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Why Many Clinical Psychologists Are Resistant to Evidence-Based Practice:

Root Causes and Constructive Remedies

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## Abstract

Psychotherapists are taught that when a client expresses resistance repeatedly, they must understand and address its underlying sources. Yet proponents of evidence-based practice (EBP) have routinely ignored the root causes of many clinical psychologists' reservations concerning the use of scientific evidence to inform clinical practice. As a consequence, much of the resistance to EBP persists, potentially widening the already large scientist-practitioner gap. Following a review of survey data on psychologists' attitudes toward EBP, we examine six sources underpinning resistance toward EBP in clinical psychology and allied domains: (a) naïve realism, which can lead clinicians to conclude erroneously that client change is due to an intervention itself rather than to a host of competing explanations, (b) deep-seated misconceptions regarding human nature (e.g., mistaken beliefs regarding the causal primacy of early experiences) that can hinder the adoption of evidence-based treatments, (c) statistical misunderstandings regarding the application of group probabilities to individuals, (d) erroneous apportioning of the burden of proof on skeptics rather than proponents of untested therapies, (e) widespread mischaracterizations of what EBP entails, and (f) pragmatic, educational, and attitudinal obstacles, such as the discomfort of many practitioners with evaluating the increasingly technical psychotherapy outcome literature. We propose educational proposals for articulating the importance of EBP to the forthcoming generation of clinical scientists and constructive remedies for addressing clinical psychologists' objections to EBP.

## Why Many Clinical Psychologists Are Resistant to Evidence-Based Practice:

### Root Causes and Constructive Remedies

As Charles Dudley Warren, and later Mark Twain, quipped, “Everyone complains about the weather, but nobody does anything about it” (Platt, 1989, p. 22). Much the same can be said about the modal attitude of clinical psychologists toward the negative attitudes of many of their colleagues toward evidence-based practice (EBP), especially the component of EBP requiring clinical decision-making to be anchored in rigorous scientific evidence (Gambrill, 1999; Spring, 2007).

It is safe to say that most academic clinical psychologists are aware that a sizeable proportion of their practitioner and research colleagues, not to mention their graduate students, are skeptical of EBP’s insistence that research data inform clinical decisions. Indeed, as we will discover (see “Psychologists’ Attitudes Toward Evidence-Based Practice: Survey Data”), these perceptions are rooted at least partly in reality, as survey evidence suggests that doubts about EBP among clinical psychologists are hardly rare (Baker, McFall, & Shoham, 2009). Yet most advocates of EBP prefer to ignore the negative attitudes of many of their colleagues and students toward EBP, or to dismiss these attitudes as reflections of ignorance or anti-intellectualism. Still others view the resistance to EBP dichotomously, perceiving psychologists as either “for” or “against” EBP when in fact much of this resistance reflects discomfort with only certain aspects of scientifically-based approaches to clinical decision-making.

The field of clinical psychology’s widespread neglect of resistance to EBP is potentially dangerous, as such resistance may inadvertently fuel the continued popularity of unscientific or even pseudoscientific interventions (see Lilienfeld, Lynn, & Lohr, 2003; Thyer & Pignotti, in

press). Specifically, practitioners who do not recognize the underlying reasons for EBP may fail to appreciate how readily they can be fooled by ineffective or harmful treatments. In addition, the neglect of psychologists' resistance to EBP may hamper the effectiveness of ongoing efforts to disseminate evidence-based therapies to practitioners (see Herschell, McNeil, & McNeil, 2004; Siev et al., 2009, for discussions of barriers to dissemination).

In this manuscript, we (1) examine the principal sources of resistance to EBP among clinical psychologists and allied mental health professionals (e.g., social workers, counseling psychologists, counselors, psychiatrists, psychiatric nurses), (2) outline the essential role of EBP in clinical education, training, and practice, and (3) propose constructive remedies for addressing resistance to EBP. We argue that this resistance typically reflects neither ignorance nor anti-intellectualism, although some of it is rooted in misunderstandings about (a) human nature and (b) what EBP does and does not entail.

We further contend that most resistance to EBP stems from several largely unarticulated sources that are routinely ignored in graduate education, six of which we focus on here. To our knowledge, no article has attempted to examine the principal psychological and educational reasons underpinning resistance to EBP, or to propose potential strategies for addressing such resistances among current and future practitioners (but see Gibbs & Gambrill, 2002, for counterarguments against widespread objections to EBP). We contend that each source of resistance affords psychology educators an opportunity to proactively address one or more widespread misconceptions regarding the role of scientific evidence when evaluating the efficacy of psychological treatments. Before delineating the key sources of resistance to EBP, we (a) define EBP and (b) summarize survey data on clinicians' attitudes toward EBP.

### What is Evidence-Based Practice?

The movement toward EBP traces its roots to medicine (Sackett, Rosenberg, Gray, Haynes, & Richardson, 1996; Straus et al., 2010). EBP began to gather momentum in the early and mid-1990s, when a growing cadre of physicians argued that medical practices needed to become more firmly grounded in scientific evidence. Over the past decade, EBP has gained increasing traction in clinical psychology, social work, and allied disciplines (American Psychological Association Presidential Task Force on Evidence-based Practice, 2006; Kazdin, 2008).

#### *EBP versus ESTs*

In contrast to the movement to establish lists of empirically supported therapies (ESTs), which focuses on specific therapeutic techniques, EBP is an *approach* to clinical decision-making. The movement toward EBP, in contrast to the movement to develop ESTs, emphasizes the scientific evaluation of evidence. Moreover, as several authors (e.g., Spring, 2007; Westen, 2005) have observed, ESTs are merely one potential operationalization of the research component of EBP. Indeed, at least some of the resistance to EBP probably reflects a failure to distinguish ESTs from EBP (Thyer & Pignotti, 2011), as many clinical psychologists who harbor reservations regarding the former may reflexively reject the latter. The confusion between EBP and ESTs is by no means rare; in a survey of 1195 clinical psychology graduate students, Luebbe, Radcliffe, Callands, Green, and Thorne (2007) found that 18% referred only to ESTs when asked to describe the research evidence relevant to EBP.

Although we regard ESTs as a helpful step in the direction of reducing error in clinical inferences (Chambless & Ollendick, 2001), they are not immune to thoughtful criticism (e.g.,

Herbert, 2000; Rosen & Davison, 20003; Westen, Novotny, & Thompson, Brenner, 2004; but see Weisz, Weersing, & Henggeler, 2005). Setting these contentious issues aside, we do not intend to revisit, let alone resolve, ongoing debates regarding the merits or demerits of current operationalizations of ESTs. Our focus is squarely on EBPs, not ESTs, and even clinical psychologists and others who oppose the current criteria and lists of ESTs can embrace EBP.

### *The Three Legs of the EBP Stool*

EBP is traditionally defined in terms of a “three legged stool” (Spring, 2007). The first leg consists of the best available research evidence bearing on whether and why a treatment works. In this respect, ESTs may sometimes inform EBP, although they are by no means equivalent to it. This leg is often conceptualized in terms of a hierarchy of evidence, with data from meta-analyses, randomized controlled trials (RCTs), and systematic within-subject designs at the apex, well conducted quasi-experimental studies in the middle, and correlational and uncontrolled case studies at the bottom (Ghaemi, 2009; Thyer & Pignotti, 2011). Data on the upper rungs of this hierarchy are, all else being equal, more trustworthy than data on the lower rungs, as they minimize more sources of error in clinical inferences. Specifically, they help us to rule out more variables that can lead observers to conclude erroneously that treatments are working when they are not (see “Causes of Spurious Therapeutic Effectiveness”).

This leg of the stool comprises a variety of sources of scientific evidence, including (a) data on therapeutic *efficacy*, which examine how well a therapy works in rigorously designed studies performed in research settings, (b) data on therapeutic *effectiveness*, which examine how well a therapy works as it is conducted in the rough-and-tumble world of actual clinical settings (see Seligman, 1995, on the efficacy versus effectiveness distinction), and (c) scientific

research on basic psychological processes (e.g., memory, problem-solving, emotion, implicit cognition, schemas, heuristics and biases, personality traits) relevant to psychotherapy (e.g., Sechrest & Smith, 1994).

This first leg – research evidence - is almost certainly the component of EBP that engenders the most resistance among clinical psychologists, some of whom are skeptical of the relevance of scientific data to evaluating the often subjective criteria of psychotherapy outcome and process. Hence, in this article we focus principally on resistance to this leg of the EBP stool.

The second leg of the EBP stool comprises clinical expertise, which can itself be decomposed into clinical judgment and clinical experience. In this component of EBP, practitioners make use of “their clinical skills and past experiences to rapidly identify each patient’s unique health state and diagnosis, [and] their (sic) individual risks and benefits of potential interventions” (Straus et al., 2010). Although lively scientific debate continues regarding the value of clinical experience in informing valid practitioner judgments (Garb, 1998; Kahneman & Klein, 2009), psychotherapy necessarily involves the incorporation of clinical expertise with scientific evidence, as data simply are not available to dictate every decision within a psychotherapy session. As Meehl (1957) observed over a half century ago, “mostly we will use our heads” when making clinical decisions, “because there just isn’t any formula” (p. 405). Still, many of these clinical decisions can be informed broadly by scientific data. For example, even when explicit data are not available to instruct a therapist how to respond to a client who is reluctant to engage in homework assignments as part of a cognitive-behavioral therapy protocol, data on the relevance of the therapeutic alliance and rapport to successful



treatment outcomes may guide the therapist in fostering more trust with the client to overcome this reluctance (Addis, 2000).

The third leg of the EBP stool consists of client preferences and values (Spring, 2007), which may often shape or even dictate clinicians' selection of interventions. For example, even when research evidence strongly supports the use of flooding (prolonged exposure to high intensity stimuli) for an anxiety disorder, a client may be unwilling to endure the overwhelming short-term fear necessitated by this intervention. In this case, the therapist may elect to administer a less efficient but still scientifically supported intervention, such as graded exposure, *in lieu* of flooding (e.g., see Rothbaum, Hodges, Opdyke, Williford, & North, 1995).

In sum, EBP comprises the thoughtful integration of the best available scientific evidence concerning psychotherapy with clinical expertise and client preferences/values. Some authors contend that all three legs of the stool should be accorded equal priority in clinical decision making (Levant, 2004; see also Thyer & Pignotti, 2011). In contrast, we side with others (e.g., American Psychological Association Presidential Task Force on Evidence-Based Practice, 2006) who maintain that scientific evidence must be accorded priority above the other two legs of the stool. We concur with Grove and Meehl (1996) that clinical experience is indispensable as a rich source of clinical hypotheses to be tested systematically and that "it is also the only way that a practitioner can acquire certain behavioral skills, such as how to ask questions of the client" (p. 26). Nevertheless, as they observe, "it is not an adequate method for resolving disputes between practitioners, because they each appeal to their own clinical experience" (p. 26; see also Hall, 2011). Hence, clinical expertise, including clinical experience, should not generally be granted equal weight to research evidence when making treatment

decisions. For example, when well replicated evidence from controlled outcome studies points to the use of Therapy X with a client but a clinician's gut hunches instead suggest the use of Therapy Y, the clinician should override the recommendation derived from research only when there is clear-cut warrant to do so (e.g., when there is unambiguous evidence that the client has repeatedly failed to respond to Therapy X even when it has been properly delivered; see Meehl, 1957, for a discussion of "broken leg" cases in the domain of clinical assessment).

#### Resistance in Psychotherapy: An Analogy

In their training, psychotherapists are routinely taught that client resistances, especially when expressed repeatedly, should not be ignored, let alone dismissed (Shea, 1998). They are further taught that if therapists do not address these resistances explicitly, they are likely to recur in various guises, potentially impeding the effectiveness of treatment.

#### *Psychologists' and Students' Resistances to EBP*

Similarly, we argue, the field of clinical psychology ignores practitioners' resistances to evidence-based practice at its peril, as such benign neglect may widen the already large gap between scientist and practitioner (see Fox, 2000; Tavis, 2003, for discussions of the scientist-practitioner gap). Moreover, to our knowledge, few educators in graduate programs in clinical psychology and allied fields attempt to address the underlying sources of skepticism toward EBP among their students. More broadly, these instructors frequently neglect to emphasize why scientific evidence is indispensable when evaluating the efficacy and effectiveness of psychotherapies, and why clinical intuition – although potentially valuable for generating fruitful clinical hypotheses – is ill-suited to judging the efficacy of interventions (Grove & Meehl,

1996; see also Kahneman, 2011 and Myers, 2003, for discussions of the limits of intuition in clinical inference).

