
BOOK REVIEW

PSYCHODYNAMIC PSYCHOTHERAPY RESEARCH: Evidence-Based Practice and Practice-Based Evidence, edited by Raymond A. Levy, J. Stuart Ablon, and Horst Kächele, New York: Humana Press, 2011, xxxiii + 646 pp., \$239.00 (cloth).

Reviewed by

John S. Auerbach, PhD

*Malcolm Randall Veterans Affairs Medical Center, Gainesville, Florida, and
University of Florida*

Let me begin by asserting two propositions that many in our field think of as opposed to each other but which I regard as adequately capturing the hybrid nature of the discipline known as psychoanalysis: First, psychoanalysis is a hermeneutic discipline, a method by which we come to understand and interpret human meanings, in particular unconscious meanings. Second, psychoanalysis is a scientific discipline, an attempt to link the meanings that humans create to basic emotional and motivational systems, systems that are in turn linked to brain function and structure. In the context of this review, I can only assert, not argue the validity of these two propositions, but I mention them for some important reasons, given that I am reviewing a lengthy book on the state of scientific evidence with regard to psychodynamic psychotherapy, a term here taken to mean, as in the book under review, psychoanalysis and the various forms of psychotherapy that are derived from psychoanalytic principles.

One reason is that there is increasing conflict within the field of as to what kind of discipline is psychoanalysis. Is it a hermeneutic discipline, perhaps a wisdom tradition or part of the humanities? Or is it a science, part of the effort to understand the natural world? Representatives of these two divergent perspectives, often leaders in the field (e.g., [Eagle & Wolitzky, 2011](#); [Hoffman, 2009](#)), seem to talk past each other, with rigid positions sometimes drawn about an essentially metatheoretical debate, rather than about what I regard as more properly psychoanalytic issues like the relative roles of relational (attachment), sexual, and selfobject (narcissistic) needs in human development or the relative roles of relationship, clinical technique, and accuracy of interpretation in the outcome of an analysis. No doubt there are many reasons for this state of affairs, but I think that any analytic clinician will recognize that rigidity often indicates defense against anxiety, and I suspect that the anxiety involved here, although of course I cannot prove it scientifically, is an anxiety about the basic nature of the field. Who and what are we really?

Please note, in case anyone misunderstands me, that I am not saying that the proponents of a hermeneutic perspective are the rigid, defensive ones here and that the

Correspondence concerning this article should be addressed to John S. Auerbach, PhD, Psychosocial Rehabilitation and Recovery Center, 825 NW 23rd Avenue, Building 3, Suite A, Gainesville, FL 32609. E-mail: john.auerbach@va.gov

proponents of a scientific view are the flexible, open ones, or vice versa. I am simply saying that a repetitive argument points to an anxiety in the field as a whole, an anxiety from which I would in no way be immune and that I believe pertains to our concerns about survival as a discipline in an academic world that seems increasingly to value reductionism, whether of the biological or behavioral kind, rather than the kind of complex, nonreductive thinking that psychoanalysis necessarily involves.

