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## WHAT QUALIFIES AS EVIDENCE OF EFFECTIVE PRACTICE?

### Clinical Expertise

*Geoffrey M. Reed*

The most widely cited definition of evidence-based practice (EBP) is one adapted from Sackett and colleagues (2000) by the Institute of Medicine (2001):

*Evidence-based practice* is the integration of best research evidence with clinical expertise and patient values. *Best research evidence* refers to clinically relevant research, often from the basic health and medical sciences, but especially from patient centered clinical research . . . *Clinical expertise* means the ability to use clinical skills and past experience to rapidly identify each patient's unique health state and diagnosis, individual risks and benefits of potential interventions, and personal values and expectations. *Patient values* refers to the unique preferences, concerns and expectations that each patient brings to a clinical encounter and that must be integrated into clinical decisions if they are to serve the patient. (p. 147)

This definition speaks to an integration of three components, without implying that one is privileged over another, and is one that few practicing psychologists would disagree with as a model for practice. Yet practitioners who have been following the discussion on EBP are chagrined to find themselves regularly portrayed as either idiots or charlatans. A relatively typical statement is "that most of the theories and approaches that are used within the community of practitioners are unsupported by empirical evidence" (Beutler, 2000, ¶ 2). Although such statements may not be untrue according to the narrowest definition of evidence, they are generally offered without additional context and therefore as having much broader implications. Psychological treatments as they are routinely practiced in the community are characterized as being on the basis of beliefs, preferences, and unvalidated theories, as were "such widely varied practices as blood-letting, demonology, the inquisition, the holocaust, and the crusades as well as every other destructive political and religious philosophy" (Beutler, 2000, ¶ 3).

Carter (2002) characterized her experience of the discussion in this way:

I keep waiting to meet the practitioners described by our scientist-academic colleagues . . . . Those practitioners are thoughtless, reckless, cavalier, and do not learn from experience. Since they follow charismatic-leader-driven treatment approaches without thought, they really need to be provided with manuals to tell them exactly what to do when . . . . They do not read, they do not think; and, above all, they have lost all capacity and interest in learning. (p. 1286)

How have we reached the point that intelligent people who are accepted into some of the most competitive doctoral programs in higher education, who complete what is generally a research-based doctoral degree, who undergo a minimum of two years of supervised clinical training, and who are able to pass national and state licensing exams are routinely discussed as though none of their knowledge is legitimate and their practice is no different from voodoo? And how has this come to be a core premise of the most widely touted initiatives in mental health?

This position paper explores some of the health system forces that have been fueling the deprofessionalization of health professionals and how these dynamics have interacted with the EBP movement. I conclude that reasserting and better defining clinical expertise is the key to helping psychology preserve its fundamentally human nature and, at the same time, meet the evidentiary standards necessary to advance health care policies that support our work.

## EBP AS A PUBLIC IDEA

During the 1990s, EBP gained currency as a public idea (Tanenbaum, 2003). By this is meant an idea that both describes a public problem and sug-

gests the wisdom of a particular response. The drunk driver, for example, became the focus of transportation safety efforts when a public idea cast him as the essential menace on American highways (Gusfield, 1981). The idea that drunk drivers cause accidents is not untrue, but accidents also have many other causes. A public idea focuses attention on one aspect of a complex problem and calls for the logical solutions to it.

In the health care arena, policy makers are indeed faced with a complex problem. Approximately 15% of the U.S. population is uninsured (Cohen & Ni, 2004; U.S. Census Bureau News, 2004). An even higher percentage (24%) has no mental health benefits, and only about half of those have coverage that could be considered reasonable (Maxfield, Achman, & Cook, 2004). We spend more per capita on health care than any other industrialized nation, yet we do not provide demonstrably better care (World Health Organization, 2001). Care is fragmented, with little coordination horizontally across systems or vertically among levels of care (Institute of Medicine, 2001). Health care costs also continue to rise. The future costs of entitlement programs, including Medicare, promise to create unacceptable burdens for the next generation. These problems seem intractable in the face of vested interests that oppose particular solutions.

Instead, Americans have been offered the public idea that the essential problem with the U.S. health care system is uninformed practice, which would be resolved if health care professionals practiced in ways that are consistent with research findings. This is the basic premise of EBP. Tanenbaum (2003) pointed out that EBP's potency as a public idea is based in part on its powerful rhetoric:

It is, in fact, a rhetorical triumph, for who can argue with evidence? Critics of EBP literally have nothing to call themselves or their position; it is not evidence, but the limitations of certain evidence hierarchies they oppose . . . . Moreover, the rhetoric of EBP raises an important question in the listener's mind: if EBP is the introduction of evidence into practice, how have clinicians been practicing all along? What is there besides evidence? . . . Even if the public never gets specifics, however, it should be clear to them that clinicians are in the wrong. (p. 294)

EBP offers a further justification for the lay management of professional behavior that has been an operating principle of managed care. The way in which EBP as a public idea links problem and solution is not questioned. In a report of a national program to assess the applications of EBP to managed care, Keckley (2003) is typical in his assertion that the impact of clinicians not following evidence-based guidelines results in suboptimal care for patients and avoidable costs due to high levels of inappropriate variability in treatment patterns. Also typically, he provides no evidence that this is the case. He proposes that EBP can be a mechanism for managed care to improve its image among stakeholders and members. EBP can be "the fundamental

basis for managing cost as well as quality” (p. 3) by providing a basis for coverage limitations and denials. This time the justification is framed in the language of science: the results of empirical research—especially randomized controlled trials—should override the judgment of the health professional in the treatment of any particular individual.

This perspective has been given legitimacy by clinical researchers eager to define the essential problem in health care services as the inadequate consumption and application of the research literature by clinicians. They argue that practice outcomes would be enhanced if psychotherapists limited their practice to the use of treatments that have substantial evidence of efficacy (e.g., Carpinello, Rosenberg, Stone, Schwager, & Felton, 2002; Chambless et al., 1996; Chorpita et al., 2002; Lampropoulos & Spengler, 2002; Nathan & Gorman, 1998). This chorus has been joined by federal research agencies eager to claim that their portfolios offer substantial public benefit and by service agencies that claim they hold the key to a system that works.

Health professionals’ resistance is generally described as the major barrier to implementing EBP (e.g., Keckley, 2003). As a result, substantial resources are being devoted to programs aimed at increasing practitioner uptake of research-based services, in spite of the fact that virtually no evidence supports the underlying assumption that their implementation will improve health care outcomes. One example is a major joint initiative of the National Institute of Mental Health (NIMH) and the Department of Health and Human Services (DHHS) Substance Abuse and Mental Health Services Administration (SAMHSA) focusing on promoting and supporting the implementation of evidence-based mental health treatment practices into state mental health systems (e.g., National Institutes of Health, 2004). This initiative focuses on identifying the most effective and feasible methods for implementing EBP in state clinical practice settings and it also provides direct support to states and localities that are ready and committed to adopting EBP.

### ARE RESEARCHERS A PART OF THE PROBLEM?

One question that may be asked is the extent to which clinical researchers have collaborated with or been used by managed care in its effort to deprofessionalize clinicians by removing their authority over treatment. Clearly, certain individuals denigrate practicing psychologists and discredit their treatments to practically anyone else who will listen. However, I believe they are a tiny (though vocal) minority and extremist subgroup that receives attention out of proportion with its number, often because its accusations make good media copy.

The best clinical scientists, however, have been engaged in different projects. For them, the central goal of the EBP movement is to demonstrate

to external decision makers and stakeholders that psychological interventions have a scientific base and finally to put psychology on a par with medicine (see Tanenbaum, 2003, 2005). These researchers have been particularly concerned about widely disseminated practice guidelines that recommend the use of medications over psychological interventions in the absence of data supporting such recommendations (Barlow, 1996; Beutler, 1998; Muñoz, Hollon, McGrath, Rehm, & VandenBos, 1994; Nathan, 1998). Barlow (in press), in a forward-looking article, makes the case for the implementation of psychological treatments as mainstream health interventions in health systems around the world. He reviews data for psychological treatments for depression and panic disorder, for example, that meet the most stringent evidentiary criteria and that have been published in the most prestigious medical journals (e.g., Barlow, Gorman, Shear, & Woods, 2000; Keller et al., 2000).

However, making the case successfully for psychological interventions within the broader medical system requires meeting evidentiary standards as they are viewed by medicine (Davidson et al., 2003). Both health care policy makers and physicians are almost universally unfriendly to the argument that the gold standard of randomized clinical trials (RCTs) is not always the best model for investigating psychological interventions. From their point of view, the general message that “psychotherapy works” is equivalent to saying that “medication works.” The finding that only a small proportion of outcome is predicted by intervention techniques makes it sound as though clinicians are not doing anything specifically important, and therefore that anyone could do it. Data regarding the importance of the therapeutic alliance may be seen as interesting, but is uninformative about what treatment should be offered to whom.

Although psychologists often view the purpose of RCTs as establishing causality, medical researchers and health care policy makers view the purpose of RCTs as providing a basis for health policy. At what age should which groups of women begin routine mammograms for breast cancer? At what cholesterol levels do statin drugs reduce cardiac risk in men? For example, on the basis of recent RCTs, routine hormone replacement therapy—for many years the most recommended treatment in North America—is now known to cause substantial harm in women by significantly increasing breast cancer rates (Chlebowski et al., 2003) and elevating cardiac risk (Manson et al., 2003). As a result, rates of hormone replacement therapy have plummeted (Hersh, Stefanick, & Stafford, 2004).

