The author argues that existing research on the outcome of psychoanalysis and the psychoanalytic therapies is sufficient to claim a solid basis in scientific evidence for psychodynamically oriented clinical work. She explores sociocultural trends that increase the probability that analytic therapists and academic researchers will misunderstand one another, and she discusses the problematic status of the randomized controlled trial as the “gold standard” of research. She urges readers to educate themselves about what the outcome research actually shows, to support empirical investigations of psychoanalytic theories and practice, to make alliances with therapists of other orientations, and to try to contribute to changing the terms in which policymakers and the public frame their understanding of mental health and mental suffering.

This paper is my personal take on the current tensions between the analytic practitioner community and contemporary researchers, with a focus on the economic, social, and intellectual context in which we practice, especially in the United States. I am hoping my reflections will contribute to a constructive conversation about the relationship between psychoanalytic theory and practice, on one hand, and systematic research on psychotherapy on the other.

Many thanks to Alan Barnett for soliciting my thoughts about the relevance of treatment-related research to psychoanalysis and psychoanalytic clinicians. I thank Jonathan Shedler for his invaluable help with this article. The comments of Michael Garrett, Kerry Gordon, and Eric Russ have also improved it. Finally, I am grateful to Mark Hilsenroth for the enormously useful special issue of Psychotherapy (March 2013) on therapy outcome that he spearheaded and edited.
Ambivalence about empirical research has suffused psychoanalysis since its inception. Despite his identification as a scientist, Freud seems to have been leery of conventional scientific evaluations of his ideas. On an occasion frequently mentioned by empirical researchers, when the experimental psychologist Saul Rosenzweig wrote to him announcing that he had demonstrated the existence of repression in the psychology laboratory, Freud responded with polite dismissiveness, stating that he felt he had demonstrated the existence of repression well enough that systematic research on the topic was unnecessary (“Still, it can do no harm”) (Gay, 1988, p. 523n.).

Although I am sympathetic to Freud's belief that his arguments, and the clinical data supporting them, were so compelling that validation from psychology labs would be superfluous, and although I continue to believe (as will be evident in what follows) that academic psychology and psychiatry laboratories tend to operate with a strategy that rewards precision of methodology over importance of content, I find myself wishing that Freud had been more generous of spirit toward those in academic settings who wanted to test his ideas. As Peter Gay (1988) has noted, his response to Rosenzweig was at best a tactical error. Many research psychologists who were potential friends of psychoanalysis experienced his indifference to their work as a narcissistic injury. What is worse, many of Freud's followers took his apparently dismissive attitude to new levels of devaluation, taking a glibly arrogant tone that has irritated researchers ever since.

In fact, the disdainful attitude of many twentieth-century American analysts arguably set the stage for the backlash against psychoanalysis and the psychodynamic therapies that has crippled the analytic tradition in the past three decades (as measured by the demise of analytic publishers, the disappearance of psychoanalytic ideas from most clinical psychology training programs, the loss of patients willing to undertake personal analysis, and the dearth of candidates at once-flourishing institutes; see Bornstein, 2001). Although that backlash, at least in the United States, owes
most of its energy to the commercial interests of pharmaceutical corporations and insurance companies, it was mainly academic psychologists with an animus against Freud and psychoanalysis who armed them with their arguments against longer-term and depth-oriented therapies.

Analysts currently face serious challenges in our efforts to claim empirical support for psychoanalysis per se. Although there is a significant and growing body of literature, there is not nearly enough high-quality research on long-term, in-depth psychoanalytic work to combat what have become the unquestioned assumptions of the anti-analytic community, especially in an era when legitimation by empirical work is demanded by mental health powers-that-be. One reason for this problem is that such research is extremely expensive; another is that psychoanalysis is widely seen as passé and therefore not on the cutting edge of what should be investigated empirically. In fact, as Kernberg (personal communication, March 24, 2013) has lamented, analysts trying to do serious research on depth approaches to therapy are in a catch-22 situation: They are told by funding agencies that grants will not be given for treatments with no demonstrated empirical validity, but analytic researchers cannot demonstrate such validity without being provided with resources with which to do empirical investigations.

Moreover, few would deny that even with adequate financial support for psychodynamic research, psychoanalytic therapies are inherently difficult to investigate empirically. Psychoanalytic therapists tend to emphasize not the application of a particular technique to a particular constellation of complaints, but an extensive collaborative effort to understand the meaning of any pattern that interferes with personal growth and life satisfaction. We respect the inevitable complexity of psychological suffering, the uniqueness of each patient, the value of a stance of not knowing, and the process of overall emotional growth rather than the conquest of discrete symptomatic complaints (see Hoffman's [2009] critique of research in the positivist tradition).

