Psychotherapy Practice and Research

Repairing a Strained Alliance

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Although the gap between psychotherapy practice and research has been present for some time, recent pressures for accountability from outside the system—managed health care and biological psychiatry—necessitate that we take steps to close this gap. One such step has been for psychotherapy researchers to specify a list of empirically validated therapies. However, as researchers who also have a strong allegiance to clinical practice, we are concerned that the conceptual and methodological constraints associated with outcome research may become clinical constraints for the practicing therapist. We firmly believe that, more than ever before, the time is ripe for us to develop a new outcome research paradigm that involves an active collaboration between researcher and practicing clinician.

Sociologists have long documented how economic and political forces can have an unexpected and very powerful impact on various scientific disciplines (Cole & Cole, 1973; Merton, 1938/1970). In the early 1940s, World War II created numerous challenges that resulted in a huge collaborative effort in the field of physics and other disciplines. Similarly, the Soviet Union’s Sputnik in the late 1950s gave rise to a host of scientific and technological advances that might not otherwise have occurred. Although there is no doubt that the field of psychotherapy has made important progress in the past decade or two, it is likely that the most dramatic changes we are about to witness are likely to come from outside forces. These external factors consist of pressures to justify empirically how we practice clinically, coming in the form of challenges from biological psychiatry and managed health care.

The impetus for writing this article was our concern that, in response to such pressures, psychotherapy outcome researchers may have overreacted and moved the field in the wrong direction, in other words, away from the day-to-day context of clinical practice. Although psychotherapy research has appropriately increased in methodological rigor, it has become overly dependent on the “clinical trials” method to determine how to best treat “disorders.” In addition to condoning the medicalization of psychotherapy, psychotherapy researchers may unwittingly be playing into the hands of third-party payers in placing unwarranted emphasis on the putative fixed efficacy of specific interventions (e.g., Task Force on Promotion and Dissemination of Psychological Procedures, 1995). It is in this context that we believe it essential for clinician and researcher to join forces to come up with a better way of demonstrating how our various psychotherapy interventions are not only empirically grounded but also clinically meaningful.

We wrote this article as psychotherapy researchers who also have a strong affinity for clinical practice. As researchers, we feel more than a little bit of guilt for having become involved in a research paradigm that has been too far removed from the real world of therapy practice. This is certainly not a new phenomenon, and it has even been suggested in the past that “there appears to be an inverse relationship between the frequency with which a treatment form is actually used by practitioners and the frequency with which that treatment has been studied” (Parloff, 1979, p. 304). At present, however, we are particularly concerned that the methodological and conceptual constraints associated with outcome research may very well turn into clinical constraints for the practicing therapist (cf. Frances, 1994). Yet, we continue to remain strong advocates of psychotherapy research. In many respects, our dilemma may be thought of as reflecting a conflict between a wish and a fear: Our wish is that therapy interventions be based on psychotherapy research; our fear, however, is that they might.

The Worlds of the Scientist and Practitioner

Although therapists and researchers often begin with similar professional training, they eventually end up living and working in very different worlds. We are keenly aware of this distinction; between the two of us, we have spent

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the last 50 years of our professional lives working in both of these worlds. Wolfe has recently completed a 22-year tenure in the extramural program of the National Institute of Mental Health (NIMH), overseeing the research grant portfolio in psychotherapy research. Goldfried has devoted his career to the development and evaluation of intervention procedures and the study of the therapeutic change process. In addition to our involvement in psychotherapy research, we have each maintained a practice of psychotherapy. Living in both these worlds, we have been able to witness firsthand the different sets of demands and limitations associated with research and practice. The professional lives of clinicians are based on reliable referrals, the ability to establish good therapeutic alliances, and a demonstration of clinical effectiveness. Researchers, on the other hand, put much of their energy in publishing, obtaining research grants, and achieving professional recognition.

One of us once participated in a roundtable discussion at a psychotherapy research conference, in which the focus was on how to best transmit psychotherapy research findings to the practicing therapist. Throughout the discussion, the point was continually made that the practicing clinician was “not a good consumer” of research findings. Were this a meeting of a corporate board seeking to understand the failure of its product to reach the intended market, the likely discussion would not have been on the shortcomings of the consumer but on what could be done to make the product more appealing. What this reflects is the somewhat dysfunctional relationship between scientist and practitioner. It is clearly not an egalitarian partnership, but rather one in which each views the other with a certain amount of disdain. To a very great extent, each has difficulty in understanding the needs and concerns of the other, and each rarely validates the legitimacy of the other’s activities.

