properly evaluated on this key dimension—which will be argued is quite problematic; second, there are exemplars where at least some appear to be quite lacking on this dimension.

The Case of "Scientific Research" and Big Pharma: Lessons Learned?

One of the best known recent examples of such problematic science and the lack of severe testing is research that has been conducted by Big Pharma, particularly numerous for clinical trials of antidepressants (Antonuccio et al., 1999). Speaking generally, this research used apparently sound methodologies such as random clinical trials, decent sample sizes, double blinds, statistical analysis, and usually was published in high-impact peer-reviewed studies—that is, with many of the apparent characteristics of sound science and indeed even exceptional quality/high-prestige science. However, numerous critics have astutely pointed out many methodological problems with this research and thus questioned the intellectual virtue of this research (e.g., Antonuccio et al.; Greenberg & Fisher, 1994; Kirsch et al., 2008; Klein, 2006). These problems occurred at a number of levels: blinds were violated; neg-

ative results were file drawered; multiple outcome measures were used but only the outcome variables that failed to reach significance failed to be reported; side effects were not fully reported; safety concerns such as increased suicidality were not explained; multiple statistical analysis were conducted until supportive results were found; statistical significance was conflated with clinical significance; process variables were not directly measured or properly reported, and so on. In addition, it is important to note that most of these problems were not immediately apparent—these were hidden by researchers and uncovered only after often arduous independent investigation. Moreover, the drug researchers themselves also had various personal motivations that were often hidden: they were financially incented in various ways by Big Pharma to find and report positive results; they were offered other inducements such as expense paid trips to present results in luxurious conferences; and the allure of publishing in high-impact journals was also present among other inducements.

Other critics have also pointed out additional problems with research involving other medications—e.g., that there were often deviations in the analysis plan between protocols and published papers, and, interestingly, that the effect sizes of drug interventions are larger in the published literature compared with the corresponding data from the same trials submitted to FDA (Ioannidis, Munafo, Fusar-Poli, Nosek, & David, 2014). Ioannidis et al. (2014) nicely summarized other problems with other drug studies:

For example, in a review of all randomized controlled trials of nicotine replacement therapy (NRT) for smoking cessation, more industry-supported trials (51%) reported statistically significant results than nonindustry trials (22%); this difference was unexplained by trial characteristics. Moreover, industry-supported trials indicated a larger effect of NRT (summary odds ratio 1.90, 95% CI 1.67 to 2.16) than nonindustry trials (summary odds ratio 1.61, 95% CI 1.43 to 1.80). Evidence of excess significance has also been documented in trials of neuroleptics. Comparisons of published results against FDA records shows that, while almost half of the trials on antidepressants for depression have negative results in the FDA records, these negative results either remain unpublished or are published with distorted reporting that shows them as positive; thus, the published literature shows larger esti-

mates of treatment effects for antidepressants than the FDA data. A similar pattern has been recorded also for trials on antipsychotics.

This is a serious concern for obvious reasons—the pollution of the scientific literature which can affect clinical decision making and thus client welfare—but it also is a parochial concern for cognitive behavior therapists because in many cases these psychotropic medications were often seen as in direct competition with cognitive behavior therapies. The general scientific question could be phrased, “Which is more efficacious, this medication, some CBT, or both?” Any jimmying of results toward the medication not only distorted information and placed patient welfare at risk but it also had direct implications for the rational appraisal of the efficacy of CBT. Obviously, something is seriously amiss here—many of the characteristics of science seem to be in place but yet all these efforts seem to be violating what Meehl (1993) once attributed to Bertrand Russell as the fundamental orientation of an intellectually virtuous scientist—“the passion not to be fooled and not to fool anybody else.”

These criticisms of Big Pharma seem reasonable, fair, and important—but this kind of scrutiny to date also seems to be somewhat one sided. Few are asking the extent to which CBT’s research house is in order with regard to such epistemic vice. At first blush, one would have to admit that some of the same personal incentives could be present in CBT research (although perhaps a bit less flush). CBT researchers can have a financial interest to produce positive results—from paid trainings, from book sales, from academic promotions, and from additional salaries from grants, and so on. CBT researchers can also be interested in other inducements such as fame, awards, professional offices, increased citations, and publications in high-prestige journals. The question becomes ought behavior therapy research and behavior therapists also be scrutinized for their epistemic virtue along lines similar to the scrutiny received by Big Pharma?

For example, a case study (see O’Donohue et al., 2016a, 2016b; and for a rejoinder, see Gregg & Hayes, 2016) of a series of publications related to Acceptance and Commitment Therapy and diabetes self-management found several similar problems, including: (a) a failure to report several key negative results from the dissertation in a subsequent peer-reviewed journal publication; (b) a series of overstatements and misstatements by the researchers in subse-

quent publications exaggerating the positive findings in the dissertation; (c) the development of a bibliotherapeutic intervention explicitly marketed to people with diabetes (claiming to be “a proven program”) in which the reader is led to believe the bibliotherapy intervention they were to use had been shown to be effective and safe in past research, when the bibliotherapy intervention had not even been studied at all; (d) the failure to accurately describe in subsequent publications, particularly in the peer-reviewed journal publication, what are at best equivocal findings regarding the role of putative ACT processes as mediating these results. Instead, the opposite is found: clear, but inaccurate, statements about ACT processes producing clinically significant changes in diabetes self-management when the original data simply do not warrant this; and (e) a lack of appropriate caution and qualification in interpreting the data relating to the effectiveness of ACT for diabetes self-management despite numerous methodological shortcomings, including, but not limited to: therapist allegiance effects, dependent measures with unknown psychometrics, no blinding, minimal follow-up, no safety measures, significant attrition, problems with alpha rate inflation, no comparison to key treatments as usual, and no replications. All of these are serious problems and problems that seem to be similar to those found in Big Pharma’s problematic pseudo-scientific research. Interestingly, the existence of these problems sometimes occurred in a context in which the authors were explicitly reassuring readers that they would refrain from excessive claims and would point out unresolved empirical issues, thus providing readers with a false assurance that good scientific practices were being followed. This certainly raises clear issues about bias, pseudoscience, and intellectual vice

It is also important to note that there is an important second-order concern that also needs to be mentioned: Bias can occur not only in the design and reporting of a particular study, but it also can occur in the way studies are aggregated or how that study is spoken about subsequently. Utterances like, “This and other studies show that this treatment is scientifically proven”; “There are 200 RCTs proving the efficacy of X therapy” and so on each can also be examined for bias and epistemic virtue. It may be particularly important to examine these statements as these summary statements may be more influential in practical

decision making than statements about an individual study.

There have been longstanding questions about the epistemic virtue of other research in psychotherapy—for example, with the refusal of proponents of facilitated communication to accept evidence that falsified the notions that facilitated was effective or that its hypothesized process variables were operative (Lilienfeld et al., 2014); the refusals of proponents of EMDR to adequately test simple exposure can explain positive results instead of finger waving (Herbert et al., 2000), or whether claims for the efficacy of positive psychology have vastly outstripped the data (Eidelson & Soldz, 2012). In all these cases, adherents are not disinterested—there are numerous payoffs for ignoring reasonable criticisms, data that is falsifying, and conducting research so that only weak tests are employed that will produce “positive” results. However, like Big Pharma’s distorting research, there are serious consequences to clients and to the scientific literature from such problematic studies.

Science and Virtue Epistemology

If there is such scrutiny of epistemic virtue of scientific practices, how ought this to be understood? One such viable candidate is virtue epistemology (Sosa, 2009). Virtue epistemology is a growing approach to understanding rational agency and the way knowledge can be legitimately gained. Kidd (2016) provides a useful summary:

The core conviction of virtue epistemology is that enquiry is an active process that can go better or worse, and that central among the factors that determine how it goes are the characters of the enquirers who perform it. Since enquiry is initiated and performed by epistemic agents, such as scientists or scholars, the stable cognitive and behavioural dispositions of those agents are surely crucial to the success of that enquiry. (p. 10)

The list of possible epistemic virtues is lengthy as rational belief formation can be evaluated on multiple dimensions: conscientiousness, transparency, discernment, intellectual honesty, and intellectual humility, for example. For our purposes here critical epistemic virtues in research are to honestly and transparently conduct and report severe tests instead of gaming of methodology and scientific reporting to produce weak or pseudo-tests to manufac-

ture in an effort to report only “confirming” results.

There is a growing recognition that such biases occur in scientific research and these epistemic problems need to be both identified and prevented. However, it seems that the field of clinical science and CBT generally has been somewhat of a laggard in this movement. For example, there is no recognition of this in most concepts of empirically supported treatments, such as the well-known Chambless report (Chambless & Hollon, 1998). Instead, any randomly controlled trial with positive results seems to be taken without scrutiny for bias or interest and is taken as sound evidence to gain the mantle of “empirically supported treatment.”

This has perhaps led to a problematic enterprise associated with research in cognitive behavior therapy: “If I can manufacture randomly controlled studies in support of my therapy, I can gain the rewards associated with this.” Of course, the easiest way to do this is to avoid severe testing, as discussed above—one would report data from the sermon not from the golf game. To be more precise, O’Donohue et al. (2016b, p. 40) suggested that these methodological moves would make the manufacture of such positive RCTS possible and would all be problematic from the view point of epistemic virtue:

1. Ensure that therapy allegiance effects are operative in favor of the experimental treatment; for example, by having one therapist strongly aligned with a therapy orientation and the other not aligned with the control condition.
2. Do not use blind data collectors, therapists, or subjects. Give every chance that biases and expectations can be communicated.
3. Once these biases and expectations have been instantiated, rely on self-report as a key outcome measure.
4. Use a small convenience sample of clients who only have relatively low levels of the clinical problem.
5. In single subject experimental designs run more than three subjects but report only the three that provide confirming results.
6. Stop collecting data once $p < .05$ is reached.
7. Do not randomly assign or sample therapists: use the more advanced, more talented, therapist in the experimental condition.

8. Use multiple outcome variables but in any discussion prioritize only those that show statistically significant results. Interpret the nonsignificant results as “minor” instead of falsifications of any beliefs or hypotheses. Or, alternatively, completely fail to mention these in subsequent publications.

9. Have a weak control condition—do not test for equivalence in initial credibility; do not test for the presence of any other key psychotherapeutic processes in the experimental condition such as the nonspecifics. Especially avoid a control that is evidence-based treatment as usual as this is a harder hurdle to beat. Ignore the iatrogenic effects that may realize if any real patients are switched from a more robustly tested treatment as usual due to one’s weak test and exaggerated results.

10. Do not analyze for clinical significance. This is a tougher hurdle, so in discussions conflate statistical significance with clinical meaningfulness.

11. If the experimental therapy condition fails to reach statistical significance on any outcome measure but the means are in the favored direction—report these positively as trends. This still gives a more favorable impression to the original belief system.

12. Do not run many or any follow-ups after therapy is completed even though one may be treating a chronic condition like diabetes. Relapse is a common problem so the absence of long-term follow-ups avoids the detection of relapse which would be a less favorable study.

13. If statistical tests show nonsignificance find another statistical test that shows a significant confirmatory result. Do not report in the publication that a previous statistical test was run that showed nonsignificant results.

14. Use a small unrepresentative sample—which increases the odds of a false positive result. However, make claims that the therapy works for a broad class of patients—seemingly all diabetics, for example.

15. Ignore initial differences if random assignment fails to produce equivalent groups, particularly if these are in favor of the experimental treatment condition.

16. Do not attempt to search for any negative side effects.

17. Do not conduct a failure analysis and do not report the percentage of patients that did not change or became worse in the experimental condition.

18. Be unclear in what exactly the key processes are, e.g., “acceptance” and “commitment,” and how these were instantiated in the research.

19. Have a vague, elastic model of therapy process in which “acceptance,” “emotional avoidance,” “mindfulness,” “valued action,” “deliteralization,” “psychological flexibility,” “recontextualization skills,” “cognitive entanglement,” “loss of core values,” “cognitive fusion,” “domination of conceptualized self over ‘self as context,’” “relational frames” and so on are all intermixed so that it is unclear exactly what actually ought to occur in treatment. Do not acknowledge that many of these allegedly key constructs were not actually tested in the study.

20. Do not provide an assessment plan for each of these many constructs in the study but still use these concepts in theoretical talk.

21. Do not report any problems in the theoretical background of the therapy—e.g., problems in the conceptualization or replication of relational frame theory (see e.g., Roche, 2010).

22. Use measures of unknown or problematic validity.

23. Run analyses on a variety of outcome measures such as change scores, and absolute differences at the end of therapy and report those that show more significant results.

24. Do not conduct analyses on both therapy completers and intent to treat. Generally, ignore attrition; especially do not interpret attrition as a problem for the experimental treatment condition.

25. Make claims that one modality of therapy (bibliotherapy) works even though another modality (a workshop) was tested.

26. In reporting results, simply do not report some hypotheses that were not confirmed.

27. If all outcome measures are negative, then use the file drawer.

28. In publications, make exaggerated summary statements of the state of the science such as “scientifically proven” that ignore any design limitations, any outcome variables that fail to reach significance, any failures, any analysis of relapse, etc.

29. Make misleading statements towards the positive, e.g., the ACT bibliography for diabetes has been studied when it simply hasn't. One can also state that ACT has shown its usefulness in integrated care settings for diabetes when there have been no studies of this.

30. Use honorific and obscurantist language to describe one's approach to science, e.g., “reticulated.”

31. Keep a scorecard regarding number of RCTs supporting one's preferred position but an incomplete one. Do not report the scorecard of the competition such as standard cognitive behavior therapy.

32. Do not mention that the results have not been replicated in an independent laboratory.

33. When asked for therapy manuals to attempt to replicate, indicate that these are not available.

34. Indicate that one is open to criticism but ignore this criticism. (O'Donohue et al., 2016b, p. 40)

Perhaps there has been too much emphasis on cognitive biases such as heuristical errors as affecting judgment of scientists and clinicians (e.g., see Garb, 1989). The biases discussed here provide a more comprehensive and thus accurate view of the biases that can affect science. Bertrand Russell (1950) in his Nobel Prize acceptance speech suggested four main desires that motivate much behavior, including scholarship: acquisitiveness (“the wish to possess as much as possible”); rivalry (“a much stronger motive”); vanity (“a motive of immense potency”); and love of power (“which outweighs them all”). We may note the tremendous degree to which all four desires seem actively at work in shaping science, including research in cognitive behavior therapy. According to Russell, it is important to be clear-sighted on this matter.

Recommendations to Identify Bias and Promote Intellectual Virtue

Figure 1 illustrates common practices and possible solutions across the workflow for addressing multiple biases (from Ioanidis et al., 2014).

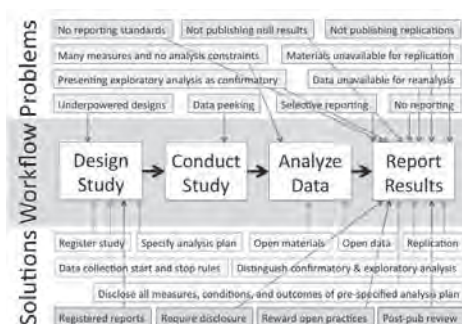


Figure 1. Common practices and possible solutions (retrieved from <https://www.ncbi.nlm.nih.gov/pmc/articles/PMC4078993/figure/F2/>)

Specific Recommendations for Controlling Bias in More Epistemically Virtuous Research

One seems to be confronted with the fact that the epistemically virtuous scientist might be rare or one should at least not assume that research being produced is based on epistemic virtue. Instead, one ought to conduct, publish, and appraise all research. The following steps are recommended:

1. Epistemic vice and virtue are taught as part of research methods and ethics courses.
2. All clinical trials are preregistered. This can allow a better assessment of the use of file drawer, p-hacking, as well as problematic deviations from protocols and post hoc analyses.
3. Part of peer review for journals and grants is evaluating the extent to which methodological decisions were made to construct a severe test vs. to manufacture a positive result. The steps described above as allowing weaker tests are made more transparent and scrutinized and are generally reasons for rejection.
4. Method sections are written to increase transparency by including a subsection in which the study's methodological decisions are elucidated in more detail and sufficient information is provided to evaluate for bias and severe testing.
5. Replications are seen as having increased value and an important part of science. This would need buy-in from journal editors and promotion committees.

6. No research such as an RCT should count as support for an honorific such as an “empirically supported treatment” if the test is a suitably severe test.

7. Summary statements about a body of research are also scrutinized for their epistemic virtue.

Conclusions

It is important that science be conducted with an integrity where its essential functions of error detection operates instead of in a manner in which only the topography of science is present (“cargo cult” science). Big Pharma provides an important object lesson and more CBT research needs to be scrutinized for its epistemic virtue. Perhaps this can result in the increased growth of knowledge and overcome what Meehl (1978) has called “the slow growth of soft psychology” by a more thoroughgoing commitment to Bertrand Russell's recommended orientation for the virtuous scientist, “the passion not to be fooled and not to fool anybody else.”

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Pseudoscience Persists Until Clinical Science Raises the Bar

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TO MANY OF US it is perplexing as to why providers or utilizers of interventions intended to help people in distress ignore research findings that document efficacious interventions in favor of unsupported pseudoscientific therapies. Clinical science programs certainly present the reports on empirically supported treatments (Chambless, 2015; Chambless et al., 1998; Chambless & Hollon, 1998; Chambless & Ollendick, 2001). In research methods or philosophy of science courses, the demarcation criteria for differentiating science from pseudoscience are often taught (Lilienfeld, Lynn, & Lohr, 2015a; Schermer, 2002). Some classes will offer classic debates about whether this distinction between the two can be reliably made (cf. Laudan, 1983; Mahner, 2013; Pigliucci, 2013). Even without philosophical tutoring, at some point it is clear that one has departed from science into pseudoscience (Lilienfeld, 2011, p. 109).

Our interest in this issue gets rekindled when a practice that represents a significant cost to society uses the trappings of science to establish credibility and attract disciples. At some point our scientific values are sufficiently offended to cause us to decry pseudoscientific practices, non-science, or antiscience. Examples have been identified for decades (e.g., Beyerstein, 2001). Researchers have proposed methods for identifying harmful practices (Dimidjian & Hollon, 2010). There may be debate about what is meant by harm. Tragically, there are some cases where harm is indisputable, therapy is abusive, and deaths occur (e.g., Advocates for Children in Therapy, 2017; Chaffin et al., 2006; Mercer, 2014; Singer & Lalich, 1996).

Outside of therapeutic interventions, fraudulent science poses threats to the collective well-being of larger groups of individuals. Recent examples of people being misled by fraud are those who oppose vac-

cinations based on false information linking vaccinations to the development of autism (see Rao & Andrade, 2011, for a synopsis and timeline of Wakefield's retracted report; Wakefield et al., 1998). This report fed into personal beliefs and heuristic errors of parents that place at risk not only their children, but other children who cannot be immunized. At a larger level of analysis, climate change deniers place multiple species at risk of extinction. One thing we know is that once misinformation is received, it is extremely hard to correct (Chan, Jones, Jamieson, & Albarracín, 2017).

If we emphasize science in the training of our students and make available lists of empirically supported treatments, why do people make use of alternative treatments? It might be useful to ask the question of why people are not persuaded by science and go on to create and consume such therapies. Paraphrasing Skinner, it behooves us to study the behavior of the person, because the person is always right. Let us leave aside such factors as greed, gullibility, lack of training, motivated reasoning, naiveté, hopelessness, etc. Let us ask the question of why our appeal to scientific evidence is not sufficiently convincing to keep

the audience for pseudoscience sufficiently small.

Context

So why do pseudoscientific practices persist? It is not because the issue has not been well articulated. For many years Lilienfeld and colleagues and many others have provided thoughtful critiques of pseudoscience in clinical psychology (Lilienfeld, Lynn, & Lohr, 2003, 2015b). Yet practitioners, some psychologists and some not, invent and practice dubious interventions even though other therapies may have documented efficacy. Perhaps there are other sufficiently potent contextual features that make the science less clearly persuasive thus allowing the production and consumption of pseudoscientific practice instead. Let's consider some reasons why clinical science has not preempted alternative practices.

The Choice of the Medical Model

Perhaps one reason the quality of our science is not sufficiently convincing is because the field took a wrong turn in 1980. Psychology misestimated the effects of generally acquiescing to an implicit medical model when accepting DSM-III as the dominant nosology to organize research and practice in clinical psychology (see Kirk & Kutchins, 1992, for a discussion). Several negative effects ensued (see Follette & Houts, 1996, for a critique and alternative; Follette, Houts, & Hayes, 1992).

- *The research strategy.* One ill-effect of this decision was that DSM influenced researchers to construe distressing behavior as one of hundreds of disorders rather than different topographies of a much smaller number of functional classes of behavior that rested on common psychological principles. DSM-III claimed to be atheoretical (American Psychiatric Association, 1980, p. 7). With the exception of PTSD, there were almost no statements of etiology of clinical problems. The result was that treatments were developed to treat disorders with little regard to commonalities that produced or maintained distress. What emerged was treatment X for depression. The same basic treatment X was later developed for (applied to) anxiety, and then other disorders. Less attention was paid to common processes for the development of these disorders from a psychological science perspective. The fact that the same basic treatments worked across several diagnostic categories should have been taken as an occasion for us to question

either our understanding of the treatments for the “disorders” or the validity of the underlying nosology itself.

One effect of adopting an atheoretical nosology seemed to be a reluctance to develop and test theories of mechanisms of etiology or change. Yet specifying testable mechanisms of change is a hallmark of science that distinguishes it from pseudoscience. Treatments often did have theories of etiology or change, but theory testing was not the focus of treatment studies. Instead, studies were often of the “horse race” variety where the winner was whatever treatment produced significantly more reduction in symptoms. The general conclusion has been that the races often ended in ties where many treatments were better than a waitlist control, and most were equivalent to each other. These types of studies are still done and mostly produce similar results. The question of interest in clinical trials was usually whether one got to the finish line but not *how*. Since scientific programs receiving significant funding were being judged by whether they produced improvement, pseudoscientific treatments can often show some amount of self-reported improvement as well. Because randomized controlled trials were not judged by the evidence testing the theories on which treatments were designed, pseudoscientific theories have not succumbed to the criticism that the theories behind them are invalid or untestable. Instead, pseudoscientific interventions are identified by the apparent absurdity of the rationale. We will discuss falsifiability later.