A second reason for starting here, with this particular debate, is that I have a public reputation for arguing that psychoanalysis is essentially a science and that its questions are best answered through scientific inquiry. So if I happen to think, for example, that humans start life in a condition of relatedness or attachment, and that there is no such thing as normal autism or primary narcissism or primary autoerotism, I think so because the weight of the scientific evidence falls on one side of that particular question, rather than the other. But I also happen to think that psychoanalysis is a hybrid discipline and that it is a science in the service of its hermeneutics, by which I mean that it uses the methods of science to study the meanings that humans create. In this context, then, I am restricting the term *hermeneutics* to refer specifically to the study (or interpretation) of human meaning, rather than to classical and more expansive definitions of the term, which also regard it, since the time of Dilthey (see, e.g., Makkreel, 1992), as an epistemology parallel to or even competing with that of science. Unfortunately, in this limited space, I cannot grapple further with these highly complex issues. Nevertheless, in defense of the hermeneutic aspects of psychoanalysis, I am also asserting that the study of human meaning is different from, say, the study of brain function in that, as I shall argue later, in reviewing the section of this book pertaining to neurobiology, minds create meanings but brains do not. One way, therefore, of characterizing psychoanalysis's particular hermeneutics is that psychoanalysis pertains not just to human meanings but specifically to unconscious human meanings, meanings that humans have difficulty in formulating or maintaining in awareness because they are linked to basic emotions and motivations that put them in conflict with other important people in their lives. On this perspective, the aim of psychoanalysis, as a discipline, is to develop a scientific understanding of these particular unconscious human meanings and their place in human affairs, but it is these unconscious human meanings, which pertain to basic issues in human life like relatedness, sexuality, identity, and death, that make psychoanalysis of interest in the first place. I mention all of this not only to provide a possible solution to one of the many, to use a properly psychoanalytic term, interminable debates that encumber our field—to help hermeneutically inclined readers to understand that science truly is necessary if the field of psychoanalysis is to advance and to remind scientifically inclined readers that the interpretation of unconscious meaning is why psychoanalysis matters at all—but also to provide readers of this review an understanding of the perspective from which I come to understand the volume before us.

So first some praise for *Psychodynamic Psychotherapy Research*: At nearly 700 pages including front matter, it is not quite a comprehensive survey of scientific developments within psychoanalysis but it certainly covers most of the best work in the field today, with nearly all of its many chapters written by leading researchers. In an academic world in which there is often active prejudice against researchers interested in psychoanalysis and psychodynamic theory, the fact that there are now so many psychoanalytic researchers that a book of this length is not quite comprehensive in its coverage of the field is a cause for celebration. Still, there are omissions (e.g., not much here on attachment theory or on cognitive science), and, no surprise in a such a long book, not all of the chapters are of the same high quality, but there are enough that are truly excellent that anyone interested

in the current state of scientific knowledge on psychoanalysis and other psychoanalytic treatments would do well to consult this particular volume. Because of the book's length, I cannot in this review discuss every contribution to it, and I will therefore apologize to all the chapter authors whose worthy efforts I cannot mention. Instead, I will provide a highly impressionistic survey of the chapters that I found particularly interesting and, when I had concerns about a chapter's thrust, will raise what I hope its authors will regard as respectful challenges meant to further dialogue and discussion.

Outcome Research

The volume then has five sections: outcome research; neurobiology; theory, technique, and process; single case studies; and assessment of change. The outcome section contains two meta-analyses, one a reprint of [Shedler's \(2010\)](#) *American Psychologist* paper and the other an updating by Rabung and Leichensring of their articles in *Journal of the American Medical Association* ([Leichsenring & Rabung, 2008](#)) and *British Journal of Psychiatry* ([Leichsenring & Rabung, 2011](#)), that are two of the most important contributions to the psychoanalytic research literature in the past decade because of their demonstration of the therapeutic effectiveness of psychodynamic treatments, particularly over the long term. In this regard, these meta-analyses indicate that dynamic therapies not only are therapeutically effective but also have long-term, posttreatment effects that are not found with other forms of psychotherapies. These chapters therefore provide proof that what we do when we do dynamic therapy actually helps our patients. Because of the vitriol of the responses to these papers when they were initially published, I wish that the editors of this handbook had found room to include those responses, together with the counterresponses by Shedler and by Leichensring and Rabung, this so that readers who are not researchers can get a clearer idea not only of the messiness and subjectivity of the supposedly objective scientific process but also of the difficulties that psychoanalytic researchers face in a hostile academic world, additionally of the academic dignity with which Shedler and Leichensring and Rabung responded to attacks of surprising intensity.