Although psychotherapy researchers presumably are engaging in activities that can have implications for practice, it is fairly well accepted that researchers typically write for other researchers; the implications for clinical practice, more often than not, are an afterthought. The description of the methodological rigor that constitutes the current state-of-the-art in psychotherapy research and the unique set of jargon associated with it (e.g., “treatment fidelity” and “end-state functioning”) receives far more emphasis in the literature than does the kind of information that clinicians would find useful to their work. We are not suggesting that our standards for methodological rigor or the way research is presented to other practitioners is an associated agoraphobic avoidance (Turner, Williams, Beidel, & Mezzich, 1986). The finding that patients with panic were more likely than control participants to experience panic attacks in laboratory settings in which they were instructed to hyperventilate led to the clinical strategy of actually provoking panic attacks within the therapeutic session as a way of exposing patients to and encouraging them to cope with such attacks (Barlow, 1988). Laboratory findings that the resting heart rates of patients with panic tend to be higher than those with other anxiety disorders suggest that clinicians may need to focus on lowering the overall chronic overarousal of patients with panic disorder. Also, the prominent role played by the catastrophic interpretations of unusual bodily sensations in the development and maintenance of panic disorder has led to the emergence of a strong cognitive focus in the treatment of panic disorder (Barlow, 1988).

### Research Bases of Psychotherapy

There are three primary sources of information that can inform the practicing therapist about how to proceed clinically. These include (a) basic research on clinical problems, (b) research on the process of change, and (c) psychotherapy outcome research.

### Basic Research on Clinical Problems

As Arkowitz (1988) suggested, basic research on psychopathology can assist practicing therapists by providing them with information about the “what” that needs to be changed. In essence, such research can provide invaluable findings relevant to the variables/dynamics/determinants that are associated with the kind of patient difficulties one is likely to encounter clinically.

Take the example of panic disorder, in which a shift was made in the centrality of panic attacks between Diagnostic and Statistical Manual of Mental Disorders, Third Edition (DSM–III; American Psychiatric Association, 1980) and the DSM–III–R (revised; American Psychiatric Association, 1987). At present, panic is seen as being primary, with the avoidance associated with agoraphobia viewed as a means of coping with the fear of panic. This change in emphasis was based on findings that panic attacks temporally precede the development of agoraphobic avoidance behavior (Tyrer & Himle, 1985) and that there appear to be comparable characteristics of patients with panic disorder, whether or not there is an associated agoraphobic avoidance (Turner, Williams, Beidel, & Mezzich, 1986). The finding that patients with panic were more likely than control participants to experience panic attacks in laboratory settings in which they were instructed to hyperventilate led to the clinical strategy of actually provoking panic attacks within the therapeutic session as a way of exposing patients to and encouraging them to cope with such attacks (Barlow, 1988). Laboratory findings that the resting heart rates of patients with panic tend to be higher than those with other anxiety disorders suggest that clinicians may need to focus on lowering the overall chronic overarousal of patients with panic disorder. Also, the prominent role played by the catastrophic interpretations of unusual bodily sensations in the development and maintenance of panic disorder has led to the emergence of a strong cognitive focus in the treatment of panic disorder (Barlow, 1988).

### Psychotherapy Process Research

In contrast to basic research, which deals with the “what” of therapeutic change, psychotherapy process research deals with the “how” (Arkowitz, 1988). Process research dates back to the 1940s, when Rogers and his associates took the bold step of making and studying wire recordings of psychotherapy sessions (Strupp & Howard, 1992). This eventually led to the development of numerous procedures for analyzing therapy sessions in the hope of better understanding the nature of the therapist–patient interaction (Kiesler, 1973). However, such research activities soon fell by the wayside (Strupp, 1973). Much of this early psychotherapy process research involved discrete and isolated transactions between therapist and patient (e.g., silences) rather than functional units that might conceivably reflect the process of change.
In more recent years, a new generation of process research has evolved in which the primary focus is to look at those aspects of psychotherapy that are likely to contribute to change (Greenberg & Pinsof, 1986). Rather than studying “psychotherapy process” in general, this more recent approach to process research has emphasized studying the “process of change.” The question addressed by this approach to psychotherapy process research is as follows: “What has the therapist done to have a particular impact on the patient?” In many respects, we can think of such research as an attempt to construct maps that can more clearly depict the therapeutic change process. The implication for the practicing clinician becomes quite clear if the research question becomes slightly rephrased to read, “What can the therapist do to have a particular impact on the client?”

Among the many process research findings having important clinical implications are those indicating that in a psychodynamically oriented intervention, the therapist’s interpretations that are based specifically on case formulations have more of an immediate emotional impact than do transference interpretations that are based on general theoretical considerations (Silberschatz, Fretter, & Curtis, 1986). In process analyses of cognitive therapy for depression, an intriguing and somewhat unexpected finding has been that clients’ level of emotional experience and a focus on their relationship with their parents are positively associated with treatment outcome (Castonguay, Goldfried, Wiser, Raue, & Hayes, 1996; Hayes, Castonguay, & Goldfried, 1996).