Unfortunately, adopting this framework in psychology has also meant accepting the “hierarchy of evidence” that evidence-based medicine has taken as its foundation (Sackett et al., 2000). An unfortunate consequence has been an enormous devaluation of the body of professional knowledge in our field. Psychological treatment as it is generally practiced in the community is fluid, self-corrective, not of fixed duration, and individualized

(Seligman, 1995). By definition, such treatment is vastly more difficult or impossible to support using RCT designs. Whether a treatment is deemed effective, then, is largely based on its compatibility with particular research methods (Tanenbaum, 2005). Further, manualization and evidence are confounded by using manualization as a criterion for whether psychological treatments can be evidence based (Chambless et al., 1996).

Widespread confusion exists in the public discussion of treatments that have been empirically disconfirmed with those that have not been tested using the methods emphasized by EBP, and a lack of clarity on this point also characterizes much of the professional literature (Westen, Novotny, & Thompson-Brenner, 2004). In corollary fashion, confusion occurs between treatments that are the “best available” and those that are more compatible with RCTs. For example, we do not in fact know that cognitive—behavior therapy (CBT) and interpersonal therapy are the most effective psychological treatments for depression, but only that these manualized, brief treatments are easier to test using RCT methodologies than alternative approaches. This has also led to a view of manuals as prescriptive and constitutive rather than representative of treatment, and it also contributes to a view of “psychotherapy as the job of paraprofessionals who cannot—and should not—exercise clinical judgment in selecting interventions or interpreting the data of clinical observation” (Westen et al., 2004, p. 639).

The challenge will be for psychology to have it both ways: to preserve the essentially human and individual nature of psychological work and, at that same time, meet the evidentiary standards that will be required of us to flourish in the health care system. Tanenbaum (2005) framed the policy-relevant question as

Can EBP in mental health commit itself to an inclusive enough evidence hierarchy not to privilege technique unfairly over relationship? Can it do so without further stigmatizing psychology vis-à-vis medicine (including psychopharmacology), thus undermining mental health care’s claim to effectiveness worthy of funding? (p. 166)

## CLINICAL EXPERTISE: BACK TO THE FUTURE?

I believe that psychology can best overcome this challenge through a data-based reassertion of the importance of clinical expertise. Psychology’s authority as a health profession is based on a body of specialized professional knowledge, which, together with a practitioner’s individual experiences, forms the basis of a practitioner’s clinical expertise. In EBP’s most literal interpretation, a clinician’s experience contributes little to, and is likely to detract from, appropriate treatment. It is ridiculous to imagine, however, that, faced with choosing a course of treatment for his mother’s life-

threatening illness, even the most ardent advocate of EBP would follow the advice of an intern who quoted the results of a single RCT over that of a widely respected physician who has treated 100 similar cases with successful results. It is obvious and inevitable that clinical expertise is of paramount importance in any form of health care, including psychological care.

Clinical expertise is certainly informed by controlled research, but controlled research may be unavailable, inconclusive, conflicting in conclusions, or misleading. A relevant example in psychology is an earlier report (Dobson, 1989) of a meta-analysis of 28 studies that reported cognitive therapy for depression had a greater effect than other treatments, including pharmacotherapy, behavior therapy, and other forms of psychotherapy. Later, Luborsky and colleagues (1999) conducted a meta-analysis of 29 treatment comparison studies in depression, incorporating measures of researchers' theoretical allegiance. They found that researcher allegiance accounted for two thirds of the variance in reported treatment differences. Cognitive therapists tended to find superior results for cognitive therapy, whereas psychodynamic researchers found more impressive results for more dynamic treatments. Not surprisingly, this phenomenon also extends to those with allegiance to the biomedical viewpoint who insist that pharmacotherapy is superior to psychotherapy in treating depression, in spite of substantial evidence from psychological research to the contrary (e.g., DeRubeis et al., in press; Hollon et al., in press).

Efficacy research should not be the sole basis for setting policies about what types of services should be encouraged or restricted. The rapid implementation of health care policies discouraging other forms of treatment than cognitive therapy on the basis of Dobson's 1989 review would likely have deprived Luborsky and colleagues of the data necessary to come to a corrective conclusion 10 years later. More important, it would have inappropriately restricted access to effective psychological services among a population that constitutes a major portion of U.S. disease burden and is dramatically undertreated (Kessler et al., 2003; World Health Organization World Mental Health Survey Consortium, 2004). In the context of other populations such as children and the persistently mentally ill, a tremendous need for interventions exists, but relatively few treatments meet the most narrowly defined criteria of evidence because of the difficulties associated with such documentation. An overemphasis on RCTs as evidence is likely to support the application of treatments that are relatively easy to study using this method, in particular the use of medication, which may not be in the best interests of these vulnerable populations. In some cases, an overreliance on controlled research may actually limit the development of cutting-edge treatments that are demonstrably better in terms of individual outcomes but that have not yet been tested in larger controlled designs. Warwick, a pediatrician whose clinic has achieved huge gains in the life expectancy of individuals

with cystic fibrosis (more than 47 years as compared to a national average at research-based centers of 33 years) says that national research-based guidelines are “a record of the past, and little more—they should have an expiration date” (Gawande, 2004, p. 82).

The appropriate application of research findings in real-world health care is a primary subject of clinical expertise. Goodheart (2004a) defined clinical expertise in psychology as the ability to integrate knowledge, experience, technical and relational skill, critical thinking, prediction, decision-making, and self-assessment within a fluid situation that often is uncertain and ambiguous. Clinical expertise is required to evaluate the client’s clinical state, prioritize treatment needs, form a therapeutic alliance with the patient, select appropriate interventions and evaluate their potential risks and benefits for the individual patient, apply these interventions skillfully, monitor progress, and adjust treatment accordingly. Clinical expertise is also necessary for effective communication with the patient and relevant others to assess the patient and family’s goals, values, choices, and desired role in treatment. Clinical expertise also entails considering an array of client factors, including age and developmental stage, culture, language, social class, personality (e.g., strengths and limitations, as well as coping style), behavioral factors (e.g., health risk behaviors and the ability to comply with complex treatments), and relevant systems factors (e.g., family and school).

It is impossible for research to direct clinical decision-making at this level of specificity. For example, research is largely unavailable regarding the impact of most psychological interventions on U.S. minority populations. The hard-line EBP stance would suggest that this makes no difference in treatment selection. I once heard a prominent researcher argue that the group psychological interventions for women with breast cancer that had been developed and tested primarily with a middle-class White population should initially be applied without modification by a community-based organization working with Spanish-speaking immigrant women because evidence exists for the intervention as previously applied and no evidence has been provided for a modification. Such a recommendation would strike most clinicians as bizarre and certainly not based on a consideration of the relevant evidence. It ignores the principles and research of community and multicultural psychology; evidence regarding the enormous impact of language, culture, and context on beliefs about illness and interactions with the health care system; and clinical knowledge regarding the importance of respect for culture and context.

The researcher’s recommendation is a direct consequence of inappropriately emphasizing and overgeneralizing from RCTs. RCTs yield group-level probabilities, often on the basis of very narrowly defined groups. Available data indicate that substantial differences can be found between patient samples enrolled in clinical trials and those seen in clinical practice (Zarin, Young, & West, *in press*), but EBP has tended to assume away the



inferential leap required to apply aggregate findings to individual cases (Tanenbaum, 1999). EBP suggests that treatment of the next individual patient can be directly determined by the calculated probability of an outcome associated with specific therapy techniques. Given that other factors are likely to be more important in determining outcome—that is, the patient, the clinician, the culture and context, the therapeutic relationship, and the match between patient and treatment—a practice that is truly evidence-based has to consider all of them.

Critics of clinical decision-making (e.g., Garb, 1998; Meehl, 1954) have tended to emphasize that clinicians are subject to information-processing biases and heuristics in aggregating data and making predictions on the basis of them, as, indeed, is everyone (Kahneman & Tversky, 1973; Nisbett & Ross, 1980). These critiques generally assume that the central goal of clinical decision-making is to predict behavior (Westen & Weinberger, in press). Indeed, statistical prediction generally outperforms clinical prediction in predicting relatively broad or distal behavioral outcomes, and clinical experience does not confer much benefit in such prediction. However, with the exception of specific legal situations, behavioral prediction is not the focus of clinical training or clinical work. Westen and Weinberger (in press) review data that make a strong argument for clinical expertise related to decisions that more closely approximate clinical training and clinical work, including judgments at moderate levels of inference, judgments in contexts in which clinicians are likely to develop expertise (e.g., diagnosis and intervention), and conditions that optimize the expression of expertise (e.g., psychometric instruments designed for expert observers).

Critiques of clinical decision-making also often focus on questions about the predictive validity of certain psychological tests (e.g., Garb, 1998). However, although a wide array of evidence indicates that psychological tests are at least as predictive of a variety of important outcomes as many medical tests (Meyer et al., 2001), this is a separate question from whether the decisions that clinicians make as a part of their assessments and treatments lead to maximally helpful results for their patients. With specific regard to diagnosis, much attention has been devoted to the lack of interrater reliability among clinicians in making a diagnosis on the basis of the *Diagnostic and Statistical Manual of Mental Disorders, Fourth Edition (DSM-IV)* (American Psychiatric Association, 1994), particularly as compared to structured interviews (e.g., Basco et al., 2000; Ventura, Liberman, Green, Shaner, & Mintz, 1998). However, although the fine-grained distinctions of the *DSM-IV* may be important for the construction of homogenous patient groups for research, many are of little relevance to clinicians in making or implementing treatment recommendations. As Westen and Weinberger stated (in press, p. 7), "From a strictly empirical perspective, we are aware of no evidence that patients who fall just below or just above threshold for the categorical diagnosis of any *DSM-IV* diagnosis respond differently to any form of treatment,

have different etiologies, or differ in any other important respect." A lack of diagnostic reliability may be more properly seen as a problem with the clinical validity of *DSM-IV* rather than with clinical judgment.