- *Inclusion and outcome measures.* To receive treatment clients had to have a diagnosable disorder. Without a diagnosis, people who were unhappy with life circumstances, relationships, prejudice, or didn't understand the relationship between the environment (writ large) with how successfully they achieved valued goals were never the focus of study. By focusing on disorders rather than including well-being as part of the assessment of outcomes, clinical researchers largely ceded these latter issues to others.

Accepting the presence or absence of a disorder as an outcome measure produces a methodological problem. Effect sizes are generally reported with respect to some measure of change in the degree of distress (e.g., reduction in depression or anxiety scores or no longer meeting criteria for a diagnosis). This choice of dependent measures creates problems for arguing for very large effects. First, the best result one could

achieve is no symptoms of a disorder. Formally, this is an instrumentation threat to internal validity due to a floor effect. If the goal of an intervention is “cure” a disorder, then the best one can do is have zero amount of the disorder. Others have argued that psychology could offer a more robust model of psychological health that would conceptually allow for a richer measurement model of outcome (Bonow & Follette, 2009; Follette, Bach, & Follette, 1993). Unfortunately, for a long time outcome assessment did not differentiate between an instance where depressive symptoms were gone and a second instance where depressive symptoms were gone and the patient was more involved with family, enjoyed an engaging social network, experienced more control over life, or worked and played with greater satisfaction.

If an alternative treatment offers an outcome that is more than misery management but also includes a richer life experience, one can appreciate the appeal of improvement claims beyond “diseased or not” as an outcome. There is no assertion here that the claims offered by alternative treatments were valid, only that the scope of outcomes addressed by alternative treatments can be more appealing. More recently, there have been thoughtful contributions on the treatment quality and outcome assessment measurement domains (e.g., Lambert, 2017; Thornicroft & Slade, 2014), but these additions are late to the game. In the meantime, interventions that made claims to improve the quality of life or enhance control had an opportunity to proliferate.

- *Dissemination.* If there were a reduced audience for pseudoscientific interventions, the problem would fade to a manageable level. Lilienfeld and colleagues have discussed sources of resistance to evidence-based practice by psychotherapists (Lilienfeld, Ritschel, Lynn, Cautin, & Lutzman, 2013). Beyond the difficulties with psychotherapists being convinced by data, others struggle with how to translate scientific information to a variety of audiences (Kaslow, 2015). However, the way clinical scientists disseminate information cannot possibly be as influential on consumer behavior as how purveyors of pseudoscience approach the task. One of the criteria used to identify pseudoscience is the use of testimonials, a practice that is prohibited by the APA in Standard 5 of the Ethical Principle of Psychologists and Code of Conduct because of concerns about vul-

nerability to undue influence (American Psychological Association, 2017). Practitioners outside the purview of APA are not always so constrained.

Social psychologists and persuasion experts have long identified the potency of personal narratives compared to how scientific information is usually conveyed to the public or policy makers. Almost every presidential State of the Union address now includes a policy initiative bolstered by a vivid story that refers to a specific individual who embodies the need for the policy or policy change. Ronald Reagan's vivid description of a "welfare queen" during the 1976 presidential campaign is one such example used to illustrate the candidate's assertion that reform was needed to protect against wanton abuse. In 2009 President Obama made references to three specific instances of individuals who were or would be impacted by policy changes during his State of the Union address. When discussing the improved state of the economy in his 2015 State of the Union address, then President Obama detailed a story of the Erler family, who fell on hard times and recovered in parallel with the economy. When the need for change is advocated or accomplishments touted, the president names such a person or family who is often in the audience who stands and receives an ovation. Certainly, the subtle but powerful influence of social media on public attitudes has been the focus of much attention since the last election, attesting to the power of repetition and volume over facts.

Thus far, clinical scientists have not identified the optimal, ethical ways to better disseminate scientifically grounded practices. Pseudoscience practitioners or complementary alternative medicine providers operate under a different dissemination model. While scientists might argue that the use of testimonials is a way to identify pseudoscience, the public views such testimonials as influential, credible sources of information. The issues related to pharmaceutical "direct to consumer" advertising is more complex than can be addressed here, but it is easy to observe the correlation between advertising and sales of a drug. Even not considering pharmaceutical marketing, woe to anyone watching late-night television who fails to have the correct pillow, doesn't hang by their feet, or does not partake of the cornucopia of dietary and vitamin supplements to improve, well, just about everything. I am unequivocally not advocating for the abandonment of ethical dissemination prac-

tices. I raise the issue that psychology persuasion science would predict that the dissemination practices of pseudoscience would be more effective than those used by psychology clinical scientists. This state of affairs is especially ironic given that social psychology provides some of the foremost experts in persuasion and influence (e.g., Pratkanis, 1995, 2007).

- *Summary.* The initial acceptance of a medical model and the emphasis on efficacy rather than the testing of psychological theory placed clinical science at a disadvantage. By failing to make model testing a primary focus of study, clinical trials did not follow its own methodology for distinguishing science from pseudoscience. The model did not include measures of well-being and improved adaptability but rather only reducing a limited set of symptoms. This constrained the ability to show larger treatment effects and a richer domain of treatment benefits. Though there have been efforts to disseminate findings to practitioners and the public, the methods of doing so are less effective than those who propose and advocate alternative treatments.

Empirically Supported Treatments (ESTs)

Following the evidence-based medicine movement in England, in 1995 APA established the Task Force on Promotion and Dissemination of Psychological Procedures with the laudable goal of identifying and disseminating treatments with known efficacy (Chambless & Ollendick, 2001). How does this decision by APA, and Division 12, contribute to the context that might paradoxically lead to the discounting of scientifically supported therapies? The enormity of the task of sifting through the literature and reliably identifying ESTs required a focus on evidence that a therapy worked. Treatments were not evaluated on how they worked, whether one worked significantly better than other ESTs, how clinically meaningful the observed changes were, or what, if anything, differentiated one therapy from another and contributed to a better outcome (Follette, 1995; Follette & Beitz, 2003; Follette & Houts, 1996; Follette et al., 1992; Jacobson, Follette, & Revenstorf, 1984; Kazdin, 2007, 2014).

- *Mechanisms.* Because mechanisms of an intervention were not the primary focus of study, it was and is possible for the "same" therapy to be reinvented under another name and subsequently appear on

the EST list. Let us consider the roads taken by two therapies, now both with some level of empirical support but dubious theoretical underpinnings. The first example is of a therapy that ultimately produced evidence of efficacy but initially was wrapped in obscurant language and contained unnecessary treatment elements. The second example is of a treatment with an initially well-received theoretical foundation and evidence of efficacy, but eventually has maintained evidence of efficacy but its proposed mechanism of change has been substantially challenged. Both are on the list of ESTs.

Eye Movement Desensitization and Reprocessing therapy for the treatment for trauma and anxiety (EMDR; Shapiro, 1998) garnered considerable criticism, in part, because, among other issues, one of the initially identified treatment components included having clients track the therapist's finger movements that were learned by participating in training and certification programs. However, data accumulated that the eye-movement component of the intervention was not necessary (Hyer & Brandsma, 1997). To many researchers, the important element of the therapy was exposure and habituation. Many considered the initial explanation to use obscurant language, invoke untestable mechanisms, and resulted in monetary gain for the developer. These and other features of the therapy satisfied some that EMDR passed the demarcation criteria for pseudoscience.

Over time an EMDR journal has formed, and studies of EMDR were conducted that met criteria for inclusion as an EST. The rationale for how EMDR worked has also changed (see references in Perkins & Rouanzoin, 2002, for some of the arguments about mechanisms). It is now argued that the intervention results in changes in adaptive information processing (Oren & Solomon, 2012). The mechanism of action still may seem to rely on technical, obscurant language (Oren & Solomon, 2012, pp. 200-201), but in a recent report EMDR does not appear to be an outlier in terms of clinical efficacy when compared to several other therapies for PTSD (Cusack et al., 2016).

The point of presenting EMDR is not to say whether it was or is pseudoscience, or is now more normal psychological science. The point is that many treatments for PTSD make use of in vivo or imaginal exposure, and many refer to changes in information processing that results from the exposure component. Because empiri-

cally supported treatments are not required to demonstrate mechanisms of change nor define ways to identify its essential treatment components, nor describe ways in which a therapy is essentially different for another therapy, nor the conditions under which the treatment and its underlying theory could be fundamentally challenged, the list of therapies continues to grow. More important, until such requirements are established, there is nothing to keep pseudoscientific treatments from competing. The effort to identify ESTs was laudable. Now a more refined strategy is required beyond just showing that a particular treatment produces change (e.g., David & Montgomery, 2011; Follette & Beitz, 2003; Lilienfeld, 2011; Tolin, McKay, Forman, Klonsky, & Thombs, 2015). Until these features of treatments are defined, there is little to dissuade treatment developers from adding superfluous but marketable components to principle-based treatment elements and creating a “new” therapy where the purported mechanism is completely unrelated to how an intervention actually works.¹

Now turning to cognitive therapy (CT) for depression: Beck, in his classic publications (Beck, 1967; Beck, Rush, Shaw, & Emery, 1979), highlighted the role of dysfunctional cognitions in depression. The theory highlighted the importance of the cognitive triad of a negative view of the self, the world, and the future as well as dysfunctional attributional styles in depression. Notions of core self-schemas evolved and CT evolved into an intervention with an articulated mechanism for the treatment of depression that was plausible and was efficacious. However, there have been some important studies along the way that have challenged the purported mechanisms underlying CT.

In 1996 Jacobson and colleagues conducted a component analysis of CT comparing the behavioral activation component of CT, CT with behavioral activation and skills to modify automatic thoughts, and the full version of CT including behavioral activation, skills to modify automatic thoughts, plus the addition of focus on core schemas. That study, involving 150 outpatients, showed that the complete version of CT did no better than its components including behavioral activation, which had

little to do with the direct correction of cognitive distortions. In 2006 another study demonstrated that behavioral activation performed better than CT (Dimidjian et al., 2006).

In 2001, a study was published that examined the relationship between depression, anxiety, and dysfunctional attitudes (DAs) in 521 patients receiving a 12-week course of CBT (Burns & Spangler, 2001). Using structural equation modeling, the study examined four hypotheses:

- (1) changes in DAs lead to changes in depression and anxiety during treatment (the cognitive mediation hypothesis);
- (2) changes in depression and/or anxiety lead to changes in DAs (the mood activation hypothesis);
- (3) DAs and negative emotions have reciprocal causal effects on each other (the circular causality hypothesis); and
- (4) there are no causal links between DAs and emotions—instead, a third variable simultaneously activates DAs, depression and anxiety (the “common cause” hypothesis) . . . This common cause accounted for all the correlations between the attitude and mood variables, and also appeared to mediate the effects of psychotherapy and medication on dysfunctional attitudes, depression, and anxiety. (p. 337)

This study was particularly interesting because the first author, having written a successful self-help book making use of CBT principles (Burns, 1980), had a strong allegiance to CBT. The findings were clearly reported and cast doubt about the mechanism of change for CBT.

What were the consequences of these findings? Certainly cognitive behavior therapy was not deleted from the EST list, but our understanding of how it works is now known to be wrong or incomplete at best. Beck’s most recent theoretic model of depression has changed considerably (Beck & Bredemeier, 2016). It is now a multicomponent model featuring several interacting systems at different levels of analysis. It is an elaborated diathesis-stress model that seems difficult to falsify. The language now states that “depression can be viewed as an adaptation to conserve energy after the perceived loss of an investment in a vital

resource . . .” (p. 596). In an article published in the *APS Observer*, the authors are quoted as saying, “Our model suggests that any intervention that targets key predisposing, precipitating, or resilience factors can reduce risk or alleviate symptoms” [Italics in original] (*Observer*, 2016, April). One can take these statements to allow for a variety of interventions to claim to influence the system Beck and Bredemeier describe.

The point of describing these lines of theory and treatment development is that the route from theory to treatment or treatment to theory unfolds over time. Components were shown to be misunderstood or superfluous in both cases. Both interventions appeal to some combination of practitioners and patients. Neither are effective in all cases. Beck’s and Bredemeier’s formulation is accepting of a vast number of interventions. Therapy designers can construct (fabricate) all kinds of explanations for how an intervention targets resilience or any other component. The reference in the theory to conserving energy almost invites “energy therapies” to justify the intervention in spite of the underlying explanation for energy therapies being considered as classic pseudoscience by many.

Both EMDR and CBT have empirical support. But what is the basis for the support? It cannot be that the theoretical basis was always (or ever was) correct. In the case of EMDR, the treatment charitably had an improbable theoretical basis. The treatment had a component, the finger movements, that seems superfluous. Yet because eventually there was evidence of efficacy, the treatment persisted while the theoretical explanation morphed. Over time, data were accumulated to qualify EMDR as an EST. In the other instance, a treatment with an initially plausible theoretical basis and good initial support was later shown to be effective but for reasons not entirely understood from an initial examination of how that intervention was thought to achieve its effect.

At some point both treatments made their way onto the EST list. At some point the theoretical rationales for both failed. In both cases the treatments remain on the EST list.

The Practice of Clinical Science Research

One of the features of a psychotherapy that gets labeled as pseudoscience is that it is, in principle, not subject to falsification. In an oft-cited paper Platt (1964) argued

¹ Space constrains don’t allow for discussions of placebo effects (Kirsch, 2008; Stewart-Williams, 2004), common factors, or the Dodo bird arguments (cf. Honyashiki, et al., 2014; Rosenzweig, 1936; Wampold, 2015).

that for the social sciences to advance at a more rapid rate, it needed to utilize strong inference tests, similar to those in physics. Such tests pit competing theories against each other. Ideally, one would identify two theories that would make opposite predictions in a particular experimental condition, run the experiment, and the result should be that one hypothesis is falsified and eliminated from further consideration.

Such experiments are actually difficult to conduct because in order to test a primary hypothesis, all related assumptions or auxiliary hypotheses must be valid. If an experiment (or clinical trial) does not perform as predicted, it may not be that the therapy or theory is incorrect, but that there may have been a problem with the measurement instruments, training, fidelity, adherence, etc., that could account for the outcome rather than a problem with the underlying theory or therapy design (see Curd & Cover, 1998, for a discussion of the Duhem Quine thesis that raises this issue).

Consider an elementary school science teacher who intends to show her class that water boils at 100° C. During the demonstration water boils at some other temperature. Rather than concluding to the class that known gas laws have been falsified, she would have to determine that all the necessary auxiliary assumptions were met, i.e., that the thermometer was accurate, that the water was free of impurities, and the experiment was conducted at 1 standard atmosphere of pressure. If any of those assumptions were shown to be false, the claim that water boils at 100° C is never directly tested, and the theory could not be falsified.

In clinical psychology the problem is considerably more difficult because the auxiliary hypotheses usually involve hypothetical constructs of cause and hypothetical constructs of effects that are not directly measured. Depression, adherence, competence, alliance or outcomes do not have the same potential to be directly assessed (or even have a consensus definition) that temperature, water purity, or the atmospheric pressure at the time of the experiment do.

- *Programmatic research.* For several decades the gold standard for programmatic research has been the randomized controlled trial (RCT). The RCT usually follows earlier phases of research to demonstrate feasibility and gather the data necessary to plan the larger-scale RCT. One of the decisions researchers have made was to use highly selected participants where the participants were eligible for inclusion

if they met the diagnostic criteria for one selected disorder but showed no other clinical problem. Such studies have been used in RCTs to identify efficacious treatments. The logic is that if the treatment does not work on a “pure” sample for which it was designed, it is probably not likely to produce an effect large enough to pursue in more complicated cases. In many such studies the comparison is made between the active treatment and a no treatment control and then to another active treatment or treatment as usual.

While this strategy has identified many empirically supported therapies for specific disorders, the scientific evidence has not been sufficient to persuade the majority of practitioners to use ESTs. As in evidence-based medicine, the adoption of ESTs by primary practitioners has been limited because practitioners do not treat highly selected samples without other complicating factors. Explaining with precision and scope, how to apply the science purported to underlie the treatment when applied to more complicated cases has not been persuasive (see Lilienfeld, et al., 2013, for an elaborated discussion of resistance to adopt evidence-based practices).

- *Has the EST effort been progressive?* Recently, NIMH has recognized that a reliance on the medical model, and DSM in particular, has not served the research agenda well. Noting that perhaps the focus on efficacy research may have been a mistake, now effectiveness research is favored, where it is hoped that less restricted criteria for inclusion might lead to larger, more general principles of intervention and results that will have more reported applicability to practitioners.

Additionally, NIMH is now interested in identifying mechanisms of change, not just evidence that change occurs, but how it occurs. These changes, for better and for worse, recognize that the earlier strategy for advancing science has not yielded the results one might anticipate given the time and money invested (Cuthbert & Insel, 2013; Insel, 2014; Insel & Gogtay, 2014). Without statements of mechanisms of change that are falsifiable, judgment about one of the central issues in the demarcation problem are almost impossible to adjudicate.

That NIMH has abandoned the research strategy used from the 1980s to the beginning of this decade suggests that the clinical research strategy has not delivered a convincing, progressive science. The change described in the Research Domain

Criteria, for better or worse, now seeks to identify mechanisms of change or influence (Cuthbert & Insel, 2013; Insel, 2014; National Institute on Mental Health, 2011).

How progressive have our treatment development programs actually been? Lakatos (1974) recognized that research is typically programmatic. Research programs do not initially start with fully developed theories and therefore may not initially be experimentally supported. He allows for modifications to either the core theory or the auxiliary hypotheses to account for contrary findings. Lakatos suggested that as long as modifications to the theory (a) account for findings that provided counter-evidence to the theory, and (b) provide for novel predictions not entailed in the prior version of the theory, such theory revisions are permissible and indicative of a progressive research program. Modifications to the theory that did not accomplish both goals and were not supported by empirical research were ad hoc modifications and indicative of a degenerating program of research.

In clinical science it seems rare that a theory is refuted, though it is easy to find individual articles attempting to do so (see above discussion of EMDR and CT). It is difficult to identify the process where a therapy is falsified and discarded. Hosts of auxiliary hypotheses are invoked to explain apparent deficiencies in the theory. Modifications are offered but rarely evaluated as to whether they are ad hoc or progressive (consider the history of modifications to the learned helpless model of depression). Perhaps this is one reason why pseudoscientific therapies persist—there is no good model for discarding a practice or defining the acceptability of a modification to a theory or practice. As Paul Meehl once stated of theories, “Most of them suffer the fate that General MacArthur ascribed to old generals—They never die, they just slowly fade away” (Meehl, 1978, p. 807). Tools for the evaluation of mediators and moderators have been developed and refined (MacKinnon, 2008), but no consensus exists about how to conceptually compare the results of such analyses with respect to how comparably sized mediation effects advance our understanding of how one theory prevails over another or whether the magnitude of a mediator is sufficient in size to be conceptually meaningful.

- *Summary.* Clinical science has focused on efficacy studies that have not been convincing to practitioners. The strat-

egy has been replaced by a call for more meaningful effects and an understanding of mechanisms. As of yet, there is no agreement about how to identify when a theory has been refuted. Without being able to define the criteria for discarding a theory on the basis of evidence, it makes it difficult to argue that scientific and pseudoscientific practices actually meet different standards. Currently the difference between a theory being not falsifiable versus not knowing when or how to falsify theories that rest on hypothetical constructs may be a distinction without a difference.

Social Factors

So far commentary on our failure to mount a powerful methodological attack on pseudoscience has focused on our research and analytic shortcomings. However, there are social influences that undermine the perceived value of making use of evidence-based practices.

- *Financial support.* Many products and practices that are considered to be examples of pseudoscience fall under the rubric of complementary alternative medicine (CAM). There is considerable variability in costs of complementary alternative treatments. The Affordable Care Act does not allow insurance companies to discriminate against health providers with a recognized state license. If an insurance policy provides for mental health services, then a consumer has the possibility of finding a licensed practitioner to deliver nontraditional therapy and get reimbursed. Reimbursement varies by state. Even where insurance may cover some licensed service, it does not allow for reimbursement for nonlicensed treatments such as aromatherapy, Ayurveda, cryotherapy, reflexology, vibroacoustic therapy, crystal therapy, and the like. However, lax restrictions imply that all permitted choices share the same evidence for efficacy. Of course, they do not. There are no data on exactly how much money people spend on pseudoscientific alternatives to psychotherapy or for what problems people seek such services that may be out of the purview of ESTs. For complementary or alternative medicine, data do indicate that consumer out-of-pocket spending is about \$34B or 1.5% of total health care expenditures in the U.S. Approximately 2/3 of those expenses were for self-care purchases (NIH National Center for Complementary and Integrative Health, 2007). In addition to public policy making it appear that all reimbursed services are equal, the fact that consumers pay

for these services establishes an expectation that they will be beneficial.

- *Reasons for seeking alternative care.* There have been attempts to identify reasons why patients seek CAM treatments. In 1994, a small sample of physicians were surveyed in Washington, New Mexico, and Israel. That study reported that in the last year, 60% of physicians made referral to alternative providers. The referrals for alternative care included spinal manipulation, naturopathy, spiritual healing, and movement therapy, among other forms of interventions. The rationale for referrals included patient requests, cultural beliefs, failure of conventional treatment, and physician beliefs that patients had nonorganic disease (Borkan, Neher, Anson, & Smoker, 1994). In a 1996 study that utilized phone interviews, a sample of CAM utilizers were characterized as unconventional and reported a lack of confidence in conventional medical treatment (McGregor & Peay, 1996).

