
Evidence-Based Treatment and Practice

New Opportunities to Bridge Clinical Research and Practice,

Enhance the Knowledge Base, and Improve Patient Care

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The long-standing divide between research and practice in clinical psychology has received increased attention in view of the development of evidence-based interventions and practice and public interest, oversight, and management of psychological services. The gap has been reflected in concerns from those in practice about the applicability of findings from psychotherapy research as a guide to clinical work and concerns from those in research about how clinical work is conducted. Research and practice are united in their commitment to providing the best of psychological knowledge and methods to improve the quality of patient care. This article highlights issues in the research–practice debate as a backdrop for rapprochement. Suggestions are made for changes and shifts in emphases in psychotherapy research and clinical practice. The changes are designed to ensure that both research and practice contribute to our knowledge base and provide information that can be used more readily to improve patient care and, in the process, reduce the perceived and real hiatus between research and practice.

Keywords: evidence-based treatment, evidence-based practice, psychotherapy, bridging research and practice

There is a well-recognized split within clinical psychology between research and practice in professional work, career paths, and training. The split has come into sharper focus with the development and evaluation of empirically supported, or evidence-based, interventions.¹ A central issue is the extent to which findings from research can be applied to clinical practice. The issue is not new, but multiple influences within and outside of psychology have heightened awareness of it and the stakes involved.

Within the profession, there have been enormous advances in psychotherapy research. Approximately 50 years ago, seminal literature reviews noted that psychotherapy did not seem to produce benefits that exceeded changes occurring over time without treatment (Eysenck, 1952, 1966; Levitt, 1957, 1963). Their conclusions, even though challenged at the time, lingered and were not easily refuted. Since these reviews, thousands of well-controlled outcome studies (randomized controlled trials, or RCTs) have been completed, reviewed, and meta-analyzed. Indeed, reviews of the reviews are needed just to keep track of the advances

(e.g., Lambert & Ogles, 2004). There is evidence in support of many treatments, and this fact alone draws attention to whether some treatments ought to be used more than others in clinical practice and under what conditions. The discussion of treatment and the delivery of services has moved into the public domain as part of the larger health-care landscape. There is an effort to provide resources that inform and make available current evidence-based interventions. Indeed, one prominent Internet link alone encompasses over 30 federal, state, professional, and university Web sites that enumerate these interventions (<http://ucoll.fdu.edu/apa/lnksinter.html>). Perhaps the most well-known effort is the Web-based resource provided by the Substance Abuse and Mental Health Services Administration (<http://www.nationalregistry.samhsa.gov>). There are also efforts among third-party payers and states that may prescribe what treatments are to be allowed and reimbursed. Researchers and clinicians alike see dangers in prescriptive and inflexible treatments. Although it is true that several treatments have empirical research to back them up, is the evidence adequate, sufficient, and generalizable to practice situations? Also, who (professionals, managed-care agencies) ought to make the decisions about when the evidence applies to a given case?

Researchers, practitioners, and health-care policy advocates continue to debate the merits of the evidence in behalf of various interventions, what counts as evidence,

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¹ The focus on evidence-based interventions spans multiple disciplines (e.g., medicine, education, social work, nursing, dentistry, and others). In relation to psychological interventions, several countries and many organizations, committees, and task forces within this country have delineated evidence-based treatment, interventions, and practice. Now that many agencies at the state and federal levels are involved, multiple and often incompatible definitions are in use for the same terms. The varied definitions and criteria for delineating treatments as evidence supported are not central to the theses of this article. This article focuses on psychosocial interventions in the context of clinical work, and I use the terms *psychotherapy* and *treatment* to refer to these interventions. Psychological interventions play a critical role in other areas of psychology (e.g., school, counseling, and correctional psychology) that are beyond the scope of the discussion.



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and how the evidence is to be used and integrated (e.g., Burns & Hoagwood, 2005; Goodheart, Kazdin, & Sternberg, 2006; Hunsley, 2007; Tanenbaum, 2005; Wampold, 2001; Westen, Novotny, & Thompson-Brenne, 2004). A heavy stream of articles continues to emerge that critically evaluate the evidence or clinical work, each one underscoring the split between research and practice. Key terms that are used to discuss treatments and the use of evidence reflect the differences between and the different priorities of research and practice. For example, empirically supported or *evidence-based treatment* (EBT) refers to the interventions or techniques (e.g., cognitive therapy for depression, exposure therapy for anxiety) that have produced therapeutic change in controlled trials. *Evidence-based practice* (EBP) is a broader term and refers to clinical practice that is informed by evidence about interventions, clinical expertise, and patient needs, values, and preferences and their integration in decision making about individual care (e.g., APA Presidential Task Force on Evidence-Based Practice, 2006; Institute of Medicine, 2001; Sackett, Rosenberg, Gray, Haynes, & Richardson, 1996). EBP is not what researchers have studied. From the standpoint of research, one might say that there is evidence for specific interventions in the highly controlled contexts in which they are studied but not yet much evidence for EBP in the clinical contexts where judgments and decisions are made by individual clinicians informed by evidence, expertise, and patient considerations. The issues embedded in this difference are part of the impetus for this article—these differences can be bridged.

The debate has focused on polarities or differences in priorities of psychotherapy research and clinical practice. Some aspects of the debate reflect testable hypotheses, but others cannot be resolved through empirical tests. These

latter aspects reflect differences in what research and practice are trying to accomplish and how they realize their goals, as elaborated below. No one is really well served by this debate and the split of research and practice. Perhaps the greatest casualty is the public at large.

The purpose of this article is to address the debate about EBTs and EBPs and the research–practice split. First, both research and practice share a keen interest in providing the highest quality care. This interest includes identifying and using the most effective and cost-effective treatments. Second, there are opportunities for a rapprochement between research and practice that will not only foster improved clinical care but will also develop and strengthen the knowledge base. Recommendations are made in this article that will help to make research and practice much more useful to each other, to our profession, and to the public at large. I begin by highlighting concerns about evidence-based interventions and clinical practice. These issues set the stage for conveying how shifts in both research and practice can have a significant impact in enhancing patient care, improving the knowledge base, and bridging research and practice.

Evidence-Based Treatments and Clinical Practice: Illustrative Concerns

Concerns About Evidence-Based Treatments

The concerns about EBTs have been the subject of many excellent articles, chapters, and books (e.g., Hunsley, 2007; Norcross, Beutler, & Levant, 2005; Westen et al., 2004). A frequently voiced and enduring concern is that key conditions and characteristics of treatment research (e.g., therapists, patients, treatment, and contexts) depart markedly from those in clinical practice and bring into question how and whether to generalize the results to practice (e.g., Hoagwood, Hibbs, Brent, & Jensen, 1995). For example, patients in controlled trials have been characterized as having less severe disorders and fewer comorbid disorders than patients who routinely come to treatment. In addition, recruiting, selecting, and enrolling cases for research (e.g., soliciting and obtaining informed consent, conveying that the treatment provided will be determined randomly) differ considerably from the processes leading individuals to come to clinical services for their treatment (e.g., Westen & Morrison, 2001). Apart from the participants, controlled studies often introduce several features of treatment (e.g., standardization and manualization of treatment, fixed number and content of the sessions) that differ from the circumstances and conditions of treatment in clinical work.