Perhaps more relevant to the question of clinical expertise are findings about differences in decision-making between novices and experts that have been tested across many content areas (see Bransford, Brown, & Cocking, 1999). Goodheart (2004a) summarized them as follows: First, experts have acquired extensive content knowledge, organize it in a way that reflects deep understanding, and are able to retrieve elements of this knowledge with little attentional effort. Experts are flexible in their approach to new situations and attend to features and patterns that are not noticed by novices, of whom rote rule-following is more characteristic (Klein, 1998). Expert knowledge cannot be reduced to a set of facts or propositions but is linked to contexts of applicability. Finally, in spite of their level of knowledge, experts may not be able to communicate that knowledge and how they use it clearly to others. This point has relevance for the present discussion.

Recent evidence from RCTs supports the importance of therapist experience and expertise in maximizing therapeutic outcomes (e.g., Huppert et al., 2001; Klein et al., 2003). For example, when Jacobson and Hollon (1996) examined the data from the only RCT that found medications to be more effective than cognitive therapy in treating depression, they found that the site with the most experienced cognitive therapists achieved outcomes that were equal to those of medication, whereas the sites with less experienced therapists did less well. Specific strategies that experienced therapists use in response to individual patients are important in determining outcomes (Anderson & Strupp, 1996; Beutler, Moleiro, & Talebi, 2002; Castonguay, Goldfried, Wisner, Raue, & Hayes, 1996). Available data suggest that rigid adherence to a treatment manual may detract from the therapeutic alliance by increasing the frequency and severity of therapeutic ruptures (Norcross & Hill, 2004), resulting in poorer therapy outcomes (Wampold, 2001).

In an editorial in the *Evidence-Based Medicine Notebook*, Haynes, Devereaux, and Gordon (2002) indicated that early conceptualizations of expertise focused on identifying the relevant research and applying it, deemphasizing other factors in clinical decision-making. They proposed a new model that locates clinical expertise at the intersection of the patient's clinical state and circumstances, the patient's preferences and actions, and the research evidence. In the psychotherapy arena, Beutler (2000) listed five therapeutic competencies: (a) communicating an attitude that is conducive to a therapeutic relationship; (b) therapeutic knowledge of principles of change; (c) therapeutic skill to implement effective techniques; (d) being sensitive to the demands and uses of time in selecting interventions; and (e) using creative imagination when established techniques are not available. Norcross and Hill (2004) similarly offered research-based descriptions and recommendations related to psychotherapy relationships.

EBPs require multiple streams of evidence, always including clinician expertise (Goodheart, 2004a). It is important to recognize that much contemporary clinical research, and the best discussions of EBP, do not support cookbook, one-size-fits-all models of treatment. More sophisticated and experientially accurate models of clinical expertise will shift our understanding of the uses of controlled research. Research is informative, but it can rarely be prescriptive in its application to an individual patient. For the most part, clinical research is most helpfully viewed as a way to assess interventions that may prove useful at particular junctures in work that is guided by clinical expertise. Framing good psychological practice, including a sophisticated understanding of clinician expertise, in terms that draw on the empirical literature will be important in reclaiming respect for the professional role and in advancing the profession in the current climate of health care.

## Scientific Research

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Scientific research is the only process by which clinical psychologists and mental-health practitioners should determine what “evidence” guides EBPs.

### THE BACKGROUND IN EVIDENCE-BASED MEDICINE

When the *New York Times* listed “evidence-based medicine” as one of the breakthrough ideas of 2001, many of its readers probably thought, “As opposed to *what?* Are there any medical treatments that are *not* evidence based?” (Hitt, 2001). The simple, straightforward answer to this question is “Yes.” Although the medical profession has long cloaked itself with the mantle of science, the fact is that until surprisingly recently physicians had relatively few effective treatments for disease. The available treatments were mostly palliative in nature, intended to ameliorate the patient’s symptoms, and make the patient comfortable while nature took its course, or else physicians simply removed diseased organs and tissues through surgery. In a very real sense, scientific medicine really only began in the latter part of the 19th century (about the same time scientific psychology began) with the laboratory revolution of Claude Bernard, and the microbe-hunting of Louis

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Pasteur and Robert Koch, followed by successive phases of the pharmaceutical revolution of the 20th century (Magner, 1992; Porter, 1997).

Nevertheless, almost 150 years after Bernard, and more than a century after Pasteur and Koch, the *Times* article cited a recent estimate that only about 20% of common medical practices were “based on rigorous research evidence,” as opposed to being “a kind of folklore” (Hitt, 2001, p. 68). It is only in the last few years that researchers have begun to systematically evaluate medical practices to determine whether they actually work, which ones work better than others, and which are cost effective (Davidoff, Haynes, Sackett, & Smith, 1995; Evidence-Based Medicine Working Group, 1992; Rosenberg & Donald, 1995; Sackett, Straus, Richardson, Rosenberg, & Haynes, 1997). But now evidence-based medicine—defined as “the conscientious, explicit and judicious use of current best evidence in making decisions about the care of individual patients” (Sackett, Rosenberg, Muir-Gray, Haynes, & Richardson, 1996, p. 71), and more broadly renamed as EBPs (Institute of Medicine, 2001)—is the way medicine increasingly does business.

## SCIENCE, PSYCHOTHERAPY, AND MANAGED CARE

We can trace a parallel history in psychology. Clinical psychology owes its professional status, including its autonomy from psychiatry and its eligibility for third-party payments, to the assumption that its procedures for diagnosis, treatment, and prevention are based on a substantial body of scientific evidence. But for a long time after the invention of psychotherapy in the latter part of the 19th century, this assumption simply went unchecked. It must have been a shock when, reviewing the paltry literature then available, Eysenck cast doubt on the proposition that psychotherapy had any positive effect at all, over and above spontaneous remission (Eysenck, 1952). It was certainly not good news for a profession facing competition from the first generation of psychotropic drugs, including lithium (introduced in 1949), the phenothiazines, imipramine, Miltown, and other benzodiazepines. For an embarrassingly long time afterward, the chief counterweight to Eysenck's expose was the assertion that psychotherapy did have effects after all, but that the negative effects balanced the positive ones, creating an illusion of no change (Bergin, 1966). It took another 25 years and the development of new meta-analytic techniques, which not only provided quantitative summaries of data trends but also enabled investigators to aggregate weak effects into strong ones, for researchers to demonstrate that psychotherapy did, in fact, on average, have a greater positive effect than nothing at all (see also Lispey & Wilson, 1993; Smith & Glass, 1977; Smith, Glass, & Miller, 1980).

One positive legacy of Eysenck's expose, and Bergin's rejoinders to it, was research intended not only to demonstrate that psychotherapy did work

after all but to identify conditions, and techniques, that would magnify the positive outcomes of psychotherapy and minimize the negative ones (Bergin & Strupp, 1970; Fiske et al., 1970; Garfield & Bergin, 1971; Strupp & Bergin, 1969). Following the rise of psychotropic drugs, the professional landscape within psychotherapy became even more competitive with the emergence of behavioral (Wolpe, 1958) and cognitive (Beck, 1970) therapies to rival more traditional psychodynamic and client-centered treatments. The first generation of behavioral and cognitive therapists took clinical psychology's scientific rhetoric seriously, and systematically set about to demonstrate the effectiveness of what they did (Yates, 1970). By the time that Smith and Glass did their meta-analysis, it did indeed seem that the CBTs were able to deliver the goods in a way that more traditional insight-oriented approaches did not. Although some observers concluded from the Smith and Glass that "everyone has won and so all must have prizes" (Luborsky, Singer, & Luborsky, 1975), this was not really the case. Even in the Smith and Glass study, the effect sizes associated with cognitive and behavioral therapies were larger than those associated with psychodynamic and humanistic ones (Smith & Glass, 1977; Smith et al., 1980). Over the succeeding years, the CBTs have gradually emerged as the standard of psychotherapeutic care.

Still, the analysis of Smith and Glass (Smith & Glass, 1977; Smith et al., 1980) suggested that there was enough success to go around, and that would probably have been enough to permit psychoanalysts, Rogerians, and behavior therapists alike to enjoy good professional livelihoods, except that the professional landscape changed once again, with the rise of health maintenance organizations and other forms of managed care. Patients and clients can pay for whatever treatment they want out of their own pockets, regardless of whether it works well or efficiently, so long as they believe they are getting some benefit—or are persuaded that some benefit will ultimately accrue to them. But when third parties foot the bill (patients and therapists are the first and second parties), strong demands for professional accountability come with the package, and this is no less true for mental health care than it is for the rest of the health care industry (Kihlstrom & Kihlstrom, 1998). As a result, the demands of managed care have combined with the rhetoric of science, and competition from both cognitive and behavioral therapy and psychotropic drugs is fostering the development of standards for ESTs (Chambless & Ollendick, 2001; Task Force, 1995) or, again, more broadly, EBP within clinical psychology.