It is of particular interest that many of the coding systems used by psychotherapy process researchers can be used as training guidelines for practicing therapists. Thus, Benjamin’s (1993) Structural Analysis of Social Behavior (SASB) can allow the clinician to monitor on an ongoing basis the nature of the therapeutic interaction along the dimensions of control and affiliation. Safran and his colleagues (Safran, Crocker, McMain, & Murray, 1990) have provided guidelines for recognizing aspects of the therapeutic interaction in which there may be a strain in the alliance and what might be done to alleviate this situation. The work of Greenberg, Rice, and Elliott (1993) on therapeutic markers can alert therapists to those points in the therapy session that can call for them to proceed therapeutically in different ways. Methods for classifying a therapist’s style of responding can provide clinicians with guidelines for how to word a verbal interaction (Hill, 1986; Stiles, 1992). Finally, process research on the therapeutic focus can have important clinical implications for identifying the dynamics/determinants of clinical problems, thereby assisting in both the initial and ongoing case formulation (Goldfried, 1995).

**Psychotherapy Outcome Research**

In 1950, Snyder provided the first review of psychotherapy research to appear in the *Annual Review of Psychology*, in which he was able to cover the work done in the entire field within a single chapter. Four decades later, Goldfried, Greenberg, and Marmar (1990), representing cognitive-behavioral, experiential, and psychodynamic views, were barely able to touch on the high points of research that had been conducted in the treatment of the wide variety of clinical problems that had been studied over the previous four years. During the 40-year interval between these two reviews, outcome research in psychotherapy increased at a dramatic rate, moving through three generations of methodological vigor.

The first generation of outcome research occurred between the 1950s and 1960s. On the basis of the work of Rogers (Strupp & Howard, 1992) and that carried out by researchers at the Menninger Foundation, the University of Pennsylvania, and other facilities, the question that was addressed was whether or not psychotherapy was effective in producing personality change (Strupp & Howard, 1992). What characterizes this very early phase in the history of psychotherapy outcome research is that there was little specification or differentiation among the forms of therapy that were addressed and that the clinical problems and the nature of outcome were usually considered in a very general way (Kiesler, 1966).

During the 1960s and 1970s, we moved into the next generation of research, in which there was a change in the question that was being addressed by psychotherapy outcome researchers. Largely because of the efforts of behavior therapists, the question became not whether “psychotherapy works” but which specific procedures are more effective in dealing with a specific clinical problem (Franks, 1969). Consequently, various target problems were dealt with (e.g., phobias and unassertiveness), and specific therapy interventions that were based on written guidelines, in which there was random assignment of patients to different treatment conditions, became the state of the art. Although a methodological advance over the earlier approach to answering the outcome question, this second generation was limited because college student volunteers rather than actual patients were used in much of the research, and graduate students often served as the therapists. Still, it set the stage for the next generation of research.

Beginning in the 1980s, psychotherapy research moved onto a third generation of methodology. Continuing in the tradition of Generation II, which involved time-limited interventions that compared different treatments for particular clinical problems, Generation III research has been called *clinical trials*—the terminology associated with drug studies. Further reflecting the shift toward the medical model, the methodology constituting this most recent approach to psychotherapy outcome research involves the use of DSM diagnoses, particularly on Axis I. In addition, highly detailed therapy manuals are used, whereby therapists are monitored for their adherence to the particular procedures being studied.

Many of these methodological “upgrades” were fostered and ratified by the NIMH Treatment of Depression Collaborative Research Program, in which the original purpose was to test the feasibility of conducting a multisite clinical trial in psychotherapy (Elkin, Parloff, Hadley, & Autry, 1985). Although this question was intrinsically of
great interest to psychotherapy researchers, reflecting the desire of the field to develop a cumulative knowledge base regarding the effectiveness of different forms of psychotherapy, it was shaped by other more social and economic variables, such as (a) the growing hegemony of the psychiatric model of mental illness; (b) the purported success of efficacy studies of various psychotropic pharmacological agents; and (c) the need to respond to institutional pressures from Congress, health care agencies, third-party payers, and the public to provide convincing data of the effectiveness of psychotherapy (Parloff & Elkin, 1992).

The immediate result of the NIMH study has been to encourage the view that the conduct of large clinical trials in psychotherapy is not only feasible but mandatory. Such research now has assumed the mantle of final arbiter of efficacy in the field of psychotherapy research. These studies are extensive, expensive, and difficult to conduct. It should be noted that these large studies focus primarily on the use of a particular theoretical approach in reduction of the symptomatology associated with specific DSM diagnosed disorders.

One of the casualties of this movement to large-scale clinical trials—at least with respect to NIMH research grant funding—was psychotherapy process research. Process research was increasingly subordinated to the task of establishing the efficacy of standardized psychotherapies. Its deemphasis was rationalized by the contention that it was not necessary or useful to conduct research on the process of a specific psychotherapy until it had been established that psychotherapy was effective. As a result, NIMH funding for psychotherapy process research began to plummet in the late 1980s. The grant portfolio in psychotherapy process research was reduced by over 60%, going from 16 grants in 1986 to 6 grants in 1990 (Wolfe, 1993).