As Shea (1998) observed with respect to clinical interviewing, many client resistances can be viewed as clinicians' "friends" of sorts in that they provide interviewers with insight into erroneous beliefs (e.g., "Because my interviewer is single, I'll bet she can't understand the problems I'm experiencing in my marriage") that can impede the information-gathering process. By making latent ("seed") resistances manifest, Shea argued, clinicians can address these roadblocks explicitly and thereby facilitate progress in the interview. We propose that practitioners' resistances to EBP can similarly be conceptualized as "friends" to the proponents of EBP, as such resistances can allow these proponents to understand why many thoughtful and well-educated psychologists are reluctant to embrace a scientific approach to clinical decision-making. In some respects, our approach to psychologists' resistance to EBP parallels that of some schools of psychotherapy, including dialectical behavior therapy (Linehan, 1993), in which accepting and validating client resistances is an essential component of treatment. Specifically, we advocate for acknowledging resistances to EBP while confronting them tactfully but firmly with potent scientific and logical counterarguments.

#### *Why Resistance to EBP is Understandable*

By its very nature, science constrains inferences; hence, resistance to EBP on the part of many clinical psychologists is understandable. In effect, science tells us that some beliefs are closer approximations to the truth than others (McFall & Treat, 1999). Similarly, the research leg of the EBP stool reminds practitioners of an inconvenient truth: Certain psychotherapies are better supported scientifically than others. In doing so, it renounces the ecumenical view that

all clinical practices are created equal. From a scientific and ethical standpoint, EBP therefore mandates that in some cases, clinicians should abandon or at least modify their longstanding practices in favor of others. Hence, it is small wonder that efforts to disseminate EBP are frequently met with stubborn resistance.

Furthermore, resistance to EBP may be especially marked for practitioners who (a) were trained in graduate programs that do not value EBP or (b) came of age in the pre-EBP era. Data suggest that older practitioners tend to harbor significantly more negative attitudes toward evidence-based interventions than do younger practitioners (Aarons & Sawitsky, 2006), perhaps in part because the former are more wedded to their interventions of choice. In addition, the former may be less accustomed to the heightened demands for accountability in mental health practice (Johnson, 1995). As Anderson and Stewart (1983) observed with respect to resistances in psychotherapy, negative attitudes to therapeutic change are natural, because change in deeply entrenched behaviors is often painful:

Unless people are immediately persuaded by overwhelming evidence that a change in their behavior is necessary or beneficial, such as responding to a fire by exiting from a building, they will resist change in the status quo. Business executives seeking to introduce new marketing techniques, doctors seeking to heal their patients, parents seeking to teach their children manners, all who seek to bring about change experience resistance to their efforts (p. 1).

Proponents of EBP must recognize that they too are seeking “to bring about change,” as they are striving to alter longstanding habits of mind, as well as deeply entrenched clinical practices, in their colleagues and students. For example, clinicians who have been using

psychoanalytic therapy for decades to treat obsessive-compulsive disorder (OCD) may be understandably reluctant to embrace research evidence that exposure and response prevention (EXRP; also referred to as exposure and ritual prevention) is the empirically demonstrated intervention of choice for this condition (Fisher & Wells, 2005). Merely informing these clinicians that “scientific evidence supports the use of EXRP for OCD” is unlikely to persuade them to alter their therapeutic practices (see also Lilienfeld, Wood, & Garb, 2007, for a discussion of “inertia” in the use of clinical assessment practices).

There are at least two reasons why such corrective information will often be insufficient to alter therapists’ longstanding choice of interventions. First, as we will discuss later, many of these clinicians may conclude that the informal “evidence” of their own clinical experience should be accorded higher priority than research evidence derived from controlled trials. Indeed, one of our core arguments is that much of the resistance toward EBP stems less from an unwillingness to examine evidence as much as a fundamentally different conception of what constitutes “evidence” to begin with (see Lilienfeld, 2010; McHugh, 2004). Second, when confronted with evidence that conflicts with their views, some clinicians may evoke the “scientific impotence excuse” (Munro, 2010). The scientific impotence excuse, a phenomenon derived from cognitive dissonance theory (Festinger & Carlsmith, 1959; Harmon-Jones & Mills, 1999) and other cognitive consistency models of attitude change, is the tendency to conclude that when scientific findings contradict our deeply held intuitions, the science that generated these findings must be flawed. Research demonstrates that when students are confronted with research evidence that challenges their entrenched beliefs, such as the disease status of homosexuality (Munro, 2010), they frequently conclude that scientific methods are simply not

up to the task of investigating these beliefs (Lilienfeld, in press). This excuse may similarly be employed by practitioners whose favored treatments have been called into question by research evidence. Again, if educators do not articulate the necessity of scientific methods in graduate training, such reactions to disconfirming evidence are understandable, and perhaps even inevitable.

#### Mental Health Professionals' Attitudes Toward Evidence-Based Practice: Survey Data

A modest but burgeoning body of survey data offers valuable insight into mental health professionals' attitudes toward EBP and more broadly, the inclusion of scientific evidence in treatment selection. These findings are valuable, as they afford us a panoramic view of the landscape of resistances that advocates of EBP confront. On balance, these data yield a mixed picture, but suggest that many practitioners view EBP with at least some degree of suspicion. Like Safran, Abrue, Ogilvie, and DeMaria (2011; see below), we are inclined to conclude that "depending on how one looks at the findings, one can see the glass as either half empty or half full" (p. 369).

#### *Positive Attitudes Toward EBP and Scientific Research*

We begin with the glass-is-half-full (or at least partly full) side. Survey data indicate that many or most mental health professionals hold reasonably positive views of EBP and more generally, of the utility of research in informing clinical practice. In a study of 59 therapists, Borntrager, Chorpita, Higa-McMillan, and Weisz (2009) found that respondents were fairly positive toward EBP as gauged by their scores on the Modified Practice Attitudes Scale (Chorpita et al., 2004), an 8-item self-report measure of positive attitudes toward EBP (e.g., "I am willing to use new and different types of treatments if they have evidence of being

effective”, p. 678) whose item anchors range from 0 (I agree not at all) to 4 (I agree to a great extent). Specifically, the mean scores per item (depending on assignment to two experimental conditions that need not concern us here) fell between 2.51 to 2.76 (see also Pignotti, 2009, for evidence that most social workers hold moderately positive views toward EBP). Even these numbers, however, suggest at least some nontrivial reluctance to embrace EBP. In addition, participants were much less favorable to EBP when asked about it in context of psychotherapy guided by treatment manuals.

Other data suggest that most clinicians perceive research as relevant to their practice. Sheldon and Chilvers (2002) found that of 1126 British social service workers, 90% saw research as pertinent to their therapeutic decisions. Similarly, in a survey of 85 recently graduated British therapists, Caldwell, Coleman, Copp, Bell, and Ghazi (2007) reported that 96% perceived research as “fairly” or “very” relevant to their clinical practice.

#### *Ambivalence Toward EBP and Scientific Research on Psychotherapy*

We now turn to the glass-is-half-empty side. Despite the fact that most practitioners view research as relevant to their clinical work, they generally perceive it is as less relevant than a host of other information sources. In a study of 30 child clinicians, Cohen, Sargent, and Sechrest (1986) found that respondents rated the usefulness of research articles (mean of 3.57 on a 1 to 7 point scale) lower than a number of other resources, including how-to books on clinical practice (4.41), theoretical books (5.07), workshops (5.31), and informal discussions with colleagues (6.67). That same year, Morrow -Bradley and Elliott (1986) found that 31% of members/fellows of APA Division 29 (Psychotherapy) responded “not at all” or “minimally” when asked to rate “the extent which research has had an impact on your practice” (only 27%

responded “A great deal” or “Quite a bit”). When offered the choice among nine information sources, only 4% rated psychotherapy research as the “most useful” form of information for their practice, compared, for example, with 48% for “ongoing experiences with clients,” 10% for theoretical or how-to books or articles, and 7% for workshops or conferences not based on research.

More recent data suggest broadly comparable trends. In a study of 508 members of APA Division 12 (Society of Clinical Psychology), Stewart and Chambless (2007) reported that respondents expressed only moderate agreement (mean of 3.09 on a 1-7 scale ranging from 1=Strongly Agree to 7=Strongly Disagree) with the proposition that controlled research on psychotherapy is pertinent to their practice. They rated “current research on treatment outcome” as somewhat influential in their treatment decisions (2.86 on the same scale), but less influential than past clinical experiences (1.53) or colleagues’ advice (2.70). von Ranson and Robinson (2006) reported that of 52 Canadian therapists specializing in the treatment of eating disorders, 39% listed research as a reason for their selection of treatments. Yet 60% and 39% listed clinical experience and compatibility with their theoretical orientation, respectively, as grounds for their treatment choices (see Riley et al., 2007, for similar findings). In a survey of 181 members of APA Division 42 (Psychologists in Private Practice), Boisvert and Faust (2006) found that participants expressed moderate agreement (5.05 on a 7 point scale) with the assertion that “Most therapists learn more about effective therapeutic techniques from their experience than from the research” (p. 712). In contrast, research-oriented psychotherapists may hold more favorable views toward research, although even these attitudes are far from ubiquitous. Safran et al. (2011) found that of 123 members of the Society for Psychotherapy



Integration (SEPI), an organization consisting largely of academic clinical psychologists, 9% “Strongly disagreed” or “Disagreed” (with 7% being neutral) that “Research has had an important impact on my practice” (p. 362).

In a survey of 400 licensed clinical social workers, Pignotti (2009) asked practitioners to rate various reasons for selecting treatments on a 1-7 scale. The most highly rated were “Clinical experience with positive results that held up over time” (M=6.50, SD=.88), “Compatibility with your theoretical orientation” (M=5.65, SD=1.38); “Compatibility with your personality” (M=5.63, SD=1.38), “Clinical experience of fast, positive results with clients” (M=5.45, SD=1.56), “Intervention emotionally resonated for you” (M=5.20, SD=1.66); “Endorsement by respected professional” (M=5.01, SD=1.41); “Your intuition” (M=4.95, SD=1.64), and “Colleagues’ reports of success” (M=4.84, SD=1.45). Rated lower, although still above the midpoint on the scale, was “Favorable research in peer reviewed journals” (M=4.74, SD=1.54).

Still other data suggest that many graduate students are less than enthusiastic regarding the role of EBP in their education and clinical training. In a study of clinical psychology graduate students described earlier, Luebbe et al. (2007) found that respondents were on average noncommittal when asked whether they wanted EBP to be more integrated into their coursework (mean of 3.13 on a 1-5 scale) and practicum work (3.37). Students were slightly more positive when asked whether they “agree with (the) general principles” behind EBP (3.90).

*Summary*

At the risk of painting with an overly broad brush, we can conclude most survey data reveal that when asked globally about their attitudes toward EBP and the value of scientific research, most practicing psychologists are reasonably positive. These findings offer some grounds for cautious optimism in disseminating EBP to clinicians. Yet the evidence also suggests at least some ambivalence toward EBP, even among clinical psychology graduate students. Moreover, most therapists rank scientific research lower, in some cases considerably lower, than other sources of evidence, such as clinical experience, intuition, and informal views of colleagues, in informing their treatment choices.

It is worth noting that the decidedly mixed attitude of practitioners toward the utility of research for their clinical practice does not necessarily betray a global antipathy toward science or scientific evidence *per se*. Some of this ambivalence may reflect the fact that the lion's share of published psychotherapy outcome research has not been communicated to practicing therapists in a format that they can readily digest, interpret, and understand (see also Cohen et al., 1986; Morrow-Bradley & Elliott, 1986). We return to this point in a later section (see "Pragmatic, educational, and attitudinal obstacles").

#### First Source of Resistance: Naïve Realism

In the bulk of the remainder of the manuscript, we delineate six major sources of resistance to EBP among psychologists and students. The first major source of resistance we address is what psychologists, following philosophers, have termed naïve realism (Ross & Ward, 1996). Naïve realism, also called common sense realism or direct realism, is the erroneous belief that the external world is exactly as we see it. This belief is deeply embedded in our intuitions. A host of phrases in everyday life attest to the power of naïve realism in our thinking:

“Seeing is believing,” “I saw it with my own eyes,” “I’ll believe it when I see it,” and “What you see is what you get.”

To a substantial extent, a preference for naïve realism over controlled research evidence reflects a prioritizing of unguided clinical intuition over systematic research. This predilection for intuition bears potentially important implications for attitudes toward EBP: In a study of 176 psychotherapists of diverse backgrounds, Gaudiano, Brown and Miller (2011) found that an intuitive thinking style was associated with more negative attitudes toward EBP.

Kahneman (2011) referred to a core principle of intuition as “WYSIATI”: What You See Is All There Is,” an assumption that dovetails closely with naïve realism. As he noted, this heuristic (mental shortcut) can result in efficient decisions, but it can also engender predictable errors, as it can lead us to ignore pertinent background evidence. The WYSIATI principle can also predispose mental health professionals to assume erroneously that the evidence of their own perceptions is as valuable, or more valuable, than evidence derived from controlled studies.

Naïve realism is misguided for one key reason: The world is not precisely as we perceive it. Instead, what we see is in part constrained by reality, along with our preconceptions, biases, and interpretations (“apperceptions”; Morgan & Murray, 1935). To a substantial extent, “believing is seeing” at least as much as the converse (Gilovich, 1991; Segall, Campbell, & Herskovitz, 1996).