### "EFFICACY" AND "EFFECTIVENESS"

Viewed from a historical perspective, EBPs are something that clinical psychology should have been striving for, and promoting, all along, and they

have a real flavor of historical inevitability to them. Remarkably, though, at least from the standpoint of a profession that takes pride in its scientific base, there has been considerable resistance to the demand for EBPs. As tempting as it might be to dismiss this resistance as coming from private-practice entrepreneurs who simply want to continue doing what they've always done and resent any infringements on their livelihoods, I suspect things are more complicated than that. Just as some well-intentioned physicians have bridled at having their clinical judgment checked by managed-care bureaucrats, some well-intentioned psychotherapists argue against any standards or guidelines at all, on the grounds that they should be free to pick whatever treatment they think will be best for the individual patient. But physicians don't have this freedom; they have to conform their practices to the available evidence, and where evidence is lacking, to the prevailing standard of care. Why should psychotherapists be any different?

Other resisters, including some clinical scientists, believe that the "efficacy" research that provides the basis for EBPs is inappropriate, or at least insufficient, because the studies are conducted under somewhat artificial conditions that do not represent the problems encountered in actual practice (e.g., Levant, 2004; Seligman, 1995; Seligman & Levant, 1998; Westen & Morrison, 2001; Westen, Novotny, & Thompson-Brenner, 2004). Instead, they propose that ESTs be based on "effectiveness" research, which, they argue, is more ecologically valid. But the distinction between efficacy research and effectiveness research seems strained. Research is research. Clinical drug trials are somewhat artificial too, but their artificiality does not prevent physicians from prescribing effective drugs in actual practice, based in large part on carefully controlled studies that show that the drugs in question really do improve the conditions being treated.

To the extent that effectiveness research attempts to extend the logic of efficacy research to more ecologically valid treatment settings—studying patients with comorbid conditions, for example, or with diagnoses on Axis II as well as Axis I, or more extended treatments—no essential difference exists between the two. But to the extent that effectiveness research loosens the standards for methodological rigor characteristic of efficacy research, then effectiveness research is a step backwards. In the *Consumer Reports* study, for example (*Consumer Reports*, 1995; Kotkin, Daviet, & Gurin, 1996; Seligman, 1995), the outcome of psychotherapy was measured by patients' self-reported satisfaction with their treatment, instead of objective evidence of actual improvement. There were no controls for a sampling bias, nor any untreated control group—a particularly egregious problem in the wake of Eysenck's (1952) analysis. It did not ask about the specificity of treatments, a question critical to distinguishing a genuine effect of psychotherapy from placebo, and for evaluating the differential effectiveness of various forms of therapy.

If the *Consumer Reports* study is an example of effectiveness research, then effectiveness research is a step backward, not a step forward, in the jour-

ney toward evidence-based treatments. Efficacy research, modeled on randomized clinical trials in drug research, is a good place to begin research on psychotherapy outcomes. Any deficiencies that efficacy studies might have with respect to ecological validity, deficiencies that might be remedied in the future by properly designed and controlled effectiveness studies, should not be taken as an excuse for discounting them in the meantime.

## RATCHETING UP THE STANDARDS

At present, the standards for EBP in psychotherapy are roughly modeled on the clinical trials required before drugs are marketed (Chambless & Ollendick, 2001). To qualify as "empirically supported" on the list maintained by the Society of Clinical Psychology (Division 12, Section III, of the APA), a treatment must yield outcomes significantly better than those associated with an adequate control (typically, patients who receive no treatment at all) in at least two studies, preferably conducted by independent research groups. These standards are a good start for putting psychotherapy, at long last, on a firm scientific base, but they are also somewhat minimal, and over time they should be progressively ratcheted up (the opposite of defining them down; Moynihan, 1993) to improve the quality of psychotherapeutic practice.

For example, two studies out of how many? The current EST standard is modeled on current Food and Drug Administration (FDA) standards, which require only two positive trials, regardless of how many negative or inconclusive trials there are, raising the file-drawer problem and the issue of selective publication of positive results. Just as the medical community is ratcheting up this requirement by requiring drug companies to preregister all drug trials as a condition of accepting reports of them for publication (Vedantam, 2004), so we might find a way to register ongoing psychotherapy outcome studies before their results are in. Certainly, this is possible for major, collaborative studies supported by federal funds.

More substantively, we might wish to drop the no-treatment control as an appropriate comparison group in favor of either an appropriate placebo or some alternative treatment. It is something to prove that psychotherapy is better than nothing, but surely it is not much. Placebo controls are not easy to implement in psychotherapy research, because it is difficult to keep psychotherapists blind to the treatment they are delivering. In drug research, especially when ethical concerns have arisen about the use of placebo controls, new medications may be evaluated against the current standard of care instead. If a new drug is not discriminably better than what is already available, and certainly if it is discriminably worse, then it is incumbent on its proponents to show that it is a reasonable alternative treatment for some individuals, for whom the currently available medications are ineffective or

inappropriate. An example of such a comparison might be the NIMH Treatment of Depression Collaborative Research Program (TDCRP), where the antidepressant drug imipramine might be construed as the established standard of (medical) care, and psychotherapy as the alternative, as well as, for that matter, CBT as the alternative to the more established interpersonal therapy (Elkin et al., 1989).

Next is the matter of how to evaluate the significance of outcomes. Long ago, Jacobson and his colleagues pointed out that a statistically significant change in some criterion measure may not reflect a clinically significant change in terms of the patient's status (Jacobson, Follette, & Revenstorf, 1984; Jacobson & Revenstorf, 1988). The question is, what are the standards for clinical significance? Although I continue to believe (Kihlstrom, 1998) that the null-hypothesis statistical test is the foundation of principled argument in psychology (Abelson, 1995), psychotherapy outcome is one case where effect sizes really *are* preferable to tests of statistical significance (Cohen, 1990, 1994). Although even small effects can be practically significant (Rosenthal, 1990), there is no question that big effects are better, and probably more significant clinically as well.

One reasonable standard for clinical significance is that a patient who enters psychotherapy by virtue of receiving a diagnosis of mental disorder should no longer qualify for that diagnosis at the end of treatment. Accordingly, Jacobson and his colleagues suggested that the outcome of psychotherapy be deemed successful if the treated patient's scores on some criterion measure fall within normal limits (e.g., within 2 *SD* of the population mean), more than 2 *SD* of the untreated patient mean, or preferably both (Jacobson et al., 1984; Jacobson & Revenstorf, 1988). Such standards are occasionally applied to the evaluation of therapeutic outcomes, including the TDCRP (Ogles, Lambert, & Sawyer, 1995). Of course, it might turn out that some mental disorders are chronic in nature, meaning that a cure, so defined, is impossible. Even so, clinically relevant standards for evaluating outcome in the treatment of chronic mental disorders might be modeled on evolving procedures for evaluating the management of chronic physical illnesses such as asthma or diabetes (Fox & Fama, 1996).

Again, this is a start, but one can imagine at least two improvements. One is to assess outcomes in terms of laboratory measures of mental and behavioral functioning, instead of symptoms, especially self-reported symptoms. In the TDCRP, for example, outcomes were measured by patients' scores on the Beck Depression Inventory (BDI), the Hamilton Rating Scale of Depression (HRSD), and the Hopkins Symptom Checklist. But although the diagnosis of mental disorder (as represented by *DSM-IV*) is based on signs and symptoms, just as it was in the 19th century, in the rest of health care the diagnosis of illness and the evaluation of treatment outcome are increasingly based on the results of objective laboratory tests, such as blood tests and radi-



ological scans, interpreted in light of an increasingly sophisticated understanding of normal structure and function. It is long past time (Kihlstrom & Nasby, 1981; Nasby & Kihlstrom, 1986) that psychology began to move away from questionnaires and rating scales, and toward a new generation of assessment procedures on objective laboratory tests of psychopathology (Kihlstrom, 2002b).

The interest of third-party payers in the outcome of both treatment and disease management suggests yet another more macroscopic approach to the evaluation of outcomes, which is to assess how the treated patient fares in the ordinary course of everyday living. Couples who go through marital therapy might reasonably expect to have happier children than they did before, and employers who pay for their employees to participate in alcohol or drug-abuse treatment programs might reasonably ask if their employees do, in fact, become more productive after treatment. These examples remind us that other stakeholders are involved in the treatment process than just the patients themselves, and that their evaluation of treatment outcome also counts.

As an example of what might be done, Rosenblatt and Attkisson have proposed a conceptual framework in which outcome evaluation proceeds along three dimensions (Rosenblatt & Attkisson, 1993): the respondent (the patient, family members, social acquaintances, the therapist, or an independent evaluator), the social context (personal, family, work or school, and community), and domain (clinical status, functional status, life satisfaction and fulfillment, and safety and welfare). So, for example, in addition to measuring clinical status with scales such as the BDI or HRSD, we could evaluate the degree to which the patients' families and coworkers notice a difference (Sechrest, McKnight, & McKnight, 1996) after treatment, or the degree to which these "third parties" feel that their own life satisfaction has improved. Such a proposal transcends quibbles about the quantitative threshold for clinical significance and brings qualitative considerations of ecological validity into the measure of treatment outcome.

Finally, it should be understood that EBPs include more than *treatments*; they also include the procedures by which patients are diagnosed and treatment outcomes are assessed. Many of the assessment techniques traditionally used by clinical psychologists (Rapaport, Gill, & Schafer, 1968) appear to rest on a surprisingly weak evidentiary base (Wood, Nezworski, Lilienfeld, & Garb, 2003). We need to extend the logic of EBPs to assessment as well as treatment, establishing and improving the validity of our current techniques, and abandoning those that do not pass muster. Moreover, it should go without saying that the logic of EBPs extends beyond clinical psychology to the broader range of professional psychology, including counseling, educational, and industrial/organizational psychology, as well as other domains where scientific knowledge is put into practice.