The medical model of outcome research, with its emphasis on disorders and their symptoms in current clinical trials, also has the particular limitation of neglecting the key determinants/dynamics that clinicians know well to be essential to the change process. This limitation is illustrated in a recent NIMH conference devoted to developing a standardized outcome battery for panic disorder research (Shear & Masera, in press). After specifying many of the indicators of symptomatology that should be included in any clinical trial focusing on panic disorder, the conference participants concluded that it was “not essential” to include any evaluation of personality variables in measuring outcome. This somewhat surprising conclusion was based on the lack of clarity in “comorbidity” between Axes I and II as well as the difficulty in measuring “personality disorders.”

Like our clients, we as psychotherapy researchers become socialized to think and behave in ways that can be limiting. In our own involvement in a 1994 American Psychological Association (APA) sponsored conference at Vanderbilt University on the development of a core assessment battery for outcome research, we encountered a similar phenomenon. Representing a cognitive-behavioral orientation, we found ourselves trying to convince psychodynamic therapy researchers about the importance of including such personality variables as interpersonal skills and self-concept in an outcome battery rather than only measures of symptom reduction. This was reminiscent of similar discussions between psychodynamic and behavior therapists in the 1970s, with the exception that the positions were exactly reversed!

On the basis of the assumption that the most recent generation of research is better than the earlier ones, there tends to be a neglect of findings obtained by psychotherapy researchers prior to the 1980s. Despite the fact that practicing clinicians will acknowledge that interpersonal assertiveness frequently plays a very important role as a determinant/dynamic in many clinical problems (e.g., depression and panic attacks), “assertiveness” remains a 1970s concept and appears to be out of fashion. Indeed, because it does not represent a DSM category, it is unlikely that NIMH funding would be available to psychotherapy outcome researchers who wanted to study this clinically relevant characteristic.

Our sense is that the field has become caught up in a research paradigm that does not faithfully reflect clinical reality. The very nature of our current research methodology, constrained by a need for DSM diagnoses, limits the kinds of questions that are studied. For example, the typical finding that patients with personality disorders have poorer prognoses in the treatment of various clinical symptoms has raised the question about the ways that Axis II disorders are related to Axis I disorders. We would think that a more relevant question to ask would be about how certain personality characteristics contribute to the development of anxiety and depression, such as the extent to which patients become depressed because of patterns of interpersonal behavior that bring about negative reactions from others; unfortunate life circumstances; the failure to grieve a loss; a passive interpersonal style in responding to problematic life issues; a tendency to cognitively distort; or a prevailing view of oneself as a failure.

In the typical clinical trials paradigm, one “pure-form” theoretical approach is compared with another in the treatment of a DSM category. Unlike clinical practice, in which we as therapists often find it more effective to use interventions associated with different therapeutic orientations (Norcross & Goldfried, 1992), our current research methodology allows little room for taking into account the relevant patient determinants/dynamics that may influence what we should do clinically. Instead, different therapy interventions are administered, which typically focus on the preferred variables associated with a given theoretical orientation (e.g., cognition, behavior, affect, and interpersonal systems). These different therapy approaches are compared, often resulting in findings that fail to result in a differential effectiveness between orientations.

Moreover, our outcome research is characterized by a basic dilemma with respect to the type of patients that eventually participate in treatment efficacy studies. This quandary might be called the interpretability/generalizability dilemma. In order to improve the interpretability...
of findings, rigorous inclusion and exclusion criteria are used for the selection of research patients. As the number and rigor of these criteria increase, the generalizability of the findings from treatment efficacy research decreases. However, any effort to reduce the gap between research patients and “real-world” patients leads to the decreased interpretability of research findings. For example, the typical panic disorder patient seen in a private practice setting would often meet criteria for other Axis I and II disorders. Yet the available treatment-efficacy research data are typically based on patients whose clinical pictures are not nearly as complicated. This leads to a translation problem when practitioners attempt to apply empirically validated treatments to real-world patients.

This fact was humorously yet sadly brought home to one of us (Wolfe) when I had organized an NIMH research workshop a number of years ago on the treatment of specific and social phobias. After listening to several of the world’s experts declaim on the ease with which simple phobias are treated, often within one to five sessions, he said with great exasperation: "I don’t know what kinds of patients are being treated in such studies, because the simple phobic patients that I see in my private practice are often quite difficult to treat and require protracted periods of treatments.” One of the participants responded, "Barry, you are overlooking the first law of research: Don’t use real patients.” This was said in jest, but the reality of the clinician–researcher gap was very much apparent in the uneasy laughter that exploded in the room.

Addressing the gap between psychotherapy outcome research and the way psychotherapy is conducted in the real world, Seligman (1995) similarly concluded the following: “The efficacy study is the wrong method for empirically validating psychotherapy as it is actually done, because it omits too many crucial elements of what is done in the field” (p. 966). Although lacking the tight controls and internal validity characteristic of Generation III research, the more externally valid Consumer Reports (1995) survey of psychotherapy patients is offered as a means of documenting therapeutic effectiveness.