#### *Naïve Realism and Erroneous Inferences of Change in Psychotherapy*

Because of naïve realism, practitioners, trainees, and others may assume that they can rely exclusively on their intuitive judgments (“I saw the change with my own eyes”) to infer that an intervention was effective (Ghaemi, 2009; Lilienfeld, Lohr, & Olatunji, 2008). As a

consequence, they may misperceive change when it does not occur, or misinterpret it when it does.

One example derives from Arnold Shapiro, who produced the Academy-Award winning documentary *Scared Straight!* In 1978 and who recently responded to scientific criticisms of *Scared Straight* interventions for adolescents at elevated risk for crime. These interventions attempt to frighten these adolescents out of criminal careers by bringing them to prisons and introducing them to inmates. Shapiro defended *Scared Straight* programs by insisting that “I’m seeing it [the change following *Scared Straight* programs] with my own eyes, I’m there for every one of those shoots” (Harrison, 2011, p. 2). Ironically, data from controlled studies and meta-analyses suggest that *Scared Straight* interventions are not merely ineffective, but harmful, in that they produce a heightened risk for antisocial behavior (Petrosino, Turpin-Petrosino, & Buehler, 2005). In another example, Healy (2002) wrote in an article, ironically entitled “Evidence-based psychiatry,” that “When treatments work, the condition being treated vanishes, and we don’t need randomized controlled trials to see this happening” (p. 1). Yet in fact, the condition being treated may vanish for a plethora of reasons other than, or in addition to, the intervention. Contrary to Healy’s implication, RCTs and similar controlled research designs are needed to exclude rival hypotheses for the observed change (see “EBP as a Necessary Antidote to Ruling out Causes of Spurious Therapeutic Effectiveness”).

In relying on “their own eyes” to judge therapeutic efficacy, practitioners may similarly assume that controlled outcome studies on psychotherapies are unnecessary to ascertain whether a treatment is efficacious. A corollary of naïve realism is the failure to appreciate the manifold sources of change during psychotherapy other than the specific ingredients of the

treatment itself. Putting it somewhat differently, it is easy to forget that change *following* therapy is not equivalent to change *because of* therapy, a logical error known as the *post hoc, ergo propter hoc* (after this, therefore because of this) fallacy (Finocchiaro, 1981). In some cases, of course, client change in treatment may indeed be due to the intervention, so therapists' inferences regarding therapeutic effectiveness are by no means always inaccurate. Yet without controlled within-subject research designs, there is no way to verify that an inference was correct in any individual case.

#### *Naïve Realism and Errors in the History of Medicine*

The history of medicine, including psychiatry, offers a powerful cautionary tale concerning the dangers of overreliance on naïve realism. Most historians of medicine concur that prior to about 1890, the history of medical treatments was largely the history of the placebo effect (see "Placebo effects"). Along with ineffective medications, bleeding, blistering, purging, and leeching were routinely prescribed and presumed to be effective based on scant more than informal clinical observations (Grove & Meehl, 1996).

Similarly, insulin coma therapy was introduced as a psychiatric intervention by Manfred Sakel in 1933, and was frequently used to treat schizophrenia during the 1930s and 1940s. This time- and labor-intensive procedure (depicted in the Academy Award-winning film "A Beautiful Mind") involved administering increasingly high doses of insulin to induce a hypoglycemic state, followed by a coma and sometimes convulsions. The coma persisted for a few days before being terminated by the introduction of sufficient levels of glucose, orally, intravenously, or intramuscularly (James, 1992). This process was repeated for several weeks at a time. Early clinical reports described encouraging results. Its troublingly high morbidity and mortality rates

notwithstanding, insulin therapy spread rapidly throughout Europe, the United States, Japan, and Australia, reflecting “a wave of unjustified enthusiasm” (James, 1992, p. 222). This wave was unceremoniously interrupted by a critical article in the *Lancet* by Bourne (1953), who concluded there was no evidence that insulin coma therapy was efficacious (Jones, 2000). As Jones (2000) described, many psychiatrists of the time published rebuttals to Bourne’s article: “Their tone was typified by remarks such as ‘it is clinical experience that counts here, despite all figures to the contrary’” (p. 148). By the late 1950s, however, insulin coma therapy was regarded as a prohibitively hazardous placebo treatment, and was soon abandoned by most hospitals (Shapiro & Shapiro, 1997).

Prefrontal lobotomy, which gave its developer, Portuguese neurosurgeon Egas Moniz, the Nobel Prize in Medicine or Physiology in 1949, offers another disturbing example of how naïve realism can dupe experienced observers. Dawes (1994) offered the example of a prominent practitioner of prefrontal lobotomy who insisted that “I am a sensitive observer, and my conclusion is that a vast majority of my patients get better as opposed to worse after my treatment” (p. 48). Yet later research revealed lobotomy to be worthless for schizophrenia, depression, and other psychological conditions, and to be associated with a host of disastrous psychological and neurological side effects (Diefenbach, Diefenbach, Baumeister, & West, 1999; Valenstein, 1986). How could so many thoughtful and intelligent observers have been so badly fooled?

#### *Causes of Spurious Therapeutic Effectiveness*

These observers were fooled because they neglected to account for a plethora of rival explanations for change during and after the treatment. We refer to the multiple ways in which

people can be fooled into believing that a treatment is working even when it is not as causes of spurious therapeutic effectiveness (CSTEs). CSTEs can make ineffective or even harmful interventions appear effective to therapists and other observers and, in many cases, clients themselves. Yet because they lie in the “causal background” rather than the foreground, CSTEs are likely to be unappreciated or ignored relative to the much more perceptually salient causal influences of psychotherapy.

Here we provide brief descriptions of 16 CSTEs that can deceive highly intelligent and well-educated people into concluding that ineffective or even harmful psychotherapies are effective (see also Beyerstein, 1997, Hall, 2011, and Hartman, 2009, for discussions of why bogus medical treatments can appear to work). CSTEs fall into two major categories. First, some CSTEs, which we term Type 1 CSTEs, lead individuals to misattribute actual client change stemming from extraneous causes to the active treatment *per se*. In these CSTEs, clients are changing, but not due to specific ingredients of the therapy itself. Second, other CSTEs, which we term Type 2 CSTEs, lead individuals to misperceive change in its absence. In these CSTEs, clients are not in fact changing, although individuals misperceive them to be changing.

Knowledge of these CSTEs, we maintain, should be a mandatory component of the education and training of all clinical psychologists and other mental health professionals. We summarize these CSTEs, separated by Type 1 and Type 2 CSTEs, below (see also Table 1).

### **Type 1 CSTEs (Misinterpretations of actual client change)**

(1) *Placebo effects*. The placebo effect is improvement resulting from the mere expectation of improvement (Novella, 2010; Shapiro & Shapiro, 1997; Steer & Ritschel, 2010). Placebo effects can be impressive. For example, some research suggests that up to 80 percent of

the effects of antidepressants are attributable to placebo effects (Kirsch, 2009; Kirsch & Sapirstein, 1998). Placebos generally exert their most potent effects on subjective reports, such as depression, and pain, rather than on objective indices, such as assays of cancer, heart disease, or other organic illnesses (Hrobjartsson & Gotzsche, 2001). By instilling hope and the conviction that one can rise above life's challenges, virtually any credible treatment can be at least somewhat helpful for combatting demoralization (Frank & Frank, 1961), which is central component of many psychological disorders (Tellegen et al., 2003).

(2) *Novelty effects*. Relatedly, clients may improve at the outset of treatment merely because they are excited by the prospects of receiving a promising intervention that is new to them (Fraenkel & Wallen, 1993). Psychotherapy outcome data suggest that about 15% of patients improve between the initial phone call and the first session (Howard, Kopta, Krause, & Orlinsky, 1986). At least some of this improvement probably reflects the installation of hope in anticipation of a novel treatment.

(3) *Spontaneous remission*. Spontaneous remission is a term that originated in medicine to describe cases in which diseases improve or resolve on their own (Beyerstein, 1997). The rates of spontaneous remission in psychotherapy are not trivial, with estimates ranging anywhere from 15% to upwards of 70% of patients (Chadwell & Howell, 1979; Eysenck, 1952; Jacobson & Christianson, 1996; McCullough, 2000). The longer people remain in therapy, the greater the opportunity for extra-therapeutic factors, including natural healing processes, to generate improvement (Jacobson & Christianson, 1996). These factors may include social support, exposure to anxiety-related cues, positive transformative experiences in everyday life (e.g., embarking on a new and rewarding



relationship), and problem solving and effective coping with life stressors. It is understandable that clients and therapists alike can mistakenly attribute changes to therapy that actually are the products of naturally occurring improvement.

- (4) *Cyclical nature of many disorders.* Another extra-therapeutic factor that may be associated with short-term improvement is the cyclical nature of many disorders (Beyerstein, 1997). Like such medical conditions as multiple sclerosis, arthritis, and gastrointestinal problems, many psychological disorders have their inevitable “ups and downs.” In disorders that are self-limiting or cyclical, people often improve, periodically, or over the long term, without any intervention. In cases of bipolar or cyclothymic disorders, for example, an ineffective treatment implemented over a relatively long period will have ample opportunities to coincide with upticks that would have occurred regardless of treatment. Accordingly, clinicians may mistakenly infer that therapy is responsible for improvement when positive changes are generated by fluctuations in the natural course of the disorder.
- (5) *Regression to the mean.* It is a statistical fact of life that extreme scores tend to become less extreme upon re-testing, a phenomenon known as regression toward the mean (Kruger, Savitsky, & Gilovich, 1999). “What goes up must come down,” as the old saw reminds us. If a patient presents to therapy severely depressed, chances are high that he or she will be less depressed in a few weeks. Regression to the mean can fool therapists and patients alike into believing that a useless treatment is effective. It is an especially thorny problem in evaluating whether psychotherapy is effective in real-world settings,

because most patients enter psychotherapy when their symptoms are most extreme, and hence when regression effects are maximized (Gilovich, 1991).

- (6) *Maturation*. Another source of erroneous inferences of therapeutic efficacy, especially for treatments delivered to children and adolescents, is maturation: improvement owing to naturally-occurring psychological growth (Cook & Campbell, 1979). Yet maturation may be a source of mistaken conclusions even among adult clients. For example, many patients with borderline personality disorder appear to improve over long stretches of time even without treatment (Gunderson et al., 2011; Shea et al., 2009), perhaps owing to a gradual solidification of their identity and feelings of self-worth in later life.
- (7) *History*. Still another extra-therapeutic factor that can contribute to the erroneous inference of a therapeutic effect is what Campbell and Stanley (1963) termed history: life events transpiring outside of the treatment setting. A client who is experiencing severe life stressors due to an exceedingly demanding and unsupportive boss may improve when his boss departs for another job. The clinician may erroneously attribute improvement during therapy to the treatment itself rather than to the salubrious changes in the client's work environment.
- (8) *Effort justification*. Because clients often invest a great deal of time, energy, effort, and money in treatment, they may feel a psychological need to justify this commitment, a phenomenon called effort justification (Cooper & Axom, 1982). In a remarkable study, college students with snake phobic symptoms improved equally when receiving flooding therapy and when performing strenuous physical exercises (e.g. running quickly in place),

perhaps because the latter treatment required considerable effort, and hence the need to rationalize this effort (Cooper, 1980; see also Axsom & Cooper, 1985).

(9) *Multiple treatment interference*. When clients elect to seek out a treatment, they often obtain other interventions simultaneously (Kendall, Butcher, & Holmbeck, 1999). Some of these adjunctive interventions may be formal treatments, such as antidepressants or marital therapy. Others may be informal “treatments,” such as exercise, which has generally been found in controlled studies to be effective for alleviating depression (Freemont & Craighead, 1987; Penedo & Dahn, 2005) or confiding in trusted friends or religious figures. Multiple treatment interference often renders it difficult or impossible to conclusively attribute client change to the active ingredients of the intervention of choice.

(10) *Initial misdiagnosis*. Even the best-trained diagnosticians are not infallible (Beyerstein, 1997). For example, relatively normal individuals undergoing temporary life stressors are at times mistakenly diagnosed as psychopathological; when they are later examined, they have improved, but not necessarily because of the treatment. Parents of a rambunctious child misdiagnosed with attention-deficit hyperactivity disorder (ADHD) who consult with a therapist practicing equine (horseback riding) therapy may witness improvement due not to the intervention itself, but to the fact that the child’s hyperactive, impulsive, and inattentive behaviors were reactions to transient stressors that have since subsided.