## THE THEORY BEHIND THE THERAPY

Documenting treatment efficacy is not just a purely empirical matter; theoretical considerations also play a part in the evaluation of any form of treatment. As my spouse once put it in a conversation about an innovative treatment, "What made them think that would work?" It is not enough that a treatment proves empirically to be efficacious. Just as sound medical treatment is based on a scientific understanding of anatomy and physiology, so sound psychotherapy must be based on a scientifically valid understanding of mental and behavioral processes. Here is where placebos and other controls may have their real value—not merely in going one step further than showing that psychotherapy is better than nothing but in evaluating claims concerning the mechanism by which a treatment achieves its effects. If some form of psychotherapy does no better than an appropriate placebo, we can begin to doubt whether that treatment has any specific effects at all. Of course, this assumes that psychotherapy is more than a placebo treatment to begin with (Frank, 1961; Rosenthal & Frank, 1956), which, in fact, is my assumption.

Other kinds of controlled therapy outcome research can also evaluate the scientific validity of certain psychotherapeutic practices. For example, Wolpe's (1958) invention of systematic desensitization was predicated on Hullian learning theory. The only problem was that psychology already had grounds to suspect that Hullian learning theory was not correct (Gleitman, Nachmias, & Neisser, 1954). Fortunately, later research (e.g., Wilson & Davison, 1971) showed that exposure was the active ingredient in systematic desensitization, a conclusion that was consistent with the new, improved, cognitive view of learning that emerged in the 1960s. Along similar lines, a more recent spate of dismantling studies indicates that exposure, not eye movement, is also responsible for the effectiveness of eye-movement desensitization and reprocessing (EMDR; e.g., Lohr, Lilienfeld, Tolin, & Herbert, 1999). Although EMDR may pass the narrowly empirical test for efficacy, claims in its behalf may be undercut by the lack of evidence for its underlying theory.

The point here is that sound treatments are not just those that are empirically supported. Sound treatments are based on scientifically valid theories of mind and behavior. Whenever an innovative therapy is accompanied by a theoretical statement of its underlying mechanism, the therapy should be evaluated not just in terms of whether it works but in terms of its proponents' theory of *why* it works. In this way, we validate the general principles that the treatment is based on and that can form the basis for other therapeutic innovations as well (Rosen & Davison, 2003). We also avoid the trap of using efficacy research to legitimize proprietary, even trademarked, therapies.

To take an example from the history of hypnosis, Mesmer's animal magnetism was not rejected by the Franklin Commission because it did not work (Kihlstrom, 2002a). Everyone agreed that it did work, and, in fact, Mesmer

had previously scored a win for scientific medicine by showing he could duplicate the effectiveness of exorcisms with a technique that was materialist, rather than supernatural, in nature. Animal magnetism was rejected solely because Mesmer's theory was wrong, and nobody had a good theory to replace it (scientific psychology not having been invented yet). Exorcism might work empirically, but even if it did, medicine would reject it as a legitimate treatment because its underlying theory—that disease is caused by demon possession—is inconsistent with everything we know about how the body works.

## SCIENCE AS THE BASIS OF PRACTICE

The examples of Mesmer and hypnosis make it clear that the relation between science and practice is not unidirectional. Studies of psychopathology and psychotherapy can alter our understandings of normal mental and behavioral functions (Kihlstrom, 1979; Kihlstrom & McGlynn, 1991), but they also underscore the point that we want our EBPs not only to be empirically valid but based on valid scientific principles as well. The scientific method is the best way we have of understanding how the world works and why. Therefore, it is also the best way we have of knowing which of our practices work (and why). In establishing the validity of our theories and practices, anecdotal evidence, impressionistic clinical observations, and customer-satisfaction ratings simply will not suffice. Enhancing the scientific basis for clinical practice by determining which practices are scientifically valid and promoting, and letting the others wither away, is the best way that clinical psychology can meet the competition from psychiatry and drugs, and meet the demands for managed care. It is the best way for clinical psychology to promote public welfare. And it is the only way for clinical psychology to achieve its aspirations.

## Patient Values and Preferences

*Stanley B. Messer*

According to the Institute of Medicine's definition of quality health care for the 21st century (2001, p. 147), "Evidence-based practice is the integration of best research evidence with clinical expertise and patient values . . . . Patient values refers to the unique preferences, concerns and expectations that each patient brings to a clinical encounter and that must be

integrated into clinical decisions if they are to serve the patient." Along with clinical expertise, taking account of patients' concerns and satisfaction is the nonevidence-based part of the Institute's definition. The need for research evidence notwithstanding, the framers of this statement recognized that without paying attention to patients' preferences and what they expect from the practitioner, they are served neither fully nor well.

In agreement with this viewpoint, this position paper argues that, despite their utility for some purposes, neither an evidence-based DSM diagnosis nor a manual-based EST is sufficient to treat psychotherapy patients. Diagnoses cannot capture the unique qualities and concerns that patients bring to the clinician nor the specifics of the context in which their problems emerged in the past and are taking place in the present. For that matter, many patients seeking psychotherapy do not carry a formal diagnosis at all, which means that one has to look beyond ESTs for guidance—to empirically supported relationships (ESRs), for example (Norcross, 2002). The strength of ESTs, which are based on randomized clinical trials, or ESRs, which typically rely on correlational data, is their application to patients in general. The clinician, although needing to attend to such empirical findings, must go beyond them to take cognizance of patients' unique qualities, circumstances, and wishes (cf. Goodheart, 2004b). In Beutler's (2004, p. 228) pithy statement, the clinician wants to know, "What can I do, given my own particular strengths and weaknesses, to help this patient, with this problem, at this time?" In other words, nomothetic and idiographic information each have a role to play in clinical practice.

The problems that patients present when first encountering a helping professional may be simply a "calling card." As patients' relationship to the therapist deepens and comfort and trust are more firmly established, the complexity of their lives becomes apparent. The nature of their problems and the specifics for which they are seeking help seem less straightforward. In other words, unless a patient and clinician are intent on treating only the DSM disorder as such, for which there may or may not be a prescribed EST, it is necessary to take a broader view of what troubles the patient. Issues are often subtle or may be unconscious, and patients are frequently conflicted and ambivalent about how to proceed with their lives. Although it has been argued that DSM disorders can be treated sequentially, say, with one EST followed by another (e.g., Wilson, 1998), patient problems, as I will demonstrate, are typically so intertwined as to preclude entirely separate treatments for each problem area.

This chapter portion presents two cases that illustrate the context-specific and complex nature of what clinicians face daily. In these presentations, I underscore the value of attending to unique patient preferences and satisfactions in effective practice. Certainly, clinicians should draw upon their clinical expertise and be knowledgeable about whatever research evidence exists as it applies to the practice of psychotherapy in general. At the

same time, they must be open to patients' special concerns, expectancies, and preferences.

Both of the following two patients presented with symptoms of post-traumatic stress disorder (PTSD). In each case, I drew upon empirical findings and theoretical principles to help treat the PTSD. Nevertheless, as is usually the case, therapy was not straightforward. The nature of the PTSD did not readily fit the ESTs available, the patients wanted more than help with the PTSD despite its initially appearing otherwise, and the specific features and comorbidities of the two patients carrying the same diagnosis were very different. All these features underscore the Institute of Medicine's call for clinicians to take account of patients' unique concerns.

### PTSD IN AN ARMY VETERAN: THE CASE OF TOM

Tom is a 26-year-old, single office worker who recently returned from a 1-year tour of army duty in a country where he subsequently spent an additional two years as a civilian worker.

#### **Presenting Problems**

Tom came in complaining that since he returned from abroad 3 months ago he has been experiencing disturbing symptoms related to a combat event. He was with soldiers from his unit in an exposed situation where grenades were going off and shots were being fired. He suffered injury to his hand, which had to be bandaged to stop the bleeding, but Tom continued to fight on during the battle. Some scars remain on his hand from the incident, but no physical impairment occurred.

Tom reported that he was emotionally numb at the time and in fact only started thinking about the event when he returned stateside. On the anniversary of the battle, he had a distressing recollection of the battle, which led to heart pounding and labored breathing. At times, he feels the incident was not real, comparing his recollection to looking at a photo. He has been having recurring dreams in which he hears the loud noise of an exploding grenade, sees a flash of light, and looks at his hands to see if they are bloody. He wakes up in a sweat, feeling very anxious, and is disoriented as to his location. He is fearful now in a way that he was not at the time of the incident.

Tom tries to avoid conversations about the trauma, which is why, he said, he delayed coming to see me for three months. He cried when he told his girlfriend about it and has mentioned it to no one but her and me. He has had trouble sleeping, is hyper-vigilant, and experiences a startle response; for example, when a friend tried to hug him from behind, "I put him on the floor." When he walks down the street, he looks over his shoulder, believing that someone may be on a roof about to shoot him. He will look at a stranger

and wonder if he has a bomb, although he acknowledges that the thought is ridiculous. He was seeking help overcoming his readily diagnosable PTSD.

In our third session, Tom let me know he has suffered from Tourette's syndrome (TS) since he was a child. In Tom's case, he has mild facial tics around his nose and cheek, drums his fingers on his leg, and occasionally makes soft clicking noises with his throat. He suppressed the symptoms in our first two meetings and perhaps only after feeling a little more comfortable with me did he reveal his condition and manifest the behaviors, which are relatively mild. He wanted help with this problem as well, because he found it had worsened in recent months. Interestingly, it was not only that Tom wished for a decrease in symptoms, but, more important to him, he hoped to become comfortable with himself as a person who has TS. He wanted to be able to manifest the behaviors in public without feeling so self-conscious. In addition to the PTSD and TS, the client suffered from attention-deficit/hyperactivity disorder (ADHD) since childhood. He cannot sit still for too long and has trouble with written expression (dysgraphia).