The Use of Outcome Research by Policymakers

The gap that exists between research and practice is certainly not a new issue. Indeed, some may argue that it is a lost cause, and the entire topic should be laid to rest. However, the current crisis associated with the influence of insurance companies on how psychotherapy is practiced makes it imperative that we examine how the results of our outcome findings may be used by policymakers. This concern was foreshadowed in an insightful article published several years ago by Parloff (1982), when it seemed that some form of national health insurance might be adopted. Parloff warned us about how the efforts of psychotherapy researchers could be misused by policymakers, subtitling his article “Bambi Meets Godzilla.” This issue has never been dealt with adequately, and we are currently concerned not only about how our research findings will be used to limit us clinically, but also how we may inadvertently be encouraging this.

To begin with, our research base of outcome research is insufficient because of what it does not tell the practicing clinician. The most general problem, as Havens (1994) noted, is that there is a gap between the global nature of research findings and the usually specific nature of clinical dilemmas. More specifically, we would suggest that clinical trial data do not tell us the following:

- How to treat patients who suffer from more than one Axis I disorder, which is more typical of what the clinician faces on a daily basis.
- Why treatments work. It may well be that there are mechanisms and processes other than those outlined by the specific brand-name treatment under study that are responsible for whatever changes occur in the patient’s symptoms.
- How to treat underlying “personality” issues and other determinants/dynamics that may be directly related to symptoms of a particular disorder. It is no longer tenable, if it ever was, to argue that these issues either do not exist or are not relevant for effective treatment.
- What to do with variations within a given disorder. For example, some years ago, Chambless and Goldstein (1988) had identified simple and complex forms of agoraphobia, which required somewhat different treatment plans. Similarly, different forms of depression have been identified, including achievement-based and abandonment-based forms of depression (Blatt, 1974). It is not clear from the available efficacy data what the practicing clinician should do with each subtype of disorder.

Clinical trial efficacy data are also mute on how to resolve clinical impasses and dilemmas that typically characterize psychotherapy, independent of the specific nature of the disorder under treatment. That our outcome research often overlooks these clinical realities is illustrated in an observation made by Raw (1993), a practicing therapist who lamented the difficulties he had in applying outcome findings to his clinical work. He reported attending a conference in which a well-known therapy researcher described his method of dealing with the problem of noncompliance, which was to tell patients that if they did not follow through on what they were supposed to do, they could not be included in the study. Not only does this procedure have limited use to practicing therapists, it also throws into sharp relief how our research methodology severely constrains our ability to generalize to the clinical situation.

We realize that these observations fly in the face of some recent attempts that are being taken to translate research findings into recommendations for clinical practice. Toward the goal of basing our therapeutic interventions on a stronger empirical foundation, the APA Division of Clinical Psychology’s Task Force on Promotion and Dissemination of Psychological Procedures (1995) was formed “to consider methods for educating clinical
psychologists, third party payers, and the public about effective psychotherapies” (p. 3). After reviewing the outcome research literature—primarily Generation III—the task force came up with a list of “empirically validated treatments” (EVTs) and recommended that EVT be used as criteria by site visitors for APA accreditation of graduate programs, the accreditation of internship facilities, approval of continuing education credit, and third-party payers.

As psychotherapy researchers, we certainly understand and support the intent of the task force. There are far too many therapists who justify what they do clinically on the basis of what has been done in the past, and they desperately need to change their practice in light of current findings. However, we have grave concerns about the possible impact of the task force’s report. A question that we often ask ourselves is whether we would want to refer a friend to a therapist whose training was based on the manual-driven intervention that has been used in our clinical trials. We are very concerned that the available outcome research findings will be taken too literally (a) by therapists, who learn only the pure-form interventions used in clinical trials; (b) by clients, who expect that their progress will parallel those carefully selected participants in the research and be symptom free by a certain number of sessions; and (c) by managed care policymakers, who will limit clinical practice so that it conforms to the methodological constraints associated with psychotherapy outcome research.

There is yet another dilemma involved in this use of clinical trial efficacy data, and that is the impossibly large volume of research that will be required to establish an EVT for a particular disorder. In 1980, Parloff and Wolfe (as cited in Herink, 1980) had calculated that the 250 identified brand-names of therapy could be reduced to 17 generic forms, and the number of groupings of patients from the 150 disorders listed in the DSM-III could be collapsed to about 50. Parloff (1982) later reported, “Even this gross oversimplification would require 6,800 clinical trial studies” (p. 724).

In the current research scene, “Parloff’s parody” still applies. Currently, the DSM-IV (American Psychiatric Association, 1994) includes over 300 separate disorders. Even if we were to collapse them into 100 different patient groups and reduce the therapeutic approaches to 10, we are talking about 1,000 clinical trials just to have 1 study of each treatment for each separate patient group. And if we adopt the APA Division of Clinical Psychology’s task force’s recommendation of requiring at least 2 studies to verify the efficacy of a potential EVT, then we are up to 2,000 studies. Our question is, who will fund this quantity of research? As many of us have so painfully learned in recent years, it will not be the NIMH. Unless the field of psychotherapy research acquires a patron with extremely “deep pockets,” the task of establishing EVT for the full range of DSM disorders is clearly not feasible.