## **Type 2 CSTE (False Perceptions of Client Change in its Absence)**

- (11) *Illusory placebo effects*. Illusory placebo effects arise when people believe that a psychological attribute, such as panic disorder, or physical condition, such as asthma (Wechsler, Kelley, Boyd, Dutile, Marigowda, Kirsch et al., 2011), improves in the absence of genuine changes on outcome measures (e.g., frequency of panic attacks, lung functioning). In a clever study (Greenwald, Spangenberg, Pratkanis, & Eskenazi, 1991), experimenters switched audiotapes containing subliminal messages so that people who thought they listened to audiotapes designed to enhance memory actually listened to audiotapes designed to enhance self-esteem, and vice-versa. Participants came away believing that their memory or self-esteem, as the case may be, had improved in response to the subliminal tape they *believed* they had heard rather than in response to the subliminal tape they had *actually* heard. In fact, on objective tests of memory and self-esteem, all of the tapes were ineffective.
- (12) *Demand characteristics*. Demand characteristics refer to the tendency of individuals to adjust their behavior in accord with what they believe to be researchers' or therapists' hypotheses (Orne, 1962), including their hypotheses regarding client change. Clients are often motivated to tell their therapists what they believe their therapists want to hear; in addition, they may be motivated to persuade themselves that they have improved. Hathaway (1948) referred to the "hello-goodbye" effect as clients' propensity to present themselves as worse than they actually are at the outset of treatment, and better than they actually are at the conclusion of treatment. As a consequence of this effect, therapists and other observers may conclude that client improvement occurred even in its absence.

Similarly, hypnosis researchers have identified a “holdback effect” when participants are tested sequentially in non-hypnosis and hypnosis conditions. The effect sometimes arises when participants are not hypnotized during an initial baseline trial, but know that they will be hypnotized in the following trial. In such cases, they may deliberately “hold back” from responding or becoming engaged in the procedure to demonstrate gains during hypnosis and thereby present themselves as good hypnotic subjects (Zamansky, Scharf, & Brightbill, 1964).

(13) *Retrospective “rewriting” of pre-treatment functioning.* In some cases, clients may persuade themselves – and others – that they have improved by misremembering their initial level of functioning as worse than it was (Ross, 1989). Such biased memories may stem from clients’ implicit expectations of change during therapy. Conway and Ross (1984) randomly assigned university students to either a study skills course designed to improve their grades or to a no-intervention control condition, and measured their study skills and grades before and after the intervention. The study skills class was apparently useless, as it failed to improve students’ grades. Yet students in the experimental condition perceived the intervention as effective, because they misremembered their initial study skills as worse than they actually were. A similar retrospective rewriting of pre-treatment functioning may sometimes occur during psychotherapy, especially when clients harbor strong expectations of change.

(14) *Absence of knowledge of the effects of the hypothetical counterfactual.* A largely unappreciated reason for erroneous inferences of therapeutic effectiveness is the absence of information regarding the hypothetical counterfactual (Dawes, 1994): that is,

our inability to know what would have occurred had we not intervened. Because clinicians in routine practice settings are necessarily unaware of how their clients would have fared in a control condition, such as a wait-list control group, they cannot gauge the extent to which the improvement they observed might have occurred in the absence of treatment, or in the presence of an alternative treatment.

An illustrative example derives from research on critical incident stress debriefing (CISD), which is widely used to decrease the risk of posttraumatic stress symptoms among trauma-exposed victims. Controlled research demonstrates that CISD is ineffective at best, and harmful at worst (Litz, Gray, Bryant, & Adler, 2003; McNally, Bryant, & Ehlers, 2003). Yet many people who have undergone CISD are convinced that it is effective (Carlier, Goerman, & Gersons, 2000). Why? A study by Mayou, Ehlers, and Hobbs (2000) offers intriguing insights. These investigators evaluated the three-year outcome of 61 patients who had experienced traffic accidents; some had been randomly assigned to receive CISD, and others to receive no intervention. Among other measures, participants completed the Impact of Events Scale (IES; Horowitz, Wilner, & Alvarez, 1979), a well validated measure of posttraumatic stress symptoms. As can be seen in Figure 1, high-scoring IES participants who received CISD indeed improved between the pre-treatment baseline and the three year follow-up. Yet remarkably, high scoring IES participants who received no intervention at all improved even more. These findings suggest that CISD can impede natural healing processes (McNally et al., 2003). They may also help us to understand why so many people are persuaded that CISD is efficacious even though it is not. Specifically, troubled individuals who receive CISD do improve, but not because of

the treatment. To the contrary, they probably would have improved even more had they received no treatment.

- (15) *Selective attrition*. This CSTE differs from the others in that it operates not at the level of the individual client, but at the level of all of the clients in a clinician's caseload. Selective attrition refers to the fact that clients who drop out of therapy are not a random subsample of all clients. Specifically, research demonstrates that clients who are not improving in psychotherapy are especially likely to leave treatment (Garfield, 1994; Lilienfeld, 2007; Tehrani, Krussel, Borg, & Monk-Jorgensen, 1996). As a result, therapists may conclude erroneously that their preferred treatments are effective merely because their remaining clients are those that have improved. One problem that has long bedeviled the evaluation of Alcoholics Anonymous and similar 12-step interventions for substance disorders is the high level of client drop-out from this intervention, often approaching 40% following one year (Kelly & Moos, 2003). The clients who remain in these treatments after several years are generally doing better than when they began, but they are unrepresentative of the clients who initially enrolled in the program. Specifically, those clients who dropped out may not have been helped, or perhaps have even harmed, by the intervention.
- (16) *Confirmation bias*. Confirmation bias is the tendency, to which we all prone, to seek out evidence that is consistent with our hypotheses, and to deny, dismiss, or distort evidence that is not (Lilienfeld, Ammirati, & Landfield, 2009; Nickerson, 1998). Although confirmation bias is a cognitive phenomenon, it can be fueled by desires to find supportive evidence for our beliefs, a phenomenon termed "motivated reasoning"

(Kunda, 1990). Because practitioners want their clients to improve, they may be motivated to perceive change in its absence. Alternatively, they may unwittingly “cherry-pick” the outcome variables on which their clients are improving, de-emphasizing or neglecting those on which their clients are stagnating or becoming worse. Confirmation bias may help to explain the consistent finding that most psychotherapists tend to substantially underestimate the rates of deterioration among their own clients (Lambert, 2011).

Clients themselves may fall prey to the same understandable error, focusing primarily on positive outcomes while ignoring or minimizing negative ones. In some cases, they may engage in “selective symptom monitoring” (Pennebaker & Skelton, 1981), focusing on symptoms they expect to change while neglecting or under-attending to others.

Confirmation bias can also foster illusory correlation: the tendency to perceive a statistical association in its objective absence (Chapman & Chapman, 1967; Garb, 1998; Hamilton & Gifford, 1976). Specifically, confirmation bias can predispose practitioners to attend to the “hits” and forget the “misses” (Gilovich, 1991), thereby overestimating the efficacy of interventions or specific therapeutic maneuvers. Imagine the case of a therapist who periodically engages in confrontational tactics with a given client. Even though these tactics are unhelpful, the therapist may selectively attend to and recall the immediately succeeding sessions in which the client was doing better, and selectively neglect and forget the immediately succeeding sessions in which the client was not doing better or doing worse. As a consequence, the therapist may conclude erroneously that his use of confrontation with the client was linked to improvement, even though it was not.



*EBP as a Necessary Antidote to Ruling out Causes of Spurious Therapeutic Change*

A key point, not sufficiently emphasized in graduate education, is that EBP is needed for one crucial reason: to help to rule out CSTE as rival explanations for therapeutic change (Lilienfeld et al., 2008; Wilson, 2011). Although clinical intuition can sometimes lead us to detect *bona fide* client change in psychotherapy, it can also lead to erroneous inferences of change in its absence. As noted earlier, CSTE can fool even the most astute observers who rely on their naïve realism (Lilienfeld et al., 2007; Ross & Ward, 1996) into concluding that ineffective interventions are effective. Without RCTs and other sophisticated research designs essential to EBP as safeguards against CSTE, there is no way to know whether client change was due to an intervention itself as opposed to a host of extraneous factors.

The research designs comprising the hierarchy of EBP, particularly those on the highest rungs of this hierarchy, serve to rule out one or more CSTE. For example, although well-executed RCTs do not eliminate Type 1 CSTE, they do at least partially exclude them as rival *explanations* for therapeutic effectiveness. For example, in an RCT, spontaneous remission and regression to the mean often occur among individuals randomly assigned to both treatment and no-treatment (or alternative treatment) conditions. Nevertheless, because individuals are randomly assigned to conditions in an RCT, spontaneous remission and regression tend to occur equally across the active treatment and comparison conditions. In this way, these CSTE can be effectively ruled out as potential counter-explanations for group differences in treatment outcome. Other Type 1 CSTE can be minimized, although not entirely eliminated, by RCTs. For example, placebo and novelty effects are often present in RCTs that compare an active intervention with a wait-list control group; nevertheless, in a properly conducted RCT,

expectancies for improvement can be measured and potentially used as covariates in analyses. Moreover, attention-placebo control groups (Paul, 1966) help to control for the nonspecific effects of expectation, attention, novelty of intervention, and related variables.

Quasi-experimental designs characterized by well-matched comparison groups similarly help to rule out Type 1 CSTE, although they generally do so less effectively than do RCTs, because they impose less stringent requirements on the comparability of treatment and no-treatment (or alternative treatment) groups. For example, the law of large numbers dictates that pre-existing differences between two randomly assigned groups will approach zero with increasing sample size, whereas two therapy groups in a quasi-experimental design may differ on a host of undetected variables. Nonetheless, by matching groups on potential confounding variables or statistically adjusting group differences for covariates, one can often eliminate certain rival hypotheses for between-group differences in outcome (but see Meehl, 1971, and Miller & Chapman, 2001, for conceptual and methodological caveats regarding these statistical strategies).

Still other research safeguards help to exclude Type 2 CSTE as explanations for client improvement. For example, the use of largely or entirely objective outcome measures (e.g., indices of suicide attempts or days lost from work in outcome studies of depression), helps researchers to rule out illusory placebo effects and demand characteristics, both of which can lead to spurious perceptions of genuine improvement in its absence. In addition, double-blinded ratings of client symptoms by outside observers can eliminate confirmation bias as a source of error. Nevertheless, because psychotherapy outcome studies can never be strictly double-blinded (i.e., therapy clients know whether they are receiving treatment), confirmation

bias can still influence client and therapist ratings of improvement. Moreover, the effects of selective attrition can be minimized by intent-to-treat analyses (Hollins & Campbell, 1999), which compare outcome differences among all individuals initially assigned to experimental and control groups, including those who dropped out. In this and a host of other ways (see Kazdin, 2002), EBP comprises a toolbox of vital safeguards against naïve realism and accompanying errors in clinical inference emanating from CSTE.

#### *The Local Clinical Scientist Model as an Alternative Framework*

One popular alternative to the use of EBP to inform therapeutic practice has been the “local clinical scientist” model (Stricker & Trierweiler, 1995), which has been adopted by many scholar-professional (Psy.D.) clinical psychology programs (Maddux & Riso, 2007; McFall, 2007). This model exhorts practitioners to think and act as scientists in the clinical setting, ruling out alternative hypotheses for changes (or the lack thereof) in their clients during and following interventions. There is much to admire in the local clinical scientist model, especially its explicit embrace of a scientific attitude within the therapeutic setting. Yet this model ultimately runs afoul of an unresolvable problem: Because of CSTE, it is impossible to know whether the intervention one administered – as opposed to one or more of a plethora of other potential change agents – was responsible for the change observed in psychotherapy. The local clinical scientist model, simply put, is not an adequate alternative to EBP. Hence the need for controlled trials and EBP more broadly, which help to rule out alternative explanations for improvements following treatment.

#### *Summary*

A host of sources can lead therapists, other observers, and clients themselves to infer therapeutic improvement in its absence. These manifold sources provide a potent reminder of why rigorous research designs, which form the backbone of the first leg of the EBP stool, are essential, and why individuals cannot rely on their naïve realism to draw conclusions regarding therapeutic efficacy. They also underscore the limits of unguided clinical intuition in gauging therapeutic change while reminding us that such intuition can nevertheless help us to develop fruitful hypotheses concerning the sources of such change.

#### Second Source of Resistance: Myths and Misconceptions Regarding Human Nature

A second source of resistance to EBP is the widespread acceptance of deep-seated myths and misconceptions regarding human nature, some of which are held by psychologists themselves (Lilienfeld, Lynn, Ruscio, & Beyerstein, 2010). Many of these false beliefs are propagated by the media, the popular psychology industry, and in some cases, self-proclaimed leaders in the psychotherapy field. In still other cases, they may be imparted to practitioners and students during their education and clinical training. Some of these unsupported assertions may render practitioners reluctant to adopt EBP, because they may imply that certain therapies demonstrated to be efficacious in controlled studies cannot be effective in real-world settings. For example, a practitioner who believes that a specific phobia, such as a fear of cats, reflects unconscious conflicts may be reluctant to adopt behavioral treatments for this condition on the grounds that these interventions will result in symptom substitution, such as a fear of dogs. In fact, data show that symptom substitution, at least as conceptualized by psychoanalysts, rarely if ever occurs (Kazdin, 1982; Tryon, 2008). A plethora of psychological misconceptions can provide a rationale for selecting interventions with little or no empirical

support at the expense of more scientifically grounded therapies; we offer a few salient examples here.