The CDC National Health Interview Survey interviewed over 30,000 U.S. adults and examined the utilization of 27 CAM treatments (Barnes, Powell-Griner, McFann, & Nahin, 2004). The study reported that 36% of adults used some form of CAM treatment in the last 12 months (62% when prayer was included). Mind-body interventions were among the 10 most common CAM therapies utilized within the last 12 months of the data collection. Respondents with anxiety or depression were the most frequently identified users of CAM for those who self-identified as having a mental disorder. CAM users reported a variety of reason for using CAM treatments including belief that a combined approach would be useful, conventional medical professionals suggested it, belief that it would be interesting, cost, and believing conventional treatment would not be helpful.

Another study by Kessler and colleagues utilized a nationally representative phone sample with over 2,000 respondents (Kessler et al., 2001). Two findings were particularly interesting. First, a majority of those with “anxiety attacks” and “severe depression” reported the use of CAM treatments. Second, the proportion of anxious and depressed respondents who reported CAM treatments “very helpful” was comparable to those who rated conventional treatment the same. The authors state that, “No evidence was found for significant variation in the perceived helpfulness of complementary and alternative therapies

on the basis of whether the respondent also used conventional therapy” (p. 291).

Reports on the efficacy of treatment for both depression and anxiety in the psychotherapy literature are variable, but a reasonable estimate is that about half the people respond significantly and half do not. Clinical science cannot yet provide outcome data so convincing as to negate the demand for alternatives. It does not seem likely that thoughtful instruction to the public will help them discern the threats to validity and heuristic errors that even clinicians make when assessing actual versus spurious therapeutic effectiveness (Lilienfeld, Ritschel, Lynn, Cautin, & Latzman, 2014).

- *Summary.* In addition to problems in being able to mount a strong theory-based argument against the use of alternative treatments, there are social and cultural factors that support the continued use of such interventions. Social policy makes many practices seem equivalent; professionals may actually refer to alternative practitioners; there is distrust of conventional treatments; combined treatments may be presumed to offer the best of both worlds; alternative treatments may address important issues consumers believe are not addressed by more conventional treatments.

Conclusion

The application of criteria to identify the differences between clinical science and pseudoscience have been noted. One of those important features is the ability to falsify the theoretical basis for an intervention. There is nothing in this paper that prevents one from identifying absurd practices. In clinical psychology the predominant research strategy has focused on efficacy and not tests of the underlying theory upon which the intervention is based. Even if we can describe a method for rejecting a clinical theory, with few exceptions, we have not done so. That means that consumers look for perceived benefits and not for scientific justification when choosing a treatment. Social influences support the notion that treatments are equivalent. Until we take on the task of defining whether a research program is progressive or not, we will be lacking the strongest argument against consumers using pseudoscientific interventions.

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De-implementation of Harmful, Pseudo-Scientific Practices: An Underutilized Step in Implementation Research

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IN CLINICAL PSYCHOLOGY, implementation research has focused on sustained use of evidence-based psychosocial treatments (EBPT) by testing different implementation strategies and models to improve treatment delivery and patient outcomes. However, an integral and foundational step of implementation has been understudied and frequently overlooked until recently: the de-implementation of less effective practices. De-implementation is defined as ceasing the use of previously implemented practices (Niven et al., 2015). Fortunately, strategies for de-implementation are emerging, which pave the way for implementation of EBPTs. We suggest that this recent work can be extended to the challenge of reducing harmful, pseudoscientific practices. In this article, we will address the danger of harmful, pseudoscientific practice and how it can inhibit the implementation of EBPTs. Using de-implementation models from the medical sector and clinical psychology implementation research, we will also briefly outline the steps required to de-implementing harmful practice in the mental health sector (e.g., Elshaug, Watt, Moss & Hiller, 2009; Niven et al.).

The Problem: Harmful, Pseudoscientific Practices

Before embarking on the process of de-implementing harmful, pseudoscientific psychotherapies, it is critical to define pseudoscience. A pseudoscientific practice is one that uses vague and broad scientific language yet falsely promotes the reliability, efficacy, or effectiveness of the practice (Hansson, 2013). Lilienfeld and his colleagues (2015) go further to point out specific tendencies of pseudoscience: “1. An overuse of ad hoc hypotheses designed to immunize claims from falsification... 2. Absence of self-correction... 3. Evasion of peer review... 4. Emphasis on confirmation rather than refutation... 5. Reversed

burden of proof... 6. Absence of connectivity... 7. Overreliance on testimonial and anecdotal evidence... 8. Use of obscurantist language... 9. Absence of boundary conditions... [and/or] 10. The mantra of holism” (pp. 7-10). This article focuses on pseudoscientific practices that include one or more of the above tendencies. More specifically, we define a harmful, pseudoscientific practice as one that has empirical evidence of long-term physical or emotional harm on patients or other individuals in the patients’ lives (Lilienfeld, 2007).

Conversion therapy, a treatment used to change sexual orientation in the mid-1900s, is a common example of a harmful, pseudoscientific practice. The American Psychological Association (APA) and the National Association of Social Workers, among other associations, deemed conversion therapy to do more harm than good to patients (APA, 2009; Jenkins & Johnston, 2004). For example, conversion therapy is unsuccessful 70% of the time and frequently leads to depression, avoidance of intimacy, de-masculinization, and loss of religion. Implementing evidence-based psychosocial treatments can serve as a solution to stop the use of harmful, pseudoscientific practice like conversion therapy (Hansson, 2013). However, to forego the de-implementation of the harmful practice before implementing a new EBPT may result in unsuccessful implementation (Niven et al., 2015). For example, if a clinician’s case conceptualization or selection of interventions remained more consistent with the theory and practices of conversion therapy, while attempting to deliver an EBPT, the EBPT would be unlikely to achieve the desired results, and their confidence in the EBPT would remain low.

A Potential Solution: De-Implementation of Harmful, Pseudoscientific Practices

Although de-implementation is most commonly studied within the realm of low-value medical treatments, the existing de-implementation frameworks can be used to guide de-implementation within mental health (e.g., Elshaug, et al., 2009; Henshall, Schuller, & Mardhani-Bayne 2012; Ibargoyen-Roteta, Gutiérrez-Ibarluzea, & Asua, 2010; Montini & Graham, 2015; Niven et al., 2015; Polisen et al., 2013; Prasad & Ioannidis, 2014). Although we focus in this article on harmful, pseudoscientific practices, the guidelines below can be used for de-implementation of ineffective practices that are not necessarily pseudoscientific or harmful.

Guideline 1: Identify and Prioritize the Harmful, Pseudoscientific Practice

Before implementation leaders can begin to de-implement a harmful, pseudoscientific practice, they need to identify which practice(s) to target. A high-level analysis should initially take place where implementation leaders conduct a meta-analysis of existing data on therapeutic outcomes with a focus on articles that provide evidence of a harmful treatment (i.e., declining patient outcomes, increase of symptoms after treatment, etc.; Lilienfeld, 2007). After an exhaustive list of harmful, pseudoscientific practices is created, the practices should be reviewed to determine which to target first. The following are aspects to consider when deciding which harmful, pseudoscientific practice to de-implement first:

- *Evidence base.* Priority should be given to a harmful practice with the most data documenting patient harm (Elshaug et al., 2009; Henshall et al., 2012; Ibargoyen-Roteta et al., 2010). In the case of psychotherapeutic practices, harm can include worsened patient outcomes, and/or emotional and physical harm to a patient’s family or friends. Moreover, it is integral to the prioritization of de-implementation to consider the populations in which the harm is documented, while focusing on research evidence that includes participants with demographics most related to the system’s specific patient population. Prioritization based on the existing evidence and highest impact changes is key to successful de-implementation of harmful, pseudoscientific practice, as it is important to be realistic about the amount of change

that is feasible in the short term within an organization, particularly if multiple new interventions will need to be introduced to replace existing practices.

- *Severity of functional impairment.* Implementation leaders and other stakeholders must consider the degree to which the functional impairment of the patients engaged in the harmful, pseudoscientific practice interferes with daily life (Elshaug et al., 2009; Ibarгойen-Roteta et al., 2010; Polisen et al., 2013). Leaders should focus de-implementation efforts on the practices delivered to patients who experience the lowest quality of life.

- *Financial burden and resource allocation.* Priority should be given to harmful, pseudoscientific practices that pose an extreme financial burden on patients, clinicians, clinics, and/or insurance companies compared to an alternative EBPT (Elshaug et al., 2009; Henshall et al., 2012; Polisen et al., 2013). Generally speaking, it is more likely that stakeholders will support de-implementation efforts if implementation leaders can report on expected savings. If the alternative EBPT costs clinicians less money to train, patients less money to complete, and clinics less money to provide, the successful de-implementation of the old, harmful, pseudoscientific practice will be more likely. While there are costs associated with all treatments, some therapies make more sense to de-implement because of the extent of the costs. For a medical example, the radical mastectomy was a popular yet expensive surgery for breast cancer in the late 1800s to early 1900s (Montini & Graham, 2015). Researchers later discovered other safer and lower cost methods of removing such tumors. Hospitals and facilities were able to successfully de-implement radical mastectomies because they recognized financial incentives of the change (Montini & Graham).

- *Policy mandates.* All individuals involved in the de-implementation process should consider harmful practices that run counter to policy, mandates or clinical practice guidelines a priority to increase patient, clinician, and clinic buy-in for change (Elshaug et al., 2009; Polisen et al., 2013). While external motivation may not always be the best way to promote change, mandating change can still help persuade clinicians to de-implement the practices that cause harm to patients.

- *Existing alternatives.* As we will discuss later, presenting an alternative EBPT is one of the most useful tools in increasing proponent buy-in and sustaining the de-implementation of a harmful, pseudoscientific practice.

Guideline 2: Increase Proponent Buy-in

After leaders identify the harmful, pseudoscientific practice to de-implement, they should begin to increase provider buy-in for the change. As described by Niven and colleagues (2015), the engagement of stakeholders is a critical step in the de-implementation process. First, implementation leaders must ascertain what pressures and barriers exist for proponents of the pseudoscientific practice before intervening. Even if some clinics and systems mandate a policy change, it is important to engage individual clinicians. We suggest first learning about the core values of clinicians who provide the identified harmful, pseudoscientific practice. These values are typically related to providing patients with the best possible care to increase the likelihood of recovery. Once those leading the implementation effort identify the core values, they can frame the need to de-implement the therapy in terms that reflect those values.

To further increase clinician buy-in, Lilienfeld and colleagues (2013) recommend involving clinicians in the dissemination of information regarding the pseudoscientific nature of the targeted practice and to present the alternative EBPT. Researchers can involve clinicians in reviewing research and evaluation data (on both pseudoscientific and evidence-based treatments) to increase the clinician's basic understanding of the effectiveness of the current practice. Moreover, researchers need to present research findings that point to a practice's lack of evidence in an easy-to-understand manner. Often clinicians resist de-implementing pseudoscientific practice or implementing an EBPT because of the complexity in which a researcher presents the findings (Lilienfeld et al.).

Developing a system of patient outcome measurement that feels relevant to the clinicians and their patients may increase clinician support to de-implement certain harmful practices. Clinicians can use this system to see firsthand whether the practice is working. In the case of harmful, pseudoscientific practice, the clinician will notice that the patient is not improving based on measured outcomes. Systematic measurements can help clinicians recognize their current strategies are not yielding

the desired effects, and can facilitate support for de-implementing the practice and a willingness to try something new.

Guideline 3: Identify Barriers and Facilitators to De-implementation

Another guideline to consider is identifying barriers and facilitators to the de-implementation of the harmful practice within a specific clinic to help inform de-implementation efforts. We suggest incorporating all types of stakeholders in this step to gather facilitators and barriers specific to different levels (e.g., patient level, clinician level, facility level, etc.). Bringing in the perspective of each stakeholder will achieve two goals: (a) to increase support and understanding of the change early on and (b) to measure feasibility across different levels. For example, patients can best explain their needs within a therapeutic context, while clinicians can express their goals and concerns, and clinic leaders can bring up structural issues within the clinic that may interfere with de-implementation. Looping individual stakeholders in at this point can be extremely helpful in building support for change throughout the entire de-implementation and implementation process. Frameworks and measures exist to guide this assessment (cf. Aarons et al., 2011; Rabin et al., 2016). Once the stakeholders identify a comprehensive understanding of barriers and facilitators to de-implement, the implementation leaders then need to select, tailor, and implement the de-implementation intervention depending directly on the stakeholders, facility, clinicians, and patient-level needs within a specific clinic (Powell et al., 2017).

Guideline 4: Develop a Sustaining De-Implementation Strategy

The next step is to determine a de-implementation strategy or, more likely, a set of strategies. Below we provide possible strategies to develop and sustain the de-implementation of a harmful, pseudoscientific practice.

- *Strategy 1: Implement an alternative EBPT.* Part of the de-implementation process is to give hope to clinicians that a better alternative exists. If implementation leaders do not present a new practice with evidence, clinicians will likely feel no need to stop the old practice, and may in fact feel pressure to offer something else, defaulting to the practices they know best. The options for new practices should be presented in an easy-to-understand manner,

and then compared with the old practice in terms of the clinic's goals and mission. If an evidence-based alternative does not exist for the specific clinic-level needs, it may be wise to collect practice-level data or partner on research to test and refine practices that are identified as the best available alternatives, perhaps benchmarking against previous program evaluation data on the pseudoscientific practice (see Strategy 3).

- *Strategy 2: Provide consultation.* Implementation leaders should provide a space in which clinicians can give feedback and ask questions about the de-implementation process. By creating this space, clinicians will feel involved in the process and ideally will align with the need to de-implement the harmful, pseudoscientific practice. Those tasked with implementation of effective practices should also provide consultation on how to de-implement the therapy and replace it with the new alternative. For example, clinicians could meet once a week and present their cases to understand what to do in place of the old pseudoscientific practice. Consultation is often studied under the context of training clinicians in EBPTs (Beidas, Edmunds, Marcus, & Kendall, 2012; Nadeem, Gleacher, & Beidas, 2013). Nonetheless, implementation researchers should incorporate consultation in the earlier phases of de-implementation to enhance the implementation process. Ongoing consultation and support, or the development of internal resources to support evidence-based practice, is likely to be necessary to ensure that the practice changes are sustained.

- *Strategy 3: Evaluate patient outcomes.* Comparing pre- and postpatient outcomes may help clinicians see the benefits firsthand of de-implementing the harmful, pseudoscientific practice. Before doing this, researchers may need to develop better methods of outcome measurement. This should take place in two ways: at the clinician level and at the clinic or system level. For example, clinicians who see patients with anxiety disorders could use the Beck Anxiety Inventory (BAI) or General Anxiety Disorder 7-item (GAD-7) to compare patient outcome before and after changing from a pseudoscientific practice to an EBPT. Measures of quality of life and functioning and client satisfaction are also important to examine. Using outcome tracking to evaluate patient outcomes gives clinicians an empirical way to see positive change among patients. Measurement-based care may also allow clinicians to feel

in control, in that they could change the treatment plan if the measures revealed patient improvement, worsening, or no change.

Clinic-wide outcome monitoring will likely help all stakeholders see the improvement in patient outcome before and after the de-implementation of the pseudoscientific practice. This will also let clinicians see that the clinic leadership is observing the degree of patient improvement and the clinician's role in achieving the improvement. By collecting and presenting the pre-post comparison data, clinics can further evaluate the extent to which of the de-implementation improves patient outcomes. If the results show little improvement or an increase in patient symptoms and other valued outcomes, clinics can use the data to rethink and reevaluate the implementation strategies or the new practices that have been identified.

- *Strategy 4: Incent the delivery of effective alternatives.* Emerging evidence suggests that clinicians and clinics are more likely to implement EBPTs when they receive external rewards for doing so (Andrzejewski, Kirby, Morral, & Iguchi, 2001; Carise, Cornely, & Gurel, 2002). At a policy level, possible strategies to facilitate de-implementation include incentives such as preferential contracting with agencies that use EBPTs (McLellan, Kemp, Brooks, & Carise, 2008), block grants to fund initial EBPT implementation, and enhanced reimbursement rates for EBPTs (Magnabosco, 2006). These incentives might increase an organization leader's support for discontinuation of pseudoscientific practices and a transition to EBPTs. Research also suggests that incenting clinicians to deliver EBPTs can lead to improved adherence and intention to deliver EBPTs (Garner et al., 2012). Such rewards could be contingent on demonstrating that EBPTs have in fact replaced harmful practices.

Guideline 5: Sustain the Implemented Effective Treatment

To ensure the permanent de-implementation of a harmful, pseudoscientific practice, implementation leaders need to focus their efforts on sustaining the implementation of the alternative EBPT. If clinicians begin to drift from the EBPT that replaced the de-implemented practice, they may fall back on old harmful practices. Ongoing support and the use of implementation strategies to promote and sustain new and effective practices are essential to

de-implementation efforts. Overviews and frameworks for implementation and sustainability will provide a richer understanding of the process of implementation (Aarons et al., 2011; Damschroder et al., 2009; Kilbourne, Nuemann, Puncus, Bauer & Stall, 2007; Stirman, Gutner, Langdon, & Graham, 2016).

Conclusion

Although relatively understudied within the mental health sector, de-implementation of harmful, pseudoscientific practice is a critical initial step in a successful implementation process of an EBPT. Failing to attend to harmful, pseudoscientific practice within a clinic or system can lead to an eventual return to the harmful practice. Clinicians should therefore work to cease the use of harmful, pseudoscientific practices to achieve the overarching goal of therapy: improve patient outcomes. The present article highlights guidelines and frameworks based from medical research that may guide the de-implementation of pseudoscientific practice: (a) identify and prioritize the harmful, pseudoscientific practice; (b) increase proponent buy-in; (c) identify barriers and facilitators of the de-implementation; (d) develop a sustaining de-implementation strategy; and (e) sustain the implemented effective treatment. These guidelines can also be used to support de-implementation of those practices that are not pseudoscientific or harmful, but less effective than established EBPTs. We advise that any time EBPTs are to be implemented, that both the less effective, and the potentially harmful existing practices be identified, and that implementation efforts focus on strategies for de-implementation of these practices as well as implementation of new practices.

A major limitation of the present article is the lack of research specific to de-implementation of harmful, pseudoscientific practice within the field of clinical psychology. We recognize the need to study such guidelines and frameworks within the mental health context. We encourage further attention to de-implementation in both research and practice contexts, as it may be necessary to ensure the delivery of effective care.

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What's a Therapist to Do When Clients Have Pseudoscientific Beliefs?

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MENTAL HEALTH PRACTITIONERS have long struggled to assert their authority on matters psychological. Even now that mental health services have become more widely available than they once were, practitioners suffer by comparison to the medical profession. Seeking psychological services is often stigmatized in a way that medical treatment is not (Sartorius, 2007; Schulze, 2007). In addition, physicians gain an air of authority from their highly technical subject matter. In contrast, everyone witnesses human behavior every day. What could be so difficult about knowing why people act the way they do?

Indeed, the problem is much larger than just mental health professions. Today, the denial of authority extends to almost anyone claiming to be an expert. Scientists—who should be afforded some credit in return for their extensive training and the quality of their data—are often at odds with the views of the general public. A recent Pew Research Center poll found an astonishing 51-point gap between the views of U.S. adults and members of the American Association for the Advancement of Science (AAAS) on the safety of genetically modified foods (GMOs; Funk & Rainie, 2015). Eighty-nine percent of AAAS members said GMOs were safe. (They can't all work for Monsanto!) Similarly, the Pew study found that 87% of scientists agreed with the statement, "Climate change is mostly due to human activity," compared with only 50% of U.S. adults.

A recent book decries the "death of expertise" (Nichols, 2017b), and there is no shortage of anecdotal evidence to certify the death. The United States recently elected a real estate developer with no prior government experience to be president, and he went on to appoint a number of people to high-level positions who were similarly lacking in expertise relevant to their assignments. As just one example, the new administration appointed a former conservative radio talk-show host to the highest science position in the Department of Agriculture—a man whose only science degree was a B.A. in political science (Geiling, 2017; Nichols, 2017a). The new president came into power by campaigning

against the "elites," repeatedly asserting, "I alone can fix it" (Jackson, 2016).

So how should we respond to these challenges? Michael Bowen (2017), writing for the World Economic Forum's Young Scientists Community, asserts that we are confronted with "a populist backlash against scientific consensus and expert opinion" and urges scientists to strengthen their resolve and fight back with facts. But it seems like scientists have been fighting back with facts and evidence for a while now, with minimal results. It has been 19 years since Andrew Wakefield published his infamous study in *The Lancet*, purporting to show a relationship between the MMR vaccine and the incidence of autism. Many failures to replicate Wakefield's results followed, and 7 years ago, the British General Medical Council revoked Wakefield's medical license and *The Lancet* withdrew his 1998 article (Offit, 2010). Much ink has been spilled and words spoken in an effort to use facts to convince parents that vaccination is safe and important, but a 2015 poll found that only 84% of Americans thought vaccination of young children was very or extremely important, down from 93% fourteen years earlier (Newport, 2015). In 2014 the Centers for Disease Control reported a record 663 cases of measles, the "greatest number of cases since measles elimination was documented in the U.S. in 2000" (Centers for Disease Control, 2017).

Lest hubris begin to set in, it should be acknowledged that therapists are far from immune to nonscientific practices. Recent evidence shows that many practicing psychologists and social workers are using techniques that are unsupported by scientific evidence (Barnett & Shale, 2012; Pignotti & Thyer, 2009; Stapleton et al., 2015). As a result, considerable effort needs to be aimed at healing ourselves (Lilienfeld et al., 2013). But putting that issue aside, let's assume you are a behavior therapist committed to evidence-based practice (EBP) who is confronted with a client who is equally committed to Reiki, chelation therapy, or homeopathic medicines. What is a therapist to do?

In the long term, the solution to these conflicts may come from better public edu-

cation in science and critical thinking. In addition, Lilienfeld, Lynn, and Lohr (2014) offered a number of suggestions for reforming the standards and training of clinical psychologists. But these societal and professional reforms will not come in time for therapists who have credulous clients in their offices today. Understanding this, I will discuss four possible strategies for dealing with unscientific client beliefs: adopting, avoiding, reasoning, and collaborating.