The psychoanalytic community values a disciplined subjectivity as a source of knowledge. In contrast, in empirical research subjective influence tends to be carefully minimized, controlled
for by strict, putatively objective methodologies—“putatively” because Luborsky and his colleagues (Luborsky et al., 1999) found that a remarkable amount of published psychological research reaches conclusions consistent with the preexisting biases of the researchers conducting it, a phenomenon they termed the “allegiance effect.” Needless to say, most analysts would see this effect as a confirmation of psychoanalytic assumptions about unconscious influences on best-laid plans. In psychoanalytic practice, rather than to try to eliminate subjectivity, we try to name, and to make careful inferences from, the consciously available subjective elements of our understanding of our patients. Later in this paper I discuss the incompatibilities between therapy models and research models and their implications.

There are many reasons for breaches in empathy between therapists and academic researchers. Judging by my experience as someone with one foot in each intellectual community, the temperament of most therapists is substantially different from that of most researchers. Academics tend to approach questions with a hermeneutic of doubt, whereas therapists tend to approach them with a hermeneutic of curiosity. Academics seem to me dominantly left-brained in their sensibilities, while most therapists strike me as right-brained—many have noted that researchers tend toward obsessional attunement to detail while therapists have a more hysterical, impressionistic cognitive style. Academics tend to value brilliance, whereas therapists value wisdom. Most academics like to critique and replace existing ideas (and this process is rewarded in universities), whereas most therapists like to synthesize and integrate what has come before.

If these differences were not sufficient to cause widespread misunderstanding between clinicians and researchers, there is the problem of current pressures on university professors to pursue grants and generate long lists of research publications as a condition of tenure and promotion. Ultimately, this situation has led to a lack of experience, and therefore understanding, among many researchers of what clinical work is really like. When I was a young psychologist, the academic world was somewhat lower-key. My clinical psychology teachers tended to see a few patients; their research was inspired by questions with which they had ongoing personal experience. In the current academic climate, a professor
would be foolish to see any clients, or more than a very few, as a practice would cut into the vast amount of time necessary to pursue research projects.

From the converse direction, there have been corresponding societal trends that discourage psychodynamic therapists' identification and empathy with researchers. The establishment of free-standing institutes at a time when analytic study and training could have been more integrated into university programs is one structural source of this estrangement. More recently, the movement toward independent professional schools that issue non-research-based PsyD degrees has had the consequence of segregating therapists from their university colleagues, making cross-fertilization of ideas rare, at least in the area of clinical psychology (see Shedler, 2006, on the “schism that won't go away”).

The number of professionals who have the temperament to be both good researchers and good therapists seems to me to be fairly small, and the potential for a confusion of tongues between the two groups is high. Those relatively few researchers who straddle psychoanalysis and empirical psychology tend to feel misunderstood and devalued: by analysts because they are researchers, and by the scientific community because they are psychoanalytically oriented. Hence, current conditions are not the best for psychoanalysts to collaborate with researchers or to understand the exigencies with which they struggle, and vice versa. When psychoanalysis was the “new thing,” there were many opportunities to learn from one another, and many more opportunities for support of psychoanalytic research, but the indifference of many analysts limited how much was accomplished.

In the next section, I review some important findings of the comparatively rare and beleaguered investigators who have appreciated both what the practice of psychotherapy demands and what useful empirical research on it requires.

**Direct Evidence Supporting Psychoanalysis and the Analytic Therapies**

The first major methodologically rigorous study of the outcome of psychoanalytic treatment, the Menninger Foundation Psychotherapy Research Project, initiated in 1954, followed its 42 adult
subjects for over thirty years (see Appelbaum, 1977; Kernberg et al., 1972; Wallerstein, 1986). The patients who were studied suffered mostly with quite serious psychopathology, including borderline conditions and severe personality disorders; many were at Menninger's because prior treatments had failed. Overall, the study reported substantial improvement in general functioning for patients in both psychoanalysis and analytically oriented psychotherapy. (They also found many specific interesting results, including that emotional support from the therapist was more related to improvement than insight or interpretation—thus corroborating Fromm-Reichmann's [1950] famous dictum that the patient needs an experience, not an explanation.) Five books and over sixty papers have appeared on this data set.

Later, the Columbia University Department of Psychiatry Center for Psychoanalytic Training and Research examined over 250 patients (considerably less disturbed than those in the Menninger study), and found a similar result (Bachrach, Weber, & Solomon, 1985; Weber, Bachrach, & Solomon, 1985a, 1985b). The longer the treatment and the more an analytic process had been developed—as would perhaps be expectable with higher-functioning clients who need less explicit support—the more benefit the patients demonstrated. In later research at Columbia, Vaughan and her colleagues (Vaughan et al., 2000) tried to apply the methodological rigor that is possible in short-term studies to long-term investigations of twenty-five patients in analytic treatment (nine in analysis and fifteen in psychodynamic therapy). Her group saw statistically significant therapeutic effects on a variety of measures after one year, despite the small sample size.