Another concern about research on psychotherapy pertains to the focus on symptoms and disorders as the primary ways of identifying participants and evaluating treatment outcomes. In clinical practice, much of psychotherapy is not about reaching a destination (eliminating symptoms) as it is about the ride (the process of coping with life). Psychotherapy research rarely addresses the broader focus of coping with multiple stressors and negotiating the difficult shoals of life, both of which are aided by speaking with a trained professional. In clinical practice,

sometimes symptoms are the focus; even when they are the focus, over half of patients seen in therapy add new target complaints or change their complaints over the course of treatment (see, e.g., Sorenson, Gorsuch, & Mintz, 1985). Outcomes that seem loose and fuzzy (e.g., angst, quality of life, coping) or that are moving targets are rarely addressed in controlled therapy trials. Understandably, there is concern about applying many of the findings from research to practice.

There are less frequently voiced concerns that might be considered more fundamental than the usual concerns about generalizability. These concerns are more fundamental in the sense that those voicing them do not begin by accepting the basic findings and then asking whether the results are generalizable. Rather, they look at the methods of analysis or the results among several studies and question whether these are satisfactory bases for concluding that treatment is effective or efficacious. The following three issues convey the point.

First, there are many different criteria for delineating whether a treatment is evidence based or empirically supported. A common criterion is that the treatment produces an outcome that differs from the outcome of a no-treatment control or treatment-as-usual condition. This criterion requires showing statistically significant differences when groups are compared at the end of treatment. Yet statistical significance (or even the more informative measure of effect size) does not necessarily mean that patients have improved in ways that are reflected in their everyday functioning. Statistical significance is a function of sample size and variability within and between subjects. The difference required for significance in the outcome (e.g., on measures of anxiety, marital discord) may not reflect a detectable or real difference in the everyday life of any individual client or even of the group. In short, conclusions about treatment that are based on studies showing statistical differences are difficult to translate into effects on the lives of participants in the study, let alone to generalize to patients seen in practice.

Second, apart from statistical issues, the outcome measures in most psychotherapy studies raise fundamental concerns. Changes on rating scales, even well-established ones such as the Beck Depression Inventory or the Minnesota Multiphasic Personality Inventory and its derivatives, are difficult to translate into changes in everyday life. A change of one standard deviation on a measure from pre- to posttreatment does not clearly portray (or map onto) how the client is functioning in everyday life. For example, if a patient with obsessive-compulsive symptoms who is checking electrical and gas outlets for 4 or 5 hours per day shows a large change on a rating scale of compulsions, one cannot really know whether the data translate into actual reduced checking of outlets (e.g., down to 2–3 hours per day?) or whether the patient is functioning better in the world. Many valid and reliable measures of psychotherapy are “arbitrary metrics” (Blanton & Jaccard, 2006). That means we do not know how changes on standardized measures translate to functioning in everyday life. Thus, a statistically significant change on standard, popular, valid,

and useful measures may not necessarily tell us how a patient is doing in the world.

To combat the inherent limitations of statistical significance and interpretation of measures, therapy researchers have devised indices of clinical significance. These indices are intended to reflect whether a change is a large and important difference. The three most common indices are showing that (a) high symptom scores at pretreatment fall within the normative range of nonclinic samples by the end of treatment; (b) large changes (e.g., two standard deviations) are made within individuals over the course of treatment; or (c) individuals no longer meet criteria for a psychiatric diagnosis (e.g., major depression) that served as a key selection criterion (see Kendall, 1999). Analyses of each of these indices, which are beyond the scope of the present article, have also indicated that they do not necessarily reflect palpable changes in everyday life. By *gedanken* experiment alone, one can show that a client can make a change that would meet criteria for clinical significance (on one of the commonly used indices) but still not change in everyday life in a way that makes a palpable difference, and vice versa (Kazdin, 2001).

The conclusion about the metrics of evaluation (statistical significance, effect size, clinical significance) is that in most cases it is difficult to tell the extent to which patients have been helped in their daily lives. There are many exceptions. When a problem (e.g., panic attacks) is measured directly in everyday life and treatment effects are strong (e.g., elimination of the attacks), one can say treatment really helped. Also, many outcome measures (e.g., glucose level, blood pressure, number of cigarettes smoked) can be interpreted more easily in terms of impact because they map directly onto other metrics that are not arbitrary.

A third fundamental issue has come from looking at results within a given study or between multiple studies that have been used as the basis for concluding a treatment is evidence based (De Los Reyes & Kazdin, 2006). Typically, in a single study, multiple measures are used to evaluate outcome, and only some of these show that the treatment and control conditions are statistically different. At the end of a study, one could say the treatment was efficacious, was not efficacious, or was mixed depending on which measures were examined or whether all measures were examined. EBTs are established because some measures in the study showed the expected effect. Replication is salvation in science and perhaps redresses the concern. Yet if one looks at two or more studies of the same treatment, one reconfirms that only some of the outcome measures reflect change within each of the studies, and a new issue emerges. The measures that show change and no change *within* a study are not necessarily the same measures that show these effects *between* or among the studies of the same treatment. In short, an EBT may have support for its effects, but within individual studies and among multiple studies, the results often are mixed (i.e., show different effects or no effects).

Some of the concerns about EBTs, their interpretation, and their applicability to clinical practice are subject to

empirical evaluation. For example, one concern is that individuals seen in controlled trials are not as severely disturbed as those seen in clinical practice, show fewer comorbid disorders, and come from situations and contexts that are not as dysfunctional or disturbed. This is not invariably true (Stirman, DeRubeis, Brody, & Crits-Christoph, 2003). Also, direct empirical tests have shown that severity, complexity, and comorbidity do not impede therapeutic change among EBTs (e.g., Doss & Weisz, 2006; Kazdin & Whitley, 2006). Yet there are multiple venues (patients, domains of functioning, contexts, and settings) where the concern would need to be tested to be put to rest. Even then, it is always possible to say, "But my patients are worse, different, older, more complex" than those in such studies, and no set of studies can resolve the concern definitively.