As for the other contributions to this section, they are a mixed lot, and I suspect that readers may find themselves frustrated with the complexity of both the Munich Psychotherapy Study and the Helsinki Psychotherapy Study, as important as both of those efforts are to the study of the therapeutic efficacy of psychoanalysis and psychoanalytic therapy. On the other hand, the chapter by Slavin-Mulford and Hilsenroth is particularly important because it is the first review ever published of the research literature on the efficacy of psychodynamic treatments for anxiety disorders. Because the received wisdom in the field is that cognitive-behavioral therapies are the most effective and efficacious treatments available for anxiety disorders, it is helpful to have available Slavin-Mulford and Hilsenroth's demonstration that such conditions do respond to psychodynamic therapies and, more important, per the aforementioned meta-analyses by Sheldler and by Rabung and Leichensring, that psychodynamic psychotherapies produce long-term symptom reduction that other forms of treatment do not. Of particular problem, therefore, is Taylor's review of the literature on psychodynamic treatment of depression. Although Taylor's presentation of the evidence demonstrating the efficacy of psychodynamic therapy for depression is thorough and helpful, his chapter could have done with less of a polemic, even though it is one with which I largely agree, about the faulty framework of assumptions involved in conceptualizing depression as a unitary disease, rather than as a group of heterogeneous disturbances.

I must discuss at greater length the chapter by Levy, Meehan, and Yeomans on the

psychotherapy of borderline personality disorder (BPD) because of the excellence and importance of the randomized controlled trial (RCT) that Kernberg's research group (see [Clarkin, Levy, Lenzenweger, & Kernberg, 2007](#); [Levy et al., 2006](#)), with which the chapter authors are affiliated, conducted demonstrating the clinical efficacy of Transference Focused Psychotherapy (TFP; [Clarkin, Yeomans, & Kernberg, 1999](#)), when compared with that of [Linehan's \(1993\)](#) Dialectical Behavior Therapy (DBT). As regards this particular RCT, let me further note that Kernberg deserves additional respect because he is one of the first major psychoanalytic theorists to subject his ideas to rigorous empirical scrutiny, something that very few major psychoanalytic thinkers have done. As for the chapter at hand, Levy et al. discuss, with intelligence and thoroughness, the growing evidence that DBT is not the only psychotherapy with research in support of its efficacy in the treatment of borderline patients. This is a particularly important matter because, until the emergence of DBT, the only approaches available for the treatment of BPD were dynamic ones, such that the psychodynamic world considered itself to own, as it were, the diagnosis, this despite having done no research proving the efficacy of its treatment efforts. As Levy et al. show, the list of treatments for BPD that have at least some research support include, in addition to DBT, three that are psychodynamic or psychoanalytic in conceptualization (Mentalization-Based Therapy [[Bateman & Fonagy, 2004](#)], Supportive Therapy [[Rockland, 1992](#)], and TFP) and one that, although cognitive-behavioral in origin, has many dynamic elements (Schema-Focused Therapy [[Young, Klosko, & Weishaar, 2003](#)]). They further note that all therapies for BPD that have research support include considerable support and supervision for the therapists involved, are well-structured affairs that emphasize the maintenance of the therapeutic frame, and rely heavily on the therapeutic relationship. In this regard, even the more behavioral approaches to the treatment of BPD would not be possible without the long history of psychoanalytic thought on the nature of the therapeutic relationship. But much though I like and respect both this contribution by Levy et al. and the research on which it is based, my main concern about their chapter is that some readers might take it to be too much of a brief in favor of TFP, as opposed to the other empirically supported approaches, when we simply do not yet have enough studies of these various therapies to be certain in our judgments of them. I would have preferred it if, in addition to demonstrating that the evidence in favor of DBT is less solid than everyone thinks, they had devoted more attention to a critique of the failure of psychoanalytic theorists, Kernberg being a very notable exception (and Fonagy another), to research their approaches to therapy with BPD until Linehan came around. In this regard, I will remind readers that, before the early 1990s, when there were already several decades of psychoanalytic writing on borderline conditions, there were no randomized clinical trials studying psychoanalytic treatments of borderline patients. But these comments are mere quibbles about what I otherwise thought was an excellent chapter; I had much more difficulty with the section of the book that pertains to neurobiology.