Researchers at the Boston Psychoanalytic Institute (Kantrowitz, Katz, Paolitto, Sashin, & Solomon, 1987a, 1987b) have conducted other methodologically rigorous investigations, including a long-term follow-up on the cases studied (Kantrowitz, Katz, & Paolitto, 1990a, 1990b, 1990c). They found that ten years later most patients had maintained their gains, a large number had continued to improve, and a small number had gotten worse. More recently, researchers in Sweden studied 450 patients in psychoanalysis and psychoanalytic therapy (Sandell et al., 2000), and found that individuals improved in direct proportion to the
frequency and duration of their treatment (see Doidge, 1997; Galatzer-Levy, Bachrach, Skolnikoff, & Waldron, 2000; McWilliams & Weinberger, 2003; and Wallerstein, 2006, for reviews of these and other psychoanalytic outcome studies).

With respect to more general research on psychotherapy, it has not escaped the attention of many in the analytic community that most of the patients whose positive experiences with therapy were summarized in the Consumer Reports study of psychotherapy effectiveness (Seligman, 1995, 1996) were seeing psychodynamic therapists, who constituted the majority of American clinicians when the study was conducted. When Freedman and his colleagues at the Institute for Psychoanalytic Training and Research (Freedman, Hoffenberg, Vorus, & Frosch, 1999) conducted an explicitly psychoanalytic replication of the Consumer Reports project, their findings were similar to Seligman's: Most people reported positive changes in therapy, and the longer and more frequent the treatment, the better the outcome. Prior research into therapy effectiveness (Smith, Glass, & Miller, 1980), conducted at a time when most therapists were even more likely to be strongly analytically influenced, had found similar results without specifying the orientation of the therapist. In Germany more recently, the extensive meta-analyses of Leichsenring and Rabung (2008, 2011) came to comparable conclusions, as did Shedler's (2010) study of meta-analytic data. In a recent naturalistic study in Sweden, Werbart, Levin, Andersson, and Sandell (2013) found similar results.

All these ambitious empirical studies of psychotherapy provide cumulative evidence that long-term psychodynamic treatment, including psychoanalysis, is effective. Many have found that it is more effective the more frequently the patient is seen and the longer he or she stays in treatment. Furthermore, it is remarkable how demonstrably helpful psychodynamic treatments have been found to be when one considers that personality differences were not figured into most of the methodologies of the outcome studies (see Blatt, Auerbach, Zuroff, & Shahar, 2006, on the relationship between personality style and outcome, and on the question of whether a particular patient is likely to respond better to support or interpretation, respectively).
But the impact of such results has paled politically when contrasted with the blizzard of empirical research, especially short-term research, conducted by investigators with other therapeutic orientations. In fact, it is commonly believed—one hears this even in academia, where people should know better—that psychoanalysis has been empirically discredited. This is an ignorant claim. One can reasonably argue that in contrast to the sheer volume of studies of short-term cognitive-behavioral therapy (CBT) treatments, psychoanalytic therapies have been insufficiently studied. A recent editorial in the *American Journal of Psychiatry (Thase, 2013)* notes, for example, that conducting randomized controlled trials was not part of the culture of academic psychiatry when psychodynamic psychotherapy was the dominant model of psychotherapy, and, in contrast to the strategy taken by the developers of “secondwave” interventions such as cognitive therapy, behavior therapy, and interpersonal psychotherapy … the traditions of psychodynamic training and practice placed little emphasis on the grouped data generated by randomized controlled trials, instead emphasizing clinical observations from individual cases. It was indeed a curious circumstance that, for several decades, there was far more evidence that the newer forms of psychotherapy were efficacious than there was for the older and more widely practiced one. (p. 954)

Similar observations have had a way of drifting, however, into equating amount of research with strength of empirical support. When analytic treatments have been studied, their effectiveness has been demonstrated. Jonathan Shedler, who helped me interpret the research data, adds that the studies of short-term manualized therapies actually show very weak effects: Patients do not get significantly better, and the clinically trivial improvements they do show are temporary. There is a strange logic at work to the effect that because there are thousands of studies of manualized short-term treatments, then there must be “strong” evidence for them. Actually, if one judges outcome by criteria of lasting change, this welter of studies has found such treatments rather ineffective.

Related to the question of the effectiveness of therapy is the question of its cost-effectiveness. Here the research is a potential powerful support to psychodynamic practice. Both the private insurance
industry and, to a lesser extent, national health services are under intense pressure to contain costs in the short term. Insurance companies, for example, evaluate their benefits managers according to the question “How much money did you save the stockholders this year?” Politicians in countries with national health services are often under pressure to save taxpayer money now.

But these realities should not distract us from the fact that research on psychotherapy cost-effectiveness consistently attests to the value of long-term and intensive treatments for mental and emotional problems (see Brooke & Axelrad, 2013; Lazar, 2010). Studies in Germany with long-term follow-up, for example, have found that the more psychoanalysis or long-term analytic therapy people have had, the less they cost the government in outlays for physical illnesses and substance abuse treatment (Beutel, Rasting, Stuhr, Ruger, & Leuzinger-Bohleber, 2004). Researchers in other countries have also found that employees who have been in psychological treatments have fewer absences and sick days and more productivity than peers without any experience of psychotherapy (e.g., Crane & Christianson, 2008). This is research that analysts should know about when we are called upon, as we increasingly are, to defend what we do.