**Where Do We Go From Here?**

There is no question that the field of psychotherapy is approaching a crisis. Whether this crisis can also be turned into an opportunity remains to be seen. However, one thing is clear: The time is ripe for us to examine very seriously the gap between research and practice and take bold steps to close this gap. We cannot afford to ignore the limitations of our current outcome research paradigm, as it very well may dictate the future direction of psychotherapy in general.

We suggest three tiers of recommendations for closing the gap. These include (a) extending our current psychotherapy outcome research paradigm; (b) increasingly focusing on a new paradigm of psychotherapy research that emphasizes the elucidation of the various processes of change and individualizes the interventions being studied; and (c) increasing collaboration between clinicians and researchers.

**Extending Our Current Psychotherapy Paradigm**

Standardized therapies that have been tested and found efficacious need to be applied in more everyday settings, such as primary care settings, outpatient clinics, and community mental health centers. At this writing, the NIMH has been attempting to support increasing research out of its Services Research Branch to test the clinical effectiveness of efficacious, standardized psychosocial treatments. Such research can give us clearer indications of the effects of treatments once they are applied in more real-world settings than those in which their efficacy in controlled settings is tested.

If we are to continue conducting clinical trials of standardized treatments, we must also focus on more than just the symptoms of a particular Axis I disorder. The high relapse rates and mediocre recovery rates that are associated with tested therapies in which efficacy has been established for specific disorders indicate that symptom-focused treatments are necessary but not sufficient (Wolfe, 1994). Clinical trials should now routinely study life events, personality characteristics, as well as other theoretically derived variables that are presumed to be involved in the generation or maintenance of symptoms of the disorders under study. A related issue is the need to study patients who are "comorbid" for more than one DSM disorder, particularly those who would meet criteria for an Axis II as well as an Axis I disorder.

In commenting on our current outcome research paradigm, other psychotherapy researchers (e.g., Borkovec, 1994; Davison, 1994; Davison & Lazarus, 1994) have noted that the typical practice of comparing one type of therapy with another is severely limited in that it provides no conceptual understanding about the clinical problem or how the treatment might work. Answers to these questions can be built into the research design, argued Borkovec, if we were to include conditions that were composed of different aspects of a treatment package, either individually or in varying combinations. This is the research strategy used in the second generation of outcome research on behavior therapy techniques, as when treatment conditions involving relaxation and imaginal exposure to fearful situations were compared with system-
atic desensitization, involving both together. A model such as this can be used to study other therapy orientations, such as a comparative investigation of the effects of transfer-ence interpretations that are linked to the past (i.e., parental reactions) or involve reference to the therapist without links to parental figures.

An Alternative Paradigm for Psychotherapy Outcome Research

Despite its merits, the above-mentioned extensions of our current research paradigm will neither provide us with all of the information that a clinician will need, nor will it substantially close the gap between research and practice. What we need is an alternate research paradigm for building and testing an effective approach to psychother-apy, one that both emerges from therapist–patient inter-actions and individualizes the intervention for the particular case at hand. The implication of such research is that what needs to be specified and replicated is not brand-name therapies but identifiable processes of patient change and the therapist behaviors that bring these about. Such research would be focused on at least two different types of change processes: (a) intrapersonal and inter-personal indications of patient change and (b) therapist actions that reliably bring about such patient change. For example, what therapist behaviors and patient indicators of change are present when intrusive traumatic memories have been emotionally processed (allowing the patient to rebuild his or her sense of trust in people)?

The need to develop a research paradigm that also individualizes the intervention on the basis of an initial assessment and case formulation is essential for closing the clinical–research gap, a point that has been discussed by Fishman (1981) and Persons (1991), both practicing clinicians. Referring to the second generation of outcome research, Fishman (1981) commented on the gap between research and practice by noting that “in their quest for ‘topographical equivalence,’ researchers tend to lump together all subjects with the same manifest problem, regardless of the etiological, mediational, contextual, and maintenance facts that underlie and act to perpetuate the maladaptive pattern” (p. 244). This issue was expanded on a decade later by Persons (1991) with the third gen-eration of outcome research. Without an individualized assessment and case formulation as a key element in our outcome research, argued Persons, there is no way that these research findings can intelligently inform practicing therapists; clinicians do not randomly assign patients to therapeutic interventions. Although we very much agree with Person’s thesis, we would go beyond her suggestion that case formulation–intervention matching be done within a particular theoretical orientation. Given the fact that practicing therapists do not typically adhere to a given orientation (Norcross & Goldfried, 1992), the most clinically valid research paradigm is one that would not necessarily involve pure-form therapies (Wolfe & Goldfried, 1988).

To make our research findings more relevant to the clinical setting, our paradigm needs to take into account other patient characteristics, in addition to the target symptom, that are critical to successful therapy. In their discussion of matching therapy intervention with relevant patient characteristics, Beutler and Clarkin (1990) have identified such dimensions as problem severity and level of impairment, the extent to which patients’ problems are delimited or part of a more complex theme, the degree to which patients are amenable to the influence of others, and whether or not patients have internal or external coping styles. This list is not necessarily exhaustive, but it illustrates some of the clinical realities that can influence the selection of the most effective intervention.