### *Myths about memory and memory recovery*

The credo that clinicians must revisit their clients' distant past to "unrepress" or excavate deeply buried memories to promote lasting change is pivotal to certain (but not all; see Wachtel, 1977) psychodynamic (Galanter-Levy, 1997) and memory recovery (Crews, 1995) therapies. Therapists who believe that their clients often repress memories of painful childhood events and that the lingering residues of trauma underpin much, if not most, psychopathology (e.g., Bremner, Vermetten, Southwick, Krystal, & Charney, 1998; Ross & Pam, 1995), may incline them toward suggestive techniques geared to unearthing these ostensible memories, such as hypnosis, guided imagery, and repeated prompting of memories. There is precious little rigorous evidence that memory recovery procedures are effective; to the contrary, they carry a markedly heightened risk of pseudomemories in at least some clients (Lynn et al., 2003). Moreover, research demonstrates that most people remember such traumatic events as the Holocaust all too well, often suffering from flashbacks and other disabling symptoms of posttraumatic stress disorder (Loftus, 1993; Shobe & Kihlstrom, 1997).

As of the mid 1990s, several surveys (Poole, Lindsay, Memory, & Bull, 1995; Polusny & Follette, 1996) revealed that approximately one quarter of doctoral-level psychotherapists used two or more suggestive techniques, including hypnosis, guided imagery, and repeated questioning ("Are you sure you weren't abused? I would encourage you to keep thinking about it"), to probe for repressed memories of abuse. Arguably, contemporary clinicians appreciate more than ever the risks of creating false memories with suggestive procedures.

Nevertheless, two recent surveys demonstrate that questionable beliefs concerning memory and memory recovery techniques are still held by many mental health professionals. In a study of 220 Canadian practicing mental health professionals, including 76 psychologists, Legault and Laurence (2007) found that 41% of psychologists agreed that “Hypnosis enables people to accurately remember things they otherwise would not” (p. 121) and that a remarkable 67% of psychologists agreed that “Hypnosis can be used to recover memories of actual events from as far back as birth” (p. 121). Twenty-seven percent of psychologists endorsed the view that “Recovered memories must be reliable because no wants to have been abused as a child” (p. 122). Although the authors did not report these comparisons for psychologists alone, they found that sizeable proportions of participants endorsed the use of hypnosis (22%) and age regression (20%) as memory recovery techniques. In a survey of 368 U.S. social workers asked about their practices over the past year, Pignotti and Thyer (2011) found that (a) 7.6% reported using age regression for the treatment of sexual abuse, (b) 2.5% reported using past lives therapy, and (c) 9.8% reported using traumatic incident reduction, a technique that involves experiencing purportedly repressed memories in a safe and comfortable environment. None of these interventions, it is worth noting, is supported by research evidence.

#### *Myths regarding the primacy of early experience*

More generally, widespread beliefs regarding the causal primacy of experiences in infancy and childhood (Kagan, 1998; Paris, 2000) in predisposing to psychopathology may encourage clinicians to select interventions that rely on recovering or confronting unresolved feelings from childhood. Although some early environmental experiences surely shape later

personality and psychopathology in substantial ways, there is little evidence that they propel children on an inevitable trajectory toward maladjustment except when extremely severe and prolonged (e.g., massive social deprivation, repeated and prolonged sexual or physical abuse). For example, follow-up studies show that 75 to 85% of children fare well as adults in the wake of parental divorce (Hetherington & Kelly, 2002). Owing in part to neural plasticity (Bruer, 2002), most children are considerably more resilient in the face of early stressors than traditionally assumed. As Sroufe (1978) argued, “We would not expect a child to be permanently scarred by early experiences or permanently protected from environmental assaults. Early experience cannot be more important than later experience, and life in a changing environment should alter the quality of a child’s adaptation” (p. 50; see also Kagan, 1998).

In 1998, Rind, Bauserman, and Tromovitch (1998) rocked the world of psychotherapy with their meta-analysis on the correlates of child sexual abuse in college students. Complementing earlier work by their team in community samples, they reported that the association between a self-reported history of child sexual abuse and 18 forms of adult psychopathology (e.g., depression, anxiety, eating disorders) were weak in magnitude. Their article provoked a firestorm of media, political, and scientific controversy (Lilienfeld, 2001). Some critics raised thoughtful questions concerning Rind et al.’s findings, especially their generalizability to more severely affected populations (Dallam et al., 2001). Yet their central argument—that many individuals with a history of early sexual abuse seem to suffer few long-term psychopathological consequences—has held up well to continued scientific scrutiny (Rind, Bauserman, & Tromovich, 2002; Ulrich, Randolph, & Acheson, 2006). If therapists neglect to

appreciate the resilience that the majority of children (and adults) exhibit (Bonanno, 2004; Garnezy, Masten, & Tellegen, 1984) in the face of events ranging from kidnapping (Terr, 1983) to divorce (Hetherington & Kelley, 2002), they may fail to capitalize on patients' resources and coping abilities that can be harnessed to advantage in present-focused and scientifically supported interventions. It may also add force to the imperative to focus on or recover memories of abuse and other aversive childhood experiences.

Wachtel (1977) invoked - and criticized - the metaphor of the "woolly mammoth" to characterize the role of early experiences in traditional psychodynamic therapies. According to this pervasive metaphor, painful childhood memories lie buried in the unconscious in their original, pristine form (much like woolly mammoths preserved intact in the Arctic ice) and continue to affect current behavior adversely. Traditional psychoanalysts believe that therapists must revisit their clients' childhoods to process these early recollections and thereby eradicate the influence of baleful memories on current functioning. Yet, as Wachtel noted, this assumption is almost certainly mistaken, because there is no evidence that an impenetrable barrier insulates early unconscious memories from current experiences. Instead, even when early childhood experiences are formative for later adjustment, there is no reason to believe that present-oriented interventions, such as behavioral and cognitive-behavioral techniques, cannot modify our perceptions and interpretations of these experiences. The present can shape and revise our views of the past.

In fact, treatments with a principal focus on the here-and-now, such as behavioral, cognitive-behavioral, and interpersonal therapies, are unquestionably effective for a wide array of psychological problems (Butler, Chapman, Forman, & Beck, 2006; Chambless & Ollendick,



2001; Tolin, 2010). In addition, they are generally more efficacious than psychoanalytic and most other approaches for anxiety, eating, and sleep disorders, and more efficacious than other treatments for children and adolescents with behavior problems, such as lying, stealing, extreme defiance, and physical aggression (Lilienfeld et al., 2010; Weisz, Weiss, Han, Granger, & Norton, 1995).

### *Myths regarding effective interventions*

Culturally prevalent beliefs about specific psychotherapeutic interventions with minimal or no scientific support may also guide therapists' treatment decisions. For example, Pignotti and Thyer (2011) found that 30% of their sample of social workers used the technique of dream interpretation during the past year. Yet scientific investigations provide scant support for the belief that dreams hold symbolic meaning or that ferreting out the ostensible meaning of dream symbols is a worthwhile means of treating anxiety, depression, or other psychological problems. To the contrary, researchers have found that authoritative interpretation of dreams can sometimes instill false memories of events, including experiences of being bullied as a child (Mazzoni, Loftus, Seitz, & Lynn, 1999).

To take another example, the erroneous belief that abstinence is the only realistic treatment goal for clients with alcohol dependence (alcoholism) may spur clinicians to forego potentially efficacious therapies, such as relapse prevention (Larimer, Palmer, & Marlatt, 1999; Marlatt & Gordon, 1985; Polivy & Herman, 2002), that are characterized by a controlled drinking goal. In fact, a large body of controlled evidence suggests that moderate drinking is effective for many, although not all, people with alcoholism (Irwin, Bowers, Dunn, & Wang, 1999).

### *Summary*

Erroneous beliefs have consequences (Lilienfeld et al., 2010). As humorist Artemus Ward wrote, “It ain’t so much the things we don’t know that get us into trouble; it’s the things we know that just ain’t so” (see also Gilovich, 1991, p. 1). Germane to our arguments, psychologists’ misconceptions regarding human nature may lead them to ignore or dismiss scientific evidence regarding therapeutic efficacy. As we will later contend, this often overlooked point implies that graduate and continuing education should focus on correcting misinformation at least as much as on imparting correct information.

#### Third Source of Resistance: The Application of Group Probabilities to Individuals

The classic distinction between *nomothetic* and *idiographic* approaches to understanding human nature (Maher & Gottesman, 2005) has long been one of the flashpoints of confusion and contention in clinical psychology (Dawes, Faust, & Meehl, 1989; Meehl, 1954). EBP relies primarily on nomothetic findings, which strive to extract universal or quasi-universal laws that apply to all or most individuals within the population. Yet the everyday task of the practitioner is necessarily idiographic: Practitioners deal with the unique case and are confronted with the exceedingly difficult task of applying group-based findings to the individual.

#### *Moving from nomothetic laws to idiographic practice*

As Meehl (1954) observed in his classic “little book” on clinical versus statistical prediction, the everyday task of the clinician is challenging, even daunting. Scientific articles generally impart nomothetic information, yet clinicians necessarily operate within the N of 1 world of the idiographic case. When confronted with this dilemma, many students and beginning clinicians presume erroneously that group probabilities, which are all that RCTs can

hope to deliver, cannot apply to the individual case. They assume that they cannot possibly bridge the nomothetic and idiographic realms of analysis. Hence, they may conclude, there is no reason to rely on EBP, because “every individual is unique.”

Of course, there is a kernel truth in this assertion: Each individual is indeed unique. Yet this undeniable fact does not imply that one cannot deduce probabilistic generalizations from controlled group studies that apply to individual clients, because groups are, after all, composed of individuals (Dawes et al., 1989).

With the aid of tangible examples, it is easy to grasp why group probabilities are relevant to individual decisions. Meehl (1973) famously gave the example of an individual forced to play the game of Russian Roulette, in which a loaded revolver contains a certain number of bullets. The player is given two options. In one condition, the barrel of the gun contains four bullets, with one canister left blank; in the other, the barrel of the gun contains only one bullet, with four canisters left blank. If the player followed the rationale that “probabilities don’t apply to the individual case” to its logical (or in this case, illogical) conclusion, the choice of the condition would not matter, as the player would be equally likely to live or die regardless of her choice (Dawes et al., 1996; Grove & Meehl, 1996). Yet this reasoning is obviously fallacious, as her odds of dying are four times higher with the first gun than with the second. Similarly, imagine a patient who has recently experienced a severe myocardial infarction. His physician presents him with two treatment options associated with identical side effect profiles: one that has been found in controlled studies to be associated with an 80% survival rate, and another that has been found to be associated with a 50% survival

rate. Again, the logic that group probabilities are irrelevant to the individual would imply incorrectly that he has no legitimate grounds for selecting the former treatment over the latter.

In clinical psychology and allied fields, probabilities are an imperfect treatment metric, but they are often far better than nothing. Meehl (1954) invoked the hypothetical example of a set of predictors of a group outcome (e.g., response to a given psychotherapy) that, when combined into a multiple regression equation, yields a multiple R of .999. He noted that no rational critic of generalizing from group to individual probabilities would quarrel with using this formula to predict treatment outcome. But, Meehl asked rhetorically, "If this is reasonable, is not .990 reasonable? And then, why not .90, and thus .75 and, to be consistent, .25?" (p. 23). As he pointed out, there is a continuous gradient of generalization from the group to the individual ranging from exceedingly confident, highly confident, moderately confident, and so on, with no bright line demarcating more grounds for generalization from fewer. Moreover, at least some basis for generalization is surely superior to none at all. Effective science, including the clinical science of EBP, reduces uncertainty in our inferences (McFall & Treat, 1999). By doing so, it can improve the quality of patient care, because it can allow us to select interventions that enhance the probability of improvement beyond baseline guessing.

#### *Using moderators in meta-analysis to bridge the nomothetic and idiographic*

Compared with the exclusive reliance on subjective clinical judgment to tailor interventions to the unique case, meta-analysis affords a better long-term solution to bridging nomothetic and idiographic approaches to treatment decisions. Specifically, moderators in meta-analyses can provide practitioners with helpful information regarding which subsets of individuals respond differentially to different interventions (Kraemer, Wilson, Fairburn, & Agras,

2002; Rosenthal & DiMatteo, 2001). For example, behavioral activation may be especially effective for major depression among patients with high initial symptom severity (Dimidjian et al., 2006). In this way, the identification of moderators can allow practitioners to partition heterogeneous groups of clients into narrower subsets of individuals who are especially likely to respond to the intervention of choice.

### *Summary*

As Bishop Joseph Butler reminded us in 1736, “probability is the very guide of life” (see Downing, 1977, p. 3). Science, especially clinical science and other domains marked by substantial individual differences, is an inherently probabilistic business, and extrapolation from group likelihoods to individuals are often the best we can hope to accomplish. Moreover, the undeniable uniqueness of all individuals does not vitiate the logic of generalizing from nomothetic studies of psychotherapy outcome to the idiographic case, because at least some grounds for statistical generalization are almost always superior to none.