The Special Status of the Randomized-Controlled Trial

Because of their methodological elegance, randomized-controlled trials (RCTs) are often referred to as the “gold standard” of psychotherapy research. RCTs are easiest to do when the issues investigated are construed in the simplest terms; thus, methodologically strict research on short-term interventions for discrete disorders have been the norm. Typically in such studies, researchers investigate the efficacy of a specific treatment for a designated Diagnostic and Statistic Manual of Mental Disorders (DSM) or International Classification of Diseases (ICD) disorder by choosing research participants who seem to have this condition without “comorbidity”—that is, without any other mental health issues, such as an addiction or a personality disorder. (This practice has been called “cherry-picking,” and there is a significant literature critiquing conventional psychotherapy research on the grounds that patients...
with one disorder “not comorbid with anything else” are rare in actual practice. Epidemiological studies of major depressive disorder, for example, have found that eighty percent of people with that diagnosis have other “comorbid” diagnoses; see Kessler et al., 2003).

Then investigators take baseline measures of symptomatic status with instruments that have been empirically demonstrated to be reliable and valid. Participants in the research are divided into an experimental group that receives the treatment under study and a control group that has some other experience, such as unstructured conversation or general encouragement. To ensure that every therapist in the research group is proceeding according to the framework of the approach being tested, the experimental subjects are seen by therapists (often graduate students in clinical psychology doctoral programs) who follow a treatment manual. Given funding limits, and because it is advantageous to scholars to generate a long list of short-term research projects for their CVs, there is usually a time limit on the treatment, at the end of which the participants are tested again, with improvement defined by observable symptom reduction, as measured by the assessment instruments previously used to get baseline ratings.

It is not hard to see immediately how unsuited such approaches are to the study of the psychodynamic therapies. First, as I previously noted, analytically oriented clinicians tend not to think in terms of discrete disorders but in terms of complex dynamic processes that cause people to suffer in multiple, interrelated ways. Second, we tend to trust inferences from a sensitive clinical interview more than “objective” data from self-report questionnaires and other assessment instruments. Third, because we construe our therapeutic work as offering a healing relationship that adapts to the unique individual patient and therapeutic dyad, and not principally as a set of technical procedures that target specific symptoms, we tend to be allergic to the idea of manualization. Fourth, we assume that a therapeutic process will come to an organic conclusion, and unless there are exigencies dictating that the work with any client must be short-term, we are reluctant to begin treatment with the assumption of an arbitrary end point.
Finally, we conceptualize change not as simple symptom reduction—which is of course welcome and expected—but in broader categories, such as increases in trust or attachment security (Mikulincer & Shaver, 2007), in love and work (Erikson, 1950), in play (Panksepp, 1998), in affect tolerance and regulation (Schore, 2003), in capacity for self-reflection and appreciation of the separate subjectivities of others (Fonagy, Gergely, Jurist, & Target, 2002), in self-esteem maintenance (Kohut, 1971, 1978), in resilience after stress (Bellak, Hurvich, & Gediman, 1973), in balancing self-definition and self-in-relationship needs (Blatt, 2008), and in emotional acceptance of painful realities (Stark, 1994). We often note that as patients progress toward these more global dimensions of mental health, they may have temporary recurrences of depressive or anxious symptoms, as they mourn what cannot be changed and as they take emotional risks to try out new ways of behaving (cf. Yalom, 1980). Consequently, short-term symptom relief has never been high on analysts' lists of ways to assess overall therapeutic improvement.

RCTs have been greatly valued for their methodological rigor. They are best at investigating simple, separable phenomena; the more complex the subject under study, the more problematic they are. Given the current climate in which RCTs are idealized, however, analytic researchers have realized that they must subject psychodynamic therapies to this model of investigation where possible. And despite how ill-fitting the typical RCT is with psychoanalytic clinical work as it is actually practiced, there are a growing number of specific psychodynamic therapies for delimited conditions—especially DSM-defined depressive and anxiety disorders and borderline personality disorder—that have been studied via RCTs. Such treatments have generally proven to be as beneficial as the much more frequently studied procedures developed by therapists in the cognitive-behavioral tradition (see Gerber et al., 2011).

In response to the efforts of psychoanalytic researchers to do RCTs where possible, the tide of opinion about the evidentiary status of psychodynamic therapies in the scientific community may already be turning. In a discussion of new study of a methodologically exemplary comparison of psychodynamic and cognitive-behavioral
therapies for major depression (Driessen et al., 2013), Michael Thase (2013), the CBT-oriented researcher and editor of the American Journal of Psychiatry, concludes that “when compared on a level playing field, the two forms of psychotherapy may be comparably useful” (p. 953).