There are inherent limitations in the ways EBTs are discussed. Large segments of the literature usually are grouped together. In many ways, the concerns about EBTs reflect the uniformity myths that we were warned of decades ago (e.g., Kiesler, 1971)—namely, that conclusions ought not to be applied equally (uniformly) across the areas of interest (e.g., patients, problems, therapies). Objections and concerns are likely to be differentially relevant or applicable as a function of critical distinctions. For example, researchers in the area of my work (the treatment of conduct disorder—serious aggressive and antisocial behavior among children and adolescents) have identified several EBTs (e.g., multisystemic therapy, the multidimensional treatment foster care model, parent management training, functional family therapy). Many controlled studies have included outpatient, inpatient, and adjudicated and incarcerated samples with severely aggressive, violent, and antisocial behaviors, comorbid disorders, and impairment in multiple domains (Kazdin, *in press*). It would be difficult to challenge the work with the question "But are they real patients or really very disturbed?" Some of the objections might not apply to this particular area of research.

A central concern about EBTs involves the generalization of the results from controlled research to clinical practice. This concern is a cogent one. Although the concern is not new, it is newly relevant because of the progress of psychotherapy research and the challenges to clinical practice and service delivery (e.g., managed care, reimbursement issues, the likelihood of clinicians being told what they can and cannot do). Debates about whether EBTs ought to be extended to practice are not likely to be resolved, partly because of the way the discussion is framed and partly because of the methods by which research and practice are conducted, as I elaborate later.

Concerns About Clinical Practice

There are parallel concerns about clinical practice; they are parallel in the sense that they reflect objections and reservations with a history spanning decades and involve substantive and methodological issues, that is, what is done in therapy and how treatment is evaluated (e.g., Dawes, 1994; Garb, 2005; Hayes, Follette, Dawes, & Grady, 1995; Meehl, 1954). I highlight a few issues here as background

for later discussion. First is the concern about clinical decision making, judgment, and expertise as a guide to individual treatment. In contemporary terms, EBP consists of integrating evidence, clinical expertise, and patient considerations and then making a judgment of what to do. Clinical judgment as a way of integrating information has not fared well over decades of evaluation. There are models for case formulation and decision making but little empirical evidence in two key areas: reliability in decision making (consistency within therapists over time and among different therapists at a given point in time) and validity (that the decision makes a difference in the outcome when compared with a less flexible algorithm or an alternative case formulation model). The absence of research cannot be attributed to clinicians, but it conveys a more subtle facet of the research–practice split, showing that many critical clinical issues and concerns are not heavily researched.

The challenge of clinical decision making can be conveyed by the effort to "tailor treatment to meet the needs of individual patients." This statement is one we make and accept routinely in our clinical work, but researchers have yet to help us do that. A clinician might use an eclectic therapy and draw on multiple resources (including EBTs) to develop a treatment package suited to the individual. There are no formal or clearly replicable procedures for how to do this, in terms of selecting one or more treatments or components of treatment among all available therapies and deciding in what proportion and sequence they ought to be delivered to patients. As clinicians, we have an idea of how to do this, but it is not yet well established that different clinicians would select the same or a similar individualized treatment plan (i.e., reliability) when presented with the same case. And even if the treatment selected were the same, we do not know that it would make a difference or achieve palpable change in this patient's life (i.e., validity). Another treatment, one recommended by the best available evidence, might do just as well as, or better than, an individually tailored treatment, perhaps especially so if treatment technique is not the most critical influence (Wampold, 2001). We know from everyday life that when we are told "one size fits all," the garment in question tends not to fit anybody very well. One assumes this is true of therapy too, but how to individualize therapy for each person and how to show that doing so makes a difference are topics researchers have still not helped elaborate and that are difficult to defend.

Second, there is the issue of generalization of results in clinical practice. Those of us involved in clinical work are apt to say that the results from a controlled trial may not generalize to our patients because of differences in recruitment and patient characteristics. This point is cogent. However, the argument is a two-edged sword. Given the uniqueness of the client in front of me who is about to begin treatment, it is not clear on what basis I can generalize from a prior client or several prior clients.

Suppose two patients come in with similar anxiety complaints and personality styles. It is likely that the individuals will differ from each other in other ways (e.g., age,

stage of life, marital history, gender, ethnic and cultural identity, socioeconomic status, family history of anxiety, and available support systems). The possibilities for differences are dramatic in the clinical service I direct. Given the criteria for diagnosing conduct disorder, there are over 32,000 combinations of symptoms that individuals can have and still meet the diagnosis (Perepletchikova & Kazdin, 2005). On the basis of the diagnosis alone, and leaving aside scores on individual-difference, family, and contextual variables, how can one decide to generalize from one case to the next? If we really believe that “every patient is different” in ways that influence treatment decisions, then there is a problem in knowing how to make a decision that is well based and defensible. Of course, the decision is our own best guess, and that brings in the old but still pertinent topic of statistical versus clinical prediction as a way of integrating information in order to make a decision.

Deciding what to do with a given patient in clinical practice is analogous to a multiple regression equation with many variables to consider, each with a beta weight (i.e., a value showing how much it contributes to the decision or outcome). Restated in English, one must determine what variables ought to be considered, how they should be weighed in one’s decision making, and how one reached the conclusion about those variables and their relative importance. In research and in practice, a variable that may make a difference is always probabilistic. That is, individualized therapy based on my judgment of what is needed will not necessarily help this patient. Moderators, whether identified from research or clinical experience, are a set of variables related to an outcome but not invariably related to outcome in any individual case.

Third, the way cases are evaluated in clinical work raises an important assessment issue. Patient progress is often evaluated on the basis of clinician impressions, as opposed to systematic observations using validated measures. Without systematic measures, the reliability, validity, and replicability of results in clinical work are easily challenged. In my own clinical work, therapeutic changes I often see seem stark and qualitative (e.g., children stop hitting parents and teachers, no longer destroy property, or begin to interact prosocially with peers). Yet cognitive heuristics and normal memory processes (of recoding rather than recording events) no doubt influence my perception and rendition of the changes (e.g., Gilovich, Griffin, & Kahneman, 2002; Pohl, 2004; Roediger & McDermott, 2000). The absence of systematic assessment raises many obstacles to making claims about what happens in therapy and the accumulation of knowledge, as discussed later in this article.

There are additional concerns about clinical practice. One of them is the proliferation of new treatments. In child and adolescent therapy, for example, over 550 treatments are in use (Kazdin, 2000). Moreover, the number continues to grow. The vast majority of treatments have never been studied in controlled or uncontrolled trials. Not all are derived from an explicit theory of etiology or theory of change (e.g., horticulture therapy for a broad range of

psychiatric disorders, smudge art therapy to provide an outlet for fear). One should question when a new technique is warranted, when it ought to be provided, and when it is a treatment of choice.