Tom has very recently met the woman he plans to marry. As the two of them have gotten to know each other better, inevitable differences have surfaced, which he wanted help to understand and negotiate. For example, he finds himself annoyed and upset when his girlfriend spends time with her friends. In addition, as the wedding day approaches, he feels increasingly nervous about taking on new responsibilities and losing his freedom as a single man.

Yet one more issue troubled Tom. While in the military, he felt a great sense of purpose in his life. He garnered respect from others, which raised his self-respect. He also developed close friendships, which have been hard to sustain over time and distance, and which enhances his need for companionship with his girlfriend. He would like to find the kind of part-time work that would improve his self-esteem.

### **Therapy Choice and Process**

Are there evidence-based treatments, pharmacological or psychological, that would be suitable for treating Tom's PTSD? To start with the medication option, some studies support the use of Selective Serotonin Reuptake Inhibitors (SSRIs) in the treatment of PTSD, but more confirmation is needed (Yehuda, Marshall, Penkower, & Wong, 2002). Are the empirical findings particular to combat-induced trauma? There have been four published clinical trials on the use of SSRIs specifically with U.S. war veterans, three of which showed no improvement over placebo, which gave me pause about referring Tom for medication. On the other hand, many of the subjects in these trials had been refractory to other treatments attempted, which was not true of Tom. Of most relevance to our present focus on considering

patient preferences, however, was Tom's stance that he would only consider medication as a last resort.

Turning to psychological treatments, is there an EST that fits the case at hand? The answer is "not too well." Therapies for PTSD that are considered efficacious do not pertain to combat but to rape, genocide, and natural disasters (Keane & Barlow, 2002). Furthermore, many PTSD studies apply only to treatment that takes place soon after the trauma (Litz, Gray, Bryant, & Adler, 2002), which was not Tom's situation. Nevertheless, a substantial amount of empirical research on PTSD and other anxiety disorders points to two important treatment factors: anxiety management and exposure, whether *in vivo* or imaginal (Keane & Barlow, 2002). I drew upon these principles in helping Tom talk about the details of the event with the accompanying emotion to help him gain a greater mastery over memories of it. Incidentally, as Keane and Barlow pointed out, it was Janet and Freud who most influenced these CBT approaches to PTSD, so that it was not difficult for me as an "assimilative" psychodynamic therapist to adapt them to the therapy. (Assimilative integration refers to the incorporation of techniques or perspectives from one theoretical orientation into one's preferred, theoretically based therapy; Messer, 2001.)

To view Tom's problems more broadly, a central dynamic ran through the three diagnoses: control. He had always connected the ADHD and TS in his mind, feeling that, because he had some measure of control over the ADHD symptoms, he should be able to do so with the TS symptoms as well, which was not the case. As a child, he was frequently out of control and feared adult situations in which this might occur as well. Tom's way of coping with these fears was taking on heroic tasks such as dangerous army duty. The traumatic combat event he experienced was especially troubling in that he felt not in control. In fact, when recounting the combat event during therapy, he reported that because civilians were close to the action he was not able to shoot back, leaving him feeling particularly helpless.

Regarding his TS, I referred him to a specialty clinic in the area where a therapist helped him learn how to focus less on the tics and "to let go," so that he could concentrate on the matter at hand. The therapist also taught him to meditate. All the while, and in accordance with his request, I worked with him to bolster his self-acceptance as a person with TS. It should be noted that no EST has been established for TS (which is sometimes considered a form of obsessive-compulsive disorder [OCD] or is comorbid with it) and no cure has been found. However, medication (albeit with side effects; Erenberg, 1999) and behavioral techniques, such as habit reversal along with parental support, have been shown to be helpful to children and younger adolescents (e.g., Azrin & Peterson, 1990).

In Tom's therapy, I also paid attention to his current dependency on his girlfriend, which was due to the fact that he no longer had the

companionship of his army buddies available, and which created tension between them. He became aware that his dependency was not the result of any purposeful deprivation on her part but rather was because of his own neediness in the current circumstances, which he subsequently took steps to change. Similarly, he found part-time work at a hospital that helped satisfy his need to be helpful to others and which augmented his sense of self-respect.

After 8 months of therapy, the PTSD was very much improved and his concern with the TS lessened. He is in the process of working out his conflicts with his girlfriend. A recent incident, however, indicated that the PTSD still lurks in the background. When firecrackers went off unexpectedly on July 4, he was very startled, his heart raced, and he broke into a cold sweat. However, he calmed down quickly, drew on his girlfriend's soothing support and now feels confident that he will be better able to handle loud noises in the future.

### **The Take-Home Point in the Case of Tom**

The therapy was guided by the client's wishes, expectancies, and concerns about his psychological and neurological problems in conjunction with my knowledge of empirical research, theoretical predilection, and clinical expertise. I did not impose a set treatment on Tom nor did I narrow my focus to the specific diagnosis he initially presented. I followed the patient's wishes to avoid medication if possible and treated issues outside the triple diagnosis he carried. These included his relationship with his girlfriend and his sense of lost purpose in life. Had I not done so, I am quite sure that he would not have been helped as much and would have been much less satisfied with the therapy I offered. Together, we also came to understand the common dynamic thread around his concern with control, which he found enlightening and helpful in becoming less preoccupied with and ruled by it.

I turn now to a second case of PTSD in an effort to demonstrate the variety typically encompassed by any single diagnosis, and the necessity for clinical sensitivity to patients' individual concerns and satisfaction.

### **PTSD IN A BUSINESS MANAGER: THE CASE OF MRS. T**

Mrs. T is a 42-year-old woman who is married with two teenage sons and one preteen daughter. She is of European background and grew up abroad. On September 11, 2001, Mrs. T witnessed the second plane crash into the World Trade Center while knowing that several of her close associates were in that building for a meeting. At the time, she had a very responsible, human relations position at a corporation where she did the hiring. In



this role, she arranged the funeral services for two of the employees and served as the liaison for their families, taking care of matters such as insurance and death certificates. She was also an emotional support for the bereaved families.

### **Presenting Problems**

Since a few months after that time and the subsequent loss of her managerial job, she has suffered from frequent crying spells, anxiety attacks, and a sense of despondency. She finds that her thoughts are disconnected, making her unable to focus on any task. Beset by physical problems, such as high blood pressure and a severe facial rash, Mrs. T has also experienced chest pains and heart palpitations, making her feel as if she were having a heart attack. Complaining of diminished libido, the client has less interest in being intimate with her husband, with whom there have been increased marital tensions. Not long after 9/11, Mrs. T withdrew from social activities, preferring to be by herself. Her feelings, except for grief, sadness, and irritability, have been numbed.

Mrs. T came to see me in a very distraught, tearful state despite a year having elapsed since 9/11. Referring to her experience at that time, Mrs. T told me she had learned that one of her female associates left the building with her body on fire and succumbed shortly afterwards. Mrs. T still imagines herself in conversation with the deceased employees who were also her friends. The 9/11 events are replayed in her mind both in the waking state and in her disturbing nightmares. Mrs. T believes she was indirectly responsible for her associates' deaths, which has been one important focus of therapy.

Due to the business downturn in New York City after 9/11, her firm decided not to do any more hiring and Mrs. T was let go. (Job loss is a frequent complication in people with PTSD; Levant, Barbanel, & DeLeon, 2003.) The termination of her high-paying, challenging position was a big blow to her self-esteem. Although she made an effort to find other employment, she was not successful and soon became unable to pursue it further because of her increasingly debilitating symptoms. Her financial situation deteriorated, made worse by her husband losing his job as well. She first sought treatment with a psychologist a few months after 9/11, which was helpful but which she had to terminate when she no longer had insurance coverage. A psychiatrist who prescribed antidepressant and anti-anxiety medications has also seen her.

### **Therapy Choice and Process**

Is there an EST suitable for Mrs. T? Regarding medication, Mrs. T had already been taking an antidepressant SSRI and a mood stabilizer for a year,

which, although helpful, were not sufficient to restore her mental stability and life functioning. With reference to psychological treatments, just as no obvious EST was available for treating Tom, the same was true of Mrs. T. (The jury is still out on the use of eye-movement desensitization and reprocessing [EMDR] for civilian PTSD; Resick & Calhoun, 2001). As in Tom's case, I drew upon the principles of anxiety management and exposure, as recommended by Keane and Barlow (2002), to help Mrs. T face up to and start to master the 9/11 trauma, as well as to reduce her avoidance of social interactions and vocational pursuits.

In both cases, I called upon therapist factors that have been shown to correlate with therapy outcomes. For example, strong empirical evidence shows that therapeutic alliance has a significant and reliable relationship to outcome (Martin, Garske, & Davis, 2000). In a recent review of therapist characteristics and techniques that enhance the therapeutic alliance, Ackerman and Hilsenroth (2003) found the following personal attributes of the therapist to be important: being flexible, honest, respectful, trustworthy, confident, warm, interested, and open. Apropos of techniques, they found the following kinds of interventions to contribute positively to the alliance: exploration, reflection, noting past therapy success, accurate interpretation, facilitating the expression of affect, and attending to the patient's experience.