In fact, some psychodynamic treatments have been found to be more effective than short-term, manualized CBT treatments, in that improvement after psychodynamic intervention tends to continue after the therapy ends, and the benefits are evident when patients are reevaluated at long-term follow-up (Shedler, 2010). Notably, this has tended to be the case in studies of psychoanalytic approaches to borderline conditions (Batemen & Fonagy, 2004; Clarkin, Yeomans, & Kernberg, 2006; Levy et al., 2006; Meares, 2012), despite the widespread public impression that Linehan's (e.g., 1993) Dialectical Behavior Therapy is the only “evidence-based” treatment for borderline personality disorder. Moreover, as Shedler (2010) has noted, the phenomena associated with improvement in the nonpsychodynamic therapies tend to be those that the psychoanalytic community has considered its core therapeutic elements, such as the strength of the working alliance, attention to the therapeutic relationship, and the opportunity for emotional expression (Blagys & Hilsenroth, 2000).

Indirect Evidence Supporting Psychoanalysis and Analytic Therapies

As I have noted, it may be legitimate for critics of psychoanalysis to complain that there are relatively few RCTs of psychodynamic treatments in comparison to the large volume of studies of manualized CBT and related treatments. But it is not legitimate to claim that there is little empirical evidence for the value of such treatments or for the concepts on which they are based (see Westen, 1998). A decade ago, Joel Weinberger and I (McWilliams & Weinberger, 2003) reviewed the empirical literature on psychodynamic psychotherapy by looking at those features of therapy that Blagys and Hilsenroth (2000) had identified, via an extensive review of the comparative psychotherapy process literature, as distinguishing psychodynamic therapies from cognitive-behavioral
treatments, namely: (1) a focus on affect and emotional expression; (2) exploration of the patient’s efforts to avoid certain topics or engage in activities that retard therapeutic process; (3) identification of patterns in the patient's actions, thoughts, feelings, experiences, and relationships; (4) emphasis on past experiences; (5) focus on interpersonal experiences; (6) emphasis on the therapeutic relationship (transference and the therapeutic alliance); (7) exploration of wishes, dreams, and fantasies.

We found that there was an empirical literature, sometimes a substantial literature, supporting each aspect of psychoanalytic therapy. For example, the classic studies of Pennebaker (e.g., 1995, 1997) corroborate the psychoanalytic idea that emotional expression is good for people. The empirically based work of Sampson and Weiss and their colleagues (Silberschatz, 2005), as well as that of Jones and his colleagues (e.g., Ablon & Jones, 1998; Jones & Pulos, 1993) found that a focus on transference and resistance enhances outcome in any type of therapy. Blatt (e.g., 2008) has illuminated a central and clinically significant personality difference in people's tendency to be preoccupied with either self-definition or self-in-relation issues. The extraordinary legacy of John Bowlby in volumes of empirical work on attachment and separation (e.g., Blatt & Levy, 2003; Holmes, 2001, Mikulincer & Shaver, 2007; Wallin, 2007) attests to the significance of personal history and interpersonal experiences for psychotherapy.

The work of Anderson and her colleagues (e.g., Anderson & Berk, 1998) and of Luborsky and Crits-Cristoph (e.g., 1990) confirms the significance of transference. Shevrin and his colleagues (e.g., Shevrin, Bond, Brakel, Hertel, & Williams, 1996) have collected decades of rigorous data on fantasy and the effects of unconscious conflicts and wish-fulfilling operations. Recent work on neuroscience has also supported many psychoanalytic ideas (e.g., Panksepp & Biven, 2012, Schore, 2003; Solms & Turnbull, 2002). And we have “hard data” that talk therapy changes the brains of both patient and therapist. As Eric Kandel (2013) recently summarized in the New York Times, “psychotherapy is a biological treatment, a brain therapy. It produces lasting, detectable physical changes in our brain, much as learning does.”

In the psychotherapy outcome literature generally, there is
now overwhelming agreement—so much so that the American Psychological Association (2013) recently issued a press release to this effect—that what is most helpful in any kind of therapy is not the technique used but the relationship that is established (see Campbell, Norcross, Vasquez, & Kaslow, 2013). Both personality factors (in the therapist as well as in the patient) and relationship factors account for the lion's share of variance in virtually every study of psychotherapy (Bindar, Holgersen, & Nielsen, 2009; Blatt & Zuroff, 2005; Norcross, 2002; Wampold, 2001, 2010). In other words, “brand names” of various techniques contribute little to outcome, and therapeutic relationships contribute greatly. To me, these findings are a vindication of psychoanalytic clinical theory, as those are the areas that our tradition has implicitly, via its emphasis on the therapeutic alliance, transference, and countertransference, regarded as most consequential for treatment.