### Fourth Source of Resistance: Reversal of the Onus of Proof

One of the core tenets of science is that the burden of proof rests on the proponents rather than the skeptics of assertions (Sagan, 1995; Saks, 2002). Science is inherently a conservative enterprise, because most novel ideas are wrong. Hence, science imposes a heavy burden on the advocates of new assertions, including untested treatments. As Dawes (1994) observed, this epistemic burden can be summarized in terms of the motto of the state of Missouri: “Show me.” That is, it is up to developers of new ideas to accrue evidence that these ideas deserve a hearing. It is not up to critics of these ideas to amass evidence that they are erroneous (Herbert, 2003). Individuals who reverse this burden of proof are committing what

logicians term the *ad ignorantium* fallacy (the argument from ignorance; Woods & Walton, 1978), that is, the error of concluding that because a claim has not been proven wrong, it must be correct or at least possess substantial merit.

#### *The Onus of Proof Requirement and EBP*

As applied to psychotherapy, it is up to proponents of novel or unsubstantiated treatments to offer compelling evidence that these treatments are supported by scientific data. Yet a number of critics of EBPs have reversed this onus of proof requirement by arguing that certain theoretically plausible treatments that have not yet been studied in controlled trials, or that have not yet been studied extensively, merit inclusion in lists of evidence-based techniques, including ESTs (e.g., Bohart, 2005; Gray, Plath, & Webb, 2009). This argument insists erroneously that it is up to skeptics to demonstrate that certain treatments are inefficacious rather than up to proponents to demonstrate that these treatments are efficacious.

#### *Distinction between invalidated and unvalidated therapies*

Much of the confusion regarding the burden of proof requirement stems from a failure to distinguish *invalidated* from *unvalidated* therapies (Westen, Novotny, & Thompson-Brenner, 2004). Invalidated therapies have been examined in systematic studies and found not to work; in contrast, unvalidated therapies have not yet been examined in systematic studies (or have not been sufficiently examined in such studies) and may or may not work (Arkowitz & Lilienfeld, 2006). As philosophers of science remind us, absence of evidence should not be confused with evidence of absence. Some authors have argued that treatments that have been omitted from the current list of ESTs are necessarily presumed, either explicitly or implicitly, to be ineffective

(Bohart, 2005; McWilliams, 2005; Wachtel, 2010). Yet the absence of a treatment from a list of scientifically supported treatments does not mean that it is invalidated (i.e., not effective), only that it is unvalidated (i.e., not yet been shown to be effective). For example, the conspicuous absence of psychodynamic and humanistic therapies from lists of evidence-based techniques (Chambless & Ollendick, 2001; Westen et al., 2004) does not imply that such treatments are ineffective; it implies only that they have not yet been studied sufficiently to merit inclusion in such lists (Arkowitz & Lilienfeld, 2006).

It is incumbent on clinical scientists to keep an open mind regarding the efficacy of unvalidated therapies, provided that their therapeutic rationale is at least marginally plausible (David & Montgomery, 2011; Lilienfeld, 2011). At the same time, clinical scientists have every right to insist on rigorous research evidence before concluding that these therapies are efficacious.

### *Summary*

The burden of evidence falls on advocates of treatments to demonstrate their efficacy and effectiveness; it does not fall on skeptics. Hence, the argument that EBP is unfair because certain treatments have not yet been studied sufficiently is misguided. If strong supportive research evidence for these interventions becomes available, they should and typically will be assimilated into the corpus of mainstream of psychotherapy practice and research.

### Fifth Source of Resistance: Mischaracterizations of What EBP Is and Is Not

Another source of resistance to EBP stems from misunderstandings and misrepresentations of what EBP entails. In some published works and workshops, EBP has been characterized in an inaccurate or even caricatured fashion (Gibbs & Gambrill, 2002). Several of

these misconceptions partly reflect thoughtful and understandable reservations concerning EBP and may even contain a kernel of truth, but are oversimplified. Because few of these misconceptions are routinely discussed in graduate training – to the contrary, some may be explicitly reinforced by faculty members or supervisors who hold them – many students may leave their graduate programs with the lingering sense that evidence-based practices are overly constraining, inapplicable to actual patients, incapable of accommodating nonspecific processes in therapy, and so on.

Here (see also Table 2) we outline eight misconstruals of EBP that have been especially widespread in the clinical literature, along with brief corrective rebuttals of them (see Gibbs & Gambrill, 2002, for other common misunderstandings regarding EBP). We have touched on several of these misunderstandings in previous sections, but present them in more elaborated form in this subsection.

(1) *EBP stifles innovativeness in the development of new treatments.* In fact, EBP places certain constraints only on the use of current treatments. It does not imply that practitioners and researchers cannot develop and test novel interventions, with the proviso that clients who receive these interventions receive full informed consent that they are experimental (Thyer & Pignotti, 2011). Moreover, EBP is not “ossified.” In keeping with the cardinal principle that science is a provisional, self-correcting process (Sagan, 1995), EBP necessarily evolves in accord with new research evidence.

(2) *EBP requires a “cookie-cutter,” “one-size-fits-all” approach to treatment.* EBP does not mandate the use of the current APA Division 12 list of ESTs; moreover, even ESTs that are manualized do not typically prescribe fixed responses to client behaviors in



psychotherapy. In fact, most manuals are increasingly serving as rough treatment blueprints that afford clinicians substantial leeway in deciding when and how best to deliver interventions (O'Donohue, Ammirati, & Lilienfeld, 2011). Kendall, Gosch, Furr, and Sood (2008) discussed "flexibility within fidelity" as a model for the use of scientifically-informed interventions. In this framework, practitioners strive to follow the basic guidelines prescribed by these interventions while avoiding rigid adherence to specific therapeutic protocols. Such unbending adherence may be associated with poor treatment outcomes (Castonguay et al., 1996).

(3) *EBP excludes nonspecific influences in therapy.* Like several mischaracterizations of EBP, this one contains a kernel of truth, but it again reflects the conflation of EBP with ESTs. It is true that ESTs focus on the specific ingredients that differentiate psychotherapies from each other, but to the extent that EBP incorporates all scientific evidence relevant to therapy outcomes (Thyer & Pignotti, 2011), it can comfortably incorporate data on the therapeutic alliance, relationship factors, inculcation of expectancies, and other nonspecific factors in treatment.

(4) *EBP does not generalize to individuals who have not been examined in controlled studies.* As we observed earlier (see "Third Source of Resistance: The Application of Group Probabilities to Individuals"), at least some basis for generalization is better than none given that generalization occurs along a gradient of certainty. Clinical science at its best reduces, although rarely eliminates, uncertainty in our inferences about clients (McFall & Treat, 1999). Hence, when selecting treatments, it will almost be better to extrapolate from studies conducted on somewhat similar individuals than to start from

scratch. Admittedly, the extent to which data from tightly controlled studies generalize to actual cases is an empirical question. Fortunately, most, although not all, studies suggest that data from rigorously designed efficacy studies often translate reasonably well to real-world effectiveness (McHugh, Murray, & Barlow, 2009).

- (5) *EBP neglects evidence other than RCTs.* As noted earlier, EBP generally regards research designs as falling along a hierarchy of evidentiary certainty. It is indeed the case that all else being equal, RCTs occupy a higher stratum in the hierarchy than do other sources of evidence. That is because RCTs rule out more sources of error, namely, more CSTEs, such as spontaneous remission, regression to the mean, history, multiple treatment interference, and the like. Nevertheless, other sources of research evidence can and often should be considered in EBP, such as systematic within–subject designs, rigorously conducted quasi-experimental studies, and therapy process data that provide helpful information concerning mediators of change (Ghaemi, 2009).
- (6) *EBP is unnecessary because all treatments are equally efficacious.* The Dodo Bird verdict of psychotherapy equivalence (Rosenzweig, 1936; see also Wampold, Mondin, Moody, Stich, Benson, & Ahn, 1997), which was named after the Dodo Bird in Lewis Carroll’s “Alice in Wonderland” who declared that “everybody has won and all must have prizes,” has frequently been used to challenge the rationale for EBP (e.g., Duncan, Miller, & Sparks, 2011). If all therapies are equal in their effects, the need for the first leg of EBP is vitiated given that the choice of treatment does not matter. This widespread claim merits closer scrutiny.

Although not widely acknowledged, the Dodo Bird verdict appears to apply to two separable assertions in the psychotherapy outcome literature: (a) collapsing across all disorders, there is no evidence for differences efficacy across treatments (*viz.*, no main effects) and (b) there is no evidence that any treatment is more efficacious than any other treatment for any psychological disorder (*viz.*, no interactions). Given that there are at least 500 different psychotherapies (Eisner, 2000) and approximately 300 diagnoses in the current DSM (American Psychiatric Association, 2000), acceptance of claim (b) would also necessitate acceptance of the remarkable claim that all 150,000 (500 times 300) treatment-by-disorder combinations yield precisely equal statistical interactions.

Setting aside the exceedingly low *a priori* likelihood of this equivalence of all treatment-by-disorder interactions, there is ample evidence that the Dodo Bird verdict, at least when stated in the form of (b), is false (*cf.*, Shedler, 2010). For example, there is substantial evidence that behavioral and cognitive-behavioral treatments are more efficacious than other treatments for some conditions, such as anxiety disorders (Hunsley & Di Giulio, 2002; Tolin, 2010), and for childhood and adolescent disorders (Chambless & Ollendick, 2001; Weisz, Weiss, Han, Granger, & Morton, 1995). In addition, there is growing evidence that at least some treatments, such as crisis debriefing for trauma-exposed victims and Scared Straight programs for adolescents at high risk for antisocial behavior, can be harmful in certain cases (Lilienfeld, 2007; Winter, 2006).

Furthermore, in either form (a) or (b), the Dodo Bird verdict applies only to that relatively small minority of psychotherapies that have been examined repeatedly in systematic studies. Even if all extensively studied psychotherapies turn out to be equally efficacious (either overall or for all disorders), a supposition we have shown to be highly implausible, this does not justify the assumption that an untested therapy can be safely be assumed to be equally efficacious to extant treatments. Nor does it imply that scientific evidence for this therapy need not be adduced in future studies. Both of these assumptions would amount to placing the burden of proof on skeptics rather than proponents of the treatment (see “Fourth Source of Resistance: Reversal of the Onus of Proof”).

(7) *EBP is inherently limited because therapeutic changes cannot be quantified.* There may well be some truth to the proposition that certain changes in psychotherapy are difficult to measure, at least given presently available instruments. Yet as the great E.L. Thorndike (1940) observed, “If something exists, then it exists in some quantity. If it exists in some quantity, then it can be measured” (p. 19). If therapists, clients, or both can notice an improvement in subjective outcomes (e.g., sense of identity, meaning in life) following treatment, there is no inherent reason why these outcomes cannot be quantified reliably. The increasing development of well validated implicit measures in clinical research (e.g., Nock & Banaji, 2007) suggests that even largely unconscious outcome measures are often amenable to quantification. Of course, to the extent that a positive therapeutic outcome suspected by a clinician cannot be measured at all using

available instruments, it is incumbent on proponents of a therapy to qualify their claims regarding its efficacy accordingly.

(8) *EBP is erroneous because human behavior is impossible to predict with certainty.* Some skeptics of EBP insist that because the behavior of clients cannot be predicted with certainty, the constraints imposed by EBP are unjustified. For example, Corsini (2008) defended his decision not to include scientific evidence bearing on the efficacy of each treatment in his widely used psychotherapy textbook on essentially these grounds. He approvingly cited Patterson's (1987) argument that to subject psychotherapy to systematic research,

“we would need (1) a taxonomy of client problems or psychological disorders...; (2) a taxonomy of client personalities; (3) a taxonomy of therapeutic techniques...; (4) a taxonomy of therapists; and (5) a taxonomy of circumstances. If we did have such a system of classification, the practical problems would be insurmountable. Assuming five classes of variables, each with ten classifications, ...a research design would require....100,000 cells...So, I conclude we don't need complex multivariate analyses and should abandon any attempt to do the crucial, perfect study of psychotherapy. It simply is not possible” (p. 247).

We are inclined to agree with Corsini (2008) that the “perfect study of psychotherapy” is not attainable, because no psychological investigation is without flaws. But this point does not warrant nihilism about multivariate analyses of psychotherapy outcome research, let alone about scientific conclusions regarding the efficacy of psychotherapies. Specifically, the fact that a plethora of variables, such as clients' personality traits and therapists' psychological characteristics, may interact statistically in complex ways in predicting

response to treatment does not undermine the possibility of substantial main effects of certain treatments relative to others.

To borrow an example from the medical literature, all individuals with melanoma surely differ from each another in myriad ways. Some are young and some are old; some are Caucasian and some are African-American; some have hypertension and some do not; some have a history of Type 2 diabetes and some do not, and so on. Yet despite these and countless other complicating variables, 90% or more of cases of melanoma are essentially curable with early surgery (Berwick, 2010). In the case of psychotherapy, we can similarly make reasonable generalizations regarding therapeutic efficacy despite the presence of potential higher-order interactions.