Although these personal characteristics and technical approaches were of some help in Mrs. T's therapy, they were only partially successful in restoring her mental health. A primary question that arose in my mind was why her symptoms had persisted so long and with such virulence. Was there something more to it than a natural, human response to disaster? Why did she feel so responsible for the fate of her employees who were simply going about their business in a usual way? After all, she did not deliberately send them to their death and was only "responsible" to the extent that she had hired them and made the case for keeping on one of them when the boss wanted to let her go. What individual characteristics and expectancies might be maintaining the symptoms?

When I asked Mrs. T to tell me what came to mind about her feeling responsible for the employees' demise, she informed me that she had long believed that she possessed magical powers, that she was, in her words, "a small deity." I asked her for examples of what she meant. In response, she told me how she is able to locate objects even years after others have lost them simply by holding something of theirs in her hand. As another example, on three separate occasions she had heard a knock on the door although no one was there, and knew instantly that someone in the family had died.

It became clear to both of us that her sense of specialness had played an important role in her slow recovery from the trauma. If she could foresee the future, she posited, why had she not prevented her associates from going into the World Trade Center on that fateful day? When I pointed out that her

excessive guilt over their deaths was closely linked to her belief in her special powers, she responded ruefully that if she were more humble she might not be suffering so much. This dynamic also helped her to understand that her sadness was not related only to the 9/11 losses but also to the diminution of her secret sense of being powerful and special. At first, she experienced this revelation as a considerable narcissistic blow (to use the language of self-psychology). With further exploration and reflection about this unusual feature of her personality, her narcissism diminished to some degree as she became more accepting of herself as an ordinary mortal. These reflections also helped her to recognize her own role in her strong reaction to 9/11. Although the insight caused her grief, it also produced relief at its exposure in a safe therapeutic setting.

Very briefly, other features of the therapy included my encouraging Mrs. T to express her mixed feelings about her marriage and her coming to the realization that she no longer wanted the kind of high-powered job she once enjoyed. That job had meant giving up what she now saw as precious time with her growing children and having too little time for herself and her husband. Although Mrs. T is not yet fully recovered after one and a half years of therapy, she is much less subject to anxiety and mood swings, is working part time, and is functioning better on a day-to-day basis. Her medications have also been substantially reduced.

### **The Take-Home Point in the Case of Mrs. T**

An EST, although of some help in this case, could not by itself cover other ground that truly mattered to this woman. Hers was not a case of pure or even typical PTSD, as is also true of many cases that are triply diagnosed or multifaceted. In fact, the complexity of peoples' lives is not readily captured by diagnosis, which is why individual client expectancies, preferences, and concerns need to be addressed. For example, after reviewing the influence of client variables on psychotherapy, Clarkin and Levy (2004, p. 214) concluded that "Nondiagnostic client characteristics may be more useful predictors of psychotherapy outcome than DSM-based diagnoses. The diagnostic categories allow for too much heterogeneity in personality traits to serve as useful predictors or matching variables." Similarly, Beutler et al.'s (2004, p. 291) review of therapist variables led them to state that "evidence is accumulating on the role of patient moderators in determining the effectiveness of interventions." The client's functional impairment, resistance level, coping style, and stage of change and expectations, among other features, are all evidence-based, transdiagnostic means of customizing therapy to improve outcomes (Norcross, 2002). In other words, it is frequently more important to know what kind of patient has the disorder than what kind of disorder the patient has.

## CONCLUDING COMMENTS

Many psychological complexities characterize these two cases that, to be treated optimally, call for a knowledge of ESTs, ESRs, therapist variables, client factors, and their interaction; the recognition of intrapsychic and interpersonal themes; a degree of psychotherapy integration; and an appreciation of unique patient needs (Messer, 2004). Both patients suffered from the same condition, PTSD, yet the nature of the trauma, its severity, and effect were very different, similarities notwithstanding. Tom's PTSD is combat related, whereas Mrs. T's is a vicarious response to witnessing the attack on the twin towers and a result of losing friends there. Tom wants medication only as a last resort, whereas Mrs. T is already on medication. Tom is multiply comorbid on *DSM Axis I*, whereas Mrs. T has *Axis II* traits that complicate the PTSD. One could not simply apply a packaged EST to them even for a diagnosis where such treatments do exist. In both cases, one would be hard pressed to limit the therapy to symptom alleviation without leaving the client dissatisfied.

In this connection, it is difficult to establish a single, consensual definition of success in psychotherapy. To be considered are the perspectives of at least four parties—client, therapist, independent judge, and society—and these are only moderately correlated (Luborsky, 1971). Although I do not advocate ignoring the other perspectives, I have emphasized the necessity of taking into account the patient's viewpoint on what matters regarding choice of treatment, its content, and its effectiveness. The patient's evaluation is a critical component of effective practice. To achieve patient satisfaction with any psychological treatment, the therapist must be flexibly attuned to the vicissitudes of patients' needs and pay careful attention to their unique characteristics, concerns, and preferences.

### Dialogue: Convergence and Contention

*Geoffrey M. Reed*

In their position papers, Kihlstrom and Messer cover much of the ground that I might have in a more complete discussion of my own point of view, as compared to my assigned focus on clinical expertise. In fact, Messer makes the case for clinical expertise more compellingly through his description of two cases, and his conclusions about the necessary ingredients of appropriate treatment are similar to my own. I agree generally with Kihlstrom

about the importance of research as a foundation for practice. I concur particularly with his emphasis on psychotherapy outcomes and share the perspective that psychologists will soon be required to document the outcomes of the treatment they provide. There are serious issues about the selection of measures and the funding and infrastructure for outcomes assessment, and we should be placing substantial energy as a field in addressing them.

However, I struggle with Kihlstrom's argument for the same reasons that professional psychologists struggle with the current discussion of EBP: its narrowness and the major implications of what it leaves out. The meaning of his opening assertion that "scientific research is the only process by which . . . practitioners should determine what 'evidence' guides EBPs" (p. 23) is unclear. Does this imply that Messer's use of the patient's wishes and goals for treatment, the developing therapy alliance, and his formulation of the deeper themes of control and narcissism does not constitute EBP? Should Messer have instead identified the treatment manual that most closely corresponded to the characteristics of these patients and implemented that treatment as described? Or, given the lack of efficacy trials of PTSD treatments with patients whose clinical characteristics matched those of Messer's patients, should no treatment have been offered at all?

Kihlstrom frames his argument around a dichotomous distinction: Either a practice is "based on rigorous research evidence" or "a kind of folklore" (p. 24, citing Hitt, 2001). However, this claim assumes that knowledge on the basis of evidence other than specific experimental manipulations either cannot be rigorous or cannot be research. Therefore, health services on the basis of such evidence are the modern equivalent of bloodletting. Such dichotomies are polemic and specious. The assertion that there is a lack of evidence supporting psychological practice is based on a highly selective and narrow reading of the literature, the result of filtering the knowledge base of professional psychology through the Division 12 evidentiary criteria to which Kihlstrom refers. By their very nature, these criteria legitimate some approaches to psychotherapy and delegitimate others (Tanenbaum, 2003), one reason that the emotional pitch of the EBP discussion within psychology is so high. A glance at the lists of evidence-based psychological treatments indicates that these are overwhelmingly behavioral or cognitive-behavioral, supporting Kihlstrom's assertion that such treatments are the standard of care. However, this conclusion arises from a confounding of the criteria with the characteristics of the treatments they are used to assess (Tanenbaum, 2005; Westen, Novotny, & Thompson-Brenner, 2004).

Kihlstrom's call for randomized comparisons of one specific treatment or modality to another as the most authoritative source of knowledge can be linked directly to the criteria. This emphasis ignores the two most robust findings in the psychotherapy literature: (a) specific treatment techniques account for little variance in psychotherapy outcomes and (b) the strongest

and most consistent predictors of psychotherapy outcomes are the characteristics of the therapist and the nature of the treatment relationship. In this regard, the EBP movement has been remarkably impervious to much of the evidence.

Kihlstrom describes the confluence of the demands of managed care and the rhetoric of science as a positive development that fosters professional accountability. Indeed, organized health care systems are moving to formulate lists of evidence-based treatments with the goal of making these the basis for reimbursement policies, and they are soon likely to attempt to proscribe treatment on this basis. EBP proponents argue that the "tested" treatments that appear on the lists of ESTs, in contrast to the "untested" treatments practiced widely in the community, are what should be offered and reimbursed until equivalent evidence is available for other forms of treatment (an eventuality that the nature of the criteria effectively prevents).

I concur that we have passed the point when it is acceptable to compare psychological treatments to no treatment. But if we propose to use these comparisons as a basis for restricting patient choice, directing reimbursement, or compelling practitioners to use particular approaches, I contend that the required comparison should be between the outcomes of the treatment being tested and those achieved by respected therapists in the community. Such comparisons have virtually never been undertaken (cf., Westen et al., 2004).

Kihlstrom is silent on the commercial interests of the managed care organizations he portrays as allies for accountability. The EBP discussion within managed care focuses clearly on limiting utilization, in spite of compelling evidence that a much higher rate of mental health services should be provided than is currently the case. The great majority of people with mental health problems delay or avoid seeking help, and, when they do, their care is dispersed through a fragmented service delivery system (Narrow, Regier, Rae, Manderscheid, & Locke, 1993; Regier et al., 1993). Many mental health disorders are chronic in nature, contributing heavily to worldwide disability and associated costs (World Health Organization, 2001). Their onset is typically early in life, and the costs and consequences of failing to treat them are cumulative across the lifespan (Kessler et al., 2003). Barely half of those in the U.S. with severe mental health problems and a minority of those with moderate problems receive any treatment at all (World Health Organization World Mental Health Survey Consortium, 2004), and treatment is often inadequate. If the goal of EBP is to improve U.S. health care, we must replace the pursuit of "psychotherapy tinkering" studies (Humphreys & Tucker, 2002, p. 130) with a focus on how we can best deliver treatment to the population in need and thereby increase the population impact of services for mental health disorders.