Adopting

Although it may seem odd to consider adopting the unscientific ideas of your clients, it is not without precedent. Confronted with a client who has a particular worldview, therapists have been known to incorporate client beliefs into the treatment plan. Sweat lodge ceremonies have been recommended as part of treatment for posttraumatic stress disorder in Native Americans, and other practitioners have suggested praying with or for clients during therapy (Meichenbaum, n.d.; Silver & Wilson, 1988). Therapists who adopt these methods may have the admirable goals of acknowledging cultural or religious differences or wanting to make clients feel more at home, but the ABCT is an organization committed to EBPs (ABCT, 2017). Without convincing empirical support, these practices represent an ethical dilemma for the therapist. Furthermore, if therapists hope to project a consistently evidence-based image to the public, adopting nonscientific methods will only muddy the waters and make it harder to distinguish the profession from other non-evidence-based practitioners. Finally, in the case of sweat lodge ceremonies and a number of other nontraditional methods, there may be substantial safety concerns (Dougherty, 2009). As a result, adopting nonscientific client beliefs as part of therapy is not a recommended strategy.

Avoiding

From a utilitarian viewpoint it might be reasonable to say nothing. As long as the client is faithfully following through with your treatment recommendations and making progress, a pragmatic strategy might be to avoid confronting the client's misconceptions and say as little as possible about the pseudoscientific methods being used or advocated by the client. When therapists are directly asked about non-EBP treatments, they are under an ethical obligation to provide accurate information,

but given that the primary goal is improving client well-being, saying nothing may sometimes be an option.

However, biting one's tongue will rarely be a comfortable choice because the therapist risks appearing to give credence to an unsupported treatment, and just as in the case of the "adopting" strategy, it is important to present the profession as consistently guided by evidence. But, in the interest of keeping positive momentum going, individual therapists may choose to avoid unnecessary battles. Unfortunately, sometimes the client's unsupported remedies obstruct the implementation of evidence-based interventions and/or are potentially harmful. In these cases, avoidance is not an option.

Reasoning

In addition to a rejection of experts, the current era has seen a decline in the value of rational argument. Indeed, sophistry appears to be enjoying a period of growth. During the 2016 U.S. presidential campaign the eventual winner was greatly rewarded for his use of derogatory nicknames for his political rivals, a practice that has continued during his presidency (Estepa, 2017), and formerly trusted news sources are now routinely labeled "fake news."

As difficult as the current environment appears to be, a discussion with clients about basic research methods and levels of evidence—or lack of evidence—supporting various methods is worth trying. It would be impractical to administer a full course in critical thinking; however, some therapists have had success giving clients reading material about both EBPs and non-EBPs (Kay, 2015). But what if those early conversations don't go smoothly? What's a therapist to do?

If there is a benefit of the current climate of rampant credulity it is the emergence of a growing literature on the best methods for debunking misinformation. In 2012, Lewandowsky and colleagues published a very useful qualitative review, and recently Chan, Jones, Jamieson, and Albaracin (2017) published a meta-analysis of the effectiveness of various debunking methods. These studies point to a number of recommendations about how to successfully counter misinformation, and several of these may be useful to the clinician who hopes to steer a client towards EBPs:

- Avoid reviewing any evidence in support of unsupported treatments. In the

interest of fairness, a therapist might admit that homeopathic medicines have an intuitive appeal and that many effective medicines were similarly derived from naturally occurring herbs and compounds, but this would be a mistake. The research on debunking suggests that any recounting of arguments in support of misinformation tends to solidify a mental model, making it more difficult to quash with new information.

- Don't just say the misinformation is wrong; provide an alternative formulation. The debunking of misinformation leaves a void that is an obstacle to a lasting effect. As time passes, the client is likely to refill the hole with the same old myth. As a result, it is important to supply the client with information about EBPs that is explained in some detail, along with the available evidence to back it up. As a result, when debunking homeopathy, the therapist should point out that the active ingredients are far too diluted to be effective, but it is also important to create a new theory of the client's problem through the lens of a sound empirical research. Be prepared to report what science has to say about the client's concern.

- Try to keep the explanation of EBPs simple and clear. Somewhat paradoxically, as important as it is to create a new evidence-based theory of the problem, debunking research suggests that an overly elaborate explanation can backfire. If the misinformation is simpler and clearer than the more valid alternative, the myth may survive. Unfortunately, it can be difficult to keep the description of an EBP simple. For example, when a therapist is confronted with a parent who is committed to the use of facilitated communication in the treatment of a child with autism, the elaborateness of an applied behavior analysis (ABA) protocol is going to come up short in relation to the far simpler explanation, "Jenny has a motor problem. She needs help steadying her hand on the keyboard."

- If there is a choice between giving information in printed or video form, choose video. A recent study showed that when fact-checking information was presented in either long-form written format or in a video, the video presentation was more effective in debunking misinformation (Young, Jamieson, Poulsen, & Goldring, 2017). Given the number of professional videos that are available both commer-

cially and on free websites (e.g., YouTube.com), it is likely that therapists can find useful material to present to clients.

Collaborating

If the rational approach does not quickly move the client in a constructive direction, a more empirical strategy can sometimes work. The therapist and client have an important common goal, helping the client. If sharing accurate information does not shake the client from unsupported or pseudoscientific beliefs, then offering to collaborate on an empirical test can be helpful. Rather than continuing to argue with the client—or sending the client away—the therapist can offer to join forces in an evaluation of the treatment options. In brief, the therapist might simply say, "OK, I can see you're not convinced. Let's perform a test with the understanding that whatever method works best will be the one we choose."

This strategy has been successfully employed by Shannon Kay (2015), a talented behavior analyst who has worked with many parents of children with autism.¹ Autism continues to be a "fad magnet" (Metz, Mulick, & Butter, 2015), attracting a seemingly endless stream of pseudoscientific treatments. As a result, Kay reported that, by the time she arrived on the scene of a newly diagnosed case, the child's parents were often already using prism glasses or sensory integration therapy. In those cases where she was unable to win parents over by sharing information and readings, she offered to conduct a single-participant study testing an applied ABA approach against the methods being used by the parents. And, of course, the subject of the research was the most important person of all, the child everyone was trying to help.

Kay described her experiences and provided data from three case studies in a chapter for the book *Controversial Therapies for Autism and Intellectual Disabilities* (Foxy & Mulick, 2015). In each of the three cases, she used an alternating treatments design and trained the parents in data collection. In all three instances, the unsupported therapy being used at the time was shown to have a negative effect on the child's behavior, rather than a positive one, and the parents and educational team

¹Shannon Kay is a former student of mine.

members quickly reversed their positions and endorsed a plan based on ABA.

When working with adults on issues other than autism treatment, it may be impractical to implement a test of competing therapies, and when a test is possible, a reversal design (e.g., ABAC) may be more appropriate than the alternating treatment design employed by Kay (2015). But introducing the client to some of the basics of research design and objective data collection can be very useful. Furthermore, it appears that one of the important features of Kay's approach is putting aside the struggle to assert one's authority as a therapist and offering to solve the dispute in a collaborative fashion. Understandably, some therapists may find it objectionable to agree to test a previously unsupported therapy. Furthermore, the empirical test approach is not without risks. Clients can rarely be blinded to the experimental conditions, and client expectations about pseudoscientific therapies can lead to measurable placebo effects. In the unlikely event of a positive outcome for a non-EBP, the therapist would be confronted with a thorny dilemma. It is best not to gamble if the outcome is in doubt. But in those cases where a collaboratively designed test seems both feasible and potentially effective, it may be more convincing than talk.

It is difficult to be optimistic about the prospect of a near future free of superstition and pseudoscience. In recent decades we have experienced an explosion in access to information; however, much of the easiest information to find is false. The Pew studies cited above suggest that many people are unable to judge the quality of information and, as a result, are unprepared to separate out the misinformation. Furthermore, as the Dunning-Kruger effect suggests, it is often the least informed people who are the most convinced they are right (Kruger & Dunning, 1999). As a result, further research on debunking strategies will be needed, and for the foreseeable future, therapists will continue to come across clients who espouse unscientific therapies.

A final thought. Fad therapies appear to reproduce at alarming rates and, in some cases, are all but impervious to rational attack. Despite the recent blows to the authority of experts of all kinds, behavior therapists are in an excellent position to speak publicly on these topics. It is unlikely that pseudoscience and superstition will ever be permanently vanquished, but behavior therapists who seek out public speaking opportunities, comment in the

media, or write for the general public can help to counter the misinformation in their communities. Although many professionals feel most comfortable speaking about the EBPs they have been trained to use, reducing the level of psychological snake oil in the marketplace will take additional efforts. According to research cited above, effective debunking will require therapists to inform people about the current scientific understanding of the disorders they treat and to call out the unsubstantiated treatments that are sometimes used. Taking these extra steps may eventually reduce the number of clients who come to you under the influence of pseudoscientific theories and will have the added benefit of publicly reinforcing the point that, in contrast to other approaches, behavior therapy is a rigorous evidence-based discipline.

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The Seductive Allure of Pseudoscience in Clinical Practice

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RECENTLY, A MAJOR PROFESSIONAL organization sponsored a webinar whereby the attendees would learn about the underlying mechanisms and procedures for Emotional Freedom Techniques (EFT; see Moran & Keynes, 2012, for overview). What was notable about this webinar offering was not so much the topic as the fact that the sponsoring organization indicates a commitment to promoting scientific foundations of assessment and treatment. One might even make a case for the scientific basis of EFT, given that there are claims in the literature of efficacious outcome with the method (Clond, 2016). However, most readers of this journal know what's coming next: namely, that EFT, as a member of the broader class of energy therapies, lacks (a) an underlying theoretical basis for different psychopathological states and (b) an empirical basis for the mechanisms of treatment efficacy. And yet, offerings like the aforementioned webinar proliferate, available through a wide range of organizations that are otherwise solidly science-minded.

Energy therapies are hardly the only example of treatment methods that lack any scientifically compelling underlying mechanisms of psychopathology or explanatory structures for the intervention methods. Indeed, there are far too many to enumerate here. Those who practice approaches that the scientific community have declared science-based smugly¹ denigrate these other approaches as nonscientific or, worse, pseudoscientific. Despite this divide, these approaches proliferate, and many practitioners offer treatments that lack qualities that we might call scientific.

Further, mental health practitioners are not the only professional group to fall prey to pseudoscientific theories. One famous example is the pursuit of achieving cold fusion in the lab, with the most recent unsubstantiated claim coming in 1989, despite the lack of a compelling theoretical basis for predicting the phenomenon could be produced (Beaudette, 2002). Philoso-

phers of science have struggled with the problem of pseudoscience, citing a demarcation problem suggesting a continuum of sorts from that which can be definitively termed science to that which is squarely pseudoscience (Popper, 1957).

While all sciences seem to be susceptible to pseudoscience, psychotherapy approaches may be at particular risk. The aim of this paper is to suggest some explanations for this problem, and some modest recommendations for remediation.

Therapy Allegiance: A Special Problem in Mental Health Delivery

Since you are reading this article, you are most likely an adherent to the theories that underlie cognitive and behavioral therapies. Asked to describe the approach to a friend or colleague in another profession, you might offer a detailed litany of justifications for the approaches based on your intimate knowledge of the theory and its accumulated empirical support. If asked on follow-up why this approach to treatment is so special and different from traditional psychotherapy, you might go so far as to explicate paradigmatic differences around the degree that each therapeutic approach values data (discussed by a psychodynamic theorist; Bornstein, 2005). But what happens should this same person ask what made you choose this therapeutic approach over all the others that are out there? You might very well offer an explanation that sounds like cold hard rationality—the data made you do it! The approach is evidence-based, and I'm an evidence-based person! But the research suggests that these explanations are as likely *ex post facto* explanations as they may be *a priori* decisions.

Research has suggested that the decision to align with CBT comes more from personal factors, whereas traditional psychotherapy approaches come more from training experiences (Buckman & Barker, 2010). That is, if you have a particularly compelling personal training experience,

¹I count myself among the smug.

you may be more likely to adopt a psychodynamic approach to treatment, whereas if you possess specific personality characteristics (low Openness to Experience, high Conscientiousness), you are more likely to choose CBT. Notice that neither justification is derived from such factors as “find data compelling” or “possess skeptical ideas about therapy research methods.” Digging a bit deeper into this single investigation, we find that a vast swath of practitioners who adhere to psychodynamic approaches are self-described as being particularly attuned to inner experiences and to find meaning in symbolic processes.

This suggests that the factors that lead to self-identification with one or another therapeutic approach is less about compelling data and more about a feeling state, an irrational basis unmoored from any scientific findings.² Long before survey data identified variables that explained the routes for how therapists sorted themselves into different theoretic camps, it was recognized that the therapy approach one practiced strongly influenced outcomes in otherwise controlled research (see Leykin & DeRubeis, 2009, for detailed discussion).

This means that should you have a good training experience, and are the kind of person who ascribes strong meaning to inner experiences, and receives training in a pseudoscientific method, you may be a new adherent to that approach. And once that happens, adherence to that method is difficult to shake.

Tribalism in Therapeutic Approaches

The factors that go into group affiliation are complex and wide ranging, certainly far beyond the scope of this article. However, in the self-sorting process that takes place following the determination of therapeutic orientation, it can be expected that we choose groups with whom to affiliate that we anticipate having similar values (Wagner, 1995). These values can be further crystallized as we further identify with the group. So what happens when the broad outlines of the values of the group are threatened? In the case of our discussion, what happens when the purveyor of

pseudoscientific methods is called out for proffering a nonscientific approach?

Douglas (1966) described a robust social process, evident in religion, group dynamics, and close-knit tribal communities, whereby external threats are identified and specific remedies are developed and sanctioned by the group. Practitioners of all stripes are members of “tribes,” and will seek out assistance from the tribe when threatened. Accordingly, the purveyor of pseudoscientific methods will find support from their “tribe” of like-minded providers when attacked for their practices. The scientific community is not a part of this equation since that is not the tribe that will be available to them. And without external structures that might restrict their practice, pseudoscientific approaches will likely continue and even thrive. The methods of assistance vary widely based on group-specific customs that develop to create a sense of group purity and cohesion.

Market Forces Support Different Tribes

Travel to areas of the desert southwest in the United States and one finds a wide range of New Age practices. For example, Sedona, AZ has numerous practitioners of physical and mental healing that relies on the local “crystal vortex” (Dannelly, 1995). This specific region is said to possess special qualities, and the crystals in the red rock formations distinctive to the town converge with mystic energies that promote a healing process. Aside from the stunning beauty of the place, there is little to support the idea that the local vortex possesses special healing properties. Nonetheless, people suffering from all types of maladies seek “treatment” from what are effectively faith healers.

These approaches persist for a variety of reasons, one of which involves strong market forces that support their demand. The various pseudoscientific practices roughly correspond to so-called New Age practices. Research suggests that segments of the population find these practices compelling and includes endorsement of magical ideation (Farias, Claridge, & Lalljee, 2005). As further evidence that there would

be a sizeable market for New Age approaches, look no further than the considerable sales of the book *The Secret* (Byrne, 2006), a bestseller with a central thesis that the way to a better life is that simply thinking positive thoughts will in and of itself change oneself and the world. Imagine for a moment now that, as a CBT practitioner, you include in your treatment plan an effort to directly challenge thought-action fusion (Shafran & Rachman, 2004), the specific cognitive distortion that thinking something bad increases its likelihood, and you learn your client subscribes to the model described in *The Secret*. At the very least the discussion that will follow will be awkward.

We can then conclude that practitioners who offer pseudoscientific approaches may do so as a consequence of true identification with a group that endorses these methods (tribalism), and that it is perpetuated through a market that supports it. Attacks on these approaches are met with credulity, counter-attacks, and retrenchment. How often have you heard some variation on the following counter-argument: “I’m not going to worry about which theory or mechanism is at work, I just do what I know is effective.” This ultimate tribal retreat allows for retention of the approach without concern for science, and retains the claim that what they do works. You might even be on the receiving end of a counter-accusation that because of a slavish reliance on science, you are lacking in compassion (discussed in McKay, 2017).

Making Pseudoscience Unappealing to the Masses

This has not been an exhaustive consideration of all the ways pseudoscience is appealing to clinicians and consumers. But some of the factors that make it appealing, and difficult to dislodge, come more from personal preferences and sociological forces than from cold hard facts. Among the challenges are: demarcation; illusory effectiveness in psychotherapy; and public education in science.

The Demarcation Problem

It was noted earlier that the demarcation problem in science has been a persistent challenge in rooting out pseudoscience from science. Indeed, some philosophers of science declared it hopeless to pursue any longer (Lauden, 1983). Since that time, a healthy reemergence of interest in establishing a specific boundary between what constitutes science and what belongs in the

²Self-disclosure moment: During my undergraduate years I self-identified as psychodynamic in orientation. It was only in graduate school that I found I had a talent for exposure, discovered accidentally over a dinner outing, and later crystallized during a training experience. So it seems that reinforcement and determinism shaped my professional trajectory rather than some clear-eyed and deliberate planning.

category of pseudoscience has sprouted (Pigliucci & Boudry, 2013). In assessing the importance of this approach, it has been suggested that pseudoscience is actually essential for understanding science itself since it permits a clarification of definition for what counts as evidence (Ladyman, 2013).

In some ways, psychotherapy research is ahead on this matter. We have begun to reckon with this problem by directly and unambiguously identifying practices that are pseudoscientific (such as the aforementioned energy therapies) by specifying the characteristics of questionable practices (see Lilienfeld, Lynn, & Lohr, 2014). Of course, this optimism is tempered by the mere fact that pseudoscientific approaches are not only still practiced, but that training in these approaches continues to proliferate.

Illusory Effectiveness and the Public

Pseudoscientific therapy approaches can retreat into pure empiricism to support the claims of efficacy. A long-standing and well-known problem in psychotherapy is that virtually any treatment performs better than waitlist (Eysenck, 1952). Early compilations of the outcomes of treatment suggested that all interventions had comparable efficacy (Smith & Glass, 1977). This led to a defense of common factors and a broad therapeutic relationship as central mechanisms in efficacy since it appeared that all treatment were on comparable footing, an argument that continues to attract supporters (Shedler, 2010).

The practice community engages in a wide range of errors in reasoning that can lead to the development and adoption of practices that lack scientific merit. Lilienfeld, Ritschel, Lynn, Cautin, and Lutzman (2014) described a taxonomy of these problems, termed causes of spurious therapeutic effectiveness. This taxonomy has three broad categories: perception of client change in its actual absence; misinterpretations of actual client change derived from extratherapeutic factors; and misinterpretation of client change resulting from non-specific factors.

However, the public has begun to identify problematic therapeutic approaches for themselves. It has been suggested for several years now that more and more clinicians recognize that clients request evidence-based treatment, and cognitive-behavior therapy in particular (McKay, 2014). The stated adoption of CBT means that increased attention must be paid to

fidelity of treatment delivery. This is important to the dissemination effort. Namely, the public has to trust that treatments delivered in everyday practice will mimic the scientific findings of efficacy for CBT from carefully controlled investigations, or come as close as reasonably possible. Otherwise, how can we disseminate that this is evidence-based if clients cannot readily access genuine CBT?

Public Education in Science

An old commercial for Syms clothing store intoned, "An educated consumer is our best customer." In a similar vein, educated consumers will be the best customers for CBT as well as for the future of scientifically informed psychotherapy. However, unlike in clothing, this will mean that consumers will need to be better educated about the science of treatment, and what counts as evidence.

This requires that the public have some layman's understanding of causation in treatment. On this we might be a bit less optimistic. First, the problem of different levels of analysis germane to psychopathology remains elusive to practitioners of the various mental health disciplines. For example, Kendler (2012) made a persuasive case that there are numerous levels of analysis appropriate for consideration in treatment, ranging from genetics up through and including culture. However, from the policy side, recent research funding priorities such as the Research Domain Criteria favor biological mechanism explanations over other levels of analysis (Insel et al., 2010). By favoring single levels of analysis over multifaceted contributions to psychopathology, the public is less likely to appreciate putative causes and correlates since the assumption across all mental health problems is that biological factors are causative, even if the evidence is not demonstrated.

Second, the public is not in an advantaged position to recognize the difference between a hierarchical view of causes of psychopathology and resultant treatment compared to a more situational-constrained perspective. This is largely because some concepts persistently escape understanding by the general public. For example, in a series of experiments it was shown that participants were equally likely to draw causal inferences from experimental data as from nonexperimental data. Further, causal inferences were more frequent when it conformed to intuitively held notions (Bleske-Rechek, Morrison, & Heidtke, 2015). This difficult situation means

that public science education is more essential than ever if consumers are going to be able to parse fraud from fact in the pursuit of good treatment.

Understanding is the first step in developing an action plan. At this point there is still an inadequate understanding of what compels well-intentioned clinicians to adopt practices that have dubious efficacy, questionable scientific foundations, and simply lack clear and compelling mechanisms for actions. There are some promising options for consideration here that include individual preferences, group processes, and market forces. Hopefully, by clarifying the role each of these play, policymakers will design methods to combat pseudoscientific practices as a means to protect an unsuspecting public.

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Why Evidence-Based Practice Isn't Enough: A Call for Science-Based Practice

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AS CLINICAL PSYCHOLOGISTS and other mental health professionals, our priority should be crystal clear: to ensure that individuals suffering from mental illness receive the highest quality psychological care. Nevertheless, survey data on therapists' treatment selection make abundantly evident that we are falling woefully short in this regard (Lilienfeld, Ritschel, Lynn, Cautin, & Lutzman, 2013). Our discipline has long been marked by a science-practice gap, a wide schism between the best available research evidence bearing on the efficacy and validity of psychological techniques, on the one hand, and their routine use in clinical practice, on the other (Lilienfeld, Lynn, Ritschel, Cautin, & Lutzman, 2013; Tavris, 2014).