General Comments

Clinical research and practice both raise concerns about treatment and what can be inferred, generalized, and applied to patients. Concerns about the research–practice split often can be reduced to empirical questions about treatment. One challenge to researchers occurs when clinicians question the generalizability of EBT findings—namely, whether an EBT works with “really disturbed patients.” Some studies answer affirmatively, but as I mentioned, there is an indefinite number of venues in which to test this. In fact, such studies could not test all plausible moderators in light of limited time and resources. Knowing that, researchers might ask, “Do we need to consider context and the individual or special features of individuals?” Why not just bolster, make more effective, and better codify (manualize) the treatments? However, we do need to consider contextual aspects of the interventions. For example, psychotherapy is more effective for some ethnic groups (if they show low acculturation to European American culture) when treatment is provided in their native languages and is specifically designed for minority groups (Griner & Smith, 2006). Culturally insensitive treatments can cause therapists unwittingly to select goals or embrace values that reflect the culture of the therapist rather than that of the patient (Comas-Díaz, 2006).

A bridge between research and practice is not likely to come from a finding that refutes a specific concern about an empirically supported treatment (“See, we told you that comorbid patients can be treated effectively.”) or a specific concern about clinical practice (“See, we told you that you have to know more about patients than their symptoms and what technique can ameliorate them.”). However, there are avenues where critical issues of concern to both researchers and practitioners come together and where advances in research and practice can build bridges between science and practice.

Rapprochement: Refocusing Research and Practice on Patient Care

There are some natural tensions between research and practice, and perhaps it would be wise not to attempt to resolve or even lament them. For example, we need the ascetic rigor of controlled experimental research to understand (e.g., test theory, identify predictors) and to evaluate the outcome effects of our interventions. This rigor includes RCTs, operational definitions, meticulous control of the intervention, quantification, precision in evaluating outcomes, and statistical significance, among other features. All those features that make us worry about the generalization of the findings from research to practice are strategically pivotal to experimental methodology. Psychology also needs the experience and expertise of those engaged in clinical work. Findings must be applied before all of the

critical answers are available. Judgment, expertise, and extrapolation of the findings are needed if we are to be able to act in the most informed, even if incompletely informed, way. As psychologists, we cannot wait until we have complete answers before addressing the immediate needs of our friends, relatives, and selves in navigating the shoals of life.

Interestingly, in their personal lives, researchers and clinicians convey more openness to the concerns of the “other side” and slightly less preoccupation with the tensions of research versus practice. For example, probably few researchers would refuse therapy for their children solely on the basis of the absence of efficacy trials. We move to treatment—almost any treatment—and alternative therapies as needed with the hope of help when the evidence cannot guide us or when it has guided us but to no avail. The researcher is not likely to say, “There is no solid evidence for any treatment, so I am going to withhold best guesses by experienced professionals.” Similarly, practicing clinicians, in need of help for their relatives, are likely to search the Web, read extensively, and make phone calls to medical centers and experts to identify what the evidence is for the various surgical, pharmacological, and other alternatives for their parents or children with significant medical problems. The clinician is not likely to say, “My relative is different and unique and the evidence really has not been tested with people like her, so I am going to forgo that treatment.” Indeed, the participants in the trials supporting the treatment may not be scrutinized at all or enough.

Psychologists need both research and practice, but we also need not assume that key differences and natural sources of tension are the only dimensions or facets. The unifying goals of clinical research and practice are to increase our understanding of therapy and to improve patient care. Also, differences in what research and practice do in relation to these goals actually can be mitigated. For example, a common way of conveying the differences between or different emphases of research and practice is to note the following: Research contributes to the knowledge base, and clinical practice applies that base to help people. Although this is true, it is not complete or very inspired. More important, this statement and the broader split it reflects do not achieve the following three critical goals:

- Optimally develop the knowledge base,
- Provide the best information to improve patient care, and
- Materially reduce the divide between research and practice.

I believe that expansion and slight shifts of emphases in both research and practice might better address these three goals.

Psychotherapy Research

I suggest three shifts in emphasis in research to advance the knowledge base, improve patient care, and reduce the gulf between research and practice. These include giving greater priority to (a) the study of mechanisms of thera-

peutic change, (b) the study of moderators of change in ways that can be better translated to clinical practice, and (c) qualitative research.

Study of mechanisms of change. Psychotherapy research focuses on many questions including the following:

1. What is the impact of treatment relative to no treatment?
2. What components contribute to change?
3. What treatments can be added (combined treatments) to optimize change?
4. What parameters can be varied to improve outcome?
5. How effective is this treatment relative to other treatments for this problem?
6. What patient, therapist, treatment, and contextual factors moderate or are correlated with outcome?
7. What processes within or during treatment are responsible for (not just correlated with) outcome (mechanisms of therapeutic change)?
8. To what extent are treatment effects generalizable across populations, problem areas, settings, and other contexts?

Debates about EBTs and their utility have emphasized Questions 1 and 8 in efficacy studies (Does treatment work?) and in effectiveness studies (Do the findings extend to practice settings?). Although a broad portfolio with the full range of questions is a wise investment, psychotherapy research could contribute enormously to clinical work by focusing more on the mechanisms of change (Question 7). The study of mechanisms of change has received the least attention even though understanding mechanisms may well be the best long-term investment for improving clinical practice and patient care.

By mechanisms, I refer to the processes that explain why therapy works or how it produces change. An RCT comparing treatment versus no treatment can establish a *causal* relation between an intervention and therapeutic change. Yet demonstrating a causal relation does not necessarily provide the construct to explain *why* the relation was obtained. The treatment may have caused the change, but we do not know whether the change can be attributed to specific or conceptually hypothesized components of treatment (e.g., cognitive restructuring, habituation, stress reduction, mobilization of hope) and how the change came about.

The distinction between cause and mechanism is readily conveyed with the familiar example of cigarette smoking. Cross-sectional and longitudinal studies with humans and experiments with nonhuman animals have established a causal relation between cigarette smoking and lung cancer. Establishing a causal relation does not explain the mechanisms, that is, the process(es) through which lung cancer develops. The mechanism was shown by describing what happens in a sequence from smoking to mutation of cells into cancer (Denissenko, Pao, Tang, & Pfeifer, 1996). A chemical (benzo[*a*]pyrene) found in cigarette smoke induces genetic mutation at specific regions of the DNA

that is identical to the damage evident in lung cancer cells. This finding conveys how cigarette smoking leads to cancer. The example is one from biology, but biology is not critical to the larger point. Mechanisms of action come from psychological influences as well. For example, the role of corporal punishment in the development and amelioration of child aggression has been demonstrated in cross-sectional and longitudinal observational studies as well as intervention studies (e.g., Patterson, Reid, & Dishion, 1992; Reid, Patterson, & Snyder, 2002).