An attempt to take into account the matching of therapy intervention with relevant patient characteristics was made by Nelson-Gray (1991) and her research group in an outcome study on the treatment of depres-sion. Their preliminary findings point to the superiority of a cognitive–behavioral intervention that focuses on the particular variables that appear to be associated with the patient’s depression, such as irrational beliefs, deficiencies in interpersonal behavior, or infrequent gratifying activities. Similar encouraging but similarly preliminary findings were obtained by Jacobson and colleagues (Jacobson et al., 1989), who found a trend for cognitive–behavioral marital therapy to be more effective when it was individually designed to the needs of the particular couple than when it was based on a predetermined intervention.

Another approach to individualizing our inter-ventions is to bring back research on the intensive study of the single case. In commenting on the need for a more idiographic research methodology for studying applied problems, Skinner was once said to have observed that no one goes to the circus to see the average dog jump through the average hoop. Single-subject methodology in the study of psychotherapy outcome has been championed in the past (e.g., Barlow, 1981; Kazdin, 1981) but has fallen by the wayside with the ascendance of clinical trials. In a more recent consid-eration of single-case research in psychotherapy, Jones (1993) noted, “The primary means of clinical inquiry, teaching, and learning in psychotherapy has been and still remains the case-study method, grounded in the tradition of naturalistic observation” (p. 371). The promise of single-case research is that it may elucidate with precision the link between what transpires in therapy and patient change.

One of the main objections to single-case research has been the difficulties that it presents for generalizing its findings to other patients. What has been either overlooked or just rejected is the idea that such studies rep-resent a different method of aggregating information across cases. Single-case data, as Hilliard (1993) proposed, are basically inrasubject designs that explore the temporal unfolding of variables in individual participants. Generalization of findings is achieved by replication on a case-by-case basis. Too often in the past, single-case research has not been conducted with clearly formulated questions in mind. However, Hilliard maintained—and
we agree—that “theory-based, question-driven, single-case research, in which disconfirmation remains a real possibility, is necessary within psychotherapy research” (p. 379).

**Increasing Clinician–Researcher Collaboration**

As we have noted earlier, there are findings that have emerged from basic, process, and outcome research that can have important implications for clinical practice. Indeed, therapists continuing to work clinically on the basis of how they may have originally been trained, without any regard for empirical advances in the field, raises serious ethical issues. As suggested by Stricker (1992), although it may not be unethical to practice in the absence of knowledge, it is unethical to practice in the face of knowledge. We must all labor with the absence of affirmative data, but there is no excuse for ignoring contradictory data. An insistence on relying on overlearned, favored, but invalid approaches is not justified. (p. 546)

For practicing therapists to keep abreast of clinically relevant research findings, they must be presented in a form they can readily use. As we suggested earlier, research reports and reviews of research findings are written mainly for other researchers, not clinicians. In doing so, these reports have understandably emphasized research design and the methodological constraints associated with various studies, pointed to important research issues, and focused on possible future research directions. Although this information is essential to researchers, it says relatively little to the practicing clinician.

As a way of addressing this shortcoming, a new journal for the practicing clinician has appeared—*In Session: Psychotherapy in Practice*. Each issue deals with a thematic topic—for example, the therapeutic alliance, panic disorder, and resistance—and contains a review of the relevant research in a format that is useful to the practicing therapist. Thus, instead of presenting study-by-study accounts of research, which emphasize their methodological strengths and limitations, reviews of both basic and applied research offer jargon-free summaries of what we currently know about the clinical problem and its treatment.

For example, the purpose of the third issue of *In Session* was to highlight, against a backdrop of current research findings, the conceptualizations and intervention strategies used by the practicing clinician in treating individuals suffering from panic disorder with agoraphobia (Wolfe, 1995). A second purpose was to explicate what clinicians deem necessary for the treatment of this disorder in addition to the symptom-focused treatments that have been empirically tested. Thus, the issue included two research reviews, one of the basic research literature and the other of the treatment research literature. In addition, four different approaches to treatment were presented by clinicians who all began with the mainstream, empirically tested, cognitive–behavioral treatment for the management of panic symptoms and agoraphobic avoidance. Once symptom reduction had been accomplished, however, they all moved on—in somewhat different ways—to deal with issues in the client’s life that appeared to have been related to the symptomatology.

Earlier in this article, we expressed some concern about clinicians learning to conduct therapy by means of treatment manuals. As we see it, the danger is that these manuals may function as more of a straitjacket than a set of guidelines. Indeed, there is some research evidence that therapists who adhere too closely to the dictates of the manual may face the risk of being less effective clinically. This has been found in process studies of both psychodynamic therapy (Henry, Strupp, Butler, Schacht, & Binder, 1993) and cognitive therapy (Castonguay et al., 1996). In each of these investigations, the problem occurred when therapists followed the manual at the expense of clinical judgment.