In medicine, EBP has three components: The “best research evidence” is integrated with “clinical expertise” and “patient values” in an effort to optimize treatment outcome and patient satisfaction (Institute of Medicine, 2001, p. 147). So far as psychotherapy is concerned, the three contributors to this chapter are all agreed on this general formula, in principle, but disagree about the place that controlled research occupies in the hierarchy of practice-relevant evidence. Geoffrey Reed laments the “deprofessionalization of health professionals” (p. 14) and reasserts the “paramount importance” (p. 19) of clinical expertise, by which he means the combination of “a body of specialized professional knowledge” with the clinician’s “individual experiences” (p. 18). Stanley Messer, for his part, argues that research evidence about “patients in general” is insufficient when it comes to “patients’ unique qualities, circumstances, and wishes” (p. 32). Both arguments make good points but do not contradict the fundamental proposition that good treatment relies, first and foremost, on good research.

To see why this is so, let us return to the definition of EBP given previously, and cited by both Reed and Messer. When diagnosing and treating illness, physicians apply the best medical knowledge to the individual case. When a patient presents with a fever, for example, they understand, from medical science, that fevers are symptoms of underlying infections. They then use scientifically validated laboratory tests to determine the nature of that infection and prescribe a particular antibiotic that scientific evidence, summarized by practice guidelines, suggests will be most effective in treating that particular infection. If several appropriate treatments are available, the physician may prescribe the one that is older, and thus whose side effects and risks are better known. Or perhaps he or she will prescribe a generic drug that is cheaper than a proprietary product. If this does not work, the physician may try another antibiotic or order another panel of tests. But in any event, scientific evidence informs the entire process from beginning to end. Why should psychotherapy be any different?

Of course, practitioner expertise is important. But expertise in what? In medicine, the physician’s expertise consists largely in applying scientific evidence, and scientifically based techniques, to the diagnosis and treatment of the individual case. Like psychotherapists, physicians treat individual patients, but this offers no dispensation from the obligation to base treatments on the best scientific evidence. To continue the fever example: If the patient presents some contraindication to the treatment of first choice, then the physician will select an alternative that has also been proven effective, but which bypasses the relevant contraindications. The physician may also have expertise in building rapport with patients, taking a history, performing

a physical examination, finding a vein, and delivering bad news. But these practical skills mean nothing unless the physician is making decisions on the basis of the best evidence available. Why should psychotherapy be any different?

And besides, how do we know that a clinician is an expert? We know that someone is an expert because we have empirical evidence concerning the outcomes of his or her encounters with patients. That evidence comes from research. In the classic scientist-practitioner model, the practitioner's scientific credentials come in two forms. First and foremost is the assumption that psychotherapy would be based on the best available knowledge about mind and behavior, and the best available evidence as to what specific treatments work best for specific conditions. These results of nomothetic research are to be complemented by idiographic research in which practitioners study their own outcomes. Physicians acquire reputations as experts, and hospitals acquire reputations as collections of experts, because empirical evidence shows that their patients get better. The collection of such data is simply another form of research. Why should psychotherapy be any different?

And, of course, patient values are important too, but they do not trump scientific evidence. With few exceptions, mostly in the domain of public health, a patient is free to reject a physician's treatment plan, but the plan itself must be based on the best available scientific evidence. A patient may appear for a diagnosis to find out what is wrong with him or her but ultimately decline treatment for religious or financial reasons. Or the patient may prefer palliative care to active treatment that is too risky or has undesirable side effects. A patient may prefer to be treated by someone of the same gender. All these patient values, and others like them, can be accommodated within the framework of EBP by allowing patients to select, among scientifically validated practices, those that are most compatible with their values. Why should psychotherapy be any different?

Even though the presenting case may be complex, there is no reason to think that EBPs do not provide the best approaches to treatment. Arguably, the most important "therapist factor" is the wisdom to be guided by available research evidence, even if the research base does not precisely match the features of the case at hand. And the most important feature of the relationship between the patient and the therapist is the patient's confidence that what the therapist does is based on the best available scientific evidence.

The constraints of EBP, including the placement of scientific research at the top of the hierarchy of evidence, over and above expertise and patient values, do not in any respect "deprofessionalize" health care. The professional status of psychotherapy and the professional autonomy of clinical psychology from psychiatry depend critically on the assumption that therapeutic practices are grounded in a firm base of scientific evidence. Professions are defined precisely by practices on the basis of specialized bodies of knowledge (Abbott,



1988; Friedson, 1970; Starr, 1982). That is what it means to be a professional, as opposed to an amateur or a dilettante. If clinical psychology is to retain its status as a profession, it needs more research, not less; the best research, not excuses; and sooner, rather than later.

*Stanley B. Messer*

A fundamental difference in outlook exists between the point of view presented by Reed and me, on the one hand, and by Kihlstrom, on the other. Reed and I emphasize the importance of taking full account of the nature of the players involved in EBP, whether it is the clinician (Reed) or the client (Messer). I would characterize our stance as falling within the romantic vision, which prizes individuality, subjectivity, and the unique qualities of every human being. By contrast, Kihlstrom's approach views individuals as interchangeable and their subjectivity an unfortunate impediment, particularly in the assessment of psychotherapy outcomes. Closely tied to this outlook is his strong reliance on the medical metaphor for describing psychological problems in which one diagnoses and treats psychopathology much like one diagnoses and treats cancer. In this view, psychotherapy is equivalent to a drug, a control condition is akin to a placebo, techniques are administered via manualized instructions, and outcomes are measured in labs through the analog of blood tests. The approach valorizes mechanism, objectivity, and technical aspects of therapy over artistry, client subjectivity, and quality of the therapeutic relationship. In this brief commentary, I will stress the importance of attending to the patient's needs and preferences and will point to some drawbacks of Kihlstrom's more objective and externally oriented position.

Regarding psychotherapy outcome measurement, Kihlstrom criticizes the *Consumer Reports* study, which used patients' self-reported satisfaction with their treatment "instead of objective evidence of actual improvement" (p. 26). Within an epistemological stance that takes patients' subjectivity seriously, their own evaluation of outcome is as "actual" as any other form of outcome assessment. Although it is different than an objective assessment, this does not make it any less real or less important, especially in the context of the highly personal realm of psychotherapy. In the same vein, Kihlstrom argues that "it is long past time that psychology began to move away from questionnaires and rating scales, and toward a new generation of assessment procedures on objective laboratory tests of psychopathology" (p. 29). If we were to do so, however, how would we judge results that showed a lessening

of psychopathology on a physiological measure, alongside a patient's continuing unhappiness with the therapy or him- or herself? For example, what if the patient's anxiety subsided after treatment, but she was still unable to achieve the intimacy she craves and continued to feel lonely and isolated? Her subjectively experienced life satisfaction, it can be argued, is every bit as important as the decrease of pathology as gauged by objective instruments.

Along similar lines, Kihlstrom is more interested in how third parties view the results of therapy than he is in the participants' satisfaction, giving as one example that couples who go through therapy "might reasonably expect to have happier children than they did before" (p. 29). What if, as a result of therapy, the couple decides to divorce, which leads to greater ultimate happiness for them but less immediate happiness for their children? In other words, can we judge the outcome of the therapy only or primarily by the children's assessment or should we not consider the major participants' desires as well? Further regarding the role of third parties, Kihlstrom states, "employers who pay for their employees to participate in alcohol or drug abuse treatment might reasonably ask if their employees do, in fact, become more productive after treatment" (p. 29). What if the client comes to realize after therapy that she is in the wrong job or even the wrong career, which has contributed to her alcohol or drug abuse? She then stops drinking or taking other drugs, leaves the company, goes back to school, and eventually becomes both happier and more productive elsewhere. Should the company be paying only for an improvement in its bottom line? Is this the kind of corporate outlook that we, as mental health practitioners, wish to promote?

Finally, Kihlstrom also proposes that we assess psychotherapy outcomes according to whether third parties "feel that their own life satisfaction has improved" (p. 29) because of the client's therapy. He refers, in this connection, to the client's family and coworkers. (It should be noted here that Kihlstrom is unwilling to grant epistemological legitimacy to the client's felt improvement in his or her life satisfaction but is prepared to do so for a third party.) But imagine, for the moment, a timid woman who becomes more self-confident, assertive, and challenging of her domineering husband because of therapy. In addition, she begins to stand up for herself at work, where her boss has quite regularly taken advantage of her timidity. Should we consider her husband's or boss's decreased satisfaction as an indicator of a failed therapy? Coming to such a conclusion is one of the risks of relying on outside parties in the service of objectivity.

To reiterate my main point, there is no escape from the subjectivity of the patient whose preferences, values, and expectations the clinician must always keep firmly in mind. Nor is there any escape from the valuative dimension of therapy, which reminds us that decisions about the way we characterize peoples' problems, conduct therapy, and assess outcomes are not determined by scientific criteria alone (Messer & Woolfolk, 1998).

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# Evidence-Based Practices in Mental Health

*Debate and  
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Questions*

Editors

**John C. Norcross, Larry E. Beutler,  
and Ronald F. Levant**

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