In the context of psychotherapy, process research is often thought to identify mechanisms of change. Such processes as the therapeutic alliance, transference, and changes in cognition have been studied, in some cases extensively (e.g., Luborsky & Luborsky, 2006; Norcross, 2002). For example, the therapeutic relationship is often considered to be a key mechanism that explains therapeutic change. An often cited feature in support of this mechanism is the fact that the therapeutic relationship accounts for a large proportion (approximately 30%) of the outcome variance in psychotherapy. However, the percentage of variance accounted for cannot answer the question of the mechanism of therapeutic change.²

In most cases, some intervening processes (relationship, cognition) are shown to emerge or change *during* treatment and to predict outcome effects at the end of treatment. Two constraints have limited the identification of mechanisms in psychotherapy research. First, studies rarely establish the timeline, that is, that the proposed cause (e.g., alliance, cognitive change) comes before the changes in symptoms. Symptom changes and processes have to be evaluated repeatedly and concurrently in a given study to establish this. Merely assessing one variable (process) early and the other variable (outcome) later does not establish the timeline. Second, studies do not explain how the process unfolds to alter patient functioning, that is, how the process moves along a pathway that directly affects a particular outcome or set of outcomes (Kazdin, 2007).

There are many reasons to study mechanisms, but one in particular will help clinical work and patient care. Knowing critical factors of treatment and the processes through which they operate can optimize therapeutic change. What ought to be fostered and optimized in therapy to effect change? An obvious priority in health care is to transplant what is learned from research to clinical practice. All agree on this, but psychologists ought to clarify precisely what should be extended and why. Knowing how therapy works will allow us to optimize the processes critical to change.

Is there any instance in which we do understand mechanisms and in which that knowledge makes a difference in helping patients? Yes—the elegant work on fear conditioning and psychotherapy (Davis, 2006). There have been decades of research on Pavlovian conditioning of fear in humans and nonhuman animals. Conditioning as an explanation of fear acquisition and extinction as an explanation of fear reduction or elimination are useful paradigms for the processes involved in treatments of fear and anxiety. Conditioning and extinction of fear depend on a particular

receptor in the amygdala (*N*-methyl-D-aspartate; see Davis, Myers, Chhatwal, & Ressler, 2006). In nonhuman animal research, chemically blocking the receptor shortly before extinction training blocks (i.e., interferes with) extinction. Blocking the receptor after extinction training also blocks extinction, which suggests that the consolidation process can be interrupted. A compound (D-cycloserine) binds to the receptor and makes the receptor work better by enhancing extinction when given before or soon after extinction training.

Exposure-based psychotherapies are empirically supported therapies for anxiety and are based on an extinction model. Laboratory research has moved to therapy trials to enhance the mechanism responsible for extinction. RCTs comparing enhanced versus regular exposure therapy have shown that activating the critical receptor (with oral doses of D-cycloserine) improves the therapeutic effects of exposure therapy (i.e., augments extinction) among patients referred for acrophobia (Ressler et al., 2004) and social anxiety (Hofmann, Meuret, et al., 2006). The effects are evident immediately after treatment and at follow-up, months later. In short, understanding mechanisms of change can enhance the effects of treatment in clinical application. Evidence-based mechanisms of change could prove to be even more interesting or important than EBTs. We might be able to use multiple interventions to activate similar mechanisms once we know the mechanisms of change and learn how to optimize their use.

Study of moderators and translation to clinical care. Moderators refer to those characteristics that influence the intervention–outcome relation. For example, if therapy is more effective for patients with a particular characteristic (e.g., sex, ethnicity, gender identity, socioeconomic status, comorbidity), that characteristic is said to be a moderator. Moderators, including characteristics of the patient, therapist, and contexts, have been studied extensively for several decades. However, it can be

² If two variables are correlated (r), then one can identify the proportion of common or shared influence (r^2). For example, therapeutic processes (e.g., alliance) “predict” therapeutic change. Researchers often note that alliance accounts for a significant proportion of variance and sometimes even more variance than other influences (e.g., treatment technique). Further interpretation is often added to suggest that this must mean that the alliance is why treatment leads to change or is the most significant or important influence in therapy. These interpretations may be true, but they do not follow from the metric. There is nothing in the measure of percentage of variance that speaks to mediators or mechanisms. First, the shared variance between alliance and outcome could be huge, but that could be due to symptom change occurring before the alliance. Second, the therapeutic alliance can “account for” treatment outcome variance but itself be explained by one or more other variables such as symptom change that occurred before or at the same time the alliance began to form, by common method variance in the alliance outcome measures, or by characteristics of the patients before they began treatment. Each of these latter influences partially accounts for the percentage of variance connection (see Kazdin, 2007, for an elaboration). I am *not* asserting that the relationship is unimportant, but rather am commenting on the overinterpretation and misinterpretation of percentage of variance. It is useful to know multiple influences and the relative strengths of their effect sizes, but this information is different from evidence supporting a mechanism of action.

difficult to translate the findings to clinical work and decision making to help individual clinicians and their patients.

There are two problems that make translation of research to practice difficult: (a) the ways in which moderators are studied and reported and (b) not knowing how the moderator works across multiple conditions or treatments. My work, perhaps typical of research on moderators of psychotherapy, nicely illustrates the problem. Our research group has identified over 10 moderators of therapeutic change among children diagnosed with conduct disorder and referred clinically for outpatient services. Some of the variables are child age, reading achievement, severity of dysfunction, parent psychopathology, parent quality of life, and parent child-rearing practices. As an example, in one of our recent therapy outcome studies, barriers to participation in therapy were shown to predict therapeutic change in separate samples of children diagnosed with oppositional defiant disorder and conduct disorder (and with many other comorbid disorders as well; Kazdin & Whitley, 2006). Barriers to participation in treatment consist of obstacles families experience once they enter treatment (e.g., seeing treatment as too demanding, questioning the relevance of treatment) and are readily distinguishable from other moderators of treatment outcome (e.g., severity of child dysfunction, parent and family dysfunction, and stress and life events in the family). All families received an EBT. Families high in perceived barriers responded less well than did families low in such barriers when other potentially confounding moderators, as noted above, were controlled. Whereas the research part is clear-cut—a significant moderator was identified, the clinical part is not so clear-cut—what should be done with this finding when making decisions in clinical work?

Closer scrutiny showed that the higher barriers group responded significantly (statistically) less well to treatment than did the lower barriers group but still showed a large effect size ($d > 1$ across multiple outcome measures).³ In other words, barriers to treatment was a moderator, but what did we learn to help us apply this knowledge clinically? These patients still changed a lot on multiple outcome measures. Moreover, the study told us nothing about whether the moderator was specific to the treatment we offered or is a characteristic these patients carry with them to all forms of treatment. A clinician learning of the finding might silently ponder at the initial interview with such a patient, “I can see that this family coming to treatment is a ‘very high-barriers’ family.” However, the finding may not help in decision making.

Three changes would improve the research on moderators, improve patient care, and help bridge research and practice. First, it would be useful to report findings in a way that makes them applicable to clinical work. A variable may moderate treatment, but that does not necessarily mean that some individuals (those on the “unfortunate” side of the moderator, such as those with high barriers) will respond poorly to treatment. We need to know more than just the fact that a variable can significantly influence outcome.