With respect to the centrality of relationship, sometimes the contributors to the empirical literature imply that the therapeutic alliance is simply a matter of the clinician's warmth, friendliness, and curiosity. But learning to create relationships with people whose capacity for relatedness is rigidly constrained or severely impaired is not simple; it arguably requires the long experience in personal analysis or therapy that the analytic tradition values. Our understanding of transference, countertransference, and enactments allows us to have therapeutic attachments with people who are extremely difficult to engage in authentic relationship.

Interestingly, the issues that analysts have considered central to healing, growth, and change are becoming central issues in the cognitive-behavioral literature. There is virtually a cottage industry among our CBT colleagues in announcing that cognitive-behavioral psychologists are now in possession of an unprecedented idea, and then describing in new terminology something that analysts have talked about for decades. Accordingly, Bornstein (2005) has published an entertaining comparison of vocabulary used by psychoanalysts and by nonanalytic clinical scholars, respectively. For example, it is widely acknowledged by all contemporary researchers that most behavior is motivated by processes that are out of conscious awareness. But rather than use the analytically tainted term “unconscious,” they reference “implicit”
processes; where analysts refer to object representation, they mention “person schemas”; what we call ego functions they call the “central executive.” (Shortly after “behavior therapy” morphed into “cognitive-behavioral” therapy, Drew Westen [personal communication, 1989] predicted that eventually CBT will be called “cognitive-affective-behavioral therapy” because as CBT-trained clinicians practice in nonacademic settings, the same phenomena that have struck analysts over time are striking them. It could be argued that despite their rejection of psychoanalysis, cognitive-behavioral psychologists are busily reinventing it.)

A Troubling Paradigm Shift/Category Mistake

Over the past several decades, I think we have witnessed in Western cultures a slow, and to my mind sinister, redefinition of therapy and therapists (and not just psychoanalytic therapists). I have written about this previously (McWilliams, 2005), but here is a capsule summary: It used to be understood that psychotherapy is a healing art. Therapy itself was understood to be a healing relationship. Within that relationship, as both parties became more aware of what was needed to help a given person to grow and change, numerous specific techniques might be called for, but the emphasis was on the relationship itself, on therapy as a special kind of relationship. In recent decades, psychotherapy has been slowly and invisibly redefined as a set of techniques to be applied to discrete disorder categories, and therapists have been essentially redefined as authoritative technicians rather than as collaborative healers.

Here is one route by which this has happened: As biological psychiatry took off in the 1970s, there evolved an optimistic sense among psychiatrists and others that much of the emotional suffering that we had previously been able to treat only in talk therapy could be alleviated chemically. Around the same time, the American health care system switched to a corporate, managed-care model with interests in short-term cost-cutting. And also during the last third of the twentieth century, the remarkable American romance with Freud and psychoanalysis encountered the inevitable fate of all idealizations, in the form of complaining laments.
Pharmaceutical corporations clearly have a strong interest in claiming that psychological suffering can be ameliorated by their medications, and with the legal decisions in the late 1970s that allowed them to market their products directly to the public at large, we all became inundated with testimonies to the virtues of psychotropic drugs. In an effort to challenge the claim that medication is the treatment of choice for common mood disorders, some academic psychologists, the most prominent of whom is probably the CBT-oriented David Barlow (see, e.g., Barlow, Boswell, & Thompson-Hollands, 2013), began designing research to test their belief that psychotherapy can work as effectively as medication in relieving the symptoms of nonpsychotic depressions and anxiety disorders. Barlow tested short-term treatments for these disorders, as defined by DSM present-versus-absent symptom criteria, and showed that, as evaluated by reduction of overt symptoms, focused therapies generally did as well as medications.

I am appreciative of all empirical challenges to the self-interested claims of the pharmaceutical corporations. Yet in the process of taking on Big Pharma, many researchers have implicitly accepted the drug companies’ way of framing the problem. That is, that good therapy is defined by the empirically observable reduction of overt symptoms by a time-limited specific intervention. One could speculate that scientists who did this were unconsciously identifying with the aggressor, but I suspect that the larger story is that commercially driven social constructions of mental health and illness, especially as reified in the DSM (see Clegg, 2013) have invaded the sensibilities of most of us as the redefinitions have evolved. In any case, the criteria that researchers use to evaluate short-term progress have gradually become the standards for assessing overall therapy outcome. This development is in the interest not only of the drug companies but of insurance companies, who want to define therapeutic progress as symptom reduction so that they can say that the job has been done (think about their pressures on clinicians to identify “target symptoms” and to specify how the therapist is measuring these).
During this sea change, there has been an insidious assault on the criteria that analytic therapists—and humanistically oriented therapists generally, and most CBT therapists who practice in real-world settings—have traditionally used to assess whether a client's mental health is improving. The more global ways I mentioned of thinking about progress in overall functioning have gotten sidelined, and the relief of overt symptoms has become the standard for what therapists should be trying to achieve. (This is not even a good medical model: No physician would buy the idea that the reduction of a fever or the amelioration of a skin rash equates with the “cure” of an underlying complex illness.) As more short-term, focused treatments began to be developed in the face of these trends, those that had been studied via RCTs in the psychology lab got labeled as “evidence-based” and were given the status of the gold standard of therapeutic treatments, even though their actual findings have been disappointing.