To take merely a handful of salient examples, a survey of 51 licensed therapists in Wyoming (Hipol & Deacon, 2013) revealed that fewer than one third adminis-

tered exposure and response prevention (ERP) for obsessive-compulsive disorder (OCD), even though ERP is the clear-cut scientific intervention of choice for OCD. Many of these therapists availed themselves of treatments boasting minimal scientific support for OCD, such as psychodynamic therapy, art therapy, and Thought Field Therapy, the latter being one of several energy therapies (more on that soon). In a survey of 130 Canadian therapists who treat patients with eating disorders (von Ransom, Wallace, & Stevenson, 2013), only 23% reported using cognitive-behavioral techniques, even though these methods are among the few empirically supported therapies (ESTs) for eating disorders. Moreover, even among therapists who claimed to administer cognitive-behavioral methods for eating disorders, sizeable pluralities or minorities did not make regular use of standard cognitive-behavioral techniques,

such as cognitive restructuring or stimulus control methods (von Ransom et al., 2013). Other survey data indicate that up to half of people who meet diagnostic criteria for major depression receive no formal psychological treatment, and fewer than 10% of those who do receive interventions consistent with scientific evidence (Layard & Clark, 2014).

Over the past decade or so, the standard remedy for bridging the science-practice gap has been evidence-based practice (EBP), which is an overarching approach to clinical decision-making (Straus, Glasziou, Richardson, & Haynes, 2010). EBP integrates three legs within a "three-legged stool": (a) the best available data on psychotherapy outcome (and to a lesser extent, process), (b) client preferences and values, and (c) clinical expertise (Anderson, 2006; Spring, 2007). EBP emanated from the evidence-based medicine movement, which was launched in McMaster University in Canada in the late 1980s and early 1990s (Guyatt et al., 1992). Later, this movement emigrated to the U.K. (Sackett, Rosenberg, Gray, Haynes, & Richardson, 1996), American medicine, and, belatedly, American psychology. Although the American Psychological Association (APA, 2006) has declined to adopt a stance on which, if any, of the three legs of the EBP stool should be accorded highest priority in treatment selection, the Canadian Psycho-

logical Association (2012) has advocated that the first leg—research evidence—should take precedence above the others, a position that we strongly endorse. Some authors have extended this three-legged stool from psychological interventions to evidence-based assessment (Bowden, 2017; Hunsley & Mash, 2007), an issue to which we briefly return (see “Concluding Thoughts”).

Ideally, the research leg of EBP should enhance the quality of mental health care by aligning clinical practice more closely with scientific evidence (Kazdin, 2008; Lilienfeld et al., 2013). As a consequence, if EBP is functioning as intended, it should help to stem the tide of pseudoscientific and otherwise questionable intervention and assessment techniques (see Lilienfeld, Lynn, & Lohr, 2014, and Thyer & Pignotti, 2016, for reviews).

In this commentary, we contend that although EBP has been a laudable and necessary first step toward ensuring high-quality mental health care, it is not sufficient. More provocatively, we maintain that in some noteworthy respects, EBP has failed and will continue to do so. Hence, the mental health disciplines need to adopt an approach that is at once considerably broader and more rigorous than EBP, namely, *science-based practice* (SBP). As we will demonstrate, SBP incorporates all the fundamental elements of EBP but goes well beyond it in one significant respect—which we soon discuss.

Our call is not entirely novel, as similar arguments have been advanced in medicine. For example, Gorski and Novella (2014) advocated for *science-based medicine* (SBM) as a more stringent and all-encompassing alternative to evidence-based medicine. We gratefully acknowledge the influence of their thinking on our analysis and adapt their terminology to mental health practice (see also Hall, 2011; Sampson & Atwood, 2005). Furthermore, as we note in a later section (“The Remedy: Science-Based Practice”), a few authors in the psychotherapy literature have anticipated our core arguments (e.g., David & Montgomery, 2011; see also Lilienfeld, 2011).

Nevertheless, to our knowledge, we are the first to call explicitly for a wholesale transition from EBP to SBP in clinical psychology and allied mental health domains, such as psychiatry, counseling, social work, and psychiatric nursing. Moreover, because several of the substantive issues and details of this approach’s pragmatic implementation differ in medicine as

opposed to psychology, an independent analysis of SBP as opposed to SBM is warranted.

The Recent Impetus for Science-Based Practice

The awareness that EBP has its noteworthy shortcomings is similarly not new. For example, some authors have observed that the APA task force on EBP was conspicuously vague when it came to operationalizing the meaning of “evidence” (Stuart & Lilienfeld, 2007). Nevertheless, the limitations of EBP have become increasingly evident in the last few years. Indeed, this article was precipitated largely by a series of relatively recent events that have raised troubling questions regarding the capacity of EBP to curtail the continued spread of pseudoscience in mental health practice. We highlight three developments in particular.

- The APA and several other national psychological associations continue to accredit sponsors for continuing education (CE) courses and workshops on intervention techniques that are premised on dubious or blatantly implausible theoretical rationales, such as energy therapies (e.g., Thought Field Therapy and Emotional Freedom Techniques; e.g., see <http://www.eftuniverse.com/certification/accreditation-information>). Energy therapies are based on the highly suspect and probably unfalsifiable suppositions that (a) humans are surrounded by invisible and unmeasurable energy fields, and (b) blockages or other disturbances in these fields produce anxiety disorders and other psychiatric conditions. Similar problems extend to social work, where licensed practitioners can obtain CE credits for a host of energy therapies, primal therapy (colloquially termed primal scream therapy), and internal family systems therapy (Thyer & Pignotti, 2016), the latter of which posits that the human mind comprises largely distinct subpersonalities, each with its distinctive way of viewing oneself and the world.

- In 2014, the Substance Abuse and Mental Health Services Administration (SAMHSA), an agency within the U.S. government, added Thought Field Therapy to its list of National Registry of Evidence-based Programs and Practices. This registry is intended to educate the public regarding efficacious interventions for substance use disorders and other psy-

chological conditions (Satel & Lilienfeld, 2016). The rationale for the inclusion of Thought Field Therapy is that this technique has been demonstrated in multiple controlled studies to be efficacious for enhancing resilience and self-concept, and for diminishing trauma- and anxiety-related symptoms, depressive symptoms, and so on, when compared to wait-list control conditions (<http://nrepp.samhsa.gov/ProgramProfile.aspx?id=60>).

- A growing number of practitioners of highly dubious interventions are now eagerly claiming the “evidence-based” mantle and advertising themselves using this moniker (Mercer & Pignotti, 2007). For example, websites for, and articles on, the following interventions describe these treatments explicitly as “evidence-based”: therapeutic drumming (<https://wakeup-world.com/2015/04/07/6-evidence-based-ways-drumming-heals-body-mind-and-soul/>); animal-assisted therapy for eating disorders (<https://www.remudaranch.com/index.php>); Thought Field Therapy (<https://www.thoughtfieldtherapy.net/tft-recognized-by-nrepp/>); Emotional Freedom Techniques (Church, 2013); Imago Relationship Therapy (<https://www.newharbinger.com/evidence-based-therapies/>); Jungian sandplay therapy (<http://sandplayassociation.com/faqs/>); primal therapy (<http://primaltherapy.com.au/frequently-asked-questions/>); dance movement therapy (<https://www.hochschule-heidelberg.de/en/academics/masters-degree/dance-movement-therapy/>); abreactive hypnosis for PTSD (Barabasz, 2013); acupuncture for clinical depression (<https://www.alternativementalhealth.com/evidence-based-uses-of-chinese-medical-therapies-in-the-treatment-of-depressed-mood/>); and neurolinguistic programming (NLP; Zaharia, Reiner, & Schütz, 2015). Furthermore, recent unpublished survey data suggest that large majorities of practitioners who administer non-evidence-based interventions for anxiety disorders nevertheless describe themselves as offering “evidence-based” services (Deacon, personal communication).

Superficially, it might seem straightforward to address all three of the aforementioned trends by means of logic alone. After all, one might presume, energy therapies—to take merely one example—cannot possibly be evidence-based given that their theoretical foundation is exceedingly

implausible scientifically. Almost surely, it is not true that humans are surrounded by invisible and unmeasurable energy fields, let alone that blockages or disruptions in these fields are the central causes of psychological distress. Hence, this reasoning continues, energy therapies cannot possibly satisfy the research leg of EBP.

Nevertheless, given how this leg of EBP is presently operationalized in American clinical psychology, proponents of energy therapies are equipped with an effective rebuttal: *If one relies exclusively on controlled outcome data on energy therapies, one can make a reasonable case that these interventions are in fact supported by research evidence.* Why? Because controlled studies reveal that energy therapies typically outperform wait-list control conditions (Feinstein, 2008, 2012). Indeed, when the first author, among others, has asked members of the APA Education Directorate why sponsors who offered courses on energy therapies were approved for CE credit, they referred in part to the published research support for these interventions.

Of course, energy therapy critics could respond with considerable justification that this apparent efficacy almost certainly derives from nonspecific influences, such as placebo effects, regression effects, spontaneous remission, and perhaps most important, the incidental repeated exposure that accompanies the intervention (Bakker, 2013; Pignotti & Thyer, 2009). Nevertheless, the APA Division 12 (Society of Clinical Psychology) criteria for ESTs, which constitutes by far the most influential instantiation of the research prong of EBP, require only that a treatment must outperform a no-treatment control condition in two or more randomized controlled trials or systematic within-subject designs (Chambless & Hollon, 1998; <http://www.div12.org/psychological-treatments/frequently-asked-questions/>). Energy therapies may very well meet this lax criterion. Hence, the APA Education Directorate, which approves CE sponsors, may have its hands tied when it comes to approving such interventions. The same problem arises for a number of the other interventions listed three paragraphs earlier. Using the current EST criteria, a host of other pseudoscientific and otherwise questionable interventions, such as animal-assisted therapies of many stripes (e.g., dolphin-assisted therapy and equine-assisted therapies; see Anestis, Anestis, Zawilinski, Hopkins, & Lilienfeld, 2014; Marino & Lilienfeld, 2007), dance therapies for severe

psychopathology, and NLP may also clear the evidence-based research bar.

When it comes to proponents of these treatments claiming evidence-based status, some readers might reasonably contend that it is unfair to lay the blame on EBP. All concepts can be misused, as the principle of *abusus non tollit usum* (the abuse of a claim does not invalidate its proper use) reminds us. Nevertheless, in many cases these proponents can legitimately lay claim to fulfilling the research leg of the EBP stool given the current EST criteria, which focus exclusively on outcome data. Hence, EBP leaves the door wide open for precisely such misuse.

The Remedy: Science-Based Practice

Fortunately, there is at least a partial solution to the aforementioned problems: science-based practice (SBP). In SBP, as in science-based medicine (SBM; Gorski & Novella, 2014), treatment outcome data are not the only source of data bearing on the research evidence for interventions. Instead, in SBP, treatment outcome data are considered along with *broader research evidence bearing on the plausibility of the treatment's theoretical rationale* when evaluating an intervention's scientific status. That is, in SBP, all forms of research evidence are relevant when evaluating the scientific status of an intervention. If the treatment is based on a grossly implausible theoretical rationale, one that runs counter to what research has consistently demonstrated about how the natural world works, it should not be regarded as fully evidence-based, even if supported by promising outcome data.

By the workings of the “natural world,” we include the laws of physics in addition to well-established principles regarding the functioning of the human mind. As noted earlier, energy therapies conflict sharply with research evidence derived from physics. Or, to take an example from the more psychological realm, primal therapy rests on the supposition that mental anguish in adulthood results from the repression of unbearable psychological pain emanating from traumatic experiences in infancy or early childhood, in some cases the trauma of birth. Such pain can purportedly be released and expunged by repeated screaming. There is no compelling or even suggestive evidence for any of these assertions (Singer & Lalich, 1996).

As noted earlier, some authors have anticipated our arguments. In medicine, Gorski and Novella (2014) and Sampson

and Atwood (2005), among others, advocated for a Bayesian approach, in which treatment outcome data are integrated with the a priori likelihood of the treatment's efficacy (“Bayesian prior probability”; see also Lilienfeld, 2011) in ascertaining an intervention's scientific status. Further, in a useful analysis, David and Montgomery (2011) proposed that the EST criteria be expanded to incorporate evidence for a given psychotherapy's theoretical rationale. Specifically, they suggested that parallel criteria be employed to evaluate the plausibility of a treatment's theoretical rationale as that currently employed to evaluate its empirical status, namely, two well-conducted supportive studies. Yet because theories are underdetermined by scientific evidence (Laudan, 1990), two supportive studies are almost always insufficient to provide compelling evidence for a treatment's theoretical rationale. Another limitation of David and Montgomery's framework is its invocation of a categorical cutoff for theoretical support (two studies), which does not necessitate consideration of the full body of high-quality scientific evidence bearing on the evidence for and against a treatment's rationale (Lilienfeld, 2011).

To be sure, the second limitation applies to the Division 12 criteria for ESTs as well. In this respect, we side with Tolin, McKay, Forman, Klonsky, and Thombs (2015), who maintained that the current EST criteria should be superseded by a much more comprehensive approach to psychotherapy and assessment methods evaluation that includes all relevant data on treatment outcomes, along with a careful analysis of the methodological rigor of the relevant studies (see also Miller & Wilbourne's 2002 “mesa grande” approach to evaluating the strength of evidence for alcohol use disorder treatments; and the theoretically motivated approach to cognitive ability assessment of Riley, Combs, Davis & Smith, 2017). In SBP, the same principle should hold for the evaluation of research evidence for the treatment rationale, namely, a comprehensive analysis of all relevant high-quality data.

In an important but largely neglected article, entitled “Psychotherapy Is the Practice of Psychology,” Sechrest and Smith (1994) argued that the practice of psychotherapy, as well as psychotherapy research, must be informed by broader knowledge of psychology, including research in neuroscience, affect, cognition, learning, social psychology, personality, culture, development, and other subfields.

Their article is worth quoting from at length:

A psychologically integrated psychotherapy will not be merely eclectic, for it will be guided by both the scientific theory and evidence available at any one time. . . . In our view . . . psychology is making great strides in knowledge about many aspects of behavior, e.g., in the workings of the brain, in the genetic bases for behavior, in cognitive functions, in the course of human development over the life span, and so on. These gains in knowledge provide a large, sound data base rich with implications for psychotherapy. It will be a shame if psychotherapy continues as a fragmented enterprise on the borders of psychology, limited both conceptually and scientifically by self-imposed insulation from what by its origins is its birthright. (p. 27)

Similar considerations apply to SBP. To properly appraise psychotherapies, we need to consider not merely how well they work when compared against wait-list control conditions, but also whether they are grounded in adequate scientific foundations, including basic psychological science.

SBP should help to solve several pressing problems. First, SBP should begin to curb the continued infiltration of pseudoscience into clinical practice, as many and arguably most poorly supported interventions rest on highly questionable theoretical premises. Second, SBP offers a cogent counterargument to assertions that scientifically dubious interventions that outperform wait-list control conditions should qualify for CE credits or clinical practice guidelines. Third, SBP renders it difficult for advocates of energy therapies and other highly dubious interventions to dub themselves “evidence-based,” which they can often do now with some justification given current EBP standards.

Potential Objections

We can envision several potential objections to SBP; we briefly address four here. First, critics of SBP might contend that “if a treatment works, it works.” So, if we wish to be blindly empirical, we should regard energy therapies as roughly equivalent to well-established ESTs in evidentiary strength, as the controlled outcome data for the former interventions are also supportive. Setting aside the question of

whether the outcome evidence for energy therapies is as persuasive as its advocates contend, which is doubtful (see Pignotti & Thyer, 2009), this argument neglects the crucial point that interventions with blatantly implausible theoretical rationales are unlikely to be both “efficacious and specific” (Chambless & Hollon, 1998). That is, they are unlikely to display efficacy above and beyond nonspecific ingredients, such as placebo effects, effort justification, or the generalized effects of attention and interpersonal support (Lilienfeld et al., 2014). As a consequence, they are far less likely to be deserving of further research investigation compared with other interventions, not to mention more efficacious than standard interventions.

A second objection is that scientists are sometimes mistaken about how the natural world works, so it is illegitimate to consider research evidence bearing on a treatment’s theoretical rationale when evaluating its scientific status. Scientific knowledge changes, in some cases radically. As one familiar example, German geophysicist Alfred Wegener was dismissed by some scientists as a crackpot after introducing his theory of continental drift in 1912, as the idea that the continents move struck them as preposterous. As we know, however, Wegener was later vindicated by studies in plate tectonics, paleontology, and other disciplines (McComas, 1995). But for every Wegener, there are at least a thousand inventors of would-be perpetual motion devices and mind-reading machines (Sagan, 1995). More important, SBP, like EBP (see Gibbs & Gambrill, 2002; Lilienfeld et al., 2013), is not ossified, as it evolves in accord with new evidence. If physicists were to uncover compelling evidence for the existence of energy fields surrounding the human body, or if psychologists were to uncover compelling evidence for the existence of internal subpersonalities, then energy therapies and internal family systems therapy, respectively, might warrant consideration as meeting SBP criteria.

A third objection is that we have not offered explicit criteria for SBP status akin to those for ESTs. To this objection, we plead guilty, as we do not intend to propose a specific operationalization of SBP here, although we hope to do so in a future communication. At this juncture, we will say only that to meet full SBP status, the two prongs of (a) controlled research outcome evidence and (b) evidence for the scientific rationale are both necessary, though neither in isolation is sufficient. The full

details of a proposed SBP operationalization for mental health care, however, have yet to be fleshed out.

A fourth objection is that the theoretical rationale for many well-established or promising psychological treatments, including exposure treatments, remain in dispute or are incompletely understood (Lilienfeld, 2011). Nevertheless, our goal in this brief communication is modest: namely, to present SBP as an overarching framework that can serve as a partial safeguard against interventions whose theoretical rationales are markedly at variance with well-replicated scientific evidence. We are far less concerned about interventions whose rationales are inadequately understood than those whose rationales are exceedingly implausible from a scientific standpoint. In this respect, SBP should be able to function as a partial bulwark against the ongoing intrusion of pseudoscience into clinical work, evidence-based practice guidelines, graduate education and training, and continuing education courses.

Concluding Thoughts

EBP has been an essential step toward grounding the field of clinical psychology more firmly in science. Nevertheless, it has not gone far enough, as it has failed to operate as an effective safeguard against the penetration of pseudoscience into myriad domains, including continuing education courses, clinical practice guidelines, and the marketing and promotion of interventions. SBP, although not a panacea, should nudge the field in the direction of a stronger scientific foundation. By incorporating evidence from *all* relevant science, including the natural sciences (e.g., physics, chemistry), rather than merely treatment outcome evidence, SBP should help to prevent advocates of treatments based on grossly implausible theoretical rationales from laying claim to the coveted evidence-based mantle.

Although we have focused our analysis on psychological treatment, many or most of the same considerations we have raised (e.g., Sechrest & Smith, 1994) apply in equal force to psychological assessment (see Bowden, 2017). For example, in neuropsychological assessment, good scientific theory plays a critical role in test score interpretation. Neuropsychological assessments rooted in stronger theory are not only likely lead to more interpretable assessments, but are also likely to reduce decision errors, because the assessment is motivated by a theory that will have under-

gone more rigorous evaluation and replication (Riley et al., 2017). One theory that accounts for a vast array of neuropsychological data is the Cattell-Horn-Carroll (CHC) model, an integration of the empirical work of the three eponymous authors over many decades (McGrew, 2009) that has been validated across diverse populations and clinical conditions (Jewsbury, Bowden, & Duff, 2016; Jewsbury, Bowden, & Strauss, 2016). This model articulates several different cognitive ability constructs that have historically been grouped under the broad rubric of “executive function.” The latter atheoretical grouping is illustrated by the cognitive ability taxonomy of DSM-5 (American Psychiatric Association, 2013). Nevertheless, if multiple constructs are assessed and interpreted together exclusively as a global construct, the risks of confounded assessments and clinical decision errors are exacerbated (Jewsbury & Bowden, 2017).

In closing, we encourage more explicit integration of SBP into CE courses as well as into graduate training and education, including clinical supervision and formal coursework. Current and would-be mental health professionals need to conceptualize and evaluate clinical practice, including psychotherapy and assessment, within the broader context of basic and applied sciences. This more encompassing perspective on evidence should help to immunize them against the seductive charms of pseudoscience.

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Pseudotherapies in Clinical Psychology: What Legal Recourse Do We Have?

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SINCE THE EARLY 90s, the field of psychotherapy has witnessed a proliferation of pseudotherapies—seemingly scientific treatments that are not actually based on scientific principles (Lilienfeld, Lynn, & Lohr, 2015b). As a result, consumers and mental health professionals are increasingly vulnerable to pseudoscientific promotions. Pseudotherapies have infiltrated popular psychological discourse and threaten to erode the scientific foundations of clinical psychology. They also pose potential harms to the public. The majority of pseudotherapies are ineffective rather than iatrogenic (Lilienfeld, 2007). However, even when not overtly harmful, they may inflict indirect harm by depriving individuals of time and money that could

have been allocated to other efficacious and effective treatments (Lilienfeld et al., 2015b).

The current measures in place to protect the field of clinical psychology and the public from pseudotherapies are inadequate. Rather than issuing practice guidelines and sanctioning the practitioners of these treatments, the American Psychological Association (APA) and various governmental organizations have arguably contributed to their proliferation. For instance, the APA offers continuing education credits for Jungian sandtray therapy and psychological theater (Lilienfeld, Lynn, & Lohr, 2015a). Recently, the Substance Abuse and Mental Health Services Administration (SAMHSA) added Thought Field

Therapy to its National Registry of Evidence-Based Programs and Practices as an effective treatment for trauma and other conditions (Lilienfeld & Satel, 2016).

The fact that we cannot rely on professional associations and national agencies to protect us from ineffective and harmful pseudotherapies underscores the importance of legal recourse for the consumer and the concerned professional.

But what legal recourse do psychologists and consumers have? The answer varies by jurisdiction.