Second, it would be helpful to know if a variable

predicts (moderates) responsiveness to a particular treatment or to multiple treatments. We cannot be confident about the answer because the pertinent research is rarely done. We would need to study a given moderator among different techniques, patient samples, and in different contexts. Without this information, we do not know whether the moderator will impede a particular therapy, several therapies, or all therapies that we might offer.

Third, it would be helpful to understand what facet of the moderator is relevant or how the moderator works. Sometimes a moderator can be a proxy variable, that is, one that stands for or represents some other influence. For example, patient age, sex, ethnic identity, or cultural identity might be moderators of dropping out of treatment or of therapeutic change among those who remain in treatment. Each of these variables may be accounted for or explained by influences with which they are highly correlated (e.g., severity of clinical dysfunction, stress, mismatch between therapist and patient ethnicity). Moderators are only correlates of outcome, but knowing more precisely the basis of the moderators may provide the option to intervene to see if those moderators that can be altered (e.g., parent stress) bear a causal relation to treatment outcome.

Clinical decision making is criticized for not relying heavily on research. Yet how variables are studied and the ways the data are evaluated and reported often make the translation of findings difficult. The usual *t* test or regression equation that shows that a variable makes a difference does not tell us what to do in decision making. Presenting the data in ways that facilitate decision making more directly, presenting precisely what proportions of cases are likely to respond in one way rather than another, and showing the strength of the effects for those who responded well or poorly are some of the options that would help clinical decision making. All decision making in clinical work will still be probabilistic; no matter what the research shows, the results may not apply to a particular patient even if he or she is very similar to those in the original study. Still, our best guess is to draw from the research—but we need more helpful ways of making that translation from research to practice.

Qualitative research. There is a third strategy for research that I believe would bridge the gap between research and practice. I mention this only briefly in part because quantitative research and null hypothesis testing dominate contemporary psychotherapy research and psychology in general. Qualitative research is not routinely taught in graduate schools (e.g., in the United States and Canada), and hence this option, while possibly useful in principle, cannot be as easily adopted as the other recommendations. Even so, in relation to bridging research and practice, qualitative research could play a special role. Qualitative methods meet the desiderata of science; the methods are systematic, replicable, and cumulative (see Berg, 2001; Denzin & Lincoln, 2005). (For those who

³ Convention has placed small, medium, and large effect sizes (*d*) at .2, .5, and .8, respectively (Cohen, 1988).

might be converted, I hasten to add that qualitative research even has software for quantitative data analysis, so all of us trained in the quantitative tradition can use numbers and statistics as a raft as we drift in the uncharted waters of experience.) The methods look at phenomena in ways that reveal many facets of human experience that the quantitative tradition has been partially designed to circumvent—in-depth evaluation, subjective views, and how individuals represent (perceive, feel) and react to their situations and contexts. For example, qualitative research can look at the experience of those who go through treatment and the thematic ways in which their lives and the lives of their partners are influenced. This information would be tremendously useful for connecting measures to new metrics that are not arbitrary and that reflect genuine changes in functioning, perception, experience, and meaning.

Qualitative research methods and their many variations are well suited to providing an understanding of the individual experience of patients, to codifying treatment changes, and to doing so in replicable ways. Also, qualitative research can both test and generate conceptual models and specific hypotheses. The methods could benefit clinical research by providing a rigorous way to codify the experiences of individuals (patients, therapists) and to do so in replicable ways. I am not advocating replacing RCTs with qualitative research. However, investing narrowly, whether in only one stock for a retirement plan or in a single methodological tradition such as quantitative psychology, invariably bears a cost. Different methods can reveal different facets of a phenomenon. In the context of psychotherapy research, scientific study of the individual would bring research much closer to the context of clinical practice. Among the benefits, it is likely that intensive and systematic study of experience would generate hypotheses directly applicable to clinical work. These hypotheses could be tested in quantitative and qualitative studies. Qualitative research would seem to be a natural way to bridge the divide between research and practice.

Clinical Practice

I have mentioned research emphases or shifts in priorities that could improve patient care, advance the knowledge base, and reduce the gulf between research and practice. Here I propose two parallel changes in clinical work to accomplish the same goals.

Use of systematic measures to evaluate patient progress. Patient progress in therapy is constantly evaluated. By systematic evaluation, I mean the use of psychological or other measures (e.g., if there are applications to physical health) that have or in principle could have reliability and validity and provide replicable information about the status of patient functioning. There are three related reasons to lobby for systematic assessment of individual therapy in clinical practice.

First and foremost, the key argument for systematic evaluation pertains to the primary goal of clinical practice: to provide high-quality care. Whether one uses an EBT or one's own brand of individualized treatment, one cannot be sure, in principle or practice, that the treatment will be

effective. Generalizing from research, experience, and their combination is always probabilistic and does not guarantee an outcome. EBTs of all sorts (e.g., aspirin, bypass surgery, plastic surgery, chemotherapy, antidepressant medication) cannot be depended on to produce the desired outcome without exception. We consider systematic evaluation as pivotal in research. Evaluation is as important in patient care because the individual is so important and because we do not have a guarantee of the result, no matter what the research or experiential base of the treatment(s) we use.

Second, it is important to monitor treatment effects in an ongoing way to make decisions about continuing, altering, or terminating treatment on the basis of how well the patient is doing. It is now well documented that some patients make rapid changes quite early in treatment (so-called sudden therapeutic gains), as has been shown, for example, in patients with depression or anxiety (e.g., Hofmann, Schultz, Meuret, Moscovitch, & Suvak, 2006; Tang & DeRubeis, 1999); others may not make expected changes and are unresponsive even to extended treatment (so-called signal-alarm cases; Lambert et al., 2003). And, of course, there are the gradations in between and the cases in which change occurs in some areas of functioning but not in others or at different rates among the various areas. Systematic assessment would permit finer delineations of therapeutic change than would more global clinical judgments and unsystematic assessment.

Third, systematic evaluation is intended to complement clinical judgment. Systematic measures are no substitute for clinical judgment, which may catch critical issues that a given measure was not designed to identify. However, the need for systematic evaluation stems in part from the limitations of judgment, perception, and memory and their implications for gathering information, as I mentioned earlier. We want systematic evaluation because of the complementary information it provides, quite apart from another advantage it confers in relation to bridging research and practice (adding to the knowledge base), which I note later in the article.

Recommendations and guidelines for using systematic evaluation of the individual case in the context of treatment are not new (e.g., Bloom & Fischer, 1982; Fishman, 2001; Hayes, Barlow, & Nelson, 1999; Kazdin, 1996; Meier, 2003). What are relatively new are the availability of measures that are validated for clinical use and the impending pressures on clinical practice to monitor patient progress. As one example, the Outcome Questionnaire 45 (OQ-45) is a self-report measure designed to evaluate client progress (e.g., weekly) over the course of treatment and at termination (see Lambert, Hansen, & Finch, 2001; Lambert et al., 2003). The measure requires approximately five minutes to complete and provides information on four domains of functioning: symptoms of psychological disturbance (primarily depression and anxiety), interpersonal problems, social role functioning, (e.g., problems at work), and quality of life (e.g., facets of life satisfaction). Total scores across the 45 items present a global assessment of functioning. The measure has been evaluated extensively and applied to over 10,000 patients. There are other avail-

able measures, including ones that can be individualized to patients and one's own practice, that can help with decision making and improve the quality of patient care. But they can do much more, as I note next.