Neurobiology

As regards the neurobiology section, I will acknowledge that part of my difficulty came from my own relatively limited knowledge in this area, even though I agree with the argument that what creates the human capacity to create meanings, especially unconscious ones, is the human brain. Indeed, to argue that the human capacity to create meanings can be separated from human brain functioning is to fall into a philosophical idealism and,

insofar as science pertains to the explanation of the material or natural world, to reject scientific explanation of mental phenomena altogether. My limitations in this area would have been significantly alleviated if the volume's index were better, but its index contains no mention of structures like, for example, the insula, a part of the brain that several of the chapters find important because its role in emotion awareness and in self-other differentiation, and inadequate mention of several other key structures (e.g., the anterior cingulate cortex, the amygdala, the hippocampus, and various parts of the prefrontal cortex) that are also discussed in many of the chapters. The matter of indexing is particularly important here because the neurobiology section is the most technical part of the volume and the section that contains material most likely to be unfamiliar to psychodynamic readers.

But more important is that, even though it is good to know, as several of the chapters point out, most notably those by Roffman, Gerber, and Glick and by Riess, that brain structures like mirror neurons, the anterior cingulate cortex, and the insula likely provide the neural underpinnings of important psychological phenomena like empathy, mentalization, and self-other differentiation, all crucial matters in psychodynamic theory, it is, in my view, premature to devote 150 pages of a book devoted to psychotherapy research to an area that is so very distant from the working experience of most psychotherapists. One key indicator of this problem is the frequent use of the word "may" in Roffman et al.'s exemplary review of the implications of the current neurobiological literature for psychodynamic practice, as in, for example, the following passage (p. 203):

For example, we may learn that some aspects of transference are rooted in procedural memories, by noting their association with activity in the basal ganglia. These elements of transference may be learned earlier in life, change more slowly, and be more amenable to supportive interventions than to higher level interpretations. In contrast, we may learn that other aspects of transference are rooted in implicit associative memories but associating them with activity in the frontal cortex and hippocampus. These elements may stem from later conflict and change relatively quickly in response to defense interpretations. Ultimately, neuroimaging paradigms... could help us clarify different aspects of transference in the laboratory in such a way that could be directly applied to clinical technique.

In short, I much appreciated Roffman et al.'s willingness to use the word "may"—to display caution where caution is warranted, a caution that I found lacking in some of the contributions to this section—because they recognize that it is highly speculative to link aspects of transference to neurobiology when, in the realm of empirical psychological research, the concept of transference is still so ill defined. Another chapter that was relatively careful in its inferences was Gabbard's discussion of the neurobiology of borderline personality disorder—especially of the potential links between unstable object relations in borderline conditions and hyperreactivity of the hypothalamic-pituitary-adrenal axis and of the amygdala—because it is clear, from the chapter, that the empirical literature in this area remains quite sparse.

Particularly troubling, therefore, about several of the chapters in this section, especially those which discuss current research on brain imaging in psychodynamic psychotherapy, is that nearly all of these chapters cite, as part of their evidence base in the published literature, the same handful of small-sample brain imaging studies in cognitive-behavior therapy, in coming to conclusions that, I think, should still be highly tentative about how psychotherapy affects the brain. I also found troubling that so many of these contributions discussed the relationship between psychodynamic phenomena and neurobiology but that so few of them had anything to say about the relationship between

psychoanalysis and cognitive science, a much more closely related discipline because it is still mainly psychological and not biological. Exceptions to this pattern include the contributions by Gerber, by Roffman et al., and by Wong and Haywood, all of which recognize that psychoanalysis will need a new cognitive psychology, an updating of Freud's antiquated topographic and structural models, to serve as a bridging language between psychodynamic phenomena and neurobiological events. Still it is a problem that so many of the chapters in this section pertain to neurobiological, not cognitive psychological, research and that none of them mention major, sometimes book-length, works by scholars like Bornstein (Bornstein & Pittman, 1992), Bucci (1997); Erdelyi (1985), and Horowitz (1988, 1998), among others, that explore the links between psychoanalysis and cognitive science.