There clearly is much that goes on in the clinical situation that cannot be easily specified in therapy manuals. Still, it is possible that the interventions that are studied in our outcome research, and even the nature of the designs themselves, can be better clinically informed than they are at present. In her presidential address before the 1994 Association for the Advancement of Behavior Therapy, Sobell (in press) addressed this issue of the gap between research and practice. Her premise was one that we touched on earlier in this article, namely that it is important for the producers of research findings to meet the needs of the clinical consumers. The impetus for her concern came from the Ministry of Health in Ontario, Canada, which was interested in having various agencies show better accountability for meeting community needs, including outpatient treatment.

Influenced by the relevance of a needs assessment as carried out in the corporate world as well as its practice of having customers participate in the development of the product, Sobell (in press) approached the clinicians in her community who dealt with addictive behaviors, the area of research in which she had been involved. Toward the goal of making her research findings more applicable to the practicing clinician, she involved them in the actual design of the intervention protocol, which ended up as having individualized flexibility, depending on the progress made by clients during treatment. In meeting the realistic clinical needs in the community, Sobell hoped that the findings would ultimately be of use to practicing therapists.

Although the success of this innovative approach to outcome research is still being evaluated, preliminary assessments have found that therapists and clients alike responded quite favorably to the project. Despite the additional efforts needed to develop a working alliance between researchers and clinician, Sobell (in press) was very much convinced it was more than worthwhile. As she indicated,

My only regret is that I did not get here sooner. By adopting a new approach to the dissemination of my research, similar to that used in the business community, I have reached more agencies, more practitioners, and ultimately, more clients than in
my 25 years in the field. The rewards of effective dissemination are immense for everyone.

Sobell (in press) has not been alone in recognizing the urgent need for collaboration between researcher and clinician. Wiens, Brazil, Fuller, and Solomon (1995), acknowledging that the "era of non-research documented practice has ended with the advent of health-care reform" (p. 47), have urged practicing clinicians to develop networks with researchers so that outcome studies can be conducted in the naturalistic clinical environment. Thomas D. Borkovec (personal communication, August 5, 1995) of Pennsylvania State University has recently spearheaded such a network in the Pennsylvania Psychological Association. With support from the APA, this network is dedicated to the evaluation of psychotherapy as it is practiced in actual clinical settings.

Conclusion

Ever since the Boulder Conference on clinical training urged the implementation of the scientist-practitioner model (Raimy, 1950), the field has struggled to foster the synergy between therapy research and practice. Given the external pressures facing the field to provide an empirical accounting for what we do clinically, the long-standing gap between practice and research is in need of serious reexamination.

Despite the fact that outcome research has advanced in its methodological rigor over the past few decades, the discrepancy between clinical practice and research continues to be large. We have discussed some of the reasons behind this and voiced our concerns that with recent efforts to certify EVTs, the methodological constraints associated with "clinical trials" for the treatment of DSM "disorders" will seriously constrain how we conduct psychotherapy. This indeed is a concern shared by other researchers and practitioners in the field (e.g., Benjamin, in press; Davison, 1994; Fensterheim & Raw, in press; Garfield, in press; Messer, 1994). Although we strongly believe that clinical practice should be informed by outcome research, it is quite another thing for it to be dictated by such findings. Group designs, in which patients are randomly assigned to treatment conditions, simply do not generalize to how we practice clinically. Moreover, we question the current process of selecting EVTs, in that the outcome research from which these treatments have been selected has involved theoretically pure interventions. Such findings clearly lack ecological validity, as practicing therapists find their clinical impact more effective when they do not limit themselves to pure-form treatments.

Although it is essential that we do clinically be tied to research findings, it is equally important that our conclusions about therapeutic effectiveness not be too far removed from clinical reality. Parloff (1982), who dedicated his entire career to fostering a research database for psychotherapy, made several suggestions that make sense to us:

Research evidence, however, is but one of the sets of pertinent information that should be considered by the policymaker in making reimbursement decisions. . . . In addition to rigorous, scientifically credible evidence, decision makers will wish to consider clinical evidence, evidence regarding the acceptability to clinicians and patients of particular therapeutic approaches, and evidence regarding social, economic, and ethical implications of the various forms of psychotherapy. (p. 724)

Parloff (1982) also suggested that a standing committee be empowered to make recommendations regarding reimbursable therapies on the basis of these often contradictory sets of data. Although he did not spell out the composition of this committee, the implications of his remarks suggest that the committee should be composed of practitioners, researchers, policymakers, and representatives of the public.

We would also maintain that psychotherapy outcome research is ready to move onto a fourth generation, one that allows us to generalize our findings more faithfully to what is needed clinically. Until that happens, however, we need to deal with the question of whether our existing psychotherapy outcome findings should be used to determine clinical practice. Our answer to this question is a qualified "yes, but." Yes, sometimes the emperor does indeed have new clothes, and it is important for us to acknowledge it. Other times, however, he is naked, and that should also be acknowledged. Researchers, working with practicing clinicians, need to collaborate in making this distinction.

REFERENCES