Clinical practice can contribute uniquely to our knowledge base. We have taken as a given that research contributes to the knowledge base and that clinical practice is the application of that base. This is an exceedingly unfortunate way of conceptualizing the contributions of each domain because it fosters and maintains the research–practice gap. Clinical work can contribute directly to the scientific knowledge base.

A lamentable feature of our field is the knowledge lost in clinical practice. The accumulated experience of a given therapist retires when the therapist does. Typically, the knowledge cannot be used in ways that will help future patients. There are, of course, exceptions. For example, clinical supervision allows a clinician to pass on accumulated experiences and beliefs. Yet this information is not accumulated knowledge and does not add to the knowledge base in the archival sense that I mean here. As another exception, surveys ask practitioners about what they know, believe, and have learned. Surveys too have their place in codifying views and experiences, but they do not accumulate substantive findings.

Our field would profit enormously from codifying the experiences of clinicians in practice so that the information is accumulated and can be drawn on to generate and test hypotheses. There is no need for clinicians to become researchers and to do complex data analyses. Yet clinicians already are researchers in the sense of hypothesizing that a particular treatment combination will have particular effects and testing this hypothesis with the individual case. This work is not evaluated, codified, and accumulated in an archival way and therefore is lost.

Apart from improving patient care, systematic evaluation in clinical practice can make novel, important, and scientifically sound contributions to the knowledge base. The accumulation of cases over time, each of which is systematically evaluated, can yield new insights about treatment processes and outcome. The fact that the conditions are not controlled, in the sense of experimentation, does not preclude their role in adding significantly to the knowledge base. In most sciences (e.g., epidemiology, geology, meteorology, anthropology, economics, and infrared astronomy), major conclusions are drawn from careful analytic work and tests of hypotheses in uncontrolled situations rather than from experiments, although many fields have variables that can be controlled and tested experimentally.

I mentioned previously the availability of measures. Perhaps there is more than a measurement issue here. A critical deficit in our clinical training is in the evaluation of clinical cases in the context of “real” therapy and clinical practice. Indeed, we have learned some things about studying the individual that are not quite true. It is possible, for example, to draw causal inferences from studying the individual, to bring to bear information from the case, to help make rival hypotheses implausible, to test and generate

hypotheses, and to provide outstanding and astounding findings that will add to the knowledge base (see Sechrest, Stewart, Stickle, & Sidani, 1996). There are ways of arranging the clinical situation that even constitute *quasi-experiments*, a term legitimized by group research. Our current training feeds a clinical–research split by not conveying that evaluation can help individual patients and contribute directly to the accumulation of knowledge in ways that span a continuum of scientific rigor. In general, data obtained in clinical practice could directly contribute to knowledge, generate hypotheses to be studied in research, and, in the process, make treatment research more aligned with and relevant to clinical practice. Although I am emphasizing the contribution to the knowledge base, the contribution of the database to clinical care is also significant. A clinician can draw on prior cases and make inferences about what treatments to consider and combine and what the outcomes were in seemingly similar patients.

Accumulated data can be analyzed in partnerships with researchers—even if the data were saved until the end of one's career, the database could still contribute to knowledge. Objections are easy to raise (e.g., “What about HIPAA [Health Insurance Portability and Accountability Act]?” “What about informed consent?”). The same issues emerge in research, and there are solutions (e.g., data can be coded, records in research or practice must be protected) that can make these manageable and feasible within a practice setting. Let us not begin with what cannot be done. The task is to identify constructive changes that might be made in therapy research and practice to do what we do even better. Clinical experience, wisdom, novel hypotheses, and knowledge are often lost because they are not in a form that we codify and accumulate. We are letting knowledge from practice drip through the holes of a colander. We can plug up those holes to retain critical information, and we can feed this information into research designed to test hypotheses and add further support for what seems to be true from the data gathered in practice.

Direct Collaborations

I have highlighted directions in which both research and practice could move to reach across the research–practice gap. However, through the way in which I chose to organize these recommendations (separate sections on research and practice), I may have unwittingly underscored the gap. A genuine collaborative bridge could address that gap directly.

We need collaborations between colleagues who identify themselves as primarily from research and those who identify themselves as primarily from practice to work directly on this bridge and evaluate clinical practice. Those coming from a strict research perspective often lament the term EBP because there is evidence for many treatments (EBTs) but not much in the way of evidence that draws on and modifies the application of these treatments in light of clinical judgment, expertise, and contextual considerations in practice. Therapy will invariably involve judgment and experience. We may always want evidence seasoned by experience and clinical judgment; when the evidence is

unusually weak or barely existent, we want experience seasoned by evidence. Although medicine and medical analogies for some reason have become inherently offensive, they convey the point even more clearly than therapy. A medical EBT for a disease or dysfunction may be firmly established (e.g., surgery, organ transplant, medication with strong side effects), but moderating variables that have yet to be studied, including individual characteristics (e.g., age of the patient, comorbid disorders, likelihood of recovery) and contextual variables (e.g., family supports, living conditions), may influence or indeed dictate the decision as to whether to apply the EBT. Given the nature of clinical care, the role of judgment, expertise, and context ought to be studied directly.

There are many questions about EBP, but asking even the most basic would be a constructive beginning. Although not necessarily the most informative, the “horse-race” question might be the place to mobilize interest. Researchers often agree that comparisons of competing treatments are not the best place to begin. Yet these comparisons often galvanize attention and bring people into the area with more nuanced questions. The horse-race question would compare the application of an unadulterated EBT (e.g., exposure-based therapy for anxiety) with the application of the same treatment (or principles or modules of that treatment) but with clinical judgment, expertise, and context considered as a supplement. The decision-making process could be codified to identify themes and how they are applied. From experience with research, one can surmise the likely result. For many patients, the standard treatment (unadulterated) would be fine, but for other patients, integrating the intervention with clinical judgment, expertise, and context would make a palpable difference in treatment outcome. A researcher–clinician collaboration that helped identify the circumstances (moderating variables) in which judgment, expertise, and context are important would be enormously helpful in patient care and of course would bridge research and practice.

A second line of direct bridging work would underscore the distinction between the technique and its method of delivery. I am considering these as separate to make a point, although I readily acknowledge their overlapping (Venn diagram) nature. It might well be that a given EBT would be effective for many different populations and in many different contexts if it could be applied. However, contextual influences related to delivery (e.g., ethnic and cultural compatibility of the treatment with patient values, delivery in schools rather than clinics, recasting the intervention outside the context of “psychotherapy” or mental health services) may be critical. The effective intervention might not be deliverable unless clinical judgment, expertise, and context modify how the treatment is presented and described. I mentioned that delivery and technique overlap—key ingredients of the therapy might need to be modified as well, because without modification they might drive patients away from treatment or make adherence to treatment unlikely.