This relative absence of discussion of cognitive psychology as a bridging field between psychodynamics and neurobiology brings us to the main problem with this section—specifically, that many of its chapters seem to have difficulty in maintaining adequate distinctions between mental phenomena and neural phenomena, between mind and brain. To be sure, there has been, since the time of Descartes, no more vexed and vexing question in Western thought than the relationship between mind and brain, so with much foolhardiness, let me assert, again not argue, two fundamental propositions: First, psychoanalysis, since the time of Freud, has aimed to study mental or psychological phenomena in a scientific manner; second, also since the time of Freud, psychoanalysis has attempted to study these things in a nonreductionistic way, one that preserves the autonomy of the psychological realm and that uses a bridging language, for too long Freud's outdated metapsychology, to link the psychological to the neurological. One reason that the relationship between mind and brain is likely to stay vexing and vexed is that it is probable that, for every mental event, there are likely multiple neural correlates and that, for every neural event, there are likely multiple mental correlates. What then are we to make, for example, of this representative statement from Viamontes's chapter on the neurobiological foundations of psychotherapy (p. 331): "Theoretically, in order to treat a patient, a therapist must first create a 'virtual' model of the patient's cognitive and affective organization within his or her own brain." This is by no means the only sentence in this chapter that confuses mind—the software, to use the now-overburdened computer metaphor, that creates meaning—with brain, the neural hardware that makes possible mental functions like creation of meaning. Another way of putting this is to say that minds have intentionality (in the philosophical sense), meaning, and purpose but that brains, even highly complex ones, do not and that a psychoanalytic clinician is mainly interested in the meanings that he or she and other minds in the room are cocreating, not in the neural events that underlie them. A chapter in the neurobiology section like that by Riess is likely to be helpful to a psychoanalytic audience because it draws connections between the brain and a psychological function or capacity like empathy; a chapter like Viamontes's makes claims about the neurobiology of psychotherapy that many will find highly problematic.

Theory, Technique, and Process

It is therefore with relief that many readers will leave, as this one did, the neurobiology section and move on to research that seems much closer to clinical experience and practice. In the next section, on theory, technique, and practice, are chapters by some of the leading psychotherapy researchers in the field, researchers who also have the sophistication of expert clinicians and who therefore know how to study psychoanalytic process

in a manner that well captures its richness and complexity. Thus, for example, Luyten, Blatt, and Mayes are concerned to tell us that therapeutic change is a matter not merely of symptom reduction but also and perhaps mainly of the interplay between two fundamental personality dimensions, relatedness and self-definition, that mutually constitute and define each other. They also propose that we think of psychotherapy as a dyadic system, the creation of a mutual interaction between patient and therapist, not something that a therapist does to a patient, and they particularly stress that psychotherapy is not a linear process, the result of something that a therapist does to a patient, but instead a complex dialogic process with many interactive and nonlinear effects. In short, a reader interested in a conceptualization of the psychotherapeutic process that is both relational and empirically grounded will find much of value in this chapter. Smith-Hansen, Levy, Seybert, Erhardt, and Ablon discuss the research on Jones's (2000) Psychotherapy Process Q-Set (PQS), a method for coding therapy sessions from an atheoretical point of view, and remind us that the label that we use for characterizing a therapist's approach and what that therapist actually does in a specific therapy are two different things. They also demonstrate the utility of Jones's method in several case studies, and Perry, Petraglia, Olson, Presniak, and Metzger do the same with the Defense Mechanism Rating Scale (DMRS; Perry, 1990), a system for coding defenses as they unfold in the context of a session and for evaluating the level of maturity of those defenses. Particularly useful about their chapter is their presentation of extensive session transcripts that help the reader to understand a session as it unfolds moment by moment. This type of material is likely to be particularly helpful to clinicians, who will be able to obtain a more direct and concrete appreciation of what defense scores mean and what kinds of defense interpretations are likely to be helpful. I must say, however, that after reading the transcripts provided here and in several other chapters in the volume, I had many concerns about whether research findings were truly influencing therapeutic technique, but I will reserve those comments until later in this review.