I think what has happened here is what the philosophers (see Ryle, 2000) call a “category mistake.” I suspect that most therapists would agree that our praxis should be based not only on personal experience, clinical anecdote, and the authority of admired teachers, writers, and supervisors, but also on scientific investigation of our assumptions about what is helpful to our patients. But contemporarily, we are being asked not simply to accept that psychotherapy should be based on a specific kind of research, but to behave as if psychotherapy is like that kind of research. We are increasingly expected to define our patients' problems narrowly, to take empirical measures that support the diagnosis and establish a baseline for change, to manualize what we do, to impose term limits on the treatment, and to evaluate progress by objective measures of symptom change. This is a research paradigm, not a therapeutic one. It is also a perverse inversion of good science and simple common sense: Instead of expecting researchers to develop investigative methods that better model the psychological complexities of real-world clinical problems, the field appears to expect clinicians to model the artificiality of the research laboratory.

In contrast to this vision, most therapists of all orientations will agree that we cannot cherry-pick our patients; that it is an extremely
rare person who has one condition that is not comorbid with anything else; that our disciplined subjective capacities are critical for picking up nonverbal and affective elements of patients' psychologies; that we need to be technically flexible (a position supported by research demonstrating that manualization does not improve outcome; see Duncan & Miller, 2005); that ideally, treatment should evolve until both parties consider it a success and be ended unilaterally only when a therapist sees no evidence that he or she is helping; and that overall progress should be evaluated by higher-order concerns like those I have mentioned. To change current social constructions in this area, we are going to have to do the heavy lifting of educating the public about how a research paradigm that is well suited to study chemical effects sheds only the faintest light on essential therapeutic questions and should certainly not be the prototype for treatment itself. Given the public relations firms and multi-million-dollar advertising campaigns we are up against, it is hard to see how we can be effective, but sometimes trying to speak one's truth has its own kind of power.

We also need to question the definition of science that is common among psychologists, which emphasizes hypothesis testing to the exclusion of hypothesis generation (see Shedler, 2004). Before the paradigm shift I have depicted, I think it was assumed that the role of therapists in science was to be good naturalistic observers who would generate hypotheses that our research colleagues could then investigate. Science would be poor indeed without its naturalistic observers and theory-generators (think Newton, Copernicus, Darwin, Lister, Pasteur). Pressures on therapists to proceed as if the only legitimate ways of working with people are the ways that have been studied by RCTs are ubiquitous but make no scientific sense (cf. Dauphin, 2008).

Implications for the Psychoanalytic Community

There are several morals, I think, to this complex story. The first is that analysts and analytically oriented therapists need to know and to state publicly that there is a robust body of scientific evidence that supports what we do. Ideally, we would have a lot more
studies, but the ones we do have are remarkably consistent in demonstrating the benefits of the analytic therapies. It is simply not true, as many insurance companies and other critics of depth treatment have tried to claim, that psychodynamic treatments lack scientific support. The meta-analytic data are compelling, and in addition, there exist huge empirical literatures on attachment, maturation, defenses, personality, relationship patterns, trauma, dissociation, epigenesis, neuroscience, and other areas that bear directly on the ways that we think about and work with our patients.

Second, as it is not reasonable to expect health care systems, whether the American system of private insurers or the nationally funded systems of other countries, to support psychoanalysis and the psychodynamic therapies without empirically demonstrable evidence of their effectiveness, and perhaps also without evidence of their overall cost-effectiveness, we need to support psychoanalytically oriented research. I have argued that the empirical evidence for the value of our work is more than adequate already to do so. But the current cultural forces arrayed against an appreciation of that argument are daunting.

Psychoanalytic therapists are not getting unfairly picked on. Similar processes are happening in medicine, education, and other professions as funding institutions demand accountability. Our current situation is the price we inevitably pay for having a seat at the health care table. In the context of a commercially driven mass society in which decisions are made by aggregate statistics and algorithms, in which the interests of the drug companies and insurance companies are powerfully represented, the voice of psychodynamic therapists is a faint one. It needs to be amplified.

Part of our current challenge is to connect the dots between what the psychoanalytically oriented therapies are and what the research shows. This involves countering the view that “mental health” is definable as nothing more than temporary symptom remission (I am currently working on a book on this subject, aimed at a popular audience). Analytic practitioners need to be talking to the larger community about what we do and why it is helpful—not in the jargon in which we speak with each other, but in terms that are meaningful to ordinary people. That means getting
out of our offices and into policy-making roles, educational roles, universities, and subcultures that have no idea that what we offer might be of use to them. The politics that we are currently facing can annihilate us if we do not educate ourselves about how to argue back and insist on intellectually honest public discourse—and not from a stance of arrogant dismissiveness toward our intellectual opponents. Those of us with academic appointments have a particular responsibility to keep challenging the dominant paradigm, despite the fact that—assuming most analysts’ sensibilities are like my own—we would much rather pass on what we know than defend what we do.