There are three interrelated ways to improve the outcomes of treatment. First, identifying effective and the most effective interventions can improve outcome. The development of EBTs is an illustration of this way. Second, understanding how and why an effective treatment works can improve outcome. Such research identifies what to focus on, manipulate, and optimize in treatment, as provided in an example earlier in this article. The third way to improve outcome is to identify moderators of treatment. Therapy works well for some individuals and in some contexts and works less well or not at all in others. The variables that delineate these different outcomes, that is, moderators, permit better triage. Outcomes for the population receiving treatment are better because one can direct patients to treatments from which they are more likely to profit. Clinical–research collaboration on EBP and on how moderators are utilized in practice can help improve outcome in this latter way.

The movement toward individualized medicine illustrates the utility of identifying moderators in clinical applications of treatment. Individualized medicine is not yet really individualized in the sense that each patient is uniquely evaluated. Rather, current work focuses on identifying a moderator (e.g., genetic characteristic, family history) that influences the impact of some intervention. (A more accurate term might be *moderated medicine*.) For example, levels of high density lipoprotein (HDL, or the “good cholesterol”) can be increased or decreased (good and bad news, respectively) with consumption of polyunsaturated fats in one’s diet. The beneficial or deleterious effects are moderated by a subtle genetic variation (polymorphism of apolipoprotein [Apo A-1]). Knowing about this genetic moderator is important for treatment recommendations because the same recommendation (increase polyunsaturated fats in one’s diet) could have opposite effects depending on the gene allele (Corella & Ordovas, 2005). Other examples have emerged in which practices (e.g., exercise, reducing cholesterol) appear to reduce risk for untoward outcomes (e.g., Alzheimer’s disease) but only for individuals without a genetic susceptibility to the disorder (see Gatz, 2007). These examples illustrate how knowledge of moderators can guide patients to and away from practices that may and may not make a difference in outcome; the knowledge can also lead researchers to pursue subgroups in which new information is lacking in order to improve clinical outcomes.

Conclusions

The hiatus between clinical research and practice has been heightened by advances in research, pressures on practice, and their combination. By combination, I refer to the public scrutiny of research. State legislatures and third-party payers, for example, are drawing on research to decide what is appropriate to do in practice, what is reimbursed, and what the rates of reimbursement will be. Our internal discussions about the merits of this or that treatment or set of studies and the generalizability of findings now have a larger audience. I mention these influences to convey that the issues addressed in this article are not occurring in a vac-

uum, free from priorities related to the funding of research and mental health services.

Two guiding questions served as the impetus for this article: (a) Are there better ways or special opportunities to bridge the divide between clinical research and practice than those currently in use? and (b) How can we improve the quality of patient care? I have suggested some changes in both research and practice that relate to these questions. In relation to research, more work is needed on the mechanisms of change—not correlates of change alone but explanations of precisely how therapy works. The best practice will continue to be based on the best science. Let us attempt to understand more about the many change processes and how they can be triggered, activated, exploited, and trained. This is different from disseminating treatment manuals and prescribing specific interventions as our primary focus.

Research can do more to identify moderators of treatment and how they make a difference (i.e., across one treatment or all treatments) and to report these results so that clinicians can make better decisions. I mentioned qualitative research as a third priority because it provides a methodology in which rigor and clinical relevance unite. I recognize that accredited programs in psychology may not even mention qualitative research yet may offer courses on the subject. I have taught research methodology at the graduate level at three universities. Within the quantitative/null hypothesis tradition there is so much to teach. Ongoing advances must constantly be added to the canon to prepare students competently. There is little time to train in other traditions (e.g., qualitative research, single-case experimental designs) given the scope of courses required, perhaps especially so for clinical and counseling psychology students, who have additional courses and experiences required for state licensing. Still, it is worth mentioning here because qualitative research allows intensive study of individuals in a scientifically rigorous way and for these reasons bridges the research–practice divide.

Shifts in emphases in clinical practice were also suggested. Monitoring treatment with systematic assessment was the first suggestion. Using an EBT, whether integrated with experience, judgment, and contextual considerations or not, of course, does not guarantee a positive treatment outcome. This is a major reason why the patient's progress should be monitored in a systematic way if at all possible. Our many unique contributions as psychologists include remarkable literatures on cognitive heuristics, memory, and perception that teach us why we need such tools. Research on measurement has provided reliable and valid tools that can be used in clinical and other applied settings to benefit directly the people we serve. Research on psychological treatments conveys what we can do to increase the likelihood of producing therapeutic change. There is no other discipline that can claim any of this or that is in such a position to provide empirically supported treatment and assessment.

Finally, assessment in the context of patient care will overcome what I consider to be a very regrettable loss of accumulated knowledge from clinical practice. We do not

benefit as a field from the accumulated practice of clinicians, with the rare exception of those whose groundbreaking treatments may spawn empirical research. For the rest of us, there is a potentially rich data set lost when our practices end. We do not need the clinician to become a researcher any more than we want the researcher to become a practitioner. Both the clinician and the researcher can mine the data for practical and scientific questions.

There are many ideas and models of integrating research and practice for clinical care, training in clinical psychology, and evaluating clinical work (e.g., Borkovec, 2004; Chorpita, Daleiden, & Weisz, 2005; Kendall & Beidas, 2007; Nelson & Steele, 2006; Schaughency & Ervin, 2006; Trierweiler, 2006; Weisz, Jensen, & McLeod, 2005). The efforts and need to integrate research and practice are timely and more important than ever before because of the stakes involved in academic and clinical training, research, practice, and health care in general. This article suggests emphases in different areas to improve patient care as well as to better unite research and practice. We can continue to argue why research may not reflect the conditions of clinical practice and how clinical decision making and judgment are difficult to defend. No one is served by such a dialogue, and the patients and the public at large are the most poorly served. The task for psychologists is to consider how we can bring together more constructively the critical facets of our field to improve our understanding of patient care and delivery of services to make a difference. Although efforts to bridge research and practice are not new, there are now special opportunities as improved treatments and measures have become available for clinical use and as the dialogue among those involved primarily in research or practice has increased (e.g., APA Presidential Task Force on Evidence-Based Practice, 2006; Goodheart et al., 2006).

In the process of bridging research and practice in the ways I am suggesting, we would serve psychology in general as well. Our training in theories, methods, hypothesis testing, assessment, and data evaluation is unique among the many disciplines that deliver services. Not only do our research-generated treatments make our work special, but our methods of evaluation can improve patient care in ways that render psychology and clinical practice by psychologists unique.

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