This article examines the legal strategies that have been used to curtail the practice of pseudoscientific therapies. In particular, it examines two legal approaches that have been used to curtail the practice of sexual orientation change efforts (SOCE), a category of pseudoscientific therapies designed to change a person's sexual orientation from lesbian, gay, or bisexual (LGB) to heterosexual.

The first strategy is the enactment of targeted legislation by states to prohibit the practice of SOCE. The second is the use of

the state's consumer fraud laws to sue SOCE practitioners. These approaches are compared and evaluated as potential models for legal approaches that can be used to curtail the practice of SOCE and other pseudotherapies.

Legislative Bans Targeting SOCE and the California Model

In 2012, California enacted legislation making it illegal for a mental health provider to practice SOCE with a minor under the age of 18. SB 1172 was landmark legislation, making California the first state in the nation to restrict the practice of SOCE. Under SB 1172, the practice of SOCE is considered unprofessional conduct and provides grounds for the therapist to lose his or her license.

The basis for California's legislation was the state's "compelling interest in protecting the physical and psychological well-being of minors . . . and in protecting its minors against exposure to serious harms caused by sexual orientation change efforts" (S.B. 1172, § 1 (n), 2011-2012, S. Reg. Sess. (Cal. 2012)).

First Amendment Challenges

Immediately after it was enacted, the legislation was challenged by mental health practitioners on First Amendment grounds as an infringement of protected speech (*Pickup v. Brown*, 2014).

The primary issue raised by this First Amendment challenge was whether the California legislation regulated a therapist's professional conduct or whether it inhibited constitutionally protected speech.

The regulation of professional speech is not a well-defined area of First Amendment law. The court acknowledged that mental health professionals have a First Amendment right to express their opinions in public. However, this protection diminishes for speech uttered in the context of the therapist-client relationship. Further, it "ultimately ceases when it is uttered in a context exclusively regulated by the accepted standards of professional conduct" (Victor, 2014, p. 1555).

The court ultimately decided the legislation was a regulation of conduct that only incidentally regulates speech. The rationale for this decision was twofold. First, the court noted that California has the authority to prohibit licensed mental health professionals from providing therapies that the legislature has deemed harmful. Second, the fact that speech is used to carry out those therapies does not transform the reg-

ulation into one of speech (Victor, 2014). As such, it is outside the scope of First Amendment protection.

Infringement on Parental Rights

The California legislation was also challenged as an infringement of parents' rights to control their children's upbringing and make important medical decisions for them. In other words, parents should have the right to choose SOCE for their children (*Pickup v. Brown*, 2014).

The court acknowledged that although parents have a constitutionally protected right to make decisions regarding the care, custody, and control of their children, this right is not without limitations. If the child's mental health is jeopardized, the state has the right to intervene to protect this child.

For this challenge, the central issue before the court was whether parents' fundamental rights include the right to choose a therapy for their children that the state has deemed harmful. The court stated that parents could not compel the state to permit licensed mental health professionals to engage in unsafe practices and cannot dictate standard of care in California based on their own views. It concluded that the fundamental rights of parents do not include the right to choose a specific medical or mental health treatment that the state has reasonably deemed harmful (*Pickup v. Brown*, 2014).

California Legislation as a Model for Other States

Although SB 1172 passed constitutional muster, similarly crafted legislation would be vulnerable to constitutional challenges.

One of the reasons for this vulnerability is that the state may not be able to meet its burden in showing that SOCE or another pseudotherapy causes harm that the state has a compelling interest to protect against. California increased the likelihood of meeting this burden by recognizing two categories of harm: (a) the cause or exacerbation of psychiatric disorders such as anxiety, depression, and suicidal behavior; and (b) the internalization of stigma and impeded development of a positive LGB identity.

To establish the first type of harm, the Legislature relied heavily on a report by a Task Force of the APA that surveyed all existing literature on SOCE. The report found compelling evidence that physically invasive forms of SOCE, such as aversion therapy or conversion therapy, cause harmful mental health effects such as

increased anxiety, depression, suicidality, and loss of sexual functioning (APA, 2009).

However, the risks of SOCE methods that exclusively involve talk therapy and exclude physical techniques are less clearly documented. For these forms of SOCE, the APA report concluded there is only anecdotal evidence of harmful outcomes and it could not definitively state how likely it is that harm would occur from them (APA, 2009). For this reason, the California legislature included the second type of harm. Reasoning that because SOCE is premised on the notion that LGB status is pathological, the legislature concluded that SOCE impedes the development of a healthy self-concept and self-acceptance, and contributes to the internalization of stigma.

Other States Adopt the California Model to Target SOCE

Several states have adopted the California model to target SOCE. However, the need to produce clinical evidence of SOCE's harmfulness has limited the scope of these legislative bans to conversion therapy because there is less evidence for the harms caused by noninvasive forms. Additionally, these bans tend to be limited to the practice of conversion therapy with minors who are generally viewed as in greater need of the state's protection than adults.

To date, eight states and the District of Columbia have enacted legislation that narrowly prohibits the practice of conversion therapy with minors, rather than SOCE more broadly and with all ages. They include Illinois, Nevada, New Mexico, New Jersey, Oregon, Rhode Island, and Vermont. New York is considering similar legislation.

Connecticut has enacted the broadest legislation against conversion therapy, banning the practice with adults, as well as children. It has also banned the expenditure of public funds on this pseudotherapy.

As in California, legislation in New Jersey was challenged on First Amendment grounds by practitioners. The court ruled that the state's interest in protecting the public from harm outweighed the therapists' free speech interests (*King v. Governor of New Jersey*, 2014).

Limitations of California Model

Although California's legislation seems like a promising model for other states to curtail the practice of SOCE and other pseudotherapies, it has significant limitations.

First, adults tend not to be protected from the harms of pseudotherapies by leg-

islative bans. Even in states with legislative bans, SOCE may be offered by any mental health practitioner to an adult with the exception of Connecticut. Second, this regulatory scheme does not prevent unlicensed providers, such as religious leaders, from administering SOCE or other pseudotherapies. Nor does it prevent licensed mental health practitioners from referring children and adults to unlicensed practitioners of SOCE and other pseudotherapies.

From a legal perspective, California's legislation and similarly modeled legislation are particularly vulnerable to First Amendment challenges because it prohibits a type of speech partly on ideological grounds.

To withstand constitutional challenge, legislation enacted by the states must show the state has a compelling interest to protect the public from harm. Accordingly, in the absence of clear evidence of harm, the legislative approach to curtail the practice of therapies is not effective. Because the vast majority of pseudotherapies are merely ineffective rather than harmful, this approach is not optimal.

Consumer Fraud Acts and the New Jersey Model

Given the limitations inherent in a legislative approach, a more promising strategy for the restriction of SOCE and other pseudotherapies may be the use of the states' Consumer Fraud Acts.

Pseudotherapies lack scientific evidence of efficacy and effectiveness. Yet, most practitioners of these treatments make misleading claims to the public about their success for treating problems. For example, SOCE practitioners misleadingly hold themselves out as being able to "convert" patients from LGB to heterosexual. Roger Callahan and other practitioners of Voice Therapy, a variant of Thought Field Therapy, have claimed 97% to 98% cure rates for all emotional disorders (Callahan & Callahan, 2000).

These misleading claims by therapists arguably fall under a broader existing legal regime that defines them as fraud (Victor, 2014). An antifraud approach casts a wider net than targeted legislation and could be used to address pseudotherapies that are merely ineffective rather than harmful. It also closes many of the loopholes that exist under legislative bans. This approach can be used to restrict the practice of pseudotherapies by unlicensed practitioners and practitioners who work with adults. Addi-

tionally, it is less vulnerable to constitutional challenge.

Ferguson v. Jonah

The same year the California legislation was enacted to restrict SOCE, another approach was being taken in New Jersey against this pseudotherapy.

In November 2012, a lawsuit was filed against a SOCE practitioner group called Jews Offering New Alternatives for Healing (JONAH) by a group of former patients.

The patients alleged that JONAH's promise to cure them of their homosexuality was fraudulent and deceptive in violation of New Jersey's Consumer Fraud Act (CFA). *Ferguson v. JONAH* (2015) is a landmark case—the first consumer fraud claim filed against conversion therapists in the nation (Dubrowski, 2015).

The bases for the fraud claim were three key misrepresentations made by JONAH: first, homosexuality is a mental disorder; second, sexual orientation can be changed; and third, that JONAH's practices were well grounded in science and that there was "empirical evidence" supporting their efficacy. Other misrepresentations included that the program's success rate was 66% and that it worked on a specified time frame (Dubrowski, 2015).

In addition to advertisements for individual and group therapy, the main evidence of fraud came from JONAH's list serve and emails to potential clients. The plaintiffs also testified that they had been personally assured they had a two out of three chance of changing their sexual orientation.

Bolstering the plaintiffs' case for fraud was the 2009 APA report discrediting any treatment model that purports to change sexual orientation. After a systematic review of the research on the efficacy of sexual change efforts, the APA's report concluded that claims of the effectiveness of SOCE for changing sexual orientation are not supported.

The APA also filed an amicus brief for the plaintiffs stating that the consensus of mental health professionals and researchers is that homosexuality is a normal expression of sexuality (Dubrowski, 2015).

To counter the claims of fraud, JONAH submitted reports from six experts including four conversion therapists, one medical doctor, and one rabbi. They all testified that homosexuality is not universally accepted as normal. Rather, they asserted that homosexuality is a learned response to childhood "wounds" and is addressable through therapy.

The court found JONAH's expert testimony inadmissible. New Jersey, like many other states, has adopted the Frye test to determine the admissibility of expert testimony (*United States v. Frye*, 1923). Under this test, the reliability of expert testimony depends on whether it has general acceptance in its field. The court found that "the overwhelming weight of scientific authority concludes that homosexuality is not a disorder" (*Ferguson v. Jonah*, 2015, p. 19). It also noted that a "group of a few closely associated experts cannot incestuously validate one another in order to establish the reliability of their shared theories" (*Ferguson v. Jonah*, p. 26).

The New Jersey court ruled that JONAH had violated the consumer fraud act by stating that homosexuality is not a normal variant of sexuality. After only 3 hours of deliberation, the jury found the defendants were guilty of unconscionable consumer fraud.

In addition to attorneys' fees and damages, the plaintiffs were granted injunctive relief and the JONAH clinic was permanently closed (Dubrowski, 2015).

The verdict in JONAH has been described as a potential "coup de grace to the remaining providers of conversion therapy in the United States" (Dubrowski, p. 79).

The case provides a powerful model for lawsuits in other states that can be used to curtail the practice of conversion therapy, and potentially other pseudotherapies.

Implementing Ferguson to Target SOCE

Every state has a consumer protection law that grants private citizens the right to enforce it through civil causes of action. Accordingly, implementation of the *Ferguson* model means that individual victims of SOCE would bring lawsuits against SOCE practitioners for deceptiveness-based professional conduct.

This antideception approach to SOCE is consistent with the ways in which many states currently regulate the advertising of licensed therapists. For example, California has a provision that prohibits public communications by psychologists that contain false, fraudulent, or misleading statements. This includes any claims intended to induce or likely to induce services that cannot be substantiated by reliable, peer-reviewed, and published scientific studies. Under these provisions, an offending practitioner can be de-licensed (Victor, 2014).

This strategy has been used successfully in Arizona to limit the practice of Voice Technology (VT), a variant of Thought

Field Therapy (TFT). In 1999, the Arizona Board of Psychologists sanctioned a psychologist for making false advertising claims of a 95% success rate for VT and forbid him from practicing both VT and TFT (Pignotti, 2007).

An antideception approach can be used to expand the protection of legislative bans against SOCE. For example, adult citizens of California who are currently not protected by the legislative ban can sue SOCE practitioners for fraud.

Similarly, in states with narrower legislative bans against conversion therapy only, the antideception approach expands protection against SOCE broadly for adults and children.

Under some state antideception laws, it's not necessary that the individual have received the services to file the complaint. For example, California law provides that "anyone who thinks that a psychologist has acted illegally or irresponsibly can file a complaint" (Victor, 2014, p. 1574).

The primary disadvantage to the use of the consumer fraud acts to curtail the practice of SOCE is its slow pace. Cases must be brought on an individual basis. However, if a sufficient number of individual complaints are brought, the state board of psychology could adopt a regulation that clarifies that SOCE advertising and SOCE efforts within a doctor-patient relationship are covered under the state's definition of unprofessional conduct.

The Comparative Benefits of an Antifraud Approach for Targeting SOCE and Other Pseudotherapies

Because there is consensus within the mental health establishment that homosexuality is not a disorder and that SOCE cannot change sexual orientation, SOCE falls squarely within the ambit of an antideception regime (Victor, 2014). However, for other pseudotherapies that lack this consensus (e.g., TFT, past life regression therapy), an anti-deceptive approach may be more challenging.

Nevertheless, in comparison to targeted legislation, the use of state consumer fraud laws to target pseudotherapies has several advantages.

First, logistically it's easier to use existing laws rather than have new laws passed. If a state doesn't have a consumer fraud law, it's likely easier to convince legislators to pass general antideception statutes than a targeted ban against a particular pseudotherapy.

Second, this approach is less vulnerable to First Amendment challenges. Fraudulent or deceptive advertising is widely considered to be outside the scope of the First Amendment and the government may ban it. For this reason, there is a general assumption that states may prohibit misleading advertising. States also have the authority to regulate the conduct of psychologists and other licensed mental health professionals. Restricting speech that is incidental to the regulation of professional conduct is not considered a free speech restriction at all. Consequently, prohibiting a psychologist from making deceptive promises about a treatment's efficacy would likely survive First Amendment challenges (Dubrowski, 2015; Victor, 2014).

Third, unlike a legislative ban, there is no need to show that the pseudotherapy is harmful and the state has a compelling interest to protect against it. Because the majority of pseudotherapies are merely ineffective rather than harmful, a larger number can be targeted with this approach.

Fourth, every state's consumer fraud law provides a plaintiff who wins their case equitable relief. This means the court can enjoin or stop the offending therapist from continuing to perpetrate the fraud on the public. In JONAH, a permanent injunction was issued closing the clinic and prohibiting the JONAH therapists from ever practicing conversion therapy again (Dubrowski, 2015).

Fifth, many states' consumer fraud laws do not require that the practitioner of a pseudotherapy knew or intended his actions to be fraudulent. Consequently, actions brought under the Consumer Fraud Acts could target pseudotherapy practitioners who seem to believe firmly in their treatments and the pseudoscientific basis for them rather than seek to defraud the public (Dubrowski, 2015). For example, Roger Callahan is the creator of TFT and frequently described as a "true believer" in the effectiveness of his treatment, despite a lack of any scientific evidence to support it (Pignotti, 2007). Similarly, the SOCE practitioners at JONAH may have genuinely believed in the effectiveness of their treatment.

For states that do require a showing of intent, the burden can be met with expert testimony that there is no science to support the efficacy of the pseudotherapy. Alternatively, expert testimony that there is a general consensus in the psychological community that the practitioner's state-

ments are false would suffice (Dubrowski, 2015).

The intent requirement poses a potential obstacle for those seeking to pursue a pseudotherapy fraud action. While consensus within the psychological community certainly exists for conversion and rebirthing therapies, it does not for many other pseudotherapies. The APA, for example, offers continuing education credits for attachment therapy and EMDR.

Conclusion

The proliferation of pseudotherapies poses harm to consumers of therapy and clinical psychologists. The inadequate protection by professional associations and governmental agencies underscores the importance of exploring legal remedies.

A review of the two primary legal strategies that have been used to curtail the practice of SOCE suggests that pseudotherapies could be effectively targeted through the state's antideception laws. This strategy seems to be an effective alternative to legislative bans and is relatively impervious to challenge on constitutional grounds. The use of the states' consumer fraud acts also obviates the need to establish the treatment is harmful. Without the burden of showing harm, this strategy casts a wider net than targeted legislation and can be used to target a broader number of pseudotherapies.

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Exposing Pseudoscientific Practices: Benefits and Hazards

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EXPOSING PSEUDOSCIENTIFIC practices comes with a price. Although I knew this when I began exposing such practices, I greatly underestimated the magnitude of vitriolic attacks from proponents of such practices; this has greatly impacted my career.

What follows is an account of my experience in writing about the harmful effects of “attachment therapies” and holding and coercive restraint therapies used in addressing behavioral problems, mostly with foster and adopted children. Such practices are lacking in scientific support, and, in some cases, have resulted in great harm, including death. Some critics have characterized this as torture. One of the most egregious examples of this is that of 10-year-old Candace Newmaker, who in 2000 was smothered to death by two unlicensed therapists in a rebirthing session, that consisted of placing pillows on top of her and having four adults sitting on top of her small frame, ignoring pleas that she could not breathe (Mercer, Sarner, & Rosa, 2003). However, despite the fact that a child died and a law was subsequently passed (Candace’s Law outlawing Rebirthing Therapy), similar and equally troubling practices continued (see Thyer & Pignotti, 2015, Chapter 3, for an overview).

Enough concern was raised about these types of attachment therapies that a special task force was convened by the American Psychological Association and the Professional Society on the Abuse of Children (APSAC; Chaffin et al., 2006), which compiled a report to review and evaluate these practices. The APSAC report noted concern about:

... a variety of coercive techniques are used, including scheduled holding, binding, rib cage stimulation (e.g., tickling, pinching, knuckling), and/or licking. Children may be held down, may have several adults lie on top of them, or their faces may be held so they can be forced to engage in prolonged eye contact. Sessions may last from 3 to 5 hours, with some sessions reportedly lasting longer. (Chaffin et al., 2006, p. 79)

The report also noted that not all attachment-based interventions were dangerous, and that some of the more focused shorter-term goal-directed interventions have some evidence of efficacy. However, the more broadly focused and extensive interventions were the ones of concern because of their potential to do harm. Conclusions and recommendations of the APSAC task force included the following:

- a. Treatment techniques or attachment parenting techniques involving physical coercion, psychologically or physically enforced holding, physical restraint, physical domination, provoked catharsis, ventilation of rage, age regression, humiliation, withholding or forcing food or water intake, prolonged social isolation, or assuming exaggerated levels of control and domination over a child are contraindicated because of risk of harm and absence of proven benefit and should not be used.
- b. Prognostications that certain children are destined to become psychopaths or predators should never be made based on early childhood behavior. These beliefs create an atmosphere conducive to overreaction and harsh or abusive treatment. Professionals should speak out against these and similar unfounded conceptualizations of children who are maltreated.
- c. Intervention models that portray young children in negative ways, including describing certain groups of young children as pervasively manipulative, cunning, or deceitful, are not conducive to good treatment and may promote abusive practices. In general, child maltreatment professionals should be skeptical of treatments that describe children in pejorative terms or that advocate aggressive techniques for breaking down children’s defenses. (Chaffin et al., 2006, p. 86)

Nevertheless, such practices still continued to be promoted and used by both licensed and unlicensed practitioners. It was out of concern for the harm (and the potential for harm) being done that the

nonprofit organization Advocates for Children in Therapy (ACT), was formed in 2003. Its mission statement is as follows:

Advocates for Children in Therapy (ACT) is a not-for-profit organization concerned with the methods used in the treatment of children's mental health. Specifically with respect to psychotherapy, parenting techniques, and other mental-health practices applied to children, ACT advocates humane, non-violent and scientifically validated treatments, and opposes the use of unvalidated practices, especially those known to be inhumane and abusive by:

- Raising general public awareness of the dangers and cruelty of such practices;
- Opposing governmental support and subsidy for such practices;
- Alerting professional organizations to inappropriate advocacy and promotion of such practices, such as in continuing education programs;
- Urging appropriate authorities to establish and then enforce standards of care and professional ethics to effectively ban the use of such practices;
- Assisting, with information and advice, in the prosecution of those who criminally defraud parents and damage children by using such practices or by recommending their use; and,
- Obtaining some measure of justice for the victims of such practices through restitution and compensation from the perpetrators (Advocates for Children in Therapy, n.d., para. 1-2).

Obviously, these are all laudable goals that few ethical mental health professionals, especially those who take an evidence-based approach, would disagree with, in spite of the virulent attacks this organization has received from proponents of the methods that they exposed.

In 2006, I was honored to accept, as a service to the profession, a position on their Board of Directors, where I served for 4 years (2006–2010). Even though I had published peer-reviewed articles, the audience for such articles was small, and the general public, especially potential consumers of such practices, needed to be educated so they could make informed choices about treatments for their children.

My interest in understanding and exposing the dangers of pseudoscientific practices predates my mental health degrees, and began with my 6-year personal experience in Scientology. After leaving Scientology in the late 1970s, I was highly motivated to understand how such groups operated to attract and retain members, as well as their practices, particularly when it came to the extraordinary mental health claims being made that were based largely on testimonials and anecdotes, utterly lacking in scientific evidence. Out of my desire to learn and practice therapies that were noncoercive and actually helped people, I obtained an M.S.W. from Fordham University in 1996. Following graduation, I worked for 5 years at Saint Vincent's Hospital in Geriatric and Palliative Care research. Additionally, I had a private practice in New York City as a certified social worker. Unfortunately, since I did not completely understand evidence-based practice at the time, around 1997, I became involved with Thought Field Therapy, which was invented by licensed psychologist Roger Callahan (see Callahan & Callahan, 2000, for a full description). Ultimately, this resulted in my conducting a much-needed controlled experiment (Pignotti, 2005) that showed this practice was not what it was claimed to be.

During my time in the Ph.D. program at Florida State University's (FSU) College of Social Work, I continued to publish articles related to the exposure and critique of pseudoscientific practices, including questionable attachment and holding therapies and coercive restraint therapies (Mercer & Pignotti, 2007; Pignotti & Mercer, 2007). It was these articles that made proponents of such therapies aware of my work, which, needless to say, did not please them. For example, one of the proponents of such therapies contacted the dean at the university where one of the critics worked, alleging that she was mentally unbalanced, had personal problems, and was transsexual (which, in addition to the obvious bigotry in such an accusation, was not true). Although I was aware already of some individuals who had been the target of vicious personal attacks and harassment after criticizing questionable mental health practices, I was not expecting what was to follow. In the summer of 2009, I defended my dissertation and was teaching classes at FSU's College of Social Work.