### *Summary*

A host of understandable misconceptions regarding EBP have arisen, and advocates of EBP have often been insufficiently proactive in combating them. It is especially crucial to note that because EBP emphasizes the scientific evaluation of therapeutic outcome and process, it is inherently provisional and open to correction. At the same time, EBP insists that certain sources of evidence concerning treatment tend to be superior to others, as these sources tend to be better suited for ruling out rival hypotheses for therapeutic improvement.

### Sixth Source of Resistance: Pragmatic, Educational, and Attitudinal Obstacles

A final major source of resistance to EBP comprises a host of pragmatic, educational, and attitudinal obstacles encountered by many psychologists, especially those working in practice settings. We delineate the primary obstacles here, although our list is surely not exhaustive.

### *Time*

Perhaps the most obvious obstacle to adopting EBP is that reading and digesting the scientific literature can be enormously time-consuming. Moreover, clinicians may find that they need to pursue additional reading, training, and supervision to properly translate their new knowledge into practice. In a survey of community practitioners, Nelson, Steele, and Mize (2006) noted that many clinicians are already working 50-60 hours per week and do not feel they have the extra time required to stay abreast of the clinical research literature.

#### *Knowledge about training materials*

In a survey of 891 practicing psychologists, Addis and Krasnow (2000) found that a third of practitioners were completely or mostly unclear about what a training manual is, and approximately half stated that they gave “little or no thought” to using treatment manuals in their work (we are unaware of any more recent survey data on this issue). Although psychotherapy training manuals are by no means required for EBP, they are one frequent means of maximizing the chances that practitioners engage in practices that are supported by controlled research. Thus, there remains a substantial gap between the output generated from research protocols, such as treatment manuals, and the use of such output by clinicians in the trenches of clinical work.

#### *Access*

Research articles, training manuals, and instructional videos are often readily available to practitioners in university or academic settings in which journal access has been obtained or libraries of training materials have accumulated over several years. In addition, training opportunities may be available for practitioners in group settings that would be less accessible to those in private practice. In sharp contrast, resources in agencies may be spread so thin that

clinicians are unsure what is available to them. In a study of social workers being trained in the implementation of EBP with families and children, Aarons and Palinkas (2007) noted the following comment from a study participant: "...it would've probably done us a world of good if we had had the [training manual], which we probably did and we didn't know it" (p. 415).

### *Money*

For those practitioners whose agencies do not subscribe to scientific journals, accessing the research literature can be an expensive endeavor (Morrissey et al., 1997). Beyond traditional reading materials, practitioners also may seek training by attending workshops, continuing education seminars, or institutes. Nevertheless, the costs associated with such training (and lack of awareness of where to obtain it) serve as additional barriers to accessing EBP (Pagoto, Spring, & Coups, 2007). Simpson (2002) found that institutional failure to provide funds for practitioners to attend conferences at which they can obtain training in EBP is a major obstacle to altering their practices. In addition, in a survey of 1291 practicing psychologists, Stewart, Chambless, and Baron (2012) found that cost and time were the most widely cited impediments to attending workshops in EBP-based interventions.

### *Steep learning curve*

For some practitioners, learning and implementing EBP confer professional advantages, including enhanced feelings of competence and motivation to treat (Aarons & Palinkas, 2007; Baumann, Kolko, Collins, & Herschell, 2006). In contrast, some practitioners may feel overwhelmed by the sheer volume of information and steep learning curve associated with the task of mastering learning a new area of the literature, a new therapeutic approach, or both.

### *Statistical complexity*



The ways in which modal psychotherapy research articles are written – with highly technical language and complex statistical analyses - may impede learning and discourage practitioners from acquiring the knowledge needed to implement EBP (Backer, 2000). As the field of statistics grows, newer and more refined analytic methods continue to emerge. Although these developments bode well for our field’s ability to conduct more fine-grained analyses, the downside is that fewer and fewer practitioners (and even researchers) are likely to understand them. Furthermore, as the field of statistics grows, its methods and approaches continue to evolve, so that as clinicians age, they are more likely to encounter research articles containing statistical methods in which they were never trained.

#### *The “Ivory Tower” mentality*

The wide gap between science and practice in clinical psychology is sometimes attributed to the failure of academic researchers to grasp the difficulties in translating well-controlled, narrowly defined studies to the real world scenarios that clinicians typically encounter (Pagoto, Spring, & Coups, 2007; Persons & Silberschatz, 1998). The perception that RCTs are fraught with methodological limitations leading to poor external validity (as in the efficacy versus effectiveness distinction as discussed earlier; Seligman, 1995) seems to have contributed to an “us vs. them” mentality that divides academics and clinicians (Nelson, Steele, & Mize, 2006). Interestingly, Aarons (2004) found that practitioners with more education had the most favorable attitudes toward EBPs. Thus, one hopeful possibility is that recent graduates of programs in which EBPs are taught and in which clinicians receive evidence-based supervision may mitigate negative attitudes toward EBPs post-graduation.

#### *Summary*

A plethora of tangible obstacles render psychologists, especially those in practice settings, reluctant to embrace EBP. Time, money, and access to resources are clearly among these impediments. So is the increasing technical complexity of the psychotherapy process and outcome literature, which can intimidate even seasoned researchers from evaluating the evidence bearing on the research leg of the EBP stool. Moreover, academic psychology has not adequately come to grips with the pressing need to address perceptions by the practice community, warranted or not, that it is often “out of touch” with the day-to-day concerns of clinicians.

#### Conclusion: Constructive Recommendations for Addressing Resistance to EBP

In this manuscript, we have contended that resistance to EBP is both widespread and understandable. The resistance is widespread in that it is displayed by sizeable minorities of practicing psychologists (e.g., Chambless & Stewart, 2007; Pignotti, 2009) and perhaps clinical psychology graduate students (Luebbe et al., 2001). The resistance is understandable in that it stems from several deep-seated sources, such as naïve realism and misconceptions regarding human nature and group probabilities, all of which are often left unaddressed in graduate training. As a field, we should not be surprised by psychologists’ prevalent skepticism toward EBP given that we have done little to understand, let alone confront, its principal underpinnings.

#### *Limitations*

One limitation of our analysis is that we have examined only the sources of opposition to EBP *per se*, namely, a systematic approach to evaluating and integrating evidence. Notably, we have not addressed the logistical obstacles that may stand in the way of implementing

EBP-based interventions among practitioners who are already *favorably disposed* toward EBP. In some cases, such hurdles may be at least as formidable as the negative attitudes toward EBP itself.

For example, obtaining proper supervision in a new technique is both required (APA Ethical Principles of Psychologists, 2002) and logistically difficult. The extent to which adequate supervision, peer support, and team meetings are available substantially influence the success or failure of EBP implementation (Kavanagh et al., 2003; Milne, Dudley, Repper, & Milne, 2001). In a study of “training the treaters,” Sholomskas et al. (2005) randomized community practitioners learning CBT to one of three conditions: manual only, manual plus a supplemental web-based training program, or manual plus training seminar and supervision. Participants in the seminar/supervision condition demonstrated the greatest gains in acquiring CBT skills, suggesting that, to maximize agency investment in training, practitioners need to be provided with ongoing supervision along with standard training materials.

Motivational issues present further barriers to the implementation of EBP. For example, many clinicians may neglect to evaluate evidence in favor of EBP because of complacency; that is, “Why fix what isn’t broken?”. Even if practitioners recognize that they should incorporate EBP into their work, they are often not incentivized by their agencies to do so (Proctor, Knudsen, & Fedoravicius, 2007). In one survey of 467 practitioners, 62% reported that they were not required by their agencies to use EBP in their work (Walrath, Sheehan, & Holden, 2006). In addition, few insurance companies base their reimbursement schedules on evidence-supported practices, and many clients do not know to request EBP as part of their care. Thus, most practitioners must find the use of EBP intrinsically motivating, as there

are few other ostensible reasons for spending the time and money to incorporate EBP into their practice. Perhaps a consideration here is to note how implementing EBPs affects the “bottom line” of the organization (Aaron & Palinkas, 2007). Notably, however, the extant literature is essentially silent regarding evidence-based methods for implementing evidence-based practices from a managerial standpoint (Proctor, Knudsen, & Fedoravicius, 2007).

For practitioners working as part of a system, the perception of institutional support also plays a key role in the implementation of EBP (Aarons & Palinkas, 2007; Baumann, Kolko, Collins, & Herschell, 2006; Nelson & Steele, 2007). Klein and colleagues (2001) found that the extent to which an organization supports EBP predicts the success of implementation effectiveness. Clinicians’ attitudes toward EBP are also influenced by the perceived fit between EBP, on the one hand, and the goals and values of their agency and the agency’s administrative team and clinical leaders, on the other (Proctor, Knudsen, & Fedoravicius, 2007). For example, practitioners may need not only assistance in acquiring materials to learn EBP, but time off to attend trainings or obtain supervision in well-supported therapeutic techniques. Moreover, when implementing an EBP requires the synthesis of a multitude of systemic resources (e.g., in a school setting), lack of cooperation at an administrative level may stymie implementation. Chinman et al. (2005) suggested that the difficulties with translating science to practice lie more firmly in the failure of community support than in the extent to which information is available to practitioners (or their willingness to use this information). Moreover, the implementation process is multi-layered; as Durlak and DuPre (2008) noted, “while organizational capacity is important, organizations

need support in conducting new interventions successfully, and this support comes primarily through training and technical assistance that is provided by outside parties” (p. 335).

Finally, organizational support is often tied to the perceived financial viability of a new treatment (Nelson, Steele, & Mize, 2006). To provide financial and temporal support to clinicians to learn a new treatment, the organization probably must perceive that the new training will translate into financial gain for the agency. Treatments that have not demonstrated cost-effectiveness (or treatments whose cost-effectiveness have not been explored) are therefore less viable options for organizations to support. In an interesting twist on EBP research, Proctor, Knudsen, Fedoracvicius, Howman, Rosen, and Perron (2007) examined the attitudes of organizational and agency directors toward EBP in their settings and found that, although most directors favored EBP, there were four major obstacles to implementation: (1) applicability to the types of problems/clients seen in their clinics; (2) accessibility of training and training materials; (3) assessment of the evidence and determining a “critical mass” at which point implementation of an EBP becomes a scientific imperative; and (4) staff-related issues, including provider resistance, heavy workloads, and lack of appropriately-trained supervisors.

Hence, we do not wish to imply that EBP would be widely, let alone universally, embraced even were psychologists’ resistances to EBP substantially mitigated. Bearing this crucial caveat in mind, in the remaining pages of the manuscript we outline several constructive recommendations for addressing resistance to EBP, both among students and psychologists. We offer such recommendations with cautious optimism, but with the proviso that they too need to be subjected to empirical scrutiny using evidence-based research. Research on the

effectiveness of debiasing individuals against cognitive errors, such as confirmation bias, is still in its infancy (Lilienfeld, Ammirati, & Landfield, 2009), but it suggests that debiasing interventions are often only modestly efficacious. Hence, we caution readers against expecting our prescriptions to be panaceas.

*Recommendations for addressing resistance among students*

In many respects, our “diagnosis” of the sources of resistance to EBP leads us to several straightforward potential remedies. First, we propose that the training of future clinical psychologists and other mental health professionals focus more explicitly on underscoring the perils of naïve realism and on the manifold rival explanations (namely, CSTEs) for an intervention’s apparent effectiveness in its absence. We especially recommend adopting an historical perspective, in which students are exposed to the lengthy history of errors in medicine, including psychiatry, that have stemmed from an overreliance on naïve realism and unguided clinical intuition (Grove & Meehl, 1996; Hall, 2011). In this way, students can come to see how even intelligent practitioners in previous generations were fooled, and how scientific methods, such as RCTs, allowed scientists to correct previous errors and thereby improve patient care.

This component of graduate training may need to be supplemented by a discussion of research on “bias blind spot” (Pronin, Lin, & Ross, 2002), which is the ubiquitous tendency of virtually all individuals to perceive biases in others but not in themselves. Students must come to understand that because of bias blind spot, they may erroneously see themselves as immune to cognitive errors to which their ostensibly less objective colleagues are susceptible. Good scientists, including clinical scientists, are probably just as prone to

confirmation bias and other errors as are poor scientists (Mahoney, 1977). The crucial difference is that good scientists are aware of their propensities toward bias and make concerted efforts to compensate for them.

Second, rather than focusing largely or entirely on conveying *accurate* information to students, graduate instructors may need to focus at least as much on disabusing students of *inaccurate* information, especially misunderstandings regarding human nature that can impede acceptance of EBP and misconceptions regarding EBP itself. Research in educational psychology suggests that providing students only with accurate information regarding a subject domain usually leaves their misconceptions in that domain intact (e.g., Winer, Cottrell, Gregg, Fournier, & Bica, 2002). Moreover, this body of literature indicates that an “activation approach,” in which misconceptions are actively raised and then rebutted by instructors, may often be successful in correcting mistaken student beliefs (Kowalski & Taylor, 2009). To the extent that these findings generalize to clinical psychology, instructors should not assume that merely imparting accurate information about EBP will temper student misconceptions regarding EBP. Instead, teachers may need to first raise and then dispel student misconceptions about EBP prior to presenting information concerning specific evidence-based techniques.