I found that the three most important chapters in the section on theory, technique, and process were those by Levy, Meehan, Temes, and Yeomans on attachment theory, by Hilsenroth, Cromer, and Ackerman on the practical implications of therapeutic alliance research, and by Høglend and Gabbard summarizing the results of what Høglend and his research group have described as the first empirical study of transference interpretations. Within the psychodynamic world, there is no theoretical perspective that has as much empirical support as does attachment theory, such that it has escaped the psychoanalytic ghetto, yet it is a puzzling fact that there are no process measures of shifts in attachment-related variables—for example, of therapist utterances designed to encourage secure-base behavior on the part of the patient or of patient utterances indicating felt security within the therapeutic relationship—within a therapy session. It would of course be possible for a system like the Psychotherapy PQS, Luborsky's (1984) Core Conflict Relationship Themes, or Benjamin's (1996) Structural Analysis of Social Behavior (not mentioned in the volume) to measure concepts like this, but the point here is that although, as ably summarized by Levy et al., there are many indications that attachment status is a powerful variable in the therapeutic process, no one seems to have developed a method for studying how attachment dynamics emerge within the context of a therapy session and whether those moment-by-moment dynamics, like the alliance ruptures and repairs studied by Safran and Muran (2000), moderate or mediate outcome. No doubt there are many good reasons for this state of affairs—research programs are complex undertakings—but the absence of such a methodology speaks to the many disconnects within our field and to the distance

we have yet to traverse if we are to integrate successfully clinical wisdom and empirical knowledge.

The chapter by Hilsenroth et al. is an excellent distillation of the empirical literature, across theoretical orientation, of what therapist traits and behaviors are likely to build the therapeutic alliance and increase therapeutic efficacy and which therapist traits and behaviors are likely to have the opposite effect. It should come as little surprise that therapists who are active, supportive, affirming, collaborative, and emotionally engaged are likely to create a strong working alliance and that therapists who are passive, distant, critical, controlling, and aloof are not. In this regard, there is little evidence to support the utility or efficacy of the so-called classical analytical stance, in which the therapist maintains abstinence, anonymity, and neutrality, and much to support the wisdom of Carl Rogers and of relational thought in general, although Hilsenroth et al. do not make this point explicitly. The matter of good therapeutic technique is more complex, however. For example, although there is empirical evidence in support of the therapist's revealing his or her emotional reactions in the here and now, the current literature finds that the therapist's discussing his or her own dynamic conflicts is likely to disrupt the therapeutic alliance, and in this regard, we might speculate, that psychoanalytic concepts like anonymity and neutrality, to my considerable surprise, are still useful and need to be reformulated, not discarded. Patients may need to know how we react to them as people but not about those aspects of our personal histories with which we have had difficulty and might be still struggling.