In the spring of 2009, a blogger who was a survivor of attachment therapy as a child and was exposing it on her blog, received a threatening email from one of the thera-

pists we had been criticizing, which she shared with me. The email warned that he would be exposing our sexual problems and Scientology past. Weeks later, some very derogatory anonymous postings, done through anonymous remailers impossible to trace, began appearing on public Internet groups about me and my colleagues at ACT. Bizarre advertisements were run on Craigslist and BackPage about me. Some of these advertisements were in the erotic section under my name. I had to cancel the text function on my cell phone because I was receiving obscene texts from men who were answering the ads. I received a phone call from a man who wanted me to work at his erotic establishment. Some of the postings claimed that I was having an affair with my Ph.D. dissertation chair and other faculty members and that I had been fired from FSU (I was not) and that I was unfit to teach. On the contrary, my course evaluations were acceptable, there had never been any complaints against me, and I simply stopped teaching there after I graduated, as all Ph.D. students did (FSU does not hire their own former students). One person even posted a bad review of me on "Rate My Professors" at the FSU Pensacola Campus (I never taught on that campus).

At the same time, one of the therapists whom my colleagues in ACT had criticized wrote a letter to the Dean of the College of Social Work complaining about me. After explaining the situation to the dean, he chose to take no action. This was the same individual mentioned previously who had also complained to the dean of the university where one of my colleagues was a Professor Emerita, again fortunately, to no avail. An anonymous individual also sent bizarre emails to faculty members and other Ph.D. students at FSU about me. The content was so incoherent that none of my colleagues believed what was alleged in them, but being a target was not helpful to my reputation. Very little was known about cyberstalking at the time, and there were people who tended to take a "blame the victim" attitude, wondering how I had gotten myself into this situation. Some people believed the old adage "where there's smoke, there's fire," and were unable to entertain the idea that the targets of such attacks were completely innocent and were instead being attacked for doing something to try to help others.

These attacks were happening at the same time as I was on the job market for a tenure-track faculty position, and this was being brought up at some of my on-campus interviews. Although the faculty I

interviewed with were largely sympathetic, my situation was also difficult for most of them to understand and, to make things even worse, there were some who were sympathetic to some of the people I had criticized. Universities aligning themselves with pseudoscientific practices is, unfortunately, not uncommon, and I had written a piece (Pignotti, 2007) exposing this practice at a top-ranked school of social work where energy-tapping therapies were being taught. At the time, a well-meaning member of the profession who was a strong proponent of evidence-based practice had warned me about this, saying that the social work profession was a small world and there could be consequences for writing such pieces. I had dismissed this warning, believing that the better, more evidence-based establishments would see value in what I did and in fact, some of them did, but ultimately, not enough to want to hire me. In fact, one director of research of a reputable university, after I had presented my research and other evidence to document the problem of pseudoscience in the social work profession, still dismissed the notion that the profession had any problem and implied that my area of interest in investigating such usage by practitioners was not a valid one. Even though I pointed out the high percentage of licensed clinical social workers that are in private practice, he maintained that the agencies were all using evidence-based practice now and that there was no problem in the profession, which ran contrary to my own investigations and research (Pignotti & Thyer, 2009, 2012). I had to wonder how many others with whom I had interviewed agreed with this research director, but were too polite to be as blunt as this particular individual had been.

After 5 years of a job search and over 100 applications, I failed to obtain a faculty position of any kind. While there is no way for me to prove a direct cause-and-effect relationship, what I do know is that every one of my peers in my Ph.D. cohort who sought faculty positions obtained them—and even though my credentials, teaching, and research experience were at least equivalent to theirs (I had more peer-reviewed publications than anyone in my cohort), I was not able to obtain such a position. I believe this was the consequence of the focus of my work in exposing and pseudoscientific practices. In addition to the more general problem faculty might have had with my involvement, the bizarre material on the Internet, even though none of them believed it was true, was likely

something that no one wanted inflicted upon any faculty at an establishment that might have hired me. It was as if I had a contagious disease; although it was not my fault, people were sympathetic, it was not something anyone wanted to be around.

Everything came to a head in December 2010 when one of the therapists sued me, along with five of my colleagues, for defamation and interference with business. Interestingly, while the lawsuit was under way, the anonymous postings, which had been occurring on an almost daily basis, almost completely stopped. Fortunately, we were able to have the suit escalated to a federal court, where the case was dismissed before the discovery phases and trial. After the lawsuit was dismissed, the anonymous attacks resumed with a vengeance, including a fake posting about me on a site designed to expose adulterers, where someone accused me of having an affair with her husband in a city I wasn't even living in. I was only able to get the site to agree to take it down after I proved to them they had copied the story from elsewhere and changed the person's name to mine. When the therapist lost the lawsuit, he put up a derogatory document about all of us on his business' website, saying that as a public figure, his lawsuit against us had been unwinnable. Actually, the case was dismissed due to failure to state a claim upon which relief can be granted and jurisdiction. The judge had noted that he would be likely considered a public figure, had the case progressed, but that was not the reason the case was dismissed. Rather, First Amendment rights to express our opinions absent factually false statements gave him no case. In addition to being a victory for Internet free speech, this is also a victory for academic freedom.

The attacks continued through 2011 and finally, by 2012, suddenly lessened with only an occasional blog post. However, when it came to my academic career, the damage had been done. Being 3 years out from graduation made it even more difficult for me to obtain a faculty position, and although I continued to try, after hundreds of applications, I did not obtain a faculty position. I continue to write and publish on understanding and exposing pseudoscience and disseminating evidence-based practice, but I make my living outside the profession.

What are the lessons learned from this experience? I have been asked if I would do what I did again, now knowing what the consequences could be. Essentially, my answer would be yes: I do not think I

could be fulfilled being in a profession where I had to keep silent in order to get ahead; I would feel as though I sold out. If my colleague and other experienced social work faculty members who had warned me about the "small world" of the profession had been right, that I needed to remain silent until getting a position and then getting tenure, I could not have lived with that decision.

Are there things I would do differently? Of course. I would have posted less lengthy responses and explanations to my critics on my blog—the feedback I received indicated that such responses did not help and made me look unbalanced. I would have instead limited myself to one statement refuting the lies that were posted about me. It is difficult to determine, though, whether that would have made a difference or lessened the attacks, as my colleagues who were silent when attacked were still just as viciously attacked as I was, the only difference being, they were not seeking faculty positions or already had tenure, so did not suffer the consequences I had.

Hillary Clinton, who has been the target of a much more highly publicized and broader attacks, recently expressed regret that she hadn't been more vociferous in her responses and fought back harder. Being silent did not stop the attacks, nor did it help her win the election. These types of attacks place the target in a double-bind situation. If we fight back hard, we are portrayed as mentally imbalanced and anything we say, no matter how seemingly innocuous, can further be twisted and distorted. On the other hand, if we do not fight back, people believe that the accusations must have some truth in them or that the person has something to hide or doesn't care.

Since cyber abuse of this kind is a relatively new phenomenon, we really do not know what tactics would be effective in stopping it. There is still a tendency to blame the victims, thinking that if only we had behaved differently, this would not have happened. Hopefully, there will be more empirical study of this phenomenon to find out what works and what doesn't work when dealing with cyber abuse.

I have also been asked what advice I would give to students interested in academic careers who are concerned about the problem and wish to expose it. What I would advise is to make an informed choice. I had been exposing various pseudoscientific practices for years with very little consequences, until I angered the wrong people by criticizing their particular

C L A S S I F I E D

FULL TIME THERAPIST POSITION AVAILABLE Dec 2017

Due to our continued growth, Mountain Valley Treatment Center seeks an additional licensed clinician to join our exceptional clinical team as a primary therapist at its beautiful new campus in Plainfield, NH. Mountain Valley, a short term residential treatment program, serves male and female adolescents and emerging adults, 13 – 20 years old, from around the globe with debilitating anxiety and OCD. Our newest campus, located near Hanover, NH and Dartmouth College, provides a unique professional and treatment environment as well as a locale to conveniently implement in-vivo exposure exercises.

Mountain Valley adds clinically intensive CBT and ERP within an experiential education program and mindfulness-based milieu. Our program was recently featured in the October 15, 2017 New York Times Magazine as well as other local and national media venues highlighting the great work being done.

Primary Therapists manage a caseload of three to five private pay residents over their 90-day treatment stay, providing individual, group, and family therapy. Designing and implementing exposures with their clients both on our 25-acre campus and within local communities provides a unique professional experience unmatched at any other residential treatment setting.

The ideal candidate will have, at a minimum, a Master's degree, be currently licensed or license-eligible in New Hampshire, and have an understanding of CBT based modalities such as DBT, ACT and ERP. Prior experience serving clients with OCD and anxiety disorders preferred.

Mountain Valley offers above average salary, full benefits package, relocation and temporary housing assistance, and sponsored professional development opportunities such as attending ABCT, ADAA and IOCDF conferences. Casual work and team focused environment. Mountain Valley supports the professional growth of all its staff.

Please contact Don Vardell, Executive Director at dvardell@mountainvalleytreatment.org for more information or to apply.

practice. That is a risk that anyone takes who chooses to expose such practices and someone who wants an academic career needs to realize that it could interfere with their ability to get hired or get tenure. One option would be to, instead, as a student and a new graduate, focus on disseminating evidence-based approaches and leave the exposure of pseudoscience to others with more secure positions, but even then, there have been severe consequences as we can see with what Elizabeth Loftus had to endure as a result of her research on recovered memory. This is an informed personal professional choice that each person will need to make for themselves, weighing the importance of their values, the benefits and possible consequences. What I do know is that if such risks are not taken, these practices will continue to be promoted, especially on the Internet, and writing for journals that only academic colleagues read is not enough. Fortunately there is a recent trend in the academic community, with projects such as the Public Voices Fellowship (OpEd Project, n.d.), to encourage people in various academic disciplines, especially those that impact the public, who regularly publish little-read peer-reviewed articles, to extend their writing and voicing of opinions to the larger community in the form of op-eds, blogs, Tweets, and other media where their knowledge and expertise is so badly needed.

Exposing pseudoscience and other misinformation disseminated to the public is truly an interdisciplinary effort that all health and mental health professions, as well as perhaps sociologists and anthropologists, could be involved in. I am not alone in challenging such practices and I am deeply grateful to all my colleagues who have taken these risks, in spite of all the consequences they suffered.

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Nominate the Next Candidates for ABCT Office

I nominate the following individuals:

PRESIDENT-ELECT (2018–2019)

REPRESENTATIVE-AT-LARGE (2018–2021)
and liaison to Convention and Education Issues

SECRETARY-TREASURER (2019–2022)

NAME (printed)

SIGNATURE (required)

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, 2018, 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. Only full and new member professionals can nominate candidates. Contact the Leadership and Elections Chair for more information about serving ABCT or to get more information on the positions. **Complete, sign, and send form to: David Pantalone, Ph.D., Leadership & Elections Chair, ABCT, 305 Seventh Ave., New York, NY 10001.**

Good governance requires participation of the membership in the elections. ABCT is a membership organization that runs democratically. We need your participation to continue to thrive as an organization.

NOTE: To be nominated for President-Elect of ABCT, it is recommended that a candidate has served on the ABCT Board of Directors in some capacity; served as a coordinator; served as a committee chair or SIG chair; served on the Finance Committee; or have made other significant contributions to the Association as determined by the Leadership and Elections Committee. Candidates for the position of President-Elect shall ensure that during his/her term as President-Elect and President of the ABCT, the officer shall not serve as President of a competing or complementary professional organization during these terms of office; and the candidate can ensure that their work on other professional boards will not interfere with their responsibilities to ABCT during the presidential cycle.

This coming year we need nominations for three elected positions: President-Elect, Secretary-Treasurer, and Representative-at-Large. Each representative serves as a liaison to one of the branches of the association. The representative position up for 2018 election will serve as the liaison to Convention and Education Issues Coordinator.

A thorough description of each position can be found in ABCT's bylaws: www.abct.org/docs/Home/byLaws.pdf.

Three Ways to Nominate

- Mail the form to the ABCT office (address above)
- Fill out the nomination form by hand and fax it to the office at 212-647-1865
- Fill out the nomination form by hand and then scan the form as a PDF file and email the PDF as an attachment to our committee: membership@abct.org.

The nomination form
with your original
signature is
required,
regardless
of how
you get
it to
us.



ABCT AWARDS & RECOGNITION 2017

51st Annual Convention | November 17 | San Diego

(left) David DiLillo
Outstanding Service to ABCT



(right) Marsha Linehan
**Lifetime Achievement Award
 (2016)**



(NOTE: The 2017 Lifetime Achievement is awarded to **Dianne L. Chambless**)

(left) President Gail Steketee with
**President's New Researcher
 Christian A. Webb**



(right) **Graduate Student
 Research Grant,
 Hannah Lawrence (l),
 and Honorable Mention
 Amanda L. Sanchez (r),**



(below) Alexandra Kredlow accepting
 the **Virginia Roswell Student
 Dissertation Award**



**Leonard Krasner Student
 Dissertation: Shannon
 Michelle Blakey**



**John R. Z. Abela Student
 Dissertation:
 Carolyn Spiro**



**Anne Marie Albano
 Early Career Award:
 Carmen P. McLean**

ABCT AWARDS & RECOGNITION 2017



*(left) Outstanding Training Program:
Lee Cooper, Director, Clinical Science
Ph.D. Program, Virginia Polytechnic
Institute*



*(right) Jennifer P. Read
Outstanding Contribution to Research*



**Elsie Ramos First
Author Memorial
Poster Award Winners**
*(left to right)
Awards Committee
member Sara Elkins,
with Kate Kysow,
Chloe Hudson, &
Christian Goans*

**Student Travel
Award Winner**
Dev Crasta



**ADAA Travel
Award Winners**
*(left to right)
President Gail Steketee,
Andrea Niles,
Amy Sewart, Jennie
Kuckertz, and Awards
Chair Katherine Baucom*





Call *for* Award Nominations

to be presented at the 52nd Annual Convention in Washington, DC

The ABCT Awards and Recognition Committee, chaired by Cassidy Gutner, Ph.D., of Boston University School of Medicine, is pleased to announce the 2018 awards program. Nominations are requested in all categories listed below. Given the number of submissions received for these awards, the committee is unable to consider additional letters of support or supplemental materials beyond those specified in the instructions below. Please note that award nominations may not be submitted by current members of the ABCT Board of Directors.

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. Recent recipients of this award include Thomas H. Ollendick, Lauren B. Alloy, Lyn Abramson, David M. Clark, Marsha Linehan, and Dianne L. Chambless. Applications should include a nomination form (available at www.abct.org/awards), three letters of support, and the nominee's curriculum vitae. Please e-mail the nomination materials as one pdf document to 2018ABCTAwards@abct.org. Include "Career/Lifetime Achievement" in the subject line. **Nomination deadline:** March 1, 2018

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. Previous recipients of this award are Richard Heimberg, G. Terence Wilson, Richard J. McNally, Mitchell J. Prinstein, Bethany Teachman, and Evan Forman. Please complete the nomination form found online at www.abct.org. Then e-mail the completed form and associated materials as one pdf document to 2018ABCTAwards@abct.org. Include "Outstanding Mentor" in your subject heading. **Nomination deadline:** March 1, 2018

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. Recent recipients of this award include Mark S. Bauer, Vikram Patel, Benedict Carey, and Patrick J. Kennedy. Applications should include a nomination form (available at www.abct.org/awards), three letters of support, and the nominee's curriculum vitae. Please e-mail the nomination materials as one pdf document to 2018ABCTAwards@abct.org. Include "Distinguished Friend to BT" in the subject line. **Nomination deadline:** March 1, 2018

Mid-Career Innovator

Eligible candidates for the Mid-Career Innovator Award are members of ABCT in good standing who are at the associate professor level or equivalent mid-career level, and who have made significant innovative contributions to clinical practice or research on cognitive and/or behavioral modalities. The previous recipient was Carla Kmett Danielson. Please complete the nomination form found online at www.abct.org. Then e-mail the completed form and associated materials as one pdf document to 2018ABCTAwards@abct.org. Include "Mid-Career Innovator" in the subject line. **Nomination deadline:** March 1, 2018

Anne Marie Albano Early Career Award for Excellence in the Integration of Science and Practice

Dr. Anne Marie Albano is recognized as an outstanding clinician, scientist, and teacher dedicated to ABCT's mission. She is known for her contagious enthusiasm for the advancement of cognitive and behavioral science and practice. The purpose of this award is to recognize early career professionals who share Dr. Albano's core commitments. This award includes a cash prize to support travel to the ABCT Annual Meeting and to sponsor participation in a clinical treatment workshop. Eligibility requirements are as follows: (1) Candidates must be active members of ABCT, (2) New/Early Career Professionals within the first 5 years of receiving his or her doctoral degree (PhD, PsyD, EdD). Preference will be given to applicants with a demonstrated interest in and commitment to child and adolescent mental health care. Applicants should submit: nominating cover letter, CV, personal statement up to three pages (statements exceeding 3 pages will not be reviewed), and 2 to 3 supporting letters. Application materials should be emailed as one pdf document to 2018ABCTAwards@abct.org. Include candidate's last name and "Albano Award" in the subject line. **Nomination deadline:** March 1, 2018

Student Dissertation Awards

- Virginia A. Roswell Student Dissertation Award (\$1,000)
- Leonard Krasner Student Dissertation Award (\$1,000)
- John R. Z. Abela Student Dissertation Award (\$500)

Each award will be given to one student based on his/her doctoral dissertation proposal. Accompanying this honor will be a monetary award (see above) 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. Eligibility requirements for these awards are as follows: 1) Candidates must be student members of ABCT, 2) Topic area of dissertation research must be of direct relevance to cognitive-behavioral therapy, broadly defined, 3) The dissertation must have been successfully proposed, and 4) The dissertation must not have been defended prior to November 2017. Proposals with preliminary results included are preferred. To be considered for the Abela Award, research should be relevant to the development, maintenance, and/or treatment of depression in children and/or adolescents (i.e., under age 18). Self-nominations are accepted or a student's dissertation mentor may complete the nomination. The nomination must include a letter of recommendation from the dissertation advisor. Please complete the nomination form found online at www.abct.org/awards/. Then e-mail the nomination materials (including letter of recommendation) as one pdf document to 2018ABCTAwards@abct.org. Include candidate's last name and "Student Dissertation Award" in the subject line. **Nomination deadline:** March 1, 2018

President's New Researcher Award

ABCT's 2017-18 President, Sabine Wilhelm, Ph.D., invites submissions for the 40th Annual President's New Researcher Award. The winner will receive a certificate and a cash prize of \$500. The award will be based upon an early program of research that reflects factors such as: consistency with the mission of ABCT; independent work published in high-impact journals; and promise of developing theoretical or practical applications that represent clear advances to the field. While nominations consistent with the conference theme are particularly encouraged, submissions will be accepted on any topic relevant to cognitive behavior therapy, including but not limited to topics such as the development and testing of models, innovative practices, technical solutions, novel venues for service delivery, and new applications of well-established psychological principles. Requirements: candidates must be the first author, and self-nominations are accepted; 3 letters of recommendation must be included; the author's CV, letters of support, and paper must be submitted in electronic form.

E-mail the nomination materials (including letter of recommendation) as one pdf document to PNRAward@abct.org. Include candidate's last name and "President's New Researcher" in the subject line. **Nomination deadline:** August 1, 2018

Nominations for the following award are solicited from members of the ABCT governance:

Outstanding Service to ABCT

Please complete the nomination form found online at www.abct.org/awards/. Then e-mail the completed form and associated materials as one pdf document to 2018ABCTAwards@abct.org. Include "Outstanding Service" in the subject line.

Nomination deadline: March 1, 2018

ABCT's 52nd Annual Convention

November 15–18, 2018 • Washington, DC

Preparing to Submit an Abstract

The ABCT Convention is designed for scientists, practitioners, students, and scholars who come from a broad range of disciplines. The central goal is to provide educational experiences related to behavioral and cognitive therapies that meet the needs of attendees across experience levels, interest areas, and behavioral and cognitive theoretical orientations. Some presentations offer the chance to learn what is new and exciting in behavioral and cognitive assessment and treatment. Other presentations address the clinical-scientific issues of how we develop empirical support for our work. The convention also provides opportunities for professional networking. The ABCT Convention consists of General Sessions, Targeted and Special Programming, and Ticketed Events.

ABCT uses the Cadmium Scorecard system for the submission of general session events. The step-by-step instructions are easily accessed from the Abstract Submission Portal, and the ABCT home page. Attendees are limited to speaking (e.g., presenter, panelist, discussant) during no more than FOUR events. As you prepare your submission, please keep in mind:

- **Presentation type:** Please see the two right-hand columns on this page for descriptions of the various presentation types.
 - **Number of presenters/papers:** For Symposia please have a minimum of four presenters, including one or two chairs, only one discussant, and 3 to 5 papers. The chair may present a paper, but the discussant may not. For Panel Discussions and Clinical Round tables, please have one moderator and between three to five panelists.
 - **Title:** Be succinct.
 - **Authors/Presenters:** Be sure to indicate the appropriate order. Please ask all authors whether they prefer their middle initial used or not. Please ask all authors their degree, ABCT category (if they are ABCT members), and their email address. (Possibilities for “ABCT category” are *current member*; *lapsed member* or *nonmember*; *postbaccalaureate*; *student member*; *student nonmember*; *new professional*; *emeritus*.)
 - **Institutions:** The system requires that you enter institutions before entering authors. This allows you to enter an affiliation one time for multiple authors. DO NOT LIST DEPARTMENTS. In the following step you will be asked to attach affiliations with appropriate authors.
 - **Key Words:** Please read carefully through the pull-down menu of already defined keywords and use one of the already existing keywords, if appropriate. For example, the keyword “military” is already on the list and should be used rather than adding the word “Army.” Do not list behavior therapy, cognitive therapy, or cognitive behavior therapy.
 - **Objectives:** For Symposia, Panel Discussions, and Clinical Round Tables, write three statements of no more than 125 characters each, describing the objectives of the event. Sample statements are: “Described a variety of dissemination strategies pertaining to the treatment of insomnia”; “Presented data on novel direction in the dissemination of mindfulness-based clinical interventions.”
- Overall:** Ask a colleague to proof your abstract for inconsistencies or typos.