Third, and more broadly, we propose that the modal overarching approach to training students in EBP be reconsidered. Traditionally, the educational approach to EBP in graduate programs has been what we term “protocol-based.” This approach focuses on the “whats” of psychotherapy research. In the protocol-based approach, instructors inform students that scientifically based therapeutic techniques are important, and they

then instruct students how to administer these interventions. Traditionally, this approach takes for granted that students will (a) grasp the value of scientific approaches to ascertaining therapeutic efficacy and (b) accept the need to learn and master evidence-based clinical practices. In many ways, the protocol-based approach resembles what Gambrill (1999) termed “authority based medicine” or what Isaacs and Fitzgerald (1999) humorously dubbed “eminence-based medicine,” in which information regarding therapeutic efficacy is passed down hierarchically in an uncritical, *ex cathedra*, fashion from teacher to student. This model also bears marked similarities to the “sponge model” of education (Keeley, Ali, & Gebing, 1998), which assumes that students will simply “absorb” information from their teachers without questioning it.

The protocol-based approach certainly has merit for some didactic purposes, as it fosters the efficient training of graduate students in scientifically supported techniques. Nevertheless, we contend that this approach is insufficient, because it leaves unaddressed the crucial question of how psychologists have ascertained that certain therapeutic protocols, but not others, are efficacious. As a consequence, a protocol-based approach may inadvertently encourage students skeptical of scientific approaches to psychotherapy to merely “go through the motions” when learning therapeutic techniques. When these students later encounter psychotherapy protocols that are marketed persuasively by their advocates but that are not adequately supported by research – techniques characterized by what Isaacs and Fitzgerald (1999) jokingly called “eloquence-based medicine” – these students may be vulnerable to their seductive allure. That is because they often have not



learned to appreciate the vital gatekeeper role of scientific evidence in evaluating psychological treatments.

Accordingly, we maintain that a protocol-based approach to clinical psychology education should be supplemented by a “rationale-based” approach, which emphasizes the *raison d’être* for a scientific approach to psychotherapy evaluation. This approach focuses on the “whys” of psychotherapy research, especially reasons for the necessity of systematic treatment outcome and process evidence. In this approach, explaining the role of science as a safeguard against manifold sources of inferential errors (Lilienfeld, 2010; McFall, 1991; Tavis & Aronson, 2007) assumes center stage, and EBP is taught as an invaluable bulwark against rival hypotheses for change in psychotherapy. In addition, in a rationale-based approach, student objections to a scientific approach to therapy are neither ignored nor dismissed. Instead, *contra* the sponge model, such objections are actively encouraged, discussed, and addressed proactively.

#### *Recommendation for addressing resistance among psychologists*

Addressing resistance to EBP among current psychologists, including practitioners, arguably poses even more of a challenge than does addressing resistance to EBP among students, as the former resistance may often be more deeply entrenched. As we noted earlier, resistance to EBP is more marked among older than younger practitioners (Aarons & Sawitsky, 2007). Although such findings are cross-sectional, they raise the possibility that negative attitudes toward EBP may become more pronounced over time, especially among clinicians who received their graduate degrees prior to the EBP era.

With this point in mind, we can turn to the treatment dissemination literature for helpful tips to addressing resistance to EBP among current practitioners. Diffusion research (Young, Connolly, & Lohr, 2008) indicates that the identity of the person transmitting the information is often a major predictor of that information's receptivity to others. If "opinion leaders" (Rogers, 2003) who deliver messages are perceived as outsiders or as individuals who do not grasp the needs of consumers, their messages may be devalued or ignored. In the case of EBP, relying exclusively on academics to disseminate information regarding evidence-based interventions may be unwise, as many clinicians may understandably feel that researchers do not appreciate the complexities confronted by psychologists "on the front lines" of everyday practice. The "Ivory Tower mentality" to which we referred earlier may fuel these perceptions. Excessive reliance on academics as opinion leaders may also engender understandable "reactance" (Brehm & Brehm, 1981) to information regarding EBP among clinicians, as it may inadvertently communicate the condescending message that "more knowledgeable" researchers are instructing "less knowledgeable" practitioners about how to conduct therapy (Herschell et al., 2002).

These considerations underscore the necessity of forging closer alliances between research-oriented and practice-oriented clinical psychologists, and enlisting the latter to play a more active role in disseminating information, and dispelling misinformation, concerning EBP. Practice-oriented psychologists, it is worth noting, may also better be able to appreciate and anticipate thoughtful objections to EBP from practitioners, such as the challenges of transporting evidence-based interventions to everyday practice, and thereby communicate effectively the clinical advantages of EBP. These considerations also highlight

the crucial role of professional organizations, such as the APA, Association for Psychological Science (APS), and Association for Behavioral and Cognitive Therapies (ABCT), in reaching out to practicing clinicians who harbor serious doubts about EBP. It is ironic that although the APA contains a Science Directorate and a Practice Directorate, it has rarely made the integration of science and practice - and the dissemination of EBP - a core focus of its primary initiatives.

Diffusion research further suggests that dissemination efforts directed primarily toward those who already are favorably inclined to the message (“preaching to the converted”) can, paradoxically, backfire among those already skeptical of the message (Young et al., 2008), perhaps by fostering the “us versus them” mentality that we have already discussed. In this way, such well-intentioned efforts may widen the gap between science and practice. In the case of EBP, diffusion research points to the need to communicate information not only to like-minded colleagues who are favorably disposed to EBP, but more important, to individuals who do not share these positive views.

Although we do not take a stance on the recent effort by the Academy of Psychological Clinical Science (APCS) to develop a new accreditation system geared exclusively toward research-oriented clinical psychology programs (see Baker et al., 2008), we urge APCS and like-minded organizations to bear in mind that dissemination efforts must be aimed not merely at students who enter graduate school with positive attitudes toward EBP, but also at students who are skeptical of EBP. Otherwise, APCS may risk engendering greater resistance to EBP among students in non-research oriented clinical psychology programs.

As we have seen, some practitioners are reluctant to embrace EBP because of their understandable difficulties in evaluating the increasingly technical literature on psychotherapy process and outcome. Yet with few exceptions, such as the APA newsletter *Clinician's Research Digest* and the ABCT journal *Cognitive and Behavioral Practice* (e.g., see Ritschel, Ramirez, Jones, & Craighead, 2011), virtually no regular publications are available to translate psychotherapy process and outcome findings into nontechnical "bottom-line" conclusions that practitioners can readily digest and use. We therefore call on APA, APS, and other major professional organizations to make the development and dissemination of user-friendly journals that summarize EBP findings for clinicians a substantially higher priority. In addition, we encourage psychologists to develop continuing education courses that focus on providing clinicians with practical skills for interpreting the results of psychotherapy research.

### *Closing Thoughts*

We suspect that some readers may perceive this manuscript as a jeremiad. Because we have delineated a multitude of reasons why many psychologists and other mental health professionals are dubious of EBP, the task ahead of us a field may seem daunting, even hopeless. Our view is far more sanguine. Just as identifying the sources of resistance in psychotherapy can be an invaluable window into unarticulated obstacles that impede client progress (Shea, 1998), pinpointing the sources of resistance to EBP may offer valuable leads for prescriptions to narrow the science-practice gap (see also Ritschel, 2005). Indeed, to the extent that much of the resistance to EBP stems from remediable misconceptions and misunderstandings, it implies that more and better communication

between researchers and clinicians may be a critical first step toward easing such resistance.

In closing, and at the risk of being provocative, we hope that this manuscript can persuade psychologists interested in promoting EBP to think more like psychologists. Specifically, we suggest that psychologists should strive to better understand why many of their fellow colleagues and students are reluctant to embrace EBP. If they were to do so, they might be less likely to dismiss or disregard resistance to EBP, and more likely to come to view the sources of such resistance as pointing the way toward its eventual resolution.

Table 1

## Causes of Spurious Therapeutic Effectiveness (CSTEs)

<i>Name of CSTE</i>	<i>Description of CSTE</i>
<b>Placebo effects</b>	Improvement resulting from the mere expectation of improvement
<b>Novelty effects</b>	Improvement stemming from the prospect of receiving a new intervention
<b>Spontaneous remission</b>	Naturally-occurring improvement in a disorder
<b>Cyclical nature of many disorders</b>	Many conditions are marked by ups and downs
<b>Regression to the mean</b>	Tendency of extreme scores to become less extreme on retesting
<b>Maturation</b>	Naturally-occurring psychological growth
<b>History</b>	Life events transpiring outside of therapy sessions
<b>Effort justification</b>	Psychological need to justify the expense and commitment of a treatment
<b>Multiple treatment interference</b>	Obtaining other treatments in conjunction with the primary treatment
<b>Initial misdiagnosis</b>	Some relatively normal patients are mistakenly diagnosed as psychopathological
<b>Illusory placebo effects</b>	The belief that one has improved even though one has not stemming from expectations of improvement
<b>Demand characteristics</b>	Tendency of clients to tell therapists what they believe their therapists want to hear
<b>Retrospective “rewriting” of pre-treatment functioning</b>	Tendency to recall one’s pre-treatment level of functioning as worse than it was

<b>Absence of knowledge of the effects of the hypothetical counterfactual</b>	Lack of information regarding how the client would have fared without treatment
<b>Selective attrition</b>	Tendency of clients who are not improving to drop out of treatment
<b>Confirmation bias</b>	Tendency to seek out and interpret evidence consistent with positive client outcomes, and to deny, dismiss, or distort evidence that is not

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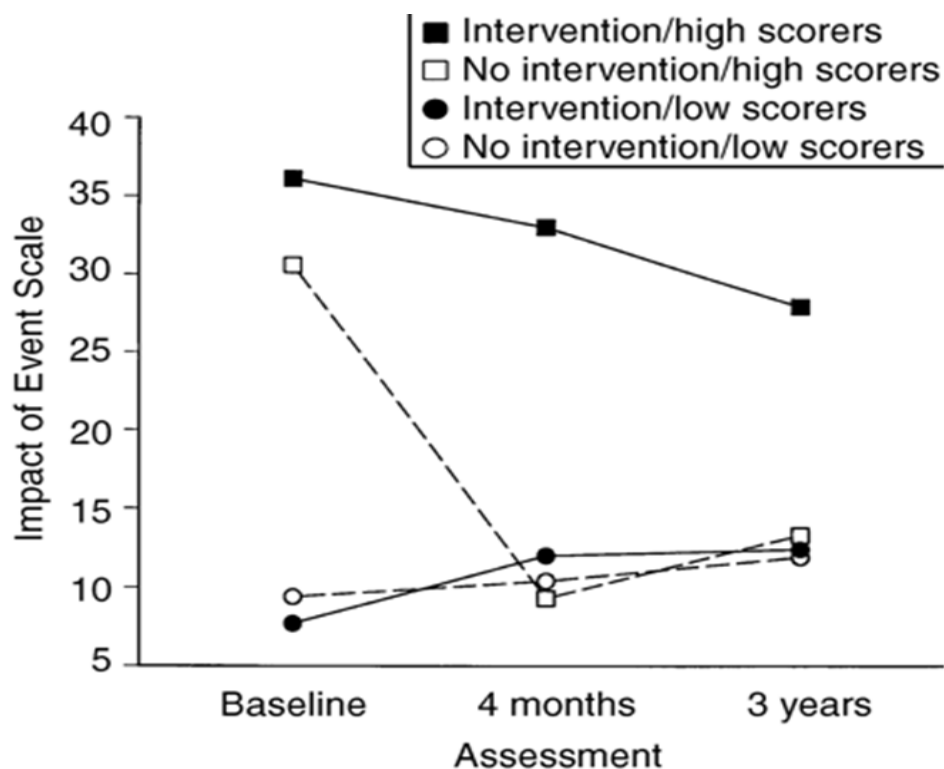
Table 2

Widespread Mischaracterizations of Evidence-based Practice (EBP)

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- (1) EBP stifles innovativeness in the development of new treatments
- (2) (EBP requires a “cookie-cutter,” “one-size-fits-all” approach to treatment
- (3) EBP excludes nonspecific influences in therapy
- (4) EBP does not generalize to individuals who have not been examined in controlled studies.
- (5) EBP neglects evidence other than randomized controlled trials
- (6) EBP is unnecessary because all treatments are equally efficacious
- (7) EBP is inherently limited because therapeutic changes “cannot be quantified”
- (8) EBP is erroneous because human behavior is impossible to predict with certainty





**Figure 1.** The effects of critical incident stress debriefing on posttraumatic stress symptoms among traffic accident victims. Note the striking difference in trajectories between high scorers who did and did not receive the intervention. Both groups improved, but the group that received the intervention would have improved more had they received no intervention at all. From Mayou et al. (2000). Reprinted with permission.