Another important issue, as Hilsenroth et al. point out, is that the modern analytic literature, increasingly relational in its orientation, often conflates working with the therapeutic relationship in the here and now with working in the transference. These are not the same thing, however, and the empirical literature supports the idea of the therapist's commenting on the therapeutic relationship in the present and being open to the patient's comments about such. These two things are what a therapist would do in repairing an alliance rupture. On the other hand, the research literature finds there are negative consequences when therapists make transference interpretations—that is, interpretations that explicitly describe the patient as treating the therapist as if he or she were the patient's parent or some other figure from the patient's past—in a rigid or unyielding manner. As Høglend and Gabbard note, in their summary of the experimental study, by Høglend's research group, of the clinical effectiveness of transference interpretations, patients often respond to such interpretations, especially if they are delivered in the form of, "You are reacting to me as if I were your parent," with considerable defensiveness, as if being criticized. Indeed, when I read interpretations of this kind, as found in therapy transcripts provided by Høglend and Gabbard in their chapter, as well as by Perry et al. in theirs, I often wondered whether the relational and self-psychological revolutions in psychoanalysis had ever happened. To my thinking and contrary to many opinions, the revolution in psychoanalytic technique in the last 30 years is not a focus on transference interpretations, which are likely to be heard by patients as accusatory, but instead on the present relationship and on the therapist's or analyst's coconstruction of it, often through what is now termed enactment of the therapist's dynamics. But aside from chapters like that by Smith-Hansen et al., which, in summarizing research using the Psychotherapy QRS, also shows the bidirectional nature of the therapist-patient interaction, there is surprisingly little discussion in the volume on relational therapeutic processes and the therapist's contribution to them, as if the relational revolution, of which attachment theory is a part, has not quite made it from the consulting room to the research team. Meanwhile, the findings by Høglend's research group that the use of transference interpretations has, for the most part,

little effect on outcome and that the patients that benefit most from transference interpretations have a lower level of object relations and representations seems counterintuitive only until one considers the clinical lore that the more impaired a patient is relationally, the more effort one spends on establishing and maintaining the therapeutic relationship and the less attention one gives to the patient's specific therapy content and dynamics. For higher functioning patients, more of the therapeutic focus will be on their specific therapy concerns, rather than on interventions needed to keep the patient in the therapy, and although transference is always a presence in the dialogue, the higher functioning patient will be less likely unconsciously to regard the therapist as a figure from the past. But because patients with a lower level of object relations are, almost by definition, more likely to be volatile and to hear transference interpretations within the context of their long-established dynamics, it should also be unsurprising that transference interpretations with them are, as [Gabbard et al. \(1994\)](#) have noted, a high-risk, high-gain intervention.

Single-Case Research

The next section of the volume, on single-case research, is likely to be of even greater interest to clinicians because it is single cases, individual patients, who therapists in fact treat and who were the means through which Breuer and Freud introduced psychoanalysis to the world in the first place. These chapters, more than any in the volume, help us to see the processes through which a given psychotherapy, whether psychodynamic or otherwise, actually works, so if any section of this book is clinically relevant, it is this one. One of the contributions in this section, that by Katzenstein, Pole, Ablon, and Olsen, presents Jones's concept of interaction structures, the long-standing patterns of bidirectional influence between patient and therapist, that are slow to change, that structure the process of a specific therapy, and that are assessed through the Psychotherapy PQS. The chapter is therefore a powerful demonstration of the use of time-series analysis to show how the bidirectional interaction between therapist and patient produces its therapeutic effects.

The other two contributions in this section discuss aspects of Kächele's case of Amalia X, which Kächele, as analyst, terms a specimen case. Even if these contributions told us nothing about the psychotherapeutic process, Kächele is to be commended for his willingness to expose his own clinical work to examination and scrutiny in such a thoroughgoing way. The psychoanalytic literature is filled with highly processed case studies that I regard as largely, if not mainly, self-serving, and few of us would have the therapeutic and scientific courage that Kächele has shown in making this particular case available to the scholarly community. Indeed, the chapter by Levy, Ablon, Thomä, Kächele, Ackerman, Ehrhardt, and Seybert contains extensive transcripts from a single session of the case, a session that Kächele regards as a specimen hour and that most readers, in my view, will regard as a superb example of psychotherapeutic attunement and responsiveness. The transcripts make for truly compelling reading, far more real than the narratives that can be found in any case report. At the same time, I found the chapter troubling because Kächele formulates the case, that of a woman with hirsutism, in terms of penis envy, and leaving aside questions of feminism, I found myself thinking, simply on empirical grounds, that there must be better ways than the largely discredited theory of penis envy to conceptualize Amalia X's concerns about her femininity—that female identity forms around many things other than the lack of a penis. My point here is that, even in a chapter by a leading psychoanalytic researcher in a hefty volume dedicated

entirely to research on psychoanalytic constructs, it remains difficult for psychoanalysis to revise itself in accordance with empirical knowledge.