Thinking about submitting an abstract for the ABCT 52nd Annual Convention in DC? The submission portal will be opened from February 14–March 14. Look for more information in the coming weeks to assist you with submitting abstracts for the ABCT 51st Annual Convention. The deadline for submissions will be 11:59 P.M. (EST), Wednesday, March 14, 2018. We look forward to seeing you in Washington, DC!

Understanding the ABCT Convention

General Sessions

There are between 150 and 200 general sessions each year competing for your attention. An individual must LIMIT TO 6 the number of general session submissions in which he or she is a SPEAKER (including symposia, panel discussions, clinical roundtables, and research spotlights). The term SPEAKER includes roles of chair, moderator, presenter, panelist, and discussant. Acceptances for any given speaker will be limited to 4. All general sessions are included with the registration fee. These events are all submitted through the ABCT submission system. The deadline for these submissions is 11:59 PM, Wednesday, March 15, 2017. General session types include:

Symposia

In response to convention feedback requesting that symposia include more presentations by established researchers/faculty along with their graduate students, preference will be given to symposia submissions that include non-student researchers and faculty members as first-author presenters.

Symposia are presentations of data, usually investigating the efficacy or effectiveness of treatment protocols. Symposia are either 60 or 90 minutes in length. They have one or two chairs, one discussant, and between three and five papers. No more than 6 presenters are allowed.

Panel Discussions and Clinical Round Tables

Discussions (or debates) by informed individuals on a current important topic. These are organized by a moderator and include between three and six panelists with a range of experiences and attitudes. No more than 6 presenters are allowed.

Spotlight Research Presentations

This format provides a forum to debut new findings considered to be groundbreaking or innovative for the field. A limited number of extended-format sessions consisting of a 45-minute research presentation and a 15-minute question-and-answer period allows for more in-depth presentation than is permitted by symposia or other formats.

Poster Sessions

One-on-one discussions between researchers, who display graphic representations of the results of their studies, and interested attendees. Because of the variety of interests and research areas of the ABCT attendees, between 1,200 and 1,400 posters are presented each year.

Targeted and Special Programing

Targeted and special programing events are also included with the registration fee. These events are designed to address a range of scientific, clinical, and professional development topics. They also provide unique opportunities for networking.

Invited Addresses/Panels

Speakers well-established in their field, or who hold positions of particular importance, share their unique insights and knowledge.

Mini Workshops

Designed to address direct clinical care or training at a broad introductory level and are 90 minutes long.

Clinical Grand Rounds

Clinical experts engage in simulated live demonstrations of therapy with clients, who are generally portrayed by graduate students studying with the presenter.

Research and Professional Development

Provides opportunities for attendees to learn from experts about the development of a range of research and professional skills, such as grant writing, reviewing manuscripts, and professional practice.

Membership Panel Discussion

Organized by representatives of the Membership Committees, these events generally emphasize training or career development.

Special Sessions

These events are designed to provide useful information regarding professional rather than scientific issues. For more than 20 years, the Internship and Postdoctoral Overviews have helped attendees find their educational path. Other special sessions often include expert panels on getting into graduate

school, career development, information on grant applications, and a meeting of the Directors of Clinical Training.

Special Interest Group (SIG) Meetings

More than 39 SIGs meet each year to accomplish business (such as electing officers), renew relationships, and often offer presentations. SIG talks are not peer-reviewed by the Association.

Ticketed Events

Ticketed events offer educational opportunities to enhance knowledge and skills. These events are targeted for attendees with a particular level of expertise (e.g., basic, moderate, and/or advanced). Ticketed sessions require an additional payment.

Clinical Intervention Training

One- and two-day events emphasizing the “how-to” of clinical interventions. The extended length allows for exceptional interaction.

Institutes

Leaders and topics for Institutes are selected from previous ABCT workshop presentations. Institutes are offered as a 5- or 7-hour session on Thursday, and are generally limited to 40 attendees.

Workshops

Covering concerns of the practitioner/educator/researcher, these remain an anchor of the Convention. Workshops are offered on Friday and Saturday, are 3 hours long, and are generally limited to 60 attendees.

Master Clinician Seminars

The most skilled clinicians explain their methods and show videos of sessions. These 2-hour sessions are offered throughout the Convention and are generally limited to 40 to 45 attendees.

Advanced Methodology and Statistics Seminars

Designed to enhance researchers' abilities, they are 4 hours long and limited to 40 attendees.

Continuing Education

See pp. 64-65 for a complete description.

ticketed
sessions

call

52nd Annual Convention

November 15–18, 2018 | Washington, DC

for Ticketed Sessions

Workshops & Mini Workshops

Workshops cover concerns of the practitioner/ educator/researcher. Workshops are 3 hours long, are generally limited to 60 attendees, and are scheduled for Friday and Saturday. Please limit to no more than 4 presenters. Mini Workshops address direct clinical care or training at a broad introductory level. They are 90 minutes long and are scheduled throughout the convention. Please limit to no more than 4 presenters. When submitting for Workshops or Mini Workshop, please indicate whether you would like to be considered for the other format as well. | *For more information or to answer any questions before you submit your abstract, contact **Lauren Weinstock, Workshop Committee Chair: workshops@abct.org***

Institutes

Institutes, designed for clinical practitioners, are 5 hours or 7 hours long, are generally limited to 40 attendees, and are scheduled for Thursday. Please limit to no more than 4 presenters. | *For more information or to answer any questions before you submit your abstract, contact **Christina Boisseau, Institute Committee Chair: institutes@abct.org***

Master Clinician Seminars

Master Clinician Seminars are opportunities to hear the most skilled clinicians explain their methods and show taped demonstrations of client sessions. They are 2 hours long, are limited to 40 attendees, and are scheduled Friday through Sunday. Please limit to no more than 2 presenters. | *For more information or to answer any questions before you submit your abstract, contact **Courtney Benjamin Wolk, Master Clinician Seminar Committee Chair: masterclinicianseminars@abct.org***

Research and Professional Development

Presentations focus on "how to" develop one's own career and/or conduct research, rather than on broad-based research issues (e.g., a methodological or design issue, grantsmanship, manuscript review) and/or professional development topics (e.g., evidence-based supervision approaches, establishing a private practice, academic productivity, publishing for the general public). Submissions will be of specific preferred length (60, 90, or 120 minutes) and format (panel discussion or more hands-on participation by the audience). Though this track is not new for 2018, this is the first time that RPD abstracts are due at the earlier deadline, along with ticketed events/mini workshops, and will also be submitted through the same portal. Please limit to no more than 4 presenters, and be sure to indicate preferred presentation length and format. | *For more information or to answer any questions before you submit your abstract, contact **Cole Hooley, Research & Professional Development Chair: researchanddevelopmentseminars@abct.org***

Submissions will now be accepted through the online submission portal, which will be open until February 1.

Submit a 250-word abstract and a CV for each presenter. For submission requirements and information on the CE session selection process, please visit www.abct.org and click on "Convention and Continuing Education."



Submission deadline: February 1, 2018

52nd Annual Convention

November 15–18, 2018 • Washington, DC

Call *for* Papers

Program Chair: Kiara R. Timpano, Ph.D.

ABCT has always celebrated advances in clinical science. We now find ourselves at the cusp of a new era, marked by technological advances in a range of different disciplines that have the potential to dramatically affect the clinical science we conduct and the treatments we deliver. These innovations are already influencing our investigations of etiological hypotheses, and are similarly opening new frontiers in the ways that assessments and treatments are developed, patients access help, clinicians monitor response, and the broader field disseminates evidence-based practices. Building on the strong, theoretical and practical foundations of CBT, we have the exciting opportunity to use our multidisciplinary values to identify new and emerging technologies that could catapult our research on mental health problems and well-being to the next level.

The theme of ABCT's 52nd Annual Convention, "Cognitive Behavioral Science, Treatment, and Technology," is intended to showcase research, clinical practice, and training that:

- Uses cutting-edge technology and new tools to increase our understanding of mental health problems and underlying mechanisms;
- Investigates how a wide range of technologies can help us improve evidence-based practices in assessment and the provision of more powerful interventions; and
- Considers the role technology can have in training a new generation of evidence-based treatment providers at home and across the globe.

The convention will highlight how advances in clinical science can be strengthened and propelled forward through the integration of multidisciplinary technologies.

Submissions may be in the form of symposia, clinical round tables, panel discussions, and posters. Information about the Convention and how to submit abstracts will be on ABCT's website, www.abct.org, after January 1, 2018.

**general
sessions**

Theme:

COGNITIVE
BEHAVIORAL
SCIENCE,
TREATMENT,
and
TECHNOLOGY

Portal opens

February 14, 2018

*Deadline
for submissions:*
March 14, 2018

*See p. 60
for information
about preparing
your abstract*

Submission deadline: February 14, 2018



ABCT and Continuing Education

At the ABCT Annual Conventions, there are Ticketed events (meaning you have to buy a ticket for one of these beyond the general registration fee) and General sessions (meaning you get in by paying the general registration fee), the vast majority of which qualify for Continuing Education credit. See the end of this document for the current list of bodies that have approved ABCT as a CE sponsor. Note that we do not currently offer CMEs. Attendance at each continuing education session in its entirety is required to receive CE credit. No partial credit is awarded; late arrival or early departure will preclude awarding of CE credit. For those who have met all requirements according to the organizations which have approved ABCT as a CE sponsor, certificates will be mailed early in the new year following the Annual Convention.

Ticketed Events Eligible for CE

All Ticketed Events offer CE in addition to educational opportunities to enhance knowledge and skills. These events are targeted for attendees with a particular level of expertise (e.g., basic, moderate, and/or advanced). Ticketed sessions require an additional payment beyond the general registration fee. For ticketed events attendees must sign in and sign out and complete and return an individual evaluation form to be awarded CE. It remains the responsibility of the attendee to sign in at the beginning of the session and out at the end of the session.

Clinical Intervention Trainings (CITs)

One- and two-day events emphasizing the "how-to" of clinical interventions. The extended length allows for exceptional interaction. Participants attending a full day session can earn 7 continuing education credits, and 14 CE credits for the two-day session.

Institutes

Leaders and topics for Institutes are selected from previous ABCT workshop presentations. Institutes are offered as a 5- or 7-hour session on Thursday, and are generally limited to 40 attendees. Participants in the full-day Institute can earn 7 continuing education credits, and in the half-day Institutes can earn 5 CE credits.

Workshops

Covering concerns of the practitioner/educator/researcher, these remain an anchor of the Convention. Workshops are offered on Friday and Saturday, are 3 hours long, and are generally limited to 60 attendees. Participants in these Workshops can earn 3 CE credits per workshop.

Master Clinician Seminars (MCS)

The most skilled clinicians explain their methods and show videos of sessions. These 2-hour sessions are offered throughout the Convention and are generally limited to 40 to 45 attendees. Participants in these seminars can earn 2 CE credits per seminar.

Advanced Methodology and Statistics Seminars (AMASS)

Designed to enhance researchers' abilities, there are generally two seminars offered on Thursday or during the course of the Convention. They are 4 hours long and limited to 40 attendees. Participants in these courses can earn 4 CE credits per seminar.

General Sessions Eligible for CE

There are 200 general sessions each year competing for your attention. All general sessions are included with the registration fee. Most of the sessions are eligible for CE, with the exception of the poster sessions, Membership Panel Discussions, the Special Interest Group Meetings (SIG), and a few other sessions. You are eligible to earn 1 CE credit per hour of attendance.

General sessions attendees must sign in and sign out and answer particular questions in the CE booklet regarding each session attended. The booklets must be handed in to ABCT at the end of the Convention. If the booklet is not completed and handed in, CE credit will not be awarded.

General session types that are eligible for CE include:

Clinical Grand Rounds

Clinical experts engage in simulated live demonstrations of therapy with clients, who are generally portrayed by graduate students studying with the presenter.

Invited Panels and Addresses

Speakers well-established in their field, or who hold positions of particular importance, share their unique insights and knowledge on a broad topic of interest.

Mini-Workshops

These 90-minute sessions directly address evidence-based clinical skills and applications. They are offered at an introductory level and clinical care or training issues.

Panel Discussions and Clinical Round Tables

Discussions (or debates) by informed individuals on a current important topic. These are organized by one moderator and include between three and five panelists with a range of experience and attitudes. The total number of speakers may not exceed 6.

Spotlight Research Presentations

This format provides a forum to debut new findings considered to be groundbreaking or innovative for the field. A limited number of extended-format sessions consisting of a 45-minute research presentation and a 15-minute question-and-answer period allows for more in-depth presentation than is permitted by symposia or other formats.

Symposia

Presentations of data, usually investigating the efficacy or effectiveness of treatment protocols. Symposia are either 60 or 90 minutes in length. They have one or two chairs, one discussant, and between three and five papers. The total number

General Sessions NOT Eligible for CE

Membership Panel Discussion

Organized by representatives of the Membership Committees, these events generally emphasize training or career development.

Poster Sessions

One-on-one discussions between researchers, who display graphic representations of the results of their studies, and interested attendees. Because of the variety of interests and research areas of the ABCT attendees, between 1,400 and 1,600 posters are presented each year.

Special Interest Group (SIG) Meetings

More than 39 SIGs meet each year to accomplish business (such as electing officers), renew relationships, and often offer presentations. SIG talks are not peer-reviewed by the Association.

Special Sessions

These events are designed to provide useful information regarding professional rather than scientific issues. For more than 20 years the Internship and Postdoctoral Overviews have helped attendees find their educational path. Other special sessions often include expert panels on getting into graduate school, career development, information on grant applications, and a meeting of the Directors of Clinical Training. These sessions are not eligible for CE credit.

Other Sessions

Other sessions not eligible for CE are noted as such on the itinerary planner and in the program book.

How Do I Get CE at the ABCT Convention?

The CE fee must be paid (see registration form) for a personalized CE credit letter to be distributed. Those who have included CE in their preregistration will be given a booklet when they pick up their badge and registration materials at the ABCT Registration Desk. Others can still purchase a booklet at the registration area during the convention. The current fee is \$99.00. We do not charge a fee that is hidden within general registration.

Which Organizations Have Approved ABCT as a CE Sponsor?

Psychology

ABCT is approved by the American Psychological Association to sponsor continuing education for psychologists. ABCT maintains responsibility for this program and its content. Attendance at each continuing education session in its entirety is required to receive CE credit. No partial credit is awarded; late arrival or early departure will preclude awarding of CE credit.

For ticketed events attendees must sign in and sign out and complete and return an individual evaluation form. For general sessions attendees must sign in and sign out and answer particular questions in the CE booklet regarding each session attended. The booklets must be handed in to ABCT at the end of the Convention. It remains the responsibility of the attendee to sign in at the beginning of the session and out at the end of the session.

Social Work

ABCT program is approved by the National Association of Social Workers (Approval # 886427222-7448) for 34 continuing education contact hours.

Continuing Education (CE) Grievance Procedure

ABCT is fully committed to conducting all activities in strict conformance with the American Psychological Association's Ethical Principles of Psychologists. ABCT will comply with all legal and ethical responsibilities to be non-discriminatory in promotional activities, program content and in the treatment of program participants. The monitoring and assessment of compliance with these standards will be the responsibility of the Coordinator of Convention and Continuing Education Issues in conjunction with the Director of Education and Meeting Services.

Although ABCT goes to great lengths to assure fair treatment for all participants and attempts to anticipate problems, there will be occasional issues which come to the attention of the convention staff which require intervention and/or action on the part of the convention staff or an officer of ABCT. This procedural description serves as a guideline for handling such grievances.

All grievances must be filed in writing to ensure a clear explanation of the problem. If the grievance concerns satisfaction with a CE session the Director of Outreach and Partnerships shall determine whether a full or partial refund (either in money or credit for a future CE event) is warranted. If the complainant is not satisfied, their materials will be forwarded to the Coordinator of Convention and Continuing Education Issues for a final decision.

If the grievance concerns a speaker and particular materials presented, the Director of Outreach and Partnerships shall bring the issue to the Coordinator of Convention and Continuing Education Issues who may consult with the members of the continuing education issues committees. The Coordinator will formulate a response to the complaint and recommend action if necessary, which will be conveyed directly to the complainant. For example, a grievance concerning a speaker may be conveyed to that speaker and also to those planning future educational programs.

Records of all grievances, the process of resolving the grievance and the outcome will be kept in the files of the Director of Education and Meeting Services. A copy of this Grievance Procedure will be available upon request.

If you have a complaint, please contact the ABCT central office at (212) 646-1890 for assistance, or email convention@abct.org.

Find a CBT Therapist



ABCT's *Find a CBT Therapist* directory is a compilation of practitioners schooled in cognitive and behavioral techniques. In addition to standard search capabilities (name, location, and area of expertise), ABCT's *Find a CBT Therapist* offers a range of advanced search capabilities, enabling the user to take a Symptom Checklist, review specialties, link to self-help books, and search for therapists based on insurance accepted.

We urge you to sign up for the *Expanded Find a CBT Therapist* (an extra \$50 per year). With this addition, potential clients will see what insurance you accept, your practice philosophy, your website, and other practice particulars.

To sign up for the *Expanded Find a CBT Therapist*, click *MEMBER LOGIN* on the upper left-hand of the home page and proceed to the ABCT online store, where you will click on "Find CBT Therapist."

For further questions, call the ABCT central office at 212-647-1890.

CBT Medical Educator Directory

Another indispensable resource from ABCT—an online directory of CBT educators who have agreed to be listed as potential resources to others involved in training physicians and allied health providers. In particular, the educators on this list have been involved in providing education in CBT and/or the theories underlying such interventions to medical and other allied health trainees at various levels. The listing is meant to connect teachers across institutions and allow for the sharing of resources.

Inclusion Criteria

1. Must teach or have recently taught CBT and/or CB interventions in a medical setting. This may include psychiatric residents, medical students, nursing, pharmacy, dentistry, or other allied health professionals, such as PT, OT, or RD. Teachers who exclusively train psychology graduate students, social workers, or master's level therapists do not qualify and are not listed in this directory.
2. "Teaching" may include direct training or supervision, curriculum development, competency evaluation, and/or curriculum administration. Many professionals on the list have had a central role in designing and delivering the educational interventions, but all educational aspects are important.
3. Training should take place or be affiliated with an academic training

facility (e.g. medical school, nursing school, residency program) and not occur exclusively in private consultations or paid supervision.

Please note that this list is offered as a service to all who teach CBT to the medical community and is not exhaustive.

To Submit Your Name for Inclusion in the Medical Educator Directory

If you meet the above inclusion criteria and wish to be included on this list, please send the contact information that you would like included, along with a few sentences describing your experience with training physicians and/or allied health providers in CBT to Barbara Kamholz at barbara.kamholz2@va.gov and include "Medical Educator Directory" in the subject line.

Disclaimer

Time and availability to participate in such efforts may vary widely among the educators listed. It is up to the individuals seeking guidance to pick who they wish to contact and to evaluate the quality of the advice/guidance they receive. ABCT has not evaluated the quality of potential teaching materials and inclusion on this list does not imply endorsement by ABCT of any particular training program or professional. The individuals in this listing serve strictly in a volunteer capacity.

ABCT's
Medical
Educator
Directory

<http://www.abct.org>

[Resources for Professionals](#)

[Teaching Resources](#)

[CBT Medical Educator Directory](#)

● *complex cases* } **Clinical**
 ● *master clinicians* } **Grand**
 ● *live sessions* } **Rounds**

- Steven C. Hayes, Acceptance and Commitment Therapy
- Ray DiGiuseppe, Redirecting Anger Toward Self-Change
- Art Freeman, Personality Disorder
- Howard Kassinove & Raymond Tafrate, Preparation, Change, and Forgiveness Strategies for Treating Angry Clients
- Jonathan Grayson, Using Scripts to Enhance Exposure in OCD
- Mark G. Williams, Mindfulness-Based Cognitive Therapy and the Prevention of Depression
- Donald Baucom, Cognitive Behavioral Couples Therapy and the Role of the Individual
- Patricia Resick, Cognitive Processing Therapy for PTSD and Associated Depression
- Edna B. Foa, Imaginal Exposure
- Frank Dattilio, Cognitive Behavior Therapy With a Couple
- Christopher Fairburn, Cognitive Behavior Therapy for Eating Disorders
- Lars-Goran Öst, One-Session Treatment of a Patient With Specific Phobias
- E. Thomas Dowd, Cognitive Hypnotherapy in Anxiety Management
- Judith Beck, Cognitive Therapy for Depression and Suicidal Ideation

3-SESSION SERIES

- DOING PSYCHOTHERAPY: Different Approaches to Comorbid Systems of Anxiety and Depression

(Available as individual DVDs or the complete set)

- Session 1 Using Cognitive Behavioral Case Formulation in Treating a Client With Anxiety and Depression (**Jacqueline B. Persons**)
- Session 2 Using an Integrated Psychotherapy Approach When Treating a Client With Anxiety and Depression (**Marvin Goldfried**)
- Session 3 Comparing Treatment Approaches (moderated by Joanne Davila and panelists Bonnie Conklin, Marvin Goldfried, Robert Kohlenberg, and Jacqueline Persons)

TO ORDER

OR, ORDER ONLINE AT www.abct.org | click on **ABCT STORE**

Individual DVDs—\$55 each • “Doing Psychotherapy”: Individual sessions — \$55 / set of three—\$200

shipping & handling

U.S./Canada/Mexico 1–3 videos: \$5.00 per video
4 or more videos: \$20.00

Other countries 1 video: \$10.00
2 or more videos: \$20.00

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Deepen your understanding

the Behavior Therapist

Association for Behavioral
and Cognitive Therapies

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This may be your last issue of *tBT*.

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