Third, we need to find common ground with practitioners of other orientations, many of whom would be open to collaborating with us if they felt we were not patronizing them. These colleagues share our dismay at the power of pharmaceutical and insurance companies to control public discourse about mental health. Any therapist of any orientation who spends enough time trying to help people suffering with real problems, not artificially isolated symptom patterns, knows that a few sessions is not enough to support significant change. The cognitive-behavioral movement is increasingly dealing with the complex issues that analysts have written about (e.g., Linehan, 1993, on borderline phenomena; Young, Klosko & Weishaar, 2006 on personality disorders; Persons, 2008, on complex case formulation), and many CBT practitioners are eager for conversations across orientational lines. The success and vigor of the Society for the Exploration of Psychotherapy Integration is only one symptom of the cross-fertilization that is now possible and welcome in many professional quarters.

**A Few Reasons for Optimism**

What keeps me going are the occasional bright spots in this struggle. They include, for example, the fact that for the first time, there is a countervoice to the dominance of the DSM and the neo-Kraepelinian diagnostic assumptions that have propelled it ever since the 1980 shift from DSM-II to DSM-III. That revision and its successors, which have explicitly discarded dimensional, contextual, inferential, biopsychosocial thinking in favor of descriptive
psychiatric diagnosis, have contributed to many of the problems I have described. Recently, Brent Robbins of the Division of Humanistic Psychology of the American Psychological Association (APA) stated at an APA convention (August 3, 2013) that he felt his organization had succeeded in influencing the media not to refer to the DSM automatically as “psychiatry's Bible” but instead to label it as “psychiatry's controversial manual.” This is no small accomplishment.

An international group of scholars has recently formed who are raising core issues about diagnosis (see www.dsm5response.com/statement-of-concern), including whether a diagnostic system that deliberately facilitates the simplest, most clinically naive version of research really represents clinical reality. In response to the critiques of DSM-5, the National Institute of Mental Health has announced that it will not be funding research based on DSM-5 criteria. (This is actually mixed news: Thomas Insel, representing NIMH, has explained that the reason for this is that we now know that all mental illness represents problems in “brain circuits”; see www.jonathanshedler.com for a withering critique of the undemonstrated assumptions behind this position.)

In Sweden a few years ago, the government was persuaded to change the health care system such that only “evidence-based” treatments (read: those that have been investigated by RCTs, and hence mostly short-term CBT treatments) would be covered by the national health policies. This was a big change from its prior funding of most psychoanalytically oriented treatments. After a couple of years of proceeding this way, the Swedes found that the government was spending more on mental health than it had when it supported psychodynamic and longer-term therapies, as people returned for one after another follow-up treatment and developed other mental health problems such as substance use disorders (see http://scottdmiller.com/icce/revolution-in-swedish-mental-health-practice-the-cognitive-behavioral-therapy-monopoly-gives-way/).

In the United States, it is not insignificant that a major, empirically oriented, APA peer-reviewed journal has recently published articles about the problems created by current research practices. For example, Clara Hill and her colleagues (Hill, Chui, & Baumann, 2013)
have argued for including individualized and qualitative measurements of outcome rather than, or in addition to, the quantitatively handled self-report measures currently used (cf. Shedler, Mayman, & Manis, 1993). Their plea echoes prior researchers’ appeals to study psychotherapy outcome by a range of methods and in actual treatment settings rather than in the artificiality of the psychology laboratory (e.g., Westen, Novotny, & Thompson-Brenner, 2004).

Perhaps our strongest weapon against the cultural forces that are industrializing and trivializing therapy is that we help people. Our patients realize after an in-depth treatment that the changes they have made are not surface phenomena. Eventually, most popular beliefs about how easy it is for people to heal or change are corrected in the crucible of painful reality. All therapy innovations have begun with assertions of greater efficiency and effectiveness than what has come before. That happened with psychoanalysis itself, which Freud originally construed as a relatively short-term treatment, and then with the humanistic approaches, and then with family systems models, and then with behaviorism and its offshoots, and it is now happening with biological psychiatry. They all started with claims of faster action and more stunning successes, and they have all been humbled by the difficulties that are inherent in trying to reduce human mental suffering.

Because our patients know we have helped them, even if analysts become marginal in health care systems, as is increasingly the case, I doubt that psychoanalytic practice will be wiped off the map. But it cannot hurt for us to support researchers whose work is critical to our credibility and to counter misinformation with knowledge of the empirical findings that do substantiate the value of our work. Psychoanalysis is all about an ethic of honesty (Thompson, 2004). An honest reading of the empirical literature on psychotherapy supports psychoanalytic practice.

References


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