Assessment

The final section of the book pertains to assessment, and although all three of its chapters are of high quality, this part of the volume comes as somewhat of a letdown after the extensive material on psychotherapy process. Nevertheless, this section is extremely useful not only for researchers who need information about important psychodynamic assessment systems like the Social Cognition and Object Relations Scale (SCORS; Westen, 1995), the Shedler-Westen Assessment Procedure (SWAP; Shedler & Westen, 2007), the DMSR, the Adult Attachment Interview (AAI; George, Kaplan, & Main, 2006), the Reflective Function Scale (RFS; Fonagy, Steele, Steele, & Target, 1997), and the Structured Interview of Personality Organization (STIPO; Clarkin, Caligor, Stern, & Kernberg, 2004), among others. For self-serving reasons, I wish that the authors here had found room to discuss the Object Relations Inventory (ORI) and the Differentiation-Relatedness Scale (D-RS; see Blatt, Auerbach, & Levy, 1997; Diamond, Kaslow, Coonerty, & Blatt, 1990), but the main service that this section of the book performs is to give clinicians ideas about how they might do psychodynamic assessment in a manner that increases diagnostic rigor—that is, what we would otherwise call reliability and validity—while maintaining clinical (and psychodynamic) richness and subtlety.

Conclusion

In sum, *Psychodynamic Psychotherapy Research* is a volume of uneven quality, as would be expected in a book of this length and with this many chapters, but it is also a book with many excellent contributions by many leading psychoanalytic researchers. Although I suspect that many in the psychoanalytic audience will be frustrated by much of the section on neurobiology, I also think that most psychoanalytic clinicians will come away from the volume with greater understanding of the hidden dimensions of the clinical process, as well as of some of the challenges involved in studying these dimensions. More important, psychoanalytic therapists will come away from this book with practical pieces of empirical knowledge that they can use to improve the effectiveness of their clinical efforts, and I have therefore reviewed this book from the perspective of what a psychoanalytic clinician might find in it that would be of therapeutic use. That is, I think that a major purpose of empirical research on psychoanalysis is to help psychoanalytic clinicians develop hermeneutic or interpretive systems that accord with current scientific knowledge, and my side wager, not yet proved, is that an interpretive system that better accords with scientific knowledge is likely to be a therapeutically more effective one. Despite its flaws, this volume succeeds, I believe, in helping psychoanalytic therapists to think clinically in an empirically more sound way. If nothing else, psychoanalytic clinicians will come away from this volume with greater knowledge of the wealth of clinically sensitive research to which they can turn if they want to understand their work in a more rigorous way and to find better ways of helping their patients. However, I will note in closing that this book likely was also written to speak to another audience—clinicians and researchers outside the psychoanalytic ghetto who think that psychoanalysis has nothing to tell them because, after all, as everyone knows, psychoanalysis has no empirical support. No doubt there are many people in that audience who will never be convinced that psychoanalysis

does have such support because, sadly, science be damned, if your mind is already made up, no one can convince you with the facts. But if there are even a handful of nonpsychoanalytic colleagues who read at least parts of the book and become intrigued by them—and if, better yet, some of those nonpsychoanalytic colleagues happen to be graduate students—then there is also a chance that we can widen the psychoanalytic world into something other than the disappearing ghetto that it is slowly becoming. To do that, we will need to convince some people with the facts, and this book, by showing how vibrant the world of psychoanalytic research actually is, helps make that possible.

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